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

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Abstract: 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 schools which begin as charters generate improvements in student behavior and attendance but not test scores. Charters that convert from regular public schools have mixed effects on test scores. 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 under-perform, 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. Of the papers which use more advanced econometric techniques, 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). Thus, 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]

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

One potential explanation for this puzzle is that charter schools affect student outcomes in ways that researchers have not investigated. In particular, charter schools may provide improvements in outcomes such as discipline and attendance. If this is true, then charter schools may attract parents without providing test score improvements. Some recent evidence supports this argument. Recent work by Glomm, Harris, and Lo (2005) suggest that charters locate in places with more diverse tastes in which case charters would likely appeal to parents across a broad spectrum of outcomes instead of just test scores. In addition, studies of the preferences of parents directly indicate that non-academic factors play important roles in decisions regarding their children's education. In a survey of Texas charter parents, Weiher and Tedin (2002) find that only one-quarter of parents list test scores as the primary reason while more than two-thirds cite moral values, discipline, or safety. Some additional evidence outside of charter research also suggests that parents care about non-academic characteristics of education. Jacob and Lefgren (2005) find evidence that suggests parents care more about their children's satisfaction than academic outcomes when choosing teachers.

While it is important to understand parental preferences for selecting into charter schools, from an economic perspective we may care more about what outcomes such as discipline and attendance imply for student's performance in the labor market. Recent research has found that "non-cognitive" skills such as motivation, self esteem and sociability play an important role in labor market outcomes. In fact these skills may be more important than cognitive skills such as academic ability(Heckman, Stixrud and Urzua, 2006; Jacob, 2002; Heckman and Rubinstein, 2001). Unfortunately, the econometrician generally cannot observe these skills directly. However, Heckman, Stixrud, and Urzua (2006) show that non-cognitive skills are associated with better behavior later in life. Thus, discipline and attendance may reflect improvements in these skills, and thus provide us with a way to indirectly measure them. In particular, if charter schools generate improvements in these outcomes that last beyond when the students return to non-charter schools then we can be reasonably confident that the charter schools generate an improvement in non-cognitive skill sets.

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, 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]

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. For startup charters, I find large statistically significant improvements in discipline of 0.62 to 0.82 fewer disciplinary infractions per year depending on the particular model specification. This is a substantial reduction of between 54% and 72% of the pre-charter entry mean. I also find evidence of improvements in attendance, although these estimates change from statistically significant to insignificant in various specifications. However, I find little evidence of improvement in test scores. For conversions, while initial estimates suggest improvements in all outcomes, once a gifted and talented magnet school is removed from the

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

analysis, the impacts mostly become statistically insignificant.

All of the previously cited papers on charter schools use individual fixed effects or similar analyses except for Hoxby and Rockoff (2004).[4] 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.. 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, 2005; 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

[4]Hoxby and Rockoff (2004) use oversubscription lotteries to identify charter impacts. These are admission lotteries conducted by schools that have more applicants than spaces available, While this strategy is effective at eliminating bias, it usually limits studies to a small number of schools, in this case three. In addition, these schools are likely of higher quality than the average charter since having a lottery is an indicator of high demand for a school.

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 (2007) 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.

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 both "in-charter" and "post-charter" indicators. 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.[5] Thus, if charters provide the same level of long-term performance and cost less money, they still enhance the efficiency of the education system.

[5]National Center for Education Statistics, School District Finance Survey.

2 Background on Charter Schools

2.1 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 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

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

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

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]

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

[8]Center for Education Reform.

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.[9]

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.[10]

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. 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.

[9] The evidence on the effectiveness of teacher certification has been mixed (Glazerman, Mayer and Decker, 2006; Chatterji, 2005; Darling-Hammond, Holtzman and Gatlin, 2005; Hoxby, 2002; Hanushek, Kain and Rivkin, 1999; Berger and Toma, 1994)

[10] Author'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.

2.2 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[11]. 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[12]. 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, although there are more than fifty non-district charter schools in ALUSD for whom I do not have student-level data. 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 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[13].

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

[12]Since I do not know how many students in the non-district charters would have attended ALUSD otherwise, the enrollment totals may overestimate the actual student population 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.

[13]One concern may be that, since the average grade in startup charters is greater than for conversion

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[14].

Since not all students take the norm-referenced examination and test data are only available starting in 1998, I generate two samples.[15] I call the first sample the "base sample." This sample is used when analyzing any outcome other than test scores. It includes students in grades 1-12 who were enrolled as of the end of October of each year, since this is when demographic information is collected by the district. The demographic files identify the school a student attends and thus I use this as the student's school for the year. Some observations are excluded due to missing attendance data ($< 0.1\%$), leaving more than 1.2 million observations of which more than 50,000 are students in charter schools.[16]

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-

charters, then this could generate different estimates for the two schools. To address this, I include grade-by-year fixed effects in all of my regressions.

[14]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.

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

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

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.[17] 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 infractions.[18]

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

[17]Twenty-four percent of elementary student-year observations in the base sample have no test score because they take the Spanish language exam, but by the time students reach middle school, almost all are taking the English language exam. In high school, 23% of student-years in the base sample are missing test scores. This is mostly due to students dropping out of school or moving out of the district between October and the testing period in late winter. Some students also are missing test scores due to illness or suspension during the testing period. A complete accounting of data exclusions by year and grade level is provided in the web appendix. Once concern that has been raised with regards to the missing test scores is that charters may include fewer LEP or special education students, thus potentially biasing the results. However, test score regressions limited to students who are not classified as LEP or special education for the duration of the test sample show very similar results to the baseline regressions, suggesting that this is not a substantial problem.

[18]Test 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.

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.

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) instead. The ATT is defined as

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

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 $E(y_{it}^0 | c_{it} = 1)$.

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

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

where ϕ_i is an time-invariant individual specific effect. Under the additional assumption of strict exogeneity which states that the outcome measure is uncorrelated with charter status and exogenous characteristics in past or future periods, or

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

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

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

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[19], $Switch_{it}$ is a set of variables that define whether a student changes schools in year t[20] ,$Gradeyear_{it}$ 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.

Since there is substantial serial correlation in test scores, researchers often use a value added version of the fixed-effects model. In this case, first-differences are applied to the dependent variable but not the right-hand side of the equation, and then both sides

[19] In all of my models this includes free-lunch status, reduced-price lunch status, other economic disadvantage, recent immigration status, and parent's migrant worker status. Descriptions of these variables can be found in Appendix Table 1.

[20] I follow Bifulco and Ladd (2006) and split school switches into "structural" and "non-structural" switches where the latter is defined as switching into a school that less than 10% of a student's previous class switches into in year t. Conversely, a student undergoes a structural switch when more than 10% of his or her previous class switch into the same school in year t. 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. 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.

of the equation are demeaned to remove the fixed-effect[21]. In this framework, my model becomes

$$\Delta y_{it} = \alpha + \theta C_{it} + Demog_{it}\Gamma + Switch_{it}\Phi + Gradeyear_{it}\Psi + \phi_i + \epsilon_{it}. \tag{5}$$

This model implies that that the lagged dependent variable predicts the current outcome with a coefficient restricted to one. Unfortunately, it is unlikely that the true coefficient on $y_{i,t-1}$ is one, since we would expect some decay in the predictive power of previous year's outcomes on current outcomes. To address this Hanushek, et al. (2007) use a model which allows for the coefficient on past outcomes to be unrestricted

$$\Delta y_{it} = \alpha + \theta\Delta C_{it} + \Delta Demog_{it}\Gamma + \Delta Switch_{it}\Phi + \Delta Gradeyear_{it}\Psi + \delta\Delta y_{i,t-1} + \Delta\epsilon_{it}. \tag{6}$$ [22]

Since $\Delta y_{i,t-1}$ is endogenous by construction, one needs to instrument for it. Hanushek, et al. use $y_{i,t-2}$ as their instrument. While this is valid under the assumption that more than once lagged test scores, and by extension unobservables, have no independent role in current test score gains, recent research has suggested that factors in children's distant youth play important roles in later achievement (Todd and Wolpin, 2004). Thus, rather than risk the problems inherent with this endogeneity, I use both level and value-added fixed effects models. It turns out that if the correct model is of a form similar to equation (6) then the expected values of these two estimates will bound the true impact. A proof of this statement is provided in the appendix.

There has also been some concern in the literature regarding the validity of fixed-

[21] An alternative model would be the random trend model where both sides of the estimating equation are differenced then demeaned. This has the advantage of allowing for an unobserved individual linear time-trend. However, it also has the undesirable effect of substantially reducing precision in the instrumental variables models I use later. It also could potentially exacerbate bias if individual specific trends are non-linear. Since the point estimates of the charter impacts in my preferred models using random trends are similar to the value-added models, I do not include them here, although those results are available upon request.

[22] Hanushek, et al. first-difference their model to remove the fixed effect, which is very similar to the demeaning framework I use in this paper.

effects as a strategy for identifying charter impacts, particularly by two recent papers (Ballou, Teasley and Zeidner, 2006; Hoxby and Murarka, 2006). 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[23]

Table 3 provides initial estimates using both levels fixed-effects models and value-added fixed-effects models. The standard errors for each regression are robust to heteroskedasticity and clustered by school.[24] In column one I group all charters together into one indicator variable. In both level and value-added models there are statistically significant reductions in disciplinary infractions and improvements in math test scores. There

[23]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 then 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.

[24]Some 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.

is a statistically significant drop in reading scores in the value-added specification but, the estimate is positive and insignificant in levels. Thus, using these basic models, we see that charters appear to be a net positive... improving both discipline and math scores.

However, these results hide a substantial amount of heterogeneity. Column 2 shows the same regressions, but the charters are now 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. In addition, these improvements are substantial. The drop of 0.68 to 0.83 infractions per year when students enter a startup charter is equal to 59% to 72% 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 statistically significant improvements in value-added attendance of 2.6 percentage points relative to a baseline absentee rate of eleven percent in the year prior to startup entry. For test scores, on the other hand, only math scores for conversions are statistically significant in both levels and value-added models. Interestingly, despite the evidence for discipline and attendance improvements in startups, test scores show no statistically significant changes and in four out of six cases the point estimates are negative.

These results rely on the assumptions underlying fixed-effects being valid. While I will address two important potential violations of the fixed-effects assumptions in the next section - endogenous entry and attrition bias - two other problems, endogenous exit and persistence, can be addressed in a simple manner. Thus, I will incorporate this correction into my baseline analysis.

The problem is two-fold. First of all, students who perform poorly in charters may leave a charter prematurely to attend a regular public school. To the extent this is determined by permanent characteristics of the student, then fixed-effects corrects the bias. However, if it is based on time-varying characteristics then the charter estimates will be biased. Secondly, if charters have long-term impacts on outcomes that remain after students return to regular public schools, then fixed-effects analyses will mistakenly apply these long-term improvements to non-charter periods, thus biasing charter estimates towards zero. One way to solve both of these problems is include indicators for whether a student is in a "post-charter" period[25]. This will cause the identification of charter effects to be based on the difference between outcomes while in the charter and outcomes prior to charter entry. It will also provide us with measures of the extent to which charter impacts persist beyond students' tenures in charter schools. Of course, the "post" variable is itself potentially endogenous due to the endogenous exit problem. To account for this I instrument for "post" with whether a student is grade ineligible for the last charter he or she attended. In order to avoid the potential endogeneity of the instrument through grade retention, I use the student's predicted grade rather than actual grade. For example, if a student is in grade 7 in year $t-1$ he is assumed to be in grade 8 for the purposes of grade eligibility regardless of whether or not he is held back.

Table 4 provides results including "post" indicators. For this table, and throughout the rest of the analysis, I focus only on models that separate conversion and startup charters. Both the fixed-effects (i) and IV-FE (ii) results in Table 4 are very similar, which suggests that endogenous exit is not a large issue in these data[26]. In addition to the econometric concerns, there are two other interesting results from this analysis. The first is that test score estimates for conversions become significant for all tests in the levels model. However, these results do not hold up in the value-added model, except for math. Attendance impacts for conversions also become statistically significant and positive. The second is that, with the exception of attendance in conversion charters, there appears to be very little persistence in charter impacts. Most dramatically, the discipline estimates for startup charters drop essentially to zero in the post-startup periods. This implies that students revert to what

[25] In models that separate charters into conversions and startups, I use both indicators for "post-conversion" and "post startup." It is possible to be in conversion charter and also in a post-startup period at the same time (and conversely for startup and post-conversion), however only 1% of charter students ever attend both types.

[26] The first stage estimates for the IV regressions show the instrument to be statistically significant at the 1% level in all cases. These are available upon request.

their infractions would have been had they never attended a charter [27] Since the main effects are similar in both the FE models with "post" and the IV-FE models, I will use the OLS based models for the rest of the paper.

For conversion charters the results suggest that there are substantial improvements in discipline, attendance, and test scores. However, these results are misleading because they are sensitive to the inclusion of one gifted and talented charter magnet school. Table 5 shows the same fixed-effects regressions as in table 4, but drop any student who aver attends the gifted and talented school and show that, except for the test scores in the levels model, all of the conversion estimates become statistically insignificant. Since the levels and value added models bound the true estimate under the assumptions outlined above, we can only take this as suggestive, but not conclusive, evidence of test score improvements in conversions.

While the drop in disciplinary infractions while students are in startup charters, and the subsequent reversion after leaving, 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 criminal activity - I find similar results.[28] 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 will later show evidence of statistically significant reductions in expulsions and the likelihood of having any infraction, so the results are consistent across different margins. Third, the marginal improvements in attendance suggest that at least

[27] A caveat to these results is that random trend IV models suggest that there is more persistence than the models shown here, but none of the "post" coefficients are statistically significant. However, these models are estimated very imprecisely, making it difficult to draw any conclusions from them. These results are available upon request.

[28] These results can be found in the Appendix Table 2. A more detailed description can be found in the web appendix.

some of the discipline effects are real, since attendance is highly correlated with behavior and is much harder to misrepresent. Later I will also show that the attendance results become statistically significant at the at a more precise level after controlling for some school characteristics. [29] Fourth, at three to 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. Finally, I show later that this result can mostly be explained by differences in school size and class size, which we would not expect to see if the discipline improvements are not real.

5 Correcting for 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 $E(y_{it}|c_{it},...,c_{iT},\mathbf{X}_{i1},...\mathbf{X}_{iT},\phi_i) \neq E(y_{it}|c_{it},\mathbf{X}_{it},\phi_i)$; i.e. y is correlated with future c. In addition, if the outcome measures

[29] The 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.

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 off 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. Figure 4 shows the same patterns for test scores in startup charters.

In order to address the potential endogeneity generated by selection based on changes in outcomes I use 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.

Table 6 provides the results of these interrupted panel estimations. In the first column, I drop all observations in the year prior to when a student enters a charter school from a non-charter school. Since, for value added regressions, the drop in level outcomes prior to charter entry will increase the value added outcome after charter entry, I replace $y_{i,t} - y_{i,t-1}$ in the year after charter entry with $\frac{y_{i,t}-y_{i,t-2}}{2}$ in column 2. In the third column I drop the two years prior and in the fourth I do the same procedure as in column 2 except now two prior years are dropped and the new dependent variable for charter enterers is $\frac{y_{i,t}-y_{i,t-3}}{3}$. For both types of charters discipline results change little when applying the interrupted panels. Attendance is consistent across specifications for conversions in both levels and value added. It is also consistent with the baseline regressions for startups in the levels models. In the value added models, the attendance impact for startups becomes statistically insignificant when dropping the pre-charter periods alone, but it remains significant at the 10% or lower level when I adjust for the contribution of the pre-charter periods to post-charter outcome measures in columns 2 and 4. Test score results are qualitatively similar across specifications for conversions in both models and startups in the levels framework. In the value-added models for startups, test scores are generally negative and insignificant in columns 1 and 3, but, as with attendance, these increase substantially to become positive in columns 3 and 4. However all of the estimates are statistically insignificant at the 10% level except for one estimate. Thus, the interrupted panel results suggest that selection into charters off of outcomes has little impact on the fixed-effects estimates[30]

5.2 Attrition

Another problem is that 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

[30] I also use interrupted panel strategies to conduct another check on the extent of bias from endogenous exit. These results showed only minor changes in estimates from dropping the year prior to charter exit and did not qualitatively change the results.

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.[31] 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 4^{th} through 7^{th} 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

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

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

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

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.

Table 7 provides a probit regression of whether a student attrits in the following year on a range of observable characteristics. If attrition is random then we would expect very few of these characteristics to have statistically significant correlations with attrition probability. Unfortunately, this is not the case. Attrition is correlated with almost all of the observable characteristics and outcomes listed. Thus the evidence in Figure 4 and Table 6 suggests that attrition is likely correlated with both y and c and therefore has the potential to generate bias.

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 reweighing 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.[32]

To apply Kyriazidou's strategy, I run a first-differenced version of (4) weighted by kernel weights of the form

$$\widehat{\psi}_{it,n} = \frac{1}{h_n} K\left(\frac{(W_{it} - W_{is})\widehat{\Omega}}{h_n}\right) \tag{8}$$

where K is a kernel function with bandwidth h_n and $(W_{it} - W_{is})\widehat{\Omega}$ is the first-differenced linear prediction from a conditional "fixed effects" logit model of being in the sample in year

[32] A more detailed description of Kyriazidou's (1997) estimator is provided in the online appendix.

t.[33] For consistent estimation W_{it} and W_{is} must contain an exclusion restriction. The bandwidth h_n falls with sample size n via the formula $h_n = h * n^{-1/(2(r+1)+1)}$ where h is some constant and r is the order of differentiability of the kernel at almost all points minus one. Thus, choosing the bandwidth is equivalent to choosing the constant itself. The appropriate bandwidth is found using the mean-squared error (MSE) minimization procedure described in Kyriazidou (1997).

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 her predicted grade as defined previously exceeding the maximum grade of that school. [34]

Table 8 provides the results of the selection corrected estimates for the value-added models along with unweighted first-differences regressions for comparison. In addition to the MSE 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 bandwiths to the unweighted estimates we see that the charter effects are very similar across the analyses. 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.

[33] This allows for unbalanced panels by differencing with respect to the last observation for individual i prior to year t, which is s, rather than always differencing with respect to $t-1$.

[34] 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. 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.

6 Extensions

Table 9 provides some results on additional outcome measures of interest and looks at whether charter effects vary by student type and school characteristics. Since these are binary outcomes, a levels framework is more appropriate. Thus, I show levels fixed-effects models which include the same covariates as in the regressions in section four. The additional outcomes include whether a student has any disciplinary actions in a year, whether a student is expelled, limited English proficiency, and at-risk status. Startup charters provide statistically significant improvements in all of these, except LEP for the Hispanic sub-sample. Conversion charters provide improvements in having any disciplinary actions and expulsions, but exhibit increases in LEP rates. There are two potential explanations for this result. One is that the conversion charters may be more effective at identifying whether a student is LEP. Another is that, since LEP status is partially based on reading and language test scores it is possible that schools are reclassifying students as LEP if their test scores fall.

Table 9 shows what happens to the estimates if I add some commonly used school quality measures. 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. For most outcomes, the results are quite stable when different school quality measures are added. However, there are some changes for startup charters that are noteworthy. The first is that, when student-teacher ratios and enrollment are added to the regressions, the discipline impacts fall by more than half in the levels model and by more than 80% in the value added model. This suggests that most of the discipline gains from startup charters may be due to closer supervision from having smaller schools and more teachers per student [35] Second, for the standard errors on attendance rates fall considerably so that the estimates become significant at the 5% level in both models, thus providing further evidence of improvements in attendance.

[35] The coefficients on enrollment and student-teacher ratios suggest that infractions positively correlate with these variables.

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.

I find weak evidence of improvements in test scores for charters which convert from regular public schools (conversion charters), but not discipline or attendance, provided that I exclude one gifted and talented magnet charter. On the other hand, my results show that schools that begin as charters (startup charters) are effective at improving student behavior and attendance, although they have no statistically significant impact on test scores. The discipline impacts, in particular, are quite large. Attending a startup charter generates a drop of 0.6 to 0.8 disciplinary infractions per year. 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 drop by 50% in the levels model and 80% in the value added model. 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 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, 2005; 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 robust to potential attrition bias. Finally, I find little evidence of persistence in charter impacts after students return to non-charters.

Taken together, these results paint a mixed picture of charter schools. On the one hand, startup 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.

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Appendix

Proof of Expected Value of Level and Value-Added Fixed Effects Estimates Bounding the Lagged-Dependent Variable Model with Fixed Effects

Let us first simplify notation and denote $\mathbf{X}$ as a $k \times nt$ vector of demeaned covariates while $\mathbf{Y}$ is a $1 \times nt$ vector of the demeaned student outcome variable and $\mathbf{Y_{t-1}}$ is the $1 \times nt$ vector of demeaned once-lagged outcome variables. Our true model becomes

$$\mathbf{Y_t} = \mathbf{X}\beta + \mathbf{Y_{t-1}}\gamma + \epsilon \tag{9}$$

In a levels framework, the lagged outcomes enter into the error term such that we have composite error

$$\mu = \gamma \mathbf{Y_{t-1}} + \epsilon. \tag{10}$$

This provides us with

$$\mathbf{E}(\hat{\beta}^{\mathbf{L}}) = \beta + \gamma[\mathbf{X'X}]^{\mathbf{-1}}[\mathbf{X'E}(\mathbf{Y_{t-1}})] \tag{11}$$

For a value added model we subtract $\mathbf{Y_{t-1}}$ from each side of (9) to get

$$\mathbf{Y_t} - \mathbf{Y_{t-1}} = \mathbf{X}\beta + (\gamma - \mathbf{1})\mathbf{Y_{t-1}} + \epsilon \tag{12}$$

which will provide us with an estimate of β such that

$$\mathbf{E}(\hat{\beta}^{\mathbf{VA}}) = \beta + (\gamma - \mathbf{1})[\mathbf{X'X}]^{\mathbf{-1}}[\mathbf{X'E}(\mathbf{Y_{t-1}})] \tag{13}$$

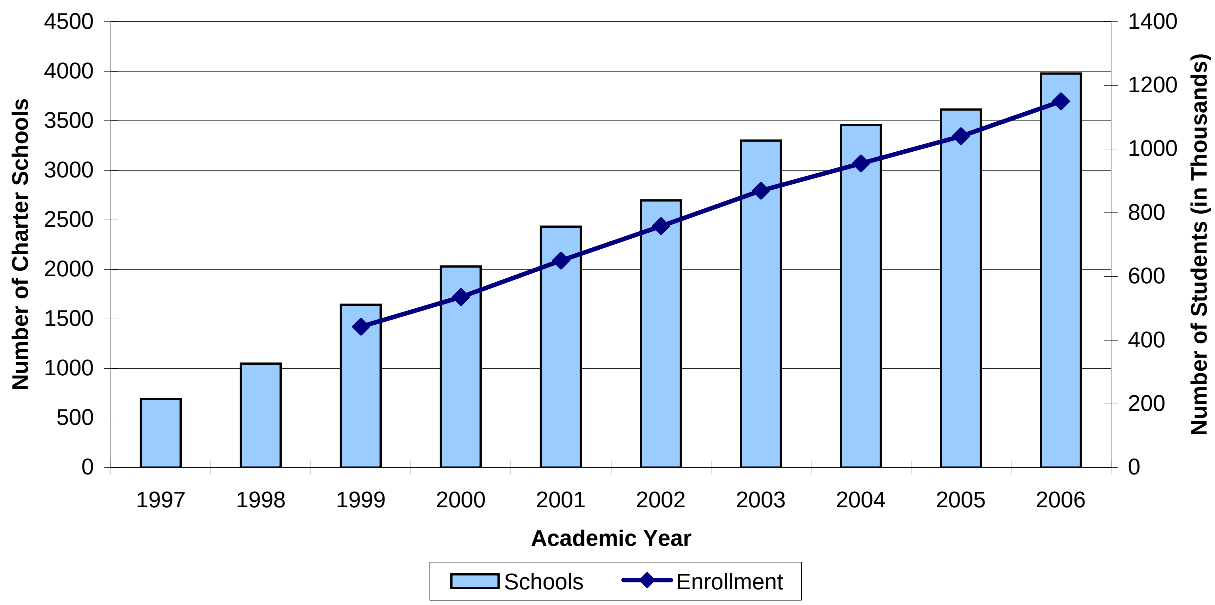
Let us further define the matrix $\mathbf{A} = [\mathbf{X'X}]^{\mathbf{-1}}[\mathbf{X'E}(\mathbf{Y_{t-1}})]$ and the k^{th} row of $\mathbf{A}$ as $\mathbf{A_k}$, hence

$$\mathbf{E}(\hat{\beta}^{\mathbf{L}}_{\mathbf{k}}) = \beta_{\mathbf{k}} + \gamma \mathbf{A_k} \tag{14}$$

$$\mathbf{E}(\hat{\beta}^{\mathbf{VA}}_{\mathbf{k}}) = \beta_{\mathbf{k}} + (\gamma - \mathbf{1})\mathbf{A_k} \tag{15}$$

Thus, assuming that $0 \leq \gamma \leq 1$, if $\mathbf{A_k} > \mathbf{0}$ then $\mathbf{E}(\hat{\beta}^{\mathbf{L}}_{\mathbf{k}}) > \beta > \mathbf{E}(\hat{\beta}^{\mathbf{VA}}_{\mathbf{k}})$ while if $\mathbf{A_k} < \mathbf{0}$ then $\mathbf{E}(\hat{\beta}^{\mathbf{L}}_{\mathbf{k}}) < \beta < \mathbf{E}(\hat{\beta}^{\mathbf{VA}}_{\mathbf{k}})$. In either case, the levels model and value added models bound β.

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

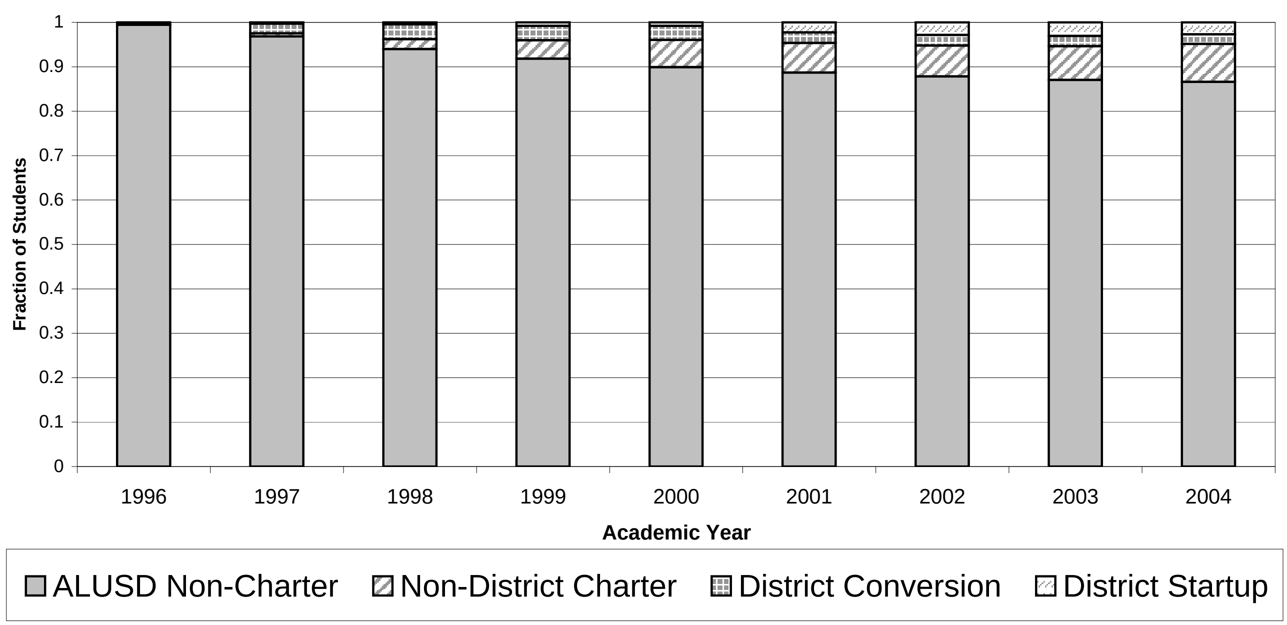
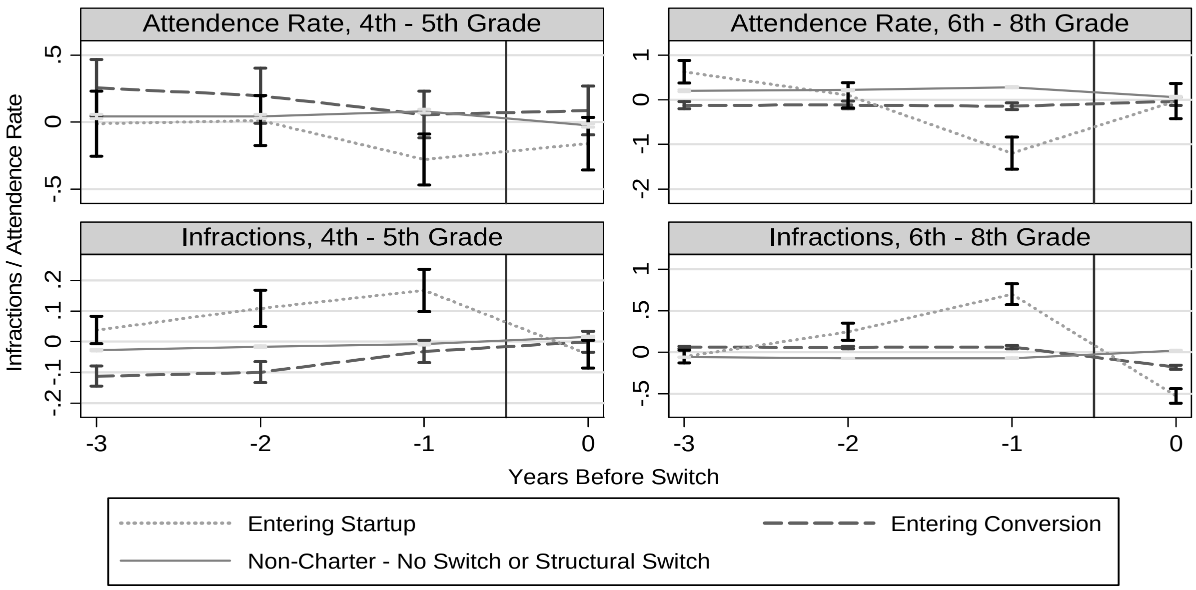


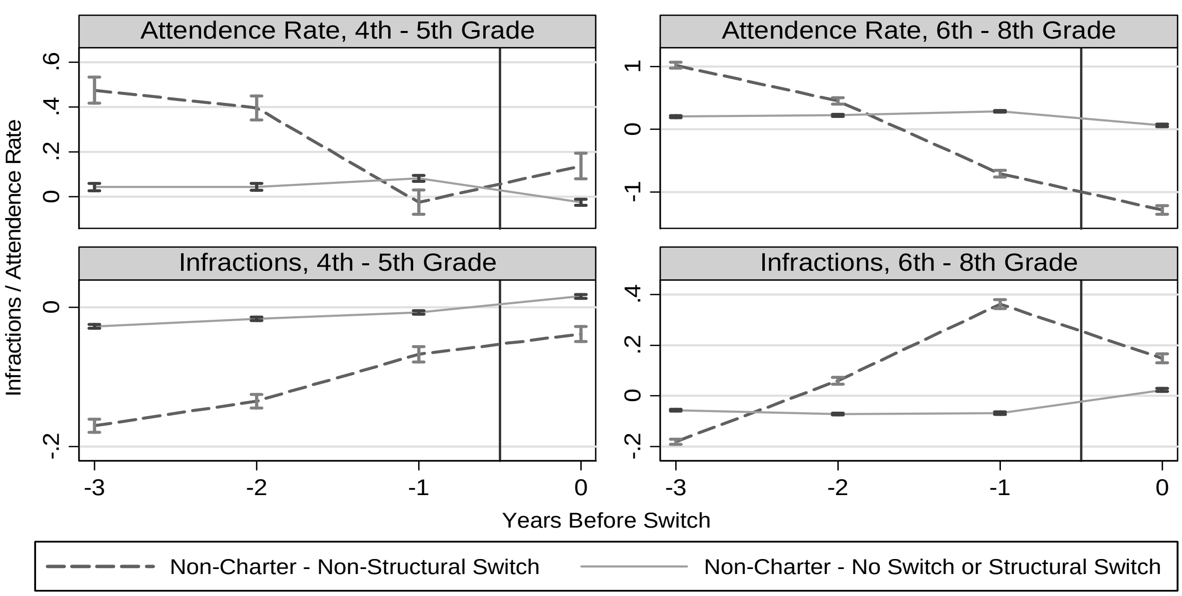
Table shows the fraction of students in each type of school in ALUSD along with non-district charters in the region around ALUSD as defined by the state Department of Education.

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



Outcomes are de-meaned 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.

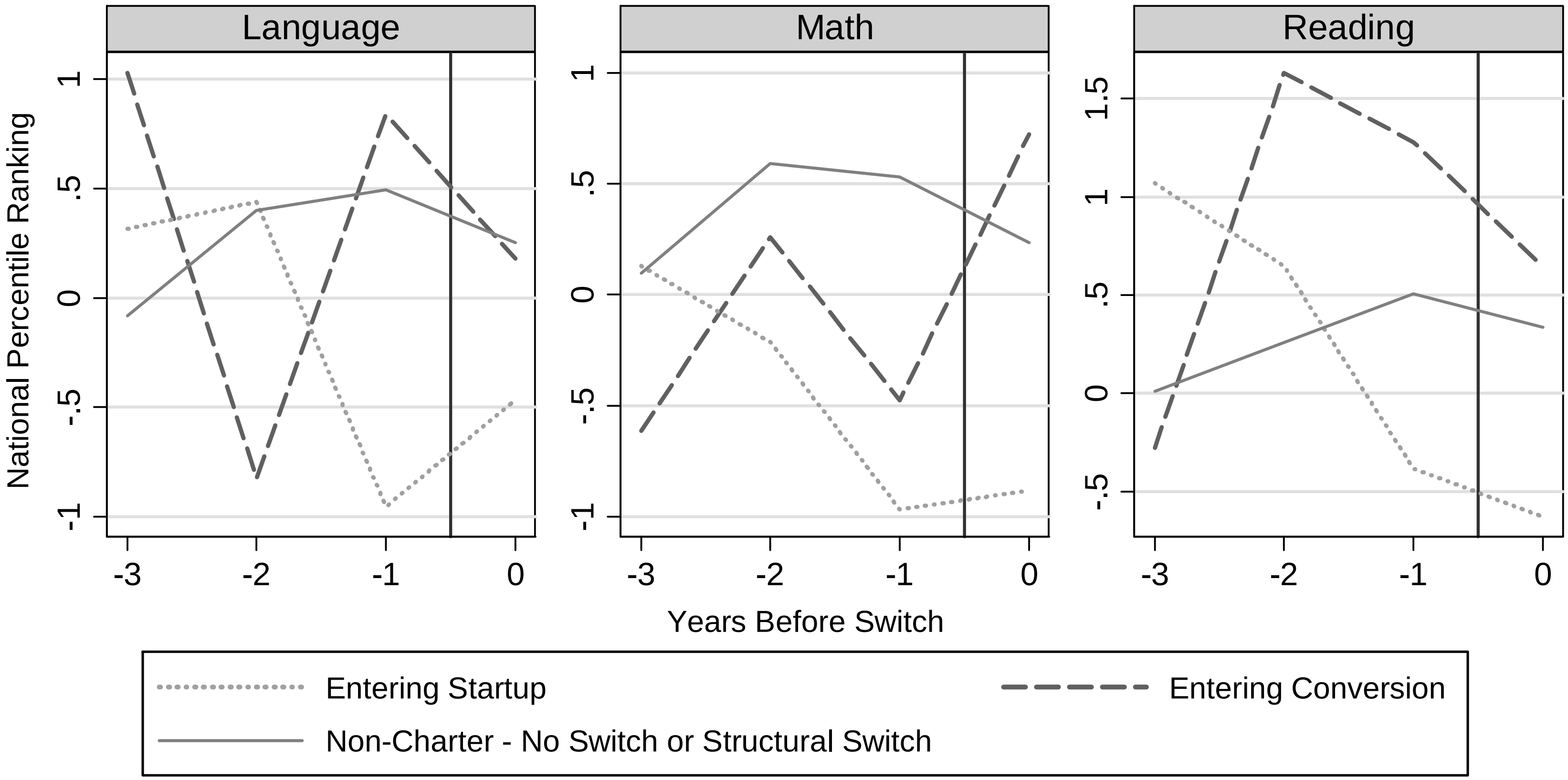
Figure 3B: Disciplinary Infractions and Attendance
Before and After Non-Charter School Switch



Outcomes are de-meaned 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 - Standardized Examination Annual Score Levels
Before and After Entering Charters

4th - 8th Grades



Outcomes are de-meaned 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 5: Transitions Between School Types

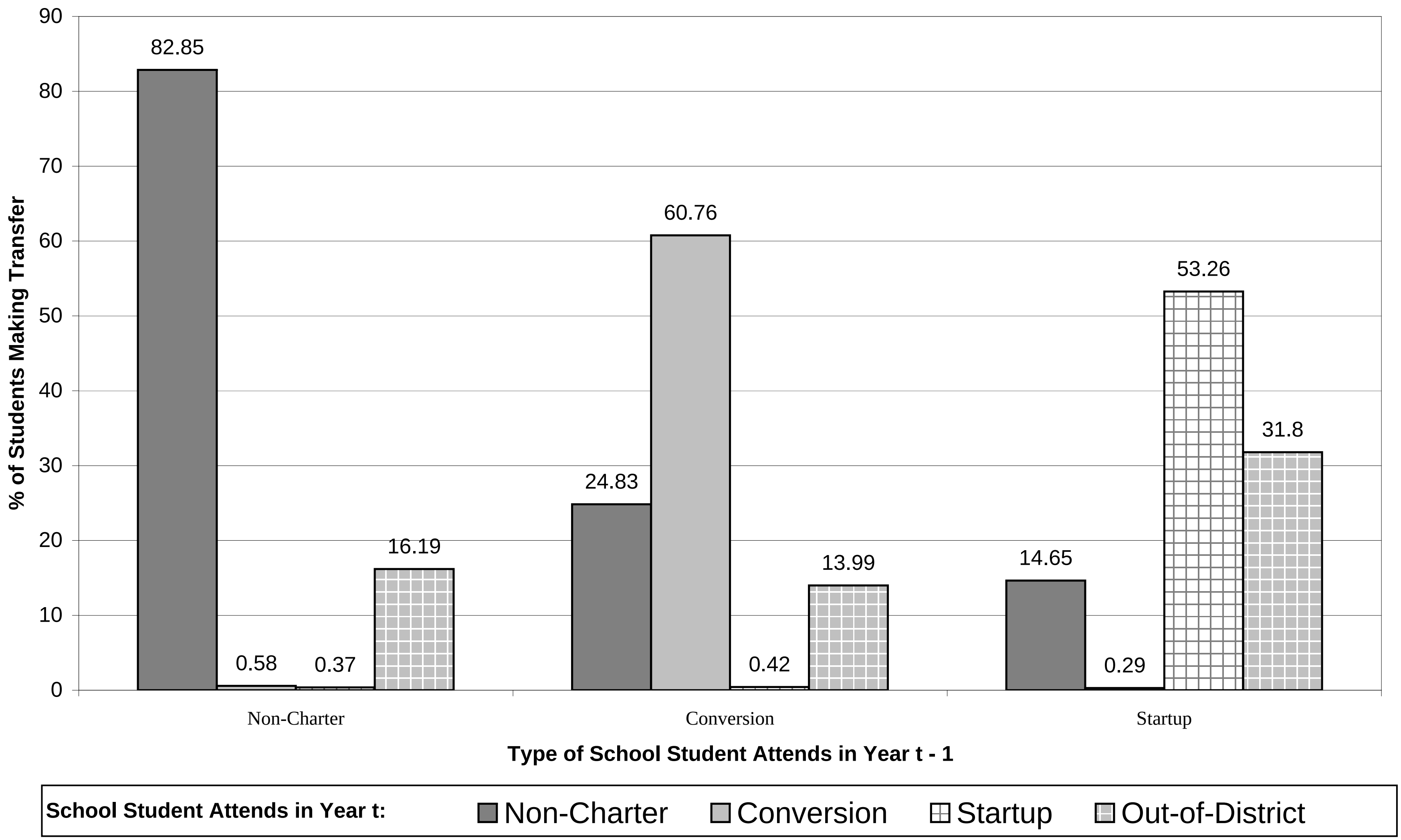


Table 1 - School Characteristics in 2004

	ALUSD Non-Charters	Conversion Charters	Startup Charters	Non-District Charters
Student Demographics (% of All Students in School)				
Limited English Proficient	30.3	18.8	12.2	10.9
		(1.4)	(3.3)	(6.3)
Economically Disadvantaged	86.0	89.2	84.2	70.9
		(0.5)	(0.4)	(5.1)
At-Risk	63.5	49.2	49.0	60.0
		(2.2)	(3.0)	(1.1)
Special Education	10.8	8.2	5.9	12.5
		(0.8)	(2.1)	(1.1)
Gifted	9.3	11.9	4.2	1.8
		(0.6)	(1.6)	(4.5)
White, Non-Hispanic	7.2	5.6	6.8	14.1
		(0.3)	(0.1)	(3.2)
School Demographics				
Teacher Experience (% of Teachers in School)				
0 - 5 Years	39.2	58.4	55.2	65.2
		(3.8)	(2.1)	(11.6)
6 or More Years	60.8	41.6	44.8	34.8
		(3.8)	(2.1)	(11.6)
Student-Teacher Ratio	16.2	16.5	17.1	17.2
		(0.2)	(0.5)	(1.8)
Per-Pupil Operating Expenditures	$6,916	$5,773	$5,032	$6,394
		(0.6)	(1.4)	(0.6)
Enrollment	895	769	433	373
		(0.6)	(3.4)	(7.5)
Student Outcomes				
Attendance Rate	95.0	97.0	93.3	91.0
		(0.8)	(0.9)	(3.3)
State Exam - Math				
% Passing at Low Level	61.9	71.6	54.6	42.0
		(1.2)	(1.2)	(5.7)
% Passing at High Level	14.7	18.2	10.9	7.4
		(0.8)	(1.1)	(4.2)
State Exam - Reading				
% Passing at Low Level	73.1	84.0	71.8	58.0
		(1.8)	(0.3)	(5.0)
% Passing at High Level	17.3	23.2	15.6	11.1
		(1.3)	(0.5)	(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

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

A. Fixed-Effects in Levels

	(1)	(2)	
	Any Charter	Conversion	Startup
# of Infractions	-0.371**	-0.218*	-0.828**
	(0.087)	(0.091)	(0.120)
Attendance Rate (%)	0.532	0.132	1.723
	(0.414)	(0.161)	(1.268)
Mathematics NPR	1.758**	2.103**	0.460
	(0.540)	(0.573)	(1.454)
Reading NPR	0.584	0.789#	-0.187
	(0.389)	(0.452)	(0.933)
Language NPR	0.754#	0.978#	-0.088
	(0.429)	(0.525)	(0.830)

B. Value Added Fixed-Effects

# of Infractions	-0.234**	-0.099#	-0.678**
	(0.086)	(0.054)	(0.199)
Attendence Rate (%)	0.657	0.058	2.625*
	(0.450)	(0.102)	(1.299)
Mathematics NPR	1.400**	1.892**	-0.639
	(0.488)	(0.489)	(0.969)
Reading NPR	-0.708*	-0.533	-1.433
	(0.324)	(0.340)	(0.900)
Language NPR	0.436	0.502	0.162
	(0.289)	(0.331)	(0.617)

Robust standard errors clustered by school in parentheses. Behavior and attendence 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 nonstructual 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 - Regressions Including Indicators for Being in a "Post Charter" Period

A. Fixed-Effects in Levels

i. Not Instrumenting for "Post"

	In Conversion	Post Conversion 1 Year	Post Conversion 2+ Years	In Startup	Post Startup 1 Year	Post Startup 2+ Years
# of Infractions	-0.263**	-0.145*	-0.077	-0.824**	0.046	-0.006
	(0.098)	(0.064)	(0.061)	(0.125)	(0.085)	(0.107)
Attendance Rate (%)	0.477**	0.799**	0.777**	1.573	-1.106	-0.875
	(0.181)	(0.283)	(0.209)	(1.359)	(0.690)	(0.702)
Mathematics NPR	2.642**	0.734	1.017	0.261	-0.383	-1.108
	(0.434)	(0.725)	(0.846)	(1.614)	(1.286)	(1.610)
Reading NPR	1.045**	0.348	0.488	-0.431	-0.104	-1.767
	(0.407)	(0.466)	(0.543)	(1.071)	(0.970)	(1.117)
Language NPR	1.356*	0.327	0.849#	-0.084	0.943	-0.903
	(0.568)	(0.466)	(0.455)	(0.909)	(0.971)	(1.265)

ii. Instrumenting for "Post"

	In Conversion	Post Conversion 1 Year	Post Conversion 2+ Years	In Startup	Post Startup 1 Year	Post Startup 2+ Years
# of Infractions	-0.253*	-0.125	-0.054	-0.834**	-0.117	0.018
	(0.108)	(0.107)	(0.077)	(0.120)	(0.299)	(0.133)
Attendance Rate (%)	0.519**	0.899*	0.873**	1.591	-1.159	-0.555
	(0.204)	(0.387)	(0.256)	(1.380)	(1.226)	(0.827)
Mathematics NPR	2.747**	0.947	1.139	0.837	1.876	1.098
	(0.587)	(1.074)	(1.013)	(1.727)	(1.973)	(1.874)
Reading NPR	0.855*	-0.209	0.363	-0.419	-1.300	-0.513
	(0.393)	(0.692)	(0.696)	(1.073)	(2.141)	(1.379)
Language NPR	1.238*	-0.037	0.784	0.010	0.188	0.556
	(0.559)	(0.576)	(0.529)	(0.923)	(2.127)	(1.602)

B. Value-Added with Fixed-Effects

i. Not Instrumenting for "Post"

	In Conversion	Post Conversion 1 Year	Post Conversion 2+ Years	In Startup	Post Startup 1Year	Post Startup 2+ Years
# of Infractions	-0.111*	0.028	-0.063	-0.625**	0.701**	-0.010
	(0.051)	(0.080)	(0.041)	(0.197)	(0.132)	(0.135)
Attendance Rate (%)	0.242*	0.431#	0.346*	2.572#	-0.318	-0.394
	(0.105)	(0.228)	(0.137)	(1.370)	(0.618)	(0.655)
Mathematics NPR	2.376**	-0.070	1.597#	-0.707	0.072	-0.755
	(0.767)	(0.942)	(0.952)	(1.054)	(1.211)	(1.339)
Reading NPR	-0.519	-0.181	0.199	-1.189	1.835#	-0.184
	(0.378)	(0.691)	(0.466)	(1.066)	(1.061)	(1.171)
Language NPR	0.169	-1.284**	0.140	0.366	1.534	-0.290
	(0.369)	(0.436)	(0.401)	(0.696)	(1.125)	(1.257)

ii. Instrumenting for "Post"

	In Conversion	Post Conversion 1 Year	Post Conversion 2+ Years	In Startup	Post Startup 1 Year	Post Startup 2+ Years
# of Infractions	-0.068	0.175	-0.010	-0.604**	1.023*	-0.046
	(0.071)	(0.152)	(0.057)	(0.184)	(0.452)	(0.154)
Attendance Rate (%)	0.199	0.138	0.396*	2.424#	-2.376*	-0.269
	(0.142)	(0.385)	(0.171)	(1.420)	(1.131)	(0.855)
Mathematics NPR	1.913#	-1.022	1.031	-0.792	-0.033	-1.586
	(1.038)	(1.485)	(1.283)	(1.149)	(2.714)	(2.344)
Reading NPR	-0.944	-1.109	-0.227	-1.973	-2.651	-1.779
	(0.950)	(1.919)	(1.242)	(1.281)	(4.182)	(2.088)
Language NPR	-0.035	-1.847#	0.071	-0.398	-2.981	-1.596
	(0.709)	(1.122)	(0.806)	(0.855)	(3.416)	(2.350)

Robust standard errors clustered by school in parentheses. Base sample regressions contain over 1,200,000 observations. Test sample 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 nonstructual switch, whether student undergoes a structural switch, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Regressions Excluding Students Who Ever Attend
Gifted & Talented Magnet Conversion

A. Fixed-Effects in Levels

	Conversion	Startup
# of Infractions	-0.028	-0.823**
	(0.097)	(0.123)
Attendance Rate (%)	0.006	1.659
	(0.274)	(1.343)
Mathematics NPR	3.560**	0.438
	(0.707)	(1.619)
Reading NPR	2.858**	-0.366
	(0.771)	(1.081)
Language NPR	3.914**	-0.037
	(0.602)	(0.925)

B. Value-Added with Fixed-Effects

	Conversion	Startup
# of Infractions	-0.031	-0.624**
	(0.061)	(0.198)
Attendance Rate (%)	-0.104	2.646#
	(0.161)	(1.366)
Mathematics NPR	0.833	-0.813
	(0.915)	(1.050)
Reading NPR	-0.160	-1.261
	(0.914)	(1.085)
Language NPR	0.911	0.384
	(0.718)	(0.700)

Robust standard errors clustered by school in parentheses. Behavior and attendence 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 nonstructual 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 - Interrupted Panel Fixed Effects Regressions of Charter Impact

A. Fixed-Effects in Levels

	Conversion				Startup			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
# of Infractions	-0.270**	-	-0.302**	-	-0.803**	-	-0.772**	-
	(0.105)	-	(0.102)	-	(0.118)	-	(0.111)	-
Attendance Rate (%)	0.556**	-	0.564**	-	1.249	-	1.109	-
	(0.201)	-	(0.203)	-	(1.403)	-	(1.429)	-
Mathematics NPR	2.841**	-	3.191**	-	-0.772	-	-1.406	-
	(0.554)	-	(0.690)	-	(1.661)	-	(1.607)	-
Reading NPR	1.455**	-	2.762**	-	-1.070	-	-1.498	-
	(0.363)	-	(0.440)	-	(1.214)	-	(1.258)	-
Language NPR	1.570**	-	0.690	-	-0.695	-	-1.324	-
	(0.477)	-	(0.592)	-	(1.045)	-	(1.041)	-

B. Value-Added with Fixed-Effects

	Conversion				Startup			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
# of Infractions	-0.305**	-0.314**	-0.335**	-0.340**	-0.821**	-0.829**	-0.782**	-0.787**
	(0.098)	(0.102)	(0.093)	(0.096)	(0.122)	(0.119)	(0.115)	(0.113)
Attendance Rate (%)	0.624**	0.702*	0.622**	0.624	0.907	2.957*	0.858	2.748#
	(0.193)	(0.337)	(0.203)	(0.515)	(1.122)	(1.305)	(1.144)	(1.416)
Mathematics NPR	3.452**	2.569#	4.046**	4.293**	-0.349	2.773	-1.536	2.385
	(0.625)	(1.497)	(0.667)	(0.698)	(1.587)	(2.355)	(1.550)	(2.594)
Reading NPR	0.948*	-0.109	2.734**	2.896**	-0.369	2.449	-0.788	2.557
	(0.390)	(1.343)	(0.410)	(0.472)	(1.239)	(2.095)	(1.344)	(2.414)
Language NPR	1.354**	0.448	0.210	0.442	0.242	3.322#	-0.434	3.071
	(0.448)	(0.897)	(0.687)	(0.704)	(1.031)	(1.920)	(1.013)	(2.097)

(1) Drop year prior to charter entry.

(2) Drop year prior to charter entry and make value-added measure of outcome $(y_{it} - y_{i,t-2})/2$.

(3) Drop two years prior to charter entry.

(4) Drop two years prior to charter entry and make value-added measure of outcome (yit - yi,t-3)/3.

Robust standard errors clustered by school in parentheses. Behavior and attendence 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 nonstructual 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 - Probit Estimates of Demographics and Outcomes on Attrition Propensity

Demographics				Outcomes	
Female	-0.031**	Other Economic Disadvantage	2.433*	# Disciplinary Infractions	0.003
	(0.005)		(0.016)		(0.006)
Native American	-0.013	Limited English Proficient	-0.077**	Attendence Rate (%)	-0.030**
	(0.060)		(0.018)		(0.003)
Asian	-2.070*	At Risk	0.020	Math NPR†	-0.0009**
	(0.030)		(0.017)		(0.0001)
Black, Non - Hispanic	-0.133**	Special Education	-0.120**	Reading NPR†	-0.0006**
	(0.025)		(0.029)		(0.0002)
Hispanic	-0.226**	Gifted and Talented	-0.350**	Language NPR†	-0.0014**
	(0.024)		(0.026)		(0.0002)
Eligible for Free Lunch	-0.079**	Recent Immigrant	0.225**		
	(0.017)		(0.013)		
Eligible for Reduced-Price Lunch	-0.061**	Parent is Migrant Worker	0.070**		
	(0.019)		(0.022)		

† Correlations with test scores are estimated in separate regression which includes all other variables used in first regression but is only conducted on test sample.

Dependent variable is whether student is in the base sample at time t +1 given student is in sample at time t. Coefficient estimates are shown. Robust standard errors clustered by school in parentheses. Regression on base sample contains over 1.2 million observations. Regression on test sample contains over 800,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also contain grade-by-year effects. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Kyriazidou (1997) Selection Corrected Estimates - Value Added Models

	Unweighted (First-Differences)		1/2 * MSE Minimizing Bandwidth	
	(1)		(2)	
	Conversion	**Startup**	Conversion	Startup
# of Infractions	**-0.146***	**-0.971****	-0.116#	-0.862**
	(0.061)	**(0.282)**	(0.064)	(0.333)
Attendance Rate (%)	**0.009**	**3.468***	-0.052	3.804*
	(0.181)	**(1.735)**	(0.196)	(1.820)
Mathematics NPR	**2.203****	**-0.004**	2.070**	0.997
	(0.618)	**(1.294)**	(0.634)	(1.289)
Reading NPR	**-0.497**	**-1.355**	-0.813	-0.742
	(0.788)	**(1.319)**	(0.857)	(1.199)
Language NPR	**0.164**	**1.424#**	0.245	1.940*
	(0.699)	**(0.828)**	(0.614)	(0.782)

	MSE Minimizing Bandwidth		2 * MSE Minimizing Bandwidth	
	(3)		(4)	
	Conversion	**Startup**	Conversion	Startup
# of Infractions	**-0.139***	**-0.935****	-0.144*	-0.961**
	(0.061)	**(0.300)**	(0.061)	(0.287)
Attendance Rate (%)	**-0.008**	**3.615***	0.004	3.510*
	(0.185)	**(1.770)**	(0.182)	(1.746)
Mathematics NPR	**2.170****	**0.377**	2.192**	0.106
	(0.621)	**(1.294)**	(0.619)	(1.294)
Reading NPR	**-0.577**	**-1.195**	-0.519	-1.321
	(0.815)	**(1.281)**	(0.796)	(1.309)
Language NPR	**0.185**	**1.554#**	0.169	1.449#
	(0.679)	**(0.794)**	(0.694)	(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 attendence 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 nonstructual 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 - Additional Outcomes

	(1)	(2)	
	Any Charter	Conversion	Startup
Any Infractions	-0.108**	-0.051*	-0.277**
	(0.027)	(0.023)	(0.037)
Expelled	-0.003**	-0.002**	-0.006**
	(0.001)	(0.001)	(0.002)
Limited English Proficient	0.013	0.034**	-0.053**
	(0.013)	(0.011)	(0.020)
Limited English Proficient	-0.005	0.011	-0.037
(Hispanic Only)	(0.011)	(0.011)	(0.027)
At Risk	-0.015	-0.004	-0.048*
	(0.014)	(0.017)	(0.021)

Robust standard errors clustered by school in parentheses. Regressions contain over 1.2 million obsrvations except the LEP-Hispanic regressions which contain over 800,000. 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 nonstructual switch, whether student undergoes a structural switch, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 10: Fixed Effects Regressions with Controls for School Characteristics

A. Conversions

i. Fixed-Effects in Levels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# of Infractions	-0.263**	-0.272**	-0.268**	-0.271*	-0.271*	-0.266*	-0.259*	-0.261*
	(0.098)	(0.102)	(0.101)	(0.119)	(0.119)	(0.106)	(0.127)	(0.128)
Attendance Rate (%)	0.477**	0.377*	0.400*	0.452*	0.392*	0.303	0.274	0.239
	(0.098)	(0.177)	(0.171)	(0.181)	(0.173)	(0.189)	(0.186)	(0.190)
Mathematics NPR	2.642**	2.562**	2.642**	2.617**	2.628**	2.764**	2.778**	2.719**
	(0.434)	(0.447)	(0.435)	(0.449)	(0.454)	(0.438)	(0.446)	(0.458)
Reading NPR	1.045**	0.990*	1.029*	1.038*	1.035*	1.188**	1.216**	1.167**
	(0.407)	(0.412)	(0.412)	(0.408)	(0.402)	(0.412)	(0.413)	(0.414)
Language NPR	1.356*	1.311*	1.334*	1.351*	1.338*	1.378*	1.401*	1.376*
	(0.568)	(0.580)	(0.577)	(0.595)	(0.582)	(0.587)	(0.619)	(0.621)

ii. Value-Added with Fixed-Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# of Infractions	-0.111*	-0.139**	-0.143**	-0.115	-0.147*	-0.173**	-0.158*	-0.166*
	(0.051)	(0.050)	(0.044)	(0.081)	(0.064)	(0.048)	(0.068)	(0.068)
Attendance Rate (%)	0.242*	0.213*	0.209*	0.238*	0.215#	0.195#	0.172	0.165
	(0.105)	(0.099)	(0.095)	(0.111)	(0.110)	(0.107)	(0.130)	(0.133)
Mathematics NPR	2.376**	2.350**	2.430**	2.379**	2.427**	2.412**	2.439**	2.415**
	(0.767)	(0.782)	(0.767)	(0.891)	(0.884)	(0.799)	(0.898)	(0.902)
Reading NPR	-0.519	-0.493	-0.489	-0.522	-0.490	-0.513	-0.527	-0.500
	(0.378)	(0.392)	(0.380)	(0.376)	(0.379)	(0.377)	(0.378)	(0.388)
Language NPR	0.169	0.174	0.172	0.180	0.180	0.052	0.030	0.054
	(0.369)	(0.383)	(0.368)	(0.369)	(0.365)	(0.346)	(0.345)	(0.357)

B. Startups

i. Fixed-Effects in Levels

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# of Infractions	-0.824**	-0.848**	-0.654**	-0.541**	-0.377**	-0.657**	-0.338**	-0.350**
	(0.125)	(0.131)	(0.101)	(0.143)	(0.103)	(0.092)	(0.105)	(0.107)
Attendance Rate (%)	1.573	1.331	1.768**	1.954	1.859**	1.334**	1.614**	1.378**
	(1.359)	(1.323)	(0.406)	(1.437)	(0.590)	(0.406)	(0.545)	(0.494)
Mathematics NPR	0.261	0.114	1.802**	0.255	1.578*	1.229	2.013**	1.900*
	(1.614)	(1.616)	(0.639)	(1.592)	(0.698)	(1.074)	(0.727)	(0.739)
Reading NPR	-0.431	-0.481	1.031	-0.356	0.730	0.345	1.050	0.966
	(1.071)	(1.095)	(0.966)	(1.088)	(0.985)	(1.366)	(1.084)	(1.098)
Language NPR	0.943	-0.199	0.911	0.152	0.885	1.019	1.350	1.241
	(0.971)	(0.927)	(0.769)	(0.945)	(0.792)	(0.820)	(0.833)	(0.844)

ii. Value-Added with Fixed-Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# of Infractions	-0.625**	-0.661**	-0.422**	-0.124	-0.111	-0.454**	-0.115	-0.159
	(0.197)	(0.174)	(0.116)	(0.280)	(0.130)	(0.106)	(0.132)	(0.136)
Attendance Rate (%)	2.572#	2.446#	1.633**	1.973	1.131**	1.575**	0.980*	0.874*
	(1.370)	(1.368)	(0.328)	(1.450)	(0.415)	(0.330)	(0.418)	(0.393)
Mathematics NPR	-0.707	-0.820	-0.140	-1.426	-0.788	-1.179	-0.638	-0.800
	(1.054)	(1.049)	(0.610)	(1.052)	(0.767)	(1.141)	(0.787)	(0.793)
Reading NPR	-1.189	-1.268	-0.426	-1.324	-0.685	-1.470	-0.639	-0.785
	(1.066)	(1.095)	(0.546)	(1.076)	(0.632)	(1.159)	(0.647)	(0.660)
Language NPR	0.366	0.297	0.469	0.206	0.256	0.427	0.415	0.339
	(0.696)	(0.713)	(0.616)	(0.741)	(0.674)	(0.638)	(0.667)	(0.680)

Quadratic In Per-Student Expenditure	N	Y	N	N	N	N	N	Y
Quadratic in Student-Teacher Ratio	N	N	Y	N	Y	N	Y	Y
Quadratic in Enrollment	N	N	N	Y	Y	N	Y	Y
Teacher Expericence†	N	N	N	N	N	Y	Y	Y

† Fraction of teachers with 0, 1 to 5, 6 to 10, and 11 - 20 years of experience. Over 20 years omitted.
Robust standard errors clustered by school in parentheses. Behavior and attendence 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: post-conversion and post-startup indicators, free or reduced price lunch status, other economic disadvantages, whether student undergoes a nonstructual switch, whether student undergoes a structural switch, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table A1 - Description of Data Elements Used in Analysis

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

Table A2 - Estimates of Effect of Charter Status on "Severe" Disciplinary Infractions

	Full Base Sample		Drop Period Prior to Charter Entry	
	Conversion	Startup	Conversion	Startup
A. Level Fixed-Effects				
Substance Abuse Infractions	-0.004**	-0.013**	-0.007**	-0.012**
	(0.001)	(0.004)	(0.002)	(0.004)
Criminal Infractions	-0.003*	-0.012**	-0.005**	-0.012**
	(0.001)	(0.002)	(0.002)	(0.002)
B. Value Added Fixed-Effect†				
Substance Abuse Infractions	-0.004**	-0.012**	-0.006**	-0.012**
	(0.001)	(0.004)	(0.001)	(0.004)
Criminal Infractions	-0.004**	-0.013**	-0.005**	-0.014**
	(0.001)	(0.003)	(0.001)	(0.003)

† Value added regressions dropping the period prior to charter attendence replace the dependend variable in the first charter period with the average gain over the previous Robust standard errors clustered by school in parentheses. Regressions contain over 1,200,000 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 nonstructual switch, whether student undergoes a structural switch, post-conversion and post-startup indicators, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.