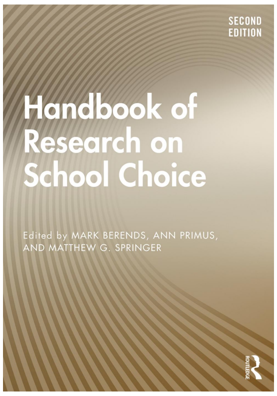

Handbook of Research on School Choice

Mark Berends, Ann Primus, Matthew G. Springer

Magnet School Outcomes

Publication details
https://test.routledgehandbooks.com/doi/10.4324/9781351210447-20
Julian R. Betts, Chenzi Cao
Published online on: 25 Jun 2019

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MAGNET SCHOOL OUTCOMES

Julian R. Betts and Chenzi Cao[1]

Magnet schools originated in the United States as a tool to desegregate school districts. The idea was to create public schools with a distinct curricular or pedagogical focus and to allow any student in the district to attend. Typically, magnet schools are located in lower-income areas of a district to draw higher-income students of different races or ethnicities from other parts of the district. In some cases, magnet schools are created in relatively affluent neighborhoods with the same strategy in mind: By opening enrollment to all students in the district, the expectation is that, on average, those who come to the school from other neighborhoods will be different. In this way, both socio-economic and racial integration may result.

Magnet schools pre-date charter schools, but both are types of public school choice. Magnet schools began in the U.S. in the 1960s and 1970s, in reaction to the perceived need to desegregate public schools. (The first magnet school was established in 1968 in Tacoma, Washington.) Court orders to desegregate schools often listed the creation of magnet schools as a tool for integrating districts. (For more information on the history of magnet schools, see Chapter 18 of this volume.)

Charter schools are a more recent innovation. What magnet and charter schools have in common is that they are both public schools that provide choice by opening up enrollment beyond a narrow attendance zone. A key distinction, though, is that magnet schools are typically administered by the host school district, while charter schools are independently managed and usually are provided exemptions from much of the home state's education code, with the goal of encouraging experimentation.

For magnet schools to indeed serve as a "magnet"—drawing students in from across the district—one would expect that parents would be attracted by the potential of the schools to deliver a higher-quality education. For example, a magnet school that routinely boosts math or reading achievement could in theory attract many applicants from around the district. This naturally leads to the question: "What do we know about the impact of magnet schools on student achievement?"

This chapter examines the literature on magnet schools and achievement. It begins with an overview of the growth of magnet schools, then proceeds to a short methodological description of the meta-analytical technique that we use for assessing the relation between magnet schools and achievement. The subsequent section presents the results of the meta-analysis. This statistical technique allows us to obtain an overall estimated relation between magnet schools and achievement, and, at least as importantly, allows us to estimate the variation in the strength of this relationship across settings. A final section summarizes and discusses promising approaches for future research.

Trends in Magnet School Growth and Enrollment

The U.S. Department of Education estimates that in 2014–2015, magnet schools enrolled 2.6 million students, up from 1.2 million in 2000–2001 (Snyder, de Brey, & Dillow, 2018). Figure 20.1 shows enrollment at magnet and charter schools as a percentage of total enrollment by school year. Both magnet and charter schools increased their share of total public school enrollment, to 5.2 percent and 5.4 percent respectively, by the 2014–2015 school year. Notably, these official statistics are likely to slightly undercount the number of magnet schools nationally. The U.S. Department of Education reports that several states in the Common Core of Data submission each year list none of their schools as magnet schools, even though the federal government has funded magnet schools in those states. For instance, in the 2013–2014 Common Core of Data, the U.S. Department of Education sets the magnet indicator to missing for Georgia, Massachusetts, and New Jersey. Magnet Schools of America, a nonprofit organization, estimates the number of magnet schools is much higher: 4,340 as of 2018, compared to the 3,285 reported in the Common Core of Data in 2014–2015 (Magnet Schools of America, 2018).

Figure 20.2 shows the number of magnet schools by year and grade span. Almost 60 percent are elementary schools.

An Overview of Meta-Analysis

A convenient and meaningful way to report results is as effect sizes. The effect size is the predicted effect of attending a magnet school on student achievement, measured as the number of standard deviations that attending a magnet school is predicted to increase or decrease test scores. We used individual studies' effect size estimates, or converted them into effect sizes by dividing by the standard deviation of test scores in the given grade as reported by the study. Thus, an effect size of 0.1 indicates that a student's test score rises by one tenth of a standard deviation relative to the comparison population if the student attends a magnet school for one year. In grade spans where we found

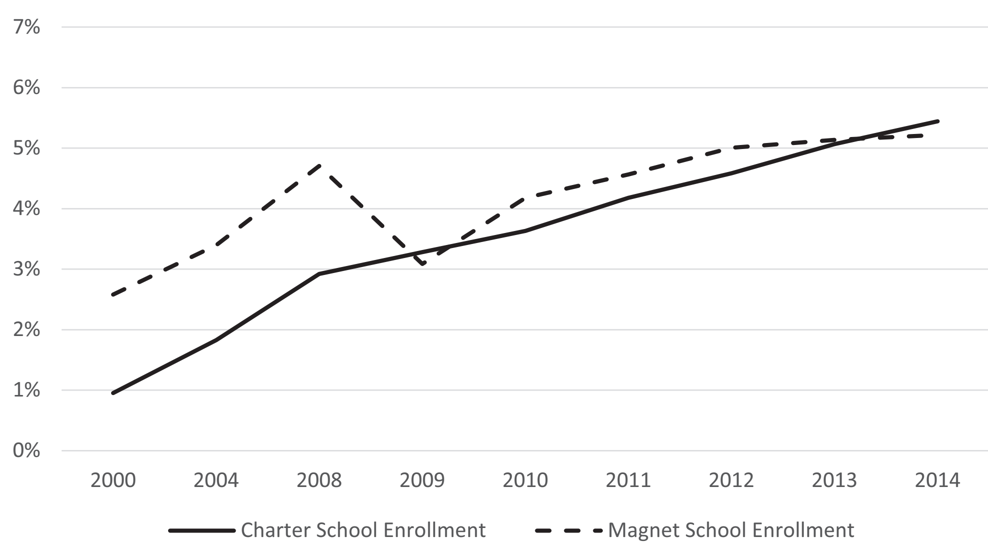

Figure 20.1 Magnet and Charter Enrollment as Percentage of Total Public School Enrollment.

Source: Snyder, De Brey, and Dillow (2018, p. 185, Table 216.20). Year refers to fall of school year.

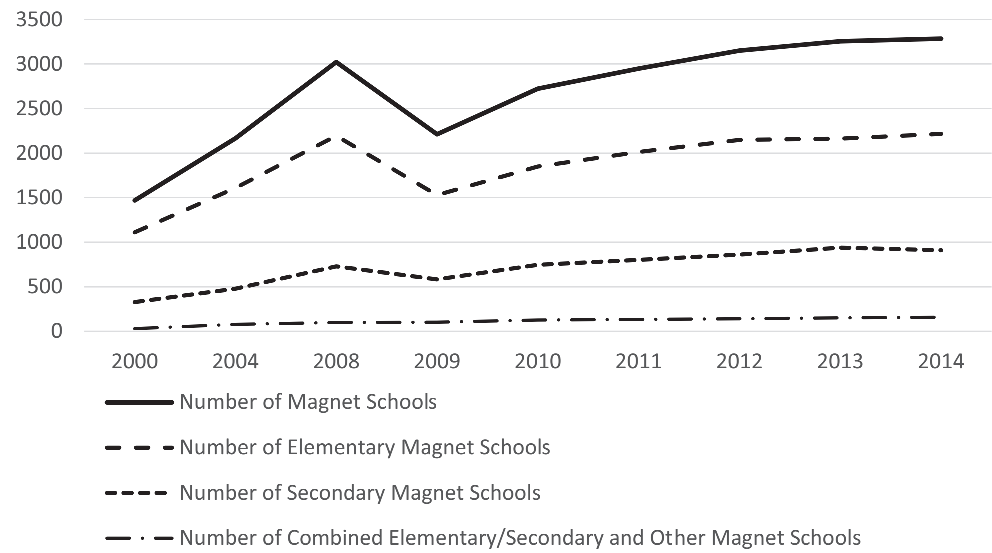

Figure 20.2 Number of Magnet Schools by Grade Span.

Source: Snyder, De Brey, and Dillow (2018, p. 185, Table 216.20). Year refers to fall of school year.

a significant overall effect of magnet schools, we also represent these as the predicted change in a student's percentile ranking from attending a magnet school for one year.

We assume that the effect of magnet schools on achievement is not fixed across studies. Given that magnet schools choose their own curricular theme and pedagogical approach, we should not expect them all to perform at the same level academically. Equally important, the comparison schools in each study are likely to vary in rates of academic progress, which matters because all studies compare achievement changes in magnet and traditional public schools. We performed a random effects meta-analysis to allow for and to measure the likely heterogeneous effects of magnet schools. (For a review of the random-effects approach to meta-analysis and measures of heterogeneity, see Borenstein, Hedges, Higgins, & Rothstein [2009], Chapters 12–16.)

In a random effects meta-analysis, we take a weighted average of the effect sizes across studies. If Y_i is the effect size for the i^{th} of k studies and W_i is the weight for each study, our overall estimated effect size M is:

$$(1) \quad M = \frac{\sum_{i=1}^{k} W_i Y_i}{\sum_{i=1}^{k} W_i}$$

The weight for each study is the inverse of the sum of the within-study variance (based on the standard error) and an estimate of the true between-study variance, T^2:

$$(2) \quad W_i = \frac{1}{V_{Y_i} + T^2}$$

The between-studies variance estimate T^2 is based on a method of moments estimate of the variance of true effect sizes. Note that as T^2 becomes large relative to the average within-study variance estimate, then we will tend toward equal weighting across studies, and as T^2 becomes relatively small the weights can become highly unequal, with heavier weight given to studies with the lowest sampling variance.

We report the I^2 statistic introduced by Higgins, Thompson, Deeks, and Altman (2003), which provides an estimate of the percentage of the variation in effect sizes that reflects true underlying variation.

To be included in a meta-analysis, a research paper must provide an estimated effect of attending a magnet school, the standard deviation of test scores that one can use to transform the magnet school coefficient into an effect size, and the standard error, which measures how precisely the coefficient is estimated. Unfortunately, a number of the older papers in the literature did not provide the standard error of the estimated effect, and/or they did not provide the standard deviation of test scores. Without the standard error, we lack a measure of precision, making it impossible to estimate the weight that the study should be given. The latter problem, in which the standard deviation of test scores was not reported, matters because without knowing how much test scores vary within a given grade, a magnet school effect of "four points" on a test is meaningless and cannot be compared to results from other studies.

Ballou (2009) offered an excellent descriptive literature review of the magnet school literature on achievement. Because Ballou focused on the overall direction of effects in a study, and did not try to estimate an overall effect or to compare studies, his sample included a number of older papers that we cannot include here because the papers failed to provide standard errors or standard deviations of test scores. His complementary approach remains highly worth reading.

Selected Studies and Data Handling

We selected studies based on Ballou's (2009) literature review and a Google Scholar search for studies related to magnet schools and achievement. To gain a sense of whether papers were relevant to the focus of our study, we examined titles, abstracts, and introductions. Chosen papers needed to either 1) use lottery data from school choice applications, which if used carefully creates a randomized control trial, or 2) contain statistically based estimates of the impact of magnet schools on achievement that account for other student characteristics *including past achievement*. All studies in this second category thus used a value-added approach, meaning that they controlled for students' past achievement. This is important, because a study that does not take into account a student's past achievement risks misidentifying as a magnet school's "success" what really might simply be the higher-than-average prior achievement of the student who transferred to the school. Of course, the reverse could occur as well. Betts, Tang, and Zau (2010) provided an example of the importance of using a value-added approach when they attempted to replicate the results of a lottery-based study of a charter school without using the lottery data. They found that it was crucial to control for students' past achievement to replicate the lottery-based result.

We excluded studies for various reasons. Figure 20.3, which is inspired by Figure 1 of Liberati et al. (2009), provides details. Of 24 studies on magnet schools and achievement, we excluded 16 studies. Seven studies were excluded because they did not control for past achievement and, in two of these cases, the analyses were not conducted at a student level. The other most common reasons we excluded a study were that standard errors were not reported for the effect size, the estimated effect could not be converted into an effect size, and the outcome combined testing on disparate subject areas.

For each of the eight remaining studies included in the meta-analysis, we recorded the effect size and standard error for a one-year effect of attending magnet schools. The first author made decisions about which estimate to use.

Table 20.1 presents details on the studies of magnet schools and achievement that are contained in our meta-analyses. Given the limited number of papers that provide estimates of both effect sizes and the corresponding standard errors, our main analysis included one representative estimate from each study, covering the maximum number of grades and schools possible. In some cases, a study

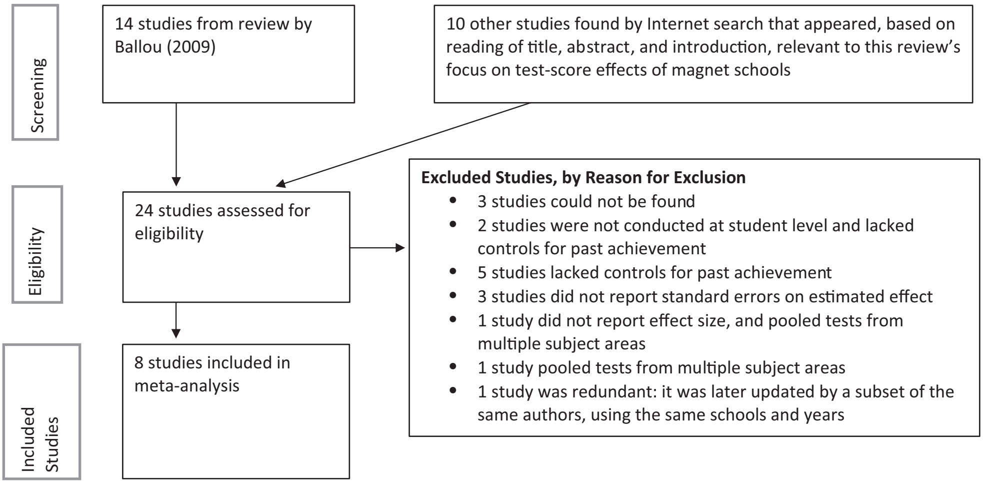

Figure 20.3 Description of the Process through which Magnet School Papers Were Selected for the Meta-Analysis.

Table 20.1 Description of Magnet School Mathematics and Reading Achievement Studies Included in the Meta-Analysis

Authors	*Year*	*Location*	*Method*	*Student Controls?*	*Beginning Year*	*End Year*	*Grade Spans Covered*
Gamoran	1996	National Study (NELS)	HLM, Value-Added	Yes	1988	1990	H
Ballou	2007	Southern District	Lottery, Value-Added	Yes	2000	2004	M (Math only)
Betts et al.	2006	San Diego	Lottery, Value-Added	Yes	2002	2004	E, M, H, A
Yu et al.	2005	Maryland	Matching, Value-Added	Yes	2002	2003	E
Hastings et al.	2012	Low-income urban school district	Lottery, IV	Yes	2006	2009	A
Wang et al.	2017	5 school districts across 4 states	Propensity Score, Value Added	Yes	2011	2013	A
Betts et al.	2015	10 districts	Difference in Difference Value-Added	Yes	2003	2010	E
Bifulco et al.	2009	Connecticut	Value-Added	Yes	2002	2007	M, H, A

Notes: The grade spans studied are listed in the final column with abbreviations as follows: E – Elementary, M – Middle, H – High, A – Multiple grade spans combined, which includes elementary, middle and high school grades for all studies except Bifulco et al., which combines middle and high school grades. In the main meta-analysis, the study estimate covering the most grade spans was used. Thus, for the Betts et al. (2006) and Bifulco et al. (2009) reports, the "All" grade span estimate was used. "Beginning Year," "End Year" = spring of given school year.

examined magnet schools for one specific grade span. In a few cases, a study obtained separate estimates of the relation between attending a magnet school and academic achievement for more than one grade span. And in a few other cases, the paper itself obtained an overall estimate for all of the grades studied across multiple grade spans. For our main analysis, we used the most aggregated estimate from a given study. But in other analyses for individual grade spans, such as elementary schools, we also included results for specific grades.

In three cases, a study produced two or more estimates for one or more grade spans. For example, Ballou (2007) provided separate estimates for two types of middle school magnets in a southern district, describing the types as "academic" and "nonacademic." In a second example, Bifulco, Cobb, and Bell (2009) provided four estimates of magnet school effect sizes, two each for middle and high schools, in each case distinguishing between applicants from central city areas versus suburban areas. Yu, Li, Tompkins, and Modarresi (2005) produced estimated effects of attending seven individual magnet schools. These detailed analyses by type of school or type of applicant are creative and potentially important if they find meaningful differences. From the standpoint of meta-analysis, though, they do create a risk: They can bias overall estimated effect sizes toward the estimates from the studies that contribute the greatest number of estimates, because they obtain unduly high weights in calculating the average in equation (1).

To address this issue, we performed a meta-analysis of the two Ballou (2007) estimates, and for the Bifulco et al. (2009) estimates, we performed an overall middle school meta-analysis, along with a high school analysis and a combined middle/high school analysis. Similarly, we performed a meta-analysis of the effects reported by Yu et al. (2005) for seven individual magnet schools in Maryland. We then used these summary effect sizes in our overall meta-analysis.

Table 20.1 shows details about the studies, including the grade spans investigated (with E, M, H, and A representing "elementary school," "middle school," "high school," and "all," or multiple grade span estimates). For our main analysis we used the estimate from each study that included the greatest number of magnet schools or magnet school applicants possible. Specifically, this means that the main meta-analysis used the A grade estimates for the Betts, Rice, Zau, Tang, and Koedel (2006) and Bifulco et al. (2009) studies. Later, we performed separate analyses that focused on results for the four grade spans separately.

To put all the estimated effects on a similar footing, we transformed them into predicted change in test scores after one year of enrollment at a magnet school. Some studies reported magnet school effects in this way, but a number reported test scores two or more years after either applying to a magnet school or entering a magnet school. We translated these estimates to a one-year effect by dividing by the number of years over which the cumulative effect was estimated. Specifically, for the Gamoran (1996) and Bifulco et al. (2009) studies, we divided two-year effects by two; for the Ballou (2007) study, we divided the overall effect by 1.5 given evidence that some students were enrolled for two years and others for one year; and for the Betts et al. (2006) and Wang, Schweig, and Herman (2017) studies, we divided three-year effects by three.

Results of the Meta-Analysis

We begin with the analysis of the full sample of all studies, using the estimate for each study that includes the widest number of grade spans available. Figures 20.4 and 20.5 show the effect sizes from each study for math and reading respectively, along with the overall estimated effect size resulting from the meta-analysis, which is displayed at the bottom. These effect sizes refer to the estimated change in achievement associated with enrollment for one year at a magnet school. The figures use horizontal lines to indicate the 95 percent confidence interval for each estimate. The rightmost column shows the weight attributed to each study. (The size of each square is proportional to these weights.) The diamond at the bottom of each figure illustrates the overall estimated effect size, with the width of the diamond indicating the 95 percent confidence interval.

In Figure 20.4 we see that the meta-analysis produced an overall effect size of 0.026 for math achievement, which is significantly different from zero at the 5 percent level (p=0.040). The diamond at the bottom shows this overall effect and the confidence interval. The effect sizes from the individual studies contributing to this overall estimate appear above it, and they vary. The topmost estimate, by Hastings, Neilson, and Zimmerman (2012), is much more positive than the others, but

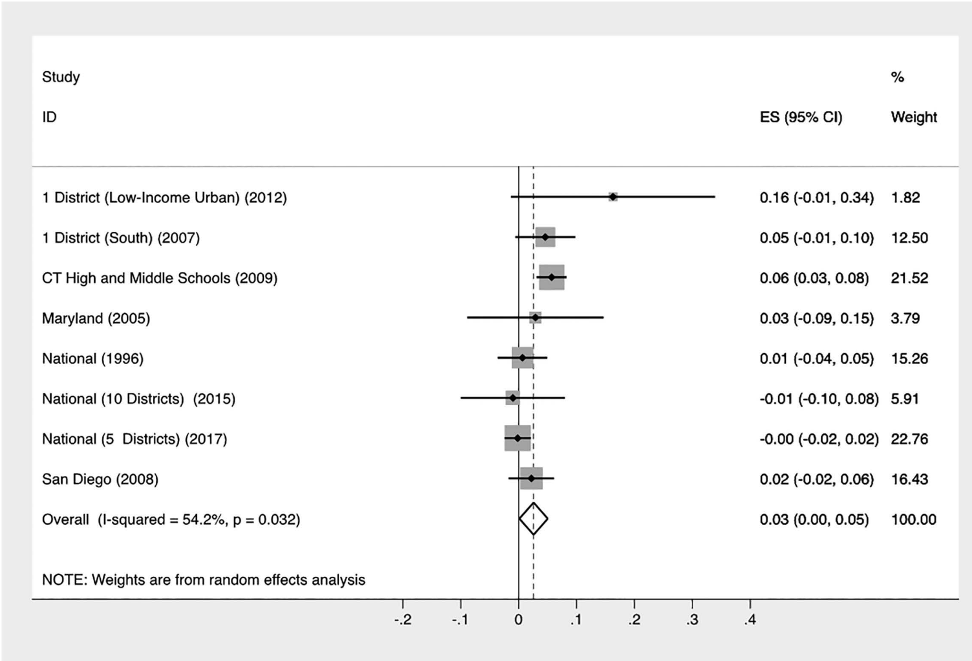

Figure 20.4 Meta-Analysis of the Effect Size of Magnet Schools on Mathematics Achievement.

Note: Weights are from random-effects meta-analysis. The horizontal lines show the 95 percent confidence interval, which is also indicated in the second column from the right. The rightmost column shows the weight ascribed to each study, with the size of the square proportional to these weights. The overall effect size estimate is shown at the bottom.

due to its relatively low precision, as shown by the large confidence interval, it has a quite small weight.

A valid question is whether the variations across the studies reflect true variation in the relative performance of magnet schools compared to traditional public schools in different locations—or simply the fact that the estimates are uncertain, as shown by the 95 percent confidence intervals plotted for each study. The bottom of the left-hand column in the figure reproduces the I^2 statistic that measures the share of the variation that reflects genuine variation across studies, along with the *p* value of a test for homogeneous effects across studies. The *p* values are close to zero, indicating that achievement changes related to enrolling in magnet schools vary considerably across locations. In other words, we cannot attribute most of the variation to statistical "noise." For math, as shown by the I^2 statistic at the bottom of the figure, about 54 percent of the variation appears to be real.

Figure 20.5 shows corresponding results for reading achievement. As displayed at the bottom of the graph, the mean overall effect size resulting from the meta-analysis is 0.036 and is statistically significant at the 5 percent level (p=0.033). All but two of the studies had a positive estimated effect, but there were variations. As shown by the I^2 statistic at the bottom of the figure, about 67 percent of the observed variation across studies is likely to be genuine, rather than due to statistical uncertainty.

Considering the mathematics and reading results together, several things stand out. First, there are relatively few studies that provided the information required for inclusion in the meta-analysis. Second, there is variation across the studies, and the more precise estimates have a bigger weight. Combining these insights, one can infer that future studies, to the extent that they produce different effect sizes that are estimated precisely, could change the conclusions we draw here. Thus, we

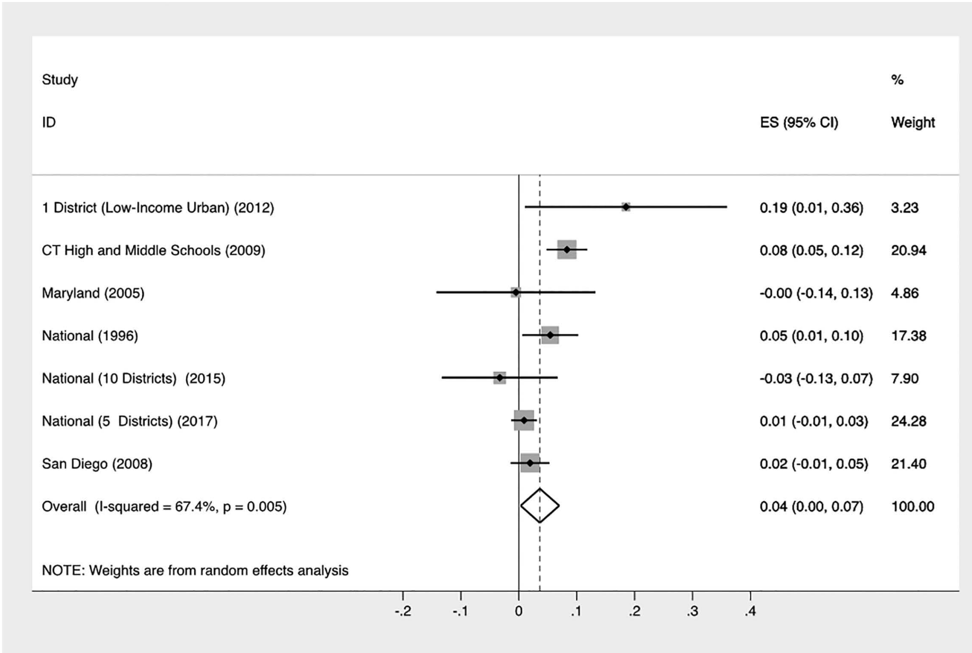

Figure 20.5 Meta-Analysis of the Effect Size of Magnet Schools on Reading Achievement.

Note: Weights are from random-effects meta-analysis. The horizontal lines show the 95 percent confidence interval, which is also indicated in the second column from the right. The rightmost column shows the weight ascribed to each study, with the size of the square proportional to these weights. The overall effect size estimate is shown at the bottom.

conclude most tentatively that on average there is overall a significant gain in math and in reading, and that new studies could alter the conclusions from this quite small set of studies materially.

As a robustness check, one can compare the results of meta-analyses that focus on the lottery-based studies and the correlational studies. Even though all of the latter studies take the important step of controlling for students' past achievement, these studies are theoretically more subject both to omitted variable bias and to endogeneity bias than the lottery-based studies. As an exploration into the possibility of bias in the nonlottery studies, we repeated the meta-analyses for math and reading, once using the lottery studies and once using the other studies. We use the word "exploration" advisedly because, with only three lottery-based math studies and two lottery-based reading studies, the precision of the various subgroup estimates is low. Two findings emerged. First, all four effect sizes for reading/math lottery/nonlottery combinations were positive, but none was significant at the 5 percent level, likely due to the small sample sizes of studies. And second, the lottery-based studies produced overall effect sizes about twice the size of those produced using the nonlottery studies, but these differences were not statistically significant. For reading, the effect sizes were 0.079 and 0.034 for the lottery-based and nonlottery-based samples, respectively. For math the corresponding effect sizes were 0.038 and 0.019.

Due to the lack of precision, we can say nothing with any confidence about whether the nonlottery-based studies are biased, but it is notable that for both subjects the lottery-based studies produced higher average estimates. Increasing the sample of lottery-based studies is obviously important, as it may provide more concrete evidence of whether the correlational studies are biased downward. The effects included in the two main meta-analyses in Figures 20.4 and 20.5 come from studies of magnet schools operating in various grade spans. A natural question is whether there is evidence of varying changes in achievement depending on whether magnet schools operate at the elementary, middle, or high school levels.

Table 20.2 shows effect sizes related to magnet school enrollment for each grade span, along with their standard errors. To provide a point of comparison, the first row repeats the results from the overall analyses shown in Figures 20.4 and 20.5, which used one estimate from each study. The subsequent rows show the results of meta-analyses for studies of magnet schools at the elementary, middle, and high school levels, and finally, studies which included multiple grade spans. Readers can find out which studies contributed estimates to a given grade span by consulting the final column of Table 20.1.

Of the eight new effect size estimates for grade spans, six are statistically insignificant and two, for high school math and reading, are statistically significant. Both of these are positive and somewhat larger than the effect sizes from the earlier analysis of the full sample of all studies. Comparing the effect sizes across elementary, middle, and high school studies, the coefficients tend to rise at the higher-grade spans, but this pattern may not be meaningful given the imprecise nature of some of these estimates.

None of the variation across the elementary school studies is necessarily a sign of genuine differences in effects across locations, as the elementary school studies produced very similar estimates relative to their standard errors. But for the other three combinations of grade spans, most of the time the I^2 statistic was above 50 percent, indicating that most of the variation was genuine and not due to statistical noise.

Overall, in our study, there are four estimated effect sizes that are statistically different from zero. Another way of interpreting the size of these effects is to translate them into the number of percentile points by which a student would be predicted to move after enrolling in a magnet school for one year. Table 20.3 shows the results. For a student beginning the year at the 50th percentile—that is, in the "middle of the pack"—the percentile ranking after one year at a magnet school in mathematics, based on the sample of full studies, is predicted to rise by 1.0 points to 51.0. For reading, the predicted gain is 1.4 percentile points, that is, a rise from 50 to 51.4. For the two high school estimates, a year at a magnet is associated with a rise of 1.7 and 2.0 percentile points for mathematics

Table 20.2 Effect Size of Magnet Schools on One-Year Mathematics and Reading, Overall and by Subgroup

Sample of Studies by Grade Span	*Mathematics*		*Reading*	
	Effect Size (s.e.)	*Number of Studies [I^2]*	*Effect Size (s.e.)*	*Number of Studies [I^2]*
Full Sample of All Studies	0.026* (0.012)	8 [54.2%]	0.036* (0.017)	7 [67.4%]
Elementary School	-0.006 (0.026)	3 [0.0%]	-0.028 (0.026)	3 [0.0%]
Middle School	0.022 (0.031)	3 [77.2%]	0.066 (0.043)	2 [69.9%]
High School	0.043* (0.019)	3 [53.8%]	0.051** (0.014)	3 [8.7%]
Studies of Multiple Grade Spans	0.032 (0.020)	4 [77.6%]	0.044 (0.023)	4 [80.8%]

Note: ** Effect size is significantly different from zero at the 1% level. * Effect size is significantly different from zero at the 5% level. The I^2 statistic reports the percentage of the variation across studies that is estimated to represent true variation rather than statistical noise.

Table 20.3 Predicted Change in a Student's Percentile Ranking after One Year at a Magnet School, for Four Cases with Significant Effects

Sample of Studies by Grade Span	*Subject Area*	*Predicted Percentile if Started Year at 50th Percentile*	*Predicted Percentile if Started Year at 25th Percentile*
Full Sample of All Studies	Mathematics	51.0	25.8
Full Sample of All Studies	Reading	51.4	26.2
High School	Mathematics	51.7	26.4
High School	Reading	52.0	26.6

Note: The table uses the four statistically significant effect sizes from Table 20.2 to predict the percentile at which a magnet student would be after one year, having started at either the 50th or 25th percentiles, based on a normal distribution.

and reading respectively. If the student had instead started the school year at the 25th percentile, the predicted gains are slightly lower, ranging between a low gain of 0.8 percentile points in mathematics from the full sample to a high gain of 1.6 percentile points for reading in high school. These are modest predicted changes, but obviously over a number of years of enrollment, the cumulative gains from enrolling in a magnet school could be quite meaningful.

Summary and Potential Future Directions for Magnet School Research

Ballou's (2009) review of the literature suggested quite mixed outcomes. As stated above, our examination of the literature using meta-analysis means that we are unable to include some of the older papers in his review; at the same time, we are able to include several new rigorous studies. Our graphs of effect sizes across studies, along with formal statistical tests of the hypothesis that all studies produce the same effect sizes, show considerable variation. These results reinforce Ballou's earlier conclusion that the literature was quite mixed. We find significant effects of enrolling in a magnet school on mathematics and reading overall. The predicted effects are modest, roughly a gain of one percentile point per year, but they could grow across several years of enrollment. Our supporting analyses of magnet schools by the grade span(s) studies suggest significant positive effects for math and reading at the high school level but not in earlier grades.

What is perhaps most striking is how small the literature on magnet school achievement is. For example, Betts and Tang (2019, Table 1) perform a review of the charter school literature on achievement and find over 90 estimates each of math and reading achievement effects, compared to the 13 and 12 estimates (for math and reading, respectively) of magnet school effects for various grade spans in the present study. This gap is remarkable given that the enrollment shares of charter and magnet schools are very similar, as shown in Figure 20.1. Charter schools have attracted more attention, perhaps because they are viewed by some as potentially more innovative than magnet schools, and perhaps because they are more controversial. But the gap in the extent of our knowledge of the achievement impacts is large, suggesting that magnet schools are being under-studied.

Thus, a first major need is for more quantitative studies of the effect of magnet schools on achievement.

A second major need is for more studies that estimate and test for differential effects across student groups. Of the eight papers included in our analysis, three of the most recent (Bifulco et al., 2009; Betts, Kitmitto, Levin, Bos, & Eaton, 2015; Wang et al., 2017) provide separate estimates for different groups of students, but the groupings of students differ somewhat across papers. We lack enough subsample estimates to draw firm conclusions at this point.

A third major need is to explore more fully the characteristics of magnet schools, including the curricular themes and pedagogical innovations selected for these schools. Likewise, because the impact of a magnet school will always be estimated relative to traditional public schools, it is important to learn more about the schools and neighborhoods from which magnet schools draw nonresident students, and whether these characteristics contribute to estimated impacts.

A number of studies have taken some preliminary steps to distinguish magnet schools and/or the schools or areas students leave when enrolling in a magnet school. Ballou (2007) distinguishes between academic and nonacademic magnet schools and finds some evidence that the latter have a larger effect on achievement. Bifulco et al. (2009) distinguish between students coming from urban versus suburban areas and find mixed evidence on which group gains more, but with effects being usually larger for central-city students. Betts et al. (2015) report effect sizes separately across districts, but they do not attempt to model differences in terms of curricular themes due to the limited sample of schools. They also distinguish between traditional magnet schools in less affluent neighborhoods and "destination" magnet schools in more affluent areas that attempt to attract students from less affluent neighborhoods.

The paper that comes the closest to the ideal of providing detailed school-by-school estimates and school context is Wang et al. (2017), who publish effect sizes for each of the 24 magnet schools in their sample. Although they do not list curricular themes or teaching approaches with this information, they do test whether the effect sizes are related to the fidelity and scope of implementation of the magnet school's plans, based on a reading of external evaluation materials. They find some evidence, especially for math, that the schools that perform better adhere to the implementation plans and have resource teachers who work with most teachers.

Three challenges to enacting this research agenda of understanding distinctions among magnet schools are: 1) the large number of curricular themes and pedagogical approaches at magnet schools, 2) difficulties in gathering data on these areas, and 3) the relatively small number of magnet schools included in a typical study. One way for researchers to work together to improve the body of knowledge about what matters in magnet school design would be to estimate magnet school effects for each school in the sample, and then to list these in publications or online appendices, along with basic school characteristics (e.g., the general curricular theme). For the most part, social scientists are wise to avoid trying to explain which aspects of a type of school matters when their sample is too small. However, meta-analysis and meta-regression techniques are designed specifically for such situations because they report school-by-school impacts along with themes or pedagogical approaches (even if one study lacked the power to identify what mattered). In other words, 20 underpowered studies of school characteristics and impacts would on their own not be conclusive, but statistically significant findings could emerge when combining the results of the studies through meta-analysis and meta-regression.

Note

1 The authors thank Dale Ballou for providing additional data related to one of his studies, and Mark Glander of the U.S. Department of Education for useful conversations regarding state submissions of magnet school counts for the Common Core of Data. Matthew Springer and Ann Primus provided very helpful editorial advice.

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