Magnet Schools and Student Achievement

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Abstract

We estimate the impact of attending a magnet school on student achievement for a mid-sized Southern district, using admissions lotteries to sort students into "treatment" and "control" groups. We find a positive magnet school effect on mathematics achievement until we add controls for student demographics and prior achievement. This suggests that despite random assignment in the lotteries, treatment and control groups differ with respect to student characteristics that have an independent impact on achievement. The most likely explanation is differential patterns of attrition among lottery winners and losers.

Evaluations of magnet schools have suffered from methodological limitations. Some merely compare the achievement of magnet and non-magnet students without controls for initial differences in achievement (Poppell & Hague, 2001). As a result, the comparison fails to inform about differences in educational value-added across the two types of schools. Problems with data quality are common. A recent evaluation of a federal program providing financial support to magnet schools, conducted by American Institutes for Research, found that academic progress in magnet schools was no greater than in a comparison set of regular public schools, once controls were introduced for changes in the demographic composition of schools (Christenson et al., 2003). However, the study had to rely on school-level data rather than longitudinal student-level records. Moreover, frequent changes to state testing regimes meant that test scores were unavailable for one-third to one-half of the schools in the study, depending on the year.

The most pervasive problem, however, is one that confronts virtually all efforts to assess school performance. Students (more precisely, their parents) seek out particular schools by residential decisions (in the case of neighborhood schools) or special application (in the case of magnet schools or other forms of school choice). This means that the effect of the school on student learning is likely to be confounded with the effect of parental and family characteristics that influence both where students go to school and how much they learn. For example, if the students who seek admission to magnet schools have parents with above-average education and commitment to their children's education, then it is unclear how much of these students' subsequent success should be attributed to the quality of the magnet schools and how much to parental influences that would have contributed to higher achievement, regardless of the school attended.

Although this example suggests the magnet school effect will be biased upward (magnet schools receive credit for parental influences that enhance achievement), in principle the bias could go in either direction. If parents seek magnet schools for children whose performance in regular public schools is slipping, the magnet school may appear to be ineffective if judged against regular schools serving students whose performance is exhibiting no decline. Even controlling for prior student achievement will not eliminate this bias if parents make such decisions on a forward-looking basis, taking into account factors that have not shown up yet in achievement scores.

Fortunately, the way school choice programs are frequently conducted provides a way of disentangling the effect of the chosen school from the influence of factors that led to that choice. Many school choice programs are oversubscribed: the number of applicants exceeds the number of vacancies. Admissions are conducted by lottery. Any differences between the students selected for a magnet school and the unsuccessful participants in the lottery therefore arise solely by chance. This makes unsuccessful participants a natural "control group" for purposes of measuring the effectiveness of the magnet schools. As in a randomized experiment with treatment and control groups, the impact of the treatment (in this case, the difference in quality of education) can be ascertained by comparing achievement of successful applicants who enroll in magnet schools with the achievement of unsuccessful participants who enroll in zoned schools.

This approach is widely regarded as the most promising way to measure the effect of school choice programs (Howell and Peterson, 2002), though to date most applications have examined the effect of school vouchers and other programs involving private school choice. One exception is a recent study of Chicago magnet schools by Cullen, Jacob and

Leavitt (2003), who concluded there are no 'differences between winners and losers on traditional outcomes measures such as test scores or school attendance" (p. 23). Whether this finding generalizes to other school systems and grade levels (the Chicago study considered only high school students) is unknown.

In this study we examine the effect of magnet schools in a mid-sized Southern city. The district serves 70,000 students, of whom half are eligible for free or reduced price lunch. The district is racially mixed, serving 40% White students, 48% Black, and 8% Hispanic students during the 2003-04 school year. The district operates magnet schools at all levels: elementary, middle school, and high school. However, the assessment program that provides our achievement data tests students in grades 3-8. As a result, we focus on academic outcomes for students who apply to one or more of the district's magnet middle schools via lotteries.[1]

We rely on standardized test results from 1998-99 through 2002-03. During most of this period the district operated five magnet middle schools. Because regular lotteries were held in only four of these schools, our focus is on those four. One of the four was an academic magnet school, with admissions requirements in the form of minimum GPA and standardized test scores. The other three magnets had no special entrance requirements.

The question we ask is whether attending a magnet school raises standardized test results. This is not the only, or even the most important, impact a magnet school might have. In future work we intend to look at the influence of magnet schools on a variety of

[1] Magnet school lotteries for elementary schools are generally conducted for students entering kindergarten or first grade. Because achievement testing begins in grade 3, this means no data on comparative outcomes are available until years later. This makes it quite difficult to study the effectiveness of these schools within the sample period for which we have data.

student behaviors. The focus of this paper, however, is on achievement in reading and mathematics.

Lottery Participation and Magnet School Enrollment

Table 1 contains descriptive statistics on lottery participation and magnet middle school enrollment for our sample. There are 6163 sample observations on 2747 different students from academic years 1999-2000 through 2002-2003. Students who entered middle school at the beginning of this period might be observed as many as four times. The first sample cohort were fourth graders in 1998-1999, the earliest year for which we have achievement data. Thus for this and subsequent cohorts we are able to control for achievement prior to middle school.[2] Given the structure of the sample, there are more observations of fifth graders than sixth graders, more of sixth than seventh, etc.

The first column of Table 1 shows the number of lottery applications filed by these students. All students in the sample participated in at least one middle school magnet lottery. Many entered multiple lotteries. (Such students are counted in the row for each school to which they applied.) The largest number of applications were submitted to the district's academic middle school magnet. There were (and are) entrance requirements to this school in the form of grades and test scores. Students not meeting the entrance requirement were not permitted to file an application. The second

[2] We do so by restricting the sample to students who attended district schools as fourth graders. Some lottery participants reside in the district but attend private schools as fourth graders. These students are likely to remain in private schools if the lottery outcome is not to their liking. Not only do we lack data on prior achievement for this group, but their inclusion in the sample exacerbates the problem of attrition among lottery losers. We follow the convention of excluding them from the analysis (see Cullen, Jacob and ….). Thus our inferences are limited to the effectiveness of magnet schools among lottery participants enrolled in the district as fourth graders. This is the population of greatest interest.

of the non-academic magnets began conducting lotteries for the 2000-2001 year. As a result, there are virtually no eighth graders in the sample at this school.[3]

Reading across the rows of Table 1 shows what happened to participants. In the non-academic magnets, roughly two-thirds to three-fourths of participants are accepted for fifth grade (column 2).[4] Additional students are accepted from a wait list for grades six or even later, which is why the acceptance rates tend to rise as one moves down the table. The academic magnet has the lowest acceptance rate, barely over one-half.

As one would expect, the proportion of those accepted who enroll is much higher at the academic magnet, for which there are fewer close substitutes in the district. Take-up rates at the other magnets are between a quarter and a half. One reason is that students are accepted into more than one magnet school. It is common for a quarter to a third of accepted students to enroll in another magnet (column 4). However, there are significant percentages of students who won a place in a magnet school through the lottery but turned it down in order to attend another district school (column 5).

Because acceptance rates at the non-academic magnets are high and many students apply to more than one, the number of applicants who cannot get into at least one of these schools is relatively small. Of the 2747 students in the sample, only 649 failed to secure admission as fifth graders to at least one magnet school. Most of those who failed had applied to the academic magnet. Only 181 failures arose among students who applied only to non-academic magnets. Because unsuccessful applicants represent

[3] The three eighth graders shown in Table 1 represent either coding errors or students who skipped a grade.

[4] These are students who were either admitted outright on lottery day or who occupied a position on the wait list that was reached by August 15, just prior to the start of the school year. Students whose wait list positions were reached by August 15 of the ensuing year were treated as "delayed winners" for admission the following year (i.e., as sixth graders). In some years the district admitted students to magnet schools after the August 15 date, so that a few students enroll before they have technically "won" admission, according to our criteria.

the natural "control group" for our analysis of magnet school effectiveness, these small numbers result in imprecise estimates.

The final column of Table 1 shows that some students enroll in magnet schools when they have not participated in the lottery. There are several reasons this can occur. Some students are admitted to magnet schools on the basis of sibling priority. When schools are not oversubscribed, no lottery is held. Schools that reach the end of their wait list can accept "walk ins" who apply too late for the lottery. Finally, it was not always straightforward to determine from district records which students had participated in lotteries. Some of the anomalous cases in the final column could represent coding errors. Because these students did not participate in the lottery for the school they attended, they fall into neither our treatment group (a subset of the winners) nor the control group (losers). Accordingly, we drop these observations from the estimation sample.

Randomization, self-selection, and instrumental variables

Because our strategy for identifying magnet school effects rests on the randomization of students into treatment and control groups via the lottery, we first investigate whether lottery outcomes indeed appear random. We have no prior reason to suspect this is not so. Lotteries are not conducted by individual schools, which might have an incentive to screen out certain students, but are run centrally and publicly. Nonetheless, there is a possibility that well-connected parents can manipulate the lottery to secure preferred outcomes. We look for evidence of this or other distortions by comparing characteristics of lottery winners to losers. (For this purpose, winners are students who were accepted in time for admission in grade 5.) Our seven variables are race (specifically, whether the student was black), low income (eligibility for free or reduced-price lunch), disability (including special ed), English-language learners, gender, and prior (4th grade) scores in reading and mathematics. Fifteen lotteries were held for four magnet schools over the sample period. This gives rise to a total of 105 comparisons (15 times 7) which we conducted using t-tests of differences in means. Of the 105 cases, in 19 differences were statistically significant with p-values of .10 or smaller. This is more than would be expected to occur by chance, if the trials were independent and the data normally distributed. However, neither of these conditions is met. In particular, there is substantial positive correlation between prior scores in reading and math (.59), reflected in the fact that in two of the three instances when winners differed from losers with respect to one of these scores, they differed as well with respect to the other. There are also high correlations between black and poverty, and between these two variables and prior scores (in the negative direction). To take this into account, we estimated linear probability models in which lottery outcomes were regressed on all seven student characteristics. In five of the fifteen lotteries the F statistic for the joint significance of the regressors had p-values below .10. None of these instances occurred in the academic magnet, contrary to expectation if they were the result of well-connected parents pulling strings. Nor is there a consistent pattern to the results. Where significant differences exist, lottery losers are more likely to be poor and black, but they are also more likely to have high test scores.

To conclude, we find no evidence of gross tampering or manipulation of the lotteries to serve private interests or create showcase schools. However, because there are significant differences between winners and losers in some instances, we add these student characteristics as controls to our model to enhance efficiency and reduce any bias resulting from imperfectly randomized lotteries.

As shown in Table 1, many lottery winners do not accept the places offered them in magnet schools. Self-selection at this stage can cause magnet school enrollees to differ from lottery losers. Accordingly, we have compared characteristics of students who enrolled in magnet schools in grade 5 to students accepted but not enrolling. There is clear evidence of non-random self-selection. Of 28 comparisons (7 characteristics in 4 magnet schools), there were significant differences at the 10 percent level in 15. Black winners were much more likely to enroll in the non-academic magnets than white winners. Enrollees in two of the three non-academic magnets had lower 4th grade reading scores. In the academic magnet, enrollees had higher 4th grade reading scores.

We extended the analysis into grade 6, asking whether students who returned for a second year in their magnet school differed from those who left. Self-selection on demographic variables is less important here, perhaps because so much of this has taken place the year before. However, in two of the four magnet schools (including the academic magnet), there was positive selection on fifth grade achievement tests (and positive though insignificant differences in the other two schools). Students are more likely to leave schools in which they are not doing as well as their peers. In some instances this may be at the urging of the school.[5]

In the presence of self-selection of enrollees, differences between lottery losers and lottery winners who actually attend magnet schools yield biased estimates of magnet school effectiveness. Our solution is to use lottery outcomes as instrumental variables for

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[5] Although there are no formal criteria for retention at the academic magnet, students who are doing poorly are discouraged from returning.

magnet school enrollment. If lottery outcomes are random (or random conditional on student characteristics also included in the model), the result will be asymptotically unbiased estimates of the effect of magnet schools on students who accept positions offered through the lottery. This approach has been used to study educational choice in contexts similar to ours, including private school voucher programs (Howell and Peterson, 2002) and charter schools (Hoxby and Rockoff, 2005). The effect estimated is known as the effect of treatment on the treated. It differs from the average gain that randomly selected district students would experience, were they to attend these magnet schools, for two reasons. First, lottery participants are a self-selected group. Among other possibilities, students seeking admission to magnet schools may profit more from attending them than the average student. In addition, many students offered places in magnet schools do not accept them. Presumably this would also be true of many lottery losers had they won, insofar as losers differ only by chance from winners. The instrumental variables estimator is silent on the potential effect of magnet schools among the students who are not induced to attend them even when they win the lottery.

Sample Attrition

Students who leave the district drop out of the data set. Like the decision on the part of lottery winners to enroll elsewhere in the system, sample attrition can introduce differences between treatment and control groups. Unlike the student who enrolls elsewhere in the district, however, students who leave the sample no longer generate usable data. Thus, the method of instrumental variables is not available, making sample attrition potentially a graver problem.

Table 2 shows that attrition is greatest between grades 4 and 5: the transition to middle school appears to be a time when parents are searching for alternatives to their zoned school in the district. Almost a fifth of the students who fail to obtain a place in the academic magnet leave the district, six percentage points higher than the rate among winners.[6] In the non-academic magnets the difference between winners and losers either is not pronounced or is greater among the former. Between grades 5 and 6, attrition rates fall by about half, with losers more likely to leave than winners.

Attrition per se does not bias our estimates of magnet school effects. Even different rates of attrition for winners and losers will not result in bias if attrition occurs at random within each group. However, higher rates of attrition among losers suggest that losers leave the system for reasons that do not apply to winners. To take the most obvious possibility, families may be more likely to pursue external options if their child fails to win a place in a magnet school. If this effect is not constant across families, but varies with the degree of their commitment to education, their resources, or other factors that have an independent bearing on achievement, these differences in attrition will bias our comparison of treatment to control groups. We cannot investigate whether exiting losers differ from exiting winners with respect to such unobservable factors as commitment to education, but we can explore this issue by examining differences in observable characteristics, including test scores prior to their departure from the system.

As we have already seen, winners and losers are not perfectly balanced with respect to observable characteristics: randomization does not assure that the two groups are exactly alike. For this reason, winners leaving the district may differ from losers who

[6] The definition of winners includes students who reach the top of the waiting list by the start of the fifth grade school year (in the case of fifth graders), or by the start of the sixth grade school year (in the case of sixth, seventh, or eighth graders).

leave even if exits occur randomly within the two groups. To find out whether attrition introduces additional differences between winners and losers not already present due to the haphazardness of the lottery, we estimate the following equation:

(1) $X_{igys} = b_0 + b_1 \text{ Winner}_{igys} + b_2 \text{ Leaver}_{igys} + b_3 \text{ (Winner*Leaver)}_{igys} + e_{igys}$.

The dependent variable is a characteristic (e.g., black) of student i in grade g and year y who participated in the lottery for school s.[7] The mean of X among winners who stay in the system is $b_0 + b_1$ while the mean among winners who leave is $b_0 + b_1 + b_2 + b_3$. The difference between them, $b_2 + b_3$, represents the effect attrition has on the make-up of the winners who remain in the data set. The corresponding difference for losers is b_2. Thus, the extent to which attrition introduces differences between winners who stay and losers who stay is b_3.

We estimate (1) for the seven student characteristics mentioned above (black, ESL, handicapped, female, poverty, fourth grade math score, fourth grade reading score). For grades six through eight, we add math and reading scores in the previous grade. (The latter two variables are the only source of variation in X over g, y, or s.) This yields a total of 294 combinations of g,y, and s, or 294 values of b_3. Of these, 54 are statistically significant at 10 percent or better.[8] The number of instances in which attrition resulted in significant differences between winners and losers who remained is much smaller, however. First, in sixteen cases, attrition differentials were in the opposite direction from differences that had arisen (presumably by chance) between winners and losers. In these cases, attrition fortuitously offset imbalances in the lottery. Partly as a result of this offset, in only five of these sixteen instances were there statistically significant

[7] We distinguish observations by year as well as grade because different cohorts participated in different lotteries in which the make-up of participants could vary.
[8] As earlier, this number must be considered in light of the dependencies among these variables.

differences between winners and losers who stayed. In many of the remaining cases, the number of students exiting was too slight for attrition differentials to have a large influence on the make-up of treatment and control groups. Of these thirty-eight cases, in only eight were winners who stayed significantly different from losers who stayed.

While the influence of attrition on treatment and control groups does not appear large, the pattern of attrition indicates that it is not simply equivalent to more random variation. Most noteworthy are the results for test scores. Of the 22 significant coefficients on prior test scores, 20 run in the same direction: the difference between test scores of lottery losers exiting the district and losers remaining in the district is lower (more negative) than the analogous difference among lottery winners. This has a straightforward interpretation: if low test scores prompt families to consider schooling alternatives, those who have won the lottery are more likely to think they have already found a satisfactory option within the district, while those who lost in the lottery will tend to look outside the system. This means the control group loses more of its weakest students than does the treatment group, introducing a bias against finding a treatment effect. Controlling for fourth grade test scores will diminish this bias, though it may not completely eliminate it.

Models

We estimate magnet school effectiveness by regressing student achievement in math and in reading on a set of dummy variables indicating which of the magnet middle schools a student attended, using a sample restricted to participants in one or more of the lotteries for these schools. The equation also includes binary variables identifying all lotteries a student entered (e.g., the 2001 lottery for the academic magnet), as there was random assignment only within lotteries. As we have seen, students often do not attend schools in which they have won a place. Because these decisions may be correlated with other factors influencing achievement, we use lottery outcomes as instrumental variables to predict the school of attendance. Some specifications of the model also include student demographic characteristics and measures of prior achievement. As just noted, these variables also help to reduce bias caused by different patterns of attrition among lottery winners and losers.

The dependent variable is the student's scale score on the Tennesssee Comprehensive Assessment of Progress (TCAP). During these years the state used the Terra Nova series developed by CTB/McGraw-Hill. Tests were administered in grades 3 through 8. To control for changes in the difficulty of the exam from one year to the next, the model also includes dummy variables for every grade/year combination. The coefficients on these variables represent the average performance of lottery participants attending non-magnet schools in a particular grade and year. The full equation, including student demographics and prior achievement, is therefore

$$(2) \quad Y_{igys} = b_0 + \Sigma_{m\epsilon M}\, b_m\, \pi_{mi} + \Sigma_y \Sigma_g\, b_{gy}\, \delta_{gy} + \Sigma_s\, b_s\, \mu_{isgy} + \mathbf{b_4}\, \mathbf{X_i} + e_{igys}\,.$$

M denotes the set of magnet school lotteries, with indicators π_{mi} denoting which of them student i entered. δ_{gy} are the year/grade indicators and μ_{isgy} the indicators of which magnet school (if any) student i actually attended in grade g, year y.

In equation (1) magnet schools are assumed to have a constant effect across grades and years: whatever the mean achievement in comparison schools, attending a magnet school shifts it by a constant magnitude. We also estimate two variants of this model that relax this assumption. In the first variant, the dummy variable for magnet school attendance is replaced by a variable measuring the number of years a student has spent in a magnet school. This is the appropriate specification if the impact of attending a magnet school grows at a constant rate over time. Thus, a student at the end of seventh grade, who has spent three years in a magnet school, will exhibit three times the magnet school effect of a student in grade 5.[9] The second variant allows for magnet school effects to vary in every grade without imposing the restriction of linear growth.

Each variant of the model requires modification of the instruments. For equation (2), the instruments used to predict the magnet school indicators are dummy variables denoting whether a student was a magnet school winner. For students in grade 5, this means admission by August 15 of the school year, as noted above. For students in grades 6-8, this includes "delayed winners" who were admitted by August 15 of their sixth grade year. For the cumulative effects model, our instruments are potential years spent in a magnet school, calculated as the number of years elapsed since winning the lottery (or zero, for non-winners). For delayed winners, the clock starts in grade 6. Finally, for the model with separate magnet school effects in each grade, our instruments are the interaction of indicators for winning with indicators for current grade. First-stage regressions also include the other exogenous variables in equation (2).

None of these models includes indicators of the specific non-magnet schools attended. With more than 50 schools in the district offering instruction in grade 5, the number of lottery participants in most was too small to distinguish the impact of one non-magnet middle school from another. This means magnet schools are being compared to an undifferentiated group of other middle schools attended by lottery participants. This is

[9] In this specification, the cumulative impact of magnet school attendance persists undiminished for students who have since returned to a regular district school.

not the same as a representative group of district middle schools. The composition of this group depends on which students are attracted to the lottery.[10] On the reasonable assumption that alternative schools vary in quality, this induces heterogeneity in treatment effects even if the impact of a magnet school relative to a given alternative is constant across students. For this reason, if no other, we are estimating the effect of treatment on the treated rather than the impact that attending a magnet school would have on a randomly selected student from the district.[11]

Results

Table 3 displays descriptive statistics for the variables in our models. Statistics are for fifth graders only and do not reflect changes in enrollment that result from intradistrict mobility and from students leaving the district, as discussed above. Except in the academic magnet, blacks comprise 40 percent or more of lottery participants and enrollees, mirroring the racial composition of the district (approximately 45 percent black, varying by year). Mean fourth grade scores are higher than in the district as a

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[10] Because our models include indicators of the lotteries a student enters, we control for variation in the alternative set among lotteries. For example, if the non-magnet alternatives of the students who applied to the academic magnet are better than those of the students applying to the non-academic magnets, this difference is picked up by indicators of participation in the academic magnet lotteries. However, these variables control only for differences among lotteries and not for the difference between lottery participants as a group and the rest of the district.

[11] To the extent that student demographic variables convey information about the set of non-magnet alternatives, the inclusion of these variables also implicitly changes the standard against which magnet schools are being measured. This will not occur if lottery winners and losers are perfectly balanced on every demographic variable. Given the use of lottery outcomes as instruments, such balance breaks the link between the quality of the non-magnet alternatives and the predicted probability of attending a magnet school. Absent this kind of balance, a variable like black will pick up racial differences in the quality of non-magnet alternatives just as it will pick up any other race-related factor bearing on achievement. While it remains true that including student demographics means that the estimated magnet school effects more nearly approximate the results that would obtain if lottery outcomes were perfectly balanced, it seems somewhat arbitrary to weight the set of non-magnet alternatives in this fashion.

whole, indicating positive selection into magnets.[12] In the academic magnet, of course, they are higher still. Students eligible for the free and reduced-price lunch program (low income), who make up half of the district overall, are underrepresented in magnet middle schools. A small number of students whom we have coded as "losers" enrolled in these magnet schools as fifth graders, principally because the district in some years accepted students from wait lists after the August 15 deadline.

With three variants of the model, there are 23 endogenous enrollment variables (including the 15 school by grade interactions in the third variant). In the 23 first-stage regressions, the F-statistic for the joint significance of the instrumental variables never falls below 40, suggesting finite sample bias should be negligible (Bound, Jaeger, and Baker, 1995).

Table 4 contains results for mathematics. In column 1, all four dummy variables for the magnet middle schools have positive coefficients significant at the 10 percent level. The F statistic for their joint significance has a p-value of only .39, however, due to the high positive correlations between these coefficients.[13] Levels of significance fall in the model of cumulative effects. About half of the school by grade interactions are significant in column 3 (and all but one are positive), but the pattern is erratic. Many coefficients are estimated imprecisely. Comparing column 1 with 2 and 3, it appears there is a positive magnet school effect, but it does not increase with time spent in a magnet school as one would expect if magnet schools had higher value added.

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[12] Between 1999 and 2002, average math scores for the district fluctuated between 620 and 625. Mean reading scores ranged from 631 to 637.

[13] The smallest of the pairwise correlations among these variables is .79. The reason is that the four magnet schools largely share the same control group of lottery losers, given the tendency of students to participate in multiple lotteries. Sampling error in this group tends to affect all four coefficients in the same direction.

Introducing student-level covariates (columns 4 and 5) reduces standard errors, as anticipated, but coefficients fall even more. When we control for 4th grade scores (column 4), three of the four magnet school effects remain significant at the 10 percent level. In column 5 we add controls for other student characteristics. Although these variables add little overall explanatory power (the R^2 rises from .61 to .63), all but one of the coefficients drop again. Only one of the magnet school effects is significant, though all point estimates remain positive.

By contrast, in reading there is much less evidence of a positive magnet school effect to begin with. The consequences of introducing student-level covariates are very similar, in that the coefficients generally fall by more than the standard errors (Table 5). Without covariates, only one of the non-academic magnets has a positive and significant coefficient (though the standard error is large). With student covariates, only the academic magnet has a significant, positive coefficient. As with math, there is no evidence of a steady cumulative gain from attending a magnet school.

Implications

Given the large standard errors on our estimated magnet school effects, it is difficult to draw any firm conclusions. In most specifications of our model, estimated effects on math and reading achievement are positive but so imprecise that we cannot rule out that the true effects are zero or even negative. This is so of all variants of the model for reading. In mathematics there are more statistically significant results, but only so long as we have not controlled for student demographics, family income, and prior achievement.

The importance of student characteristics implies that we cannot rely solely on randomization via the lottery to identify magnet school effects. The exact reason is less apparent. We have noted three reasons that student characteristics may matter: (1) Notwithstanding random assignment, treatment and control groups are not balanced on all characteristics (equivalently, our instrumental variables are not orthogonal to student covariates); (2) Attrition introduces systematic differences between lottery winners and losers; (3) Controlling for student covariates implicitly alters the weights given to different schools attended by students in the comparison sample.

The first of these refers to sampling error in the lottery: by chance, treatment and control groups end up looking different. Controlling for student characteristics should therefore improve efficiency (as it did). But if this is the only problem, we would not expect controlling for student covariates to have a systematic impact on the estimated magnet school effects. The fact that these effects uniformly declined in math and in reading indicates there must be something more at work.[14]

Attrition bias is one candidate. Were higher-scoring students more likely to leave the system when they lose the lottery than when they win, this would depress mean achievement in the comparison group and make magnet schools look better than they are. Adding student covariates to the model would then diminish this bias the estimated magnet school effect. However, our analysis of attrition shows the opposite pattern: as noted earlier, the test scores of lottery losers exiting the district, relative to lottery losers remaining in the district, are lower than the corresponding difference among lottery

[14] Instrumental variables estimators are also subject to finite sample bias, a second consequence of chance correlation between instruments and the error term in a sample. However, given the large F statistics in the first stage regression, this would not appear to be the culprit.

winners. This suggests that the problem lies rather in the initial assignment to treatment and control groups by the lotteries and in the small number of students in the control groups, though more work is needed to confirm this conjecture.

The inclusion of student covariates in a model raises questions about what we are controlling for. Given that race, income, etc. do not affect achievement per se, such variables are proxies for direct but unobservable educational inputs. Normally these are thought to be family or home inputs, but they could also be school-level inputs correlated with these variables, such as teacher quality. Suppose there is such a correlation between school quality and family income. If higher-income lottery losers are more likely to leave the system than lower-income losers (and if no such difference emerges among winners, who are content with the lottery outcomes), then low quality schools will be overrepresented in the comparison group, magnifying the magnet school effect. Controlling for family income reverses this.

Summary

We have estimated the impact of attending a magnet school on student achievement for a mid-sized Southern district, using admissions lotteries to sort students into “treatment” and “control” groups. We find a positive impact on mathematics achievement until we add controls for student demographics and prior achievement. This suggests that despite random assignment in the lotteries, treatment and control groups differ with respect to student characteristics that have an independent impact on achievement. Further work is required (and planned) to explain this phenomenon.

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Table 1: Magnet School Lottery Participation and Enrollment

	Participants observed at this grade level:	Participants accepted for this grade (% of all participants):		Accepted, enrolled in this magnet school (% of those accepted)		Accepted, enrolled in another magnet school (% of participants; % of accepted)			Accepted, not enrolled in any magnet (% of all participants)		Not accepted, not enrolled in any magnet (% of participants)		Non-participants enrolled in this magnet (% of enrollment)	
	(1)	(2)		(3)		(4)			(5)		(6)		(7)	
School	Grade 5 (four cohorts)													
Academic Magnet	1510	736	49%	597	81%	25	2%	3%	114	8%	469	31%	1	0%
Non-Academic Magnets														
#1	933	580	62%	216	37%	188	20%	32%	176	19%	115	12%	19	9%
#2	996	587	59%	275	47%	136	14%	23%	176	18%	131	13%	66	24%
#3	1032	746	72%	354	47%	148	14%	20%	244	24%	75	7%	2	1%
Non-Academic Total	2961	1913	65%	845	44%	472	16%	25%	596	20%	321	11%	87	10%
	Grade 6 (three cohorts)													
Academic Magnet	1080	550	51%	415	75%	21	2%	4%	114	11%	320	30%	1	0%
Non-Academic Magnets														
#1	629	438	70%	142	32%	124	20%	28%	172	27%	50	8%	17	12%
#2	636	388	61%	169	44%	64	10%	16%	155	24%	59	9%	56	33%
#3	709	573	81%	210	37%	118	17%	21%	239	34%	25	4%	2	1%
Non-Academic Total	1974	1399	71%	521	37%	306	16%	22%	566	29%	134	7%	75	14%
	Grade 7 (two cohorts)													
Academic Magnet	608	334	55%	227	68%	25	4%	7%	82	13%	103	17%	0	0%
Non-Academic Magnets														
#1	344	254	74%	58	23%	97	28%	38%	99	29%	23	7%	18	31%
#2	286	169	59%	58	34%	59	21%	35%	52	18%	52	18%	17	29%
#3	432	329	76%	98	30%	90	21%	27%	141	33%	24	6%	3	3%
Non-Academic Total	1062	752	71%	214	28%	246	23%	33%	292	27%	99	9%	38	18%
	Grade 8 (one cohort)													
Academic Magnet	296	159	54%	108	68%	6	2%	4%	45	15%	47	16%	0	0%

Non-Academic Magnets														
#1	181	147	81%	51	35%	46	25%	31%	50	28%	12	7%	3	6%
#2	3	1	33%	0	0%	1	33%		0	0%	0	0%	9	n.a.
#3	199	155	78%	39	25%	37	19%	24%	79	40%	10	5%	2	5%
Non-Academic Total	383	303	79%	90	30%	84	22%	28%	129	34%	22	6%	14	16%
Total over all grades & cohorts														
Academic Magnet	3494	1779	51%	1347	76%	77	2%	4%	355	10%	939	27%	2	0%
Non-Academic Magnets														
#1	2087	1419	68%	467	33%	455	22%	32%	497	24%	200	10%	57	12%
#2	1921	1145	60%	502	44%	260	14%	23%	383	20%	242	13%	148	29%
#3	2372	1803	76%	701	39%	393	17%	22%	703	30%	134	6%	9	1%
Non-Academic Total	6380	4367	68%	1670	38%	1108	17%	25%	1583	25%	576	9%	214	13%

Data from academic years 1999-2000 through 2002-2003. Sample comprises all students enrolled in the system in 4th grade who
participated in a lottery for one or more of the four middle schools shown.

Table 2: Departures from the District (Sample Attrition) by Lottery Outcome and Grade

	Participants in the system in grade 4		Attrition by Grade 5		Attrition by Grade 6		Attrition by Grade 7		Attrition by Grade 8	
	(1)		(2)		(3)		(4)		(5)	
	Winners	Losers	Winners	Losers	Winners	Losers	Winners	Losers	Winners	Losers
School	Number of Students									
Academic Magnet	847	959	113	183	36	58	34	32	7	5
Non-Academic Magnets										
#1	673	395	97	40	41	30	23	17	10	1
#2	660	465	78	51	25	28	25	13	n.a.	n.a.
#3	857	304	115	18	61	14	38	5	10	0
	As Percentage of Students from the Previous Grade									
Academic Magnet			13.3%	19.1%	4.9%	7.5%	4.9%	4.5%	1.1%	0.7%
Non-Academic Magnets										
#1			14.4%	10.1%	7.1%	8.5%	4.3%	5.2%	2.0%	0.3%
#2			11.8%	11.0%	4.3%	6.8%	4.5%	3.4%	n.a.	n.a.
#3			13.4%	5.9%	8.2%	4.9%	5.6%	1.8%	1.6%	0.0%

Data from academic years 1999-2000 through 2002-2003. Sample comprises all students enrolled in the system in 4th grade who participated in a lottery for one or more of the four middle schools shown.

Table 3: Student Demographic Characteristics and Prior Achievement: Fifth Grade Samples

Schools	Lottery Status	Enrollment	N	Black	ESL	Low Income	Female	Disability	Mean 4th Grade Math	Mean 4th Grade Reading
Academic Magnet	loser	does not enroll	745	23.6%	7.2%	15.6%	54.0%	10.2%	666.4	684.3
	loser	enrolls	29	27.6%	13.8%	6.9%	58.6%	13.8%	678.0	692.3
	winner	does not enroll	139	22.3%	5.0%	25.9%	43.2%	10.8%	663.9	677.8
	winner	enrolls	597	18.6%	10.1%	11.7%	53.6%	14.4%	668.2	683.6
Non-Academic Magnets										
#1	loser	does not enroll	348	43.7%	8.9%	29.1%	56.0%	11.8%	648.5	666.1
	loser	enrolls	5	20.0%	0.0%	0.0%	20.0%	0.0%	671.4	679.2
	winner	does not enroll	364	43.7%	7.7%	33.5%	54.7%	10.7%	646.2	660.8
	winner	enrolls	216	51.9%	3.7%	34.9%	53.2%	6.5%	646.6	663.3
#2	loser	does not enroll	390	45.1%	10.0%	27.9%	48.7%	9.7%	651.6	663.7
	loser	enrolls	19	36.8%	15.8%	15.8%	36.8%	10.5%	652.7	661.5
	winner	does not enroll	312	39.1%	8.0%	28.8%	43.9%	13.1%	653.0	666.9
	winner	enrolls	275	51.6%	12.7%	20.7%	45.8%	8.4%	649.6	660.6
#3	loser	does not enroll	274	55.5%	7.3%	35.4%	65.0%	8.8%	643.2	658.6
	loser	enrolls	12	66.7%	0.0%	41.7%	75.0%	0.0%	635.6	642.9
	winner	does not enroll	392	41.8%	3.3%	30.2%	55.6%	11.2%	640.7	657.0
	winner	enrolls	354	66.4%	3.1%	36.4%	59.3%	9.9%	629.5	648.0

Data from academic years 1999-2000 through 2002-2003. Sample comprises all students enrolled in the system in 4th grade who participated in a lottery for one or more of the four middle schools shown.

Table 4: Magnet School Effects in Mathematics

Independent Variables	(1)	(2)	(3)	(4)	(5)
Current Enrollment					
Academic Magnet	5.57 (2.91)			4.45* (2.43)	2.62 (2.22)
Non-Academic Magnets					
#1	16.94* (0.38)			14.87* (7.79)	4.25 (6.65)
#2	15.33* (8.81)			9.09 (7.28)	12.30 *(7.19)
#3	21.34* (11.21)			17.72* (9.36)	11.13 (8.40)
Cumulative Exposure					
Academic Magnet		2.18 (1.56)			
Non-Academic Magnets					
#1		5.93 (3.99)			
#2		6.07 (3.89)			
#3		7.69* (4.09)			
Current Enrollment, By Grade					
Academic Magnet					
Grade 5					
Grade 6			5.31* (3.19)		
Grade 7			8.24 (5.80)		
Grade 8			11.29* (6.56)		
Non-Academic Magnets			-6.49 (9.87)		
#1 - grade 5					
#1 - grade 6			10.50 (8.84)		
#1 - grade 7			30.37** (19.14)		
#1 - grade 8			34.44 (22.55)		
#2 - grade 5			4.10 (19.38)		
#2 - grade 6			8.65 (5.64)		
#2 - grade 7			27.53** (13.78)		
#3 - grade 5			30.84* (16.72)		
#3 - grade 6			13.06 (10.53)		
#3 - grade 7			37.16** (15.38)		
#3 - grade 8			36.98* (20.41)		
Student Characteristics			6.63 (29.90)		
4th grade math				.50*** (.01)	.46*** (.01)
4th grade reading				.22*** (.01)	.19*** (.01)
Black					-7.86*** (1.18)

ESL	6.63*** (1.38)
Female	-1.63 (1.38)
Low Income	-4.29*** (.99)
Disability	2.20** (1.07)

* (**) (***) Significant at 10% (5%) (1%)
All models included controls for lotteries in which students participated and year by grade interactions.

Table 5: Magnet School Effects in Reading

Independent Variables	(1)	(2)	(3)	(4)	(5)
Current Enrollment					
Academic Magnet	4.36 (2.78)			5.46** (2.18)	3.33* (2.01)
Non-Academic Magnets					
#1	15.98* (8.34)			9.51 (6.49)	3.32 (5.96)
#2	-5.15 (8.88)			-3.57 (6.90)	-7.88 (6.48)
#3	10.53 (10.65)			5.84 (8.38)	-2.29 (7.59)
Cumulative Exposure					
Academic Magnet		2.18 (1.99)			
Non-Academic Magnets					
#1		7.32** (3.67)			
#2		-.73 (3.75)			
#3		3.33 (3.89)			
Current Enrollment, By Grade					
Academic Magnet					
Grade 5			3.16 (3.01)		
Grade 6			8.89 (5.52)		
Grade 7			6.02 (6.24)		
Grade 8			-2.31 (9.38)		
Non-Academic Magnets					
#1 - grade 5			-8.06 (8.30)		
#1 - grade 6			6.66 (13.38)		
#1 - grade 7			1.90 (21.34)		
#1 - grade 8			-19.63 (18.46)		
#2 - grade 5			11.65 (8.14)		
#2 - grade 6			25.34* (13.00)		
#2 - grade 7			29.80* (15.55)		
#3 - grade 5			9.79 (9.98)		
#3 - grade 6			23.65* (11.52)		
#3 - grade 7			8.94 (19.37)		
#3 - grade 8			-3.81 (28.53)		
Student Characteristics					
4th grade math				.12*** (.01)	.19*** (.01)
4th grade reading				.59*** (.01)	.46*** (.01)
Black					-7.86*** (1.18)

ESL	6.63*** (1.38)
Female	-1.63*** (.68)
Low Income	-4.20*** (.99)
Disability	2.20 (1.67)

* (**) (***) Significant at 10% (5%) (1%)
All models included controls for lotteries in which students participated and year by grade interactions.