

Achievement and Behavior in Charter Schools: Drawing a More Complete Picture

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Abstract: Charter schools are publicly funded schools which, in exchange for expanded accountability, receive more autonomy and experience fewer regulations than traditional public schools. From 1997 to 2006 the number of charters in the US grew from 693 to 3,977. Perhaps surprisingly, given this growth, previous work has found mixed evidence on the impacts of charter schools on student performance. However, these studies focus almost exclusively on test scores as the outcome of interest. Thus, one potential explanation for this discrepancy is that charter schools affect student performance in ways that cannot be measured by test scores. In this paper, I use new longitudinal data from an anonymous large urban school district to assess how charter schools affect student discipline, attendance, and retention and compare these to test score impacts. Using individual fixed-effects analyses I find that charter schools generate improvements in student behavior and attendance but the effects on test scores differ by subject. While I find evidence of selection into charter schools based on changes in outcomes, these results change little after applying interrupted panel strategies. Using Kyriazidou's (1997) estimator I also find that the results are robust to adjustments for endogenous attrition. Finally, I find little evidence that charter schools generate long-term benefits if students return to non-charter schools.

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1 Introduction

One of the fastest growing education reforms in the US today is the charter school movement. Charter schools operate under a contract, called a charter, with a government agency. These schools are provided a degree of autonomy from local school boards and freedom from some regulations in return for additional accountability requirements. Despite often being managed by private organizations, charters are public schools and receive almost all of their funding from government sources. Since 1997 the number of charter schools in the US has increased almost six fold, and the number of charter students has more than doubled since 1999, as is shown in Figure 1. Today, 1.15 million students nationwide attend charter schools.

One of the largest questions in the charter literature is how charter schools affect the outcomes of students who attend them. It is unclear whether charters are beneficial or detrimental to students on average. On one hand, charters have fewer regulatory burdens and are at higher risk of being shut down if they underperform, thus providing incentives to increase effort. On the other hand, charters have high levels of student turnover and eliminating some regulations may be detrimental to students. In addition to this theoretical ambiguity, the empirical evidence has been mixed. We might conclude from these studies that the effect of charter schools on academic performance is, at best, unclear. Why then does the number of charter students and schools continue to rise while survey and anecdotal evidence suggest that parents are generally satisfied with charters?² One potential explanation for this puzzle is that charter schools affect student outcomes in ways that researchers have not investigated. These alternative outcomes may be particularly important in light of recent evidence of how non-cognitive skills improve education and labor market outcomes (Heckman, Stixrud and Urzua, 2006; Jacob, 2002; Heckman and Rubinstein, 2001). In addition, work by Weiher and Tedin (2002) and Jacob and Lefgren (2005) suggest that parents are more concerned with discipline, safety, and student satisfaction than academic

²See Bulkley and Fisher (2003) for a brief review of the survey literature and for anecdotal evidence.

performance.

To my knowledge, no studies using individual panel data have looked at the effects of charters on discipline and attendance. In order to study these outcomes, along with retention rates, I use new data from an anonymous large urban school district (ALUSD). This district has one of the largest and oldest district-level charter programs in the US. It has provided me with discipline and attendance records for all charter and non-charter students from 1994-2004, along with test score records from 1998-2004. This offers me an opportunity to investigate how charter schools affect outcomes other than test scores and compare these results directly to test score impacts.³ I find that charter schools are effective at improving student behavior but effects on test scores vary by subject matter. Impacts on retention rates and attendance rates are not statistically significant. Thus, the missing information on these alternative outcomes could help explain the mixed results found in the literature.

In addition to considering non-test outcomes, I investigate whether impacts vary across different types of charter schools, since charters exhibit substantial amounts of heterogeneity. Thus, in addition to estimating average charter impacts, I consider the impacts of schools that begin as charters (startup charters) and those that convert from regular schools into charter schools (conversion charters) separately. While both types of schools are subject to additional accountability requirements and gain freedom from some regulations, conversions often retain the same staff and facilities after converting, while startups begin as completely new schools. Thus, the effects of these two types of charters could differ substantially. In addition, identifying whether these schools provide different impacts may have policy implications, since states and districts could allow only one type when starting a charter program. My findings show that discipline impacts are larger in startup charters than in conversion charters while test-score and retention impacts are similar. I also find evidence that suggests attendance improves in startup charters.

³ Note that from now on, I will refer to these outcomes collectively as "student performance."

Nonetheless, there are some potential problems with individual fixed effects analyses that could affect my estimates along with most of the recent work on charter schools. Luckily, the large size of the district I study and the long time span of the data provide me with the ability to study some of these problems in-depth and to account for them in ways that previous work has not been able to.

One potential problem is that the assumptions underlying fixed effects are invalid if students choose to attend charter schools based on changes in outcomes. If this occurs then the estimates of charter impacts may be contaminated by mean reversion. This phenomenon has been widely noted in the job-training literature (Heckman and Smith, 1999; Ashenfelter, 1978) while, in education, mean-reversion has been shown to occur in standardized exams (Chay, McEwan and Urquiola, 2005). Previous research has not found evidence of this phenomenon in charter schools, but this work only considers test scores. I find evidence that suggests there is selection due to changes in discipline, attendance, and test scores in charters. I use interrupted panel strategies (Hanushek, Kain, Rivkin and Branch, 2007; Hanushek, Kain and Rivkin, 2002) in order to mitigate the extent of this bias. When I use this strategy, discipline and attendance estimates are not substantially affected while the impacts on test scores remain mixed.

Another potential problem is non-random attrition. Many administrative datasets have individuals entering and leaving the data. A particular concern with respect to charter schools is that charter students may be more inclined to leave for private schooling than non-charter students. This could create bias if the reason charter students leave the district for these private schools is related to their performance in the charter schools. Although there is little evidence of this type of student movement, since it is difficult to track students as they enter private schools, Hanushek, Kain, Rivkin, and Branch (2005) find that charter students leave Texas public schools at more than 2.5 times the rate of non-charter students. Thus, differential attrition could be a substantial problem if the underlying causes of attrition are correlated with outcomes. To address non-random attrition I use Kyriazidou's (1997)

estimator for sample selection in panel data models. I find little to suggest that non-random attrition has a substantial effect on the charter impact estimates. In addition, I also generate false data for attrited students under various assumptions to test the sensitivity of the discipline results to attrition. These suggest that only under the most extreme assumptions could endogenous attrition eliminate the discipline impacts.

A third complication arises if charter schools affect students after they return to non-charter schools. In this case, fixed effects estimates may be biased since these "persistent" outcomes will be applied to periods when the charter indicator equals zero. In addition, whether or not charter school impacts are long-term is relevant to policy. For the foreseeable future, the stock of charter schools in the US will be small relative to non-charters. Thus most students who enter charters in elementary and middle school will return to non-charter schools before leaving the public school system. If charters provide short-term benefits but no long-term benefits, the usefulness of these schools for generating human capital improvements will be limited. The long time coverage of my data allows me to measure the extent of this problem by conducting regressions with lagged measures of charter status. I find little evidence of persistence in charter impacts after students return to non-charter schools. Nonetheless, even if charter schools generate only temporary performance improvements, they also tend to spend less money than non-charter schools. In 2002, median per-student expenditures for charter districts were 13% lower than in non-charter districts.⁴ Thus, if charters provide the same level of long-term performance and cost less money, they still enhance the efficiency of the education system.

Overall, these results imply that charter schools in ALUSD provide improvements in student discipline and attendance with mixed effects on test scores. However, these impacts are only temporary. While these results are not necessarily representative of charter schools in other states and districts, they generate two important implications for the charter literature. First, they provide evidence that individual fixed effects strategies are robust to multiple bias

⁴National Center for Education Statistics, School District Finance Survey.

reducing procedures, suggesting that this econometric strategy is appropriate in the charter context. Second, they highlight that the singular focus of the charter literature, and many other branches of the economics of education, on test scores misses key pieces of information which could lead to erroneous policy recommendations.

2 Background on Charter Schools

2.1 Previous Literature

Research on the effects of charter schools on charter students has been mixed overall. Of the papers which use individual panel data, some researchers find insignificant or negative impacts of attending a charter school (Hanushek, Kain, Rivkin and Branch, 2007; Bifulco and Ladd, 2006; Sass, 2006; Zimmer and Buddin, 2003), while others find positive impacts (Booker, Gilpatric, Gronberg and Jansen, 2007; Hoxby and Rockoff, 2004; Solmon and Goldschmidt, 2004; Solmon, Paark and Garcia, 2001).

With the exception of Solmon and Goldschmidt (2004) who look at retention, all of these papers only investigate the impacts on test scores. However, student "performance" could encompass an array of outcome measures in addition to academic achievement such as behavior, attendance, and social skills. These non-cognitive outcomes have been shown to play important roles in educational attainment and job market success (Heckman, Stixrud and Urzua, 2006; Jacob, 2002; Heckman and Rubinstein, 2001). Other research suggests that parents care about non-academic outcome measures when they make decisions regarding their children's schooling. Weiher and Tedin (2002) survey charter parents in Texas and find that only 22% cite test scores as the most important reason for sending their children to charter schools while 38% specify discipline or safety and 26% cite moral values. Jacob and Lefgren (2005) study parents' preferences when choosing teachers and find that for most parents their children's satisfaction is more important than academic performance. If charter schools seek to improve these alternative outcomes then they may shift resources

away from improving test scores. Such a phenomenon could partially explain the range of estimates of charter effectiveness that researchers have found.

All of the previously cited papers on charter schools use individual fixed effects or similar analyses except for Hoxby and Rockoff (2004).⁵ Thus, another potential reason that these estimates are inconclusive is that there could be aspects of charter schools which generate violations of the assumptions that underlie fixed effects analyses, and hence could lead to bias.

2.2 Charter Schools in the United States

Charter schools have become relatively commonplace across the US since the first states enacted charter laws in the early 1990's. Today approximately 2.2% of public school students attend charter schools. Charters are more common in urban areas than suburban or rural. In 2003, the most recent year detailed national charter data is available, charter students were more than twice as likely to reside in urban areas than non-charter students.⁶

Although it is common in charter research to classify charters homogeneously, there is substantial heterogeneity across schools in how they are managed, their goals and aims, the student populations they cater to, and their level of independence from local school systems. Perhaps the most significant difference between charters is to whom they are accountable. Every charter school has a relationship with some government institution. However, this can be a local school district, state or county government, independent chartering board, or a university. As of 2003, 51% of all charter students were in a school chartered by a local school district.⁷

A second important distinction to make between charter schools is whether they are new schools (startup charters), or if the schools were previously non-charter schools that switched to charter status (conversion charters). Understanding this distinction may shed

⁵Hoxby and Rockoff (2004) use oversubscription lotteries to identify charter impacts.

⁶Common Core of Data, National Center for Education Statistics, US Department of Education.

⁷Common Core of Data, National Center for Education Statistics, US Department of Education.

light on the mechanism through which charters affect student outcomes since attending a conversion charter may be a less substantial change than attending a startup charter. When a school converts to charter status it usually remains in the same building and keeps the same teachers, administrators, and students. In addition, most students continue to attend conversions because they are assigned to the school based on the location of their residence. Thus, comparing conversion charters to startups gives us insight into how reducing regulations and providing autonomy alone, without an influx of new staff or facilities, affects student performance. Different impacts between these two charter types may also have policy implications, since some districts and states could permit only one type of charter school. This distinction has been the subject of some previous research suggesting that the effects on student achievement differ across these two types of schools (Sass, 2006; Buddin and Zimmer, 2005; Zimmer and Buddin, 2003).

Despite these differences, there are a number of similarities that are present in nearly every charter. First, charters are often exempt from many regulations. These can range from the relaxation of minor regulations such as being able to adjust the length of the school-day or provide classes on weekends, to relaxing more fundamental regulations such as teacher certification and unionization rules. Second, in the case of startup charters, parents have the option to enroll their child in a charter school or in their assigned public school. This means that startup charters need to attract students or risk being closed down. Third, charter schools gain autonomy from the administration of the local school district. The extent of this can range from complete autonomy to allowing school officials more flexibility to manage the school as they see fit. Fourth, charters are more able to focus on certain student groups, such as at-risk students, or on particular subjects, such as fine arts. Last, charters often receive less money per-student from tax revenues than the local public schools do, though the extent varies by state. For example, charter schools in Michigan get 100% of the state and local per-student funding level while Pennsylvania charters get only 70%-82%.⁸

⁸Center for Education Reform.

Although charter schools have a number of advantages that may generate improvements in student performance, there are some disadvantages as well. Thus, net impacts are theoretically ambiguous. While there are many ways that charters may affect students, there are a few mechanisms that are particularly important. The first is freedom from regulations. Charter proponents argue that reducing regulations makes it easier for schools to innovate and experiment. However, this does not necessarily improve student performance since the experiments could turn out poorly. Charters also may be reluctant to abandon an ineffective experimental strategy if there are high fixed costs to changing, such as for retraining teachers. In addition, some regulations, such as teacher certification, may be helpful.

Another argument made by charter proponents is that charter schools perform better because they are at some risk of losing their charters. This could be a powerful incentive for charter administrators and teachers to put more effort into improving student performance, since they need to show improvement to keep their jobs. The involuntary loss of a charter usually occurs for one of three reasons - low enrollment, revocation by the chartering authority, or financial problems. While the first two reasons provide incentives to exert more effort, the third may force schools to cut spending, potentially reducing performance. Unfortunately, it is difficult to determine how common involuntary losses of charters are since national data on charter schools is very limited. Nonetheless we can identify an upper bound by looking at overall closure rates for charters, which between 2000 and 2004 averaged 5.0% per year compared to a closure rate in non-charter public schools of 1.8% during this period.⁹

While researchers have generally thought about how these characteristics of charter schools may affect academic outcomes, they also could play a role in non-academic outcomes. For example, many charters are permitted to require students to wear uniforms.

⁹Author's calculation from Common Core of Data, National Center for Education Statistics, US Department of Education. A school is considered to have closed if it is classified as operational in year $t - 1$ and is no longer classified as such in year t .

Most traditional public schools do not have this ability. These uniforms may reduce misbehavior and violence in schools by, for example, preventing students from displaying gang colors. Charters may also provide innovative techniques to improve student behavior such as by maintaining longer hours to keep children occupied during late afternoons or providing monetary rewards for high attendance.

2.3 Charter Schools in ALUSD

ALUSD was one of the first school districts in the US to institute a charter program. Although the program has been in existence since 1996, it did not start in earnest until 1997. Half of the charter schools created to date by ALUSD were started in 1997 or 1998. Today there are more than twenty charter schools in ALUSD along with over 200 non-charter schools.¹⁰ There is also a large number of non-district charter schools in the ALUSD area. Figure 2 shows the evolution of the charter program in ALUSD by examining the fraction of enrollment by school type. As of the 2004-2005 school year nearly five percent of students in the ALUSD area attended a district charter school while 8.5% attended a non-district charter.¹¹ Charter students in ALUSD are also more likely to be in grades below high school.

All of the charter schools I study are chartered by the ALUSD district. Nonetheless, Table 1 provides some information aggregated to the school level about district startup, district conversion, and non-district charters as well as non-charter ALUSD schools. The schools that convert are poorer and have more minorities than non-charters while district startups are on-par with non-charters and non-district charters are wealthier with fewer minorities. Startups and non-district charters also have lower passing rates for state exams and lower attendance rates than non-charters while conversion charter outcomes are better than for

¹⁰Due to risk of revealing the district, I cannot provide the exact number of schools in ALUSD.

¹¹Since 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 of 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.

non-charters. All three types of charters have lower rates of limited English proficiency (LEP), have less experienced teachers, are smaller, and spend less money per-student than non-charters. However, for outcome measures it is unclear how much of the differences are due to composition effects or charter impacts.

3 Data

In this paper I utilize a new set of 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, coursework, 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 1994-1995 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 for early education, pre-kindergarten, and kindergarten, 55% of students who are first observed in the data prior to ninth grade have at least four observations. In addition, 65% of charter students have a pre-charter observation and only 20% have neither pre nor post-charter observations. A drawback of this dataset, however, is that I do not observe students in non-ALUSD charter schools within the district's geographic boundaries.

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

¹²Norm-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.

observations are excluded due to missing attendance data ($<0.1\%$), leaving more than 1.2 million observations of which more than 50,000 are students in charter schools.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 exams 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 so I do not include them in the analysis.¹⁴ The final test sample includes over 900,000 student-year observations, approximately 40,000 of which are students in charter schools.

Table 2 provides summary statistics for the base sample. There are a number of differences between charter students and non-charter students in ALUSD. Charter students tend to be less wealthy, are less likely to be at-risk or limited English proficient, and perform better than non-charter students on every outcome measure listed. Comparing conversion charters to startups, startup students are more likely to be minorities, less likely to be limited English proficient, more likely to be at-risk, less likely to be gifted, and perform worse than conversion students on every outcome measure considered in the table except disciplinary

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

¹⁴Twenty-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 web appendix.

infractions.¹⁵

4 Baseline Empirical Strategy

Since most charter schools are schools of choice, it is likely that parents send their children to charters for reasons that are unobservable to the econometrician. We may be particularly concerned that students who enter charters differ from non-charter students in terms of unobserved ability, parental motivation, or tendency to misbehave. The summary statistics in Table 2 suggest that in ALUSD lower ability students enter startups and higher ability students attend conversions. If this selection is not properly addressed then my estimates of the charter impacts may be biased.

In the absence of a natural experiment or the ability to use an instrumental variables approach, charter researchers have turned to panel data methods. Following this line of research, I use individual fixed effects strategies to assess the effectiveness of charter schools in ALUSD. However, this strategy has some limitations. Three complications that may be important are selection based on changes in outcomes, non-random attrition, and the persistence of charter effects. Hence, I separate the main analysis into two sections. In this section I set up the baseline fixed effects strategy. In the next section, I explain how each of the previously stated complications could generate bias and I provide estimates that account for each of them.

If the effect of attending a charter on outcomes is constant across individuals then my goal would be to estimate the effect of attending a charter school in ALUSD on any student - the treatment effect (TE). However, treatment effects are likely to vary across individuals and schools. Thus, I aim to estimate the average effect of treatment on the treated (ATT)

¹⁵Test scores are measured by national percentile ranking, which is the percent of students in a nationally representative sample of test takers who scored lower than the observed student.

instead. The ATT is defined as

$$(1) \quad ATT = E(y_{it}^1 | c_{it} = 1) - E(y_{it}^0 | c_{it} = 1)$$

where c_{it} is an indicator of whether a student is a charter student, y_{it}^1 is the outcome while enrolled in a charter and y_{it}^0 is the outcome while not enrolled in a charter for student i in year t . It is not possible to calculate (1) since an individual cannot be enrolled in a charter and enrolled in a non-charter at the same time. Thus, we need to find a counterfactual group that will provide us with an accurate approximation of $E(y_{it}^0 | c_{it} = 1)$. The simplest solution would be to use the outcomes for students who do not attend charters as the counterfactual, $E(y_{it}^0 | c_{it} = 0)$. This is sufficient if students are assigned to charter schools randomly. However, parents and students choose whether to enroll in charters. If this choice is correlated with y_{it}^0 then $E(y_{it}^0 | c_{it} = 0) \neq E(y_{it}^0 | c_{it} = 1)$ and any attempt to estimate ATT using this counterfactual will be biased.¹⁶

In order to address the bias in the comparison group one could condition on a set of observables \mathbf{X}_{it} to control for observable differences between treatment and comparison groups, but this still leaves the possibility that the choice of c_{it} will be caused by y_{it}^0 through some omitted factor. The availability of panel data provides me with a strategy that may correct this problem. If the decision to attend a charter is not correlated with unobserved characteristics of students that vary over time then the ATT can be identified by

$$(2) \quad \theta = E(y_{it}^1 | c_{it} = 1, \mathbf{X}_{it}, \phi_i) - E(y_{it}^0 | c_{it} = 0, \mathbf{X}_{it}, \phi_i).$$

where ϕ_i is an time-invariant individual specific effect. Under the additional assumption of

¹⁶One strategy to correct for this is to use data on oversubscription lotteries (Cullen, Jacob and Levitt, 2006; Hoxby and Rockoff, 2004). However, the small number of such lotteries that are available make such an analysis infeasible in most datasets, including ALUSD, which has not had any lotteries. Another strategy that has been used for a similar school reform in Britain (Clark, 2005) is to see how schools that barely vote to switch to charter status compare to those that barely fall short. However, in ALUSD schools choose to convert to charter status by petition rather than election, and thus there is no information on those schools that do not get enough signatures to convert.

strict exogeneity which states that the outcome measure is uncorrelated with charter status and exogenous characteristics in past or future periods, or

$$(3) \quad E(y_{it}|c_{i1}, \dots, c_{iT}, \mathbf{X}_{i1} \dots \mathbf{X}_{iT}, \phi_i) = E(y_{it}|c_{it}, \mathbf{X}_{it}, \phi_i)$$

we can estimate θ consistently using individual fixed effects. In addition, the estimate of θ , $\hat{\theta}$, has a causal interpretation. Thus, initially, I estimate θ using the following regression equation:

$$(4) \quad y_{it} = \alpha + \theta C_{it} + Demog_{it}\Gamma + Switch_{it}\Phi + Gradeyear_{it}\Psi + \phi_i + \epsilon_{it}$$

where y_{it} is some outcome measure for student i at time t such as discipline or changes in test scores, c_{it} is an indicator of charter status, $Demog_{it}$ is a vector of time-variant observable demographic characteristics, $Switch_{it}$ is a set of variables that define whether a student changes schools in year t , $Gradeyear_{it}$ is a set of grade-by-year indicator variables which account for changes in outcomes over time and grade level, ϕ_i is defined as above, and ϵ_{it} is i.i.d. error. This equation can also be modified such that C_{it} contains indicators for multiple types of charters ($C_{it} \equiv [C_{conv}, C_{start}]'$ and $\theta \equiv [\theta_{conv}, \theta_{start}]$) so that the average effect of treatment on the treated can be calculated for different types of charter schools.

Two recent papers (Ballou, Teasley and Zeidner, 2006; Hoxby and Murarka, 2006) have raised concerns regarding the validity of using the individual fixed effects strategy to identify charter effects. Thus, I would like to briefly outline how I address some of the problems they raise. The largest concern these papers have is that by using fixed-effects, the charter impact is identified by using only those students who switch between charter and non-charter schools and thus may not be representative of all charter students. In the ALUSD data, this concern is mitigated by the fact that 80% of charter students have at least one non-charter period and thus, most of the charter students are identified in the regressions. In addition,

the long time-span and the fact that grades one through eleven are tested in ALUSD, ensures that the identified sample is more representative of charter students in the district overall than the samples used in previous research. A second concern they have is that endogenous switching based off of temporary shocks could bias the estimates. The interrupted panel strategy I use in the next section addresses this problem. A third concern is that the fixed effects analyses drastically reduces the size of the identified sample, making estimates imprecise. However, the ALUSD data includes a large number of identified charter students - 24,000 in the base sample. Thus, my estimates are reasonably precise.

Hoxby and Murarka also argue that using oversubscription lotteries to identify charter effects is a superior strategy to fixed-effects regressions. While they are correct that a lottery based strategy has substantial advantages over fixed-effects, there are two important aspects of lotteries that may be undesirable. The first is that, since oversubscribed schools are likely to be of higher quality than schools with spaces available, a comparison of lottery winners and losers will only identify the impacts for the best charter schools. While this is useful information if we are trying to see whether charters can, in ideal situations, be effective, it only generates as an upper bound estimate of *ATT*. Second, lotteries may be subject to substantial attrition bias, since parents who lose lotteries may be more likely to send their children to private school than those who win. Since sending a child to private school is correlated with the parent's wealth, motivation, and interest in their children's education, this would leave students with less motivated and poorer parents in the comparison group, generating an upwards bias in the charter impact estimates.

Another issue that has arisen in charter research is whether one should analyze test score levels or annual changes. Most charter research uses the latter when panel data are available. The reason is that, even after accounting for innate ability with fixed effects, test scores reflect both knowledge stock and flow. For example, suppose test scores are defined

by

$$(5) \quad y_{it} = \gamma_0 y_{i,t-1} + \gamma_1 x_{it} + \gamma_2 z_i + \nu_{it}$$

where $y_{i,t-1}$ is lagged test scores, x_{it} represents time-variant characteristics of an individual such as what school she attends in year t , z_i represents time-invariant characteristics, and ν_{it} is a random shock. The reason $y_{i,t-1}$ is included in this equation is that educational input from previous years also plays a role in current test scores. For example, a student cannot pass an algebra test if he never learned how to do arithmetic. Thus, in order to ensure that the test scores reflect the added value of the student's current school, we need to account for this stock component of achievement. One strategy would be to include lags of the outcome variable in the regression, but lagged dependent variables are generally endogenous. Thus, a common solution is to restrict $\gamma_0 = 1$ so that

$$(6) \quad y_{it} - y_{i,t-1} = \Delta y_{it} = \gamma_1 x_{it} + \gamma_2 z_i + \nu_{it}.$$

Therefore, using this value-added framework, we difference out the contribution of previous schools to student test scores.

While this procedure seems reasonable for test scores, it does not necessarily extend to other outcomes. Consider the case of discipline. One could make the argument that discipline has a much stronger relationship with a student's current environment than past schooling environments (i.e. $\gamma_0 \approx 0$). However, one could also reasonably argue the opposite. This same situation applies to attendance as well. Thus, while I consider value-added models for test scores, I study both levels and value-added models for discipline and attendance. For retention I only consider levels.

Unless specified otherwise, all regressions in this paper include the grade-by-year indicators along with the time-variant demographic characteristics - whether the student is eligible for free lunch, is eligible for reduced price lunch, has some other economic disadvantage, has

immigrated within three years, and whether one of the student's parents is a migrant worker.

I also include a measure of student mobility in the model ($Switch_{it}$). Previous research has shown that switching schools can have a detrimental effect on performance (Hanushek, Kain and Rivkin, 2004). To account for this, I follow previous work on charter schools by controlling for whether a student switches schools in a given year (Booker, Gilpatric, Gronberg and Jansen, 2007; Hanushek, Kain, Rivkin and Branch, 2007; Bifulco and Ladd, 2006). In addition, I split school switches into "structural" and "non-structural" switches where the latter is defined as switching into a school that less than ten percent of a student's previous class switches into in year t . Conversely, a student undergoes a structural switch when more than ten percent of his or her previous class switch into the same school in year t . This is the same definition used by Bifulco and Ladd (2006). Since ALUSD has a liberal space-available transfer program, non-structural switches could result from students changing addresses or transferring schools. I also define students as non-structural switchers during the year when they enter the base sample, except for those who enter during first grade.¹⁷ Thus, 21% of student-years undergo non-structural switches (10% of student-years are non-structural switches between two ALUSD schools) and 10% of student-years undergo structural switches.

The reason I make the distinction between structural and non-structural switches is that a structural switch is likely to be exogenous while non-structural switches are choices made by the students and parents. In this sense charter students are similar to those who make non-structural switches between non-charter schools, and it is possible that the two types of switches have different effects on charter impacts. In addition, the fact that non-structural switching is a choice variable has implications for the interrupted panel estimates I provide in the next section.

Table 3 provides regression estimates of the model in equation (4). The standard errors

¹⁷I can identify whether students switch in 1994, the first year of data I use in the analysis, based on information on the schools they attended in 1993.

for each regression are robust to heteroskedasticity and clustered by school.¹⁸ In column one I group all charters together into one indicator variable. There is a statistically significant reduction in both level and value-added measures of disciplinary infractions, a statistically significant improvement in math test score changes, and a statistically significant drop in reading. Impacts on attendance rates, retention rates, and language test impacts are not statistically significant.

These results hide a substantial amount of heterogeneity. Column 2 shows the same regression, but the charters are split into conversions and startups. The two types of charters show similar patterns in the estimates but the magnitudes differ substantially. For example, most of the discipline improvements from column one occur in startup charters. The drop of 0.79 infractions per year when students enter a startup charter is equal to 69% of the mean infraction rate in the year prior to startup entry. For attendance, neither type of charter produces a statistically significant effect on levels but students who attend startup charters show improvements in value-added attendance of 2.5 percentage points relative to a baseline absentee rate of eleven percent in the year prior to startup entry. This impact is statistically significant at the ten percent level. Turning to other results, there is no statistically significant change in retention rates in either type of charter. The only significant effect on test scores is for math scores in conversions.¹⁹

While the discipline results for startups are dramatic, since they are based on a measure that can be manipulated by the charter schools there is a question as to whether these are real behavioral changes or the result of charter schools being more lenient with students. Nonetheless, there are a few reasons to believe that these reflect real behavioral changes in the students. First, when I run regressions that focus on severe infractions - substance abuse and

¹⁸Some campuses are contained within a group of schools with the same administration. Thus, for the purposes of standard error clustering I consider campuses within a school group to be one cluster. For other purposes they are classified as separate schools.

¹⁹I also found the baseline results to be similar for test score levels and for test scores measured by raw scores and normal curve equivalents. They are also robust to reweighting the sample by number of days enrolled. These results are available from the author upon request.

criminal activity - I find similar results.²⁰ Since the margin I am considering is the number of in-school suspensions or more severe punishments, then we should only see reductions in these types of infractions if there are real behavioral improvements since principals would be very unlikely to punish students for these infractions with less severe punishments in a systematic manner. Second, I also find statistically significant reductions in expulsions and the likelihood of having any infraction, so the results are consistent across different margins. Third, I will show later that there are significant improvements in attendance when one accounts for persistence. Since attendance is highly correlated with behavior and is much harder to misrepresent we would expect there to be improvements in behavior based on these results alone.²¹ Finally, at seven times the standard error, the results are very large and would require a large amount of leniency in order to make the estimates statistically insignificant.

Why are the results different for conversion charters and startup charters? One potential explanation could be that there is little benefit to freeing schools from regulations without providing new staff and facilities. However, this does not explain why math test scores improve in conversions but not startups while discipline and attendance improve more in startups. Another potential explanation is that charters tend to focus on particular aspects of student performance. That is, perhaps startups try to specialize in helping children with behavioral problems while conversions focus more on academic performance. Even if this is the case in ALUSD, it is not clear if this is due to a random assignment of each focus across the two types of schools or if there is some systematic reason that startups focus on behavior (i.e. perhaps parents are more willing to change their children's schools if they are misbehaving or are in an unsafe environment then if they are simply not performing well academically but are well-behaved). A third potential explanation is that there may be aspects of the parents' decision making processes when choosing to send their children to

²⁰These results along with a more detailed description can be found in the web appendix.

²¹The district's auditing policy for attendance is to check the reported attendance against individual teachers' log books. Thus, in order to falsify attendance rates a school would need the participation of both administrators and a large number of teachers in the scheme.

a charter, or, for those whose children already attend charters, when choosing whether to exit the charter, that could bias the estimates due to failures of strict exogeneity. The next section addresses this issue in detail.

5 Correcting for Three Potential Sources of Bias

5.1 Selection Into Charters Based on Pre-Charter Outcomes

Researchers have been concerned about the possibility that selection of students into charter schools is based on changes in the dependent variable, or changes in unobserved factors that could affect the dependent variable, in which case fixed effects estimates will be inconsistent (Booker, Gilpatric, Gronberg and Jansen, 2007; Hanushek, Kain, Rivkin and Branch, 2007; Bifulco and Ladd, 2006; Sass, 2006). In particular, we may suspect that students select into the charter school due to a change in test scores or discipline, or a change in some strong correlate with these outcomes. Such a situation has been widely noted in the job-training literature and is commonly called "Ashenfelter's dip" (Heckman and Smith, 1999; Ashenfelter, 1978). Since a parent may see a drop in performance as an indicator that the current school does not meet his or her child's needs, it is reasonable to believe that students change schooling environments in response to poor performance. If this is true, then the strict exogeneity assumption is violated since $E(y_{it}|c_{it}, \dots, c_{iT}, \mathbf{X}_{i1}, \dots, \mathbf{X}_{iT}, \phi_i) \neq E(y_{it}|c_{it}, \mathbf{X}_{it}, \phi_i)$; i.e. y is correlated with future c . In addition, if the outcome measures exhibit mean reversion then fixed effects would tend to overestimate the charter impacts, since this would generate spurious improvements in outcomes at the time of charter entry.

Figures 3A and 3B investigate whether this phenomenon occurs in ALUSD with respect to discipline and attendance. Figure 3A shows how these outcomes change in the years prior to charter entry in grades four and five or grades six through eight for both conversions and startups. An additional line shows students in these grades who are not observed in charters at any time from 1994-2004 and do not make non-structural switches during the

grades listed at the top of each graph. Figure 3B shows the same outcomes for students who undergo a non-structural switch between traditional schools. All outcome measures in these graphs are demeaned within individuals then regression adjusted for free lunch status, reduced-price lunch status, other economic disadvantage, recent immigration status, parents' migrant status, and grade-by-year effects.

In Figure 3A, there is a noticeable drop in attendance rates and an increase in disciplinary infractions in the year or two prior to entry into startup charters. There are also similar "dips" for conversion charters, although the magnitude is far lower. However, in Figure 3B we see the same patterns for non-structural switchers between two traditional schools as for students entering startup charters. This suggests that selection of outcomes is not a characteristic of entering a charter school, but rather is a more general characteristic of non-structural switchers, since 95% of students who enter startup charters from a non-charter ALUSD school are also non-structural switchers.

Table 4 provides some regression estimates that identify the Ashenfelter dips in the outcome variables shown in Figures 3A and 3B along with retention and test scores. Each regression is run on the entire base sample for outcomes other than test scores and the entire test sample for test score outcomes. They contain indicators for being in a period that is three, two, and one year prior to entry into a conversion or startup charter or prior to switching non-structurally between traditional schools. They also include indicators for being in the year of the switch, denoted by year g in the table, given the student is observed in the sample in the year prior to the switch. The regressions confirm the graphical observations in Figures 3A and 3B. Students in conversions show no substantial drops in discipline and attendance prior to entry while there is clear evidence of dips for students in startups for the two years immediately prior to entry. All of the estimates for annual changes in test scores except one drop in the year prior to entry for both types of charters along with retention rates. In addition, the patterns for students who undergo non-structural switches between non-charter schools are similar to students who enter startup charters, as suggested by the

figures.²²

In order to address the potential endogeneity generated by selection based on changes in outcomes I use a procedure called interrupted panel estimates (Hanushek, Kain, Rivkin and Branch, 2007; Hanushek, Kain and Rivkin, 2002; Ashenfelter, 1978). The idea is that by dropping the periods prior to entry into a charter school, I can mitigate the effect of the selection by comparing periods students are enrolled in charters to periods well before charter entry. However, the results in Table 4 show that this selection also occurs in students who undergo non-structural switches between traditional schools. Thus, I also drop observations in the periods prior to non-structural switches between any two schools.

Table 5 provides the results of these interrupted panel estimations. Except for retention and reading tests in startup charters, the results change little regardless of whether periods before or immediately after charter entry are dropped. The reading test impacts become significantly negative, but overall the pattern of test scores across subjects do not change. One particularly interesting result is that when both the year before and year of the switch are dropped the added value measures of discipline and attendance improvements for startups fall. This is in part due to increased precision, but it also suggests that these behavioral improvements occur once a student enters a charter with little improvement afterwards. Results in section 5.3 will later confirm this. Nonetheless, the discipline measure is still statistically significant.

In addition to changes in outcomes affecting entry into charter schools, they may also affect exit from charter schools. If a parent takes outcome measures as indicators of match quality with the charter school then he may repeat the selection process for charter entry and once again seek other educational options. A potential consequence of this endogenous exit is that when the students return to ALUSD non-charter schools after performing poorly in a charter, they may experience mean-reversion back to higher performance levels. Since,

²²The fact that non-structural switchers have similar pre-switch patterns to startup charter students suggest that they could provide a good comparison group in a difference-in-differences analysis focusing on these two groups. This analysis provides results very similar to the individual fixed-effects results and are available from the author upon request.

in fixed effects analyses, students who are in charters are essentially compared to periods when they are not in charters, endogenous exit of this type could impose a downward bias the charter impacts.

To address this issue, in column four, I provide interrupted panel estimates where the year prior to when a student exits a charter school and enters a non-charter school is dropped. I also drop the year prior to non-structural switches between non-charter schools. I caution, however, that using interrupted panel estimates for endogenous exit is a more problematic strategy for removing bias than for endogenous entry since these estimates identify the charter effects off of those who spend at least two years in a charter. Nonetheless, this would tend to increase the change in the estimates from the baseline result since students who benefit more from charters are more likely to remain in them. Thus, as long as the changes in the estimates are small, then there is little need for concern. This appears to be the case in ALUSD. When we compare the results in column four to column one the impact estimates change slightly, usually in the direction suggesting a better impact, but not enough to have any bearing on the statistical significance of the outcomes except for reading impacts in conversion charters. Finally, in column five I drop both the year prior to charter entry and before charter exit to see what effect adjusting for both types of endogeneity has. This strategy seems to magnify the charter effects but does not change the pattern of the estimates. Thus, overall, the results from the interrupted panel analyses suggest that charters provide improvements in discipline and attendance, but have mixed results for test scores, which is the conclusion drawn from the baseline estimates. The only difference is that startup charters display an increase in retention rates.

5.2 Attrition

While neither the endogenous entrance of students into charter schools nor the endogenous exit of students out of charter schools into non-charter schools affect the estimates considerably, some parents may choose to leave ALUSD altogether if students perform poorly

in charter schools. Although we may believe that parents of students who perform poorly in non-charters would be as likely to leave the district as charter students, the fact that they choose to send their children to charters suggests they have preferences for alternative educational environments. In addition, charter parents are more likely to be dissatisfied with the non-charter schools their children previously attended or with the district in general. Thus, charter parents may be more likely than non-charter parents to send their children to a private school or a non-district charter school if their ALUSD schools are bad matches.

The evidence from the ALUSD data suggests that there is substantially more attrition in charters than non-charters, particularly in startup charters. Figure 4 shows transitions between school types for ALUSD students in grades one through eleven from 1998-2003. While about 16% of non-charter students exit ALUSD each year, that number drops to 14% for conversion charters and jumps to nearly 32% for startup charter students.²³ The differences are more dramatic over longer time periods. For example, 38% of non-charter third graders are no longer in ALUSD five years later while that number is 43% for conversion students and 58% for startup students. Other research has shown differential attrition rates for charters as well, even in statewide data. Hanushek, Kain, Rivkin, and Branch (2007) show that while 7% of non-charter students leave their population of 4th through 7th grade students in Texas public schools each year, 18% of charter students leave.

The potential econometric problem when there is a substantial amount of attrition is that if students select out of the sample in a non-random manner then the results may be inaccurate representations of the effect of treatment on the treated. While a fixed effects regression would ideally provide a consistent estimate of the parameter θ in equation (2), if there is attrition from the population - defined here as any student who attends ALUSD between 1994 and 2004 - then fixed effects will estimate

$$(7) \quad \theta' = E(y_{it}|c_{it} = 1, \mathbf{X}_{it}, \phi_i, s_{it} = 1) - E(y_{it}|c_{it} = 0, \mathbf{X}_{it}, \phi_i, s_{it} = 1)$$

²³While some of this is due to dropouts, the numbers for grades one through eight show similar patterns.

where $s_{it} = 1$ if the student is in the sample in year t , while $s_{it} = 0$ if the student is not observed in the sample and is not expected to have graduated by year t , assuming normal grade progression. This is because I only observe those students who have not attrited. If $E(s_{it}|y_{it}, c_{it}, X_{it}, \phi_i) = E(s_{it}|X_{it}, \phi_i)$ so that s is mean independent of y and c conditional on observables and the fixed-effect, then running regressions on the attrited sample will lead to consistent estimates. However, this is a strong assumption in most panels, especially in administrative datasets.

To address this problem, I use an estimator proposed by Kyriazidou (1997). Her insight is that if one can find those observations for which attrition does not play an independent role in the outcome equation (i.e., the error term in the outcome equation is uncorrelated with attrition propensity), then by reweighting the sample to focus on those observations, we can correct for endogenous attrition. In addition, her estimator allows for the inclusion of individual specific intercepts in both the outcome and the selection equation, which is essential to the identification of the model used in this paper. The reader is referred to the online appendix for a detailed description of the procedure.

In order to estimate the selection equation, I expand the data so that any student observed in ALUSD has observations until she is expected to graduate assuming normal grade progression or until the year 2004, whichever comes first. For my exclusion restriction, I use whether the student is not eligible to attend her previous school due to exceeding the maximum grade of that school. The idea behind this exclusion restriction is that a student would be more likely to leave the district if she has to switch schools anyway; that is the relative costs of leaving the district falls if students are forced to switch schools. Since the student will always be grade-eligible for her last school if she is retained, I use the predicted grade based on the student's grade in $t - 1$ rather than the actual grade when determining eligibility. Thus if a student is in grade six in a school that goes up to that grade, but is held back, he will still be considered ineligible for that school since his predicted grade is seven. The model includes as covariates indicators for whether the last school the student

is observed attending prior to year t is a conversion or a startup, as well as the last observed free lunch, reduced-price lunch, other economic disadvantage, recent immigration status, and parents' migrant status. In addition the regression includes grade-by-year effects. If $s = 0$, the grade is predicted based on normal grade progression from the student's most recent observation.

Table 6 provides the results of the selection corrected estimates along with unweighted first-differences regressions for comparison.²⁴ In addition to mean-squared error minimizing bandwidths, I also provide results using bandwidths 50% smaller and 100% larger to test the sensitivity of the results to bandwidth selection. Comparing the results for the MSE minimizing bandwidths to the unweighted estimates we see that the charter effects are very similar regardless of whether they are split by type of charter. The results also appear to be robust to the size of the bandwidth. Thus, there is little evidence to suggest that endogenous attrition has a substantial effect on the fixed effects estimates.

Another strategy one can use to test the sensitivity of results to endogenous attrition is to impute the missing data under different assumptions about the outcomes students would have achieved had they remained in ALUSD. In Table 7, I show the results of these analyses. A detailed account of how data was imputed can be found in the online appendix. Under each scenario a group of attrited students are assumed to have not attrited and have had disciplinary infractions equal to the maximum or a certain percentile of the distribution of disciplinary infractions for their predicted grade-year or infractions are set to zero. In the first scenario, in panel B, students who ever are observed in a charter attend non-charter schools instead of attriting. In panel C, all students who attrit attend non-charter schools. In both scenarios, even in the most extreme situations the discipline results for startups are statistically significant, at least at the 10% level. For panels D and E, rather than assign all attriters to non-charter schools they are assigned to schools in a probabilistic fashion based on the transition probabilities imputed from a multinomial logit regression on students who

²⁴In order to avoid multicollinearity issues in the first stage due to the nature of the exclusion restriction, I drop first graders from the regressions.

remain in the data. In these cases, all students who are imputed to attend non charter schools have infractions set to 0 while those who are imputed to attend charter schools have their infractions set to different levels relative to their predicted grade-year. As in panels B and C, panel D only imputes data for students who ever attended a charter and E imputes data for all attrited students. While the evidence in these panels are not as strong as B and C, they still suggest that we need to make very extreme assumptions about the attrited students for attrition to make the discipline results statistically insignificant.

5.3 Persistence of Charter Effects

Bias can also arise if the treatment affects outcomes in multiple periods. Thus, we may be concerned that charter attendance in year t could affect outcomes in $t + 1$, $t + 2$, and so on. This "persistence" causes fixed effects regressions to attribute charter impacts to periods after students return to non-charter schools, biasing the estimates. This is particularly important in the ALUSD data since 69% of charter students return to non-charter schools at some point. More technically, the existence of persistence violates strict exogeneity since y_{it} becomes a function of $c_{i,t-k}$, i.e. $E(y_{it}|c_{i1}, \dots, c_{it}, X_{i1}, \dots, X_{iT}, \phi_i) \neq E(y_{it}|c_{it}, X_{it}, \phi_i)$.

In addition to the econometric issues it raises, persistence in charter impacts has policy implications as well. As of 2003 only 3.5% of public schools were charter schools and most students attend charters in elementary grades. Thus, until the number of charter schools in secondary grades becomes much larger, the vast majority of students who attend charters will return to regular public schools at some point. If charter impacts have little effect on students after they return to regular schools, then charters will not provide long-term benefits for most students.

I aim to identify the persistence effect by using two models. The first model includes lagged measures of charter status in the fixed-effects regressions. This strategy will reduce the bias generated by persistence, although if persistence lasts beyond the number of periods lagged some bias will remain (Wooldridge, 2002, pp. 301). In order to prevent the loss of

too many observations, I use two lags in this analysis. Separate regressions using three lags provide similar results. Thus, I estimate the following model

$$(8) \quad y_{it} = \alpha + \theta_0 c_{it} + \theta_1 c_{it-1} + \theta_2 c_{it-2} + \mathbf{X}_{it}\mathbf{\Gamma} + \mathbf{G}_{it}\mathbf{\Psi} + \phi_i + \epsilon_{it}$$

where c_{it-1} and c_{it-2} are first and second lags of c_{it} , which is defined as in equation (4). It should be noted, however, that including the lags limits the regressions to include only those observations where the student has been in the sample for at least three consecutive years.

Table 8, panel A, provides the results from these regressions, which include the same covariates as in the baseline regressions in section four. The first three columns show the effects of charter status in periods t , $t-1$, and $t-2$ on outcomes in period t . The second and third sets of three columns show the same results broken down by conversion and startup charter status. When I add the lagged charter status, only the attendance impacts for startups and the language test scores for conversions change substantially. In addition, both of these impact estimates increase, suggesting that if persistence is generating bias, it is generating an underestimation of the charter effects.

Nonetheless, while this strategy is useful for establishing the extent of the bias from persistence it is an impractical way to measure the extent of persistence, since the lagged charter indicators do not distinguish between individuals who are still in charters and those who have left. To address this, I consider a second model where an indicator is added for whether a student has previously attended a charter and is not currently enrolled in one. In addition, in order to see if charter impacts vary with the length of time spent in a charter, I also separate the indicators for charter enrollment into indicators for being in the first year of a charter spell and being past the first year of a charter spell. Thus, I estimate the model

$$(9) \quad y_{it} = \alpha + \theta_0 c_{it}^1 + \theta_1 c_{it}^{2+} + \theta_2 Post_{it} + \mathbf{X}_{it}\mathbf{\Gamma} + \mathbf{G}_{it}\mathbf{\Psi} + \phi_i + \epsilon_{it}$$

where $c_{it}^1 = 1$ if the student is in the first year of a charter spell, $c_{it}^{2+} = 1$ if a student is in another year of a charter spell, and $Post_{it} = 1$ if the student was previously in a charter but is not currently enrolled. As in the previous analysis, I also estimate a model that separates each of these indicators by conversion or startup status, so that, for example, $Post_{it}$ splits into two indicators. The first equals one whether the student was previously in a conversion charter and is not currently in a conversion and the second is defined similarly for startups. One potential concern with this model is that endogenous exit could be a more substantial problem here than in other models, since our outcome of interest is the effect of a charter after leaving. To address this, I use whether a student is grade ineligible for the last charter he or she attended as an instrument for being in a post charter period. As in section 5.2, in order to avoid the potential endogeneity of the instrument through retention, I use the student's predicted grade rather than actual grade. Panel B shows the second-stage results of this analysis.²⁵ Note that these are similar to the results from a regular fixed-effects estimation, supporting the results in section 5.1 that suggested endogenous exit is not a major concern. These results are available from the author upon request. The most remarkable result here is the sharp increase in disciplinary actions after a student leaves a charter. While the increase is larger for startups, it is clearly observed for both types of charters. As for other outcomes, in startups all of the point estimates suggest worsening outcomes after students leave the startups and attend other schools, although only retention is statistically significant. For conversions, there are persistent improvements in attendance and retention, but a drop off in test scores after students leave. Thus, there is essentially no persistence for startup charters, and some evidence of persistence for conversions. The results for the value added measures of discipline and attendance also confirm the suggestion from the interrupted panel estimates in section 5.1 that behavioral improvements occur at the time of entry into the startup charters.

²⁵First-stage results are provided in the online appendix.

6 Controlling for School Characteristics

Table 9 runs the baseline fixed effects regressions while including some observed characteristics of the charter schools. The purpose of this analysis is to see if we can get a bit inside the "black box" and determine what characteristics of charter schools drive the results found in the previous sections. Each panel provides results from regressions for a single outcome including various combinations of controls for per-student expenditures, student-teacher ratios, total enrollment, and teacher experience. Panel A looks at disciplinary actions and shows an interesting result. When I control for student-teacher ratios, enrollment, and teacher experience the entire impact estimate for startup charters drops to statistical insignificance and becomes very close to 0. In fact, controlling for student-teacher ratios and enrollment alone makes the estimate fall in absolute value to a statistically insignificant -0.142 compared to a significant -0.786 without the controls. More disciplinary actions are associated with more students per teacher, higher enrollment, and less experienced teachers, but the driving force seems to be the student-teacher ratio and enrollment. This suggests that the effectiveness of startup charters in improving discipline is almost entirely due to keeping the school small and maintaining a large staff.

On the other hand, controlling for these school characteristics, seems to increase the startup charter impact on attendance as is shown in panel B. Most of this change in the estimates seems to be driven by teacher experience in that having less experienced teachers is associated with higher attendance rates. While the correlation between teacher experience and student discipline and attendance may seem counter-intuitive, one potential explanation is that younger teachers may be more energetic and are more able to thrive in a charter school that wants to try new pedagogical techniques. These characteristics may allow the younger teachers to keep tighter control over their classes and make the classes more interesting, thus encouraging students to attend.

Panels C, D, and E show how controlling for school characteristics affects the estimates for test scores. The only test score estimate that substantially changes is math test scores.

While the math estimates for conversion charters improve a bit, there is a large drop in test score estimates for startup charters. These results suggest that, if enrollment were higher and student-teacher ratios were lower, then test scores would be lower in startup charters.

7 Conclusion

Charter schools have become an important and increasingly popular school reform over the last decade. Despite this, we know surprisingly little about the effectiveness of charter schools on charter students beyond their impact on test scores. Previous research has not considered how charters affect other outcomes such as discipline and attendance. In addition most previous research has treated charter schools as homogenous institutions and has not distinguished between the different types of charters, nor has previous work examined whether students gain any long term improvements in performance from attending charters. In this paper, I have tried to address these gaps in the literature using new data from an anonymous large urban school district (ALUSD) with an extensive charter program. Through the use of individual fixed effects, I am able to account for potential bias resulting from time-invariant unobserved characteristics of students. There are some potential pitfalls from using this strategy. fixed effects estimates can be biased if there is selection into and out of charter schools based on changes in outcomes, non-random attrition, or persistence in charter effects. I adjust my estimates for these complications using a variety of econometric techniques.

My estimates suggest that charters are effective at improving student behavior, on average, while their impact on test scores is mixed. There is no statistically significant effect on retention or attendance.. However, startup charters, which are schools that open as charters, provide larger improvements in discipline than conversion charters, which are traditional public schools that convert to charter status. In addition, when I control for lagged charter attendance, the attendance results become positive and statistically significant for

startups. While there are a number of potential reasons for there being such large discipline impacts in startup charters, there are two that may play particularly large roles. The first is that startup charters are much smaller than non-charters and conversions, providing administrators with the ability to closely oversee their schools and students. For example, one principal of a startup charter in ALUSD is able to meet with each of her students at least once a semester due to the small size of the school. This seems to play a large role in the results. Controlling for enrollment and student-teacher ratios makes the impact estimate for disciplinary infractions change from a highly significant -0.79 per year to a statistically insignificant -0.14 . Controlling for teacher experience cuts that estimate further to -0.07 . Another possibility is that charter schools are able to more easily remove students who have particularly bad behavior problems, making the administrators and teachers more able to aid students with mild problems. This could also increase the likelihood of well behaved students influencing the behavior of misbehaving students through peer-effects mechanisms.

In addition to the impact estimates, I also find substantial evidence of selection based on changes in outcome measures, particularly for students in startup charters. I correct for this using interrupted panel estimates (Hanushek, Kain, Rivkin and Branch, 2007; Hanushek, Kain and Rivkin, 2002; Ashenfelter, 1978) and find little to suggest that the selection has a substantial effect on the fixed effects estimates. In addition, I account for the potential endogeneity of attrition by using a semi-parametric estimator proposed by Kyriazidou (1997). These estimates suggest that my fixed effects estimates are not substantially affected by non-random attrition. Finally, I find little evidence of persistence of charter impacts for startup charters after students return to non-charter schools, though there may be some persistence for conversions. There is a particularly large increase in disciplinary actions after students leave startup charters.

Taken together, these results paint a mixed picture of charter schools. On the one hand, charters seem to be effective at improving student discipline and attendance while students are enrolled. On the other hand, the evidence suggests that these effects do not

last after students return to non-charter schools. Thus, as long as students return to non-charter schools after attending a charter, the evidence presented here suggests that they will not garner any long-term benefits. Hence, if charters are to be an effective strategy for improving student performance, there would need to be a large enough supply so that students could attend charters throughout their entire academic careers.

I should note that the results presented in this paper are only for one school district. Therefore, they do not necessarily extend to charter schools in other locations. Nonetheless, this paper has two important implications for the charter literature. First, my individual fixed effects results have been shown to be robust to multiple bias reducing procedures. These results suggest that this econometric strategy is appropriate in the context of charter schools, though more research is needed to ensure that this holds for other locations. Second, while the literature on charter schools has done an excellent job of analyzing how charters affect test scores while students are enrolled in them, this paper shows that there are other aspects of charter schools that need to be investigated. The fact that I find large impacts of charters on discipline and evidence that startup charters improve attendance rates suggests that studies that only look at test scores may not have all of the information needed to accurately assess the effectiveness of these schools.

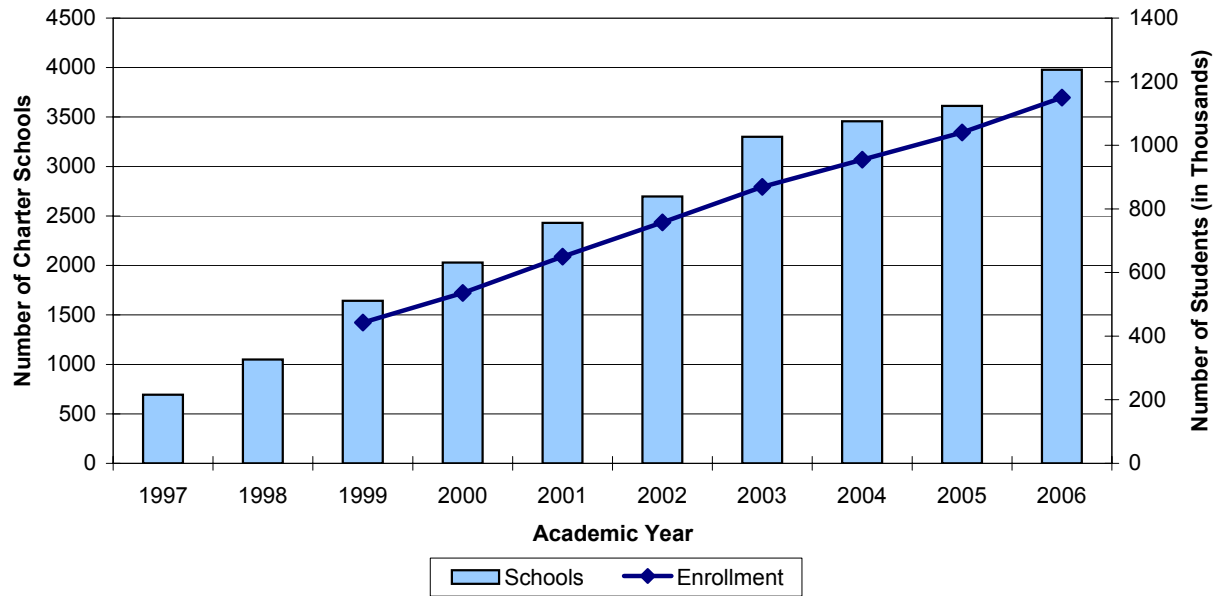
This paper looks at one aspect of charter schools - how they affect students who enroll in them. While more research is needed on this issue, there are other aspects of charter schools which also require further study before we can have a complete picture of how these schools work. We also need to get inside the "black box" of charter schools and establish why charter schools work or do not work. In particular, the role of spending in charter schools can be very important. If charters are no more effective at instruction than non-charters, they may still be efficiency enhancing if expenditures are lower.

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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. 2006, Center for Education Reform. 2004 data are unavailable so a linear interpolation is provided

Figure 2 - Fraction of Enrollment in ALUSD Area by Type of School and Year

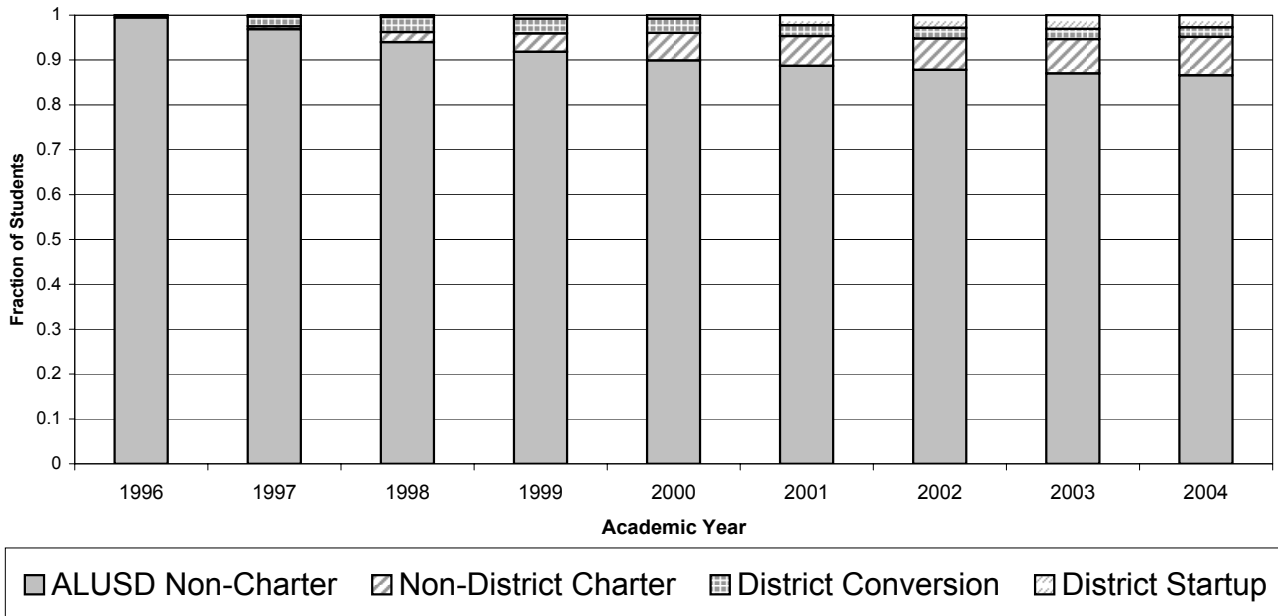
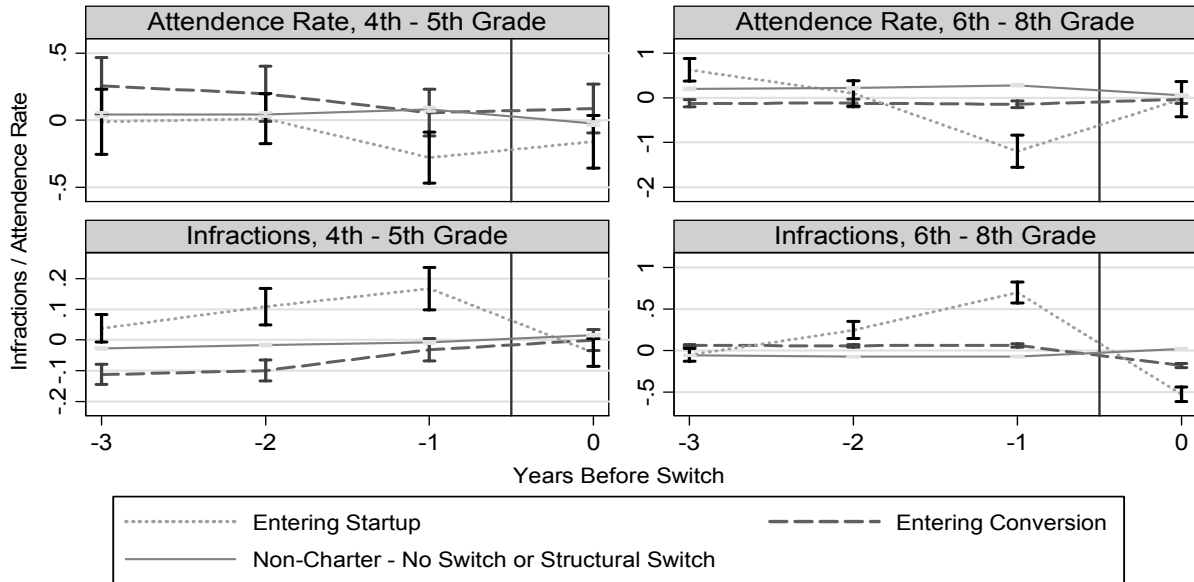


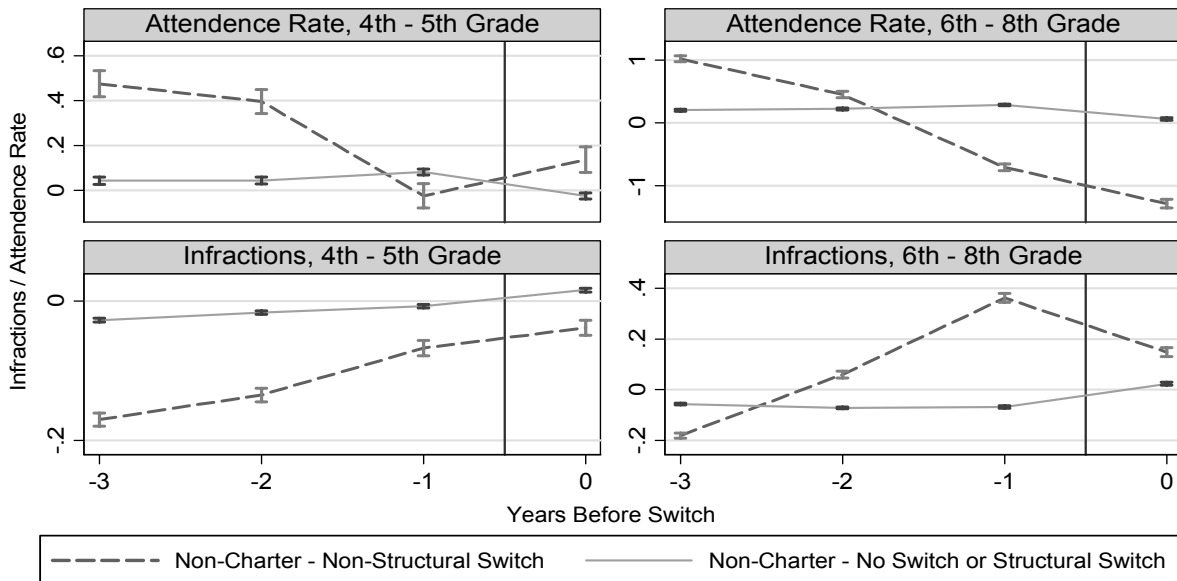
Table shows the fraction of students in each type of school in ALUSD along with non-district charters in the region around ALUSD as defined by the state Department of Education.

Figure 3A: Disciplinary Infractions and Attendance Before and After Entering Charters



Outcomes are de-meanned within individuals to remove fixed-effect then regression adjusted by free/reduced-price lunch status, having other economic disadvantage, recent immigration status, parents' migrant status, and grade-by-year effects.

Figure 3B: Disciplinary Infractions and Attendance Before and After Non-Charter School Switch



Outcomes are de-meanned within individuals to remove fixed-effect then regression adjusted by free/reduced-price lunch status, having other economic disadvantage, recent immigration status, parents' migrant status, and grade-by-year effects.

Figure 4: Transitions Between School Types

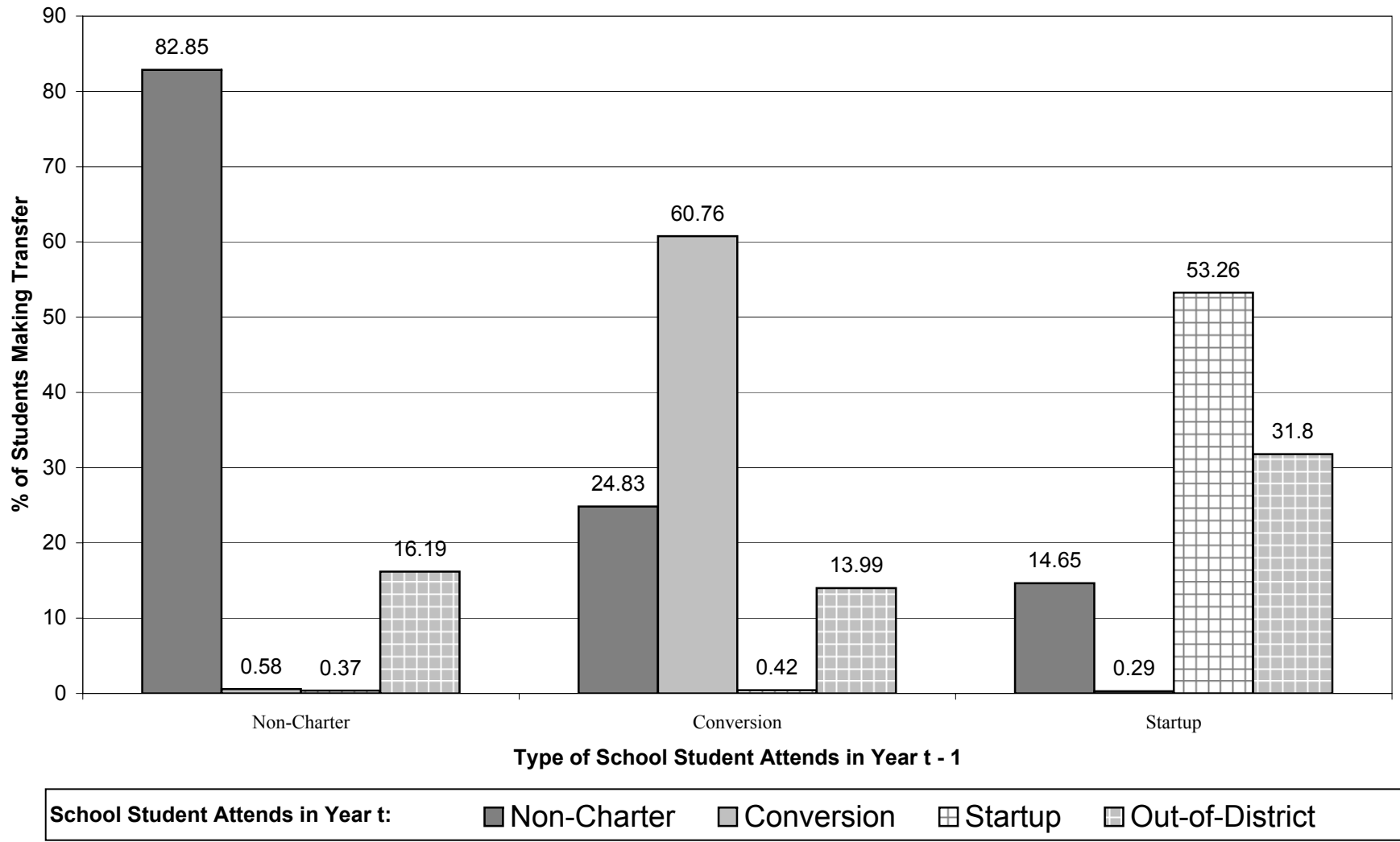


Table 1 - School Characteristics in 2004

	ALUSD Non- Charters	Conversion Charters	Startup Charters	Non-District Charters
Student Demographics (% of All Students in School)				
Limited English Proficient	30.3	18.8 (1.4)	12.2 (3.3)	10.9 (6.3)
Economically Disadvantaged	86.0	89.2 (0.5)	84.2 (0.4)	70.9 (5.1)
At-Risk	63.5	49.2 (2.2)	49.0 (3.0)	60.0 (1.1)
Special Education	10.8	8.2 (0.8)	5.9 (2.1)	12.5 (1.1)
Gifted	9.3	11.9 (0.6)	4.2 (1.6)	1.8 (4.5)
White, Non-Hispanic	7.2	5.6 (0.3)	6.8 (0.1)	14.1 (3.2)
School Demographics				
Teacher Experience (% of Teachers in School)				
0 - 5 Years	39.2	58.4 (3.8)	55.2 (2.1)	65.2 (11.6)
6 or More Years	60.8	41.6 (3.8)	44.8 (2.1)	34.8 (11.6)
Student-Teacher Ratio	16.2	16.5 (0.2)	17.1 (0.5)	17.2 (1.8)
Per-Pupil Operating Expenditures	\$6,916	\$5,773 (0.6)	\$5,032 (1.4)	\$6,394 (0.6)
Enrollment	895	769 (0.6)	433 (3.4)	373 (7.5)
Student Outcomes				
Attendance Rate	95.0	97.0 (0.8)	93.3 (0.9)	91.0 (3.3)
State Exam - Math				
% Passing at Low Level	61.9	71.6 (1.2)	54.6 (1.2)	42.0 (5.7)
% Passing at High Level	14.7	18.2 (0.8)	10.9 (1.1)	7.4 (4.2)
State Exam - Reading				
% Passing at Low Level	73.1	84.0 (1.8)	71.8 (0.3)	58.0 (5.0)
% Passing at High Level	17.3	23.2 (1.3)	15.6 (0.5)	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: Summary Statistics of ALUSD Base Sample By Charter Status

Variable	Non-Charter vs. Charter		Conversion vs. Startup	
	Non-Charter	Charter	Conversion	Startup
% Female	49.2	48.5 (3.1)	49.3	46.0 (6.6)
% White, Non-Hispanic	10.6	11.8 (8.5)	14.8	2.1 (40.4)
Grade level	5.9	5.2 (46.5)	4.8	6.6 (69.4)
Year	1999.0	2000.8 (134.6)	2000.4	2001.9 (68.3)
% Eligible for Free Lunch	59.5	59.7 (1.2)	61.9	52.7 (18.9)
% Eligible for Reduced Price Lunch	6.7	7.7 (9.7)	7.2	9.4 (8.5)
% Other Economic Disadvantage	5.2	7.2 (21.5)	5.1	13.9 (34.7)
% Limited English Proficient	25.1	21.0 (22.4)	22.0	17.9 (10.1)
% At Risk	55.4	49.6 (26.9)	44.4	66.3 (45.0)
% Special Education	11.2	8.1 (23.0)	8.9	5.3 (13.4)
% Gifted and Talented	10.2	16.1 (44.9)	20.9	0.7 (57.1)
% Recent Immigrant (within 3 years)	6.1	4.0 (21.1)	4.0	3.8 (1.3)
% Parent is Migrant Worker	0.6	0.7 (1.4)	0.6	0.9 (4.0)
# of Disciplinary Infractions (Suspension or More Severe)	0.42	0.26 (27.4)	0.30	0.16 (14.0)
Attendance Rate (%)	93.9	95.2 (29.8)	96.0	92.4 (49.5)
% Retained	8.6	5.2 (23.7)	4.0	11.25 (24.7)
Reading & English Grades	80.0	82.9 (57.4)	83.2	80.9 (18.1)
Math Grade	79.7	82.7 (55.7)	83.2	79.7 (25.1)
Average Grade	80.2	83.2 (65.9)	83.8	80.4 (28.5)
Math Exam National Percentile Ranking (1998 and Later)	49.9	56.1 (40.9)	58.9	48.1 (30.7)
Reading Exam National Percentile Ranking (1998 and Later)	44.8	52.1 (47.6)	55.5	42.2 (38.1)
Language Exam National Percentile Ranking (1998 and Later)	49.7	56.5 (44.5)	59.7	46.9 (37.2)

Absolute t-statistics in parentheses. Sample contains over 1.2 million non-charter student-year observations, approximately 40,000 observations of students in conversion charters and approximately 13,000 observations of students in startup charters. Exact sample sizes cannot be revealed due to confidentiality restrictions.

Table 3 - Fixed Effects Regressions of Charter Impact

	(1)	(2)	
	Any Charter	Conversion	Startup
# of Infractions	-0.357** (0.085)	-0.213* (0.090)	-0.786** (0.107)
Attendance Rate (%)	0.451 (0.383)	0.126 (0.163)	1.416 (1.191)
Δ # of Infractions	-0.223** (0.086)	-0.097# (0.054)	-0.634** (0.201)
Δ Attendance Rate (%)	0.646 (0.443)	0.078 (0.097)	2.487# (1.300)
Likelihood of Being Retained	0.006 (0.010)	-0.002 (0.008)	0.044 (0.042)
Δ Mathematics NPR	1.379** (0.484)	1.873** (0.483)	-0.673 (0.952)
Δ Reading NPR	-0.698* (0.319)	-0.543 (0.340)	-1.342 (0.874)
Δ Language NPR	0.457 (0.289)	0.498 (0.330)	0.287 (0.596)

Robust standard errors clustered by school in parentheses. Behavior and attendance regressions contain over 1,200,000 observations. Retention regressions contain over 1,000,000 observations. Test score regressions contain over 500,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also include the following covariates: free or reduced price lunch status, other economic disadvantages, peer mobility rate, whether student undergoes a nonstructural switch, whether student undergoes a structural switch, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4 - Fixed Effects Regressions of Pre and Post School Entry Effects

	Conversion Entry				Startup Entry				Traditional Non-Structural Entry			
	g - 3	g - 2	g - 1	g	g - 3	g - 2	g - 1	g	g - 3	g - 2	g - 1	g
# of Infractions	0.156** (0.046)	0.028 (0.041)	0.006 (0.020)	-0.120* (0.047)	0.253** (0.049)	0.279** (0.055)	0.326** (0.071)	-0.537** (0.127)	-0.024** (0.009)	0.079** (0.012)	0.163** (0.017)	-0.053** (0.017)
Attendance Rate (%)	-0.575** (0.115)	-0.159# (0.087)	-0.241** (0.091)	-0.312* (0.137)	-0.470* (0.210)	-0.832** (0.246)	-2.417** (0.316)	0.120 (0.927)	0.421** (0.057)	-0.040 (0.040)	-0.749** (0.063)	-0.475** (0.120)
Δ # of Infractions	0.068** (0.026)	-0.011 (0.033)	0.007 (0.043)	-0.165** (0.061)	-0.021 (0.040)	0.033 (0.068)	0.012 (0.065)	-1.011** (0.228)	0.036 (0.027)	0.111** (0.023)	0.135** (0.016)	-0.227* (0.092)
Δ Attendance Rate	-0.394** (0.084)	-0.039 (0.094)	-0.151 (0.129)	0.024 (0.106)	-0.063 (0.141)	-0.386# (0.219)	-1.499** (0.291)	3.122# (1.665)	-0.016 (0.048)	-0.382** (0.051)	-0.877** (0.080)	0.281** (0.106)
Likelihood of Being Retained	0.011# (0.006)	0.004 (0.004)	0.011** (0.004)	0.003 (0.008)	0.029** (0.009)	0.055** (0.011)	0.193** (0.020)	0.125** (0.048)	-0.020** (0.002)	0.003 (0.002)	0.047** (0.004)	0.004 (0.004)
Δ Mathematics NPR	-2.771** (0.849)	2.314** (0.683)	-1.459* (0.581)	1.493 (1.648)	0.302 (0.622)	-0.183 (0.624)	-1.271# (0.751)	-1.335 (1.676)	0.104 (0.198)	-0.126 (0.165)	-0.339* (0.158)	0.092 (0.205)
Δ Reading NPR	0.988* (0.453)	1.375** (0.504)	-1.397** (0.425)	0.612 (0.774)	-0.304 (0.513)	0.230 (0.509)	-1.639** (0.562)	-1.860# (1.122)	-0.168 (0.162)	0.330* (0.144)	-0.639** (0.135)	-0.426* (0.190)
Δ Language NPR	-1.309** (0.474)	-0.627 (0.536)	2.320** (0.677)	0.793 (0.977)	-0.113 (0.479)	0.364 (0.571)	-1.568* (0.718)	0.060 (0.806)	-0.393* (0.183)	0.147 (0.157)	-0.847** (0.155)	-0.586** (0.167)

Robust standard errors clustered by school in parentheses. Only students with a g-1 observation are classified as switchers in g. Each row is a separate regression. Behavior and attendance regressions contain over 1,200,000 observations. Retention regressions contain over 1,000,000 observations. Test score regressions contain over 500,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also include the following covariates: free or reduced price lunch status, other economic disadvantages, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5 - Interrupted Panel Fixed Effects Regressions of Charter Impact

A. General Charter Indicator	(1)	(2)	(3)	(4)	(5)	(6)
# of Infractions	-0.357** (0.085)	-0.336** (0.081)	-0.308** (0.073)	-0.289** (0.077)	-0.381** (0.086)	-0.372** (0.081)
Attendance Rate (%)	0.451 (0.383)	0.392 (0.404)	0.319 (0.375)	0.133 (0.205)	0.538 (0.497)	0.415 (0.479)
Δ # of Infractions	-0.223** (0.086)	-0.217** (0.076)	-0.214** (0.073)	-0.074* (0.036)	-0.249** (0.074)	-0.258** (0.077)
Δ Attendance Rate	0.646 (0.443)	0.571 (0.441)	0.559 (0.429)	0.064 (0.147)	0.775 (0.535)	0.820 (0.567)
Likelihood of Being Retained	0.006 (0.010)	0.025* (0.012)	0.032* (0.014)	0.016# (0.009)	0.022# (0.013)	0.024# (0.014)
Δ Mathematics NPR	1.379** (0.484)	1.385* (0.583)	1.528* (0.614)	1.206* (0.593)	2.013** (0.564)	1.909** (0.569)
Δ Reading NPR	-0.698* (0.319)	-1.710** (0.321)	-1.535** (0.335)	-1.955** (0.287)	-0.979** (0.339)	-1.382** (0.351)
Δ Language NPR	0.457 (0.289)	0.220 (0.276)	0.094 (0.287)	0.262 (0.274)	0.169 (0.268)	1.167** (0.348)

B. Charters Split by Conversion and Startup

Conversion	(1)	(2)	(3)	(4)	(5)	(6)
# of Infractions	-0.213* (0.090)	-0.185* (0.084)	-0.162* (0.072)	-0.196* (0.093)	-0.209* (0.088)	-0.205* (0.084)
Attendance Rate (%)	0.126 (0.163)	0.016 (0.152)	-0.040 (0.156)	-0.032 (0.140)	0.061 (0.162)	0.040 (0.175)
Δ # of Infractions	-0.097# (0.054)	-0.079 (0.050)	-0.069 (0.047)	-0.061 (0.043)	-0.122* (0.054)	-0.122* (0.055)
Δ Attendance Rate	0.078 (0.097)	0.016 (0.106)	-0.009 (0.123)	-0.045 (0.095)	0.066 (0.110)	0.070 (0.121)
Likelihood of Being Retained	-0.002 (0.008)	0.011 (0.009)	0.016 (0.012)	0.016 (0.010)	0.007 (0.007)	0.008 (0.009)
Δ Mathematics NPR	1.873** (0.483)	1.514* (0.620)	1.703** (0.640)	1.250* (0.632)	2.240** (0.556)	2.153** (0.564)
Δ Reading NPR	-0.543 (0.340)	-1.616** (0.357)	-1.356** (0.377)	-1.996** (0.291)	-0.945** (0.348)	-1.367** (0.361)
Δ Language NPR	0.498 (0.330)	0.236 (0.293)	0.133 (0.295)	0.160 (0.290)	0.152 (0.288)	1.142** (0.387)

Startup	(1)	(2)	(3)	(4)	(5)	(6)
# of Infractions	-0.786** (0.107)	-0.786** (0.101)	-0.759** (0.085)	-0.748** (0.110)	-0.853** (0.104)	-0.797** (0.088)
Attendance Rate (%)	1.416 (1.191)	1.520 (1.239)	1.434 (1.128)	0.950 (0.767)	1.841 (1.427)	1.367 (1.350)
Δ # of Infractions	-0.634** (0.201)	-0.674** (0.168)	-0.722** (0.153)	-0.135* (0.054)	-0.629** (0.156)	-0.638** (0.156)
Δ Attendance Rate	2.487# (1.300)	2.406# (1.294)	2.542* (1.224)	0.614 (0.558)	2.898* (1.396)	2.918* (1.404)
Likelihood of Being Retained	0.044 (0.042)	0.092# (0.048)	0.113* (0.055)	0.021 (0.028)	0.096 (0.062)	0.097 (0.062)
Δ Mathematics NPR	-0.673 (0.952)	0.383 (0.854)	-0.230 (0.892)	0.839 (0.927)	0.057 (0.912)	0.414 (1.006)
Δ Reading NPR	-1.342 (0.874)	-2.437** (0.715)	-3.341** (0.791)	-1.605* (0.771)	-1.276 (0.832)	-1.474# (0.852)
Δ Language NPR	0.287 (0.596)	0.103 (0.557)	-0.303 (0.768)	1.122* (0.528)	0.318 (0.707)	1.324# (0.753)

- (1) No Dropped Years (from Table 3)
- (2) Drop Year Prior to Charter Entry or Non-structural Switch Between Non-Charters
- (3) Drop Two Years Prior to Charter Entry or Non-Structural Switch Between Non-Charters
- (4) Drop Year Prior to and Year of Charter Entry or Non-structural Switch Between Non-Charters
- (5) Drop Year Prior to Charter Exit or Non-Structural Switch Between Non-Charters
- (6) Drop Year Prior to Charter Exit and Entry or Non-Structural Switch Between Non-Charters

Robust standard errors clustered by school in parentheses. Behavior and attendance regressions contain over 1,200,000 observations. Retention regressions contain over 1,000,000 observations. Test score regressions contain over 400,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also include the following covariates: free or reduced price lunch status, other economic disadvantages, peer mobility rate, whether student undergoes a nonstructural switch, whether student undergoes a structural switch, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Kyriazidou (1997) Selection Corrected Estimates

	Unweighted (First-Differences)			1/2 * MSE Minimizing Bandwidth		
	(1)	(2)		(3)	(4)	
	Charter	Conversion	Startup	Charter	Conversion	Startup
# of Infractions	-0.359** (0.100)	-0.161* (0.078)	-0.930** (0.187)	-0.323** (0.101)	-0.137# (0.076)	-0.916** (0.206)
Attendance Rate (%)	0.777 (0.665)	-0.025 (0.237)	3.090# (1.823)	0.763 (0.685)	-0.083 (0.247)	3.450# (1.937)
Δ # of Infractions	-0.333** (0.098)	-0.146* (0.061)	-0.971** (0.282)	-0.274** (0.104)	-0.116# (0.064)	-0.862** (0.333)
Δ Attendance Rate	0.794 (0.580)	0.009 (0.181)	3.468* (1.735)	0.765 (0.595)	-0.052 (0.196)	3.804* (1.820)
Likelihood of Being Retained	0.013 (0.012)	0.012 (0.014)	0.018 (0.020)	0.023 (0.016)	0.018 (0.017)	0.037 (0.032)
Δ Mathematics NPR	1.745** (0.619)	2.203** (0.618)	-0.004 (1.294)	1.864** (0.590)	2.070** (0.634)	0.997 (1.289)
Δ Reading NPR	-0.675 (0.703)	-0.497 (0.788)	-1.355 (1.319)	-0.800 (0.745)	-0.813 (0.857)	-0.742 (1.199)
Δ Language NPR	0.425 (0.618)	0.164 (0.699)	1.424# (0.828)	0.570 (0.560)	0.245 (0.614)	1.940* (0.782)

	MSE Minimizing Bandwidth			2 * MSE Minimizing Bandwidth		
	(5)	(6)		(7)	(8)	
	Charter	Conversion	Startup	Charter	Conversion	Startup
# of Infractions	-0.347** (0.100)	-0.154* (0.077)	-0.931** (0.194)	-0.355** (0.100)	-0.159* (0.078)	-0.931** (0.189)
Attendance Rate (%)	0.765 (0.662)	-0.039 (0.241)	3.204# (1.852)	0.773 (0.663)	-0.028 (0.238)	3.121# (1.830)
Δ # of Infractions	-0.312** (0.099)	-0.139* (0.061)	-0.935** (0.300)	-0.327** (0.098)	-0.144* (0.061)	-0.961** (0.287)
Δ Attendance Rate	0.781 (0.579)	-0.008 (0.185)	3.615* (1.770)	0.789 (0.579)	0.004 (0.182)	3.510* (1.746)
Likelihood of Being Retained	0.016 (0.014)	0.013 (0.016)	0.024 (0.025)	0.014 (0.013)	0.012 (0.015)	0.019 (0.021)
Δ Mathematics NPR	1.810** (0.605)	2.170** (0.621)	0.377 (1.294)	1.764** (0.615)	2.192** (0.619)	0.106 (1.294)
Δ Reading NPR	-0.701 (0.718)	-0.577 (0.815)	-1.195 (1.281)	-0.684 (0.708)	-0.519 (0.796)	-1.321 (1.309)
Δ Language NPR	0.460 (0.605)	0.185 (0.679)	1.554# (0.794)	0.432 (0.614)	0.169 (0.694)	1.449# (0.814)

Robust standard errors clustered by school in parentheses. Students in first grade are dropped to avoid multicollinearity in the first stage. First-stage regressions contain over 1.2 million observations and also includes grade-by-year dummies along with the student's last known status of the following once-lagged covariates: free or reduced price lunch status, other economic disadvantages. Each Behavior and attendance regressions contain over 800,000 observations. Retention regressions contain over 800,000 observations. Test score regressions contain over 300,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also include the following covariates: free or reduced price lunch status, other economic disadvantages, whether student undergoes a nonstructural switch, whether student undergoes a structural switch, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Sensitivity of Discipline Results to Different Attrition Scenarios

A. Infraction Statistics by Grade Level

Grade	Maximum	Percentile Within Grade Grouping					Mean
		99th	95th	90th	80th	70th	
1st - 5th	23	2	0	0	0	0	0.08
6th - 8th	45	9	5	3	1	1	0.87
9th - 12th	36	7	3	2	1	0	0.57

B. Imputations for Charter Students Only - Attend Non-Charter With Infractions Imputed to be X

X:	Maximum for Grade-Year	Percentile Within Grade-Year					0
		99th	95th	90th	80th	70th	
Conversion	-4.894* (2.120)	-1.726* (0.687)	-0.867** (0.299)	-0.518** (0.153)	-0.248** (0.087)	-0.130 (0.102)	0.001 (0.144)
Startup	-6.152* (2.534)	-2.406** (0.773)	-1.411** (0.311)	-1.026** (0.151)	-0.754** (0.112)	-0.587** (0.141)	-0.491** (0.183)

C. Imputations for All Students - Attend Non-Charter With Infractions Imputed to be X

X:	Maximum for Grade-Year	Percentile Within Grade-Year					0
		99th	95th	90th	80th	70th	
Conversion	-4.611** (1.604)	-1.903** (0.710)	-0.985** (0.352)	-0.585** (0.208)	-0.283* (0.111)	-0.172# (0.098)	-0.008 (0.122)
Startup	-8.718** (2.810)	-3.289** (0.930)	-1.795** (0.397)	-1.225** (0.205)	-0.755** (0.094)	-0.502** (0.119)	-0.335# (0.193)

D. Imputations for Charter Students Only - Type of School Attended Random Function of Observed Characteristics

When Attrited Student is in a Non-Charter Infractions = 0; When Attrited Student is in Charter Infractions = X

X:	Maximum for Grade-Year	Percentile Within Grade-Year					0
		99th	95th	90th	80th	70th	
Conversion	2.079 (1.944)	0.711 (0.769)	0.310 (0.428)	0.138 (0.286)	0.018 (0.191)	-0.037 (0.151)	-0.108 (0.109)
Startup	3.716 (3.135)	0.871 (1.166)	0.079 (0.621)	-0.227 (0.411)	-0.440 (0.268)	-0.606** (0.164)	-0.675** (0.124)

E. Imputations for All Students - Type of School Attended Random Function of Observed Characteristics

When Attrited Student is in a Non-Charter Infractions = 0; When Attrited Student is in Charter Infractions = X

X:	Maximum for Grade-Year	Percentile Within Grade-Year					0
		99th	95th	90th	80th	70th	
Conversion	7.051# (4.261)	2.682 (1.705)	1.298 (0.891)	0.720 (0.553)	0.273 (0.295)	0.079 (0.186)	-0.156* (0.079)
Startup	9.377* (4.438)	3.079# (1.689)	1.327 (0.927)	0.637 (0.627)	0.131 (0.408)	-0.221 (0.261)	-0.386* (0.192)

Robust standard errors clustered by school in parentheses. Regressions contain over 2,000,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also include the following covariates: free or reduced price lunch status, other economic disadvantages, whether student undergoes a nonstructural switch, whether student undergoes a structural switch, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 8 - Persistence Regressions

A: Fixed Effects Regressions with Lagged Charter Indicators

	(1)			(2)					
	Any Charter			Conversion			Startup		
	t	t-1	t-2	t	t-1	t-2	t	t-1	t-2
# of Infractions	-0.356** (0.085)	-0.052# (0.029)	-0.075* (0.030)	-0.204** (0.072)	-0.072* (0.031)	-0.056# (0.031)	-0.851** (0.130)	-0.066 (0.057)	-0.004 (0.080)
Attendance Rate (%)	0.489 (0.372)	-0.055 (0.106)	0.448** (0.128)	0.013 (0.167)	0.095 (0.107)	0.359** (0.119)	2.026* (0.979)	-0.458* (0.225)	0.394 (0.248)
Δ # of Infractions	-0.289** (0.099)	0.217** (0.062)	-0.021 (0.031)	-0.131* (0.061)	0.093# (0.049)	0.014 (0.029)	-0.806** (0.233)	0.718** (0.156)	0.002 (0.084)
Δ Attendance Rate (%)	0.772 (0.528)	-0.303 (0.233)	0.367** (0.142)	0.048 (0.127)	0.009 (0.112)	0.245** (0.080)	3.132* (1.435)	-1.326# (0.748)	0.135 (0.351)
Likelihood of Being Retained	0.013 (0.013)	-0.012 (0.008)	-0.025** (0.007)	0.011 (0.012)	-0.015 (0.009)	-0.029** (0.007)	0.023 (0.043)	0.011 (0.017)	0.021 (0.018)
Δ Mathematics NPR	1.396** (0.506)	-0.482 (0.815)	-0.659 (0.410)	1.842** (0.569)	-0.861 (1.000)	-0.377 (0.455)	-0.305 (1.159)	1.212 (1.296)	-2.257 (1.381)
Δ Reading NPR	-0.506 (0.422)	-0.618 (0.453)	0.758 (0.468)	-0.263 (0.496)	-0.936# (0.553)	0.943# (0.543)	-1.379 (0.938)	0.968 (0.670)	-0.255 (0.789)
Δ Language NPR	0.920* (0.394)	-1.196** (0.420)	0.475 (0.497)	0.942* (0.459)	-1.397** (0.528)	0.645 (0.575)	0.965 (0.689)	-0.090 (0.719)	-0.754 (1.178)

B: 2SLS Fixed Effects Persistence Regressions

	(1)			(2)					
	Any Charter			Conversion			Startup		
	Year 1	Year 2+	Post	Year 1	Year 2+	Post	Year 1	Year 2+	Post
# of Infractions	-0.416** (0.114)	-0.440** (0.132)	-0.153# (0.093)	-0.235* (0.101)	-0.275# (0.145)	-0.091 (0.095)	-0.795** (0.148)	-0.879** (0.151)	-0.016 (0.180)
Attendance Rate (%)	0.803 (0.542)	1.093# (0.600)	1.157** (0.417)	0.474* (0.202)	0.734* (0.324)	1.125** (0.354)	1.378 (1.368)	1.952 (1.761)	-0.543 (0.849)
Δ # of Infractions	-0.353* (0.154)	-0.108 (0.077)	-0.020 (0.073)	-0.120 (0.090)	-0.046 (0.074)	0.033 (0.073)	-0.853** (0.293)	-0.200 (0.143)	0.158 (0.265)
Δ Attendance Rate (%)	1.126 (0.862)	0.590# (0.347)	0.546 (0.333)	0.193 (0.138)	0.292 (0.186)	0.369# (0.220)	3.087 (1.990)	1.148 (0.815)	-0.819 (0.895)
Likelihood of Being Retained	-0.009 (0.015)	-0.011 (0.010)	-0.043# (0.023)	-0.026** (0.007)	-0.023** (0.006)	-0.057* (0.023)	0.055 (0.056)	0.062 (0.042)	0.109** (0.036)
Δ Mathematics NPR	1.382 (0.992)	0.806 (1.959)	-0.641 (1.860)	2.732** (0.885)	1.710 (2.435)	0.301 (2.186)	-1.216 (1.842)	-0.597 (1.625)	-1.162 (2.488)
Δ Reading NPR	-0.463 (0.793)	-2.426# (1.350)	-1.775 (1.744)	0.217 (0.930)	-1.932 (1.584)	-1.069 (1.922)	-1.724 (1.333)	-2.242 (1.847)	-2.464 (2.399)
Δ Language NPR	0.644 (0.854)	-0.715 (0.890)	-1.187 (1.071)	0.846 (1.156)	-0.392 (1.043)	-0.756 (1.188)	0.273 (0.925)	-0.950 (1.238)	-2.133 (2.531)

Robust standard errors clustered by school in parentheses. Behavior and attendance regressions contain over 1,000,000 observations. Retention regressions contain over 1,000,000 observations. Test score regressions contain over 500,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also include the following covariates: free or reduced price lunch status, other economic disadvantages, peer mobility rate, whether student undergoes a nonstructural switch, whether student undergoes a structural switch, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Fixed Effects Regressions with Controls for School Characteristics

	A: # of Dsciplinary Infractions								B: Attendance Rate (%)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Convert	-0.213* (0.090)	-0.226* (0.093)	-0.224* (0.092)	-0.231* (0.110)	-0.235* (0.106)	-0.221* (0.098)	-0.213# (0.115)	-0.214# (0.115)	-0.097# (0.054)	0.017 (0.159)	0.066 (0.153)	0.117 (0.160)	0.082 (0.154)	-0.072 (0.170)	-0.050 (0.171)	-0.095 (0.177)		
Startup	-0.786** (0.107)	-0.855** (0.127)	-0.446** (0.086)	-0.552** (0.135)	-0.142 (0.092)	-0.595** (0.116)	-0.073 (0.097)	-0.072 (0.103)	1.416 (1.191)	1.437 (1.218)	2.805** (0.314)	2.087 (1.350)	2.957** (0.639)	1.841** (0.443)	2.543** (0.615)	2.218** (0.566)		
Per-Student Expenditure (\$1000's)		-0.028** (0.006)						-0.009 (0.010)									-0.184 (0.113)	
Per-Student Expenditure^2 (\$1000's)		0.0001* (0.0000)						0.000 (0.000)									0.000 (0.002)	
Student-Teacher Ratio			0.094** (0.021)		0.054* (0.022)		0.058** (0.022)	0.038# (0.020)			0.118 (0.201)		0.103 (0.189)		0.096 (0.193)		-0.278 (0.222)	
Student-Teacher Ratio^2			-0.002** (0.001)		-0.002** (0.001)		-0.002** (0.001)	-0.001** (0.000)			-0.001 (0.005)		-0.001 (0.004)		-0.001 (0.004)		0.008 (0.005)	
Enrollment (1000's)				0.494** (0.085)	0.374** (0.085)		0.375** (0.087)	0.352** (0.088)				0.461 (0.671)	-0.177 (0.644)		-0.212 (0.648)		-0.661 (0.537)	
Enrollment^2 (1000's)				-0.092** (0.027)	-0.071** (0.027)		-0.070* (0.028)	-0.066* (0.028)				-0.028 (0.154)	0.112 (0.145)		0.116 (0.148)		0.210# (0.126)	
Teacher Experience: 0 Years						-0.002 (0.002)	-0.005** (0.002)	-0.005** (0.002)						0.020* (0.008)	0.015* (0.007)		0.013* (0.006)	
Teacher Experience: 1-5 Years						0.001 (0.002)	-0.003# (0.002)	-0.003# (0.002)						0.023** (0.005)	0.019** (0.006)		0.017** (0.005)	
Teacher Experience: 6-10 Years						0.003 (0.002)	-0.001 (0.002)	-0.001 (0.002)						0.019* (0.008)	0.014* (0.007)		0.013* (0.007)	
Teacher Experience: 11-20 Years						0.004# (0.002)	-0.001 (0.002)	-0.001 (0.002)						0.016# (0.009)	0.011 (0.007)		0.014# (0.008)	
				C: Δ Math NPR						D: Δ Reading NPR								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Convert	1.873** (0.483)	1.864** (0.491)	1.923** (0.476)	2.127** (0.599)	2.166** (0.575)	2.009** (0.508)	2.404** (0.591)	2.370** (0.596)	-0.543 (0.340)	-0.528 (0.354)	-0.526 (0.338)	-0.455 (0.308)	-0.435 (0.310)	-0.497 (0.309)	-0.391 (0.293)	-0.393 (0.301)		
Startup	-0.673 (0.952)	-0.973 (0.917)	-1.447# (0.862)	-1.333 (0.960)	-1.693 (1.092)	-5.056* (2.136)	-2.560* (1.292)	-2.784* (1.302)	-1.342 (0.874)	-1.447 (0.923)	0.093 (0.710)	-1.457# (0.884)	0.154 (0.750)	-3.305 (2.569)	0.537 (0.867)	0.270 (0.867)		
Per-Student Expenditure (\$1000's)		-0.062 (0.073)						-0.203# (0.110)									-0.205* (0.084)	
Per-Student Expenditure^2 (\$1000's)		0.001 (0.001)						0.002 (0.002)									0.003* (0.002)	
Student-Teacher Ratio			0.082 (0.084)		0.120 (0.103)		0.187# (0.103)	0.067 (0.110)			0.166 (0.104)		0.168 (0.113)		0.202# (0.113)		0.133 (0.114)	
Student-Teacher Ratio^2			-0.004* (0.002)		-0.006* (0.002)		-0.007** (0.002)	-0.005* (0.002)			-0.005# (0.003)		-0.005# (0.003)		-0.005* (0.003)		-0.004 (0.003)	
Enrollment (1000's)				-2.297** (0.779)	-1.991* (0.884)		-1.975* (0.886)	-2.454* (0.955)				-0.597 (0.535)	-0.578 (0.614)		-0.528 (0.600)		-1.031 (0.630)	
Enrollment^2 (1000's)				0.723** (0.212)	0.675** (0.223)		0.652** (0.226)	0.744** (0.237)				0.219 (0.160)	0.230 (0.173)		0.210 (0.169)		0.305# (0.169)	
Teacher Experience: 0 Years						-0.019 (0.021)	-0.030 (0.024)	-0.032 (0.024)						-0.005 (0.016)	-0.001 (0.020)		-0.002 (0.020)	
Teacher Experience: 1-5 Years						0.000 (0.014)	-0.012 (0.016)	-0.010 (0.016)						-0.012 (0.009)	-0.012 (0.012)		-0.011 (0.012)	
Teacher Experience: 6-10 Years						-0.018 (0.022)	-0.034 (0.025)	-0.032 (0.025)						-0.036* (0.017)	-0.039# (0.020)		-0.038# (0.020)	
Teacher Experience: 11-20 Years						-0.019 (0.019)	-0.036 (0.024)	-0.030 (0.025)						-0.015 (0.014)	-0.014 (0.018)		-0.009 (0.019)	

E: Δ Language NPR

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Convert	0.498 (0.330)	0.491 (0.335)	0.456 (0.326)	0.450 (0.334)	0.417 (0.330)	0.427 (0.290)	0.365 (0.297)	0.381 (0.303)
Startup	0.287 (0.596)	0.153 (0.620)	0.965 (0.864)	0.148 (0.635)	0.759 (0.895)	0.135 (0.970)	0.724 (0.986)	0.576 (0.998)
Per-Student Expenditure (\$1000's)		0.006 (0.044)						-0.067 (0.071)
Per-Student Expenditure ² (\$1000's)		-0.001 (0.001)						0.001 (0.001)
Student-Teacher Ratio			0.097 (0.076)		0.111 (0.077)		0.138# (0.080)	0.091 (0.088)
Student-Teacher Ratio ²			-0.004 (0.002)		-0.004 (0.002)		-0.004# (0.002)	-0.003 (0.002)
Enrollment (1000's)				-0.031 (0.471)	0.050 (0.539)		0.049 (0.494)	-0.237 (0.532)
Enrollment ² (1000's)				-0.061 (0.127)	-0.072 (0.138)		-0.083 (0.127)	-0.021 (0.131)
Teacher Experience: 0 Years						-0.010 (0.014)	-0.010 (0.017)	-0.012 (0.017)
Teacher Experience: 1-5 Years						-0.004 (0.008)	-0.006 (0.010)	-0.005 (0.010)
Teacher Experience: 6-10 Years						-0.016 (0.014)	-0.018 (0.016)	-0.018 (0.016)
Teacher Experience: 11-20 Years						-0.021* (0.011)	-0.024# (0.015)	-0.024 (0.015)

Robust standard errors clustered by school in parentheses. Behavior and attendance regressions contain over 1,200,000 observations. Test score regressions contain over 500,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also include the following covariates: free or reduced price lunch status, other economic disadvantages, whether student undergoes a nonstructural switch, whether student undergoes a structural switch, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.