The Effect of Charter Schools on Achievement and Behavior of Non-Charter Students:

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March 30, 2008

Abstract: Proponents of charter schools claim that through generating competition for students charters provide incentives for non-charter public schools to provide more effort towards improving student performance. However, theoretically it is unclear whether public schools respond to charter schools, if at all. In this paper, I look at how charter schools affect student achievement and behavior in nearby regular public schools using data from an anonymous large urban school district. In addition to analyzing students' behavior along with test scores, I utilize an instrumental variables strategy to address endogenous charter location. My instruments use variation in the characteristics of pre-existing building stock near public schools. Most charter schools need rentable space of sufficient size to a large number of students but not too large as to be paying excessive rent. Unlike school fixed-effects, my IV strategy accounts for endogenous location of charter schools based off of time-varying characteristics of public schools or neighborhoods. My results show that when charter school penetration increases, elementary schools suffer statistically significant reductions in test scores. These drops are particularly strong for mathematics. However, test scores appear to improve after just two years. This is consistent with a story of temporary disruptions generated by charter schools as resources and teachers are removed from the public school or re-allocated within schools to respond to the charter school's entry. Discipline appears to follow a similar pattern while attendance results are generally not statistically significant. For middle and high school students, while charters appear to have no statistically significant impact on test scores or attendance, I find statistically significant and large improvements in discipline. Thus, while initially charter schools appear to be disruptive to public school students, over time the charter impacts become positive.

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1 Introduction

Charter schools have become one of the most controversial issues in education today. Despite this controversy, since 1997 the number of charter schools in the US has increased more than fivefold, and the number of students has more than doubled since 1999, as is shown in Figure 1. Today, over a million students attend charter schools. In some states, the charter population is a substantial portion of the total student population. For example, ten percent of students in Arizona attend charter schools[2].

Charter schools are public schools that are given autonomy from local school districts and are subject to fewer regulations than regular public schools. Generally, enrollment in charters is voluntary and public schools lose some funding if a student leaves for a charter. Proponents of charters have argued that this threat of losing students should induce public schools to improve student outcomes. However it is theoretically unclear whether this is true and only a handful of papers have looked at the empirical evidence of how charter schools affect students in traditional public schools using individual data (Bifulco and Ladd 2006, Sass 2006, Booker, Gilpatric, Gronberg and Jansen 2004).

There are a few mechanisms through which charter schools may affect traditional public schools. The most commonly cited is a competition effect. When a charter school enrolls a student usually they get a set amount of money from the chartering authority, be it the state government, a university, or a local school district. Almost always, some portion of this funding would have gone to the public school the student would have attended otherwise and thus there is a financial incentive for public schools to prevent students from attending charter schools. In addition, public schools may wish to prevent students from leaving if they can be closed down for low

[2]Author's calculation from data provided by Center for Education Reform and National Center for Education Statistics, US Department of Education.

enrollment. If these two incentives spur public school teachers and administrators to increase effort and efficiency, then charters would exert a positive competition effect on public schools. On the other hand, if charters are pulling too many students from one school, districts may ‘give-up’ and reallocate funding towards other schools. In addition, some theoretical work by Cardon (2003) suggests that if there are capacity constraints on charters then public schools may not respond to charter competition. Indeed, if public schools are overcrowded, they may welcome the charter schools, since they would serve as a release valve.

Even without an explicit response from the school district, charters can impact non-charter public schools. For example, if charters pull enough students out of a public school, the principal may have to reduce faculty and staff. While this may be good in the long-run, in the short-term it could reduce morale while generating confusion and uncertainty. In addition, the sudden loss of funding from a charter opening nearby could force the school to temporarily make drastic changes, creating a period of disruption in the school’s ability to function well.

Another mechanism is through changes in peer composition. In most cases, though there are some exceptions, previous research has found that charter students tend to have lower income and are more likely to be racial minorities than non-charter students (Hanushek, Kain, Rivkin and Branch 2007, Imberman 2007, Bifulco and Ladd 2006, Sass 2006). In addition, Christensen (2007) finds charter schools report fewer behavioral problems with students then traditional public schools and Imberman (2007) shows that students tend to select into charters based on worsening discipline and falling test scores. Indeed, changes in peer composition was found to be true in schools in California (Booker, Zimmer and Buddin 2005). Thus, it is possible that by attracting lower (or in some cases better) performing students, charter schools may change how peer-effects mechanisms operate in non-charter schools (Cooley 2006, Hoxby and Weingarth 2005, Angrist and Lang 2004, Hanushek, Kain, Markman and

Rivkin 2003, Zimmerman 2003, Sacerdote 2001)[3].

Even if we are to abstract away from the mechanism of charter impacts, identifying the effects of charter schools on non-charter students is problematic because both a student's choice of what school to attend and a charter school's choice of where to locate are not random. Thus, any study of charter school impacts on non-charter students must account for these two potential types of selection bias. Previous work has used student fixed-effects to account for endogenous movements of students and school fixed-effects to account for charter location. However, we may be concerned that panel data methods are insufficient for eliminating bias in the charter competition context. For the student's decision, finding a natural experiment or instrument for charter attendance has been difficult. While some researchers have used lotteries for charter entry as a natural experiment (Hoxby and Murarka 2008, Hoxby and Murarka 2006, Hoxby and Rockoff 2004) this strategy does not apply to analyzing charter competition since there are no lotteries for attending public schools near charters and the location of the charter school itself is endogenous. Nonetheless, there are some potential instruments for charter location which would allow us to address the endogenous location problem.

In this paper, I look at how charter schools in an anonymous large urban school district (ALUSD) affect students who remain in traditional public schools. I add to the current literature in a few ways. First, in addition to standard estimates with school and student fixed effects, I provide estimates that use characteristics of the building stock near regular public schools as instruments for charter location. The intuition behind this is that when a charter is started, one of the most restrictive

[3]The standard model of peer effects is the linear-in-means model where the effect is linear in average peer ability. In this model we'd expect non-charter students to improve due to charters drawing out lower performing students. However, recent evidence by Hoxby and Weingarth (2005) suggests that the linear-in-means model is wrong. They find evidence suggesting that a more appropriate model is one where outcomes improve when there are concentrations of students of similar ability. In this case charters would also tend to improve outcomes since they would tighten the distribution of students in non-charters

constraints is finding space available for rent[4]. While the actual level of vacancies is potentially endogenous, I argue that the building stock is a plausibly exogenous source of variation in charter location.

In addition, this paper adds to the existing literature by assessing the effects of charter schools on discipline and attendance of non-charter students in addition to test scores, looking at the dynamic impacts of charter schools on non-charter students, and considering how charter impacts vary by grade level.

Using both levels and value-added frameworks, my IV estimates show consistently negative and statistically significant impacts of charter schools on math and language test scores. Reading scores are weaker, with value-added models statistically insignificant, but of the same sign. These results differ considerably from models using school fixed effects, which show a statistically insignificant relationship in levels and a positive and often statistically significant relationship in value-added models. However, for discipline, the IV estimates show statistically significant improvements in discipline when charter share increases and some weaker evidence of improvements in attendance.

These apparently contradictory results are explained by the existence of heterogenous effects on primary and middle/secondary schools, where the test-score impacts are concentrated in the former and the discipline impacts in the latter. Even so, the test score impacts are counterintuitive, as we would generally expect charter schools to have, at worst, a zero or slightly negative impact on non-charter schools due to funding losses. This issue is, at least in part, explained by the existence of a dynamic impact of charters on non-charter students. I find evidence that while current and once-lagged charter share generate worse test scores for elementary students, twice-lagged charter share generates test score improvements. This suggests that initially charter schools cause test scores to fall, perhaps due to a period of disruption

[4] While some charters purchase land, since relatively little start-up capital is provided by the state or local school districts, most tend to rent their space or use donated space

as a result of funding losses or firing of teachers because of a drop in enrollment, but eventually cause test scores to rise.

The rest of this paper is as follows. Section 2 considers the previous literature and why whether students attend schools near charters and charter location may be endogenous. Section 3 describes the charter schools in ALUSD. Section 4 provides an overview of the ALUSD data. Section 5 outlines my empirical strategy. Section 6 discusses my baseline fixed-effects and IV results. Section 7 looks at heterogenous and dynamic impacts. Section 8 concludes.

2 Literature and Selection

Previous Literature

A substantial amount of research has looked at how charter schools affect student outcomes (Hoxby and Murarka 2008, Booker, Gilpatric, Gronberg and Jansen 2007, Hanushek et al. 2007, Imberman 2007, Bifulco and Ladd 2006, Sass 2006, Hoxby and Rockoff 2004, Zimmer and Buddin 2003). While the estimates of how charter schools affect test scores have been mixed, Imberman (2007) provides evidence of improvements in student discipline and attendance in certain types of charter schools[5]. On the other hand only a handful of papers have considered how charter schools affect non-charter students. Some early work on the topic has used school level data to answer this question. Bettinger (2005) finds little effect of charter schools on public schools while Hoxby (2004) and Holmes, Desimone and Rupp (2003) find positive effects of charter schools on public schools. While these papers were instrumental in starting this line of literature, since all outcome measures are aggregated to the school level it is impossible to tell whether these results are due to charter competition or changes in the student body composition.

[5]See Imberman(2007) for a full discussion of this literature

Recent work on whether charter schools affect non-charter students have turned to individual panel data in order to address concerns regarding changes in composition. In addition, panel data can be used to account for unobserved heterogeneity of students across different levels of charter penetration, as long as the selection of students into schools near or far from charters is based on time-invariant characteristics. Sass (2006) and Booker, et. al. (2004) find that charter schools have positive impacts on non-charter public schools while Bifulco and Ladd (2006) and Buddin and Zimmer (2005) find statistically insignificant impact estimates.

Thus, in general, researchers have found that charter schools have, at worst, no significant effect on non-charter public schools and, at best, a large positive effect. However, despite the systematic results, there are still a number of unanswered questions that remain. First, although researchers have used school fixed-effects to account for the endogenous location decision of charter schools, estimates will be inconsistent if charter schools select their locations based on time-variant characteristics. For example, charters may prefer to locate in areas where schools are on downwards achievement trends so that demand is expected to increase in the future. This may be particularly important in the district I look at since charters often open with a small number of students and then grow for a few years. Second, all of papers listed above only consider how charter schools affect test scores. However, charter schools may have impacts along other dimensions as well. For example, if parents choose to send their children to charters because of discipline and safety problems, as suggested by Imberman (2007) and Weiher and Tedin (2002), then these outcomes in non-charter schools could also be affected by charter schools.

Endogenous Student Movements and Charter Location

One of the largest problems researchers on this topic have faced is how to deal with multiple layers of selection. The first problem is that a parent's choice of school is not random. Thus we may be concerned that parents would select into or out of schools near charters for unobservable reasons that are correlated with student ability and behavior. Perhaps more importantly, it is likely that some parents respond to observed changes in traditional public schools that result from charter competition. For example, let's take for the moment as given that charters generate positive competition effects in non-charter schools. Some parents with high achieving students who planned to send their children to magnet or private schools may now decide to keep their children in their newly improved neighborhood school, thus increasing the estimated charter impact. In order to address this problem researchers have used student level fixed-effects in panel datasets. This will sufficiently correct for student selection if the selection is based on time-invariant characteristics of the students, such as their parents' motivation.

The second problem is that the location of charter schools themselves is not random. There are a number of factors which go into the decision of where a charter school locates including space availability and transportation options, since most charters do not have access to district provided bussing. This is not a problem if these factors are uncorrelated with student and non-charter school characteristics. However, an additional factor which likely plays a large role in the decision of where to locate is the demand for an alternative schooling environment, which would likely be higher in areas with low-performing schools. Indeed, many charters are created through grass roots organizing in a community, often in response to the poor quality of the local schools.

Depending on the nature of this selection, the bias in the charter impact estimates could be positive or negative. If charters locate near low-performing schools based on time-invariant characteristics of the public schools (i.e. the charters locate near schools which have been low performing for many years and have shown little improvement or worsening), then simple OLS regressions would underestimate the effects of charters. Researchers have addressed this type of selection by including school fixed-effects in OLS regressions with student fixed-effects. However, if location is, at least partially, based on time-variant characteristics of non-charter schools then this strategy will not eliminate, and in fact may exacerbate, the bias. One possible way this can occur is if charters locate in areas where performance is worsening on the belief that this will generate higher demand in the future. Since many charters face high startup costs and thus open with few students and expand later, having an anticipated increase in demand could be desirable. Another mechanism for this selection would be if parents and community leaders do not try to start charter schools until they see performance in the public schools worsening. The direction of this type of bias depends on whether the trends are permanent or temporary.

To illustrate this, Figure 2 shows the difference between estimated and actual charter impacts under the two types of trends. Notice that school fixed effects essentially align the mean outcomes for each school. However, they do not align the schools' performance trajectories. Suppose that charter schools do try to locate near public schools that are worsening over time. The first panel shows what happens to the estimated charter impacts if this trend exhibits mean reversion and thus would have reversed had the charter school not opened, i.e. the trend is "temporary." Rather than estimate the charter impact as the deviation from the trend, the estimates with school fixed-effects ignore the trend, over-estimating the impact. The bottom panel shows that if the trend would have continued after the charter opens, then by ignoring the trend, school-FE regressions would under-estimate the charter impacts. More generally, we may also be concerned that charter selection is correlated with other characteristics of the schools and neighborhoods that vary over time and thus would not be accounted for by the fixed-effects estimates.

One way to deal with this potential endogeneity is to find an instrument that is correlated with charter location but uncorrelated with student outcomes except through the charter location itself. I argue that characteristics of the pre-existing stock of buildings, in particular total building space on a property and the location of shopping centers and strip malls, are plausibly exogenous instruments for charter location[6]. The fact that my IV results differ considerably from fixed-effects results, suggest that analyses utilizing school fixed-effects may not remove all bias.

3 Charter Schools in ALUSD

ALUSD was one of the first school districts in the US to face competition from charter schools. Both district and non-district authorized schools began appearing in 1996[7]. Today there are more than fifty charter schools in the county along with over 200 non-charter schools in ALUSD.[8] Figure 3 shows the evolution of charter school growth in and near ALUSD by examining the fraction of enrollment by school type. As of the 2004-2005 school year nearly five percent of public school students in the ALUSD area attended a district charter school while 8.5% attended a non-district charter.[9]

While it may be interesting to differentiate between the effects of district and non-district charters, unfortunately my instrument is too weak for district charters[10] Nonetheless, the most substantial competition is likely to come from the non-district

[6]Charter schools in ALUSD are also commonly found on the property of religious institutions, low-rise office buildings, and office-warehouses. Exploratory analyses, however, only showed shopping centers and strip malls to have a positive and statistically significant impact on nearby charter share when also including building size.

[7]The vast majority of non-district charters are authorized by the state government, but a few are authorized by local universities.

[8] Due to risk of revealing the district, I cannot provide the exact number of schools in ALUSD.

[9]Since I do not know how many students in the non-district charters would have attended ALUSD otherwise, the enrollment totals may overestimate the actual student population in the ALUSD boundaries. However, almost all of the non-district charters in the area are located within the boundaries of ALUSD and thus it is reasonable to assume that most of the students in these schools would have attended ALUSD otherwise.

[10]This is mainly due to two reasons. The first is that some district charters are conversions which were previously regular public schools and thus do not need to search for alternative locations. The second is that, after removing the conversion charters, the number of district charters remaining is less than 20, leaving little variation across regular public schools.

charters since ALUSD loses state aid when a student leaves for these charters but not for district charters. In addition, the local school district cannot control where non-district charters locate, which is important for competitive pressures to be effective. Note that in all of my regressions students in district charters are dropped from the analysis so that we may focus on the impacts on non-charter public schools. I also drop five non-district county-run charters that are residential treatment facilities for substance abusers or juvenile detention centers since enrollment in these is not voluntary.

Table 1 provides summary information on traditional public schools and non-district charters separated by grade-level for the years 1996 - 2004[11]. The first column in each grade-level includes all non-charter schools in ALUSD. Charter students differ substantially from non-charter students. Regardless of whether we look at elementary or middle/high school grades, the patterns are similar. Charter students are generally wealthier and are less likely to have special needs, as shown by the lower rates of limited English proficiency, special education, and gifted & talented. Charters are also attract disproportionately fewer blacks and more Hispanics. The white population of charters, while slightly lower, does not statistically significantly differ from non-charter schools. However, despite the higher wealth status of charter students, their test score performance is lower than non-charter students. This is particularly apparent in elementary grades. Thus, these results suggest that charter students tend to be under-performers who do not have special needs.

4 Data

In this paper I utilize administrative records from an anonymous large urban school district. This dataset includes information on disciplinary infractions warranting an in-school suspension or harsher punishment, attendance, scores from a

[11]Results are qualitatively similar using 1993 - 2004 and 1998 - 2004

nationally norm-referenced examination and a criterion-referenced state examination, grades, course work, and a number of student characteristics. A full accounting of the variables used in this paper with definitions can be found in Appendix Table 1. The data cover the 1993-1994 to 2004-2005 academic years and I am able to follow individual students for as long as they attend school in ALUSD, providing a long time-series on many students. After dropping observations before first grade, with missing data, or of students in charter schools, 55% of students who are first observed in the data prior to ninth grade have at least four observations.

Since not all students take the norm-referenced examination and test data are only available starting in 1998, I generate two samples.[12] I call the first sample the "base sample." This sample is used when analyzing any outcome other than test scores. It includes students in grades 1-12 who were enrolled as of the end of October of each year, since this is when demographic information is collected by the district. The demographic files identify the school a student attends and thus I use this as the student's school for the year. Some observations are excluded due to missing attendance data ($< 0.1\%$), leaving more than 1.2 million observations.[13]

I call the second sample the "test sample," which includes all students in the base sample from 1998-2004 who have scores recorded for the mathematics, reading, and language portions of the norm-referenced examination. If any one of these scores are missing I drop the observation so that all three test scores are analyzed using the same sample. The test is a commonly-used nationally norm-referenced examination and was given to all English-speaking students in grades 1-8 and all students in grades 9-11. This provides wider coverage of grades than previous work on charter schools, since most districts and states do not start testing until third grade and often stop testing by eighth grade. Students who were not proficient enough in English in grades

[12] Norm-referenced examinations are tests which are scaled to match a representative sample of students in the same grade. Some papers use criterion-referenced examinations instead, which are exams where the student's grade is based on a set of standards.

[13] Due to requirements regarding the anonymity of the district, I cannot reveal exact sample sizes.

1-8 took a separate Spanish language exam. While I have data on these exam results, the scores are not directly comparable to those of students taking the English exam.[14] The final test sample includes over 800,000 student-year observations. After creating both samples, I further drop any observations that are missing data on charter share or the instrument, and any students enrolled in district charter schools.

School addresses were derived from the US Department of Education's Common Core of Data. Any missing addresses were filled in using school directories acquired directly from ALUSD. These addresses were then converted to latitude and longitude using the geocoder.us website. If an address could not be matched using geocoder.us then I used Google Earth™ to find the latitude and longitude. Afterwards, distances between schools were derived using the sphdist command in Stata™. Data on local building stock comes from the county appraisal district. Schools were matched to plots with the appropriately sized buildings using ArcGIS™. Economic characteristics and population density of census tracts were obtained from the 2000 Census Summary Files.

Table 2 provides summary statistics for schools that are between the 0^{yh} and 59^{th}, 60^{th} and 74^{th}, 75^{th} and 89^{th}, and 90^{th} and 99^{th} percentiles of charter penetration within two miles. I only show the years 1998 - 2004 since prior to 1998 very few charter schools were operational in ALUSD. Charter penetration is defined the fraction of students who attend schools within 2 miles of and are in grades covered by the school being observed who attend non-district charters. A more detailed description of how this variable is constructed is provided in the results section below. While schools with charters nearby tend to have fewer free-lunch eligible students, they tend to

[14] Twenty-four percent of elementary student-year observations in the base sample have no test score because they take the Spanish language exam, but by the time students reach middle school, almost all are taking the English language exam. In high school, 23% of student-years in the base sample are missing test scores. This is mostly due to students dropping out of school or moving out of the district between October and the testing period in late winter. Some students also are missing test scores due to illness or suspension during the testing period. A complete accounting of data exclusions by year and grade level is provided in the Appendix Tables 2 and 3 of Imberman (2007).

have more at-risk students and recent immigrants. These schools also have more disciplinary infractions and lower attendance rates, though this likely is due to the differences in grade-levels taught.

5 Empirical Strategy

Baseline Model

I begin my outline of the empirical strategy used in this paper by establishing a simple equation of the form

$$y_{igjt} = \alpha + \beta C^d_{jt} + \mathbf{X}_{igjt}\Omega + \mathbf{G}_{gt}\Theta + \epsilon_{igjt} \tag{1}$$

where y_{ijt} is an outcome measure for student i in grade g in school j during academic year t, C^d is the a measure of charter penetration for non-district charters within a radius d of the regular public school j, $\mathbf{X}$ is a set of observable student characteristics, G_{gt} is a set of grade-by-year indicators, and ϵ is an error term. y_{ijt} could be either a level measure of an outcome or a value added measure where the previous year's outcome is subtracted from the current year's outcome. Imberman (2007) shows that in the fixed-effects framework the estimates from the levels and value added models bound a lagged-dependent variable model in expected value. It is straightforward to show that this extends to the two-stage least-squares estimator with fixed effects[15]. ϵ can further be broken down into student and school error components

$$\epsilon_{igjt} = \gamma_{ijgt} + \eta_{jt}. \tag{2}$$

[15]For the 2SLS version, one needs to simply replace $\mathbf{X}'$ in the proof provided in the appendix to Imberman (2007) with $\mathbf{Z}'$ where $\mathbf{Z}$ also includes both $\mathbf{X}$ and the excluded instrument. The rest of the proof follows exactly as in Imberman (2007).

The concern is that both γ_{ijgt} and η_{jt} will be correlated with C^d_{jt} through some unobserved factors.

Student Selection Into Schools

One problem we face is the potential that $cov(\gamma_{ijgt}, C^d_{jt}) \neq 0$, i.e. that something unobservable is driving student selection into schools facing more or less charter competition. The most obvious type of selection is that only certain types of students may leave non-charters for charter schools. As was shown in Table 1, students who attend charter schools appear to differ considerably from ALUSD non-charter students. Thus the loss of these students from schools with a large amount of charter penetration could bias the results. Another type of selection is that students may remain in ALUSD but change schools in response to charter competition. For example, if new charter schools cause nearby public schools to improve, the schools may attract new students who would have attended other ALUSD schools or private schools had the improvements not occurred.

In order to address this problem, I use a student-fixed effects strategy. This strategy has also been used in Bifulco and Ladd (2006), Sass (2006), and Booker et al. (2004).. More precisely, I assume that

$$\gamma_{ijgt} = \lambda_i + \nu_{igjt} \tag{3}$$

where $cov(\lambda_i, C^d_{jt}) \neq 0$ but $cov(\nu_{igjt}, C^d_{jt}) = 0$. Under this assumption we can remove λ from (1) by demeaning the model with respect to the individual as such

$$\tilde{y}_{igjt} = \tilde{\alpha} + \beta\tilde{C}^d_{jt} + \tilde{\mathbf{X}}_{igjt}\Omega + \tilde{\mathbf{G}}_{gt}\Theta + \tilde{\nu}_{igjt} + \tilde{\eta}_{jt}. \tag{4}$$

where $\tilde{B} = B_{ijgt} - \bar{B}_i + \bar{B}$.

Selection of Charter School Location

While the student fixed-effects procedure corrects for student selection under the assumption stated above, if charter location is endogenous then $cov(\tilde{\eta}_{jt}, \tilde{C}^d_{jt}) \neq 0$. For example, we may be concerned that charter schools tend to locate near low-performing public schools or in locations that are economically depressed as demand for charters may be higher in these locations. One way to address this type of selection is to use school fixed-effects as in Bifulco and Ladd (2006), Sass (2006), and Booker et. al. (2004). For this strategy to be valid it must be that

$$\tilde{\eta}_{jt} = \tilde{\zeta}_j + \tilde{\theta}_{jt} \tag{5}$$

where $cov(\tilde{\zeta}_j, \tilde{C}^d_{jt}) \neq 0$ and $cov(\tilde{\theta}_{jt}, \tilde{C}^d_{jt}) = 0$. Under this assumption we can add school indicator dummies to the regression which will eliminate $\tilde{\zeta}_j$. Thus, our regression equation becomes

$$\tilde{y}_{igjt} = \beta \tilde{C}^d_{jt} + \tilde{\mathbf{X}}_{igjt}\Omega + \tilde{\mathbf{G}}_{gt}\Theta + \tilde{\mathbf{S}}_{jt}\Gamma + \tilde{\nu}_{igjt} + \tilde{\theta}_{jt}. \tag{6}$$

where $\mathbf{S}$ is the vector of school indicators. However, if charters select locations based on trends in local school performance, or, similarly, if grass root efforts to create charters are spurred by trends in local schooling conditions, then equation (5) will be incorrect and including school indicators will not correct for selection. One possible solution to this problem is to use an instrumental variables strategy.

Instrumental Variables

I propose using characteristics of building stock near non-charter public schools as an instrument for charter share. The idea behind this instrument is that certain types of buildings are better suited for a school then others. For example, charters are more likely to locate in shopping centers and strip malls, low-rise office buildings and churches then in warehouses or private residences. In addition charter schools are more likely to locate on plots of land with certain amounts of building space. If the building space is too small, then the charter will not have enough space to operate. If it is too large, then much of the space goes unused and the charter is unlikely to be willing to pay the rent premium. Also, since charter schools tend to rent or use donated space as little funding is available to build new structures, the availability of existing building space is particularly important. Since these characteristics are unlikely to be correlated with other factors that could influence student outcomes, I argue that they serve as plausibly exogenous instruments for charter share. I will also provide evidence that the results are robust to tests of instrument validity. My data on building supply comes from the county tax appraisal office and is based on their 1995 tax records. I use the year 1995 to address the potential concern that building supply could be correlated with localized economic trends. Since 1995 is prior to the opening of charters in ALUSD, I avoid concurrent changes in building supply and charter share as local economic conditions vary over time. Later I will provide specification tests that offer evidence that the instruments are not picking up static variation in local economic conditions. After trying multiple combinations of these variables using 1995 data from the county tax appraisal office, the best performing option was to use the number of buildings between 30,000 and 60,000 square feet and the number of shopping centers or strip malls within a specified distance radius. Thus, I use these as my instruments in all models. Thus, the two-stage least-squares model is

$$\tilde{C}^d_{gjt} = \delta_1 \widetilde{Buildings}^d_{jt} \delta_2 \widetilde{ShoppingCenters}^d_{jt} + \tilde{\mathbf{X}}_{igjt}\Omega + \tilde{\mathbf{G}}_{gt}\Theta + \tilde{\nu}_{igjt}. \tag{7}$$

$$\tilde{y}_{igjt} = \widehat{\tilde{C}}^d_{gjt} + \tilde{\mathbf{X}}_{igjt}\Omega + \tilde{\mathbf{G}}_{gt}\Theta + \tilde{\nu}_{igjt}. \tag{8}$$

where $\widetilde{Buildings}_{jt}^{d}$ is the instrument described above demeaned within individuals.

6 Results

Defining Charter Penetration

Before conducting this analysis, one needs a definition of "charter penetration." Early measures of charter penetration were similar to that proposed by Hoxby (2001),. Her measure was whether a school district has over 6% of enrollment in charter schools. But this does not inform us about school level penetration, nor does it necessarily apply to locations other than Michigan where her analysis had been conducted.

There are two issues to consider when measuring charter penetration at the school level. The first is, for a given geographic area, what is the proper measure of charter penetration. Previous work has used the number of charters near a traditional public school and the share of total enrollment in charter schools (Bifulco and Ladd 2006, Sass 2006, Booker et al. 2004, Holmes, DeSimone and Rupp 2003). I use a modification of the second measure which uses enrollment only in the grades covered by the regular public school. I believe this measurement best reflects the pressures that non-charter schools face from charter schools. Thus, I define charter penetration as follows. Define a set of schools within a distance (d) of school j, including j as $J = 1, 2, ..., N_{nc}^{d}, N_{nc}^{d} + 1, N_{nc}^{d} + 2, ..., N_{nc}^{d} + N_{c}^{d}$ where N_{nc}^{d} is the total number of non-charter schools and N_{c}^{d} is the total number of charter schools. Charter penetration is calculated as

$$(9) \qquad ChartPen_{jt}^{d} = \frac{\sum_{g=Gmin_j}^{Gmax_j} \sum_{l=N_{nc}^{d}+1}^{N_{c}^{d}} Enroll_{glt}}{\sum_{g=Gmin_j}^{Gmax_j} \sum_{l=1}^{N_{c}^{d}} Enroll_{glt}}$$

where $Gmin$ and $Gmax$ are the lowest and highest grades, respectively for school j and $Enroll_{gnt}$ is enrollment in grade g, school l and year t. For example, suppose I am measuring charter penetration within one mile of a school, j, that serves grades kindergarten through five. In this case, for the denominator I calculate the total number of students attending schools within one mile (including those in j) who are in grades kindergarten through five. For the numerator, I do the same calculation, but limit only to non-district charter schools. Thus, my charter penetration measure is the fraction of all public school and charter school students in overlapping grades who attend a non-district charter school within a geographic radius around the public school.

The second issue is how wide of a geographic area defines a schools' "market area" within which it would be subject to competitive pressures from charters. A necessary condition for this pressure to exist is that there must at least be the potential for charters to draw students away from regular public schools. While I cannot directly test this potential, I can investigate whether increases in charter enrollment are associated with reductions in enrollment in nearby regular public schools. Table 3 shows results that try to answer this question by running regressions of the form

$$Enroll_{jt} = \alpha + \beta ChartEnroll_{jt}^{d} + \mathbf{X_{jt}}\mathbf{\Psi} + \epsilon_{jt} \tag{10}$$

where $Enroll_{jt}$ is enrollment in a regular public school j at time t, $ChartEnroll_{jt}^{d}$ is enrollment in the specified type of charter school within d miles of the regular public school and X includes year effects and/or school fixed-effects depending on the specification. When school and year fixed effects are added, a clear pattern emerges. The results suggest that an increase in charter enrollment within one mile of 100 students is associated with a loss of twelve students from the local public school. As expected, this number drops when we look at one to two miles, but remains statistically significant at seven students per 100 charter seats. However, for charters opening between two and three miles, there is no statistically significant relationship with regular public school enrollment[16]. This suggests that in ALUSD, any regular public school would likely only be affected by charters which open within two miles of their boundaries. Thus, for the purposes of this paper, I focus my attention on schools where charters open within two miles[17].

OLS and Fixed Effects Estimates

Table 4 shows the results of regressions of charter impacts on non-charter students with and without school fixed-effects. All regressions include student fixed-effects In addition to the specified variables, each regression is demeaned to remove the student fixed-effect and also includes some time-variant student characteristics: free lunch eligibility, reduced price lunch eligibility, whether the student has another economic disadvantage, whether the student is a recent immigrant, whether the student's parents are migrant workers, and grade-by-year indicator variables[18] I consider five outcome measures - the number of disciplinary infractions warranting an in-school suspension or more severe punishment, the attendance rate, standardized test scores in math, reading, and language. The test-score measure I use is the national percentile ranking (NPR) for a commonly used national norm-referenced examination. NPR is the percent of students in a nationally representative sample of test-takers

[16]Regressions using years 1993 - 2004 and 1998 - 2004 provide qualitatively similar results

[17]Previous papers which look at charter impacts on non-charter schools use considerably varying distances. Bifulco and Ladd (2006) and Sass (2006) use 2.5, 5, and 10 miles, while Holmes, Desimone, and Rupp (2003) use distances ranging from 5 to 20 kilometers (3.1 to 12.4 miles) and also use the county as the local education market. Booker, et. al. (2004) use the school district as the local education market. These longer distances are more appropriate in the context of these papers, since their data include many suburban and rural areas where school attendance zones are larger. However, my results do suggest that the proper distance should vary with urbanicity.

[18]A student is defined as having some other economic disadvantaged if he or she satisfies one of the following conditions but does not qualify for free or reduced price lunch - (1) has family income at or below the Federal poverty line, (2) is eligible for TANF or other public assistance, (3) is eligible for programs under Title II of the Job-Training Partnership Act, (4) is eligible for food stamps, or (5) receives a Federal Pell Grant.

who score lower than the observed student. Separate regressions are run for charter share within 1, 1.5, and 2 miles. I also conduct regressions where the dependent variable is left in levels and where the dependent variable is first differenced to generate a value added measure. As mentioned previously, these two model provide bounds in expected value if the true model is a lagged-dependent variable model.

Columns (1) and (3) show the baseline regressions with no school fixed-effects. Note that each coefficient is from a separate regression. We can interpret the estimates as a 10 percentage point increase in charter share generating an impact of the coefficient divided by 10. Thus the coefficient of 0.30 in the first row of column 1 implies that a 10 percentage point increase in charter share within one mile is correlated with an increase in test-scores of 0.03 NPR. The results from these models provide a mixed picture. While most of the estimates are not statistically significant, there is some evidence that reading improves while language scores fall. There are statistically significant results for attendance rates, but they are sometimes negative and sometimes positive. In columns (2) and (4) I add school fixed-effects. These models are similar to those used in the rest of the literature on charter competition. First, for test scores, while the levels models generally have negative point estimates, only math at one mile is statistically significant. On the other hand, the value-added model shows statistically significant improvements in reading and language scores, but not math. Thus, overall, these models provide weak evidence of improving test scores. The point estimates suggest that the improvement is up to 0.52 NPR in reading and 0.8 NPR in language for a 10 percentage point increase in charter share. For discipline we see some evidence of improvement in levels models. The point estimates suggest that disciplinary infractions fall by up to 0.34 per year from a 10 percentage point increase in charter share. However, the value added model estimates are positive and, while not statistically significant, suggest disciplinary infractions rise by up to 0.2 infractions per year. Finally, for attendance, only value-added models at one mile is statistically significant in a positive direction. Thus, the fixed effects estimates provide little evidence of an impact of charter penetration on non-charter students, although there they are weakly suggestive of a positive impact on test scores.

Instrumental Variables Estimates

Tables 5 and 6 show the results of my baseline instrumental variables estimates. In table 6 I provide first-stage and reduced-form results. Note that each regression contains both of the instruments. Thus, each row of column 1 and of column 2 in each panel is a separate regression. For the first stage results, shopping centers and strip malls is always significant at the 1% level. The number of buildings between 30,000 and 60,000 square feet performs better at lower distances, and is generally zero at 2 miles. F-tests show that the instruments are jointly significant at the 1% level in all specifications. Since the first stage appears to be dominated by the shopping centers and strip malls, I later show specification tests using only the buildings variable. The estimates from those regressions are, as expected, less precise but provide similar results to regressions using both instruments. In addition, the reduced form estimates in panel B suggest that both instruments play roles in the identification. The building size variable appears to have a negative effect on test scores. While the estimates in value-added models are generally not statistically significant, there are some statistically significant estimates in level models. The variable appears to have less of an effect on discipline and attendance. Shopping centers and strip-malls appear to improve discipline and attendance but the estimates are not statistically significant for attendance in levels models.

Table 6 shows the baseline IV results. Out of thirty regressions, in all but one case, the IV estimates pass a Sargon-Hansen over-identification test at the 5% level and in only two other cases does it not pass at the 10% level, which provides some evidence that the instruments are valid. For test scores, the IV estimates show a consistently *negative*effect of charter schools on non-charter students in both levels and value-added models. Results for math are the strongest with estimates statistically significant at the 1% or 5% level in all specifications. These results suggest that a 10 percentage point increase in charter share within one mile generates a drop in math scores by 3.0 to 3.9 NPR. At two miles the test score drop is between 1.5 and 2.7 NPR. While this may seem large, the standard deviation of national percentile rankings for math in ALUSD is 27.6, thus these estimates amount to, at most, 0.14 of a standard deviation. Nonetheless, the results are statistically significant and substantial considering the indirect nature of the intervention. One may be concerned that if these impacts compound annually, then the effect could be implausibly large. However, I will show later that this drop in test scores is only temporary. For reading the levels models are statistically significant (though only at the 10% level for one mile) at 1.3 to 1.7 NPR but the value added model, while negative, is not statistically significant. Language scores have statistically significant drops in both levels and value-added between 1.5 and 2.7 NPR at all distance levels. Thus, overall, the results suggest that charter schools generate statistically significant and substantial drops in math and language scores with a considerably weaker impact on reading scores.[19]

Discipline and attendance, on the other hand, appear to be affected in the opposite direction. Discipline impacts are negative (meaning disciplinary infractions fall) and significant at the 1% or 5% level for all models except value-added at 2 miles, which is significant at the 10% level. The results suggest that a 10 percentage point increase in charter share within one mile reduces disciplinary infractions by 0.7 to 0.9, and the charter impacts weaken substantially as distances increase. Attendance rates show some weak improvements. While levels models are positive but not statistically significant, value-added models are marginally significant at the 5% or 10% level.

[19]Since some LEP students in grades 1-8 take a Spanish language version of this exam which is not included in this regression, we may be concerned that this could play a role in the results. Nonetheless, regressions that include NPR from either exam with dummies for taking the Spanish language exam interacted with year dummies show qualitatively similar results.

Taken at face value, the point estimates suggest that a 10 percentage point increase in charter share increases attendance rates by 0.8 to 2.4 percentage points. However, these estimates are relatively imprecise and thus it is more reasonable to look at the 1.5 mile estimates which suggest an increase of only 0.3 to 1.3 percentage points.

In table 7, I provide some specification checks to test potential concerns with the 2SLS results. Column (1) shows the estimates from table 6. One concern is that the shopping centers instrument is driving the estimates. This variable is more likely to be correlated with economic conditions then the building size variable and also likely influences fewer charter school decisions, thus pinpointing much of the variation on a handful of schools. However, it is useful to include this variable as it improves the precision over using the building size instrument on its own. Nonetheless, in columns (2) and (3) I test use only building size as an instrument. The column (2), I only use the number of buildings between 30,000 and 60,000 square feet. In column (3) I also include the number of buildings between 0 and 30,000 sf, 60,000 and 90,000 sf, between 90,000 and 120,000 sf, and greater than 120,000 sf. In both cases the results are qualitatively similar. Most of the estimates for math and language retain their statistical significance. For reading, level estimates lose their significance in column 3, but the reading results were relatively weak in the main regressions, so this is not surprising.

In columns (4) - (6) I add controls for local economic conditions. In column (4) I add a quadratic in the number of commercial properties within the given distance of the non-charter school. This accounts for the possibility that the instruments are picking up variation in residential and commercial zones. In column (5) I include a series of controls for characteristics of each schools' census tract as of the 2000 census. The controls include the fraction of residents who are black, Hispanic, born in another country, have a high-school degree or some college, and have a college degree along with the male 25 years or older labor force participation rate, and the log of annual average household income. Column (6) provides estimates from regressions including fixed effects for each schools' zip code. For math exams, language exams, discipline, and attendance, the results in all three specifications are qualitatively similar to the main results and retain their statistical significance in most cases. For reading exams, the level models lose their statistical significance but keep the same sign, with the exception of the model with zip-code fixed effects at two miles. The value-added models for reading switch signs in some cases, but never become statistically significant. Nonetheless, these specification tests are consistent with the baseline regressions since those are statistically insignificant and relatively close to zero. Thus, overall, the baseline estimates appear to be robust to the specification tests.

7 Heterogeneity and Dynamic Impacts

The results in tables 6 and 7 provide a puzzle in that test scores worsen while discipline and attendance improve. A potential explanation is that there is heterogeneity in how different types of schools are affected by charter penetration. In addition, we may suspect that since attendance and discipline problems are much more common in higher grades, these results could be driven by middle and high schools. Thus, in table 8, I allow the coefficient on charter share to vary by whether a student is in grades 1 - 5 or in grades 6 - 12 (6 - 11 for test regressions)[20]. The instruments are also interacted with the student's grade level to provide additional variation. When I do this, it becomes clear that almost all of the discipline improvements come from the older students. This is not surprising, since primary school students have far

[20]Another specification may be to separate schools by primary and middle/secondary level. However, in ALUSD some primary schools include 6^{th} grade and some do not. In addition, some schools are classified as being combination of primary/middle/secondary. One could also split the sample into grades 1-5 and 6-12. Since this limits the amount of observations which are used in determining the student fixed-effect, this is not ideal. Nonetheless, regressions using this strategy provide estimates that have the same direction with somewhat larger magnitudes.

fewer discipline problems on average. Students in grades 6 - 12 experience an average drop of 1.1 disciplinary infractions from an 10 percentage point increase in charter share within one mile, and between 0.6 and 0.7 infractions within 1.5 miles. At two miles, the estimates are only statistically significant at the 10% level. For attendance rates, the results are not generally statistically significant but the point estimates are consistently negative for grades 1-5 and positive for grades 6 - 12.

What is somewhat surprising is that almost all of the test score reductions are coming from elementary students. Students in grades 1 - 5 show math drops of between 4.5 and 5.8 NPR and language drops of 2.5 to 5.0 NPR. Reading drops are again statistically significant in levels but not value added. For grades 6 - 12, however, the test score estimates vary between positive and negative and are not statistically significant in any model.

Another puzzle the results present is why tests scores drop in non-charter schools. While the schools lose some money and may need to fire teachers in response to lower enrollment, it seems unlikely that these would affect students in perpetuity. However, it is plausible that the loss of funds and teachers could generate temporary disruptions and drops in morale which could lead to a sudden drop in test scores which would subsequently return to a more positive trajectory. To test this, I modify the model as such,

$$\tilde{y}_{igjt} = \tilde{\alpha} + \beta_0 \tilde{C}^d_{j,t} + \beta_1 \tilde{C}^d_{j,t-1} + \beta_2 \tilde{C}^d_{j,t-2} + \tilde{\mathbf{X}}_{igjt}\Omega + \tilde{\mathbf{G}}_{gt}\Theta + \tilde{\nu}_{igjt} + \tilde{\eta}_{jt}. \tag{11}$$

Thus, I incorporate lagged charter share for a student's school into the analysis. If the increase in charter share generates worsening test scores for multiple years, then we should expect both recent and earlier charter shares to have significant negative impacts on test scores. If charter schools generate only temporary drops in test scores then we would expect recent charter impacts to be negative while the impacts of earlier charter shares to be positive. Table 9 provides evidence in support of the latter situation using this model with separate estimates for students in grades 1 - 5 and students in grades 6 - 12. Note that each row in the "estimates" columns refers to a single regression, from which I collect six point estimates. In order to add precision to the estimates, I interact the instruments used in table 8 with year dummies to take advantage of secular increases in charter share over time across the entire district.

While individual point estimates for elementary test scores are imprecise, so we should be wary of accepting them at face value, the results shows a consistent pattern for elementary students. The light-gray boxes highlight estimates that are negative and statistically significant and the dark-gray boxes highlight positive and statistically significant estimates. In the levels models, any statistically significant test score estimate for contemporaneous (t) or once lagged (t - 1) charter share is followed by a consistently positive and often statistically significant estimate for twice-lagged charter share (t - 2). Disciplinary infractions also show this pattern for elementary students. This suggests that even though the current level of charter share has a detrimental effect on test scores and discipline, if the school faced high charter shares previously then test scores and discipline improve. These results are consistent with charter schools generating temporary disruptions which can be detrimental to student outcomes, but lead to improvements in the long-term. I should caution, however, that it is unclear how further lags would affect the estimates, and due to increasing imprecision I cannot use more than two lags with my data.

8 Conclusion

Charter schools have the potential to generate strong incentives for public school administrations and teachers to increase effort and improve student performance. However, losing students can cause disruptive reductions in funding and staff that may worsen student outcomes. Using an instrumental variables strategy to address potentially endogenous charter location and student fixed-effects to address endogenous movement of students across schools, I find evidence suggesting that charter schools have a deleterious impact on math and language exams in non-charter schools. The estimates for reading are also negative, but generally weaker and only statistically significant in levels models. These effects appear to be particularly strong in elementary schools. A 10 percentage point increase in charter share generates a drop in math scores for grades 1 - 5 of between 4.5 and 5.8 national percentile rankings while the drop in language scores is between 2.5 and 5.0 national percentile rankings depending on the distance used to measure charter penetration and whether one uses a levels or value-added model. Math and language scores for grades 6 - 12 are generally negative but statistically insignificant. However, the impacts on elementary students appear to be temporary. While current and once-lagged charter shares for a school generate drops in test-scores, twice-lagged charter shares generate improvements. This suggests that, while non-charter schools may suffer an initial disruption from increasing charter share, over time the impacts of charter schools wane and may induce improvements in non-charter schools. Unfortunately, data limitations prevent me from looking at impacts beyond two lags. Thus, I must leave longer-term impacts to future research.

In addition to test scores, I also look at disciplinary infractions - measured by the number of in-school suspensions or more severe punishments a student incurs over the course of an academic year - and attendance rates. I find relatively strong evidence that discipline in grades 6 - 12 improves when charter penetration increases. In particular, an 10 percentage point increase in charter share within 1.5 miles reduces disciplinary infractions by 0.6 to 0.7 per student per year. Attendance results, while positive for grades 6 - 12, are not generally statistically significant.

When interpreting these estimates, a note of caution is warranted. I am looking only at one school district in this data. Thus, I cannot say what impact charter schools would have on the district as a whole. Since most funding decisions are made by the district and not the schools, it is likely that this paper only picks up a portion of the impacts on non-charter schools. In addition, the instrument used in this analysis pulls out the impacts from schools where charters were induced to pick a location based on building characteristics. Most likely, a charter school would pick a general area where it wants to operate and then decide where to locate within this area based, in part, on suitable buildings. Thus, the local average treatment effect from this instrument is only based on small changes in location from charters which move them closer or farther from a particular school. It does not say what would happen if a charter school was to locate on one side of a city versus another, nor does it provide information into how a large influx of charter schools in a district would affect student outcomes.

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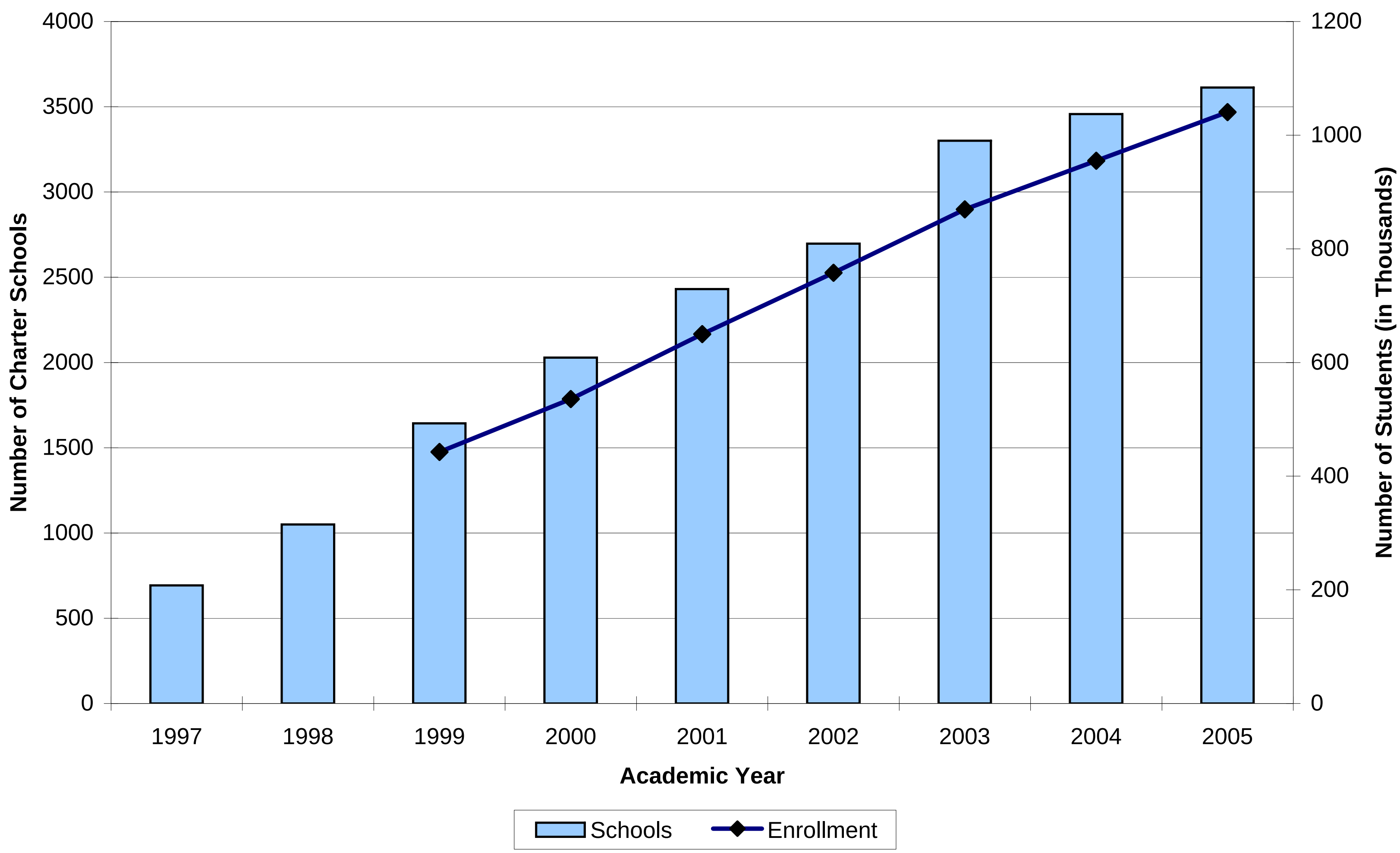
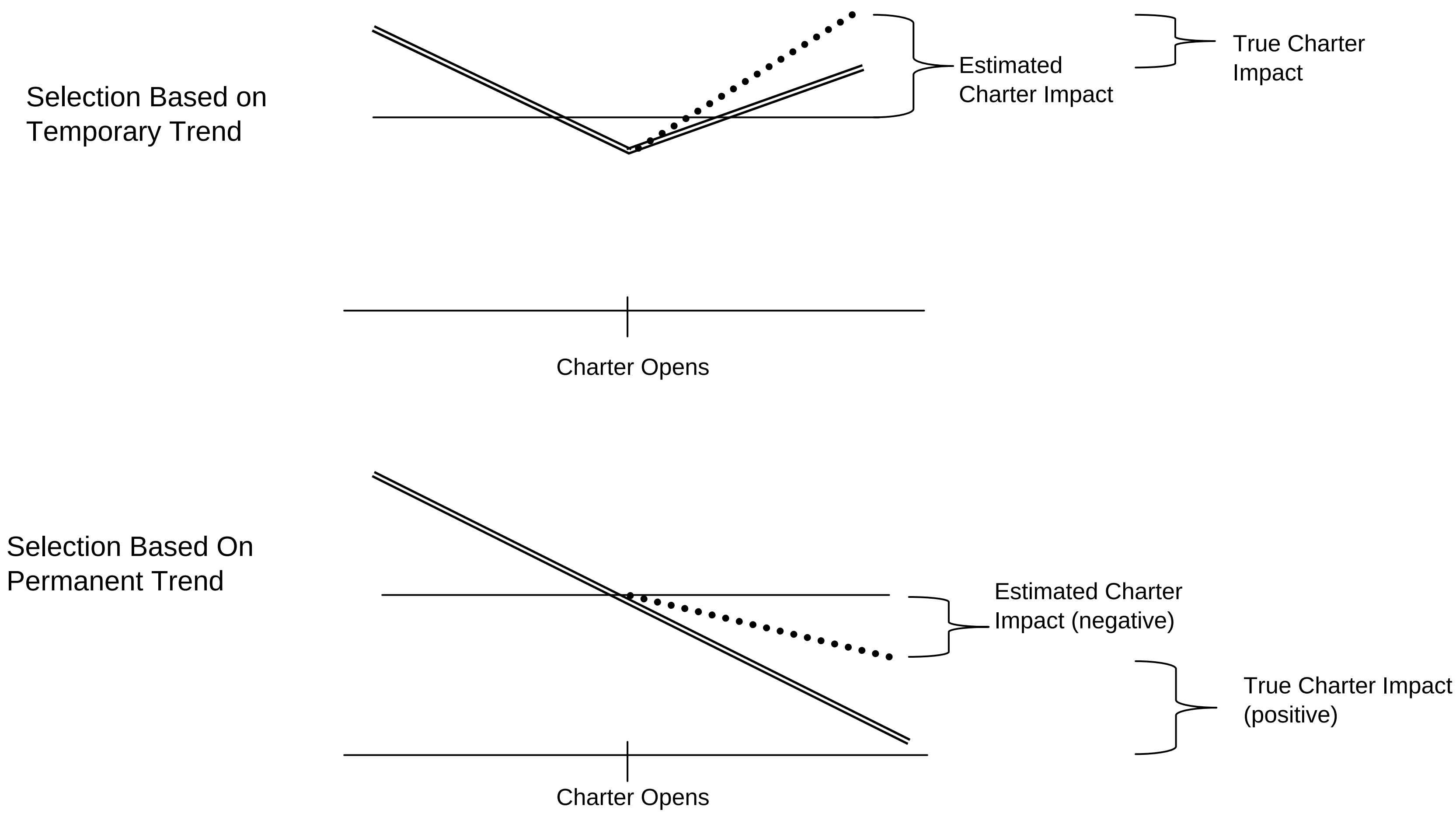
Figure 1: Charter Growth In the US



Sources: 1997 - 1998, US Dept. of Education National Charter School Reports. 1999 - 2003, US Dept. of Education Common Core of Data. 2005, National Alliance for Public Charter Schools. 2004 data is unavaialable so a linear interpolation is provided.

Figure 2- Bias of School Fixed-Effects from Selection Of Charter Location Based on Non-Charter Trends



Single solid line is outcome in school that faces no charter competition after removing school fixed effect. The double solid line is the outcome path taken in a school that faces charter compeition had no charter school opened nearby. The dotted line shows what happens to the outcome after the charter opens.

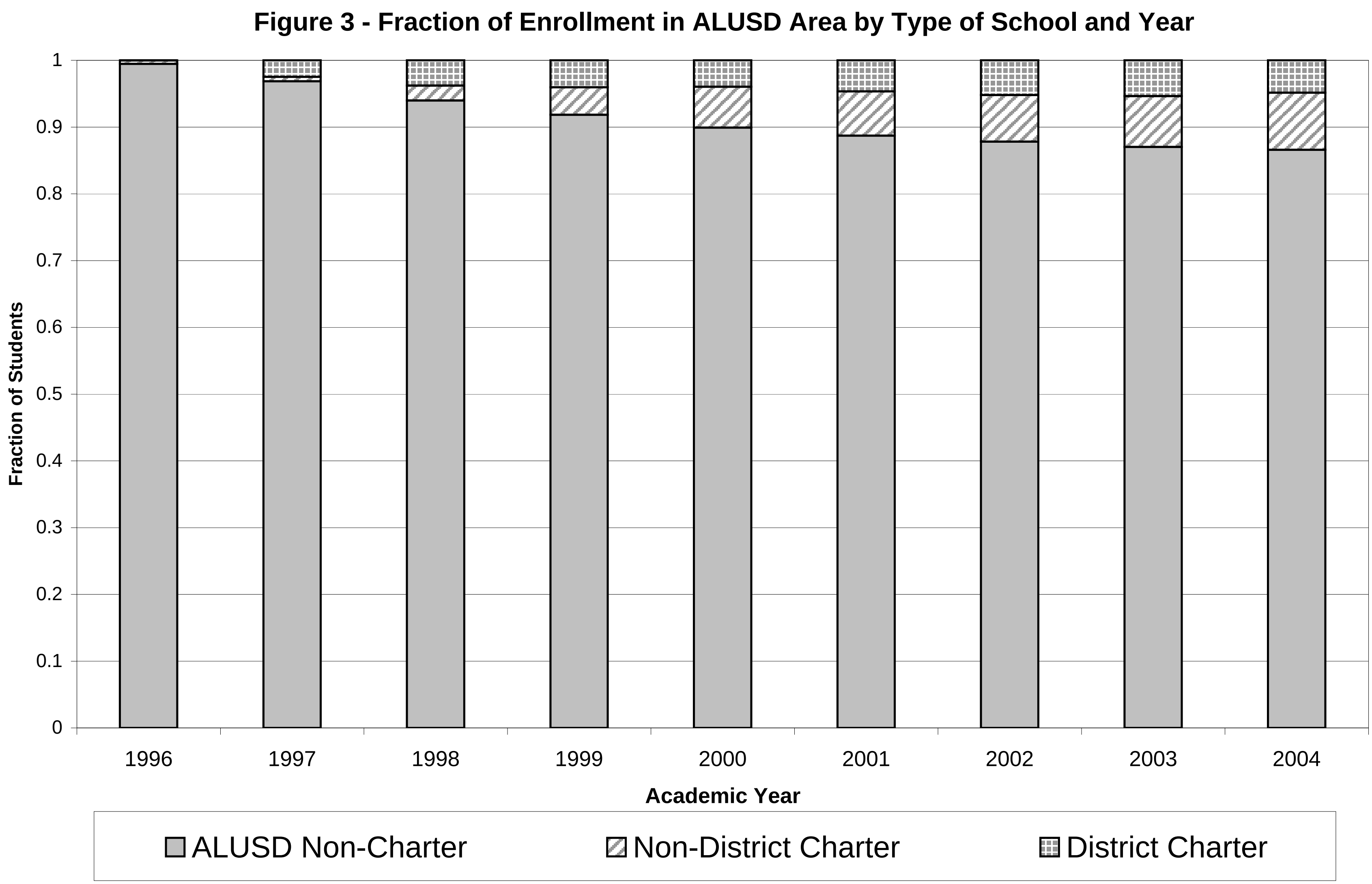


Table shows the fraction of students in each type of school in ALUSD and non-district charters in the region around ALUSD as defined by the state department of education.

Table 1 - Charter and Non-Charter Student Characteristics, 1998 - 2004

	Grades 1 - 5		Grades 6 - 12	
	ALUSD Non-Charter Schools	Non-District Charters	ALUSD Non-Charter Schools	Non-District Charters
Economically disadvantaged†	84	67**	70	61**
	(23)	(29)	(24)	(26)
Limited English proficient	38	15**	14	6**
	(23)	(28)	(11)	(11)
Special education	8	6**	13	8**
	(4)	(6)	(7)	(9)
Gifted	8	2**	11	2**
	(12)	(6)	(14)	(6)
White, non-Hispanic	9	6	11	8
	(16)	(17)	(14)	(18)
Black, non-Hispanic	58	31**	53	43**
	(32)	(36)	(29)	(35)
Hispanic	29	62**	33	48**
	(30)	(39)	(28)	(37)
% Passing State Exams at 2004 Level (2002 - 2004 only)	53	42**	35	31
	(18)	(20)	(19)	(27)
Observations (1998 - 2004; approximate)	1400	200	900	300
Observations (2002 - 2004; approximate)	600	100	300	100

† A student is economically disadvantaged if he or she satistfies one of the following conditions: (1) is eligible to receive free or reduced price lunch, (2) has family income at or below the Federal poverty line, (3) is eligible for TANF or other public assistance, (4) is eligible for programs under Title II of the Job-Training Partnership Act, (5) is eligible for food stamps, or (6) receives a Federal Pell Grant.

Observations are school level aggregates in each year. Standard deviations are provided in parentheses. Results are weighted by enrollment. **, *, and # denote that a t-test of the difference in weighted means between charter and non-charter schools is significant at the 1%, 5%, and 10% levels, respectively. In order to maintain the anonymity of the district, I can only provide approximate observation counts.

Table 2 - Characteristics of ALUSD Schools by Non-District Charter Penetration

	(1)	(2)	(3)	(4)
Percentiles of Charter Penetration†	0 - 59	60 - 74	75 - 89	90 - 99
Range of Charter Penetration Rates	0.0% - 2.4%	2.4% - 5.5%	5.5% - 10.2%	10.2% - 43.5%
Demographics				
Female	0.49	0.49	0.47*	0.49
	(0.08)	(0.07)	(0.07)	(0.09)
White	0.08	0.09	0.07	0.09
	(0.14)	(0.16)	(0.14)	(0.14)
Free Lunch Eligible	0.67	0.67	0.69	0.59*
	(0.23)	(0.26)	(0.24)	(0.25)
Reduced-Price Lunch Eligible	0.08	0.08#	0.08	0.08
	(0.04)	(0.04)	(0.04)	(0.04)
At Risk	0.57	0.60	0.66**	0.65*
	(0.19)	(0.19)	(0.20)	(0.22)
Limited English Proficient	0.26	0.27	0.32#	0.27
	(0.21)	(0.23)	(0.26)	(0.25)
Special Education	0.09	0.09	0.08	0.13
	(0.14)	(0.13)	(0.11)	(0.21)
Gifted & Talented	0.12	0.11	0.11	0.15
	(0.08)	(0.08)	(0.12)	(0.19)
Recent Immigrant	0.05	0.06#	0.08**	0.08*
	(0.04)	(0.09)	(0.10)	(0.10)
Grade Level	4.51	4.35	4.58	6.07**
	(2.57)	(2.66)	(2.72)	(3.19)
Achievement				
Math NPR Score	48.6	50.9#	49.1	48.2
	(12.6)	(13.3)	(13.2)	(15.6)
Reading NPR Score	48.8	49.0	47.8	47.4
	(12.3)	(13.2)	(13.1)	(16.7)
Language NPR Score	43.9	44.8	43.3	42.6
	(13.0)	(13.9)	(13.5)	(17.2)
Behavior				
# of Disciplinary Infractions	0.32	0.26	0.38	0.54*
	(0.52)	(0.43)	(0.67)	(0.60)
Attendence Rate (%)	95.0	95.1	94.3	92.0**
	(4.6)	(4.3)	(5.2)	(8.0)
Observations (approximate)	1100	280	280	180

† - Charter penetration is defined the fraction of students who attend schools within 2 miles of and are in grades covered by the school being observed who attend non-district charters.

**, *, and # denote that the mean is stastistically significantly different from column one using standard errors clustered by school at the 1%, 5%, and 10% levels, respectively. Covers 1998 - 2004 only, so that only years with a large number of charter schools are considered. Each observation is the school-year mean. Exact sample sizes cannot be revealed due to confidentiality restrictions.

Table 3: Relationship Between Charter Penetration and School Enrollment (1996 - 2004)

Non-District Charter Enrollment Within	(1)	(2)	(3)
0 - 1 Mile	0.051	0.058	-0.123*
	(0.123)	(0.133)	(0.052)
1 - 2 Miles	0.135	0.152	-0.067*
	(0.098)	(0.111)	(0.033)
2 - 3 Miles	0.146	0.169	-0.008
	(0.091)	(0.105)	(0.022)
Year Fixed Effects	N	Y	Y
School Fixed Effects	N	N	Y

Dependent variable is total school enrollment. Observations are school level aggregates. Total number of non-charter schools is over 200. Total number of non-district charter schools is over 40. Observations total more than 2400. Exact sample sizes cannot be provided due to confidentiality restrictions. Regressions contain no covariates except those specified. Robust standard errors clustered by school are in parentheses. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Fixed-Effects Estimates of Charter Schools on Non-Charter Students

	Levels		Value-Added	
	No School FE	School FE	No School FE	School FE
	(1)	(2)	(3)	(4)
Math NPR				
1 Mile	0.30	-4.27*	0.35	-1.57
	(0.21)	(2.15)	(0.22)	(3.48)
1.5 Miles	-0.42	-2.51	-1.53*	2.44
	(0.77)	(1.75)	(0.71)	(2.65)
2 Miles	0.27	-1.64	0.03	0.21
	(0.32)	(1.51)	(0.28)	(2.29)
Reading NPR				
1 Mile	-0.56	-2.59	-0.90	5.05
	(0.71)	(2.34)	(0.57)	(3.36)
1.5 Miles	0.76*	-2.20	0.35	5.55*
	(0.31)	(1.74)	(0.27)	(2.50)
2 Miles	-1.12	-0.89	-1.24	5.21*
	(0.85)	(1.42)	(1.03)	(2.31)
Language NPR				
1 Mile	-4.14**	-1.27	1.12	7.97**
	(1.37)	(2.31)	(2.42)	(2.86)
1.5 Miles	-2.97*	-0.67	2.36	5.80*
	(1.39)	(1.56)	(1.81)	(2.47)
2 Miles	-1.51	1.30	2.63#	5.30*
	(1.45)	(1.40)	(1.55)	(2.31)
# of Disciplinary Infractions				
1 Mile	-0.38	-2.49#	-0.38	1.98
	(0.40)	(1.33)	(0.40)	(1.72)
1.5 Miles	-0.81	-3.39**	-0.68	0.76
	(0.92)	(1.22)	(0.69)	(1.33)
2 Miles	0.06	-1.98	-0.45	1.96
	(0.31)	(1.27)	(0.37)	(1.31)
Attendence Rate (%)				
1 Mile	-2.09*	0.16	-0.96	3.07*
	(1.04)	(1.63)	(0.80)	(1.52)
1.5 Miles	1.00**	-0.29	0.35	1.14
	(0.27)	(1.15)	(0.26)	(1.38)
2 Miles	-3.20**	1.19	-3.13**	1.16
	(1.05)	(1.07)	(0.75)	(1.33)

Charter penetration measure is share of enrollment in overlapping grades within specified distance. All regressions are demeaned within individuals to remove student fixed effects and include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, school zip-code fixed effects, and grade*year dummies as covariates. Huber/White standard errors clustered by school in parentheses. Behavior and attendence regressions contain over 1,200,000 observations in levels and 1,000,000 observations in value-added models. Test score regressions contain over 800,000 observations in levels and over 500,000 in value added models.. Exact sample sizes cannot be revealed due to confidentiality restrictions. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5 - First Stage & Reduced Form Estimates

A. First - Stage Results

Dependent Variable: Share of students within X miles in overlapping grades who attend a charter school.

	Levels			Value-Added		
	(1)			(2)		
	# buildings between 30k & 60k square feet	# of shopping centers or strip malls	F-Test of joint significance of excluded instruments	# buildings between 30k & 60k square feet	# of shopping centers or strip malls	F-Test of joint significance of excluded instruments
Base Sample (Discipline, Attendence)						
1 Mile	0.036**	0.075**	28.2**	0.036**	0.072**	28.6**
	(0.016)	(0.030)		(0.0016)	(0.031)	
1.5 Miles	0.006	0.079**	20.9**	0.008	0.078**	19.2**
	(0.010)	(0.018)		(0.010)	(0.019)	
2 Miles	-0.003	0.070**	10.26**	-0.001	0.070**	10.4**
	(0.010)	(0.021)		(0.11)	(0.022)	
Testing Sample (Math, Reading, Language)						
1 Mile	0.029#	0.132**	32.4**	0.023	0.157**	37.8**
	(0.016)	(0.035)		(0.020)	(0.043)	
1.5 Miles	0.018#	0.074**	21.0**	0.021#	0.080**	20.0**
	(0.010)	(0.019)		(0.011)	(0.022)	
2 Miles	0.001	0.069**	9.5**	0.003	0.074**	9.0**
	(0.007)	(0.018)		(0.008)	(0.021)	

Note: For ease of exposition, coefficients were multiplied by 100.

B. Reduced Form Results

	Levels		Value Added	
	(1)		(2)	
	# buildings between 30k & 60k square feet	# of shopping centers or strip malls	# buildings between 30k & 60k square feet	# of shopping centers or strip malls
Math NPR				
1 Mile	-0.0045	-0.0600**	0.0111	-0.0692**
	(0.0136)	(0.0225)	(0.0141)	(0.0230)
1.5 Miles	-0.0140*	-0.0124	-0.0095	-0.0069
	(0.0063)	(0.0081)	(0.0069)	(0.0088)
2 Miles	-0.0085*	-0.0105#	-0.0043	-0.0069
	(0.0039)	(0.0055)	(0.0041)	(0.0058)
Reading NPR				
1 Mile	-0.0088	-0.0171	0.0109	-0.0282#
	(0.0108)	(0.0174)	(0.0102)	(0.0150)
1.5 Miles	-0.0089#	-0.0012	-0.0033	0.0021
	(0.0052)	(0.0073)	(0.0048)	(0.0069)
2 Miles	-0.0074*	-0.0040	-0.0060#	0.0046
	(0.0034)	(0.0050)	(0.0032)	(0.0047)

Language NPR				
1 Mile	-0.0106	-0.0301	-0.0058	-0.0247#
	(0.0117)	(0.0200)	(0.0091)	(0.0142)
1.5 Miles	-0.0072	-0.0146#	-0.0030	-0.0119*
	(0.0058)	(0.0082)	(0.0046)	(0.0059)
2 Miles	-0.0043	-0.0143**	-0.0015	-0.0101**
	(0.0035)	(0.0049)	(0.0029)	(0.0038)
# of Disciplinary Infractions				
1 Mile	-0.0006	-0.0082*	0.0002	-0.0118#
	(0.0019)	(0.0038)	(0.0022)	(0.0068)
1.5 Miles	-0.0002	-0.0037*	0.0001	-0.0051
	(0.0012)	(0.0019)	(0.0012)	(0.0035)
2 Miles	-0.0003	-0.0019	-0.0007	-0.0027
	(0.0008)	(0.0013)	(0.0006)	(0.0020)
Attendence Rate (%)				
1 Mile	-0.0042	0.0188	0.0060	0.0211*
	(0.0121)	(0.0167)	(0.0088)	(0.0101)
1.5 Miles	-0.0032	0.0062	0.0023	0.0087#
	(0.0050)	(0.0082)	(0.0037)	(0.0047)
2 Miles	-0.0040	0.0062	0.0018	0.0058#
	(0.0037)	(0.0054)	(0.0025)	(0.0030)

All regressions are demeaned within individuals to remove student fixed effects and include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade-by-year dummies as covariates. Behavior and attendence regressions contain over 1,500,000 observations in levels and 1,000,000 observations in value-added models. Test score regressions contain over 800,000 observations in levels and over 500,000 in value added models. In order to maintain the anonymity of the district I cannot provide exact observation counts. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 6 - Baseline IV Estimates

Endogenous Variable: Share of students within X miles who attend charter school with overlapping grades.
Instruments: Post 1997 * # of buildings in 1995 within X miles between 30,000 & 60,000 square feet
Post 1997 * # of shopping centers or strip malls in 1995 within X miles

	Levels		Value Added	
	Estimates	P(Hansen's J)	Estimates	P(Hansen's J)
Math NPR				
1 Mile	-38.5**	0.65	-29.5**	0.18
	(9.9)		(9.4)	
1.5 Miles	-31.5**	0.21	-18.1**	0.40
	(8.5)		(6.9)	
2 Miles	-27.3**	0.08	-14.8*	0.40
	(9.3)		(6.9)	
Reading NPR				
1 Mile	-17.0#	0.71	-7.7	0.19
	(9.1)		(6.2)	
1.5 Miles	-13.1*	0.19	-2.1	0.54
	(6.6)		(4.9)	
2 Miles	-16.4*	0.05	-2.4	0.07
	(7.3)		(4.5)	
Language NPR				
1 Mile	-25.9*	0.80	-17.2**	0.86
	(11.1)		(6.5)	
1.5 Miles	-24.7**	0.63	-14.7**	0.99
	(9.5)		(5.1)	
2 Miles	-26.6*	0.30	-15.0**	0.74
	(10.4)		(5.8)	
# of Disciplinary Infractions				
1 Mile	-6.8**	0.41	-8.6*	0.29
	(1.8)		(3.4)	
1.5 Miles	-4.6**	0.98	-5.8*	0.70
	(1.2)		(2.7)	
2 Miles	-3.3*	0.67	-4.8#	0.41
	(1.3)		(2.8)	
Attendence Rate (%)				
1 Mile	8.5	0.45	23.6#	0.74
	(14.5)		(12.9)	
1.5 Miles	3.2	0.50	12.9*	0.73
	(8.5)		(6.4)	
2 Miles	4.0	0.32	10.6#	0.54
	(7.4)		(5.8)	

All regressions are demeaned within individuals to remove student fixed effects and include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade-by-year dummies as covariates. Behavior and attendence regressions contain over 1,500,000 observations in levels and 1,000,000 observations in value-added models. Test score regressions contain over 800,000 observations in levels and over 500,000 in value added models. In order to maintain the anonymity of the district I cannot provide exact observation counts. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 7 - Specification Tests for IV Estimates

	Levels						Value-Added					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Math NPR												
1 Mile	-38.5**	-33.2*	-24.2*	-26.5**	-38.1**	-34.1**	-29.5**	-14.4	-25.0*	-20.7*	-39.0**	-25.1**
	(9.9)	(13.8)	(11.7)	(8.5)	(14.4)	(10.2)	(9.4)	(13.6)	(12.2)	(8.3)	(14.3)	(9.2)
1.5 Miles	-31.5**	-41.4**	-27.2*	-26.2**	-35.1**	-21.5*	-18.1**	-24.3*	-22.5*	-12.8*	-30.4**	-18.5*
	(8.5)	(14.2)	(12.1)	(7.4)	(13.0)	(8.9)	(6.9)	(11.3)	(10.5)	(6.2)	(10.2)	(8.3)
2 Miles	-27.3**	-50.2*	-32.7**	-24.6**	-20.7#	-9.8	-14.8*	-23.6#	-30.1**	-11.9#	-16.2#	-15.0
	(9.3)	(22.0)	(11.9)	(8.5)	(11.5)	(12.3)	(6.9)	(14.3)	(10.6)	(6.2)	(9.1)	(12.1)
Reading NPR												
1 Mile	-17.0#	-20.1	-10.2	-6.9	-4.5	-10.3	-7.7	2.9	-6.5	-2.9	-3.4	2.0
	(9.1)	(12.9)	(10.5)	(7.8)	(10.7)	(8.5)	(6.2)	(11.0)	(8.5)	(5.6)	(7.9)	(6.3)
1.5 Miles	-13.1*	-20.9*	-8.3	-8.3	-4.9	-0.6	-2.1	-5.1	-13.2*	1.2	0.7	9.0
	(6.6)	(10.2)	(7.8)	(6.1)	(8.4)	(7.2)	(4.9)	(7.3)	(5.8)	(5.0)	(6.7)	(5.8)
2 Miles	-16.4*	-36.4*	-11.5	-14.9*	-6.0	7.4	-2.4	-16.0	-11.1#	-0.4	5.9	12.3
	(7.3)	(18.0)	(8.0)	(6.7)	(8.5)	(9.3)	(4.5)	(10.0)	(6.3)	(4.4)	(7.0)	(8.6)
Language NPR												
1 Mile	-25.9*	-28.5#	-18.4	-15.2	-18.0	-19.4*	-17.2**	-18.7#	-19.4*	-11.4*	-19.8*	-18.2**
	(11.1)	(14.8)	(12.3)	(9.6)	(12.6)	(8.3)	(6.5)	(10.6)	(9.5)	(5.7)	(9.4)	(7.1)
1.5 Miles	-24.7**	-27.9*	-19.4#	-21.8*	-20.7#	-14.4#	-14.7**	-14.7#	-15.4*	-10.7*	-21.2**	-9.6
	(9.5)	(12.9)	(10.2)	(8.7)	(12.1)	(7.6)	(5.1)	(7.6)	(6.9)	(4.5)	(8.0)	(6.3)
2 Miles	-26.6*	-38.0#	-27.7*	-26.7**	-18.4	0.8	-15.0**	-17.4#	-18.8*	-13.8**	-17.1*	-16.6#
	(10.4)	(20.1)	(11.7)	(9.8)	(11.4)	(9.2)	(5.8)	(10.3)	(7.8)	(5.2)	(7.7)	(8.9)
# of Disciplinary Infractions												
1 Mile	-6.8**	-5.7**	-6.0**	-5.9**	-9.1**	-7.6**	-8.6*	-6.7**	-5.9**	-9.2*	-11.1**	-8.0**
	(1.8)	(1.8)	(1.7)	(1.7)	(2.4)	(2.3)	(3.4)	(2.3)	(1.8)	(4.2)	(3.9)	(2.6)
1.5 Miles	-4.6**	-4.5*	-5.7**	-4.5**	-5.8**	-5.4**	-5.8*	-5.1**	-3.8**	-6.4#	-7.1*	-4.9**
	(1.2)	(1.9)	(1.6)	(1.3)	(1.3)	(1.3)	(2.7)	(2.0)	(1.0)	(3.3)	(2.8)	(1.4)
2 Miles	-3.3*	-4.5	-4.6**	-3.2*	-4.0**	-3.4*	-4.8#	-6.7#	-3.4**	-4.9#	-5.2*	-2.7*
	(1.3)	(2.8)	(1.7)	(1.4)	(1.4)	(1.5)	(2.8)	(3.9)	(1.2)	(2.9)	(2.5)	(1.3)
Attendence Rate (%)												
1 Mile	8.5	4.3	0.7	4.6	12.4	19.0	23.6#	22.2	20.3	17.5	25.2#	28.0*
	(14.5)	(16.6)	(14.8)	(14.1)	(16.6)	(15.9)	(12.9)	(14.8)	(13.7)	(11.7)	(14.6)	(14.2)
1.5 Miles	3.2	-1.7	-2.7	2.5	2.7	9.0	12.9*	14.6	2.6	11.2#	11.1#	15.3**
	(8.5)	(11.2)	(7.6)	(9.1)	(8.8)	(8.4)	(6.4)	(10.1)	(4.9)	(6.2)	(6.4)	(5.6)
2 Miles	4.0	-7.2	-2.0	4.0	4.4	9.0	10.6#	15.9	6.6	9.8#	7.5#	9.4*
	(7.4)	(12.5)	(8.9)	(7.6)	(6.8)	(7.1)	(5.8)	(12.8)	(5.9)	(5.6)	(4.5)	(4.6)

1 - Baseline estimates (from table 6).

2 - Instrumented by # of buildings between 30k & 60k square feet of building space only.

3 - Instrumented by # of buildings between 0 & 30k, 30k & 60k, 60k & 90k, 90k & 120k, and more than 120k square feet of building space.

4 - Control for quadratic in number of commercial properties within distance range.

5 - Control for census tract characteristics - fraction black, fraction Hispanic, fraction non-native born, fraction with HS degree or some college, fraction with college degree, male 25yr+ labor force participation, log of average household income.

6 - Control for zip-code fixed effects.

All regressions are demeaned within individuals to remove student fixed effects and include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade-by-year dummies as covariates. Behavior and attendence regressions contain over 1,500,000 observations in levels and 1,000,000 observations in value-added models. Test score regressions contain over 800,000 observations in levels and over 500,000 in value added models. In order to maintain the anonymity of the district I cannot provide exact observation counts. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 8 - Charter Impacts on Non-Charter Students by Grade Level

	Levels		Value Added	
	(1)		(2)	
	Charter Share* Grades 1 - 5	Charter Share* Grades 6 - 12†	Charter Share* Grades 1 - 5	Charter Share* Grades 6 - 12†
Math NPR				
1 Mile	-53.5**	-18.5	-44.9*	-16.4
	(18.1)	(17.2)	(20.6)	(17.0)
1.5 Miles	-58.4**	-12.2	-47.5*	-11.3
	(19.9)	(12.3)	(20.8)	(12.8)
2 Miles	-45.6*	0.8	-56.5**	-3.1
	(23.2)	(13.4)	(20.3)	(14.3)
Reading NPR				
1 Mile	-49.9**	17.3	-23.0#	18.3
	(15.7)	(15.8)	(12.9)	(13.9)
1.5 Miles	-39.7*	15.6	-2.1	15.6
	(15.9)	(11.8)	(10.5)	(9.5)
2 Miles	-34.0	18.7	-3.4	15.2
	(21.6)	(18.3)	(14.4)	(14.4)
Language NPR				
1 Mile	-45.9**	-5.4	-26.6*	-15.2
	(15.6)	(13.2)	(13.2)	(10.8)
1.5 Miles	-49.7**	-3.7	-25.3*	-5.1
	(15.7)	(9.7)	(11.5)	(7.6)
2 Miles	-34.4#	10.9	-34.1*	-11.6
	(17.7)	(14.6)	(14.8)	(12.4)
# of Disciplinary Infractions				
1 Mile	0.41	-11.13*	0.99	-10.50#
	(1.39)	(5.20)	(1.85)	(5.41)
1.5 Miles	0.77	-6.95**	1.20	-6.07*
	(1.49)	(2.48)	(1.68)	(2.47)
2 Miles	3.73#	-4.32#	4.15#	-3.40#
	(2.03)	(2.47)	(2.21)	(2.00)
Attendence Rate (%)				
1 Mile	-1.5	21.6	-7.1	40.2
	(5.4)	(25.9)	(7.1)	(24.7)
1.5 Miles	-4.7	12.1	-7.6	20.2*
	(6.8)	(11.0)	(6.2)	(8.8)
2 Miles	-5.8	11.1	-14.9*	11.8
	(6.9)	(9.4)	(6.7)	(7.2)

† Grades 6 - 11 for exams.
Each row in column (1) and in column (2) are separate regressions.
Endogenous Variable: Share of students within X miles who attend charter school with overlapping grades interacted with grade level.
Instruments:
(in grade 1 - 5)*(post 1997)*(# of buildings in 1995 within X miles between 30,000 & 60,000 square feet)
(in grade 1 - 5)*(post 1997)*(# of shopping centers or strip malls in 1995 within X miles)
(in grade 6 - 12)*(post 1997)*(# of buildings in 1995 within X miles between 30,000 & 60,000 square feet)
(in grade 6 - 12)*(post 1997)*(# of shopping centers or strip malls in 1995 within X miles)

All regressions are demeaned within individuals to remove student fixed effects and include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year dummies as covariates. Behavior and attendence regressions contain over 1,200,000 observations in levels and 1,000,000 observations in value-added models. Test score regressions contain over 800,000 observations in levels and over 500,000 in value added models. Exact sample sizes cannot be revealed due to confidentiality restrictions. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 9 - Dynamics of Charter Impacts on Non-Charter Students

Endogenous Variable: Share of students within X miles who attend charter school with overlapping grades.
Instruments: (year indicator)*(post 1997)*(# of buildings in 1995 within X miles between 30,000 & 60,000 square feet)
(year indicator)*(post 1997)*(# of shopping centers or strip malls in 1995 within X miles)

A. Levels

	Estimates					
	Grades 1 - 5			Grades 6 - 12		
	Year t	Year t-1	Year t-2	Year t	Year t-1	Year t-2
Math NPR						
1 Mile	-10.2	-90.1*	49.8#	-5.0	-48.3#	27.3
	(22.2)	(36.7)	(27.4)	(9.5)	(25.7)	(18.0)
1.5 Miles	-0.7	-118.4**	31.3	12.3	-5.0	-28.0*
	(23.9)	(30.4)	(26.7)	(18.5)	(12.3)	(12.9)
2 Miles	32.4	-208.7**	98.1*	2.1	3.0	-21.1
	(27.2)	(46.0)	(39.8)	(17.1)	(10.3)	(15.2)
Reading NPR						
1 Mile	-57.3**	-2.7	24.1	10.2	-24.6#	46.8*
	(20.4)	(28.0)	(22.6)	(9.4)	(14.9)	(20.6)
1.5 Miles	-40.0*	-47.8*	42.5*	-1.6	9.4	6.2
	(15.6)	(20.6)	(19.9)	(10.7)	(8.2)	(8.2)
2 Miles	5.2	-164.6**	136.4**	-5.8	9.4	11.9
	(23.6)	(42.0)	(36.4)	(10.4)	(7.7)	(8.1)
Language NPR						
1 Mile	-36.6*	-16.1	1.5	10.3	-15.9	9.8
	(16.3)	(25.5)	(21.9)	(7.3)	(11.4)	(9.3)
1.5 Miles	-31.9*	-45.5*	9.7	-17.5	26.6#	-8.9
	(15.5)	(20.8)	(18.6)	(11.5)	(14.3)	(8.3)
2 Miles	8.9	-135.0**	65.8*	-9.5	18.8	-5.4
	(18.8)	(32.6)	(31.3)	(11.4)	(13.7)	(8.9)
# of Disciplinary Infractions						
1 Mile	0.8	0.9	-1.3*	-2.3	0.0	-2.9#
	(0.8)	(0.6)	(0.6)	(1.5)	(1.4)	(1.6)
1.5 Miles	0.4	1.7**	-0.7	-3.8*	0.7	-2.1
	(0.7)	(0.6)	(0.7)	(1.6)	(2.2)	(1.5)
2 Miles	2.0*	3.8**	-2.5*	-1.6	-0.9	-0.5
	(0.8)	(1.2)	(1.1)	(1.1)	(1.1)	(1.2)
Attendence Rate (%)						
1 Mile	2.3	-9.1**	3.1	-6.1	0.8	1.9
	(2.6)	(3.2)	(3.0)	(6.9)	(3.9)	(6.9)
1.5 Miles	-1.3	-7.0*	-2.1	9.2	-10.9	4.8
	(3.8)	(3.0)	(2.9)	(9.5)	(9.3)	(6.4)
2 Miles	-4.8	-10.6*	0.9	3.9	0.9	-3.3
	(3.8)	(4.2)	(4.6)	(6.7)	(3.8)	(3.8)

B. Value - Added

	Estimates					
	Grades 1 - 5			Grades 6 - 12		
	Year t	Year t-1	Year t-2	Year t	Year t-1	Year t-2
Math NPR						
1 Mile	-20.9	-19.1	-23.2	-17.2	-31.1	38.2
	(31.0)	(56.5)	(42.2)	(18.4)	(22.2)	(23.6)
1.5 Miles	60.8	-159.9*	42.1	6.6	-14.0	0.4
	(44.7)	(64.4)	(36.4)	(17.8)	(24.0)	(14.5)
2 Miles	53.8	-138.2*	7.1	-8.7	14.2	-4.0
	(34.1)	(56.7)	(43.3)	(17.9)	(19.9)	(16.9)
Reading NPR						
1 Mile	-17.0	56.6	-81.7*	-6.7	-4.5	16.0
	(27.8)	(52.6)	(35.4)	(12.5)	(12.9)	(12.7)
1.5 Miles	22.8	-13.8	-29.1	-6.3	16.4	-4.2
	(34.9)	(47.5)	(23.3)	(14.4)	(19.7)	(11.0)
2 Miles	60.1*	-44.4	-36.0	-17.2	23.1	0.3
	(26.9)	(40.0)	(27.4)	(15.5)	(18.7)	(12.9)
Language NPR						
1 Mile	-4.7	3.7	-40.4	1.9	-12.6	-7.2
	(23.9)	(45.2)	(35.4)	(7.9)	(14.8)	(8.6)
1.5 Miles	9.2	-44.3	-1.0	3.0	6.6	-13.7
	(30.0)	(42.1)	(29.7)	(12.8)	(18.2)	(10.0)
2 Miles	40.3	-94.9*	12.4	0.1	8.8	-15.8
	(27.0)	(42.5)	(36.1)	(14.6)	(17.0)	(12.1)
# of Disciplinary Infractions						
1 Mile	-0.4	0.7	-0.6	-2.7	-1.2	-2.9
	(1.1)	(0.9)	(0.7)	(1.8)	(1.4)	(3.2)
1.5 Miles	-1.5	1.8*	-0.3	-5.1**	2.1	-2.6
	(1.3)	(0.8)	(1.0)	(1.9)	(3.1)	(2.5)
2 Miles	-0.3	1.8	-0.1	-3.1*	-0.2	-0.3
	(1.5)	(1.5)	(1.6)	(1.4)	(1.7)	(1.7)
Attendence Rate (%)						
1 Mile	0.6	-8.2*	3.7	10.3	1.7	-3.8
	(3.4)	(3.9)	(3.3)	(10.8)	(5.0)	(5.2)
1.5 Miles	0.9	-8.4*	0.4	21.2*	-16.7	6.9
	(3.9)	(3.5)	(3.4)	(10.5)	(13.8)	(8.1)
2 Miles	-6.5#	-10.1*	4.0	7.5	-0.9	-4.1
	(3.6)	(4.1)	(4.8)	(5.4)	(4.4)	(4.0)

All regressions are demeaned within individuals to remove student fixed effects and include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year dummies as covariates. Behavior and attendence regressions contain over 1,200,000 observations in levels and 1,000,000 observations in value-added models. Test score regressions contain over 800,000 observations in levels and over 500,000 in value added models. Exact sample sizes cannot be revealed due to confidentiality restrictions. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table A1 - Description of Data Elements Used in Analysis

Student Level Variables

At risk	At risk classification varies by grade: K - 3: Student fails a state reading exam or is LEP. 4 - 12: Student fails any section of state exam on most recent attempt, is LEP, or is overrage for grade. A student is also classified "at-risk" if he/she is pregnant, abused, a parent, homeless, has previously dropped out, resides in a residential placement facility, attends an alternative education program, is on conditional release from juvenile corrections, or has previously been expelled.
Attendance rate	Percent of days the student is enrolled during which the student attends class.
Average grade	Annual average of quarterly (grades 1 - 5) or biannual (grades 6-12) grades in mathematics, reading, English, science, and social studies courses.
Free lunch	Whether student is eligible for free lunches under the Federal free-lunch program.
Gifted and talented	Student is enrolled in a gifted and talented program.
Infractions	Number of disciplinary infractions a student has during a given year warranting a punishment of one day suspension or higher.
Language NPR	National percentile ranking on language standardized examination.
Limited English proficient (LEP)	A student is categorized as LEP if (a) he or she speaks a language other than english at home and (b) scores below English proficiency level on a oral language proficiency test or scores below the 40th percentile in total reading and language on standardized tests
Math NPR	National percentile ranking on mathematics standardized examination.
Other economic disadvantage	Student is designated as having another economic disadvantage if the student does not qualify for free or reduced-price luncha and one of the following conditions hold: (1) family income is below Federal poverty line (2) is eligible for public assistance (i.e. TANF, Food Stamps, etc.) (3) family received a Pell Grant or comparable form of state financial aid (4) eligible for training under Title II of the Job Training Partnership Act
Parents are migrants	Student meets the following conditions for eligibility for the Migrant Education Program (MEP): (1) aged 3 - 21 (2) has a parent, guardian, or spouse who is a migratory agricultural or fishing worker (3) has moved between school districts withing 3 years for said parent, guardian, or spouse to seek temporary or seasonal work in agriculture or fishing
Reading NPR	National percentile ranking on reading standardized examination.
Recent immigrant (within 3 years)	Student is aged 3 - 21, was born outside the US, and has not been enrolled in a US school for more than 3 years (based on eligibility requirements of the Emergency Immigrant Education Program (EIEP) of 1994.
Reduced price lunch	Whether student is eligible for reduced price lunches under the Federal free-lunch program.
Special education	Student is eligible for special education services.

Census Tract Variables (from 2000 Census Summary File)

Population Density	Population count of Census tract divided by land area of tract. In miles.
Fraction Black	Fraction of people in Census tract who are black.
Fraction Hispanic	Fraction of people in Census tract who are Hispanic.
Fraction Non-Native	Fraction of people in Census tract who were not born in the United States.
Fraction w/ HS or Some College	Fraction of people in Census tract who graduated high school but did not complete a 4-year college degree.
Fraction w/ College or Advanced Degree	Fraction of people in Census tract who completed a 4-year college degree.
Labor Force Participation	Fraction of males aged 16+ in Census tract who are in the labor force.
Ln (Household Income)	Natural logarithm of median household income in Census tract.
Fraction receiving Public Assistance	Fraction of people in Census tract who receive money from a Federal, state, or local anti-poverty program.