The Effect of Charter Schools on Non-Charter Students: An Instrumental Variables Approach

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Abstract: Proponents of charter schools claim that charters provide incentives for non-charter public schools to provide more effort towards improving student performance. However, it is unclear whether schools respond to competition and other mechanisms may counteract competitive impacts. In this paper I investigate how charter schools affect behavior, attendance, and test scores for students in non-charter schools using new data from an anonymous large urban school district (ALUSD). I compare three econometric methods which attempt to account for the endogenous location decision of charter schools - school fixed-effects, school fixed-effects combined with school-specific time-trends, and instrumental variables. Results using school fixed effects with or without school specific time trends suggest that impacts on test scores are statistically insignificant in levels models but significantly positive in value-added models. On the other hand, IV results show consistently *negative*, and often statistically significant, impacts of charter schools on test scores in both levels and value-added models. However, I also find large and statistically significant improvements in discipline in schools facing charter competition that also differ from the fixed-effects estimates. These results suggest that previous work on this topic may suffer from substantial selection bias.

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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].

A substantial amount of research has looked at how charter schools affect student outcomes (Booker, Gilpatric, Gronberg and Jansen 2007, Hanushek, Kain, Rivkin and Branch 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. On the other hand, there is comparatively little 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, the loss of funding from students switching to charters may make it more difficult for schools to improve, causing outcomes to worsen. 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.

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 et al. 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

[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-charter

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, concerns have been raised 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 2006, Hoxby and Rockoff 2004) this strategy is inappropriate for assessing charter competition since whether or not one applies to a charter is partially a function of charter location. Nonetheless, there are some potential instruments for charter location along with additional modifications to the school fixed-effects framework 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 two additional models which try to address endogenous charter location. The first adds a school-specific linear time-trend to the baseline regressions. The second uses the number of large buildings near regular public schools as an instrument for charter location. When a charter is started, one of the most restrictive 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, charters tend to locate in a specific range of building sizes because they most often

[4]While some charters purchase land, since charters in the state I'm looking at do not automatically receive capital startup funds from the state or local school district, most tend to rent their space or use donated space

use abandoned schools, churches, strip malls, office complexes, and industrial space.

The second way this paper adds to the existing literature is by assessing the effects of charter schools on discipline and attendance of non-charter students in addition to test scores. I also investigate how much of the charter impacts are likely due to changes in peer composition.

My instrumental variables analysis provide very different results from the school fixed-effects model. Using both levels and value-added frameworks, I find that test score estimates with school fixed effects suggest that the charter impacts are statistically insignificant (levels) to positive (value added). Adding school specific time-trends provide similar results. On the other hand, IV results show consistently negative impacts of charter schools on test scores. These are statistically significant in most cases in levels models, though value added models have some insignificant and some marginally significant results. For discipline, school fixed effects estimates show no statistically significant change, but IV estimates show statistically significant improvements in discipline when charter share increases. There is no statistically significant effect on attendance in any of the models. 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.

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 main fixed-effects and IV results. Section 7 looks at heterogenous impacts and the potential roles for various mechanisms. Section 8 concludes.

2 Literature and Selection

Previous Literature

While there is a large literature on how charter schools affect students who attend them[5], 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.

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 character-

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

istics. 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. 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 regular public schools may respond by trying to improve their students' discipline.

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 to locate a charter school 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. 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 are not spurred 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. If the trends are permanent, then school-FE regressions would underestimate the charter impacts. If the trends are temporary and schools exhibit mean reversion in their performance, then school-FE regressions would overestimate the charter impacts. In this paper, I expand upon the school-fixed effects approach to charter location endogeneity by utilizing two additional econometric models. The first is to add school-specific linear time trends to the school fixed-effects. This will correct for the endogeneity if selection is based on permanent trends. However, if selection is based on temporary trends, then this strategy could potentially exacerbate the bias. Thus, I then turn to instrumental variables techniques in order to extract the causal impact of charter schools on non-charter students.

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[6]. Today there are more than fifty charter schools in the county along with over 200 non-charter schools in ALUSD.[7] 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.[8]

While it may be interesting do differentiate between the effects of district and non-district charters, unfortunately my instrument is too weak for district charters[9] Nonetheless, the most substantial competition is likely to come from the non-district charters since ALUSD loses state aid when a student leaves for these charters but not

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

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

[8]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.

[9]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.

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.

Table 1 provides summary information on traditional public schools and non-district charters. While the charter students are wealthier and more likely to be white than traditional public school students, they are substantially less likely to pass the state criterion reference exams and have lower attendance rates. In terms of inputs, charter schools have less experienced teachers, lower expenditures, and slightly higher student-teacher ratios. However, the charters are much smaller than the regular public schools. In addition, this is not a result of differing grade levels as the differences remain when schools are split into elementary and middle/high.

Do these charter schools exert competitive pressure on the regular public schools? A necessary condition for there to be pressure 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 2 shows results that try to answer this question by running regressions of the form

$$Enroll_{jt} = \alpha + \beta ChartEnroll^{d}_{jt} + \mathbf{X_{jt}}\mathbf{\Psi} + \epsilon_{jt} \tag{1}$$

where $Enroll_{jt}$ is enrollment in a regular public school j at time t, $ChartEnroll^{d}_{jt}$ 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. In Table 2 I show the results of these regressions. 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 eight students per 100 charter seats. However, for charters opening between two and three miles, there is no significant relationship with regular public school enrollment. 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 assume that any charter school farther than two miles away has no effect on non-charter schools and thus focus my attention on schools where charters open within two miles[10].

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 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.

[10]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.

Since not all students take the norm-referenced examination and test data are only available starting in 1998, I generate two samples.[11] 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.[12]

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 based on 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 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.[13] 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

[11] 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.

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

[13] 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).

charter share, 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 and is current as of October, 2007. 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 3 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 from 1998 - 2004. I define charter penetration here as the fraction of students within a specified radius who are in grades covered by the non-charter school but attend a charter school. Schools with non-district charters nearby tend to have more recent immigrants and at-risk students, along with more disciplinary infractions and lower attendance rates, although 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

$$y_{igjt} = \alpha + \beta C^d_{jt} + \mathbf{X}_{igjt}\Omega + \mathbf{G}_{gt}\Theta + \epsilon_{igjt} \tag{2}$$

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[14]. ϵ can further be broken down into student and school error components

$$\epsilon_{igjt} = \gamma_{ijgt} + \eta_{jt}. \tag{3}$$

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. Table 4 looks at this issue. While I cannot directly track students who enter the non-district charter schools, we can get an idea of their characteristics by looking at students who leave ALUSD from schools near charters. In order to minimize the number of dropouts, I look only at students in grades 1 - 8 and schools with a non-district charter within 2 miles. These

[14]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).

summary statistics suggest that there are substantial differences between charter and non-charter students. While the leavers less likely to be minority, LEP, and at-risk, they are also less likely to be non-immigrants and gifted. In addition, and perhaps more importantly, leavers have worse test scores, attendance, and discipline. In addition, as described previously, new students moving into schools facing competition in anticipation of higher quality (or moving out in anticipation of lower quality) could also bias estimates.

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

$$\gamma_{ijgt} = \lambda_i + \nu_{igjt} \tag{4}$$

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 (2) by demeaning the model with respect to the individual as such

$$\tilde{y}_{igjt} = \tilde{\alpha} + \beta\tilde{C}^d_{jt} + \tilde{\mathbf{X}}_{igjt}\Omega + \tilde{\mathbf{G}}_{gt}\Theta + \tilde{\nu}_{igjt} + \tilde{\eta}_{jt}. \tag{5}$$

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. 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

$$\tilde{\eta}_{jt} = \tilde{\zeta}_j + \tilde{\theta}_{jt} \tag{6}$$

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

$$\tilde{y}_{igjt} = \beta \tilde{C}^d_{jt} + \tilde{\mathbf{X}}_{igjt}\Omega + \tilde{\mathbf{G}}_{gt}\Theta + \tilde{\mathbf{S}}_{jt}\Gamma + \tilde{\nu}_{igjt} + \tilde{\theta}_{jt}. \tag{7}$$

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 (6) will be incorrect and including school indicators will not correct for selection. One way we can address this issue is to add school specific time-trends to the regression.

$$\tilde{y}_{igjt} = \beta \tilde{C}^d_{gjt} + \tilde{\mathbf{X}}_{igjt}\Omega + \tilde{\mathbf{G}}_{gt}\Theta + \tilde{\mathbf{S}}_{jt}\Gamma + \widetilde{\mathbf{S_{jt}}t}\Lambda + \tilde{\nu}_{igjt} + \tilde{\theta}_{jt}. \tag{8}$$

As long as charter location is correlated with linear permanent trends but uncorrelated with non-linear or temporary trends, then this will eliminate the bias. If this is not the case, however, then we need some other strategy for removing bias. One possible solution is to use an instrumental variables strategy.

Instrumental Variables

I propose using an instrumental variables (IV) regression analysis for charter share. My instrument is the availability of buildings between 30,000 and 90,000 square feet near regular public schools. The idea behind this instrument is that charter schools need a space of substantial size in order to operate. Indeed, most charter schools tend to locate in strip malls, office complexes, churches, abandoned schools, and warehouses, all of which use a large amount of building space. Thus, an increase in the number of buildings suitably sized to house a charter increases the probability of a charter opening nearby in a manner that is orthogonal to student outcomes. Thus, the two-stage model is

$$\tilde{C}^d_{gjt} = \delta \widetilde{Buildings}^d_{jt} + \tilde{\mathbf{X}}_{igjt}\Omega + \tilde{\mathbf{G}}_{gt}\Theta + \tilde{\nu}_{igjt}. \tag{9}$$

$$\tilde{y}_{igjt} = \widehat{\tilde{C}}^d_{gjt} + \tilde{\mathbf{X}}_{igjt}\Omega + \tilde{\mathbf{G}}_{gt}\Theta + \tilde{\nu}_{igjt}. \tag{10}$$

where $\widetilde{Buildings}^d_{jt}$ 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 what is the proper measure of charter penetration in a given geographic area. 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 $J = 1, 2, ..., N^d_{nc}, N^d_{nc}+1, N^d_{nc}+2, ..., N^d_{nc}+N^d_c$ where N^d_{nc} is the total number of non-charter schools and N^d_c is the total number of charter schools. Charter penetration is calculated as

$$ChartPen^d_{jt} = \frac{\sum_{g=Gmin_j}^{Gmax_j} \sum_{l=N^d_{nc}+1}^{N^d_c} Enroll_{glt}}{\sum_{g=Gmin_j}^{Gmax_j} \sum_{l=1}^{N^d_c} Enroll_{glt}} \tag{11}$$

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 I 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.

OLS and Fixed Effects Estimates

Table 5 shows the results of regressions of charter impacts on non-charter students with and without school 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. I consider five outcome measures - the number of disciplinary infractions warranting an in-school suspension or more severe punishment, the attendance rate, and annual changes in math, reading, and language standardized exam scores. 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 who score lower than the observed student. Separate regressions are run for charter share by 1, 1.5, and 2 miles. I also conduct regressions where the dependent variable is left in levels (L-1, L-2, L-3) and where the dependent variable is first differenced to generate a value added measure (VA-1, VA-2, VA-3).

The columns L-1 and VA-1 show the baseline regressions with no school fixed-effects. While most of the value-added regressions show no statistically significant relationship, there is some evidence of worsening test scores and attendance rates in the levels models. In columns L-2 and VA-2 I add school fixed-effects. These models are similar to those used in the rest of the literature on charter competition. In the levels models the test score estimates are statistically insignificant, except in the case of math scores within one mile, which is significantly negative. In addition, there is no statistically significant effect on discipline or attendance. On the other hand, the value added model shows evidence of significant improvements in test scores. The estimates suggest that, while there does not appear to be any substantial impact for charters within one mile, if the radius is expanded to 1.5 or 2 miles, an increase in charter share of 10 percentage points is associated with a 0.46 to 0.76 NPR increase in each of the tests. This is a large improvement. Average annual gains in math, reading and language are 0.67, 0.80 and 0.06 NPR, respectively. The value added model with fixed-effects also suggests worsening attendance, but only for a one-mile radius. Finally, columns L-3 and VA-3 add school specific time-trends to models L-2 and VA-2, respectively. Adding the time trends shows no qualitative difference from the school fixed effects, although now discipline statistically significantly worsens. Thus, overall, the fixed-effects results show marginal evidence of improvements in test scores, but worsening discipline and attendance.

Instrumental Variables Estimates

Tables 6 and 7 show the results of my instrumental variables estimates. As my instrument, I use the number of buildings within 1, 1.5 or 2 miles of the regular public schools which have between 30,000 and 90,000 square feet of space[15]. I further interact the instrument with a post-1997 dummy variable, since charter schools did not start in ALUSD until 1996 and remained very few in number until 1998[16]. Table 6 shows the first stage of the IV estimates in both the base and test samples. The first column shows the results from the specification I use in this paper. The instrument is statistically significant at the one percent level in both samples for all three distances used, though, not surprisingly, the relationship weakens as the distance radius increases, as there is less variation across locations in the number of buildings in the chosen size range. One concern with the validity of this instrument is that it may be correlated with neighborhood characteristics of the schools. To address this, in column 2 I show first-stage regressions which also include zip-code fixed effects. Only for 2-miles in the base sample is there a statistically significant drop in the coefficient estimates. In column 3, I add zip-code specific time trends to account for the possibility that areas with particular growth patterns have more buildings of this size range. In this

[15] I attempted a number of variations of building size ranges within 10,000 and 150,000 square feet. The range of 30,000 to 90,000 square feet provided the strongest first stage estimates. In addition, when one looks at the actual size of buildings housing charter schools, the ratio of charter schools to plots for building size of 0 (vacant lot or missing building data) is 0.000018, for a building size of 10,000 to 29,999 sf is 0.0018 , for a building size of 30,000 to 89,999 sf is 0.0037, and for a building size of 90,000 sf or greater is 0.0013. Thus it does appear that charters are substantially more likely to locate in a building of the 30,000 - 90,000 sf range than other ranges.

[16] Interacting the buildings count with a post-1995 dummy, district-wide average charter share, or year dummies instead of the post-1997 dummy provided qualitatively similar results.

case there is no statistically significant drop with respect to column 1. Finally, in column 4 I add a set of census tract characteristics[17] to account for additional within zip-code variation. The only statistically significant drop in the coefficient estimate from column 1 is for 2 miles. In addition, only one estimate, test sample at 2 miles, is the difference statistically significant at the 1% level. Indeed, while the estimates weaken somewhat at 2 miles, they are robust to the different specifications at 1 and 1.5 miles. In addition, at 2 miles we would expect there to be little variation in the number of buildings between 30,000 and 90,000 square feet within zip-codes, thus adding these local area controls would be more likely to reduce the predictive power of the instrument at this distance.

Table 7 shows the IV results. The main results are shown in column one. The IV estimates show statistically significant improvements in discipline at all distances and in both the levels and value-added models. These suggest that if the charter share within 1.5 miles increases by 10 percentage points, disciplinary infractions fall by 0.5 - 0.6 per student. For attendance there is no statistically significant impact. For test scores, the IV results are consistently negative with the exception of math and reading value added within one mile, but those are not statistically significant. All but two of the estimates in the levels model are statistically significant. The value added model is generally not statistically significant except for language tests. At face value, the levels model suggests that an increase in charter share of 10 percentage points within 1.5 miles generates 1.9 - 3.8 national percentile ranking point drop on each exam while the value added model shows a more modest drop of 0.4 to 1.2 NPR. However, due to the imprecision of the IV estimates, we should be wary of taking these as the true impact.[18]

[17] A list of these can be found in Appendix Table 1.

[18] 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.

One concern we may have is that it may take time for schools to react to charter competition. Thus, it is possible that lagged charter share is a more appropriate measure of charter competition. In addition, if lagged charter share plays a role, then the instrument will be correlated with the error term (where the lagged charter share resides) and would be invalid. Ideally, I could use lags of the instrument as instruments for lagged charter share. Unfortunately, most of the variation in my instrument is cross-sectional, making this strategy infeasible. Instead, I conduct regressions using moving averages of charter share instead of just contemporaneous charter share as the charter competition measures. This subsumes any lagged charter effects into the regression without adding an extra endogenous term[19]. These are shown in columns 2 and 3 of table 7 and provide results that are qualitatively similar to the contemporaneous results[20]

Thus, the instrumental variables results show a substantially different story from the school fixed effects estimates. Indeed, rather than improvements in test scores, the IV estimates show test scores worsening when charters locate nearby. However, for discipline we find the opposite story... while fixed effects estimates show discipline worsening, the IV estimates show it getting substantially better. While this may seem like a paradox, it turns out that these different results are almost entirely the result of a heterogenous impacts by school type.

[19]Implicitly, what I am assuming here is that, conditional on current charter share, there is a large role for once lagged charter share (and twice lagged in the three-year moving average) but no role for additional lags.

[20]One could also simply run regressions of lagged charter share without current, but this suffers from the same instrument invalidity problem. Nonetheless, regressions using this strategy were qualitatively similar for both first and second lags, with the exception of reading scores in the second lag.

7 Heterogeneity and Potential Mechanisms

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)[21]. 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 fewer discipline problems on average. What is somewhat surprising is that almost all of the test score reductions are coming from elementary students. Due to the split of the endogenous variable, the IV estimates become considerably more imprecise, however the estimates are statistically significant and negative at the 10% or lower level in 16 out of 18 estimates for the primary students, but only two regressions for the middle and secondary students.

Table 9 shows regressions which attempt to determine the school characteristics charter impacts work through. These are the same regressions as in Table 7, column 1, but I also add controls for per-student expenditures, total expenditures, enrollment, student-teacher ratios, and teacher experience. In levels models, controlling for these factors does reduce the charter impacts, both discipline and attendance. However, only reading scores drop to statistical insignificance. In value added models, only the discipline estimate changes substantially. In regressions not shown here, but available upon request, I control for each characteristic separately. These regressions suggest that most of the increase in the test-score estimates come from controlling for teacher experience while most of the increase in the discipline estimates come from controlling for per-student expenditure.

[21] 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.

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, they also have the potential to make increasing performance in traditional public schools more difficult through reducing funds and changing student's peer groups. In this paper, using data from an anonymous large urban school district, I add to the current literature in a few ways. First, I provide estimates that use school specific time trends and instrumental variable techniques to account for the potential that charter schools endogenously locate near particular types of non-charter schools. Second, I assess the effects of charter schools on discipline and attendance of non-charter students in addition to test scores. Third, I investigate potential mechanisms for charter impacts.

Using an instrumental variables procedure, I show that using school fixed effects does not remove the bias from endogenous charter location and may even exacerbate it. While my school fixed-effects results show moderate gains in test scores and worsening discipline, my IV estimates show that charter schools *improve* discipline and *worsen* test scores in non-charter schools. IV estimates suggest that a ten percentage point increase in charter share within 1.5 miles of a non-charter school reduces disciplinary infractions by a statistically significant 0.55 per year while test scores drop by up to 4 national percentile rankings in levels models and 1.5 NPR in value added models depending on the test. While these estimates may be high due to imprecision of the IV estimates, the test score estimates are consistently negative for 16 out of 18 regressions and of those 16 negative estimates, 10 are statistically significant at the 10% or lower level. There is no statistically significant impact on attendance rates. The apparent inconsistency between the discipline and test score results is rectified by showing that almost all of the discipline impact occurs in grades 6 - 12 while most of the test score impact occurs in grades 1 - 5, suggesting that impacts are heterogenous across schools.

Unfortunately I cannot establish why I get the results of improving discipline in middle/secondary schools and worsening test scores in primary schools. Part of the impacts appear to be due to changes in expenditures and teacher experience, but there is a substantial portion of the impacts estimates for which the mechanism is unclear. While it is possible that the discipline improvements are from a competitive response, since a large number of charter students appear to have behavioral problems, it is likely that at least a portion of the improvement is due to changes in peer characteristics. For test scores, on the other hand, peer effects are unlikely to play the same role since I find worsening test scores while charters seem to attract lower performing students. Thus, an important line of future research on this subject would be to establish the role of changes in peer composition in charter impacts on non-charter students.

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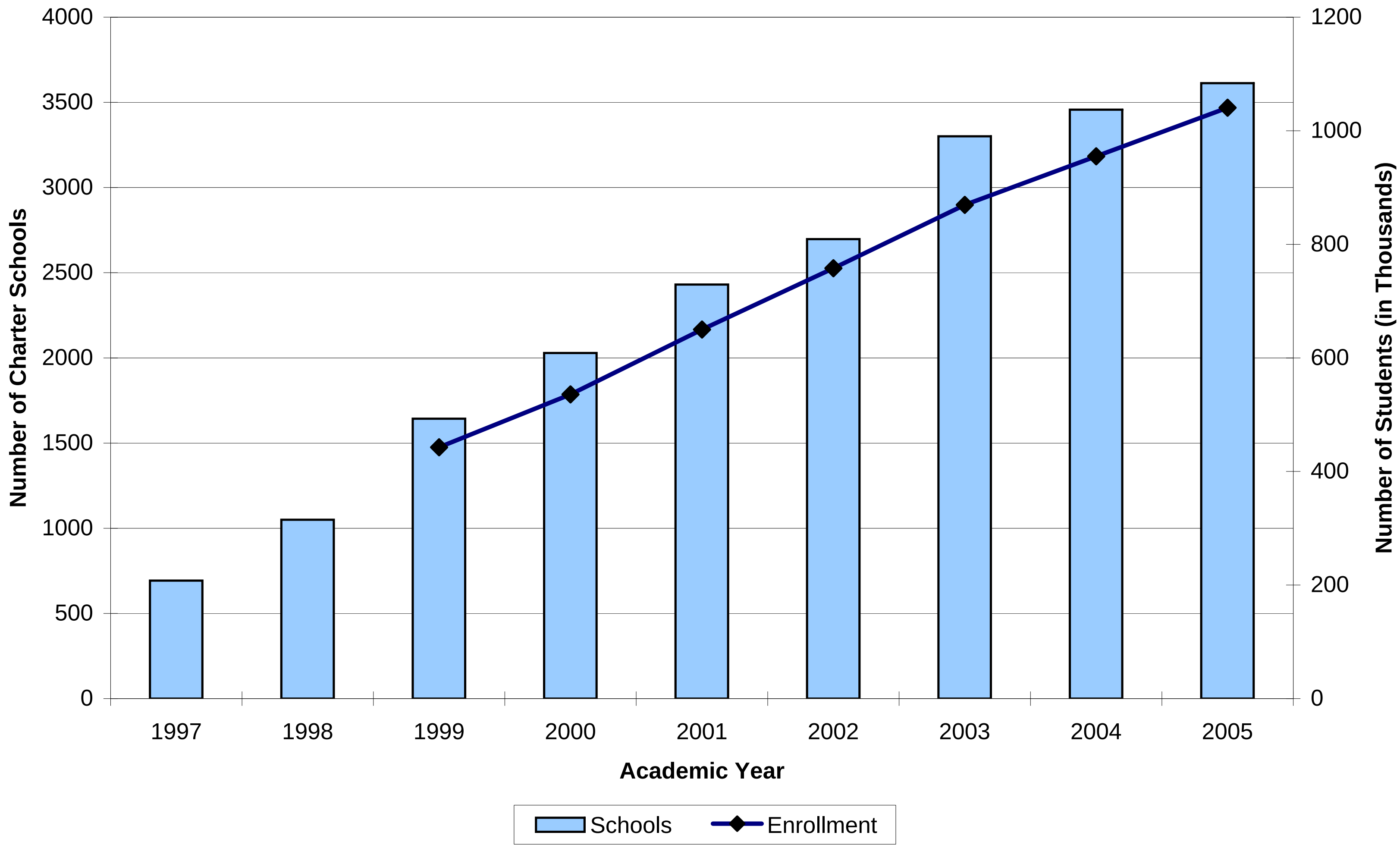
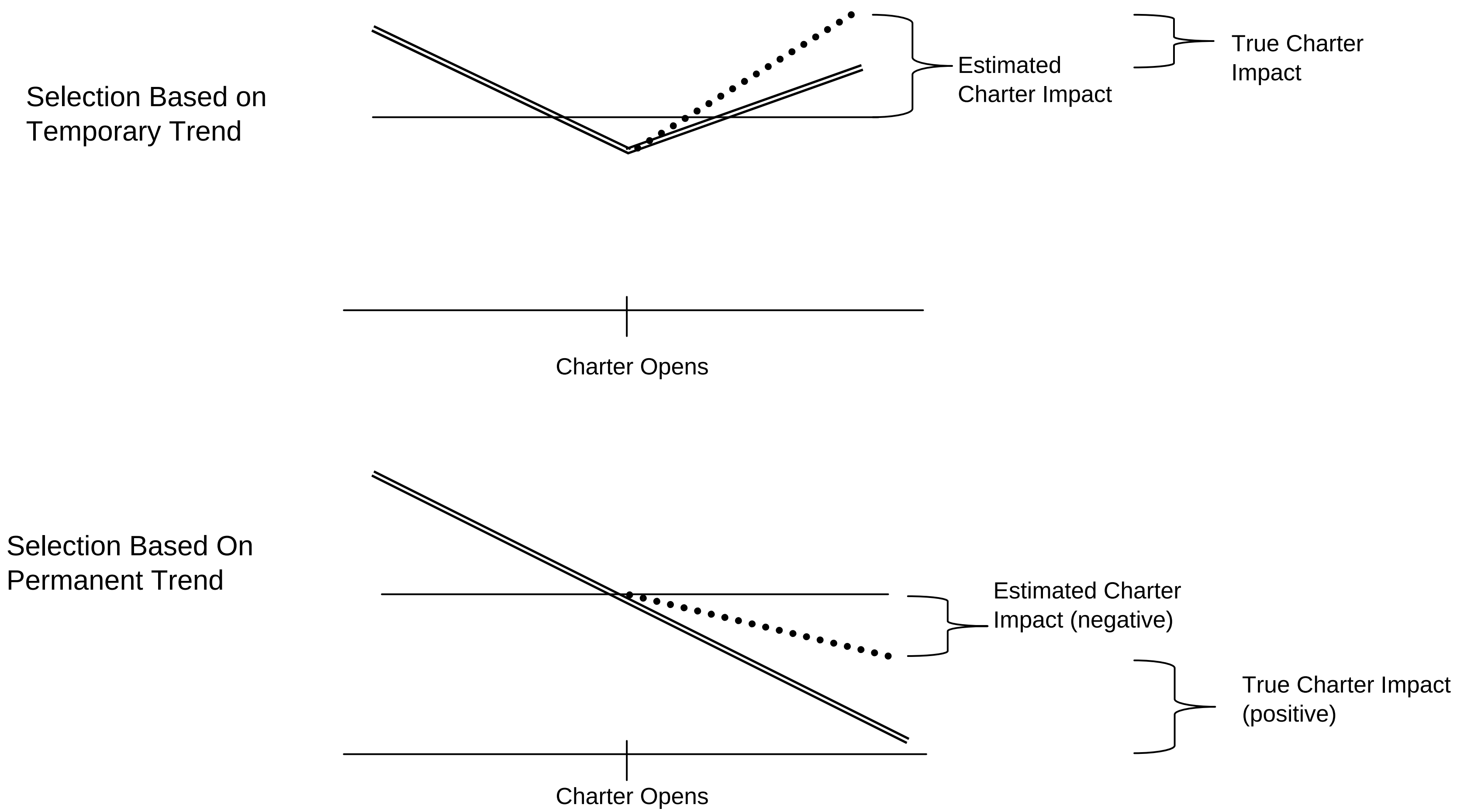
Figure 1: Charter Growth In the US



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.

Figure 3 - Fraction of Enrollment in ALUSD Area by Type of School and Year

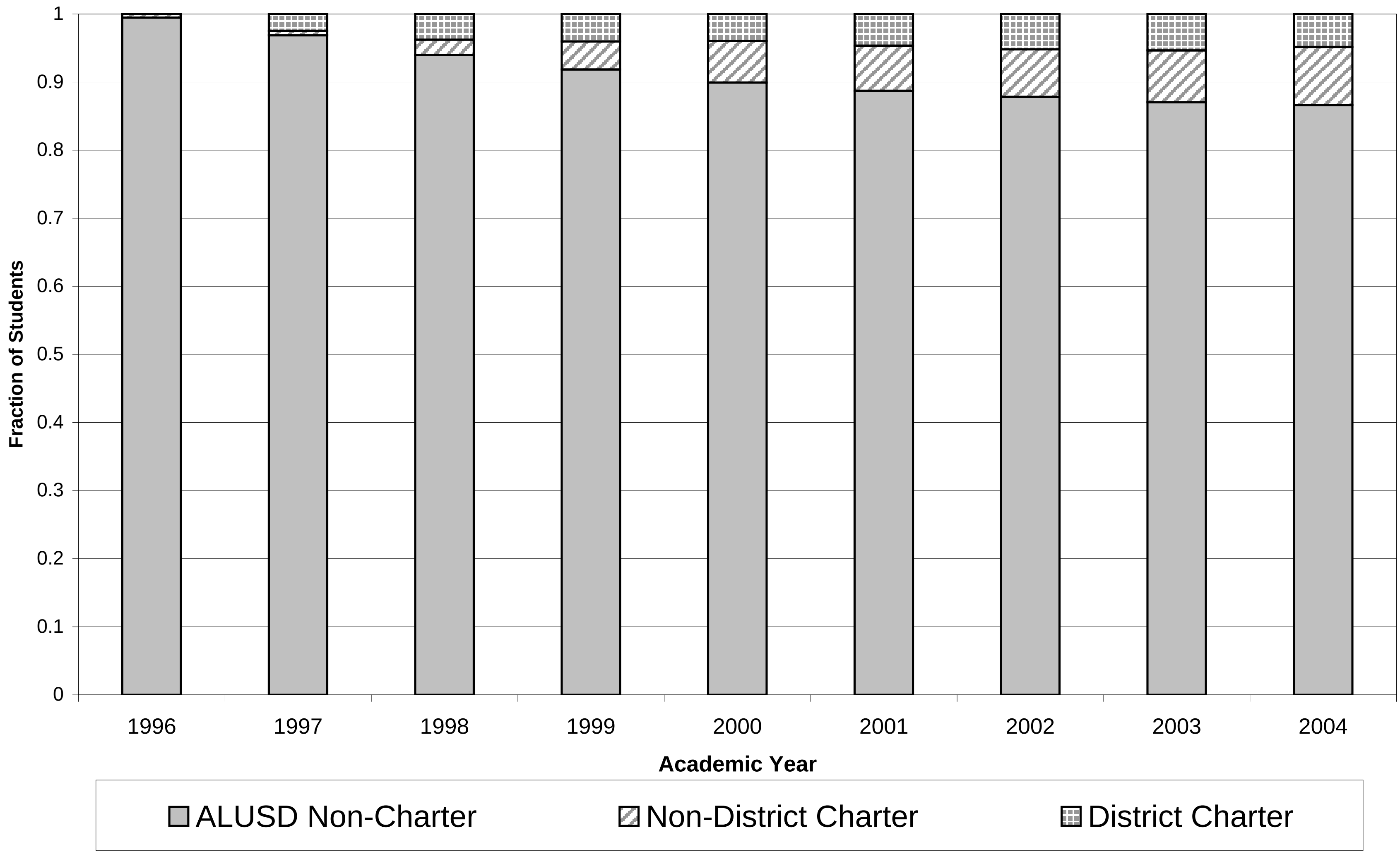


Table shows the fraction of students in each type of school in ALUSD and non-district charters in the region around ALUSD as defined by the state department of education.

Table 1 - School Characteristics in 2004

	ALUSD Non-Charters	Non-District Charters
Student Demographics (% of All Students in School)		
Limited English Proficient	30.3	10.9
		(6.3)
Economically Disadvantaged	86.0	70.9
		(5.1)
At-Risk	63.5	60.0
		(1.1)
Special Education	10.8	12.5
		(1.1)
Gifted	9.3	1.8
		(4.5)
White, Non-Hispanic	7.2	14.1
		(3.2)
School Demographics		
Teacher Experience (% of Teachers in School)		
0 - 5 Years	39.2	65.2
		(11.6)
6 or More Years	60.8	34.8
		(11.6)
Student-Teacher Ratio	16.2	17.2
		(1.8)
Per-Pupil Operating Expenditures	$6,916	$6,394
		(0.6)
Enrollment - All	895	373
		(7.5)
Enrollment - Elementary	730	400
		(6.5)
Enrollment - Middle/High	1352	286
		(6.0)
Student Outcomes		
Attendance Rate	95	91
		(3.3)
State Exam - Math		
% Passing at Low Level	62	42.0
		(5.7)
% Passing at High Level	15	7.4
		(4.2)
State Exam - Reading		
% Passing at Low Level	73	58.0
		(5.0)
% Passing at High Level	17	11.1
		(3.4)

Observations are school level aggregates. Total number of non-charter schools is over 200. Total number of district and state charter schools is over 40. Exact sample sizes cannot be provided due to confidentiality restrictions. Absolute t-statistic of mean relative to non-charter mean in parentheses.

Table 2: Regressions of Relationship Between Charter Penetration and School Enrollment

	1993 - 2004			1996 - 2004		
Charter Enrollment Within	(1)	(2)	(3)	(4)	(5)	(6)
0 - 1 Mile	0.016	0.029	-0.121*	0.024	0.029	-0.116*
	(0.119)	(0.130)	(0.051)	(0.121)	(0.130)	(0.051)
1 - 2 Miles	0.147	0.176	-0.077*	0.157	0.176	-0.067*
	(0.097)	(0.113)	(0.034)	(0.100)	(0.113)	(0.034)
2 - 3 Miles	0.132	0.163	-0.013	0.142	0.163	-0.009
	(0.087)	(0.104)	(0.023)	(0.090)	(0.104)	(0.022)
Year Fixed Effects	N	Y	Y	N	Y	Y
School Fixed Effects	N	N	Y	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 district and state charter schools is over 40. Observations total more than 2500 in 1993 - 2004 regressions and more than 2000 observations in 1996 - 2004 regressions. 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 3 - Characteristics of ALUSD Schools by Non-District Charter Penetration

Percentiles of Charter Penetration†	0 - 59	60 - 74	75 - 89	90 - 99
Range of Charter Penetration Rates	0.0% - 2.7%	2.7% - 6.0%	6.0% - 10.4%	10.4% - 43.5%
Demographics				
Female	0.486	0.485	0.472#	0.488
		[0.1]	[1.9]	[0.1]
White	0.085	0.091	0.071	0.096
		[0.3]	[0.8]	[0.4]
Economically Disadvantaged‡	0.813	0.803	0.823	0.748
		[0.3]	[0.4]	[1.6]
At Risk	0.571	0.601	0.649**	0.656*
		[1.4]	[3.4]	[2.4]
Limited English Proficient	0.264	0.262	0.319#	0.259
		[0.1]	[1.7]	[0.1]
Special Education	0.093	0.093	0.090	0.129
		[0.0]	[0.2]	[1.0]
Gifted & Talented	0.117	0.116	0.116	0.144
		[0.2]	[0.2]	[1.0]
Recent Immigrant	0.047	0.054	0.077*	0.082*
		[1.3]	[2.5]	[2.1]
Grade Level	4.444	4.402	4.697	6.357**
		[0.2]	[0.8]	[4.1]
Achievement				
Math NPR Score	48.789	50.512	49.247	46.976
		[1.2]	[0.3]	[0.8]
Reading NPR Score	48.820	48.904	48.005	46.287
		[0.1]	[0.5]	[1.0]
Language NPR Score	44.007	44.800	43.430	41.433
		[0.5]	[0.4]	[1.0]
Behavior				
# of Disciplinary Infractions	0.305	0.290	0.379	0.557**
		[0.3]	[1.0]	[2.9]
Attendence Rate (%)	94.883	94.966	94.253	91.744**
		[0.2]	[1.1]	[2.8]

† - Charter penetration is measured as the fraction of students within a 2 mile radius of a school and who are
‡ - Combination of students who qualify for free or reduced price lunch, or qualify for some other Federal anti-
T-statistics of difference from 1st quartile are in brackets and are based on standard errors clustered by school.
Covers 1998 - 2004 only, so that only years with a large number of charter schools are considered.
Observations total more than 1500. Exact sample sizes cannot be revealed due to confidentiality restrictions.

Table 4: Characteristics of Students Who Enter Charters or Leave ALUSD

	ALUSD Leavers and Stayers for Schools with an Overlapping Non-District Charter Within 2 Miles (Grades 1 - 8 Only)	
	Stayers	Leavers
Demographics		
Female	0.491	0.487
		[1.5]
White	0.092	0.110**
		[2.7]
Economically Disadvantaged‡	0.805	0.795
		[0.9]
Limited English Proficient	0.333	0.269**
		[6.3]
At Risk	0.561	0.537*
		[2.4]
Special Education	0.108	0.116
		[1.5]
Gifted & Talented	0.101	0.068**
		[4.2]
Recent Immigrant	0.077	0.090**
		[3.2]
Achievement		
Math NPR Score	49.022	44.963**
		[6.4]
Reading NPR Score	44.318	41.191**
		[4.5]
Language NPR Score	49.598	45.842**
		[5.8]
Behavior		
# of Disciplinary Infractions	0.324	0.476**
		[4.5]
Attendence Rate (%)	95.994	93.287**
		[9.6]

‡ - Combination of students who qualify for free or reduced price lunch, or qualify for some other Federal anti-poverty program.

T-statistics of differences in means in parentheses and based on standard errors clustered by school. Sample limited to base sample students from 1996 - 2002 in schools that never become charters, are in grades 1 - 11, and are enrolled in a school in year t which will still be in operation in yeart t+1. Includes over 500,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions.

Table 5: Fixed-Effects Estimates of Charter Schools on Non-Charter Students

	L-1	L-2	L-3	VA-1	VA-2	VA-3
# of Disciplinary Infractions						
1 Mile	-0.18	0.26	0.50*	-0.39	0.32	0.54*
	(0.29)	(0.20)	(0.21)	(0.38)	(0.21)	(0.24)
1.5 Miles	-0.05	0.21	0.58#	-0.47	-0.03	0.25
	(0.35)	(0.32)	(0.31)	(0.35)	(0.28)	(0.27)
2 Miles	0.74*	0.51	0.50	0.27	0.21	0.14
	(0.34)	(0.31)	(0.34)	(0.26)	(0.27)	(0.30)
Attendence Rate (%)						
1 Mile	-1.33	-0.44	-1.74**	-0.42	-1.59*	-1.79*
	(1.06)	(0.74)	(0.65)	(0.71)	(0.69)	(0.78)
1.5 Miles	-2.12#	-0.42	-0.84	-0.78	-0.93	-1.31
	(1.17)	(0.72)	(0.64)	(0.80)	(0.57)	(0.81)
2 Miles	-2.72**	-0.60	-0.51	-2.80**	-1.40	-1.78
	(1.04)	(0.85)	(1.18)	(0.75)	(1.04)	(1.51)
Math NPR						
1 Mile	-4.44**	-4.46*	-2.71	1.13	-2.05	-2.39
	(1.37)	(2.13)	(2.14)	(2.38)	(3.44)	(3.83)
1.5 Miles	-2.93*	-2.41	-0.37	1.70	4.59	4.20
	(1.39)	(1.74)	(1.75)	(1.68)	(3.30)	(3.82)
2 Miles	-1.77	-1.62	-0.94	2.64#	7.55**	6.29*
	(1.44)	(1.49)	(1.48)	(1.46)	(2.75)	(3.01)
Reading NPR						
1 Mile	-3.08*	-3.14	-0.29	2.36	2.34	2.69
	(1.33)	(2.27)	(2.58)	(1.78)	(2.62)	(3.12)
1.5 Miles	-3.76**	-2.36	2.76	0.62	5.78*	6.63*
	(1.20)	(1.76)	(1.74)	(1.30)	(2.45)	(3.04)
2 Miles	-2.21#	-1.41	1.86	1.08	5.84*	5.58*
	(1.25)	(1.44)	(1.80)	(1.33)	(2.45)	(2.84)
Language NPR						
1 Mile	0.07	-1.02	1.23	2.63#	0.11	1.18
	(1.57)	(2.23)	(2.27)	(1.53)	(2.26)	(2.82)
1.5 Miles	-0.09	-0.16	2.98#	1.97	4.99*	7.61**
	(1.18)	(1.53)	(1.72)	(1.30)	(2.26)	(2.81)
2 Miles	1.16	1.42	3.88*	0.87	4.98*	5.84*
	(1.12)	(1.41)	(1.56)	(1.28)	(2.26)	(2.46)
Student Fixed Effects	Y	Y	Y	Y	Y	Y
School Fixed Effects	N	Y	Y	N	Y	Y
School Specific Linear Time Trends	N	N	Y	N	N	Y

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, 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 6: First Stage IV Estimates of Effect of Charter Share on Non-Charter Students

Endogenous Variable: Share of students within X miles who attend charter school with overlapping grades.
Instrument: Post 1997 * # of buildings within X miles between 30,000 & 90,000 square feet

	(1)	(2)	(3)	(4)
Base Sample (1993 - 2004)				
1 Mile	0.045**	0.042**	0.039**	0.037**
	(0.007)	(0.006)	(0.008)	(0.008)
		[0.362]	[0.446]	[0.352]
1.5 Miles	0.028**	0.027**	0.030**	0.028**
	(0.006)	(0.005)	(0.006)	(0.005)
		[0.681]	[0.757]	[0.900]
2 Miles	0.019**	0.012**	0.007#	0.006
	(0.006)	(0.004)	(0.004)	(0.004)
		[0.021]	[0.066]	[0.030]
Test Sample (1998 - 2004)				
1 Mile	0.059**	0.071**	0.072**	0.075**
	(0.008)	(0.009)	(0.009)	(0.011)
		[0.204]	[0.186]	[0.178]
1.5 Miles	0.035**	0.046**	0.046**	0.040**
	(0.006)	(0.006)	(0.006)	(0.005)
		[0.050]	[0.047]	[0.404]
2 Miles	0.022**	0.009	0.010	-0.003
	(0.006)	(0.008)	(0.007)	(0.006)
		[0.068]	[0.079]	[0.000]
Zip Code Dummies	N	Y	Y	Y
Zip Code Trends	N	N	Y	Y
Census Tract Characteristics	N	N	N	Y

* Denotes jointly significant at the 1% level. Estimates are multiplied by 100 and thus should be interpreted as the increase in charter share for 100 building increase. 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. Huber/White standard errors clustered by school in parentheses. Brackets contain the p-value of a chi-squared test of differences in the coefficients in a seemingly unrelated regression. Base sample regressions contain over 1,200,000 observations. Test sample regressions contain over 800,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions.

Table 7 Second Stage IV Estimates of Effect of Charter Share on Non-Charter Students

Endogenous Variable: Share of students within X miles who attend charter school with overlapping grades.
Instrument: Post 1997 * # of buildings within X miles between 30,000 & 90,000 square feet.

	(1) Contemporaneous		(2) Two-Year Moving Average		(3) Three-Year Moving Average	
	IV-L	IV-VA	IV-L	IV-VA	IV-L	IV-VA
# of Disciplinary Infractions						
1 Mile	-5.88**	-6.08**	-6.73**	-6.49**	-6.36**	-6.13**
	(1.56)	(1.94)	(1.92)	(1.98)	(1.83)	(2.13)
1.5 Miles	-5.48**	-5.63**	-6.13**	-6.01**	-5.85**	-5.72**
	(1.77)	(2.05)	(1.96)	(2.12)	(1.95)	(2.19)
2 Miles	-3.85*	-5.55#	-4.79#	-6.35#	-4.01#	-5.54#
	(1.87)	(2.98)	(2.45)	(3.60)	(2.23)	(3.31)
Attendence Rate (%)						
1 Mile	-7.0	14.4	-6.7	25.8#	-4.6	21.6
	(14.3)	(12.0)	(12.8)	(15.0)	(11.3)	(13.5)
1.5 Miles	-10.0	9.9	-7.9	18.6#	-5.1	16.3#
	(10.7)	(8.5)	(10.1)	(11.0)	(8.4)	(9.8)
2 Miles	-10.0	10.3	-10.6	20.3	-8.1	15.7
	(10.0)	(8.8)	(11.1)	(13.8)	(8.7)	(11.0)
Math NPR						
1 Mile	-24.7*	2.2	-26.3*	2.0	-22.5*	6.9
	(10.1)	(11.0)	(11.2)	(12.3)	(11.5)	(12.3)
1.5 Miles	-37.6**	-15.2#	-37.3**	-16.2#	-35.8**	-14.2
	(11.4)	(8.7)	(11.9)	(9.5)	(12.6)	(10.4)
2 Miles	-35.8*	-13.1	-35.1*	-13.5	-32.7*	-12.0
	(14.3)	(9.5)	(14.5)	(10.1)	(14.5)	(10.8)
Reading NPR						
1 Mile	-10.5	7.6	-9.1	6.7	1.2	14.9
	(9.7)	(9.1)	(10.4)	(10.1)	(10.4)	(10.9)
1.5 Miles	-19.0*	-4.2	-17.0#	-5.3	-8.0	0.9
	(8.8)	(5.8)	(9.3)	(6.3)	(9.6)	(6.9)
2 Miles	-26.6*	-12.2	-24.2#	-13.2	-14.0	-6.5
	(12.7)	(7.4)	(12.6)	(8.1)	(11.3)	(7.7)
Language NPR						
1 Mile	-15.1	-3.6	-14.5	-4.8	-7.1	0.5
	(10.5)	(7.4)	(11.4)	(8.3)	(11.0)	(8.4)
1.5 Miles	-25.8*	-12.0*	-23.2*	-12.9#	-17.4	-10.4
	(11.1)	(6.0)	(11.3)	(6.6)	(11.2)	(6.7)
2 Miles	-28.8*	-13.6#	-24.3#	-14.0#	-17.2	-11.6
	(14.1)	(7.9)	(13.2)	(8.2)	(11.7)	(8.0)

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. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

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

Endogenous Variable: Share of students within X miles who attend charter school with overlapping grades
Instrument: Post 1997 * # of buildings within X miles between 30,000 & 90,000 square feet interacted with grade

	IV-L		IV-VA	
	Charter Share* Grades 1 - 5	Charter Share* Grades 6 - 12†	Charter Share* Grades 1 - 5	Charter Share* Grades 6 - 12†
# of Disciplinary Infractions				
1 Mile	-0.37	-8.20**	-0.72	-7.83**
	(0.86)	(2.55)	(0.89)	(2.93)
1.5 Miles	-0.83	-6.12**	-2.26	-5.98**
	(1.69)	(2.14)	(1.92)	(2.27)
2 Miles	1.27	-4.29*	-1.76	-5.78#
	(2.21)	(2.15)	(2.60)	(3.16)
Attendence Rate (%)				
1 Mile	-0.4	-9.8	0.5	19.0
	(3.6)	(19.0)	(3.6)	(16.4)
1.5 Miles	-7.9	-10.2	1.1	10.9
	(10.3)	(11.1)	(8.4)	(9.1)
2 Miles	-6.4	-10.3	2.3	10.8
	(10.1)	(10.2)	(9.1)	(9.2)
Math NPR				
1 Mile	-42.3**	-16.2	-24.1	12.4
	(13.9)	(12.6)	(18.5)	(16.3)
1.5 Miles	-115.3**	-25.7**	-81.8#	-8.3
	(36.3)	(9.8)	(45.2)	(9.6)
2 Miles	-116.7**	-24.3*	-99.0*	-3.7
	(36.5)	(12.4)	(43.2)	(11.1)
Reading NPR				
1 Mile	-62.7**	14.9	-26.0	20.7
	(14.5)	(11.6)	(16.6)	(14.4)
1.5 Miles	-139.2**	-0.6	-68.2#	2.4
	(40.7)	(8.1)	(38.3)	(6.4)
2 Miles	-158.7**	-7.8	-81.1*	-4.6
	(40.0)	(10.0)	(36.0)	(7.5)
Language NPR				
1 Mile	-56.3**	5.0	-23.5#	4.2
	(12.5)	(10.3)	(12.3)	(9.6)
1.5 Miles	-130.5**	-9.8	-70.6*	-6.0
	(36.8)	(8.2)	(31.1)	(5.7)
2 Miles	-142.8**	-12.5	-80.6**	-6.2
	(35.5)	(10.2)	(29.8)	(7.0)

† Grades 6 - 11 for exams.

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 - Charter Impacts on Non-Charter Student Controlling for School Characteristics - 1.5 Mile Radius Only

Instrument: Post 1997 * # of buildings within 1.5 miles between 30,000 & 90,000 square feet

	Levels				
	Disciplinary Infractions	Attendence Rate (%)	Math NPR	Reading NPR	Language NPR
	(1)	(2)	(3)	(4)	(5)
Share of students within 1.5 miles with overlapping grades who attend charter school.	-3.49**	-3.33	-27.28**	-6.86	-15.20#
	(1.33)	(7.22)	(9.85)	(7.79)	(9.07)
Per student expenditures ($ thousands)	-0.03**	-0.14*	-0.16*	-0.18**	-0.19**
	(0.01)	(0.06)	(0.06)	(0.05)	(0.05)
Total expenditures ($ thousands)	0.0000	0.0003#	0.0003	0.0003#	0.0003**
	(0.0000)	(0.0001)	(0.0002)	(0.0002)	(0.0001)
Enrollment (thousands)	0.05	-0.85*	-0.97	-1.35*	-1.40**
	(0.06)	(0.40)	(0.70)	(0.60)	(0.53)
Student - teacher ratio	-0.01*	0.05	-0.01	0.09#	-0.01
	(0.01)	(0.03)	(0.05)	(0.05)	(0.04)
Teacher Experience - % 0 years	-0.007**	0.016#	-0.093**	-0.046**	-0.074**
	(0.002)	(0.009)	(0.018)	(0.015)	(0.014)
Teacher Experience - % 1 - 5 years	-0.005*	0.020**	-0.062**	-0.058**	-0.053**
	(0.002)	(0.007)	(0.018)	(0.014)	(0.014)
Teacher Experience - % 6 - 10 years	-0.002	0.019*	-0.048*	-0.030#	-0.062**
	(0.002)	(0.008)	(0.023)	(0.016)	(0.016)
Teacher Experience - % 11 - 20 years	-0.002	0.013	-0.005	-0.013	-0.030*
	(0.002)	(0.009)	(0.023)	(0.018)	C

	Value Added				
	Disciplinary Infractions	Attendence Rate (%)	Math NPR	Reading NPR	Language NPR
	(6)	(7)	(8)	(9)	(10)
Share of students within 1.5 miles with overlapping grades who attend charter school.	-2.03*	1.50	-15.56#	-1.71	-11.79#
	(1.01)	(5.90)	(8.30)	(6.18)	(6.06)
Per student expenditures ($ thousands)	-0.04**	0.03	-0.04	-0.03	-0.02
	(0.01)	(0.06)	(0.08)	(0.05)	(0.05)
Total expenditures ($ thousands)	0.0000	0.0001	-0.0001	-0.0002	-0.0001
	(0.0000)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
Enrollment (thousands)	0.02	-0.32	0.34	0.59	0.00
	(0.07)	(0.40)	(0.81)	(0.58)	(0.54)
Student - teacher ratio	0.00	0.04	-0.10	0.00	-0.04
	(0.01)	(0.04)	(0.06)	(0.04)	(0.04)
Teacher Experience - % 0 years	-0.004*	0.007	-0.071**	-0.031#	-0.038*
	(0.002)	(0.009)	(0.024)	(0.018)	(0.018)
Teacher Experience - % 1 - 5 years	-0.003	0.017*	-0.026	-0.016	-0.015
	(0.002)	(0.008)	(0.022)	(0.015)	(0.014)
Teacher Experience - % 6 - 10 years	-0.002	0.006	-0.036	-0.035	-0.023
	(0.002)	(0.010)	(0.030)	(0.022)	(0.018)
Teacher Experience - % 11 - 20 years	0.001	0.003	-0.031	-0.006	-0.022
	(0.002)	(0.010)	(0.027)	(0.021)	(0.017)

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. Test score regressions contain over 300,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. **, *, and # denote significance at the

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.