

DOCUMENT REVIEWED: “Charter Schools in Eight States: Effects on Achievement, Attainment, Integration and Competition”

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Summary of Review

Charter Schools in Eight States uses longitudinal data from eight states to evaluate the effects of charter schools on achievement, attainment, integration, and competition. The findings are mixed. *Achievement*: The study examines seven jurisdictions and finds insignificant effects on reading and math performance in five, and small negative effects in two others. *Attainment*: In the two jurisdictions with data, the study finds positive effects for charter high schools’ rates of graduation and college matriculation. *Integration*: The study finds no evidence that charter schools are skimming high-achieving students away from public schools, or that charter schools lead to increased racial/ethnic stratification, but these findings should be regarded as equivocal because the supporting analyses use highly aggregated data. *Competition*: The study finds no evidence that the average student achievement at public schools either increases or decreases in response to entry of

charter schools to the educational “marketplace.” On the whole, the methods used in this report are exemplary. The authors describe their statistical analyses in a transparent manner that makes it possible for readers to form their own opinions about the strength of the argument being advanced. The review does raise questions about all four of the report’s sections, particularly stressing some weaknesses in the data and analyses regarding the integration and competition findings. Overall, however, the report makes an important contribution to the empirical literature on charter school effectiveness.

Review

I. INTRODUCTION

For some time now the effectiveness of charter schools has been a controversial topic. In theory, by “freeing” teachers and principals from many (if not most) of the district-level constraints placed on traditional public schools, a learning environment can be fostered in which instructional innovations (such as new curricula and new pedagogical strategies) are encouraged and readily implemented. This would be matched with increased communication and involvement from parents and local communities. The appeal of this vision is considerable. If charter school attendance leads to improvements in the way children are taught, it would seem hard to imagine a result that did not involve an effect on academic outcomes. In his first public address on the topic of education after taking office, President Obama expressed support for an expansion of charter schools as an alternative to traditional public schools, saying “I call on states to reform their charter rules and lift caps on the number of allowable charter schools, wherever such caps are in place.”¹

Despite the political enthusiasm for charter schools, the empirical evidence of their effects on academic achievement has been mixed. In the cases where positive effects on achievement have been found,² these effects have tended to be relatively small and difficult to generalize. Given this backdrop, the RAND Corporation study, *Charter Schools in Eight States: Effects on Achievement, Attainment, Integration and Competition*, by Ron Zimmer, Brian Gill, Kevin Booker, Stephane Lavertu, Tim Sass and John Witte,³ is very timely. The authors pose four primary research questions about charter schools:

1. What are the characteristics of students transferring to charter schools?
2. What effect do charter schools have on test-score gains for students who transfer between traditional public schools and charter schools?
3. What is the effect of attending a charter high school on the probability of graduating and of entering college?
4. What effect does the introduction of charter schools have on the test scores of students in nearby public schools?

The first of these questions is addressed through a descriptive analysis; the remaining three are addressed through the use of regression models.

II. FINDINGS AND CONCLUSION OF THE REPORT

The findings can be summarized as follows:

1. On average, students who transfer into charter schools have test scores similar to, or lower than, those of their peers in the public schools they previously attended. The authors conclude from this that charter schools are not “skimming” high-achieving students away from public schools. They also conclude that there is little evidence that students tend to transfer to charter schools with considerably different racial/ethnic distributions of students. An important exception is African-American students, who are most likely to attend charter schools with higher concentrations of African-Americans than the public schools they leave.
2. In five jurisdictions, test score gains associated with charter schools in reading

and math were about the same as those associated with traditional public schools. Only in Texas and Chicago were significant effects found for charter schools, and in those cases the effects were negative. In both cases the most defensible estimates generalize only to charter schools enrolling students in the secondary (called “nonprimary” in the report) grades. In addition, the authors point to evidence that (a) charter schools in their first year of operation and (b) virtual charter schools (prevalent in Ohio) are most likely to have students who experience lower score gains than they experience in traditional public schools. Finally, there is limited evidence that charter school effects on student achievement are considerably more variable than public school effects. (Note that Florida was not included in this achievement analysis.)

3. Students who attend charter high schools in Florida and Chicago are significantly more likely to graduate and more likely to attend a two- or four-year college than their peers in traditional public high schools. These results do not appear to be a function of school size or underlying differences in academic achievement. No graduation or college matriculation data were available for charter high schools in the other six geographic locations.
4. There is no evidence to support the hypothesis that the presence of charter schools affects the performance of nearby public schools, in either a positive direction (e.g., through competition) or a negative direction (e.g., by diverting financial resources).

In the report’s final chapter the authors speculate about possible explanations for some of their more unusual findings, about broader implications for policymakers, and

about methodological implications for future research on charter schools.

III. THE REPORT’S RATIONALE FOR ITS FINDINGS AND CONCLUSIONS

The report employs student-level panel data ranging from the 1994-95 through the 2007-08 school years across eight geographic locations. Three of these locations include the schools in an entire state (Florida, Ohio and Texas) and five include the schools in large urban school districts (Chicago, Denver, Milwaukee, Philadelphia, and San Diego). The number of charter schools in each location ranges from a low of 21 in Denver to a high of 246 in Ohio. The number of years of data available ranges from a low of 4 on Ohio to a high of 9 in Chicago, San Diego and Texas. Information at the student level includes student grade, race and ethnicity, and test scores in math and reading. In Chicago and Florida information about high-school graduation and college attendance were also available. Within a given location, test scores used longitudinally were placed onto a common scale after standardizing each unique year and grade combination (using the district or state means and standard deviations). Some concerns with this approach are described in section V below.

Relatively simple descriptive techniques involving the comparisons of means via crosstabulations are used to describe the characteristics of students transferring into charter schools, estimates of charter school effects on achievement, attainment and nearby public school performance are derived using statistical modeling.

The report’s estimates of charter effects on achievement are based on a linear regression model known as a “fixed-effects” regression, a method that allowed the researchers to compare the average test score gains of

students over the years when they are enrolled in charter schools with the average gains when they are enrolled in public schools. Taking the difference of these gains (charter school gain minus public school gain) provides an estimate of the effect of a charter school on the academic achievement for each student; in other words, each student serves as his or her own control. In this sense, each student represents a fixed effect. The overall effect of charter schools is subsequently estimated as the average of these within-student effects across all students.

Estimates of charter effects on educational attainment in the RAND study are based on a probit analysis, in which is the authors initially control for observable student-level variables that might confound the charter effect. To address the possibility of self-selection bias, they then control for unobservable factors that may contribute to both the decision to attend charters and to the likelihood of graduating from high school or matriculating to college by using a bivariate probit model. In this model two correlated equations are involved—one that predicts charter attendance and another that predicts either future high school graduation or college attendance. If the assumptions of the model are correct, this serves to purge the estimated charter effect of bias due to student and family self-selection into charters.

Finally, the effects of charter school competition on nearby public school performance are estimated using another form of a fixed-effects regression. Competition is measured for each public school as either the distance to nearest charter school or as the number of charter schools within 2.5 miles. The fixed effects of interest in this analysis consist of student-by-school interactions. As the authors write: “Competitive effects are, therefore, estimated by examining the growth of achievement of the same students in the

same schools as the level of charter competition” (p. 80).

IV. THE REPORT’S USE OF THE RESEARCH LITERATURE

The RAND study has a quasi-experimental design. Students and their families are not randomly assigned to charter or public schools, but self-select for reasons that may be observable (e.g., prior academic achievement, demographics) or unobservable (e.g., motivation, culture). The RAND authors use statistical models to adjust for selection bias, and the extent to which such adjustments lead to valid inferences will always be open to debate, even when the approaches taken are defensible, as is typically the case in this report. The fact that most of the empirical research on charter school effects is based upon quasi-experimental designs represents a primary reason that the results from this rapidly expanding literature are often taken with a grain of salt. The authors demonstrate a solid appreciation for this through the research that they cite and in the approaches that they take to estimate and interpret charter effects. Give their study design, they justify the use of fixed-effects regression models by noting that this approach has been endorsed in a methodological review by an organization known as the Charter School Achievement Panel.⁴ However, the authors also clearly convey the potential problems with the fixed-effects regression model, problems recently elaborated upon in chapters by Caroline Hoxby and Sonali Murarka,⁵ and by Dale Ballou, Bettie Teasley and Tim Zeidner⁶ in *Charter School Outcomes*.⁷

One conspicuous omission is any discussion of modeling approaches for estimating charter effects that involve the use of hierarchical linear modeling, also known as mixed-effects modeling. A high-profile and large-

scale study of charters that applied a hierarchical linear model was conducted Henry Braun and colleagues. The Braun study evaluated charter effects using NAEP data to resolve a preexisting dispute.⁸ The implication of the RAND authors' choice of a fixed-effects model is that such an approach is superior to the one taken by Braun and colleagues—but the rationale for this important methodological decision is not provided.

V. REVIEW OF THE REPORT'S METHODS

The Characteristics of Students who Transfer to Charter Schools

The evidence that charter schools are not “skimming” high-achieving students or causing increases in racial/ethnic segregation would be more convincing if it were also examined at disaggregated levels. In a given geographic location, the RAND authors show that the average test scores of students transferring to charters tend to be lower than the average scores of students in the public schools they left. But no information is provided about the variability of this contrast across schools or school districts. This makes it difficult to rule out a competing hypothesis that within school districts, certain charter schools skim high-achieving students from traditional public schools while others have a mission that focuses specifically on low-achieving students. As illustrated later in this review, the approach used by the RAND authors does not allow such patterns to be detected.

By contrast, in their analysis of achievement effects (as described below), the authors were very careful to present their results at both aggregate and disaggregated levels. An analysis that examined patterns in student-level transfer decisions over time using either a logistic or probit model might lead to

more nuanced interpretations, along the lines of the approach taken by the authors in their Chapter 4 analysis.

The Effects of Charter Schools on Academic Achievement and Educational Attainment

A consistent theme in the analyses of achievement and attainment effects in Chapters 3-4 is the tradeoff between internal and external validity. These are issues that had been raised by Hoxby & Murarka and Ballou, Teasley & Zeidner in the context of estimating achievement effects, and the authors are very attentive to them throughout their report. The key idea is that to make the case that an internally valid achievement effect has been estimated, the reader must be convinced that a reasonable proxy can be found for the key counterfactual outcome—the test score gain a charter school student *would have* experienced had he or she been enrolled in a public school. This counterfactual is identified under a fixed-effects regression as the observed gain of the *same* charter student when that student was (previously or subsequently) attending a public school. The upshot of this is that the RAND authors are restricting the sample of students used to estimate a charter effect to only those students who had been enrolled in both public and charter schools (e.g., “switchers”) over a span of three or more years. To the extent that unobservable student-specific factors (e.g., motivation or family circumstances) do not change systematically over time, the resulting charter effect will be internally valid for charter switchers, but not externally valid (generalizable) to charter stayers—those students enrolled only in charter schools during the time period included in the dataset. This generalizability limitation is especially true if these stayers differ considerably from charter switchers.

The same issue arises in reverse when the study attempts to estimate internally valid charter effects on educational attainment (graduation and college matriculation) by restricting the sample to only those students who had attended a charter school prior to high school. The authors estimated the effects by comparing probabilities of graduating high school or attending college for those students attending charter schools in both middle school and high school relative to those students attending a charter in middle school and a traditional public school thereafter. As such, no charter effect on attainment is being estimated for students who switch from a public middle school to a charter high school. It would, of course, be possible to estimate effects of charter schools that apply to all enrolled students. Indeed, such an approach has been taken in other, previous studies.⁹ But while such effects might be more externally valid, the authors of the RAND study appear to be unwilling to make this tradeoff at the perceived expense of internal validity.

A strength of the RAND study is that the authors consistently anticipate possible threats to validity, both internal and external, and bring them to the surface for objective consideration. When the estimates depend upon testable assumptions through choices made in the specification of their regression models, the authors test them against alternative specifications. For example, after estimating the effects of charter schools on math and reading test scores across seven geographic locations, the authors conduct a sensitivity analysis in which they restrict the sample of charter school students to those in secondary grades (middle and high school). The logic here is that students who switch from a public school to a charter school (or vice-versa) “midstream” during the primary grades are different from those who switch at a structural transition point such as the

completion of elementary school. (While the authors do not confirm this latter intuition empirically, they do show that across all charter schools, switchers tend to have lower test scores than stayers.) Interestingly, the results from this sensitivity analysis are largely consistent with those from their initial regression analysis: the effects of charter schools on reading and math scores are generally insignificant. Nonetheless they conclude that these are the most defensible estimates of charter effects. In only one location (Ohio) do the results change appreciably, moving from strongly negative to insignificant once the sample of students and schools is restricted.

As another example of a type of sensitivity analysis, the authors relax a constraint imposed by their initial regression model: that charter schools produce a single aggregate effect on achievement by test subject in each geographic location. First, the authors disaggregate charter effects in Ohio as a function of charter type (classroom-based or virtual/computer-based). Next, the authors differentiate their effect estimates by age of the charter schools (1, 2 or >2 years of operation). Finally, they disaggregate effects by the race/ethnicity of the student samples (African-American, Hispanic, and White). These analyses provide nuanced insights about the conditions under which charter schools might be expected to be most effective or ineffective.

- The negative effects of charter schools in Ohio appear to be driven by a large presence of virtual charter schools in the primary grades. The authors note that this seems consistent with findings in an earlier analysis of virtual charter schools in California by the report’s first author, Ron Zimmer, and his colleagues.¹⁰
- The authors find that in Chicago, Ohio and Texas, charter schools are most

likely to have significant negative effects on achievement in their first year of operation. By contrast, in Denver, charter effects are large and positive for 12 schools in their first year of operation, but only with respect to test scores in math. In Milwaukee, Philadelphia and San Diego there is no relationship between the size of charter effects and school age. Curiously, the authors conclude from these mixed results that “across locations, the performance of charter schools as measured by their achievement generally improves after their first year of operation” (p. 85). This statement does not seem consistent with the results they report.

- There are no clear patterns with respect to the differential effectiveness of charter schools by racial groupings. Most effect estimates are statistically insignificant. Unfortunately the authors provide no information about the sample sizes being used to estimate charter effects by race. If the number of switching students is small, the number of switching students of a particular race is even smaller. Hence it comes as little surprise that these estimates are noisy, given the low statistical power. One unexpected result to which the authors give little attention is the large positive effect of charter schools on the math scores of white students in Denver. One explanation for this might come from the authors’ finding in Chapter 2 that white students who transfer to charter schools in Denver have slightly higher math scores than their public school peers. In contrast, African American and Hispanic students who transfer to Denver charters tend to have lower math scores than their public school peers.

In a related review, Robert Bifulco has pointed out three potential weaknesses in the

achievement-oriented analyses in the RAND report.¹¹ The first is that the test score outcomes used across grades, years and states may not be adequately comparable. The second is that no other models beyond a fixed-effects regression appear to have been considered or applied as a sensitivity check. The third is that the inclusion of a better set of mobility control variables could change the interpretation of charter effects—although in which direction is not entirely clear. For details on the second and third points, the reader is referred to Bifulco (2009), pp. 3-4. The first of these points merits a brief elaboration.

The longitudinal test score outcomes used in the RAND analyses appear at first glance entirely comparable, but this is an artifact of the choice made to standardize these scores by grade and year within each state or district under analysis. This approach, however, sweeps under the rug a number of important issues. To begin with, how well are the tests in a given state aligned with the respective curricula in public schools and charter schools? For example, if charter schools choose to implement innovative curricula, this content may not be captured on a traditional large-scale assessment. If true, this would bias the results against charter schools. Perhaps more importantly, when gain scores are used as the outcome in a fixed-effects regression, an implicit assumption is that these scores are continuous measures with interval properties. In other words, a 10-point score gain from, say, grade 3 to 4 should have the same meaning regardless of the initial score in grade 3. This assumption has been recently called into question in the context of psychometric approaches taken to scale tests such that score magnitudes are consistent within and across grades.¹² Even in an appendix section devoted specifically to their data sources, the authors of the RAND study provide

minimal information that supports the validity of the tests used as outcomes measures.

Finally, the RAND authors never mention one puzzling and potentially related finding from their analysis: the R^2 from the fixed-effects regressions varies quite dramatically by geographic location, from a low of 0.17 to a high of 0.46. Given that the variables included in the models are the same from location to location, this seems unexpected.

The Effect of Charter Competition on Nearby Public Schools

There are at least two principal reasons why estimating this sort of indirect effect using a fixed-effects regression (or for that matter, any statistical model) is especially difficult. First, under economic theory the effect of competition occurs over the long term and would be unlikely to occur over the relatively short time span considered in the present study unless it were dramatic. The statistical power to pick up a small effect over a short time span is small. Second, any determination of when a charter school is “close enough” to put competitive pressure on a public school will be equivocal at best. Hence, beyond any issues that could be raised about the tenability of the assumptions of the specified fixed-effects regression, the construct validity of the “treatment” variable is very questionable, something that the RAND authors acknowledge.

VI. REVIEW OF THE VALIDITY OF THE FINDINGS AND CONCLUSION

The Characteristics of Students who Transfer to Charter Schools

The RAND report indicates that charter schools do not appear to be skimming students by achievement or leading to the stratification of students by race. Aggrega-

tion bias is a plausible threat to this conclusion. One can imagine a hypothetical scenario of a school district with two public schools (A and B) and two charter schools (C and D). Imagine further that charter school C skims high-achieving students from school A, while charter school D attracts the low-achieving students from school B. Depending on the relative sample sizes of these schools and the students transferring between them, an aggregate finding consistent with those set forth by the RAND report would not be surprising, even though public school A may have lost a disproportionate number of its best students. A second level of potential aggregation bias comes from averaging over all years of available data. This may obscure important trends in the extent to which skimming by achievement may or may not be occurring. The same criticism applies to the RAND analysis of student sorting by race. The results found here seem to contradict the results found in studies by Bifulco & Ladd and Dee & Fu.¹³

The Effect of Charter Schools on Academic Achievement

The conclusions reached with respect to this research question tend to be carefully qualified and supported by defensible empirical analyses. The evidence with respect to charter effects on student achievement also seems consistent with findings of previous quasi-experimental studies. At best, students in charter schools appear to show score improvements similar to the gains they demonstrated while in public schools. In two locations (Chicago and Texas) there is evidence they may do about a tenth of a standard deviation worse, and this would seem to be cause for concern.

The biggest threat to the internal validity of the report’s estimates for charter effects on achievement is the largely untestable as-

sumption that all confounding explanations for a difference in a given student's test score gains as that student shifts from a charter school to a public school (or vice-versa) stay constant over time. If this assumption is wrong, estimated charter effects could be either too high or too low. For example, if students transfer to charter schools because of a positive shift in their motivation to do well in school, then the true effect of charters is probably lower than what has been found in this study; if the transfers are because students have hit a rough patch and parents are seeking a way to improve things, the true effect is probably higher than what has been found in this study. To their credit, the authors note this issue from the outset of the report. The best way to address it empirically would be to conduct a study under a randomized experimental design, estimate a "true" average effect, and then attempt to estimate the same effect using a fixed-effects regression with a nonexperimental control group. In fact, according to the authors, this approach is currently being taken in a federally funded study being conducted by Mathematica Inc.

The Effect of Charter Schools on Educational Attainment

The RAND report provides novel evidence that charter schools have a positive effect on educational attainment. The effect was estimated by comparing probabilities of graduating high school or attending college for those students attending charter schools in both middle school and high school relative to those students attending a charter in middle school and a traditional public school thereafter. As the authors note, the finding of positive attainment effects is based on a restricted sample of students in two locations (charter high schools in Chicago and Florida), so this limits the generalizability of the inference.

Some concerns about the internal validity of these results can be found by scrutinizing the parameter estimates from the underlying probit analyses. For example, the bivariate probit model suggests the presence of strongly *negative* selection bias. This implies that students who choose to attend charter high schools are those who are less likely to graduate or attend college relative to those students who choose to attend traditional public schools. As the authors note, this finding seems counterintuitive. In addition, the results suggest that after controlling for prior test scores, demographic variables and self-selection, special education students attending charter schools are anywhere from 4% to 8% more likely to graduate from high school (whether the high school is a charter or traditional public) than their non-special education peers. (A similar result was found in the univariate probit specification.) This result is conceivable, but also seems counterintuitive. These sorts of unusual parameter estimates do not necessarily invalidate the estimate of charter effects on attainment, but they do hint at a possible mismatch between the data and the statistical model—a mismatch the authors may wish to explore in the future should they seek to publish this study in a peer-reviewed journal.¹⁴

The Effect of Charter Competition on Nearby Public Schools

The RAND authors find no evidence of an indirect effect (negative or positive) on public schools through the competition engendered by their proximity to charter schools. This analysis comprises the shortest chapter in the RAND report (6 pages) and also the most equivocal. The authors note, "we regard the results in this chapter as suggestive but not definitive" (p. 80). Indeed, unlike the analyses in chapters 3 and 4, the authors pay much less attention to checks on the sensitivity of their findings to alternate specifica-

tions of the statistical model. What can be concluded from these analyses is that there is no apparent evidence of short-term competition effects large enough to register as either statistically or practically significant.

VII. USEFULNESS OF THE REPORT FOR POLICY AND PRACTICE

The RAND report represents one of the most extensive studies on charter school effectiveness across the United States to date and is sure to be influential as state policymakers decide whether to encourage or discourage an expansion in charter school availability. On the whole, the methodological approaches it takes are sophisticated, thoughtful, and even-handed. As with any analyses of this sort, the causal inferences that might be drawn are subject to threats against internal and (particularly with this study) external validity in varying degrees. Interestingly, this report will probably be used as empirical ammunition by those who support charter schools as well as by those who oppose them.

Those predisposed to support charter schools are likely to emphasize the findings

that charter schools do not appear to skim high-achieving students or increase racial or ethnic stratification, have positive effects on educational attainment (graduation and college matriculation), and do not appear to have negative impacts on nearby public schools. Those predisposed to oppose charter schools are likely to emphasize the findings of insignificant charter effects on student achievement in 5 of 7 locations, negative effects in 2 of 7 locations, and the lack of evidence for positive competition effects on public schools.

While the findings related to skimming, sorting and competition are probably best classified as suggestive—since some very plausible alternative explanations can be advanced—the findings related to achievement and attainment effects rest on somewhat stronger methodological ground. In any case, it is important to note that these findings only generalize to (a) students who transfer from charter schools to public schools or vice-versa, and (b) charter schools serving students in the secondary grades. The evidence on the effects of elementary charter schools on student outcomes to date is still unclear.

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