ABSTRACT

Title: ESSAYS ON THE ECONOMICS OF EDUCATION

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Part I: Charter schools are publicly funded schools that, in exchange for expanded accountability, receive more autonomy and experience fewer regulations than traditional public schools. Previous work has found mixed evidence on the impacts of charter schools on both charter and non-charter students. However, these studies focus almost exclusively on test scores and may not fully account for endogenous movements of students and location of schools. Using data from an anonymous large urban school district, I investigate how charter schools affect both charter and non-charter students. In the first chapter I look at the effects of charter schools on charter students. I find that charter schools generate improvements in student behavior and attendance but the effects on test scores differ by subject. These results change little after correcting for selection based on changes in outcomes, endogenous attrition, or persistence. In the second chapter I investigate whether charters affect students who remain in non-charter schools. I find little evidence of charter school impacts on non-charter students. However I also find evidence that regressions using school fixed-effects may be biased. Changes in peer characteristics do not appear to play a large role in the charter impacts.

        Part II: Strains on the Federal budget have created worries that Federal funding of aid for higher education will fall in the future. If this happens, state governments will need to try to re-allocate their higher education spending more efficiently. One possible way to do this would be to shift funding away from public provision towards demand-side subsidies so that more students could attend private colleges.

However, this will only work if private colleges provide benefits to students over public. In order to answer this question, I use highly detailed and rich data sets to assess whether there are benefits to attending private colleges over public ones. For males the wage return is small and statistically insignificant during their twenties but statistically significant at around 11 percentage points by their mid-thirties. For females the wage returns are negative and statistically insignificant. Both males and females exhibit increases in the likelihood of finishing a bachelor's degree.

ESSAYS ON THE ECONOMICS OF EDUCATION

By

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INTRODUCTION TO THE DISSERTATION

In recent years the field of economics has expanded to study many topics not traditionally considered be "economics." Nowhere is that more apparent than in the study of education. Historically, the interests of economists in education is how it affects wages and the macroeconomy. Today, however, economic research has gone beyond looking at what the effects of education are, to studying how to improve education itself. Economists have been able to shed new light on our understanding of the education process by highlighting the substantial role of incentives and the importance of school governance.

This dissertation falls squarely into that line of research by studying two aspects of school governance. The first is a relatively new type of school in the United States called charter schools, one of the fastest growing reforms in education today. Charter schools are publicly funded schools that operate under a contract, called a charter, with a government agency, rather than being directly run by a local school district. They 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 1.1. Today, 1.15 million students nationwide attend charter schools.

Because of their incredible growth over the last ten years, charter schools have gained a lot of attention from researchers and policy analysts alike. Most of the research has focused on two major questions. The first is how charter schools affect students who attend them. Theoretically it is unclear whether charters would be 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,

Figure 1.1 - Charter Growth In the US
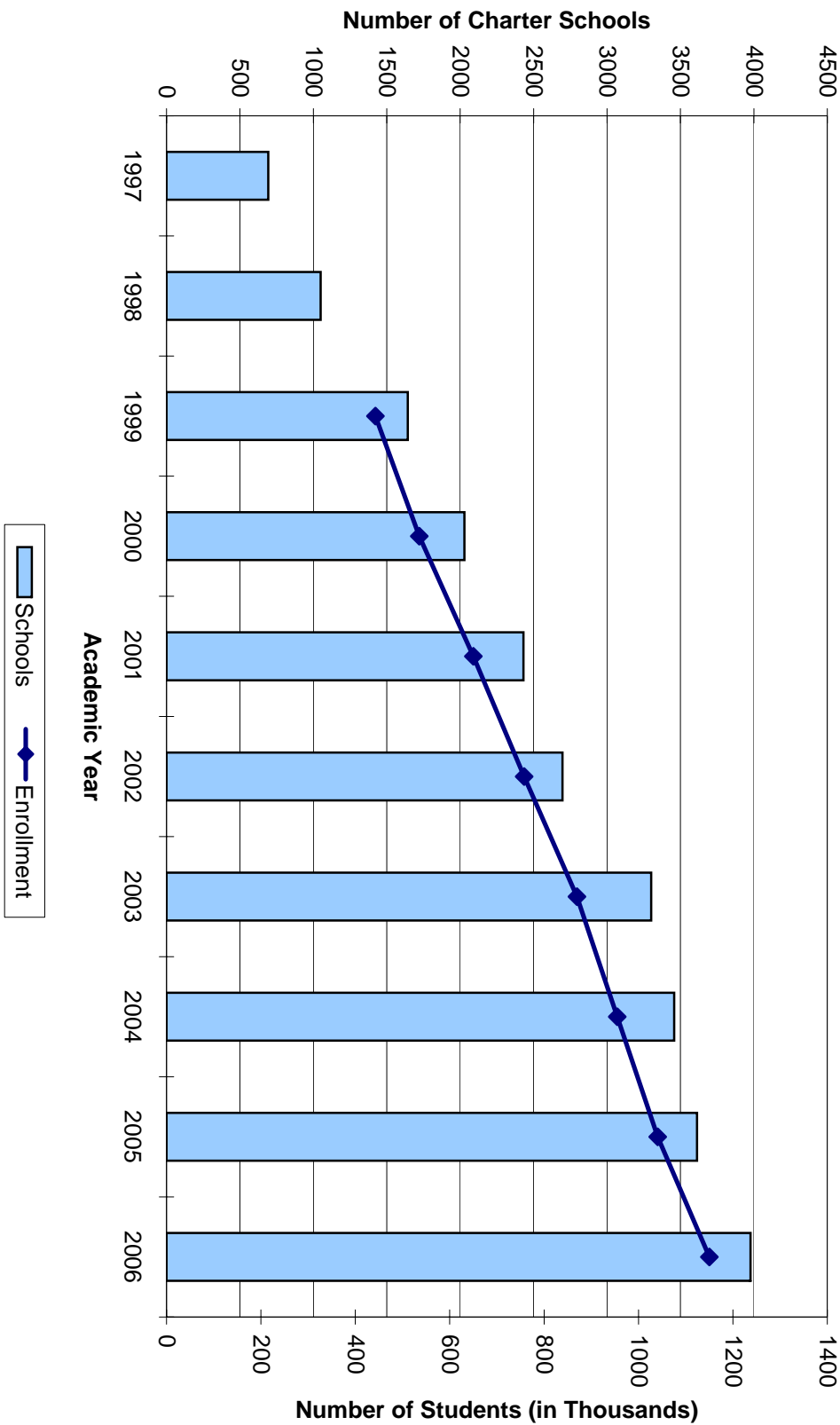
thus providing incentives to increase effort. On the other hand, charters have high levels of student turnover and eliminating some regulations may be detrimental to students. I study this question in the first chapter using data from an anonymous large urban school district (ALUSD). My findings on test scores are consistent with the previous literature and suggest that charter schools have little impact. However, unlike previous work, I am able to study the effects of charters on student behavior and attendance. I find evidence that schools that begin as charters (startup charters) improve discipline and attendance. In addition, I find these results to be robust to corrections for selection based on changes in outcomes, endogenous attrition, and persistence. In fact, it also appears that these impacts do not persist after students return to non-charter schools and can be explained by differences in school size, student-teacher ratios, and teacher experience. Thus, it seems that the benefits of charter schools are unlikely to be a characteristic of their governance structure and are more a characteristic of their small size and younger or less experienced teaching staff.

The second major question regarding charter schools that researchers have considered is how charters affect students who remain in regular public schools. Often charter proponents will argue that charter schools generate incentives for non-charter public schools to improve, since the non-charters lose money if one of their assigned students choose to attend a charter. However charter schools may also affect the characteristics of a non-charter student's peers in a detrimental way. The loss of funding from students attending charter schools could also make it difficult for non-charter schools to improve. Once again using the data from ALUSD, I study this question in chapter two. In addition to analyses with student and school-fixed effects, which is the standard identification strategy in this literature, I also conduct analyses that add school specific time-trends and use instrumental variable techniques. I find little evidence that charter schools affect non-charter students. However, my results

suggest that previous work using only school fixed-effects may have biased estimates. I also find that controlling for peer characteristics only slightly changes estimates suggesting that changes in peer composition plays only a small role in charter impact estimates on non-charter students.

In the third chapter, I move to governance of schools in higher education. Just as in K-12 education, the governance of colleges and universities could have important implications for their effectiveness in increasing human capital. In particular, I look at the differences between colleges that are subsidized by state and local governments (public colleges) and those that are privately owned and managed, usually by not-for-profit organizations (private colleges). Thus, this is essentially an extension of the literature on the effectiveness of public K-12 schools relative to private schools (Altonji, Elder and Taber 2005a, Rouse 1998, Neal 1997, Evans and Schwab 1995). I look at how wages, labor force participation, and educational attainment differ for students from these two types of schools. I find statistically significant increases in male wages and educational attainment. However for women, I find no impact on wages, although there are improvements in educational attainment. Unfortunately, because of selection, at best my estimates are upper bounds. Nonetheless, this means that, while the effects on male wages may be inconclusive, women do no better by attending a private college than a public. I do find lower divorce rates amongst private women, suggesting there may be marriage market benefits.

1. ACHIEVEMENT AND BEHAVIOR IN CHARTER SCHOOLS: DRAWING A MORE COMPLETE PICTURE

1.1 Introduction

In this chapter I investigate how charter schools affect students who attend them. In addition to this theoretical ambiguity discussed in the introduction to this dissertation, the empirical evidence has been mixed. We might conclude from these studies that the effect of charter schools on academic performance is, at best, unclear. Why then does the number of charter students and schools continue to rise while survey and anecdotal evidence suggest that parents are generally satisfied with charters?[1] One potential explanation for this puzzle is that charter schools affect student outcomes in ways that researchers have not investigated. These alternative outcomes may be particularly important in light of recent evidence of how non-cognitive skills improve education and labor market outcomes (Heckman, Stixrud and Urzua 2006, Jacob 2002, Heckman and Rubinstein 2001). In addition, work by Weiher and Tedin (2002) and Jacob and Lefgren (2005) suggest that parents are more concerned with discipline, safety, and student satisfaction than academic performance.

To my knowledge, no studies using individual panel data have looked at the effects of charters on discipline and attendance. In order to study these outcomes, along with retention rates, I use new data from an anonymous large urban school district (ALUSD). This district has one of the largest and oldest district-level charter

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

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.[2] I find that charter schools are effective at improving student discipline and attendance but effects on test scores vary by subject matter. Impacts on retention rates and attendance rates are not statistically significant. Thus, the missing information on these alternative outcomes could help explain the mixed results found in the literature.

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

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

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

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, Ashenfelter 1978) in order to mitigate the extent of this bias. When I use this strategy, discipline and attendance estimates are not substantially affected while the impacts on test scores remain mixed.

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

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

A third complication arises if charter schools affect students after they return to non-charter schools. In this case, fixed effects estimates may be biased since these "persistent" outcomes will be applied to periods when the charter indicator equals zero. In addition, whether or not charter school impacts are long-term is relevant to policy. For the foreseeable future, the stock of charter schools in the US will be small relative to non-charters. Thus most students who enter charters in elementary and middle school will return to non-charter schools before leaving the public school system. If charters provide short-term benefits but no long-term benefits, the usefulness of these schools for generating human capital improvements will be limited. The long time coverage of my data allows me to measure the extent of this problem by conducting regressions with lagged measures of charter status. I find little evidence of persistence in charter impacts after students return to non-charter schools. Nonetheless, even if charter schools generate only temporary performance improvements, they also tend to spend less money than non-charter schools. In 2002, median per-student expenditures for charter districts were 13% lower than in non-charter districts.[3] Thus, if charters provide the same level of long-term performance and cost less money, they still enhance the efficiency of the education system.

Overall, these results imply that charter schools in ALUSD provide improvements in student discipline and attendance with mixed effects on test scores. However, these impacts are only temporary. While these results are not necessarily representative of charter schools in other states and districts, they generate two important

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

implications for the charter literature. First, they provide evidence that individual fixed effects strategies are robust to multiple bias reducing procedures, suggesting that this econometric strategy is appropriate in the charter context. Second, they highlight that the singular focus of the charter literature, and many other branches of the economics of education, on test scores misses key pieces of information that could lead to erroneous policy recommendations.

1.2   Background on Charter Schools

1.2.1   Previous Literature

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

With the exception of Solmon and Goldschmidt (2004) who look at retention, all of these papers only investigate the impacts on test scores. However, student "performance" could encompass an array of outcome measures in addition to academic achievement such as behavior, attendance, and social skills. These non-cognitive outcomes have been shown to play important roles in educational attainment and job market success (Heckman et al. 2006, Jacob 2002, Heckman and Rubinstein 2001). Other research suggests that parents care about non-academic outcome measures when they make decisions regarding their children's schooling. Weiher and Tedin (2002) survey charter parents in Texas and find that only 22% cite test scores as the most important reason for sending their children to charter schools while 38% specify

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discipline or safety and 26% cite moral values. Jacob and Lefgren (2005) study parents' preferences when choosing teachers and find that for most parents their children's satisfaction is more important than academic performance. If charter schools seek to improve these alternative outcomes then they may shift resources away from improving test scores. Such a phenomenon could partially explain the range of estimates of charter effectiveness that researchers have found.

All of the previously cited papers on charter schools use individual fixed effects are 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 that generate violations of the assumptions that underlie fixed effects analyses, and hence could lead to bias.

1.2.2   Charter Schools in the United States

Charter schools have become relatively commonplace across the US since the first states enacted charter laws in the early 1990's. Today approximately 2.2% of public school students attend charter schools. Charters are more common in urban areas than suburban or rural. In 2003, the most recent year detailed national charter data is available, charter students were more than twice as likely to reside in urban areas than non-charter students.[5]

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 substantial difference between charters is to whom they are accountable. Every charter school has a relationship with some

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[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.

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

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.[6]

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 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 2005b, 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

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[6] Ibid.

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%.[7]

Although charter schools have a number of advantages that may generate improvements in student performance, there are some disadvantages as well. Thus, net impacts are theoretically ambiguous. While there are many ways that charters may affect students, there are a few mechanisms that are particularly important. The first is freedom from regulations. Charter proponents argue that reducing regulations makes it easier for schools to innovate and experiment. However, this does not necessarily improve student performance since the experiments could turn out poorly. Charters also may be reluctant to abandon an ineffective experimental strategy if there are high fixed costs to changing, such as for retraining teachers. In addition, some regulations, such as teacher certification, may be helpful.[8]

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

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[7] Center for Education Reform.

[8] 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 2002b, Hanushek, Kain and Rivkin 1999, Berger and Toma 1994)

to cut spending, potentially reducing performance. Unfortunately, it is difficult to determine how common involuntary losses of charters are since national data on charter schools is very limited. Nonetheless we can identify an upper bound by looking at overall closure rates for charters, which between 2000 and 2004 averaged 5.0% per year compared to a closure rate in non-charter public schools of 1.8% during this period.[9]

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

1.2.3 Charter Schools in ALUSD

ALUSD was one of the first school districts in the US to institute a charter program. Although the program has been in existence since 1996, it did not start in earnest until 1997. Half of the charter schools created to date by ALUSD were started in 1997 or 1998. Today there are more than twenty charter schools in ALUSD along with over 200 non-charter schools.[10] There is also a large number of non-district charter schools in the ALUSD area. Figure 1.2 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

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[9] 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.

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

district charter school while 8.5% attended a non-district charter.[11] Charter students in ALUSD are also more likely to be in grades below high school. All of the charter schools I study are chartered by the ALUSD district. Nonetheless, Table 1.1 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.

1.3 Data

In this chapter I utilize a new set of administrative records from an anonymous large urban school district (ALUSD). This dataset includes information on disciplinary infractions warranting an in-school suspension or harsher punishment, attendance, scores from a nationally norm-referenced examination and a criterion-referenced state examination, grades, course work, and a number of student characteristics. A full accounting of the variables used in this chapter 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

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[11] 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.

Figure 1.2 - Fraction of Enrollment in ALUSD Area by Type of School and Year
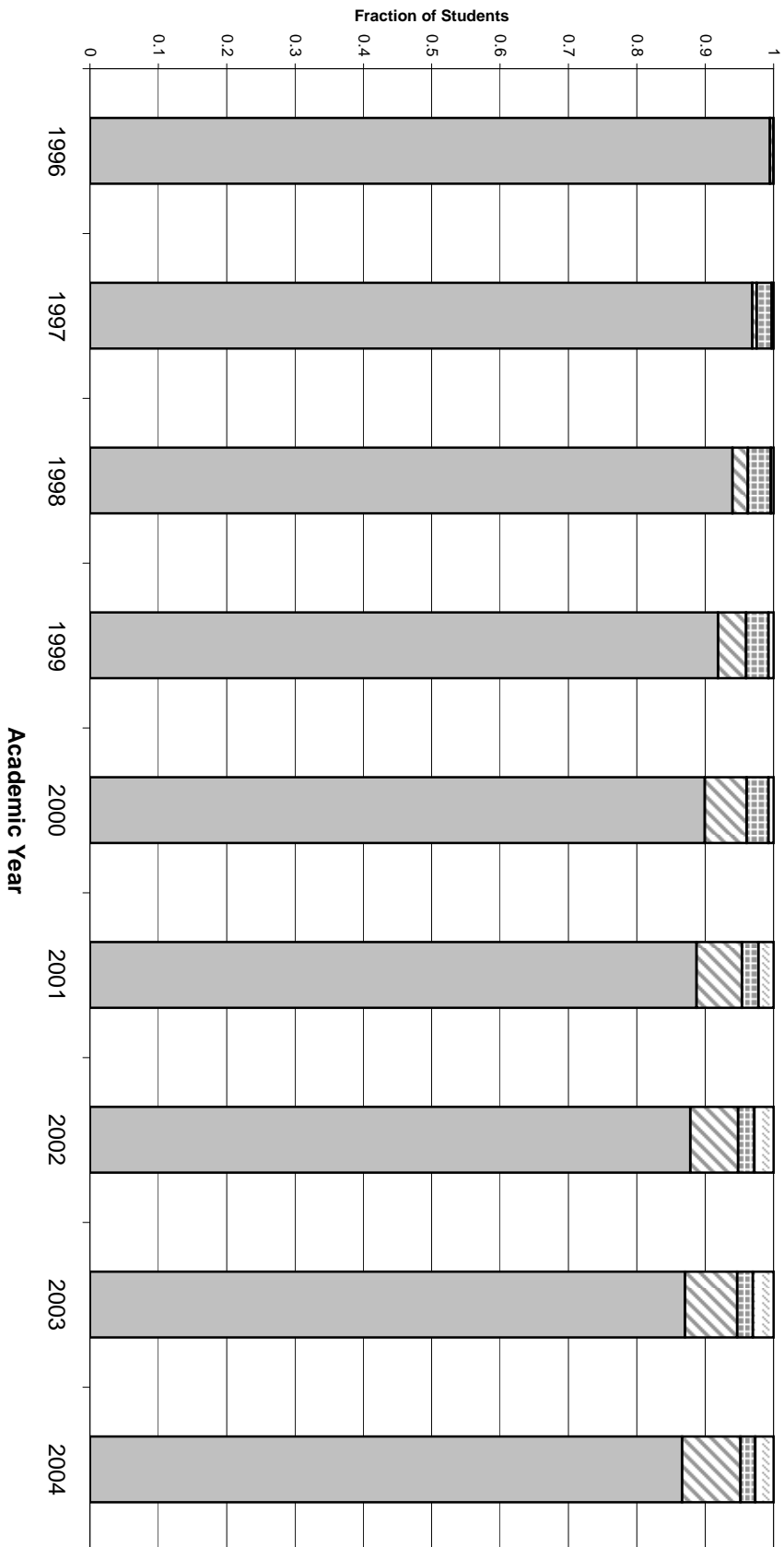
Table shows the fraction of students in each type of school in ALUSD along with non-district charters in the region around ALUSD as defined by the state Department of Education.

■ ALUSD Non-Charter   ▨ Non-District Charter   ▦ District Conversion   ▨ District Startup

Table 1.1 - 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.

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

Since not all students take the norm-referenced examination and test data are only available starting in 1998, I generate two samples.[12] I call the first sample the "base sample." This sample is used when analyzing any outcome other than test scores. It includes students in grades 1-12 who were enrolled as of the end of October of each year, since this is when demographic information is collected by the district. The demographic files identify the school a student attends and thus I use this as the student's school for the year. Some observations are excluded due to missing attendance data ($< 0.1\%$), leaving more than 1.2 million observations of which more than 50,000 are students in charter schools.[13]

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

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

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

English in grades 1-8 took a separate Spanish language exam. While I have data on these exam results, the scores are not directly comparable to those of students taking the English exam so I do not include them in the analysis.[14] The final test sample includes over 900,000 student-year observations, approximately 40,000 of which are students in charter schools.

Table 1.2 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.[15]

1.4   Baseline Empirical Strategy

Since most charter schools are schools of choice, it is likely that parents send their children to charters for reasons that are unobservable to the econometrician. We may be particularly concerned that students who enter charters differ from non-charter students in terms of unobserved ability, parental motivation, or tendency to misbehave. The summary statistics in Table 1.2 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

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

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

Table 1.2 - Summary Statistics of ALUSD Base Sample By Charter Status

Variable	Non-Charter vs. Charter		Conversion vs. Startup	
	Non-Charter	Charter	Conversion	Startup
% Female	49.2	48.5	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.

biased.

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

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

$$ATT = E(y_{it}^1|c_{it} = 1) - E(y_{it}^0|c_{it} = 1) \tag{1.1}$$

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 (1.1) since an individual cannot be enrolled in a charter and enrolled in a non-charter at the same time. Thus, we need to find a counterfactual group that will provide us with an accurate approximation of $E(y_{it}^0|c_{it} = 1)$. The simplest solution would be to use the outcomes for students who do not attend charters as the counterfactual, $E(y_{it}^0|c_{it} = 0)$. This is sufficient if students are assigned to charter schools randomly. However, parents and students choose whether to enroll in charters. If this choice is correlated with y_{it}^0 then $E(y_{it}^0|c_{it} = 0)$

$\neq E(y_{it}^0 | c_{it} = 1)$ and any attempt to estimate ATT using this counterfactual will be biased.[16]

In order to address the bias in the comparison group one could condition on a set of observables $\mathbf{X}_{it}$ to control for observable differences between treatment and comparison groups, but this still leaves the possibility that the choice of c_{it} will be caused by y_{it}^0 through some omitted factor. This is the general strategy used in much of the early research on charter schools with some using school level data (Bettinger 2005, Hoxby 2004, Hoxby 2003, Hoxby 2002a) and others using student level data (Buddin and Zimmer 2005b, Nelson, Rosenberg and Van Meter 2004, Goldstein 2004, Eberts and Hollenbeck 2002).

The availability of panel data provides me with a strategy that may correct this problem. If the decision to attend a charter is not correlated with unobserved characteristics of students that vary over time then the ATT can be identified by

$$\theta = E(y_{it}^1 | c_{it} = 1, \mathbf{X}_{it}, \phi_i) - E(y_{it}^0 | c_{it} = 0, \mathbf{X}_{it}, \phi_i). \tag{1.2}$$

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

$$E(y_{it} | c_{i1}, ..., c_{iT}, \mathbf{X}_{i1}...\mathbf{X}_{iT}, \phi_i) = E(y_{it} | c_{it}, \mathbf{X}_{it}, \phi_i) \tag{1.3}$$

we can estimate θ consistently using individual fixed effects. In addition, the estimate

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

of θ, $\widehat{\theta}$, has a causal interpretation. Thus, initially, I estimate θ using the following regression equation:

$$y_{it} = \alpha + \theta C_{it} + Demog_{it}\Gamma + Switch_{it}\Phi + Gradeyear_{it}\Psi + \phi_i + \epsilon_{it} \qquad (1.4)$$

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

Two recent papers (Ballou, Teasley and Zeidner 2006, Hoxby and Murarka 2006) have raised concerns regarding the validity of using the individual fixed effects strategy to identify charter effects. Thus, I would like to briefly outline how I address some of the problems they raise. The largest concern these papers have is that by using fixed-effects, the charter impact is identified by using only those students who switch between charter and non-charter schools and thus may not be representative of all charter students. In the ALUSD data, this concern is mitigated by the fact that 80% of charter students have at least one non-charter period and thus, most of the charter students are identified in the regressions. In addition, the long time-span and the fact that grades one through eleven are tested in ALUSD, ensures that the identified sample is more representative of charter students in the district overall than the samples used in previous research. A second concern they have is that endogenous switching based off of temporary shocks could bias the estimates. The interrupted

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panel strategy I use in the next section addresses this problem. A third concern is that the fixed effects analyses drastically reduces the size of the identified sample, making estimates imprecise. However, the ALUSD data includes a large number of identified charter students - 24,000 in the base sample. Thus, my estimates are reasonably precise.

Hoxby and Murarka also argue that using oversubscription lotteries to identify charter effects is a superior strategy to fixed-effects regressions. While they are correct that a lottery based strategy has substantial advantages over fixed-effects, there are two important aspects of lotteries that may be undesirable. The first is that, since oversubscribed schools are likely to be of higher quality 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.

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

$$y_{it} = \gamma_0 y_{i,t-1} + \gamma_1 x_{it} + \gamma_2 z_i + \nu_{it} \tag{1.5}$$

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

$$y_{it} - y_{i,t-1} = \Delta y_{it} = \gamma_1 x_{it} + \gamma_2 z_i + \nu_{it}. \tag{1.6}$$

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

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

Unless specified otherwise, all regressions in this chapter include the grade-by-year indicators along with the time-varying demographic characteristics - whether the student is eligible for free lunch, is eligible for reduced price lunch, has some other economic disadvantage, has immigrated within three years, and whether one of the

student's parents is a migrant worker.

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

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

Table 1.3 provides regression estimates of the model in equation (1.4).[18] The

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

[18] Appendix Table 4 provides estimates without student fixed-effects

Table 1.3 - Fixed Effects Regressions of Charter Impact

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

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

standard errors for each regression are robust to heteroscedasticity and clustered by school.[19] In column one I group all charters together into one indicator variable. There is a statistically significant reduction in both level and value-added measures of disciplinary infractions, a statistically significant improvement in math test score changes, and a statistically significant drop in reading. Impacts on attendance rates, retention rates, and language test impacts are not statistically significant.

These results hide a substantial amount of heterogeneity. Column 2 shows the same regression, but the charters are split into conversions and startups. The two types of charters show similar patterns in the estimates but the magnitudes differ substantially. For example, most of the discipline improvements from column one occur in startup charters. The drop of 0.79 infractions per year when students enter a startup charter is equal to 69% of the mean infraction rate in the year prior to startup entry. For attendance, neither type of charter produces a statistically significant effect on levels but students who attend startup charters show improvements in value-added attendance of 2.5 percentage points relative to a baseline absentee rate of eleven percent in the year prior to startup entry. This impact is statistically significant at the ten percent level. Turning to other results, there is no statistically significant change in retention rates in either type of charter. The only statistically significant effect on test scores is for math scores in conversions.[20] However, we must be cautious about interpreting the results for the conversion charters. Since one of the conversion charters includes a gifted and talented magnet program, and it also happens to be the largest charter school, these results may not be representative of conversion charters more generally. To address this, I reran these regressions while dropping any student who ever attends that particular conversion charter. The results for this analysis

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[19] 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.

[20] I also found the baseline results to be similar for test score levels and to reweighing the sample by number of days enrolled. These results are provided in Appendix Table 5.

are provided in Appendix Table 6 and show that while the discipline estimates become statistically insignificant for conversions, the math test score estimates remain statistically significant at the 10% level and language estimates become statistically significantly positive. Retention estimates become statistically significantly negative.

While the discipline results for startups are dramatic, since they are based on a measure that can be manipulated by the charter schools there is a question as to whether these are real behavioral changes or the result of charter schools being more lenient with students. Nonetheless, there are a few reasons to believe that these reflect real behavioral changes in the students. First, when I run regressions that focus on severe infractions - substance abuse and criminal activity (shown in Appendix Table 7)-, I find similar results. Since the margin I am considering is the number of in-school suspensions or more severe punishments, then we should only see reductions in these types of infractions if there are real behavioral improvements since principals would be very unlikely to punish students for these infractions with less severe punishments in a systematic manner. Second, I also find statistically significant reductions in expulsions and the likelihood of having any infraction, so the results are consistent across different margins. Third, I will show later that there are statistically significant improvements in attendance when one accounts for persistence. Since attendance is highly correlated with behavior and is much harder to misrepresent we would expect there to be improvements in behavior based on these results alone.[21] Finally, at seven times the standard error, the results are very large and would require a large amount of leniency in order to make the estimates statistically insignificant.

Why are the results different for conversion charters and startup charters? One potential explanation could be that there is little benefit to freeing schools from

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[21] 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.

regulations without providing new staff and facilities. However, this does not explain why math test scores improve in conversions but not startups while discipline and attendance improve more in startups. Another potential explanation is that charters tend to focus on particular aspects of student performance. That is, perhaps startups try to specialize in helping children with behavioral problems while conversions focus more on academic performance. Even if this is the case in ALUSD, it is not clear if this is due to a random assignment of each focus across the two types of schools or if there is some systematic reason that startups focus on behavior (i.e. perhaps parents are more willing to change their children's schools if they are misbehaving or are in an unsafe environment then if they are simply not performing well academically but are well-behaved). A third potential explanation is that there may be aspects of the parents' decision making processes when choosing to send their children to a charter, or, for those whose children already attend charters, when choosing whether to exit the charter, that could bias the estimates due to failures of strict exogeneity. The next section addresses this issue in detail.

1.5   Correcting for Three Potential Sources of Bias

1.5.1   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 et al. 2007, Hanushek et al. 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 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 1.3A and 1.3B investigate whether this phenomenon occurs in ALUSD with respect to discipline and attendance. Figure 1.3A 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 1.3B 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 1.3A, 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 1.3B 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 discipline and attendance 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

Figure 1.3A - Disciplinary Infactions and Attendance
Before and After Entering Charters
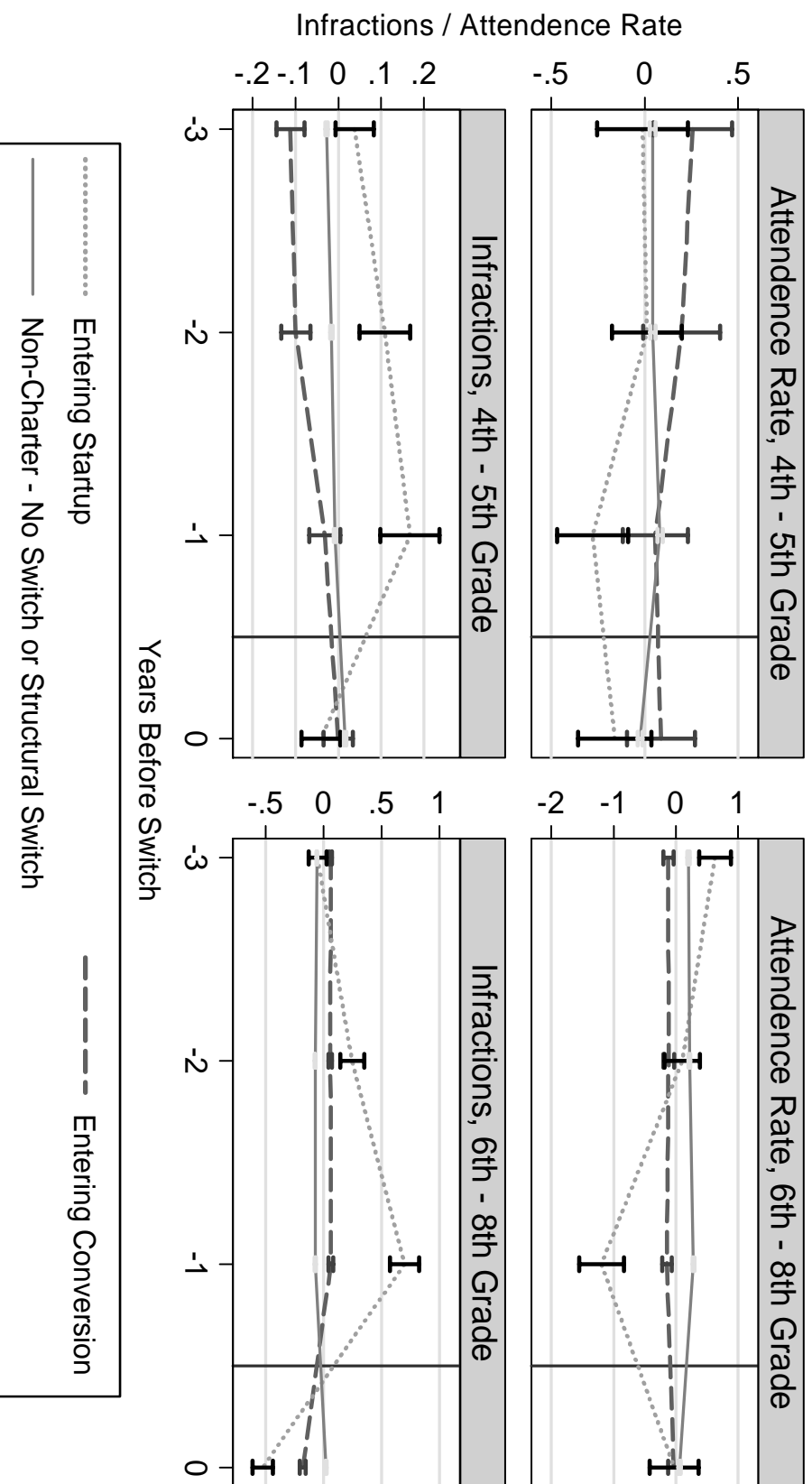
Infractions / Attendence Rate

Years Before Switch

Attendence Rate, 4th - 5th Grade

Infractions, 4th - 5th Grade

Attendence Rate, 6th - 8th Grade

Infractions, 6th - 8th Grade

Entering Startup

Non-Charter - No Switch or Structural Switch

Entering Conversion

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.

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Figure 1.3B - Disciplinary Infractions and Attendance
Before and After Non-Charter School Switch
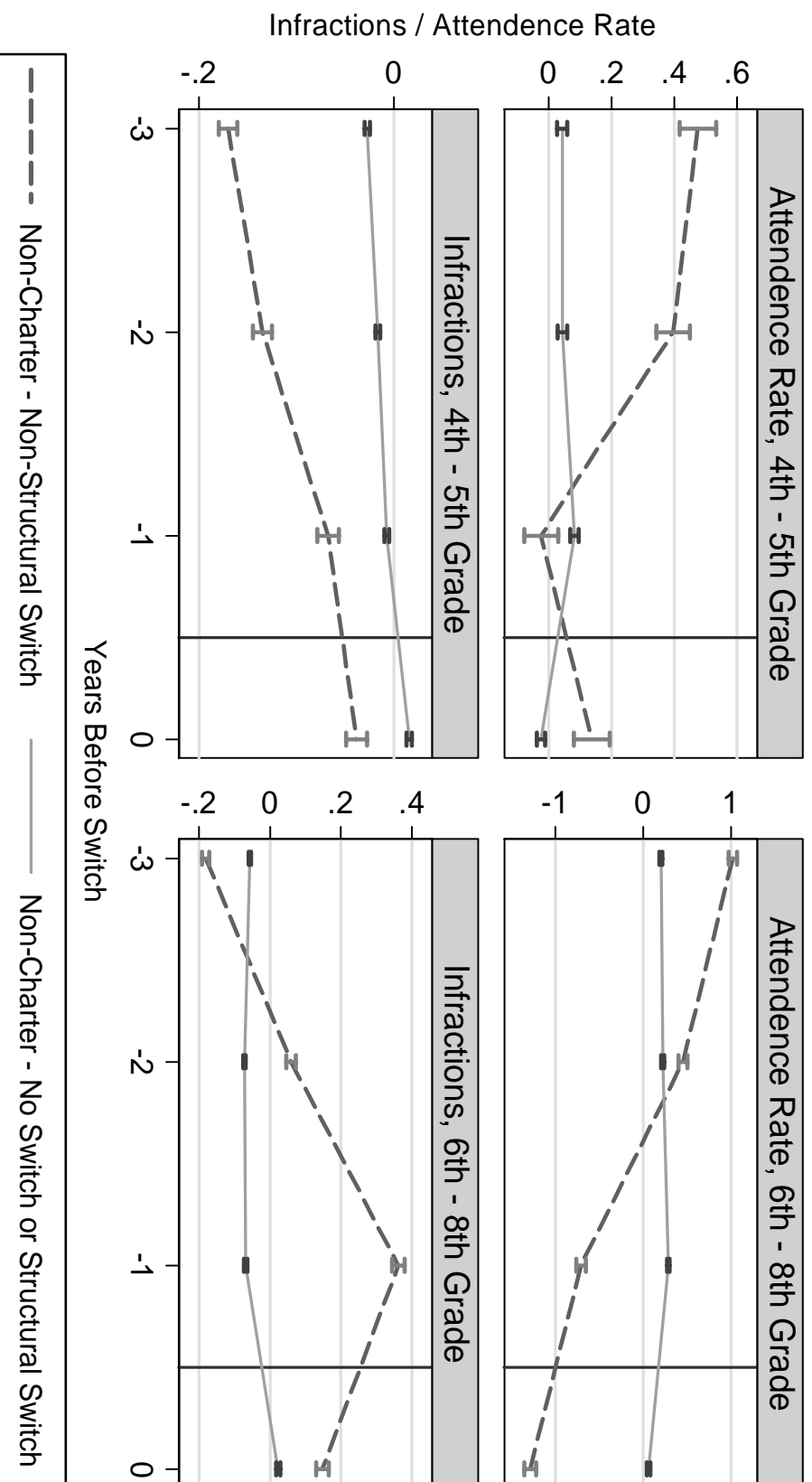
Non-Charter - Non-Structural Switch

Non-Charter - No Switch or Structural 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.

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are also non-structural switchers.

When we look for dips in annual test score changes, in Figures 1.4A and 1.4B it is difficult to discern any pattern for charter schools or non-structural switches. However, Figure 1.5A shows a noticeable dip in test score levels immediately prior to startup charter entry, but as Figure 1.5B shows, there is no test score dip for non-structural switchers. Thus, while there appears to be some selection into charters based on test scores, non-structural switches, in general, appear from these graphs to be associated more with worsening behavior.

Table 1.4 provides some regression estimates that identify the Ashenfelter dips in the outcome variables shown in Figures 1.3A and 1.3B along with retention and test scores. Each regression is run on the entire base sample for outcomes other than test scores and the entire test sample for test score outcomes. They contain indicators for being in a period that is three, two, and one year prior to entry into a conversion or startup charter or prior to switching non-structurally between traditional schools. They also include indicators for being in the year of the switch, denoted by year g in the table, given the student is observed in the sample in the year prior to the switch. The regressions confirm the graphical observations in Figures 1.3A and 1.3B. Students in conversions show no substantial drops in discipline and attendance prior to entry while there is clear evidence of dips for students in startups for the two years immediately prior to entry. All of the estimates for annual changes in test scores except one drop in the year prior to entry for both types of charters along with retention rates. In addition, the patterns for students who undergo non-structural switches between non-charter schools are similar to students who enter startup charters.[22]

In order to address the potential endogeneity generated by selection based on

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[22] The fact that non-structural switchers have similar pre-switch patterns to startup charter students suggest that they could provide a good comparison group in a difference-in-differences analysis focusing on these two groups. In Appendix Table 8 I provide the results of these regressions that are remarkably similar to the estimates provided in Table 1.3.

Figure 1.4A - Standardized Examination Annual Score
Changes
4th - 8th Grades
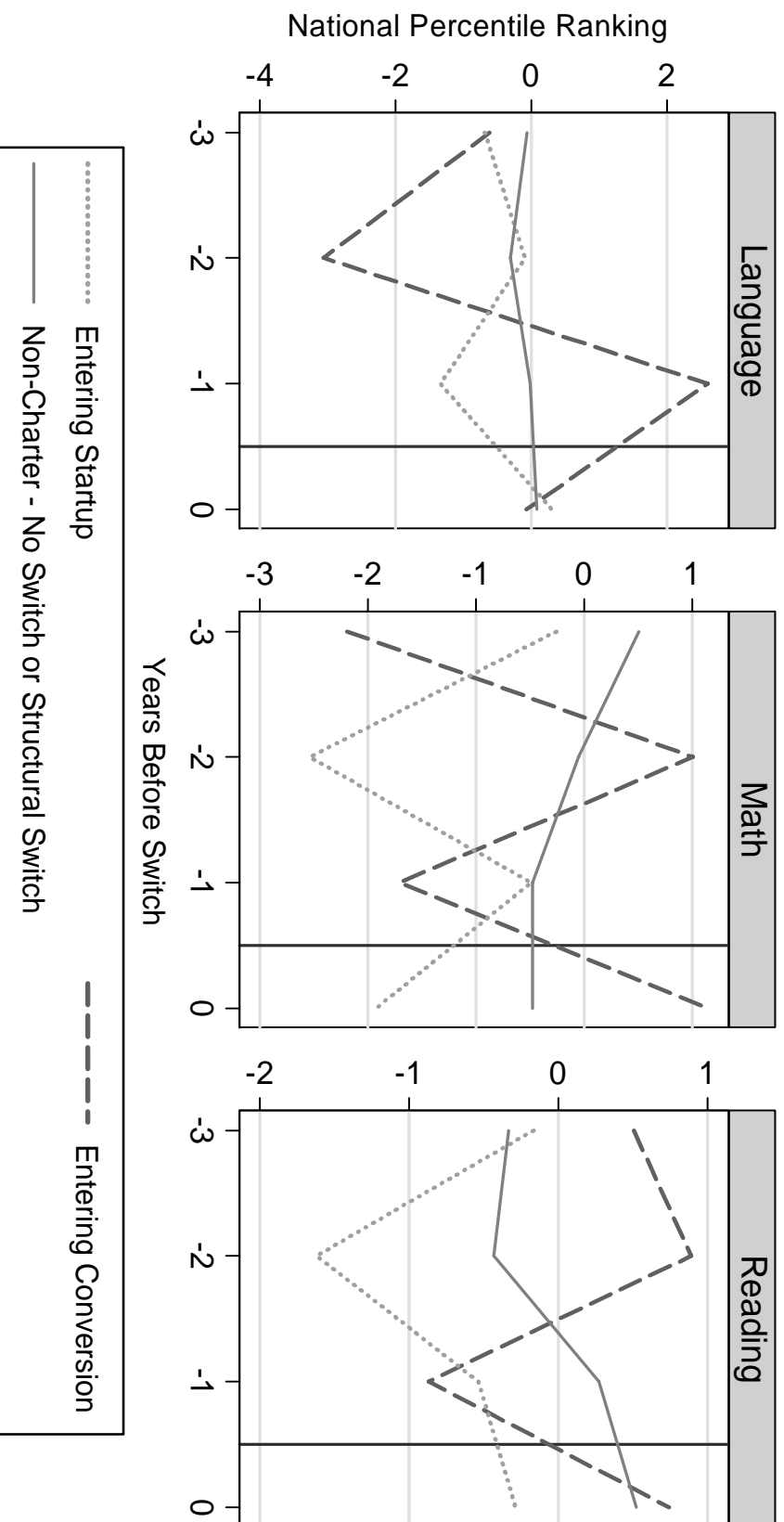
National Percentile Ranking

Language

Math

Reading

Years Before Switch

Entering Startup

Non-Charter - No Switch or Structural Switch

Entering Conversion

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 1.4B - Standardized Examination Annual Score
Changes
4th - 8th Grades

National Percentile Ranking

Language

-.6   -.4   -.2   0

-3   -2   -1   0

Math

-2   -1   0   1

-3   -2   -1   0

Reading

-.5   0   .5

-3   -2   -1   0

Years Before Switch

- - - Non-Charter - Non-Structural Switch
——— Non-Charter - No Switch or Structural 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.

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Figure 1.5A - Standardized Examination Annual Score Levels
Before and After Entering Charters

4th - 8th Grades

National Percentile Ranking

Language

Math

Reading

Years Before Switch

Entering Startup

Non-Charter - No Switch or Structural Switch

Entering Conversion

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.

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Figure 1.5B - Standardized Examination Annual Score Levels
Before and After Non-Charter School Switch

4th - 8th Grades
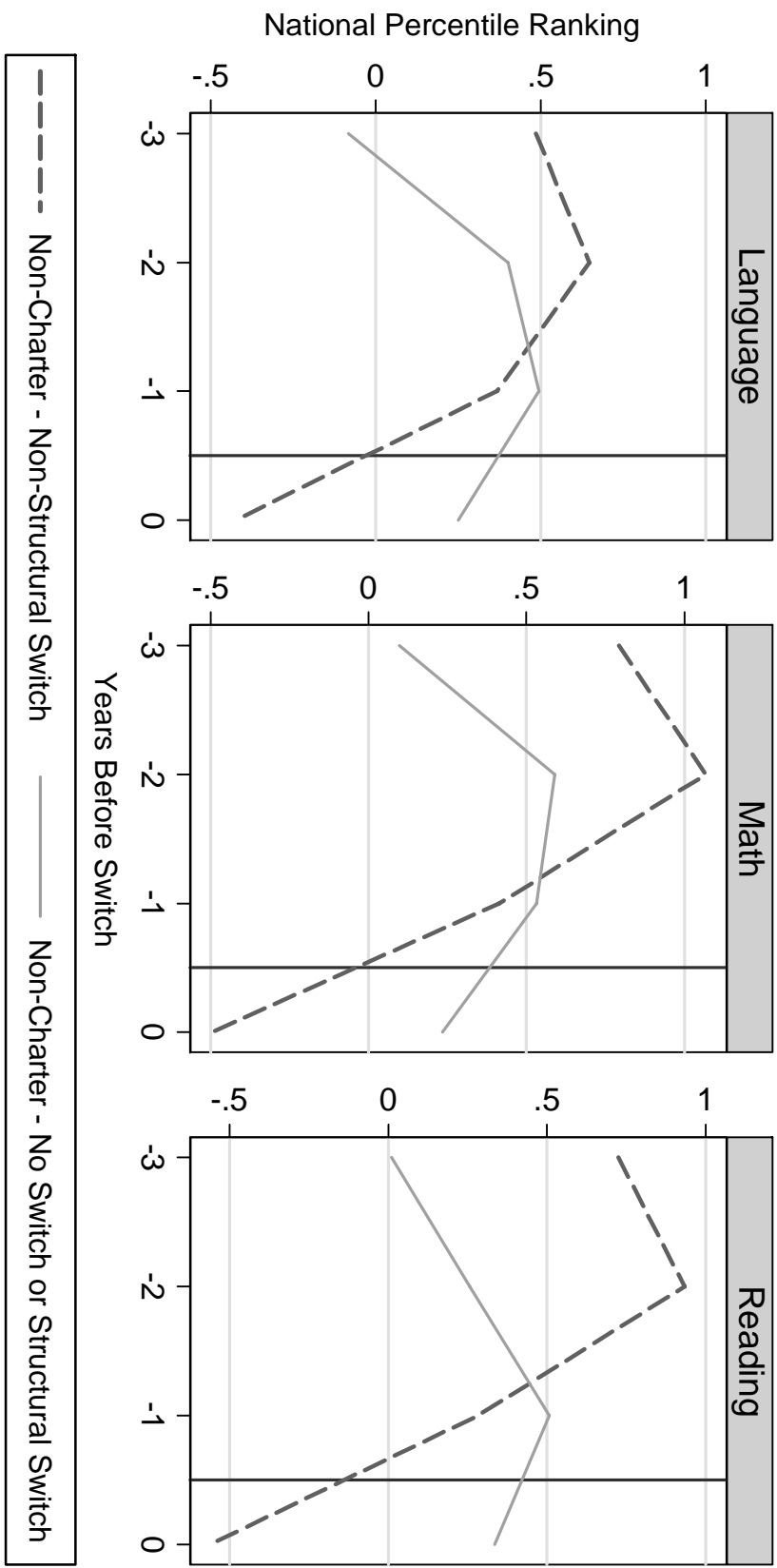
National Percentile Ranking

Language

Math

Reading

Years Before Switch

- - - - Non-Charter - Non-Structural Switch

——— Non-Charter - No Switch or Structural 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.

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Table 1.4 - Fixed Effects Regressions of Pre and Post School Entry Effects

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

Robust standard errors clustered by school in parentheses. Only students with a g-1 observation are classified as switchers in g. Each row is a separate regression. Behavior and 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: free or reduced price lunch status, other economic disadvantages, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

changes in outcomes I use a procedure called interrupted panel estimates (Hanushek et al. 2007, Hanushek et al. 2002, Ashenfelter 1978). The idea is that by dropping the periods prior to entry into a charter school, I can mitigate the effect of the selection by comparing periods students are enrolled in charters to periods well before charter entry. However, the results in Table 1.4 show that this selection also occurs in students who undergo non-structural switches between traditional schools. Thus, I also drop observations in the periods prior to non-structural switches between any two schools.

Table 1.5 provides the results of these interrupted panel estimations. In the first column, I show the results from Table 1.3 for comparison. In the second column, I drop all observations in the year prior to when a student enters a charter school from a non-charter school or when a student makes a non-structural switch between non-charter schools. In the third column I drop the two years prior and in the fourth I drop the year prior to a charter entry or non-structural switch and the year of the entry or switch. Panel A conducts the analysis using a single charter indicator. In the second column the estimates change little, except that reading score impacts become more negative and retention rate impacts are statistically significantly positive now. The results in the third and fourth columns are similar to the second. When I split the charter impacts by conversion or startup charters in panel B the results are similar to those in panel A. For conversions, the impact on discipline falls but not enough to change the statistical significance of the level estimate, while reading impacts become negative and statistically significant. For startups, there is little change in levels of discipline and attendance impacts while retention impacts become statistically significant and reading impacts become negative and statistically significant. One particularly interesting result is that when both the year before and year of the switch are dropped the added value measures of discipline and attendance improvements for startups fall. This is in part due to increased precision, but it also suggests

Table 1.5 - Interrupted Panel Fixed Effects Regressions of Charter Impact

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

B. Charters Split by Conversion and Startup

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

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

(1) No Dropped Years (from Table 3)

(2) Drop Year Prior to Charter Entry or Non-structural Switch Between Non-Charters

(3) Drop Two Years Prior to Charter Entry or Non-Structural Switch Between Non-Charters

(4) Drop Year Prior to and Year of Charter Entry or Non-structural Switch Between Non-Charters

(5) Drop Year Prior to Charter Exit or Non-Structural Switch Between Non-Charters

(6) Drop Year Prior to Charter Exit and Entry or Non-Structural Switch Between Non-Charters

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

that these behavioral improvements occur once a student enters a charter with little improvement afterwards. Results in section 5.3 will later confirm this. Nonetheless, the discipline measure is still statistically significant. Thus, while the coefficients on some outcomes change, the interrupted panel estimates are not substantially different from the baseline estimates.

In addition to changes in outcomes affecting entry into charter schools, they may also affect exit from charter schools. If a parent takes outcome measures as indicators of match quality with the charter school then he may repeat the selection process for charter entry and once again seek other educational options. A potential consequence of this endogenous exit is that when the students return to ALUSD non-charter schools after performing poorly in a charter, they may experience mean-reversion back to higher performance levels. Since, in fixed effects analyses, students who are in charters are essentially compared to periods when they are not in charters, endogenous exit of this type could impose a downward bias the charter impacts.

To address this issue, in column four, I provide interrupted panel estimates where the year prior to when a student exits a charter school and enters a non-charter school is dropped. I also drop the year prior to non-structural switches between non-charter schools. I caution, however, that using interrupted panel estimates for endogenous exit is a more problematic strategy for removing bias than for endogenous entry since these estimates identify the charter effects off of those who spend at least two years in a charter. Nonetheless, this would tend to increase the change in the estimates from the baseline result since students who benefit more from charters are more likely to remain in them. Thus, as long as the changes in the estimates are small, then there is little need for concern. This appears to be the case in ALUSD. When we compare the results in column four to column one the impact estimates change slightly, usually in the direction suggesting a better impact, but not enough to have any bearing on the statistical significance of the outcomes except for

reading impacts in conversion charters. Finally, in column five I drop both the year prior to charter entry and before charter exit to see what effect adjusting for both types of endogeneity has. This strategy seems to magnify the charter effects but does not change the pattern of the estimates. Thus, overall, the results from the interrupted panel analyses suggest that charters provide improvements in discipline and attendance, but have mixed results for test scores, which is the conclusion drawn from the baseline estimates. The only difference is that startup charters display an increase in retention rates.

1.5.2   Attrition

While neither the endogenous entrance of students into charter schools nor the endogenous exit of students out of charter schools into non-charter schools affect the estimates considerably, some parents may choose to leave ALUSD altogether if students perform poorly in charter schools. Although we may believe that parents of students who perform poorly in non-charters would be as likely to leave the district as charter students, the fact that they choose to send their children to charters suggests they have preferences for alternative educational environments. In addition, charter parents are more likely to be dissatisfied with the non-charter schools their children previously attended or with the district in general. Thus, charter parents may be more likely than non-charter parents to send their children to a private school or a non-district charter school if their ALUSD schools are bad matches.

The evidence from the ALUSD data suggests that there is substantially more attrition in charters than non-charters, particularly in startup charters. Figure 1.6 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%

Figure 1.6 - Transitions Between School Types

% of Students Making Transfer

Non-Charter: 82.85, 0.58, 0.37, 16.19

Conversion: 24.83, 60.76, 0.42, 13.99

Startup: 14.65, 0.29, 53.26, 31.8

Type of School Student Attends in Year t - 1

School Student Attends in Year t:

■ Non-Charter □ Conversion ⊞ Startup ▦ Out-of-District
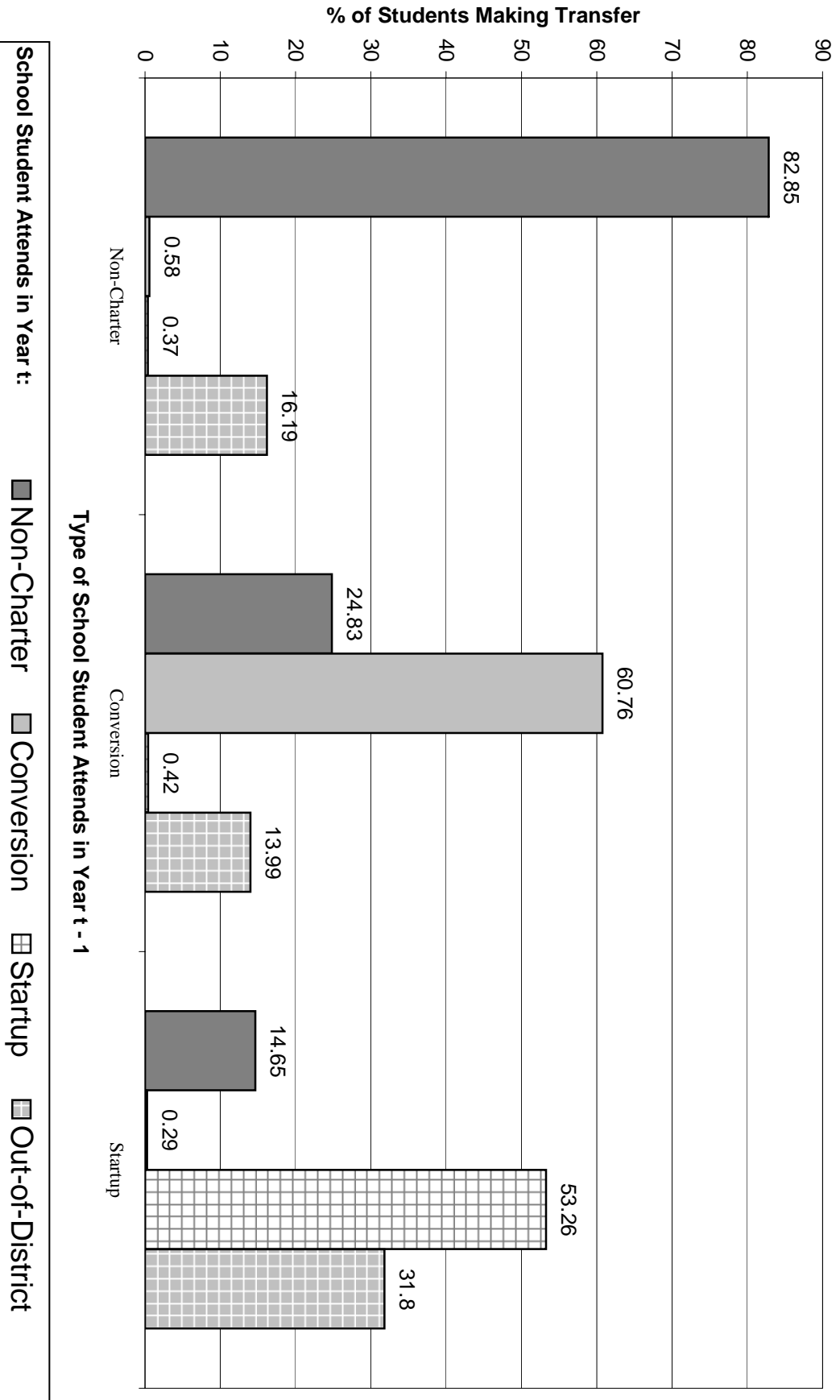
for startup charter students.[23] The differences are more dramatic over longer time periods. For example, 38% of non-charter third graders are no longer in ALUSD five years later while that number is 43% for conversion students and 58% for startup students. Other research has shown differential attrition rates for charters as well, even in statewide data. Hanushek, Kain, Rivkin, and Branch (2005) show that while 7% of non-charter students leave their population of 4th through 7th grade students in Texas public schools each year, 18% of charter students leave.

The potential econometric problem when there is a substantial amount of attrition is that if students select out of the sample in a non-random manner then the results may be inaccurate representations of the effect of treatment on the treated. While a fixed effects regression would ideally provide a consistent estimate of the parameter θ in equation (1.2), if there is attrition from the population - defined here as any student who attends ALUSD between 1994 and 2004 - then fixed effects will estimate

$$\theta' = E(y_{it}|c_{it} = 1, \mathbf{X}_{it}, \phi_i, s_{it} = 1) - E(y_{it}|c_{it} = 0, \mathbf{X}_{it}, \phi_i, s_{it} = 1) \qquad (1.7)$$

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 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 1.6 provides a probit regression of whether a student attrits in the following year on a range of observable characteristics. If attrition is random then

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

Table 1.6 - Probit Estimates of Demographics and Outcomes on Attrition Propensity

Demographics		Outcomes	
Female	-0.031**	Other Economic Disadvantage	2.433*
	(0.005)		(0.016)
Native American	-0.013	Limited English Proficient	-0.077**
	(0.060)		(0.018)
Asian	-2.070*	At Risk	0.020
	(0.030)		(0.017)
Black, Non - Hispanic	-0.133**	Special Education	-0.120**
	(0.025)		(0.029)
Hispanic	-0.226**	Gifted and Talented	-0.350**
	(0.024)		(0.026)
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)
		# Disciplinary Infractions	0.003
			(0.006)
		Attendance Rate (%)	-0.030**
			(0.003)
		Math NPR†	-0.0009**
			(0.0001)
		Reading NPR†	-0.0006**
			(0.0002)
		Language NPR†	-0.0014**
			(0.0002)

$\dagger$ Test score effects 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.

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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. In addition, Table 1.7 shows that attriters from charter schools differ along multiple dimensions from non-charter attriters. These differences become even more apparent when charter students are separated by whether they attend a conversion or startup charter. Thus the evidence in Figure 1.4 and Tables 6 and 7 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 where 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. This is essential to the identification of the model used in this chapter.

To produce Kyriazidou's (1997) estimator, one must first define the selection equation,

$$s_{it} = W_{it}\Omega + \zeta_i + \mu_{it} \tag{A1}$$

where W_{it} is a set of variables that are observed whether or not the individual has attrited, ζ_i is an individual specific fixed-effect, and μ_{it} is random i.i.d. error. W_{it} need not contain all (or any) of the variables in the outcome equation, but it does need to contain at least one variable that is not included in the outcome equation; an exclusion restriction. The outcome equation in my model is equation (1.4). After removing the fixed-effect in the outcome equation through first-differencing, Kyriazidou argues that in observations where $(W_{it} - W_{is})\Omega = 0$ for $s < t$, the individual has not had a change in circumstances that affects attrition. Since a student's innate

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Table 1.7 - Comparison of Charter and Non-Charter Attriters (1997 - 2003)

Variable	Non-Charter vs. Charter		Conversion vs. Startup	
	Non-Charter	Charter	Conversion	Startup
% Female	47.5	46.7	47.9	44.7
		(1.6)		(2.8)
% White, Non-Hispanic	11.2	8.7	11.8	3.6
		(7.2)		(13.0)
Grade level	6.0	567.1	4.5	7.5
		(10.3)		(50.9)
Year	2000.0	2000.5	2000.1	2001.2
		(23.5)		(26.9)
% Eligible for Free Lunch	58.2	55.1	64.7	39.4
		(5.6)		(23.3)
% Eligible for Reduced Price Lunch	6.6	7.5	7.9	6.9
		(3.2)		(1.6)
% Other Economic Disadvantage	9.0	11.2	5.7	20.2
		(6.8)		(21.0)
% Limited English Proficient	22.0	16.7	16.3	17.4
		(11.3)		(1.4)
% At Risk	60.9	54.2	42.7	73.1
		(12.2)		(28.3)
% Special Education	12.2	8.0	9.6	5.5
		(11.5)		(6.6)
% Gifted and Talented	5.9	7.8	12.5	0.0
		(6.8)		(21.3)
% Recent Immigrant (within 3 years)	7.7	5.7	4.8	7.1
		(6.7)		(4.5)
% Parent is Migrant Worker	0.7	0.6	0.4	0.9
		(1.0)		(2.6)
# of Disciplinary Infractions	0.61	0.29	0.39	14.6
(Suspension or More Severe)		(18.2)		(10.1)
Attendance Rate (%)	89.5	91.6	94.0	87.6
		(13.3)		(25.8)
Reading & English Grades	76.9	80.8	81.8	77.2
		(24.9)		(14.0)
Math Grade	76.6	80.2	81.7	74.6
		(21.4)		(19.9)
Average Grade	76.9	80.7	82.2	75.5
		(25.8)		(22.5)
Math Exam National Percentile Ranking	44.8	48.6	53.1	38.4
(1998 and Later)		(8.2)		(15.2)
Reading Exam National Percentile Ranking	40.2	45.3	50.1	34.8
(1998 and Later)		(11.4)		(15.6)
Language Exam National Percentile Ranking	44.8	49.4	54.3	38.6
(1998 and Later)		(9.9)		(16.3)

Absolute t-statistics in parentheses.

tendency to switch schools is captured by fixed-effects we can generate consistent estimates of θ, the charter effect, by using only those observations where this holds true. The validity of this procedure requires a conditional exchangeability assumption. That is, the error terms in both equations must be identically (though not necessarily independently) distributed across time periods conditional on the fixed effect. That is

$$F(\epsilon_{it}^*, \epsilon_{is}^*, \mu_{it}, \mu_{is}|\zeta_i) = F(\epsilon_{is}^*, \epsilon_{it}^*, \mu_{is}, \mu_{it}|\zeta_i) \text{ for } s \neq t \tag{A2}$$

where ϵ_{it}^* is the error term from outcome equation in the selected sample.

Of course Ω is unknown, but we can estimate it via a conditional "fixed-effects" logit regression or some other maximum likelihood or maximum score method to get $\widehat{\Omega}$. A problem that arises is that there will almost always be very few observations where $(W_{it} - W_{is})\widehat{\Omega} = 0$, and so limiting to only those observations will greatly reduce power. Instead, Kyriazidou's proposal is to use kernel weights to reweigh the regressions towards observations where there is little change in attrition propensity over time.

To apply Kyriazidou's strategy, I run a first-differenced version of (1.4) weighted by kernel weights of the form

$$\widehat{\psi}_{it,n} = \frac{1}{h_n} K\left(\frac{(W_{it} - W_{is})\widehat{\Omega}}{h_n}\right) \tag{1.8}$$

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 t.[24] 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

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[24] 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 $t-1$.

$h_n = h * n^{-1/(2(r+1)+1)}$ 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.

A difficulty with kernel weighted estimation methods is that the choices of the kernel and bandwidth are often subjective. I use the normal density as the kernel in this chapter which is the density used by Kyriazidou in her Monte-Carlo analysis.[25] Generally, researchers have found that the choice of bandwidth is more important than the choice of kernel, and thus the estimates may be very sensitive to the choice of bandwidth. As the bandwidth increases, the variance of the weights falls and the model converges to the unweighted model, increasing the bias. On the other hand, as the bandwidth decreases more observations are given trivial weight in the regression, which increases the variance of the estimates. Thus there is a trade-off between bias and variance.

Researchers have proposed a number of strategies for choosing the bandwidth, each with benefits and drawbacks.[26] Kyriazidou provides a modification of Horowitz's (1992) "plug-in" strategy for bandwidth selection. This strategy identifies a bandwidth that minimizes the mean-squared error (MSE) based on the asymptotic properties of the estimator. A drawback of this method is that the MSE minimizing bandwidth is sensitive to the choice of an initial bandwidth. One strategy that has been used previously is to choose an initial bandwidth such that it equals the MSE minimizing bandwidth (Dustman and Rochina-Barrachina 2000). The intuition behind this strategy is that, since both the initial and MSE minimizing bandwidths converge at the same rate, asymptotically they are equivalent.[27] I use this method to choose my initial bandwidth and I also test the sensitivity of the estimates to the choice of bandwidth.

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[25] $r = 1$ in the case of the normal density.

[26] See Pagan and Ullah (1999) and Blundell and Duncan (1998) for excellent discussions of bandwidth selection in the context of non-parametric regression.

[27] I am greatly appreciative to Jose Galdo for pointing this out to me.

Kyriazidou's estimator also involves a correction for asymptotic inconsistency. To make the correction, one needs to generate estimates with a "slow" convergence bandwidth of $h_n = h * n^{-\varphi/(2(r+1)+1)}$ where $0 < \varphi < 1$. Following the Monte-Carlo simulation in Kyriazidou (1997) I choose $\varphi = 0.1$. Denoting the estimate using this bandwidth as $\widehat{\theta}_\varphi$ and the estimate using the "fast" bandwidth $\widehat{\theta}$, the correction formula is

$$\widehat{\widehat{\theta}} = \frac{\widehat{\theta} - n^{-(1-\varphi)(r+1)/(2(r+1)+1)}\widehat{\theta}_\varphi}{1 - n^{-(1-\varphi)(r+1)/(2(r+1)+1)}}.$$

The standard errors remain the same as in the "fast" bandwidth regression.

In order to estimate the selection equation, I expand the data so that any student observed in ALUSD has observations until she is expected to graduate assuming normal grade progression or until the year 2004, whichever comes first. For my exclusion restriction, I use whether the student is not eligible to attend her previous school due to exceeding the maximum grade of that school. The idea behind this exclusion restriction is that a student would be more likely to leave the district if she has to switch schools anyway; that is the relative costs of leaving the district falls if students are forced to switch schools. Since the student will always be grade-eligible for her last school if she is retained, I use the predicted grade based on the student's grade in $t-1$ rather than the actual grade when determining eligibility. Thus if a student is in grade six in a school that goes up to that grade, but is held back, he will still be considered ineligible for that school since his predicted grade is seven. The model includes as covariates indicators for whether the last school the student is observed attending prior to year t is a conversion or a startup, as well as the last observed free lunch, reduced-price lunch, other economic disadvantage, recent immigration status, and parents' migrant status. In addition the regression includes grade-by-year effects. If $s = 0$, the grade is predicted based on normal grade progression from the student's most recent observation.

Table 1.8 provides the results of the selection corrected estimates along with

unweighted first-differences regressions for comparison.[28]   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 bandwidths to the unweighted estimates we see that the charter effects are very similar regardless of whether they are split by type of charter.   The results also appear to be robust to the size of the bandwidth.   Thus, there is little evidence to suggest that endogenous attrition has a substantial effect on the fixed effects estimates.

Another strategy one can use to test the sensitivity of results to endogenous attrition is to impute the missing data under different assumptions about the outcomes students would have achieved had they remained in ALUSD.   In Table 1.9, I show the results of these analyses.   A detailed account of how data was imputed can be found in the appendix.   Under each scenario a group of attrited students are assumed to have not attrited and have had disciplinary infractions equal to the maximum or a certain percentile of the distribution of disciplinary infractions for their predicted grade-year or infractions are set to zero.   In the first scenario, in panel B, students who ever are observed in a charter attend non-charter schools instead of attriting.   In panel C, all students who attrit attend non-charter schools.   In both scenarios, even in the most extreme situations the discipline results for startups are statistically significant, at least at the 10% level.   For panels D and E, rather than assign all attriters to non-charter schools they are assigned to schools in a probabilistic fashion based on the transition probabilities imputed from a multinomial logit regression on students who remain in the data.   In these cases, all students who are imputed to attend non charter schools have infractions set to 0 while those who are imputed to attend charter schools have their infractions set to different levels relative

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[28] Since I cannot cluster standard errors for the fixed-effects logit model, I check the first stage of the exclusion restriction using a linear probability model. This is statistically significant at the 1% level. In order to avoid multicollinearity issues in the first stage due to the nature of the exclusion restriction, I drop first graders from the regressions.

Table 1.8 - Kyriazidou (1997) Selection Corrected Estimates

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

	MSE Minimizing Bandwidth			2 * MSE Minimizing Bandwidth		
	(5)	(6)		(7)	(8)	
	Charter	Conversion	Startup	Charter	Conversion	Startup
# of Infractions	-0.347**	-0.154*	-0.931**	-0.355**	-0.159*	-0.931**
	(0.100)	(0.077)	(0.194)	(0.100)	(0.078)	(0.189)
Attendance Rate (%)	0.765	-0.039	3.204#	0.773	-0.028	3.121#
	(0.662)	(0.241)	(1.852)	(0.663)	(0.238)	(1.830)
Δ # of Infractions	-0.312**	-0.139*	-0.935**	-0.327**	-0.144*	-0.961**
	(0.099)	(0.061)	(0.300)	(0.098)	(0.061)	(0.287)
Δ Attendence Rate	0.781	-0.008	3.615*	0.789	0.004	3.510*
	(0.579)	(0.185)	(1.770)	(0.579)	(0.182)	(1.746)
Likelihood of Being Retained	0.016	0.013	0.024	0.014	0.012	0.019
	(0.014)	(0.016)	(0.025)	(0.013)	(0.015)	(0.021)
Δ Mathematics NPR	1.810**	2.170**	0.377	1.764**	2.192**	0.106
	(0.605)	(0.621)	(1.294)	(0.615)	(0.619)	(1.294)
Δ Reading NPR	-0.701	-0.577	-1.195	-0.684	-0.519	-1.321
	(0.718)	(0.815)	(1.281)	(0.708)	(0.796)	(1.309)
Δ Language NPR	0.460	0.185	1.554#	0.432	0.169	1.449#
	(0.605)	(0.679)	(0.794)	(0.614)	(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 attendance regressions contain over 800,000 observations. Retention regressions contain over 800,000 observations. Test score regressions contain over 300,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also include the following covariates: free or reduced price lunch status, other economic disadvantages, whether student undergoes a 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 1.9 - Sensitivity of Discipline Results to Assumptions About Attrition

A. Infraction Statistics by Grade Level

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

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

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

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

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

D. Imputations for Charter Students Only - Type of School Attended Random Function of Observed Characteristics
When Attrited Student is in a Non-Charter Infractions = 0; When Attrited Student is in Charter Infractions = X

			Percentile Within Grade-Year				
X:	Maximum for Grade-Year	99th	95th	90th	80th	70th	0
Conversion	1.708	0.168	-0.042	-0.130	-0.193*	-0.219*	-0.252**
	(1.811)	(0.353)	(0.184)	(0.123)	(0.092)	(0.087)	(0.089)
Startup	2.893	0.228	-0.333	-0.551*	-0.716**	-0.824**	-0.882**
	(3.038)	(0.862)	(0.425)	(0.260)	(0.149)	(0.103)	(0.103)

E. Imputations for All Students - Type of School Attended Random Function of Observed Characteristics
When Attrited Student is in a Non-Charter Infractions = 0; When Attrited Student is in Charter Infractions = X

			Percentile Within Grade-Year				
X:	Maximum for Grade-Year	99th	95th	90th	80th	70th	0
Conversion	5.266	1.391	0.525	0.170	-0.102	-0.211**	-0.334**
	(3.835)	(0.989)	(0.467)	(0.256)	(0.107)	(0.073)	(0.096)
Startup	7.550	1.723	0.407	-0.111	-0.518**	-0.752**	-0.886**
	(4.679)	(1.316)	(0.645)	(0.383)	(0.185)	(0.090)	(0.082)

to their predicted grade-year. As in panels B and C, panel D only imputes data for students who ever attended a charter and E imputes data for all attrited students. While the evidence in these panels are not as strong as B and C, they still suggest that we need to make very extreme assumptions about the attrited students for attrition to make the discipline results statistically insignificant.

1.5.3   Persistence of Charter Effects

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

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

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

analysis. Separate regressions using three lags provide similar results. Thus, I estimate the following model

$$y_{it} = \alpha + \theta_0 c_{it} + \theta_1 c_{it-1} + \theta_2 c_{it-2} + \mathbf{X}_{it}\mathbf{\Gamma} + \mathbf{G}_{it}\Psi + \phi_i + \epsilon_{it} \qquad (1.9)$$

where c_{it-1} and c_{it-2} are first and second lags of c_{it}, which is defined as in equation (1.4). It should be noted, however, that including the lags limits the regressions to include only those observations where the student has been in the sample for at least three consecutive years.

Table 1.10 provides the results from these regressions. These include the same covariates as in the baseline regressions in section four. The first three columns show the effects of charter status in periods t, $t-1$, and $t-2$ on outcomes in period t. The second and third sets of three columns show the same results broken down by conversion and startup charter status. When I add the lagged charter status, only the attendance impacts for startups and the language test scores for conversions change substantially. In addition, both of these impact estimates increase, suggesting that if persistence is generating bias, it is generating an underestimation of the charter effects.

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

Table 1.10 - Fixed Effects Regressions with Lagged Charter Indicators

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

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

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$$y_{it} = \alpha + \theta_0 c_{it}^1 + \theta_1 c_{it}^{2+} + \theta_2 Post_{it} + \mathbf{X}_{it}\mathbf{\Gamma} + \mathbf{G}_{it}\Psi + \phi_i + \epsilon_{it} \qquad (1.10)$$

where $c_{it}^1 = 1$ if the student is in the first year of a charter spell, $c_{it}^{2+} = 1$ if a student is in another year of a charter spell, and $Post_{it} = 1$ if the student was previously in a charter but is not currently enrolled. As in the previous analysis, I also estimate a model that separates each of these indicators by conversion or startup status, so that, for example, $Post_{it}$ splits into two indicators. The first equals one whether the student was previously in a conversion charter and is not currently in a conversion and the second is defined similarly for startups. One potential concern with this model is that endogenous exit could be a more substantial problem here than in other models, since our outcome of interest is the effect of a charter after leaving. To address this, I use whether a student is grade ineligible for the last charter he or she attended as an instrument for being in a post charter period. As in section 5.2, in order to avoid the potential endogeneity of the instrument through retention, I use the student's predicted grade rather than actual grade. Table 1.11 shows that this instrument is a strong predictor of being in a post-charter period. Table 1.12 shows the second-stage results. Note that these are similar to the results from a regular fixed-effects estimation, supporting the results in section 5.1 that suggested endogenous exit is not a major concern. These results are available from the author upon request. The most remarkable result here is the sharp increase in disciplinary actions after a student leaves a charter. While the increase is larger for startups, it is clearly observed for both types of charters. As for other outcomes, in startups all of the point estimates suggest worsening outcomes after students leave the startups and attend other schools, although only retention is statistically significant. For conversions, there are persistent improvements in attendance and retention, but a drop off in test scores after students leave. Thus, there is essentially no persistence for startup charters, and some evidence of persistence for conversions. The results

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Table 1.11 - 2SLS Fixed Effects Persistence Regressions, First Stage

Exogenous Variables ↓	(1) Post Charter	(1) Post Conversion	(2) Post Startup	(3) Post Charter	(3) Post Conversion	(4) Post Startup
Charter - Year 1	-0.131** (0.019)	-	-	-0.263** (0.034)	-	-
Charter - Year 2+	-0.221** (0.044)	-	-	-0.378** (0.057)	-	-
Grade Ineligible for Last Charter	**0.775** (0.032)**	-	-	**0.490** (0.061)**	-	-
Conversion - Year 1	-	-0.142** (0.027)	0.001 (0.001)	-	-0.285** (0.049)	0.001 (0.002)
Conversion - Year 2+	-	-0.238** (0.058)	-0.001 (0.001)	-	-0.401** (0.078)	-0.003# (0.002)
Grade Ineligible for Last Conversion	-	**0.788** (0.034)**	0.001 (0.001)	-	**0.495** (0.075)**	0.003 (0.002)
Startup - Year 1	-	0.000 (0.005)	-0.072** (0.016)	-	0.009 (0.009)	-0.152** (0.035)
Startup - Year 2+	-	0.004 (0.004)	-0.133** (0.041)	-	0.005 (0.006)	-0.221** (0.035)
Grade Ineligible for Last Startup	-	-0.005* (0.002)	**0.738** (0.069)**	-	-0.002 (0.006)	**0.506** (0.086)**
Sample	Base			Test		

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. Base sample 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, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

Table 1.12 - 2SLS Fixed Effects Persistence Regressions

	(1) Any Charter			(2) Conversion			Startup		
	Year 1	Year 2+	Post	Year 1	Year 2+	Post	Year 1	Year 2+	Post
# of Infractions	-0.416**	-0.440**	-0.153#	-0.235*	-0.275#	-0.091	-0.795**	-0.879**	-0.016
	(0.114)	(0.132)	(0.093)	(0.101)	(0.145)	(0.095)	(0.148)	(0.151)	(0.180)
Attendance Rate (%)	0.803	1.093#	1.157**	0.474*	0.734*	1.125**	1.378	1.952	-0.543
	(0.542)	(0.600)	(0.417)	(0.202)	(0.324)	(0.354)	(1.368)	(1.761)	(0.849)
Δ # of Infractions	-0.353*	-0.108	-0.020	-0.120	-0.046	0.033	-0.853**	-0.200	0.158
	(0.154)	(0.077)	(0.073)	(0.090)	(0.074)	(0.073)	(0.293)	(0.143)	(0.265)
Δ Attendance Rate (%)	1.126	0.590#	0.546	0.193	0.292	0.369#	3.087	1.148	-0.819
	(0.862)	(0.347)	(0.333)	(0.138)	(0.186)	(0.220)	(1.990)	(0.815)	(0.895)
Likelihood of Being Retained	-0.009	-0.011	-0.043#	-0.026**	-0.023**	-0.057*	0.055	0.062	0.109**
	(0.015)	(0.010)	(0.023)	(0.007)	(0.006)	(0.023)	(0.056)	(0.042)	(0.036)
Δ Mathematics NPR	1.382	0.806	-0.641	2.732**	1.710	0.301	-1.216	-0.597	-1.162
	(0.992)	(1.959)	(1.860)	(0.885)	(2.435)	(2.186)	(1.842)	(1.625)	(2.488)
Δ Reading NPR	-0.463	-2.426#	-0.775	0.217	-1.932	-1.069	-1.724	-2.242	-2.464
	(0.793)	(1.350)	(1.744)	(0.930)	(1.584)	(1.922)	(1.333)	(1.847)	(2.399)
Δ Language NPR	0.644	-0.715	-1.187	0.846	-0.392	-0.756	0.273	-0.950	-2.133
	(0.854)	(0.890)	(1.071)	(1.156)	(1.043)	(1.188)	(0.925)	(1.238)	(2.531)

Robust standard errors clustered by school in parentheses. Behavior and attendence regressions contain over 1,000,000 observations. Retention regressions contain over 1,000,000 observations. Test score regressions contain over 500,000 observations.

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for the value added measures of discipline and attendance also confirm the suggestion from the interrupted panel estimates in section 5.1 that behavioral improvements occur at the time of entry into the startup charters.

1.6   Additional Outcomes, Heterogenous Impacts, And Controlling for School Characteristics

Table 1.13 provides some results on additional outcome measures of interest and looks at whether charter effects vary by student type and school characteristics. All regressions are linear fixed effects models and include the same covariates as in the baseline regressions in section four. Panel A looks at the additional outcomes. These 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.

Panels B and C look at how the charter impacts vary by type of student. In order to limit the number of estimates displayed, I only show regressions using the general charter indicator. Panel B considers variation by race. Charters provide Hispanics with more discipline and attendance improvements than blacks and other races, while blacks get larger improvements in test score changes. Panel C shows that males have higher test score impacts than females but there is no statistically significant difference for other outcomes.

Table 1.13 - Additional Outcomes and Variation by Race and Gender

A. 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)

Regressions contain over 1.2 million obsrvations except the LEP-Hispanic regressions which contain over 800,000.

B. Charter Impacts by Race

	Charter	Charter*Hispanic	Charter*Non-Hispanic Black
# of Infractions	-0.458**	0.023	0.292*
	(0.073)	(0.103)	(0.135)
Attendance Rate (%)	0.075	1.206*	-0.046
	(0.261)	(0.574)	(0.373)
Likelihood of Being Retained	0.023	-0.013	-0.036*
	(0.017)	(0.021)	(0.017)
Math NPR Gain	2.022**	-0.967	-0.333
	(0.482)	(0.598)	(0.810)
Reading NPR Gain	-2.004**	1.547**	2.239**
	(0.257)	(0.487)	(0.654)
Language NPR Gain	-0.032	0.468	1.073#
	(0.267)	(0.307)	(0.566)

21,672 observations are dropped due to multiple races being listed for an individual.

C. Charter Impacts by Gender

	Charter	Charter*Female
# of Infractions	-0.390**	0.080
	(0.098)	(0.067)
Attendance Rate (%)	0.656	-0.024
	(0.440)	(0.106)
Likelihood of Being Retained	0.002	0.007
	(0.011)	(0.005)
Math NPR Gain	1.788**	-0.532
	(0.545)	(0.354)
Reading NPR Gain	-0.176	-0.940**
	(0.364)	(0.259)
Language NPR Gain	0.966*	-0.968*
	(0.404)	(0.385)

31,566 observations are dropped due to multiple genders being listed for an individual.

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

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

Table 1.14 - Fixed Effects Regressions with Controls for School Characteristics

A: # of Dsciplinary Infractions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Convert	**-0.213***	**-0.226***	**-0.224***	**-0.231***	**-0.235***	**-0.221***	**-0.213#**	**-0.214#**
	(0.090)	**(0.093)**	**(0.092)**	**(0.110)**	**(0.106)**	**(0.098)**	**(0.115)**	**(0.115)**
Startup	**-0.786****	**-0.855****	**-0.446****	**-0.552****	**-0.142**	**-0.595****	**-0.073**	**-0.072**
	(0.107)	**(0.127)**	**(0.086)**	**(0.135)**	**(0.092)**	**(0.116)**	**(0.097)**	**(0.103)**
Per-Student Expenditure ($1000's)		-0.028**						-0.009
		(0.006)						(0.010)
Per-Student Expenditure^2 ($1000's)		0.0001*						0.000
		(0.0000)						(0.000)
Student-Teacher Ratio			0.094**		0.054*		0.058**	0.038#
			(0.021)		(0.022)		(0.022)	(0.020)
Student-Teacher Ratio^2			-0.002**		-0.002**		-0.002**	-0.001**
			(0.001)		(0.001)		(0.001)	(0.000)
Enrollment (1000's)				0.494**	0.374**		0.375**	0.352**
				(0.085)	(0.085)		(0.087)	(0.088)
Enrollment^2 (1000's)				-0.092**	-0.071**		-0.070*	-0.066*
				(0.027)	(0.027)		(0.028)	(0.028)
Teacher Experience: 0 Years						-0.002	-0.005**	-0.005**
						(0.002)	(0.002)	(0.002)
Teacher Experience: 1-5 Years						0.001	-0.003#	-0.003#
						(0.002)	(0.002)	(0.002)
Teacher Experience: 6-10 Years						0.003	-0.001	-0.001
						(0.002)	(0.002)	(0.002)
Teacher Experience: 11-20 Years						0.004#	-0.001	-0.001
						(0.002)	(0.002)	(0.002)

B: Attendence Rate (%)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Convert	**-0.097#**	**0.017**	**0.066**	**0.117**	**0.082**	**-0.072**	**-0.050**	**-0.095**
	(0.054)	**(0.159)**	**(0.153)**	**(0.160)**	**(0.154)**	**(0.170)**	**(0.171)**	**(0.177)**
Startup	**1.416**	**1.437**	**2.805****	**2.087**	**2.957****	**1.841****	**2.543****	**2.218****
	(1.191)	**(1.218)**	**(0.314)**	**(1.350)**	**(0.639)**	**(0.443)**	**(0.615)**	**(0.566)**
Per-Student Expenditure ($1000's)		-0.178**						-0.184
		(0.057)						(0.113)
Per-Student Expenditure^2 ($1000's)		0.001						0.000
		(0.000)						(0.002)
Student-Teacher Ratio			0.118		0.103		0.096	-0.278
			(0.201)		(0.189)		(0.193)	(0.222)
Student-Teacher Ratio^2			-0.001		-0.001		-0.001	0.008
			(0.005)		(0.004)		(0.004)	(0.005)
Enrollment (1000's)				0.461	-0.177		-0.212	-0.661
				(0.671)	(0.644)		(0.648)	(0.537)
Enrollment^2 (1000's)				-0.028	0.112		0.116	0.210#
				(0.154)	(0.145)		(0.148)	(0.126)
Teacher Experience: 0 Years						0.020*	0.015*	0.013*
						(0.008)	(0.007)	(0.006)
Teacher Experience: 1-5 Years						0.023**	0.019**	0.017**
						(0.005)	(0.006)	(0.005)
Teacher Experience: 6-10 Years						0.019*	0.014*	0.013*
						(0.008)	(0.007)	(0.007)
Teacher Experience: 11-20 Years						0.016#	0.011	0.014#
						(0.009)	(0.007)	(0.008)

Panels C, D, and E show how controlling for school characteristics affects the estimates for test scores. The only test score estimate that substantially changes is math test scores. While the math estimates for conversion charters improve a bit, there is a large drop in test score estimates for startup charters. These results suggest that, if enrollment were higher and student-teacher ratios were lower, then test scores would be lower in startup charters.

1.7 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 chapter, I have tried to address these gaps in the literature using new data from an anonymous large urban school district (ALUSD) with an extensive charter program. Through the use of individual fixed effects, I am able to account for potential bias resulting from time-invariant unobserved characteristics of students. There are some potential pitfalls from using this strategy. Fixed effects estimates can be biased if there is selection into and out of charter schools based on changes in outcomes, non-random attrition, or persistence in charter effects. I adjust my estimates for these complications using a variety of econometric techniques.

My estimates suggest that charters are effective at improving student behavior, on average, while their impact on test scores is mixed. There is no statistically significant effect on retention or attendance.. However, startup charters, schools that

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

In addition to the impact estimates, I also find substantial evidence of selection based on changes in outcome measures, particularly for students in startup charters. I correct for this using interrupted panel estimates (Hanushek et al. 2005, Hanushek et al. 2002, Ashenfelter 1978) and find little to suggest that the selection has a substantial effect on the fixed effects estimates. In addition, I account for the potential endogeneity of attrition by using a semi-parametric estimator proposed by Kyriazidou (1997). These estimates suggest that my fixed effects estimates are not substantially affected by non-random attrition. Finally, I find some evidence of persistence in charter impacts for conversion charters, but no evidence of persistence

for startup charters. There is a particularly large increase in disciplinary actions after students leave startup charters.

Taken together, these results paint a mixed picture of charter schools. On the one hand, charters seem to be effective at improving student discipline and attendance while students are enrolled. On the other hand, the evidence suggests that these effects do not last after students return to non-charter schools. Thus, as long as students return to non-charter schools after attending a charter, the evidence presented here suggests that they will not garner any long-term benefits. Hence, if charters are to be an effective strategy for improving student performance, there would need to be a large enough supply so that students could attend charters throughout their entire academic careers.

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

This chapter looks at one aspect of charter schools - how they affect students who enroll in them. While more research is needed on this issue, there are other aspects of charter schools that also require further study before we can have a complete

picture of how these schools work, such as how charter schools affect non-charter students. I address this question in the next chapter. We also need to get inside the "black box" of charter schools and establish why charter schools work or do not work. In particular, the role of spending in charter schools can be very important. If charters are no more effective at instruction than non-charters, they may still be efficiency enhancing if expenditures are lower.

2. HOW DO CHARTER SCHOOLS AFFECT NON-CHARTER PUBLIC SCHOOL STUDENTS?

2.1 Introduction

In the previous chapter, I investigate how charter schools in ALUSD affect those students who choose to attend them. In this chapter, I look at what happens to those students who remain in regular public school. Compared to the large amount of literature on how charter schools affect students who attend them, there is surprisingly little evidence of how charter schools affect students in traditional public schools using individual data (Bifulco and Ladd 2006, Sass 2006, Buddin and Zimmer 2005a, Booker, Gilpatric, Gronberg and Jansen 2004).

There are a few mechanisms through which charter schools may affect traditional public schools. The most commonly cited is a competition effect. When a charter school enrolls a student usually they get a set amount of money from the chartering authority, be it the state government, a university, or a local school district. Almost always, some portion of this funding would have gone to the public school the student would have attended otherwise and thus there is a financial incentive for public schools to prevent students from attending charter schools. In addition, public schools may wish to prevent students from leaving if they can be closed down for low enrollment. If these two incentives spur public school teachers and administrators to increase effort and efficiency, then charters would exert a positive competition effect on public schools. On the other hand, the loss of funding from students switching to charters may make it more difficult for schools to improve, causing outcomes to

worsen. In addition, some theoretical work by Cardon (2003) suggests that if there are capacity constraints on charters then public schools may not respond to charter competition. Indeed, if public schools are overcrowded, they may welcome the charter schools.

Another mechanism is through changes in peer composition. In most cases, though there are some exceptions, previous research, including that presented in chapter 1 of this dissertation, has found that charter students tend to have lower income and are more likely to be racial minorities than non-charter students (Hanushek et al. 2007, Bifulco and Ladd 2006, Sass 2006). In addition, Christensen (2007) finds charter schools report fewer behavioral problems with students then traditional public schools and in the previous chapter I show that charter students in the school district studied here tend to select into charters based on worsening discipline and falling test scores. Thus, it is possible that by attracting lower (or in some cases better) performing students, charter schools may change how peer-effects mechanisms operate in non-charter schools.

Even if we are to abstract away from the mechanism of charter impacts, identifying the effects of charter schools on non-charter students is problematic because both a student's choice of what school to attend and a charter school's choice of where to locate are not random. Thus, any study of charter school impacts on non-charter students must account for these two potential types of selection bias. Previous work has used student fixed-effects to account for endogenous movements of students and school fixed-effects to account for charter location. While some researchers consider the former to be a sufficient correction for student movements, concerns have been raised that school fixed-effects are insufficient for addressing endogenous charter location since they only account for selection based on time variant characteristics. Fixed effects also insufficiently account for sample selection derived from specific types of students leaving regular public schools to attend charter schools.

In this chapter, I look at how charter schools in ALUSD affect students who remain in traditional public schools. These include both the charters that are sponsored by ALUSD and those that are sponsored by other entities, mostly the state government. I add to the current literature in four ways. First, I provide estimates that use school specific time trends and instrumental variable techniques to account for the potential that charter schools endogenously locate near particular types of non-charter schools. Second, I assess the effects of charter schools on discipline and attendance of non-charter students in addition to test scores. Third, I account for the contamination of competition impacts with changes in peer composition by controlling for twice lagged average peer discipline and grades. Fourth, I look at whether there are different impacts of charter schools based on whether they are conversions, schools that were originally traditional public schools but convert to charter status, or startups, schools that begin as charters and by whether the charter is granted by the local school district or some other government entity.

I find that when school specific trends are added to regressions, which correct for charter location based on permanent trends of non-charter schools, there is little impact of charter schools on non-charter students. In addition, some of the estimates change considerably when trends are added. This highlights the possibility that school fixed-effects are insufficient corrections for endogenous charter locations. Instrumental variable estimates using the number of bus routes nearby as an instrument for charter penetration provide further evidence that school fixed-effects estimates are insufficient. While these estimates are not conclusive due to large standard errors, they suggest that using school fixed effects or school fixed effects combined with trends underestimate the charter schools' effects on discipline and overestimate the charter schools' effects on test scores. I also find that controlling for lagged peer effects has little effect on the estimates using school fixed-effects or school fixed-effects combined with school specific trends.

2.2 Literature, Mechanisms, and Selection

2.2.1 Previous Literature

While there is a large literature on how charter schools affect students who attend them,[1] only a handful of papers have considered how charter schools affect non-charter students. Some early work on the topic has used school level data to answer this question. Bettinger (2005) finds little effect of charter schools on public schools while Hoxby (2004) and Holmes, Desimone and Rupp (2003) find positive effects of charter schools on public schools. While these papers were instrumental in starting this line of literature, since all outcome measures are aggregated to the school level it is impossible to tell whether these results are due to charter competition or changes in the student body composition.

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

Thus, in general, researchers have found that charter schools have, at worst, no statistically significant effect on non-charter public schools and, at best, a large positive effect. However, despite the systematic results, there are still a number of unanswered questions that remain. First, all of the papers listed above only consider how charter schools affect test scores. Charter schools may have impacts along other dimensions as well. For example, if parents choose to send their children to

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[1] Please see chapter 1 for a discussion of this literature

charters because of discipline and safety problems, evidence supported by the first chapter of this dissertation and Weiher and Tedin (2002), then regular public schools may respond by trying to improve their students' discipline. Second, although most of the work in this literature refers to charter impacts as "charter competition," as mentioned above, there are multiple mechanisms through which charters may affect non-charter students. In addition to competition, one potentially important mechanism is changing peer group composition. This, in turn, can affect the peer effects mechanism. Indeed, work by Booker, Buddin and Zimmer (2005) find considerable changes in peer characteristics when charter schools open near California public schools. Third, although researchers have used school fixed-effects to account for the endogenous location decision of charter schools, estimates will be inconsistent if charter schools select their locations based on time-varying characteristics. For example, charters may prefer to locate in areas where schools are on downwards achievement trends so that demand will likely increase in the future.

2.2.2   How Charter Schools May Affect Non-Charter Students

Charter schools may affect non-charter students in public schools in a number of ways. The most commonly cited mechanism is through a competition effect. Since charter schools draw enrollment and, as a result, funding away from regular public schools, charter proponents have argued that administrators and teachers in traditional public schools will increase effort and innovation so that they may prevent their students from leaving. However, there are a number of reasons why traditional public schools may not respond to charter competition. First, schools are not profit maximizing firms. They are more like not-for-profit firms, and thus may depart from profit-maximizing behavior (Duggan 2002, Glaeser 2002, Glaeser and Shleifer 2001, Duggan 2000, Lakdawalla and Philipson 1998, Shleifer 1998). Thus, it is not clear that loss of funding provides a large incentive to improve. Indeed, if schools are

overcrowded principals may welcome the charters, as that would make instruction easier. Second, work by Cardon (2003) shows that, in theory, even if the utility functions of school administrators and teachers do desire higher enrollment, schools may not respond to charter competition if there are capacity constraints placed on charters. Many states impose these constraints by limiting the size of individual charter schools or limiting the number of charters that may be opened. Third, it is also possible that the loss of funds that traditional schools incur when charters draw students away would hamper the flexibility that administrators would have to make adjustments. Fourth, if districts are able and willing to divert funds easily from schools faced with little charter competition to schools facing a large amount of competition, the incentive to improve may be small.

In addition to the competition effect, charter schools may impact non-charter students through changes in peer effects. Previous work, along with the first chapter of this dissertation, has shown that there is substantial selection into charter schools (Hanushek et al. 2007, Bifulco and Ladd 2006, Sass 2006). Thus, we would expect the composition of students left in schools with charters nearby would change. This was found in schools in California (Booker, Zimmer and Buddin 2005). The changing composition could impact students through peer effects (Cooley 2006, Hoxby and Weingarth 2005, Angrist and Lang 2004, Hanushek, Kain, Markman and Rivkin 2003, Zimmerman 2003, Sacerdote 2001). A priori it would seem that, since charters generally attract lower ability students, non-charter students would likely improve due to peer-effects.[2] Thus estimates of charter impacts may overestimate the actual "competition" effect.

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[2] The standard model of peer effects is the linear-in-means model where the effect is linear in average peer ability. In this model we'd expect non-charter students to improve as described. However, recent evidence by Hoxby and Weingarth (2005) suggests that the linear-in-means model is wrong. They finds evidence suggesting that a more appropriate model is one where outcomes improve when there are concentrations of students of similar ability. In this case charters would also tend to improve outcomes since they would tighten the distribution of students in non-charter schools. Other evidence provided by Foster(2006) suggests that peer effects may not work through social interactions. It is unclear what implication this may have for charter schools.

2.2.3   Endogenous Student Movements and Charter Location

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

The second problem is that the location of charter schools themselves is not random. There are a number of factors that go into the decision of where to locate a charter school including space availability and transportation options, since most charters do not have access to district provided bussing.[3] To the extent that these factors are not associated with student characteristics this is not a problem. However, an additional factor that likely plays a large role in the decision of where to locate is the demand for an alternative schooling environment. This would likely be higher in areas with low-performing schools. Indeed, many charters are created through grass roots organizing in a community, often in response to the poor quality of the local schools. In some cases charters convert from regular public schools rather than

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[3] In ALUSD, charters that convert from regular public schools have busing available, but district startups and non-district charters do not. Nonetheless, in some cases charter schools provide their own busing.

choose their location. For these schools we would expect endogenous location to be less of a problem. Nonetheless, the choice of whether or not to convert may be correlated with neighborhood characteristics.

Depending on the nature of this selection, the bias in the charter impact estimates could be positive or negative. If charters locate near low-performing schools based on time-invariant characteristics of the public schools (i.e. the charters locate near schools that have been low performing for many years and have shown little improvement or worsening), then simple OLS regressions would underestimate the effects of charters. Researchers have addressed this type of selection by including school fixed-effects in OLS regressions. However, if location is, at least partially, based on time-varying characteristics of non-charter schools then this strategy will not eliminate, and in fact may exacerbate, the bias. One possible way this can occur is if charters locate in areas where performance is worsening on the belief that this will generate higher demand in the future. Since many charters face high startup costs and thus open with few students and expand later, having an anticipated increase in demand could be desirable. Another mechanism for this selection would be if parents and community leaders are not spurred to start charter schools until they see performance in the public schools worsening. The direction of this type of bias depends on whether the trends are permanent or temporary. To illustrate this, Figure 2.1 shows the difference between estimated and actual charter impacts under the two types of trends. If the trends are permanent, then OLS regressions would underestimate the charter impacts. If the trends are temporary and schools exhibit mean reversion in their performance, then OLS regressions would overestimate the charter impacts.

Figure 2.1- Bias of School Fixed-Effects from Selection Of Charter Location Based on Non-Charter Trends

Selection Based on
Temporary Trend

Charter Opens

Estimated
Impact

True
Impact

Selection Based On
Permanent Trend

Charter Opens

Estimated Impact
(negative)

True Impact
(positive)
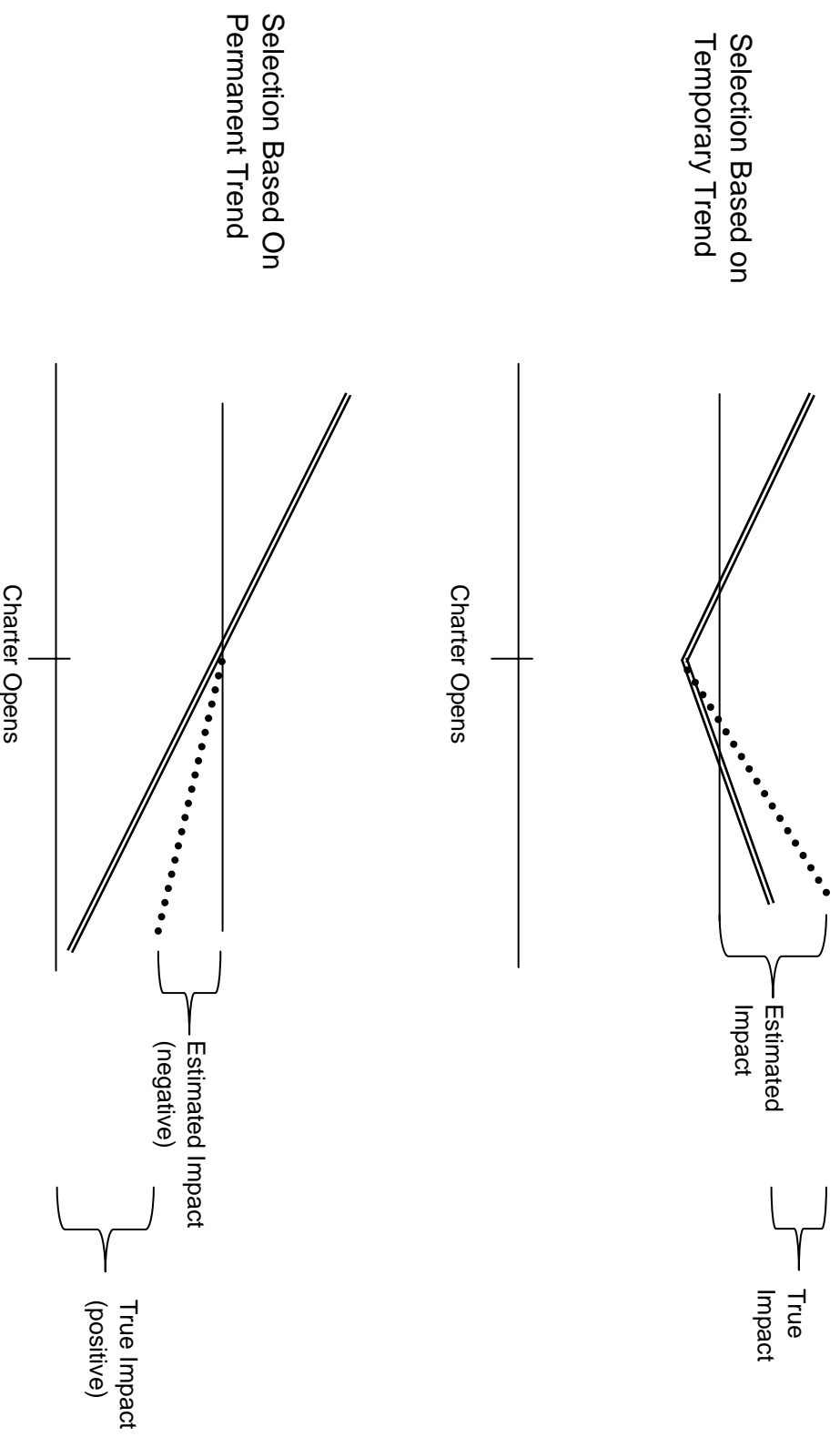
Single line is outcome in school without charter nearby after removing school fixed effect. Double line reflects outcome for student in school with charter opening nearby if charter did not open after removing school fixed effect. Dotted line is what happens to the outcome after a charter opens nearby.

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2.3   Charter Schools in ALUSD

ALUSD is an ideal location to study the effects of charters on non-charter students because there are both charters authorized directly by the district (district charters) and charters authorized by other authorities (non-district charters).[4]   In addition to separating charters by whether they are district or non-district, I also separate the district charter schools further by whether they are conversions or startups. Please refer to section 1.2.3 for a detailed description of charter schools in ALUSD.

2.4   Data

In this chapter I make use of the same data as in chapter 1. For a description of how I create my two samples, the "base" sample and the "test" sample, please see section 1.3. Unlike in the first chapter, however, I limit the base sample to 1996 and later, since charter schools only began appearing in 1996. In addition, school location data for 2004 had not been made available until recently, thus I exclude data from that year. The final base sample has over 1.2 million student-year observations for students in non-charter public schools. The final test sample includes over 900,000 observations, although this falls to approximately 500,000 observations when using value-added specifications of test-score regressions.[5]

School addresses were derived from the US Department of Education's Common Core of Data. Any missing addresses were filled in using school directories acquired directly from ALUSD. These addresses were then converted to latitude and longitude using the geocoder.us website. If an address could not be matched using *geocoder.us* then I used Google Earth™to find the latitude and longitude. Afterwards, distances between schools were derived using the sphdist command in

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[4] The vast majority of these charters are authorized by the state government, but a few are authorized by local universities

[5] 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.

Stata$^{\text{TM}}$. In order to use bus routes as an instrument for charter penetration I obtained geographic information system (GIS) files for the 2006 bus route maps from ALUSD's local transportation authority. Schools were matched to routes within various distances using ArcGIS$^{\text{TM}}$. Economic characteristics and population density of census tracts were obtained from the 2000 Census Summary Files.

Table 2.1 provides summary statistics for school-years that are between the 0th and 49th, 50th and 74th, 75th and 89th, and 90th and 99th percentiles of charter penetration within two miles from 1998 - 2003. I define charter penetration here as the fraction of students within a specified radius who are in grades covered by the non-charter school but attend a charter school. The first four columns show characteristics based on penetration by any charter type. As charter penetration increases students are more likely to be at-risk of dropping out, have limited English proficiency, and be recent immigrants. In addition, test scores and attendance are lower in schools with more charter penetration. The last four columns show penetration by startup charters (both district and non-district) only. Since these charters choose their location while the locations of conversions is pre-determined, there may be different characteristics of the schools with high penetration by this measure. This does not appear to be the case, as the descriptive statistics cut by this measure of penetration are similar to those cut by the other measure.

2.5 Empirical Strategy

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

$$y_{igjt} = \alpha + \beta C_{jt} + \mathbf{X}_{igjt}\Omega + \mathbf{GradeYear}_{gt}\Pi + \epsilon_{igjt} \tag{2.1}$$

Table 2.1 - Characteristics of ALUSD Schools by Charter Penetration

	Conversions Included in Penetration Measure				Conversions Not Included in Penetration Measure			
Percentiles of Charter Penetration [†]	0 - 49	50 - 74	75 - 89	90 - 99	0 - 49	50 - 74	75 - 89	90 - 99
Range of Charter Penetration Rates	0.0% - 0.1%	0.1% - 0.8%	0.8% - 2.3%	2.3% - 15.7%	0.0% - 0.05%	0.05%-0.5%	0.5%-1.6%	1.6% - 11.5%
Demographics								
Female	0.484	0.492	0.487	0.466	0.487	0.489	0.483	0.466
		[1.2]	[0.3]	[1.2]		[0.4]	[0.6]	[1.3]
White	0.092	0.078	0.077	0.085	0.089	0.084	0.069	0.096
		[0.8]	[0.7]	[1.2]		[1.1]	[1.1]	[0.2]
Economically Disadvantaged‡	0.783	0.828#	0.824	0.796	0.796	0.819	0.808	0.776
		[1.8]	[1.4]	[0.4]		[0.9]	[0.4]	[0.5]
At Risk	0.545	0.615**	0.639**	0.627**	0.554	0.588#	0.629**	0.666**
		[3.6]	[3.7]	[2.6]		[1.9]	[3.0]	[3.3]
Limited English Proficient	0.236	0.318**	0.302*	0.262	0.256	0.290	0.276	0.276
		[3.5]	[2.2]	[0.7]		[1.6]	[0.7]	[0.5]
Special Education	0.100	0.085	0.089	0.070	0.095	0.086	0.086	0.091
		[1.0]	[0.6]	[1.2]		[0.6]	[0.5]	[0.5]
Gifted & Talented	0.118	0.109	0.117	0.152	0.117	0.116	0.110	0.151
		[1.2]	[0.6]	[1.2]		[0.6]	[0.6]	[0.1]
Recent Immigrant	0.047	0.065**	0.064*	0.068#	0.048	0.061**	0.062*	0.077*
		[3.8]	[2.4]	[1.9]		[2.9]	[2.2]	[1.1]
Grade Level	4.773	4.444	4.584	4.724	4.621	4.344	4.796	5.417
		[1.2]	[0.5]	[0.1]		[1.1]	[0.5]	[1.6]
Achievement								
Math NPR Score	48.509	49.135	48.555	44.389#	47.937	50.304#	48.318	44.630
		[0.5]	[0.0]	[1.8]		[1.9]	[0.2]	[1.4]
Reading NPR Score	48.906	48.226	47.991	43.732*	48.422	49.523	47.516	43.553#
		[0.5]	[0.6]	[2.2]		[0.9]	[0.2]	[1.9]
Language NPR Score	43.890	43.192	42.732	39.061*	43.212	44.528	42.909	38.775#
		[0.5]	[0.7]	[2.1]		[1.0]	[0.2]	[1.7]
Behavior								
# of Disciplinary Infractions	0.305	0.300	0.389	0.294	0.294	0.263	0.391	0.435
		[0.1]	[1.1]	[0.2]		[0.8]	[1.4]	[1.6]
Attendance Rate (%)	94.829	94.798	93.788	92.669#	94.791	95.115	93.952	91.816*
		[0.1]	[1.4]	[1.8]		[1.0]	[1.2]	[2.4]

† - Charter penetration is measured as the fraction of students within a 3 mile radius of a school and who are in a grade covered by the school who are in a charter.

‡ - Combination of students who qualify for free or reduced price lunch, or qualify for some other Federal anti-poverty program.
T-statistics of difference from 1st quartile are in brackets and are based on standard errors clustered by school. Covers 1998 - 2003 only, so that only years with a large number of charter schools are considered. Observations are greater than 1300. Exact sample sizes cannot be revealed due to confidentiality restrictions.

where y_{igjt} is an outcome measure for student i in grade g in school j during academic year t, C is some measure of charter school penetration into the school's education market, $\mathbf{X}$ is a set of observable student characteristics, $\mathbf{GradeYear}_{gt}$ is a set of grade-by-year indicators and ϵ is an error term. Epsilon can further be broken down into student and school error components

$$\epsilon_{igjt} = \gamma_{ijgt} + \eta_{jt} \tag{2.2}$$

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

To deal with the potential that $cov(\gamma_{ijgt}, C_{gjt}) \neq 0$ I assume that

$$\gamma_{ijgt} = \lambda_i + \nu_{igjt} \tag{2.3}$$

where $cov(\lambda_i, C_{gjt}) \neq 0$ but $cov(\nu_{igjt}, C_{gjt}) = 0$. Under this assumption we can remove λ from (2.1) by demeaning the model with respect to the individual as such

$$\bar{y}_{igjt} = \beta \bar{C}_{jt} + \bar{\mathbf{X}}_{igjt}\Omega + \overline{\mathbf{GradeYear}}_{gt}\Pi + \bar{\nu}_{igjt} + \bar{\eta}_{jt}. \tag{2.4}$$

Note that while all students contributed to the identification of equation (2.1), now only those students who are in the sample for multiple years and have changes in charter penetration contribute to the identification of β.

Although this procedure corrects for student selection under the assumption stated above, if charter location is endogenous then $cov(\bar{\eta}_{jt}, C_{jt}) \neq 0$. To address this type of selection, researchers have made the assumption that

$$\bar{\eta}_{jt} = \bar{\zeta}_j + \bar{\theta}_{jt} \tag{2.5}$$

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where $cov(\bar{\zeta}_j, C_{gjt}) \neq 0$ and $cov(\bar{\theta}_{jt}, C_{gjt}) = 0$. Under this assumption we can add school indicator dummies to the regression that will move $\bar{\zeta}$ into the observable part of the equation. Thus, our regression equation becomes

$$\bar{y}_{igjt} = \beta\bar{C}_{jt} + \bar{\mathbf{X}}_{igjt}\Omega + \overline{\mathbf{GradeYear}}_{gt}\Pi + \bar{\mathbf{S}}_{igjt}\Gamma + \bar{\nu}_{igjt} + \bar{\theta}_{jt}. \qquad (2.6)$$

where $\mathbf{S}$ is the vector of school indicators. In this case, β is identified off of students who are in ALUSD for multiple years and attend schools that experience a change in charter penetration. However, if charters select locations based on trends in local school performance, or, similarly, if grass root efforts to create charters are spurred by trends in local schooling conditions, then equation (2.5) will be incorrect and including school indicators will not correct for selection. One way we can address this issue is to add school specific time-trends to the regression.

$$\bar{y}_{igjt} = \beta\bar{C}_{gjt} + \bar{\mathbf{X}}_{igjt}\Omega + \overline{\mathbf{GradeYear}}_{gt}\Pi + \bar{\mathbf{S}}_{igjt}\Gamma + \mathbf{S}\bar{*}t_{igjt}\Lambda + \bar{\nu}_{igjt}. \qquad (2.7)$$

Identification in this case is based on the same students as in equation (2.4). As long as charter location is correlated with linear permanent trends but uncorrelated with non-linear or temporary trends, then this will eliminate the bias. If this is not the case, however, then this strategy will not solve the problem. Thus, we may want to use an instrumental variables strategy defined by the equations

$$\bar{y}_{igjt} = \beta\hat{\bar{C}}_{jt} + \bar{\mathbf{X}}_{igjt}\Omega + \overline{\mathbf{GradeYear}}_{gt}\Pi + \bar{\nu}_{igjt} \qquad (2.8)$$

$$\hat{\bar{C}}_{jt} = \phi\bar{Z}_{igjt} + \bar{\mathbf{X}}_{igjt}\Theta + \overline{\mathbf{GradeYear}}_{gt}\Upsilon + \bar{\rho}_{igjt} \qquad (2.9)$$

I propose using the availability of bus routes near a regular public school as an instrument for whether a district startup or non-district charter school opens nearby. This is similar to the strategy used by Lavy (2006) in his analysis of school choice in Tel

Aviv. Since these types of charters do not have access to bussing provided by ALUSD, parents usually need to rely on their own vehicles, public transportation, or walking to send their children to these schools. Because of these transportation restrictions and the high poverty rates of charter students, it is reasonable to believe that charters attempt to locate in areas where there is substantial public transportation. Thus, identification of β in this case is based on students who are in ALUSD for multiple years, experience a change in charter penetration, and attend schools where charter schools are induced to locate nearby due to the availability of bus transportation.

Nonetheless, even if one is able to surmount these identification concerns, the estimates may still be misrepresentative. Generally, the estimates of charter impacts on non-charter students have been interpreted as charter competition effects. However, other mechanisms, such as changes in peer composition generating adjustments in peer effects, may also play a role. How much is due to peer composition changes is particularly important, since compositional changes could conceivably be achieved through the regular public school system. For example, districts could increase the availability of "alternative" schools for students with behavioral or academic problems. Thus, from a policy perspective it is important to remove the impact of peer composition changes from the overall charter school impacts. Table 2.2 shows how charter students differ from non-charter students. I focus on district startups and non-district charters since there is much more movement in and out of these schools then there is in conversion charters. In addition, since I cannot observe directly whether a student enters a non-district charter, I use students who leave the dataset in schools within two miles of an overlapping non-district charter as a proxy for charter students. I also limit this group to those in first through eighth grade so as to minimize the number of dropouts in the group. In both cases there are substantial differences between charter students and those who do not attend charters. Generally the charter students and the ALUSD leavers are more disadvantaged and have

Table 2.2 - Characteristics of Students Who Enter Startup Charters or Leave ALUSD

	Startup Enterers and Non-Charter Stayers		ALUSD Leavers and Stayers for Schools with an Overlapping State Charter Within 2 Miles (Grades 1 - 8 Only)	
	Stayers	Enterers	Stayers	Leavers
Demographics				
Female	0.491	0.416** [4.6]	0.491	0.487 [1.5]
White	0.106	0.029** [6.9]	0.092	0.110** [2.7]
Economically Disadvantaged‡	0.729	0.791* [2.3]	0.805	0.795 [0.9]
Limited English Proficient	0.260	0.225# [1.7]	0.333	0.269** [6.3]
At Risk	0.531	0.709** [8.2]	0.561	0.537* [2.4]
Special Education	0.112	0.088** [3.5]	0.108	0.116 [1.5]
Gifted & Talented	0.102	0.044** [5.1]	0.101	0.068** [4.2]
Recent Immigrant	0.068	0.064 [0.4]	0.077	0.090** [3.2]
Achievement				
Math NPR Score	46.266	36.730** [8.7]	49.022	44.963** [6.4]
Reading NPR Score	41.913	30.950** [9.1]	44.318	41.191** [4.5]
Language NPR Score	47.498	36.911** [7.4]	49.598	45.842** [5.8]
Behavior				
# of Disciplinary Infractions	0.396	1.126** [6.0]	0.324	0.476** [4.5]
Attendance Rate (%)	94.028	89.525** [5.9]	95.994	93.287** [9.6]

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

T-statistics of differences in means in parentheses and based on standard errors clustered by school. Sample limited to base sample students in schools that never become charters, are in grades 1 - 11, and are enrolled in a school in year t which will still be in operation in year t+1. Only 1996 - 2002 considered, since school distance data only available up to 2003.

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considerably worse academic and behavioral outcomes. Thus, to address the potential contamination of charter competition effects with changes in peer composition I run variations of the models described above that also include measures of lagged peer characteristics.

2.6 Results

2.6.1 Defining Charter Penetration

Before conducting this analysis, one needs a definition of "charter penetration." The first measure of charter penetration, by Hoxby (2001), is whether a school district has over 6% of enrollment in charter schools. But this does not inform us about school level penetration, nor does it necessarily apply to locations other than Michigan, which is the state she studies.

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

$$ChartPen_{jtd} = \frac{\sum_{g=Gmin_j}^{Gmax_j} \sum_{n=1}^{N_c^d} Enroll_{gnt}}{\sum_{g=Gmin_j}^{Gmax_j} \sum_{n=1}^{N_{nc}^d + N_c^d} Enroll_{gnt}} \qquad (2.10)$$

where $Gmin$ and $Gmax$ are the lowest and highest grades, respectively for school j and $Enroll_{gnt}$ is enrollment in grade g, school n and year t. For example, suppose I am measuring charter penetration within one mile of a school, j, that serves grades kindergarten through five. In this case I calculate the total number of students in those grades attending charter schools within one mile and divide by the total number of students attending any school within one mile (including those in j) in those grades. Thus, my charter penetration measure is the fraction of all public school and charter school students in overlapping grades who attend a particular type of charter school within a geographic radius around the public school

The other determinant of charter penetration is the distance radius around a school in which charters would be considered competitors. Since I am only looking at one school district, I cannot use the district itself as the market. Indeed, it is not clear whether competitive incentives are stronger at the school or district level. Thus, previous research that looks at school-level competition has generally defined some linear distance from a school as the competitive market. However, the correct distance probably varies considerably with the urbanization of the locations being considered. Because of this, previous papers include estimates along a range of distances, although these ranges also vary widely. Bifulco and Ladd (2006) and Sass (2006) use 2.5, 5, and 10 miles, while Holmes, Desimone, and Rupp (2003) use distances ranging from 5 to 20 kilometers (3.1 to 12.4 miles) and also use the county as the local education market. Booker, et al. (2004) use the school district as the local education market. All of these papers use state level data and so their distances may not work well in an urban environment such as ALUSD. To figure out the optimal distance measure, I run regressions of the form

$$y_{jt} = \alpha + \beta ChartDist_{jt} + \mathbf{X}_{jt}\Omega + \mathbf{School}_j\Theta + \mathbf{Year}_t\Lambda + \epsilon_{jt} \qquad (2.11)$$

where y_{jt} is the fraction of students who leave school j and enter a district startup charter in year $t+1$, $ChartDist_{jt}$ is the number of startup charters with overlapping grades within a certain radius, $\mathbf{X}_{jt}$ is a set of observable school characteristics, and **School**$_j$ and **Year**$_t$ are school and year dummies, respectively. The results of this regression are shown in panel A of Table 2.3. In panel B, I show similar regressions for

Table 2.3 - Relationship Between Student Movements and the Distance Between Non-Charter
Schools and Charter Schools with Overlapping Grades

A. Percent of Students in a Non-Charters in Year t Who Switch to a Startup District Charter in Year t + 1

	(1)	(2)	(3)	(4)	(5)	(6)
# of Startups within 1 Mile	0.173*					0.173*
	(0.068)					(0.071)
# of Startups within 2 Miles		0.069				
		(0.049)				
# of Startups within 3 Miles			0.053#			
			(0.030)			
# of Startups within 4 Miles				0.031		
				(0.025)		
# of Startups within 5 Miles					0.022	
					(0.020)	
# of Startups between 1 and 2 Miles						0.018
						(0.077)
# of Startups between 2 and 3 Miles						0.045
						(0.031)
# of Startups between 3 and 4 Miles						-0.001
						(0.028)
# of Startups between 4 and 5 Miles						0.000
						(0.022)

B. Percent of Students in Non-Charters in Year t Who Leave ALUSD in Year t + 1

	(1)	(2)	(3)	(4)	(5)	(6)
# of Non-District Charters within 1 Mile	0.423*					0.467*
	(0.179)					(0.187)
# of Non-District Charters within 2 Miles		0.267*				
		(0.115)				
# of Non-District Charters within 3 Miles			0.017			
			(0.084)			
# of Non-District Charters within 4 Miles				0.026		
				(0.059)		
# of Non-District Charters within 5 Miles					0.012	
					(0.054)	
# of Non-District Charters between 1 and 2 Miles						0.170
						(0.123)
# of Non-District Charters between 2 and 3 Miles						-0.162
						(0.110)
# of Non-District Charters between 3 and 4 Miles						0.030
						(0.089)
# of Non-District Charters between 4 and 5 Miles						-0.083
						(0.096)

Unit of observation is the school-year. Each regression includes over 1500 school-year observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Analysis covers years t = 1993 to 2002. Panel titles denote the dependent variables. One school is dropped due to missing data. Each regression also includes the percent of students who are black, hispanic, native american, asian or pacific islander, economically disadvantaged, limited English proficient, in vocational programs, in special education programs, in bilingual programs, and in gifted programs. The regressions also include the percent of teachers who have 1-5, 6-10, 11-20, and more than 20 years of experience as well as the student-faculty ratio, and year and campus fixed effects. Robust standard errors clustered by school are in parentheses. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

the number of non-district charter schools with overlapping grades nearby. However, since I cannot observe when students enter non-district charters, I proxy using the

percent of students who leave ALUSD the following year. The regressions consider how student movements change with the distance of the charter schools from the non-charter schools. The results show that only schools with charters very close by - less than one mile - lose a statistically significant fraction of their students to charter schools. Each district charter within one mile is associated with a loss of 0.2% of students while each non-district charter within one mile is associated with 0.4% more students leaving ALUSD. Both of these are statistically significant at the five percent level. Unfortunately there is not enough variation in charter location to look at distances less than one mile, thus, in light of these results, I use the distances of 1, 1.5, and 2 miles in my measures of charter penetration.

2.6.2   Estimates Using School Fixed Effects and School Time Trends

Table 2.4 shows the baseline results for this chapter. Each dependent and independent variable used in the regressions is de-meaned to remove the student fixed-effect. In addition to the variables listed in the table, the regressions also includes some time-varying student characteristics: free lunch eligibility, reduced price lunch eligibility, whether the student has another economic disadvantage, whether the student is a recent immigrant, whether the student's parents are migrant workers, and grade-by-year indicator variables. The first column for each distance radius shows regressions with no corrections for endogenous location. The second column includes a set of school indicator variables. The third column shows results from regressions that include both school indicator variables and school-specific linear time trends. All standard errors are clustered by schools to allow for correlation of the error terms across students who attend the same school. I consider five outcome measures - the number of disciplinary infractions warranting an in-school suspension or more severe punishment, the attendance rate, and annual changes in math, reading, and language standardized exam scores. The test-score measure I use is the national percentile

Table 2.4 - Student Fixed Effects Estimates of Effect of Charter Schools on Non-Charter Students

Dependent Variable	Measure of Charter Penetration: Share Within 1 Miles			Measure of Charter Penetration: Share Within 1.5 Miles			Measure of Charter Penetration: Share Within 2 Miles		
	No Endogenous Location Corrections	School Fixed Effects	School Fixed-Effects & School Specific Time Trends	No Endogenous Location Corrections	School Fixed Effects	School Fixed-Effects & School Specific Time Trends	No Endogenous Location Corrections	School Fixed Effects	School Fixed-Effects & School Specific Time Trends
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Enrollment Share in Conversion (%)									
# Disciplinary Infractions	-0.007*	-0.106*	-0.034	-0.007*	-0.024	-0.026	-0.019	-0.052**	-0.065**
	(0.003)	(0.042)	(0.042)	(0.003)	(0.059)	(0.045)	(0.012)	(0.019)	(0.019)
Attendence Rate (%)	0.036**	0.440*	0.557*	0.036**	0.153	0.307#	0.022	-0.059	-0.036
	(0.010)	(0.224)	(0.224)	(0.009)	(0.160)	(0.169)	(0.025)	(0.072)	(0.072)
Δ Math NPR	-1.827	0.388	5.871	0.414	0.694	2.390	-0.508**	-1.207*	-1.747**
	(1.501)	(3.531)	(8.647)	(0.923)	(1.644)	(2.719)	(0.095)	(0.550)	(0.072)
Δ Reading NPR	-2.280*	2.754	10.302#	-1.564*	1.441	3.003	0.291	-0.191	-0.152
	(1.031)	(2.068)	(5.868)	(0.611)	(1.085)	(2.490)	(0.187)	(0.518)	(0.518)
Δ Language NPR	-1.182	-5.142**	3.727	0.053	-0.771	-1.642	-0.010	0.032	-0.417
	(1.931)	(1.815)	(7.686)	(1.108)	(2.233)	(2.020)	(0.162)	(0.589)	(0.494)
Enrollment Share in Startup (%)									
# Disciplinary Infractions	0.460**	0.117*	0.071	0.155#	0.060	0.021	0.145#	0.127#	0.099*
	(0.120)	(0.055)	(0.090)	(0.090)	(0.038)	(0.033)	(0.078)	(0.049)	(0.042)
Attendence Rate (%)	-0.344	-0.217	0.232	-0.381	-0.087	-0.270	-0.270	-0.211*	0.180
	(0.383)	(0.293)	(0.557)	(0.273)	(0.096)	(0.197)	(0.169)	(0.093)	(0.179)
Δ Math NPR	-1.327	-0.373	1.104	-0.579	0.196	1.470	-1.151*	-1.082	-1.707
	(1.395)	(1.947)	(1.351)	(0.795)	(1.018)	(1.343)	(0.529)	(0.720)	(1.138)
Δ Reading NPR	-0.082	-0.123	0.251	0.020	-0.020	0.590	-0.300	-0.783	-1.765*
	(0.854)	(1.059)	(0.861)	(0.549)	(0.669)	(0.734)	(0.454)	(0.640)	(0.810)
Δ Language NPR	-0.614	0.127	0.902	-0.155	0.244	1.229	-0.589	-0.157	0.756
	(0.567)	(0.857)	(1.222)	(0.798)	(1.189)	(1.571)	(0.387)	(0.736)	(1.055)
Enrollment Share in Non-District Charter (%)									
# Disciplinary Infractions	-0.068**	0.002	0.053*	-0.106**	-0.045	-0.001	-0.018	-0.030	-0.014
	(0.025)	(0.013)	(0.027)	(0.038)	(0.030)	(0.035)	(0.038)	(0.018)	(0.032)
Attendence Rate (%)	-0.004	-0.043	-0.145	-0.438#	-0.161	-0.101	-0.371**	-0.126	-0.036
	(0.165)	(0.091)	(0.149)	(0.248)	(0.126)	(0.141)	(0.137)	(0.100)	(0.118)
Δ Math NPR	-1.881**	-2.800*	-1.534	0.250	0.609	0.400	0.174	0.667*	0.348
	(0.666)	(1.095)	(1.543)	(0.317)	(0.626)	(0.711)	(0.168)	(0.299)	(0.378)
Δ Reading NPR	-1.627*	-1.545#	-1.101	0.017	0.791#	0.830	0.007	0.450*	0.223
	(0.656)	(0.928)	(1.388)	(0.421)	(0.421)	(0.531)	(0.119)	(0.188)	(0.299)
Δ Language NPR	-0.441	-0.882	-0.972	-0.073	0.645	1.190#	-0.094	0.537#	0.582#
	(0.785)	(1.098)	(1.740)	(0.285)	(0.564)	(0.609)	(0.149)	(0.275)	(0.317)

All regressions are demeaned within individuals to to remove student fixed effects and include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year dummies as covariates. Huber/White standard errors clustered by school in parentheses. Behavior and attendence regressions contain over 1,200,000 observations. Test score regressions contain over 400,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

ranking (NPR) for a commonly used national norm-referenced examination. NPR is the percent of students in a nationally representative sample of test-takers who score lower than the observed student. Charter penetration is broken into three types: conversion, district startup, and non-district. Each of these measures are used in separate regressions.

Let us begin by considering the effects of charters on student discipline. Without school fixed-effects or school specific trends (columns 1, 4 and 7), conversions and non-district charters appear to improve discipline while district startups increase disciplinary infractions. When school fixed effects are added (columns 2, 5 and 8), the effect of conversions on discipline becomes stronger while the estimates for startups increase and non-district estimates decrease, to the point where non-district impacts are statistically insignificant. Finally, the specification using both school fixed-effects and school specific trends (columns 3, 6 and 9) appears to do little to the estimates for conversions, except within one mile, but move the estimates for startups and non-district charters further in the direction they moved when school fixed effects were added. Using this as the preferred specification, it appears that charters have little effect on discipline - for each school type two out of three of the estimates are statistically insignificant.

For attendance, results with no school fixed-effects or trends suggest that conversions improve attendance, while startups have no statistically significant impact, and non-district schools have worsened attendance. However, when school fixed-effects are added the attendance impacts increase, in some cases quite substantially. Adding school specific time-trends to these regressions increase the attendance impacts further. In this case, the attendance impacts are positive and statistically significant for conversion charters, but there is no statistically significant impact for startups and non-district charters.

Unfortunately, the effect of adding school fixed-effects and school specific

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time-trends on the test score estimates are less clear. Without school FE or trends, all three types of charters seem to have statistically insignificant to negative impacts on test scores. When school fixed-effects are added, although the point estimates change dramatically in some cases, this general pattern holds for conversions and startups, but non-district charters appear to generate negative impacts within one mile and positive impacts within two miles. Nonetheless, when school specific trends are added on top of school fixed effects, these results become statistically insignificant in almost all cases.

Thus, my preferred results using school specific time trends suggest there is little evidence for charters affecting non-charter schools, although conversions appear to generate improvements in attendance. Despite the lack of statistically significant results, these regressions do show that adding school specific trends generates substantial changes in the estimates. This suggests that school fixed-effects do not sufficiently account for endogenous locations of charter schools.

2.6.3 Instrumental Variables Estimates

Even after controlling for school fixed-effects and school specific time-trends we may still be concerned if there is some residual endogeneity of charter penetration. As mentioned before, one possible source would be if charters locate near non-charter schools that are temporarily on downward trends in outcomes or are anticipated to worsen in the future. To address this issue, I use the availability of public transportation, specifically busses, near a non-charter school as an instrument for charter penetration. Since most charter schools need to provide their own transportation, all else equal, we would expect them to locate where public transportation options are plentiful. Since this reasoning does not apply to conversion charters, I will not include them in this analysis.

More specifically, the exclusion restriction I use is the number of bus routes

within a specified radius around a charter school. A bus route is defined as being within the established radius if the bus passes through the radius during its travels. Thus if I'm using one mile as my competition radius, then I'll use bus routes within one mile, competition within 1.5 miles will correspond to bus routes within 1.5 miles, and so on. Table 2.5 shows how the number of bus routes near a school correlates with student and school characteristics. This table highlights the main problem with this instrument, which is that public transportation tends to be more common in areas where people are low-income since wealthier people tend to use private transport. Indeed, the number of bus-routes correlates strongly with economic conditions of the census tract the school is in. Nonetheless, when zip-code indicators for each school are added to the regression most of the economic measures become statistically insignificant. While the estimates once again become statistically significant when student fixed-effects are added, this is due to increased precision rather than a change in the point estimates.

Whether these correlations are a substantial problem depends on whether they are indicative of unobserved characteristics that have independent effects on outcomes that are correlated with bus routes. Unfortunately, there is no way to test this directly. However, we can look at reduced form regressions of bus routes on outcomes to get a sense of the exogeneity of the exclusion restriction. If bus-routes have no predictive effect on outcomes, especially in years prior to charter schools opening, then it is unlikely that unobserved correlates with bus routes affect outcomes, and thus the instrument is exogenous. Table 2.6 shows the results of these regressions. In panel A, I look at how the number of bus routes correlates with discipline and attendance in three periods. The first period, 1993 - 1995, is prior to any charters opening. The second period, 1996 - 1998, is early in the charter period, while the third period, 1999 - 2004 is after charters have become well established. Without student fixed-effects there is no statistically significant

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Table 2.5 - Regressions of Number of Bus Routes on Observable Characteristics

Dependent Variable:	# Busroutes within 1 Mile			# Busroutes within 1.5 Miles			# Busroutes within 2 Miles		
Student Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Female	0.040	0.031	-	0.011	-0.011	-	0.018	0.000	-
	(0.073)	(0.030)	-	(0.098)	(0.043)	-	(0.111)	(0.048)	-
Black	0.993*	0.156	-	1.496*	0.204	-	0.816	-0.279	-
	(0.436)	(0.214)	-	(0.735)	(0.285)	-	(0.883)	(0.282)	-
Hispanic	0.880#	0.132	-	1.448*	0.247	-	0.827	-0.169	-
	(0.523)	(0.182)	-	(0.714)	(0.250)	-	(0.818)	(0.230)	-
Asian	0.195	0.278	-	0.369	0.593#	-	-0.273	0.506	-
	(0.595)	(0.212)	-	(0.975)	(0.359)	-	(1.141)	(0.470)	-
Native American	0.378	0.151	-	0.432	0.120	-	0.286	0.082	-
	(0.610)	(0.262)	-	(0.913)	(0.352)	-	(1.317)	(0.528)	-
Recent Immigrant	0.400	0.104	0.046	0.420	0.110	0.010	0.249	0.146	0.144*
	(0.305)	(0.102)	(0.045)	(0.476)	(0.133)	(0.061)	(0.512)	(0.135)	(0.067)
Parents are Migrant Workers	-0.111	0.180	0.199*	-0.238	0.124	0.079	-0.370	0.008	0.178#
	(0.267)	(0.121)	(0.096)	(0.376)	(0.164)	(0.103)	(0.396)	(0.195)	(0.106)
Limited English Proficient	-0.134	0.016	0.002	-0.537	-0.025	0.016	-0.415	-0.010	0.085
	(0.604)	(0.125)	(0.085)	(0.815)	(0.201)	(0.120)	(0.886)	(0.219)	(0.139)
At Risk	0.196	-0.015	0.000	0.595	-0.005	-0.028	0.456	0.042	0.035
	(0.320)	(0.097)	(0.025)	(0.542)	(0.195)	(0.037)	(0.621)	(0.221)	(0.045)
Gifted	0.356	-0.357	-0.174	1.233	-0.235	0.089	1.459	-0.217	0.134
	(0.564)	(0.357)	(0.120)	(1.375)	(0.436)	(0.152)	(1.527)	(0.415)	(0.159)
Special Education	0.072	0.001	0.087	-0.053	-0.034	0.044	0.071	0.025	-0.009
	(0.243)	(0.091)	(0.058)	(0.390)	(0.155)	(0.082)	(0.459)	(0.178)	(0.072)
Free Lunch Eligible	1.107*	0.201#	0.084**	1.444*	0.411*	0.142**	1.753*	0.057	0.047
	(0.468)	(0.122)	(0.031)	(0.673)	(0.198)	(0.047)	(0.810)	(0.214)	(0.055)
Reduced Price Lunch Eligible	0.429	0.112	0.082**	0.397	0.174	0.124**	0.628	-0.208	-0.004
	(0.459)	(0.129)	(0.028)	(0.668)	(0.190)	(0.045)	(0.769)	(0.198)	(0.053)
Other Economic Disadvantage	0.479*	0.010	0.033	0.875*	0.171	0.082*	0.955*	-0.045	0.081#
	(0.239)	(0.094)	(0.022)	(0.393)	(0.163)	(0.034)	(0.455)	(0.185)	(0.043)
Neighborhoold Characteristics of School									
Population Density	0.072	-0.051	-0.039	0.015	-0.234	-0.221*	-0.042	-0.414**	-0.403**
	(0.214)	(0.123)	(0.072)	(0.299)	(0.166)	(0.106)	(0.298)	(0.153)	(0.090)
Fraction Black	0.916	0.334	-1.224	-5.686	-4.982	-8.614#	-5.488	0.613	-1.516
	(4.174)	(4.072)	(2.689)	(6.846)	(6.953)	(4.752)	(8.379)	(7.092)	(4.330)
Fraction Hispanic	6.848	-9.259	-10.294#	-0.181	-25.363#	-31.948**	20.743	-3.079	-5.502
	(9.893)	(8.577)	(5.482)	(15.155)	(13.176)	(10.102)	(17.834)	(13.677)	(8.359)
Fraction Non-Native	-30.855**	18.356	13.155*	-52.425**	5.262	4.334	-78.237**	-15.197	-13.544
	(10.428)	(11.583)	(6.624)	(13.658)	(13.518)	(7.583)	(15.590)	(13.934)	(8.662)
Fraction w/ HS or Some College	-49.674**	-13.425	-18.453*	-84.264**	-52.648**	-59.410**	-79.063**	-36.927**	-34.902**
	(12.334)	(11.804)	(7.351)	(18.034)	(17.095)	(12.411)	(20.408)	(14.207)	(9.941)
Fraction w/ College or Advanced Degree	1.295	-15.654	-19.494*	-7.380	-47.388*	-59.026**	30.058	-5.927	-10.617
	(11.580)	(11.660)	(7.714)	(18.424)	(20.965)	(16.945)	(21.355)	(18.941)	(12.657)
Labor Force Participation (Male 16+)	24.480*	9.453	10.204*	38.242**	21.344	23.807*	31.322*	18.223	19.795*
	(10.100)	(7.031)	(4.334)	(12.947)	(13.401)	(9.315)	(14.319)	(12.661)	(8.085)
Ln (Household Income)	-9.614**	0.641	1.642	-20.085**	-0.687	1.836	-30.742**	-7.749#	-6.150*
	(2.842)	(2.065)	(1.386)	(3.904)	(4.437)	(2.877)	(4.352)	(4.121)	(2.869)
Fraction receiving Public Assistance	-17.966	27.527#	29.575**	-73.668*	-0.219	0.393	-79.255*	10.415	-0.412
	(24.559)	(15.361)	(9.907)	(31.542)	(17.378)	(12.865)	(38.581)	(17.368)	(11.700)
Grade-Year Indicators	N	Y	Y	N	Y	Y	N	Y	Y
Zip-Code of School Indicators	N	Y	Y	N	Y	Y	N	Y	Y
Student Fixed-Effects	N	N	Y	N	N	Y	N	N	Y

Huber/White standard errors clustered by school in parentheses. Behavior and attendance regressions contain over 1,200,000 observations. Test score regressions contain over 400,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

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Table 2.6A - Reduced Form Estimates of Discipline & Attendance on Number of Bus Routes Near School

	Pre-Charter (1993 - 1995)				Early Post-Charter (1996 - 1998)				Late Post-Charter (1999 - 2003)			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Bus Routes Within 1 Mile												
# Disciplinary Infractions	0.001 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.001 (0.003)	0.000 (0.002)	0.001 (0.003)	-0.002 (0.002)	0.000 (0.004)	0.001 (0.003)	-0.003 (0.003)	-0.002 (0.003)	-0.008# (0.004)
Attendance Rate (%)	-0.003 (0.017)	-0.018 (0.025)	0.024 (0.016)	-0.021 (0.023)	0.012 (0.017)	0.014 (0.023)	0.031** (0.010)	0.021 (0.016)	-0.005 (0.014)	-0.013 (0.023)	0.002 (0.006)	0.025* (0.010)
Bus Routes Within 1.5 Miles												
# Disciplinary Infractions	0.001 (0.001)	0.000 (0.002)	-0.001 (0.001)	0.000 (0.002)	0.000 (0.002)	0.003 (0.002)	0.000 (0.001)	0.006** (0.002)	0.000 (0.002)	-0.006* (0.003)	-0.003 (0.002)	-0.010** (0.004)
Attendance Rate (%)	-0.010 (0.015)	-0.035 (0.027)	0.002 (0.010)	-0.042** (0.016)	0.003 (0.013)	0.001 (0.027)	0.014# (0.008)	0.016 (0.012)	-0.008 (0.012)	-0.013 (0.028)	0.001 (0.006)	0.023* (0.010)
Bus Routes Within 2 Miles												
# Disciplinary Infractions	0.000 (0.001)	-0.002 (0.002)	-0.003* (0.001)	-0.004* (0.002)	0.000 (0.001)	0.004 (0.003)	0.002 (0.001)	0.007* (0.003)	0.000 (0.002)	-0.006* (0.003)	-0.002 (0.002)	-0.010** (0.003)
Attendance Rate (%)	-0.007 (0.013)	-0.035 (0.025)	0.005 (0.009)	-0.029* (0.012)	-0.005 (0.011)	-0.024 (0.025)	0.000 (0.006)	0.005 (0.010)	-0.011 (0.011)	-0.012 (0.024)	-0.004 (0.005)	0.017 (0.010)
Grade-Year Indicators	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Student Time-Variant Characteristics	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Census Tract of School Characteristics (Including Quartic in Population Density)	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
School Zip - Code Indicators	N	N	N	Y	N	N	N	Y	N	N	N	Y
Student Fixed-Effects	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y

Table 2.6B - Reduced Form Estimates of Passing State Criterion Referenced Exam on Number of Bus Routes Near School

	Pre-Charter (1993 - 1995)				Early Post-Charter (1996 - 1998)				Late Post-Charter (1999 - 2001)			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Bus Routes Within 1 Mile												
Passed Math Exam	0.0000 (0.0009)	0.0002 (0.0010)	-0.0007 (0.0016)	-0.0011 (0.0022)	0.0004 (0.0006)	-0.0002 (0.0006)	-0.0002 (0.0011)	-0.0007 (0.0018)	-0.0003 (0.0005)	-0.0004 (0.0007)	-0.0004 (0.0008)	-0.0006 (0.0014)
Passed Reading Exam	-0.0003 (0.0008)	0.0000 (0.0009)	-0.0008 (0.0015)	-0.0015 (0.0020)	-0.0001 (0.0006)	-0.0004 (0.0008)	0.0001 (0.0011)	-0.0003 (0.0019)	-0.0005 (0.0005)	-0.0011 (0.0007)	-0.0006 (0.0008)	-0.0015 (0.0016)
Bus Routes Within 1.5 Miles												
Passed Math Exam	0.0001 (0.0006)	0.0008 (0.0010)	-0.0003 (0.0010)	-0.0007 (0.0017)	0.0002 (0.0006)	0.0005 (0.0010)	-0.0002 (0.0008)	-0.0006 (0.0019)	-0.0003 (0.0003)	0.0001 (0.0007)	-0.0001 (0.0007)	-0.0001 (0.0012)
Passed Reading Exam	0.0000 (0.0005)	0.0008 (0.0009)	-0.0003 (0.0009)	-0.0001 (0.0014)	0.0001 (0.0004)	0.0005 (0.0008)	-0.0002 (0.0007)	-0.0006 (0.0014)	-0.0003 (0.0003)	0.0001 (0.0007)	-0.0003 (0.0007)	-0.0009 (0.0012)
Bus Routes Within 2 Miles												
Passed Math Exam	0.0006 (0.0007)	0.0011 (0.0009)	-0.0001 (0.0013)	-0.0005 (0.0017)	0.0004 (0.0004)	0.0005 (0.0008)	0.0001 (0.0007)	-0.0003 (0.0014)	-0.0001 (0.0003)	-0.0001 (0.0006)	-0.0001 (0.0007)	-0.0001 (0.0012)
Passed Reading Exam	0.0004 (0.0006)	0.0007 (0.0009)	-0.0002 (0.0011)	-0.0003 (0.0016)	0.0003 (0.0004)	0.0002 (0.0008)	0.0004 (0.0007)	0.0003 (0.0014)	-0.0002 (0.0003)	0.0000 (0.0006)	-0.0003 (0.0007)	-0.0008 (0.0012)
Grade-Year Indicators	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Student Time-Variant Characteristics	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Census Tract of School Characteristics (Including Quartic in Population Density)	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
School Zip - Code Indicators	N	N	N	Y	N	N	N	Y	N	N	N	Y
Student Fixed-Effects	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y

Student time-variant characteristics include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year indicators. For a complete list of census tract controls, please see appendix table 1. Huber/White standard errors clustered by school in parentheses. Regressions in table 7A contain over 500,000 observations for "pre-charter" and "early post-charter" and over 800,000 observations for "late post-charter". Regressions in table 7B contain over 250,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. ***, **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

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relationship between discipline or attendance and bus routes in the pre-charter or early post-charter periods, while only discipline is statistically significant in the late-charter period. With student fixed-effects the relationship is stronger, but once again this is mostly due to increased precision rather than changes in the point estimates. In addition, the relationship appears rather weak in the pre-charter period relative to the late charter period, though this could also be due to the fact that the bus route data is for 2006.

Unfortunately I cannot do the same exercise with test scores since schools did not start giving the exam until Fall, 1997. Instead, I am able to use data on whether or not a student passed a state criterion referenced exam that was given from 1993 - 2001. The examination was given in reading and math to all students in grades 3-8 and 10. The results from this analysis is provided in panel B. In no case does the number of bus routes have a statistically significant relationship with whether a student passes the exam. Thus, the evidence in Table 2.6 does suggest that bus routes are a valid instrument for charter penetration, provided that one accounts fully for economic conditions.

In light of this requirement, I control for a number of economic characteristics of each school's local neighborhood using census tract level data from the 2000 census. In addition to the covariates used in the regressions in Table 2.4, I also include a quartic in population density, along with the fractions of the population that are black, Hispanic, not native born, high school graduates without a college degree, college graduates, or receiving public assistance. I also include labor force participation for males aged sixteen and over, the log of household income, and indicator variables for the zip-code of the school. Descriptions of these variables can be found in Appendix Table 9. Thus the first stage is identified off of differences in the number of bus routes within zip codes.

Another problem with this choice of exclusion restriction is that, while the

first stage coefficients on bus routes are statistically significant in most cases, they are not very strong. Thus, in order to improve the strength of the instrument, I use only the combined measure of charter penetration for both district startups and non-district charters - i.e. non-conversion charters. For purposes of comparison, I also provide estimates from the non 2SLS regressions using this measure of charter competition.

Table 2.7 provides the results for this analysis. For discipline, the estimates with school fixed effects and school trends are positive, but generally statistically insignificant. On the other hand, the 2SLS results find a *negative* impact on discipline that is statistically significant at the 10% level for distances of one and two miles, and at the 5% level for distances of 1.5 miles. For attendance, regressions with school fixed effects are negative and statistically significant at the 5% level for distances of two miles, but when school specific trends are added, the attendance effect becomes statistically insignificant at all distances. The 2SLS results are also statistically insignificant, but positive. Thus, for discipline and attendance, the 2SLS results are suggestive, though by no means conclusive, of a bias towards estimating charters having undesirable impacts on non-charter schools in regressions with school fixed-effects. Adding school specific trends gets us closer, but does not seem to remove the bias entirely.

On the other hand, test score results tell a different story. Regressions using school fixed effects or school specific trends show the impacts to be statistically insignificant at the 10% level in most cases, and statistically insignificant at the 5% level in all cases. In addition, the point estimates are sometimes positive and sometimes negative. The 2SLS estimates, however, are consistently negative. While most of the estimates are not statistically significant, three out of nine measures are statistically significant at the 10% level and one is statistically significant at the 5% level. Thus, the 2SLS estimates are suggestive, although once again they are not

Table 2.7 - Estimates of Effect of Non-Conversion Charter Schools on Non-Charter Students with Student Fixed-Effects

Mesure of Charter Penetration: Share Within 1 Mile

	(1)	(2)	(3)	(4)	
Dependent Variable:	No Endogenous Location Corrections	School Fixed Effects	School Fixed-Effects & School Specific Time Trends	2SLS First Stage - Exclusion Restriction is # of Bus Routes within 1 Mile	2SLS Second Stage
# Disciplinary Infractions	-0.026# (0.016)	0.010 (0.012)	0.052*** (0.020)	0.0066*** (0.0018)	-0.858# (0.504)
Attendance Rate (%)	0.005 (0.147)	-0.074 (0.069)	-0.145 (0.105)	0.0066** (0.0018)	2.097 (1.703)
Δ Math NPR	-1.067*** (0.357)	-1.098# (0.568)	-0.197 (0.908)	0.0072*** (0.0017)	-2.794 (4.396)
Δ Reading NPR	-0.768* (0.381)	-0.561 (0.594)	-0.146 (0.770)	0.0072*** (0.0017)	2.478 (3.486)
Δ Language NPR	-0.299 (0.341)	-0.152 (0.501)	0.115 (0.762)	0.0072*** (0.0017)	-1.503 (2.927)

Mesure of Charter Penetration: Share Within 1.5 Miles

	(5)	(6)	(7)	(8)	
Dependent Variable:	No Endogenous Location Corrections	School Fixed Effects	School Fixed-Effects & School Specific Time Trends	2SLS First Stage - Exclusion Restriction is # of Bus Routes within 1.5 Mile	2SLS Second Stage
# Disciplinary Infractions	-0.032 (0.021)	0.002 (0.011)	0.008 (0.014)	0.0128*** (0.0022)	-0.438* (0.215)
Attendance Rate (%)	-0.242# (0.135)	-0.109# (0.062)	0.001 (0.071)	0.0128*** (0.0022)	0.960 (1.047)
Δ Math NPR	-0.023 (0.188)	0.154 (0.288)	0.354 (0.396)	0.0122*** (0.0028)	-1.295 (2.179)
Δ Reading NPR	-0.015 (0.152)	0.192 (0.237)	0.402 (0.289)	0.0122*** (0.0028)	-1.389 (1.702)
Δ Language NPR	-0.024 (0.215)	0.256 (0.370)	0.720 (0.477)	0.0122*** (0.0028)	-3.558* (1.434)

Mesure of Charter Penetration: Share Within 2 Miles

	(9)	(10)	(11)	(12)	
Dependent Variable:	No Endogenous Location Corrections	School Fixed Effects	School Fixed-Effects & School Specific Time Trends	2SLS First Stage - Exclusion Restriction is # of Bus Routes within 2 Mile	2SLS Second Stage
# Disciplinary Infractions	0.011 (0.022)	0.008 (0.010)	0.006 (0.016)	0.0077*** (0.0027)	-0.710# (0.394)
Attendance Rate (%)	-0.255** (0.088)	-0.123* (0.050)	-0.012 (0.061)	0.0077*** (0.0027)	0.421 (1.558)
Δ Math NPR	-0.038 (0.136)	0.241 (0.227)	0.155 (0.294)	0.0075* (0.0033)	-1.384 (2.653)
Δ Reading NPR	0.001 (0.102)	0.171 (0.151)	0.072 (0.294)	0.0075* (0.0033)	-5.590# (3.332)
Δ Language NPR	-0.096 (0.111)	0.281 (0.204)	0.496# (0.259)	0.0075* (0.0033)	-4.138# (2.411)

All regressions include quartics in population density of school's Census tract, controls for Census tract economic conditions, student fixed effects, zip-code of school indicators, and time-variant individual controls. Student time-variant characteristics include free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year indicators. For a complete list of census tract controls, please see Appendix Table 1. Huber/White standard errors clustered by school in parentheses. Behavior and attendance regressions contain over 1,200,000 observations. Test score regressions contain over 400,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. ***, **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

conclusive, of a upwards bias in impact estimates of charter schools on test scores in non-charter schools. Hence, while I cannot conclude from this evidence that analyses that rely on school fixed-effects or school specific trends are incorrect, these results raise substantial questions about their validity and suggest that, in a worst case scenario, previous research has over-estimated the charter impact on test scores.

2.7   Accounting for Changes in Peer Composition

The strategies used in the previous section do not account for changes in peer composition. If charters change the characteristics of a student's peers, then this in turn could affect a student's own outcomes through the peer-effects mechanism. In order to address this issue, I account for peer composition by adding controls for quartics in the average of twice-lagged peer course grades and disciplinary actions along with a quartic in their interaction.[6] If I use current peer grades and discipline, charter competition effects will also improve outcomes for peers. Thus, accounting for peer composition will bias the charter competition effects towards zero. Thus, in order to reduce the extent of this "contamination" the peer variables are calculated from other students twice lagged characteristics, when they were likely to be in other schools and at a time when fewer charter schools existed in ALUSD.

Table 2.8 provides the results for these regressions. Since I include lagged peer characteristics, the sample changes to drop the years 1993 and 1994 and grades one and two. Thus, in order to make a reasonable comparison, the first panel repeats the regressions from table 5 and 6 with the new sample. In each case there is very little change in the estimates from adding the controls for peer characteristics. Thus, I find little evidence that changes in peer composition affects the estimates of charter school impacts on non-charter students. However, this is, admittedly, a low power

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[6] I use course grades instead of test scores since grades are available for all years back to 1993. However, course grades in 1994 are only available for grades 1 - 5

Table 2.8 - Accounting for Changes in Peer Characteristics in Estimates of Charter Impacts on Non-Charter Students

A. Explanatory Variable: Enrollment Share in Non-Conversion (%)

	School Fixed Effects		School Fixed-Effects & School Specific Time Trends	
	No Peer Characteristics Controls	Controls for Quartics in Peers' Mean Twice-Lagged Discipline, Grades, and Interaction	No Peer Characteristics Controls	Controls for Quartics in Peers' Mean Twice-Lagged Discipline, Grades, and Interaction
i. Measure of Charter Penetration: Share Within 1 Miles				
Dependent Variable:				
# Disciplinary Infractions	-0.003	-0.008	0.055	0.050
	(0.020)	(0.019)	(0.036)	(0.036)
Attendence Rate (%)	-0.299**	-0.300**	-0.243	-0.236
	(0.095)	(0.093)	(0.171)	(0.167)
Δ Math NPR	-0.652	-0.681	-0.393	-0.414
	(0.976)	(0.975)	(1.168)	(1.162)
Δ Reading NPR	-0.142	-0.106	-0.256	-0.303
	(0.770)	(0.769)	(1.009)	(1.036)
Δ Language NPR	-0.243	-0.259	-0.788	-0.784
	(0.684)	(0.686)	(0.849)	(0.859)
ii. Measure of Charter Penetration: Share Within 1.5 Miles				
Dependent Variable:				
# Disciplinary Infractions	-0.004	-0.004	0.004	0.003
	(0.023)	(0.022)	(0.023)	(0.022)
Attendence Rate (%)	-0.224*	-0.217*	-0.048	-0.043
	(0.095)	(0.096)	(0.118)	(0.122)
Δ Math NPR	0.315	0.259	0.133	0.094
	(0.363)	(0.373)	(0.373)	(0.382)
Δ Reading NPR	0.303	0.281	0.146	0.149
	(0.251)	(0.257)	(0.266)	(0.272)
Δ Language NPR	0.173	0.160	0.188	0.187
	(0.325)	(0.329)	(0.349)	(0.353)
iii. Measure of Charter Penetration: Share Within 2 Miles				
Dependent Variable:				
# Disciplinary Infractions	-0.001	0.000	0.008	0.007
	(0.015)	(0.015)	(0.021)	(0.021)
Attendence Rate (%)	-0.176**	-0.172**	-0.054	-0.052
	(0.058)	(0.057)	(0.082)	(0.085)
Δ Math NPR	0.484#	0.464#	0.085	0.073
	(0.270)	(0.273)	(0.304)	(0.311)
Δ Reading NPR	0.181	0.168	-0.100	-0.105
	(0.155)	(0.159)	(0.225)	(0.228)
Δ Language NPR	0.154	0.133	0.172	0.165
	(0.214)	(0.211)	(0.244)	(0.246)

B. Explanatory Variable: Enrollment Share in District Startup (%)

	School Fixed Effects		School Fixed-Effects & School Specific Time Trends	
	No Peer Characteristics Controls	Controls for Quartics in Peers' Mean Twice-Lagged Discipline, Grades, and Interaction	No Peer Characteristics Controls	Controls for Quartics in Peers' Mean Twice-Lagged Discipline, Grades, and Interaction
i. Measure of Charter Penetration: Share Within 1 Miles				
Dependent Variable:				
# Disciplinary Infractions	0.099#	0.077	-0.061	-0.090
	(0.058)	(0.057)	(0.095)	(0.097)
Attendence Rate (%)	-0.162	-0.194	0.505	0.495
	(0.439)	(0.447)	(0.587)	(0.582)
Δ Math NPR	1.380	1.349	0.902	0.861
	(1.703)	(1.776)	(1.585)	(1.627)
Δ Reading NPR	0.920	0.961	0.597	0.724
	(0.904)	(0.939)	(0.829)	(0.846)
Δ Language NPR	-0.221	-0.206	-0.339	-0.194
	(0.937)	(0.921)	(0.736)	(0.727)
ii. Measure of Charter Penetration: Share Within 1.5 Miles				
Dependent Variable:				
# Disciplinary Infractions	0.077#	0.069	-0.020	-0.034
	(0.044)	(0.042)	(0.060)	(0.064)
Attendence Rate (%)	-0.080	-0.070	0.529#	0.536#
	(0.196)	(0.190)	(0.315)	(0.315)
Δ Math NPR	0.299	0.229	0.098	-0.029
	(1.614)	(1.680)	(1.631)	(1.688)
Δ Reading NPR	0.146	0.068	-0.174	-0.170
	(0.873)	(0.938)	(0.870)	(0.936)
Δ Language NPR	-0.107	-0.242	-0.420	-0.397
	(1.050)	(1.034)	(0.951)	(0.927)
iii. Measure of Charter Penetration: Share Within 2 Miles				
Dependent Variable:				
# Disciplinary Infractions	0.190**	0.185**	0.091#	0.077
	(0.072)	(0.071)	(0.050)	(0.047)
Attendence Rate (%)	-0.356**	-0.360**	0.197	0.196
	(0.133)	(0.136)	(0.361)	(0.361)
Δ Math NPR	-1.539	-1.539	-4.018**	-4.074**
	(1.246)	(1.269)	(1.053)	(1.050)
Δ Reading NPR	-0.434	-0.517	-1.862**	-1.848*
	(0.667)	(0.686)	(0.711)	(0.719)
Δ Language NPR	-0.662	-0.811	-0.455	-0.482
	(0.805)	(0.800)	(0.907)	(0.895)

	School Fixed Effects		School Fixed-Effects & School Specific Time Trends	
	No Peer Characteristics Controls	Controls for Quartics in Peers' Mean Twice-Lagged Discipline, Grades, and Interaction	No Peer Characteristics Controls	Controls for Quartics in Peers' Mean Twice-Lagged Discipline, Grades, and Interaction
i. Measure of Charter Penetration: Share Within 1 Miles				
Dependent Variable:				
# Disciplinary Infractions	-0.024	-0.026	0.068	0.067
	(0.028)	(0.029)	(0.046)	(0.046)
Attendence Rate (%)	-0.325*	-0.318*	-0.324#	-0.315#
	(0.156)	(0.156)	(0.165)	(0.166)
Δ Math NPR	-2.648#	-2.693#	-1.766	-1.853
	(1.599)	(1.561)	(1.953)	(2.020)
Δ Reading NPR	-1.486	-1.450	-2.567	-3.022
	(1.268)	(1.234)	(1.874)	(1.927)
Δ Language NPR	-1.005	-1.064	-2.558	-2.835
	(1.267)	(1.263)	(1.929)	(1.951)
ii. Measure of Charter Penetration: Share Within 1.5 Miles				
Dependent Variable:				
# Disciplinary Infractions	-0.074	-0.073	-0.004	-0.004
	(0.050)	(0.049)	(0.054)	(0.051)
Attendence Rate (%)	-0.321#	-0.311#	-0.171	-0.165
	(0.170)	(0.173)	(0.197)	(0.201)
Δ Math NPR	0.983	0.917	0.516	0.544
	(0.613)	(0.606)	(0.721)	(0.726)
Δ Reading NPR	0.913#	0.908*	0.536	0.542
	(0.473)	(0.454)	(0.561)	(0.561)
Δ Language NPR	0.626	0.656	0.789	0.797
	(0.560)	(0.544)	(0.540)	(0.531)
iii. Measure of Charter Penetration: Share Within 2 Miles				
Dependent Variable:				
# Disciplinary Infractions	-0.043#	-0.040#	0.000	0.001
	(0.022)	(0.023)	(0.038)	(0.038)
Attendence Rate (%)	-0.158	-0.152	-0.071	-0.070
	(0.099)	(0.098)	(0.116)	(0.117)
Δ Math NPR	0.873**	0.867**	0.449	0.478
	(0.313)	(0.308)	(0.427)	(0.420)
Δ Reading NPR	0.290	0.280	-0.095	-0.106
	(0.201)	(0.204)	(0.343)	(0.351)
Δ Language NPR	0.406	0.401	0.350	0.346
	(0.323)	(0.309)	(0.350)	(0.341)

All regressions include quartics in population density of school's Census tract, controls for Census tract economic conditions, student fixed effects, zip-code of school indicators, free or reduced price lunch status, other economic disadvantages, recent immigration status, parents' migrant status, and grade*year dummies For a complete list of controls see the appendix. Huber/White standard errors clustered by school in parentheses. Behavior and attendence regressions contain over 1,200,000 observations. Test score regressions contain over 400,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

test, so we must interpret these results cautiously.

2.8 Conclusion

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

the contamination of competition impacts with changes in peer composition by controlling for twice lagged average peer discipline and grades. Fourth, I look at whether there are different impacts of charter schools based on whether they are conversions, schools that were originally traditional public schools but convert to charter status, or startups, schools that begin as charters and by whether the charter is granted by the local school district or some other government entity.

Estimates using school-fixed effects to correct for endogenous location of charter schools show a mixed picture. While most estimates are statistically insignificant, discipline seems to improve in startup charters while for non-district charters test score impacts of charter penetration within one mile are statistically significantly negative, but within two miles they are statistically significantly positive. However, these results become statistically insignificant when school specific trends are added with only one exception. Indeed, when school specific trends are added, overall the results suggest there is little impact of charter schools on non-charter students. In addition, some of the estimates change considerably when trends are added. This highlights the possibility that school fixed-effects are insufficient corrections for endogenous charter location.

Instrumental variable estimates using the number of bus routes nearby as an instrument for charter penetration provide further evidence that school fixed-effects estimates are insufficient. While these estimates are not conclusive due to large standard errors, they suggest that using school fixed effects or school fixed effects combined with trends underestimate the charter schools effects on discipline and overestimate the charter schools' effects on test scores.

Finally, I find that controlling for lagged peer effects has little effect on the estimates using school fixed-effects or school fixed-effects combined with school specific trends. However, these tests are low power, and thus, at best, they are suggestive of there being only a small role for peer composition changes.

3. ARE THERE RETURNS TO ATTENDING A PRIVATE COLLEGE?

3.1 Introduction

The average net cost (tuition, fees, room, and board minus grants) of attending an in-state public four-year college or university was \$5,695 in 1999 (in \$2004). By comparison, attending a private four-year college or university cost an average of \$15,020. Over a 4-year college career from 1999 – 2003 the average private college student paid \$38,817 more than his or her public counterpart.[1] In addition, this gap has widened over the past 40 years. Figure 3.1 shows that gross tuition and fees have increased by nearly \$16,844 on average for private universities since 1964. In contrast, in-state tuition and fees at public universities only increased by \$3,695 on average. With these substantial cost differences one would expect there to be significant benefits to attending a private college rather than a public college. However, previous research has been unclear as what effect attending a private college has on wage and education outcomes.

Understanding whether there are benefits to attending a private college has substantial policy implications. Large deficits are putting pressure on the Federal government to cut funding for financial aid. In fact, the 2007 President's Budget proposed cutting Pell Grants by 27%. In the expectation that less Federal spending for higher education will be forthcoming, state governments will need to distribute

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[1] National Postsecondary Student Aid Study (NPSAS), 2000 Digest of Education Statistics, 2003 - US Department of Education

Figure 3.1 - Tuition and Fees for 4 - Year Colleges and Universities (In-State, Full-Time)

Tuition and Fees ($2004)

Year
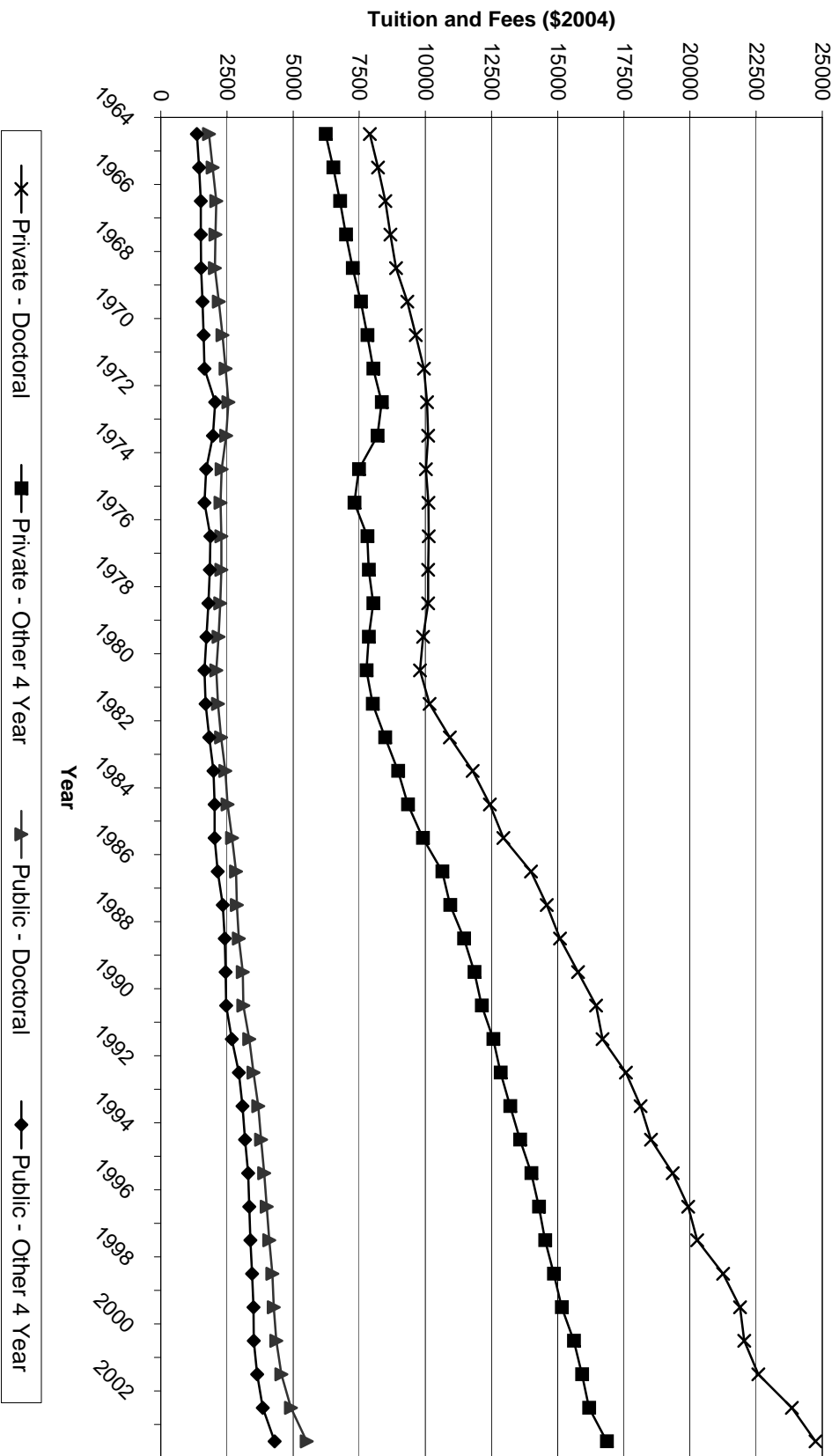
Source: Digest of Education Statistics, 2004

Private - Doctoral
Private - Other 4 Year
Public - Doctoral
Public - Other 4 Year

their education funding more efficiently. One potential avenue to improve efficiency would be to divert funds from public colleges to financial aid programs. If this occurs, then more students would likely attend private colleges (Long 2004). This can be efficiency enhancing if attending a private college provides a higher return, in terms of the student's labor market outcomes, per dollar of government funds spent.

On the surface, there is considerable evidence that a substantial private premium exists. First of all, private colleges are generally ranked higher than their public counterparts in commonly used lists. For example, of national universities ranked 50 or higher by US News and World Report's 2006 rankings, only 13 are public, despite public universities in this group outnumbering private by 2 to 1. In addition, most easily observable statistics, such as average SAT scores and faculty-student ratios, consistently show private schools performing better than public schools. Using data from US News & World Report Directory of Colleges, 1991, Table 3.1 shows how public & private colleges compare on a set of 9 common college quality measures.

Table 3.1 - Average College Quality for Public & Private Four-Year Schools

Measure	Public	Private
Avg SAT Score	975	1027
% of Students in Top 10% of High School Class	26%	33%
% of Students in Top 25% of High School Class	53%	58%
% of Faculty with PhD	78%	72%
Graduation Rate (%)	42%	59%
Freshman Retention Rate (%)	74%	80%
Rejection Rate (%)	27%	29%
Per-Student Expenditures	$11,213	$15,435
Faculty-Student Ratio	0.060	0.080

Source: *US News and World Report Directory of Colleges and Universities, 1991.* Weighted by full-time enrollment + 2/3*part-time enrollment.

Public colleges are higher in only 1 of the categories, percent of faculty with PhD's.

These measures and rankings are misleading, however, when investigating

the quality of a private college education. The problem is that they depend on the quality of schools and the quality of the students in those schools. For example, consider graduation rates. Table 3.1 shows that only 42% of public school students eventually graduate while 59% of private students do. At the same time, however, the average SAT score of public school students is 52 points lower than for private school students. Thus, while the lower graduation rate suggests poorer performance on the part of the public college, most or all of that difference could be due to lower ability students enrolling in public schools.

Thus, the positive selection of higher ability and/or more motivated students into private schools makes finding an accurate measure of relative quality difficult. Research into this topic at the primary and secondary level has used an instrumental variables approach to correct for selection utilizing religion as an instrument for attending a Catholic school (Grogger and Neal 2000, Neal 1997, Evans and Schwab 1995). The concept of comparing public to private non-profit firms has also been extensively researched in the health care sector where the private for-profit, private non-profit, and public sector all play substantial roles in hospital ownership (Shen, Eggleston, Lau and Schmid 2005) with some use of natural experiments to identify the differences (Duggan 2000).

Unfortunately, a lack of natural experiments or feasible instruments means that neither of these strategies will work well for higher education. In this chapter, I take a different approach to addressing selection. Rather than modeling the selection process, I assume that the selection is positive in the sense that higher ability and more motivated students will select into private colleges. This is essentially a "selection on observables" analysis, but rather than assuming that selection is based solely on observables, I assume that the selection on unobservables (primarily selection based on unobserved ability and motivation) is positive. Later in this chapter, I provide some evidence that suggests this is a reasonable assumption. Thus, I try to "knock

out" the observed return premium using a rich set of covariates. Any return left over can be considered an upper bound estimate. This procedure is similar to that used by Fryer and Levitt (2004) when they look at the black-white test-score gap in early elementary education.

Therefore, my contribution is to use very rich data from two different datasets, the National Longitudinal Survey of Youth 1979 (NLSY) and the High School and Beyond Survey (HSB), In so doing, I estimate an upper bound on the average returns to attending a private college making use of some unique variables that can help proxy for motivation and other additional controls not previously used in this context. In addition, I focus on differences by gender. Previous research on this topic has not done despite evidence suggesting that the returns to education for women are substantially different than those for men (Dougherty, 2005).

I find that for males, the upper bound on the wage returns is small and statistically insignificant between ages 24-29 but statistically significant at around 11% by the time the student reaches ages 36-41. On the other hand, for females I do not find any statistically significant wage returns. In both cases, however, I cannot eliminate the possibility that attending a private school enhances future education outcomes, particularly the likelihood of obtaining a bachelor degree. These estimates are quite large, particularly for men where a private student is 13.5 percentage points more likely to obtain a bachelor degree than his public counterpart, off of a baseline of 56%. For females the estimate is 9 percentage points off of a 54% baseline. Even though this is an upper bound, the size of the estimate suggests that there could very well be substantial non-pecuniary benefits to attending private colleges. I also show that there may be other benefits to attending a private college, such as lower divorce rates for women.

3.2 Background

3.2.1 Using Prices as a Measure of Quality

Theoretically, it is unclear whether a private or a public college would provide a higher quality education. In a standard market we would expect the large price difference to reflect the provision of a higher quality good. However, the higher education market is far from a standard market. One major complication is that both supply-side and demand-side subsidies play a large role in this market. In FY2002 state and local governments spent $157 billion on higher education with the federal government providing another $17 billion, though some of the federal funding is already included in the state and local figure through intergovernmental grants.[2] These subsidies could drive a wedge between the marginal cost of providing a higher quality education and the price paid by students and their families. The subsidies take a number of forms including direct aid from state governments to public institutions, financial aid for low-income students, and tax-preferences for non-profit public and private institutions (which enroll 96% of students in four-year institutions.[3]) Thus the prices may be as much a reflection of how much government support institutions receive as they are indicators of quality.

Another major complication in the higher education market that makes it difficult to use prices as a measure of quality is that as a student selects the school he or she wants to purchase the education from, the school also selects the student it wants to provide education to. Thus, not only do students need to agree to the price and quality of the education the school is providing before they purchase, but the school also has to approve of the quality of the students purchasing it. Part of the reason for this is that students are both consumers of education and inputs in

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[2] US Census Bureau - Census of Governments, 2002; Statistical Abstract of the United States, 2004-2005.

[3] Digest of Education Statistics, 2004

the educational process through mechanisms like peer effects (Rothschild and White 1995). The schools thus have an incentive to reduce prices overall to attract high quality students or they may charge lower prices to high quality students specifically through adjustment of institutional financial aid packages and through provision of scholarships (Singell 2002).

3.2.2 Prior Literature

Researchers have spent considerable effort trying to assess whether private primary and secondary schools provide better education than public schools. A few papers have tried to use religion as an instrument for attending a private school, particularly Catholic schools (Neal 1997, Evans and Schwab 1995). In both cases, the authors find improvements in educational outcomes from attending Catholic schools. Altonji, Elder and Taber (2005a) also find positive returns to attending Catholic schools using a strategy that utilizes the amount of selection on observable characteristics as a guide for approximating the amount of selection on unobservables. Another strategy used in primary & secondary education is the natural experiment approach. Using this method, Rouse (1998) finds that students in Milwaukee, Wisconsin who were provided vouchers to attend selected private schools performed better on math exams, though no different on reading exams, than students randomly denied the vouchers.

Unfortunately, in the higher education literature, natural experiments and instruments are hard to come by. It is for this reason that, most of the college quality literature has relied on selection on observables techniques to study whether attending a college of higher observable quality increases earnings. Loury and Garman (1995) and Black, Daniel and Smith (2005) control for large sets of observable characteristics and run OLS analyses of the relationship between college quality and wages. Both of these papers find that higher quality colleges increase wages. While making important

contributions to the literature, their results may overestimate the returns to attending a higher quality college since residual unobserved ability and motivation bias would likely bias the results upwards.

Another more potentially problematic issue is the lack of common support across students who attend high and low quality colleges. The concern is that students who attend high quality schools may be so fundamentally different from those who attend low quality schools that any analysis that does not directly focus on similar students will be biased. Dale and Krueger (2002) notice this problem and attempt to correct for it by grouping students based on the types of schools they apply to and are accepted into and then including group fixed-effects. This ensures that each student is only compared to students who apply to similar sets of schools. This strategy has the additional benefit that, under the assumption that students who apply to and are accepted into similar schools are similar along unobservable dimensions, then it will also correct for selection on unobservables. They find that students who attend higher quality colleges measured by average SAT scores garner little to no wage benefit. However, Black and Smith (2004) use propensity score matching as an alternative correction for common support problems. They find that students who attend high quality colleges garner higher wages than those who attend low quality colleges.

Only a handful of papers have addressed the returns to attending a private college specifically. The most important paper in this line of research is Brewer, Eide, and Ehrenberg (1999; hereafter BEE). They separate colleges into 6 groups based on ratings in *Barron's Profiles of American Colleges*: Private top, middle, and bottom and public top, middle, and bottom. Using data from the National Longitudinal Study of the High School Class of 1972 (NLS72) and the High School and Beyond (HSB) study they find that attending a top private college increases wages and earnings over a bottom public college. While they provide a significant

contribution to the college quality literature, the paper potentially suffers from the common support problem, since Black and Smith (2004) show that few high ability students attend low quality schools and even fewer low ability students attend elite schools. This lack of common support means that papers like BEE which rely on parametric functional forms with only a few covariates may still suffer from bias even if they do sufficiently correct for selection on unobservables. In light of this problem, Dale and Krueger (2002) use the same data as BEE and find that by simply controlling for the SAT scores of schools the students applied to and the number of schools the students apply to, then OLS estimates of a return to attending an elite private college are essentially reduced to zero. While this is not directly comparable to BEE since they use a Heckman selection model, it does highlight potential problems with BEE's analysis.

Other papers that have studied the returns to attending a private college include Bowman and Mehay (2002) who use a similar method to BEE to look at job performance and promotions of naval officers, Eide, Brewer, and Ehrenberg (1998) who look at the impact of college quality on graduate school attendance, and Brewer and Ehrenberg (1996) who use the same strategy as BEE but only looking at the 1986 followup of the HSB Seniors cohort. All three papers find positive impacts of attending an elite private college. Behrman, Rosenzweig, and Taubman (1996) also consider an indicator for attending a private college when looking at the returns to different college qualities between female twins. Their estimate is strongly positive.

3.2.3   Selection

As was discussed in the literature review, there is a concern in the college quality literature that there is substantial selection of students into schools of differing quality based on ability and motivation. These concerns remain when considering the choice of whether to attend a public or private college. For example, it is possible that

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students (and parents) who take a more active interest in their (children's) education may be willing to pay more for college, thus they would be more likely to attend private schools. The ideal way to solve the ability selection problem would be to find an instrument that is correlated with attending a private college but uncorrelated with other factors that are related to the outcome variables of interest.

Unfortunately, it is very difficult to find a feasible instrument for individuals' educational decisions. Researchers who have studied the returns to education have made some attempts to address ability and motivation bias with instrumental variables regression and the related bivariate probit model, which relies on some stricter assumptions. Evans and Schwab (1995) is one of the most influential papers to use this approach in education economics. They estimate the returns to attending Catholic school using a students' religion as an instrument. Neal (1997) conducts a similar analysis with bivariate probit models where the main instruments (exclusion restrictions) are based on the supply of Catholic schools near the students' residences. However, Grogger and Neal (2000) later find this instrument to be invalid and Altonji, Elder, and Taber (2005a, b) show that both of these instruments are likely correlated with unobservables.

In the returns to higher education literature BEE try to correct for selection while estimating the returns to attending an elite private college by predicting the type of college a student is likely to attend through a multinomial logit model based on exogenous characteristics and then including a selection correction term in the reduced form equations. However, as stated before, Dale and Krueger (2002) find results using an alternative selection correction strategy that contradict BEE's. This raises the possibility that there may be problems with BEE's choices of exclusion restrictions.

In the context of this chapter, one potential exclusion restriction is to use relative supply of public and private colleges near student residences. This is similar

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to the strategy used by Card (1995) and Kling (2001) to estimate the returns to a college education. For the students in one of my datasets, I calculated multiple measures of relative supply and prices of private and public colleges at various distances from the county of the students' high schools.[4] In all cases, these supply conditions were highly correlated with local labor market conditions both near the student's high school and eventual college. Since most students tend to reside post-college near their pre-college residences, these supply and price variables are likely invalid as instruments.

Thus, in this chapter, I am unable to fully account for unobserved ability bias. However, this does not mean that we cannot garner some accurate information from the estimates. First of all, I make use of very detailed data with unique variables. No work has been done on this question previously that takes into account as many potentially important control variables. One particularly useful variable is the number of extracurricular activities or clubs the students in which the students participated during high school. Arguably more motivated students would participate in more clubs, thus I use this variable as a proxy for motivation. I also include a number of high school quality variables and measures of the quality of the student's family life as a child.

Of course, even with all of these additional covariates we may still be left with residual bias. However, there is a prior belief that unobserved ability and motivation would be positively correlated with attending a private college if such a correlation exists. Thus, if my estimates suggest that there are no returns to attending a private college then those results would likely remain even if I fully correct for ability bias. Therefore, we could view the estimates provided in this chapter as upper bound

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[4] I used the High School and Beyond Sophomore Cohort restricted access dataset to conduct this analysis. While the counties are not identified in the dataset, it does provide some information for each county from the US Census Bureau's County Statistics File 1. With the permission of the Department of Education, I was able to identify each of the students' counties by matching to the Census bureau data with at 100% match rate.

estimates on the returns to attending a private college.

As with the other research, common support is also a problem in this analysis. To deal with this, I use a highly flexible functional form for my OLS analyses. This would allow parametric modeling to better account for differences between students who attend public and private schools. In addition, since my focus here is on whether or not a student attends any private college rather than comparing high quality private colleges to low quality public colleges, there is much more overlap of student characteristics in the treated and untreated groups. Thus, common support problems are likely to be less of an issue in this analysis than in previous papers.

3.3 Model and Data

The basic model I will use for wages and earnings in this chapter is as follows:

$$Y_i = \alpha + \beta Private_i + \mathbf{\Gamma X_i} + \epsilon_i \tag{3.1}$$

Y is an outcome variable, either log of hourly wage, log of annual earnings, or years of school completed, Private is an indicator for whether or not a student's main school is a private school, and X is a vector of individual, high school, and family characteristics. In the NLSY I define a student's "main school" as the undergraduate four-year school where the student received the most credit hours before completing grade 16, regardless of from where he or she received any degrees. For HSB, because the transcript data is very poor, I define the "main school" as the four-year school where the student spent the most months working towards a bachelor degree. Rather than assume that ϵ_i is i.i.d. with mean 0, I assume that it is a composite term as such:

$$\epsilon_i = \gamma Z_i + \nu_i \tag{3.2}$$

where Z is unobserved ability and motivation and ν is i.i.d. error with mean 0. In this case, my estimate of β, $\hat{\beta}$, will have the following relationship with β:

$$plim\hat{\beta} = \beta + \gamma \frac{cov(Private, Z)}{var(Private)}. \qquad (3.3)$$

In this situation, $\hat{\beta}$ will be inconsistent and biased, but the direction of the bias will be positive provided that $cov(Private, Z) > 0$ and $\gamma > 0$. While it seems reasonable to assume that ability and motivation are positively related with wages and academic success, it is not as clear that high ability and more motivated people will sort into private schools, giving us a positive covariance, even after controlling for a rich set of covariates. Later on, I will provide evidence using observed variables that suggests this is an accurate assumption. For other outcomes that are binary – labor force participation, schooling outcomes, and employment – I use the probit equivalent of the above, rather than a linear probability model.

The data for this chapter comes from two sources. The primary source is the Geocoded National Longitudinal Survey of Youth 1979. This is a restricted access, nationally representative survey of all persons aged 14-22 in 1979. After an initial interview in 1979, the survey follows people every year until 1994 and every 2 years thereafter through 2002. This survey is very useful because it has different types of post-treatment outcomes over many years, has a large set of pre-treatment observable characteristics, and it has college identifiers that allow me to match students to the colleges they attend.

The initial survey contains 12,686 observations. However, since I am looking at a specific subset of the NLSY population, that number falls considerably to 4,595 when I restrict to people who ever attended a four-year college. Unfortunately, the NLSY suffers from a large amount of missing data. For most variables, rather than reduce the sample further, I include a "missing" dummy in my regressions.

Nonetheless, for some variables there was no other choice than to reduce the sample if they were missing. These sample restrictions are described in Appendix Table 10. The final sample includes 3,819 observations, with 1,792 males and 2,027 females.

A description of all variables used in the NLSY main regressions can also be found in the appendix. Nevertheless, there are several variables that warrant extra attention, especially those that serve as measures of student ability and motivation. The key motivation variable is the number of clubs the student participated in during high school. Arguably, a student who participates in more clubs in high school cares more about his or her education. The main student ability variable is his or her performance on the Armed Services Vocational Aptitude Battery. This is a test given by the US Armed Forces to new recruits. As part of a renorming process, the military gave the test to NLSY participants. The battery includes questions on 10 subjects. Rather than include each score and interactions in the regressions, I follow Cawley, Conneely, Heckman and Vytlacyl (1996); Cawley, Heckman and Vytlacil (2001); Black and Smith (2004); and Black, Daniel and Smith (2005) in using principal component analysis to collapse the age-adjusted ASVAB scores into two linearly independent measures, called principal components. In addition to the ASVAB, the NLSY also has some information on the quality of the student's high school. This can provide additional measures of student ability and/or motivation.

The second dataset I utilize is the High School and Beyond Sophomore Cohort. This is a nationally representative study of 14,825 students who were sophomores in high school in 1980 and then re-interviews them in 1982, 1984, 1986, and 1992. As with the NLSY, I first cut the sample to students who attend a four year college or university. In this case, however, to ensure that there is a post-college outcome in 1992, I limit to only those students who had attended by 1986. This leaves a base sample of 4,237. Also, like the NLSY, the HSB suffers from a substantial amount of missing data and thus I use the same procedures as described above.

The final sample contains 3,526 people, including 1,669 males and 1,857 females. A detailed description of the sample cut is provided in Appendix Table 10.

While this dataset does not provide me with as much post-treatment information as the NLSY (it does not provide any information on outcomes later than 10 years after high school) it has some different covariates that could potentially reduce selection further. First of all, not only does it have the number of clubs the student participates in, but it also has the number of clubs a student had a leadership position in. It also has data on how often the student reads for pleasure and how often the student completed his or her homework in high school. For ability measures, the HSB survey also has a test battery specifically created for the survey. Students were given the exam in 1980 and again in 1982. In this case, the measure I use is the student's average percentile on the exam over those two years. The HSB also provides the students' self reported average grades in high school.

I should also note that in analyses based on both of these datasets, I make use of many more observable variables than BEE, despite their use of the HSB as one of their sources. They include only race, gender, family size, family income, parents' education, test scores, and indicators for whether the individual holds a part-time job and whether he or she is still enrolled in school. Amongst other items, I also include the motivation measures described above, region of residence, family structure, high school quality measures, whether the student attends a public or private high school, and the student's intended major in college in the HSB analysis. Except for the public/private high school and intended major variables (these are not available) I include the variables just described in the NLSY analysis along with variables on whether someone in the student's family had a library card, magazine subscription, or a newspaper subscription when the student was 14, whether the student ever knew his or her mother or father, and the parents' ages. These additional variables will help control for other factors that influence future earnings and college completion.

3.4 Main Results

Tables 3.2A and 3.2B provide summary statistics for various outcome measures by gender from the NLSY and the HSB, respectively. Both the log wage and log earnings measures are averages over all years when wages or earnings were reported and the person's age falls in the limits provided in the table. They are also adjusted for inflation to $2002. Also note that the means are weighted using customized weights that take into account the construction of these samples. A description of the weighting procedures used in this chapter is provided in the appendix.

For males, there are substantial differences in many outcomes between public and private school attendees. Private males have higher wages in all age groups, higher earnings in all age groups, are more likely to complete their bachelor degree, attain graduate degrees, and attend post-bachelor classes. In addition, they complete more years of schooling. However, there is very little difference for labor force participation, unemployment, and enrollment. For women, the differences are generally in the same direction but the wage, earnings, and schooling differences are smaller than for men. Women also seem more likely to differ in LFP at young ages and in school enrollment at age 27 and 36/37.

Unlike the NLSY, the HSB only provides outcomes up to 11 years after the sophomore year of high school. Students who are age 16 at the end of their sophomore year, will thus be age 27 when earnings are observed. This would roughly correspond to the 24 – 29 age group in the NLSY. With this in mind, the means for the HSB data differ somewhat from the NLSY data. Earnings differences for men are essentially 0 while private men in the HSB are less likely to be employed or in the labor force. However, the differences for schooling outcomes and earnings for women are broadly consistent with the NLSY data. For both genders there are positive differentials in the amount of education completed, but men have larger differences. The women's earnings differential is 12 log points, similar to the 10 log point difference for NLSY at

Table 3.2A - NLSY Summary Statistics for Outcome Variables

Outcome	Males					Females				
	Public	Private	Diff	T-Stat	N	Public	Private	Diff	T-Stat	N
Log Wages By Age										
24-29	2.66	2.76	0.096	2.56	1687	2.47	2.58	0.103	2.42	1928
30-35	2.90	3.03	0.124	2.50	1480	2.63	2.73	0.100	2.10	1700
36-41	3.07	3.26	0.183	3.93	1257	2.77	2.83	0.068	1.24	1386
Log Earnings by Age										
24-29	10.10	10.20	0.097	2.06	1694	9.81	9.91	0.101	1.68	1922
30-35	10.57	10.80	0.230	2.51	1472	9.96	10.01	0.057	0.61	1684
36-41	10.81	11.03	0.221	3.85	1237	10.14	10.16	0.013	0.15	1341
Out of Labor Force by Age										
24	0.11	0.15	0.045	1.81	1735	0.15	0.09	-0.065	-3.44	1962
27	0.05	0.08	0.025	1.49	1589	0.15	0.15	-0.009	-0.45	1844
30/31	0.04	0.02	-0.011	-1.00	1490	0.19	0.19	0.003	0.12	1743
33/34	0.04	0.03	-0.004	-0.29	1358	0.18	0.22	0.033	1.10	1594
36/37	0.04	0.02	-0.020	-1.43	901	0.20	0.21	0.010	0.26	1058
Unemployed by Age										
24	0.06	0.04	-0.022	-1.47	1735	0.06	0.05	-0.002	-0.18	1962
27	0.03	0.04	0.010	0.91	1589	0.03	0.03	0.000	-0.03	1844
30/31	0.03	0.02	-0.015	-1.69	1490	0.03	0.03	0.002	0.17	1743
33/34	0.03	0.01	-0.015	-2.26	1358	0.03	0.03	0.002	0.17	1594
36/37	0.01	0.02	0.006	0.57	901	0.03	0.02	-0.011	-1.07	1058
Enrolled by Age										
24	0.31	0.34	0.029	0.85	1734	0.22	0.20	-0.017	-0.57	1959
27	0.16	0.20	0.038	1.43	1586	0.15	0.20	0.045	1.96	1842
30/31	0.11	0.12	0.009	0.41	1489	0.15	0.12	-0.021	-1.01	1742
33/34	0.08	0.07	-0.012	-0.67	1358	0.10	0.13	0.029	1.21	1594
36/37	0.05	0.06	0.012	0.57	901	0.11	0.07	-0.040	-2.12	1057

Sample weighted by customized weights. See appendix for description. T-statistics are adjusted for clustering at the primary sampling unit.

Degrees & Grad School	Males					Females				
	Public	Private	Diff	T-Stat	N	Public	Private	Diff	T-Stat	N
Complete Bachelors'	0.56	0.75	0.195	5.02	1070	0.54	0.71	0.173	4.20	1244
Post-Bac Schooling	0.28	0.38	0.096	2.16	1155	0.28	0.37	0.091	2.87	1366
Graduate Degree	0.12	0.18	0.061	2.11	1155	0.11	0.14	0.025	0.87	1366
Highest Grade Completed	15.49	16.33	0.836	4.20	1155	15.45	16.09	0.633	3.85	1366

Table 3.2B - HSB Outcome Summary Statistics

Outcome	Males					Females				
	Public	Private	Diff	T-Stat	N	Public	Private	Diff	T-Stat	N
Log(Earnings) in 1991†	10.11	10.10	-0.01	-0.25	1517	9.85	9.97	0.12	2.82	1605
Attempt Advanced Degree	0.14	0.31	0.17	5.83	1668	0.16	0.22	0.06	2.46	1854
Obtained Bachelor Degree	0.68	0.83	0.15	5.08	1669	0.70	0.76	0.06	1.87	1857
Labor Force Participation For A	0.93	0.88	-0.04	-1.98	1669	0.84	0.82	-0.02	-0.80	1857
Employed For All of 1991††	0.87	0.80	-0.07	-2.58	1669	0.79	0.76	-0.02	-0.75	1857
Enrolled in School in 1991	0.11	0.10	0.00	-0.20	1669	0.12	0.14	0.01	0.55	1857

† Earnings are adjusted for monthly labor force participation. See paper text for details.

†† Conditional on labor force participation.

Means based on weighted sample. T-statistics are adjusted for clustering at the high-school level.

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ages 24 – 27. Thus, the basic conclusion we can draw from these summary statistics is that private students have better labor market and education outcomes than public students, and the differences are somewhat greater for men.

Tables 3.3 and 3.4 provide summary statistics on the covariates that are used in the regressions. Almost all of the means split across public and private students as we would generally expect. In both datasets, private students have higher test scores and/or grades, come from higher quality high schools, have mothers with higher education, participate in more clubs in high school, are more likely to have college prep curricula in high school, are more likely to have 2-parent families, and have higher incomes or live in wealthier counties. Table 3.4 also shows some of the advantages the HSB has over the NLSY in terms of available covariates. In addition to the results just described, the HSB also shows that private students read more and are more likely to have attended a private high school.

Tables 3.5 through 3.8 provide the main results of this analysis. In tables 3.5 and 3.6 I show the changes in the private estimate when I add covariates for selected outcome measures. The important thing to note here is that for most of the outcomes shown, the coefficients move substantially closer to 0. This is particularly true for the wage measures. The coefficients on wages in the NLSY & earnings in the HSB fall between 60% and over 100%. Similar drops, not shown, are found for NLSY earnings as well. At the same time, the standard errors either remain relatively constant or even fall. This suggests that the declining degrees of freedom do not pose a problem to efficient estimation. For educational outcomes, the drops in the coefficients are less substantial, particularly for bachelor degree attainment.

Tables 3.7 and 3.8 provide the final regression estimates after all covariates are added for the NLSY and HSB samples, respectively. One key aspect of these results is that if we were to just consider the gender pooled sample, we would mistakenly find that the returns are always statistically insignificant. In Table 3.7, we see that

Table 3.3 - NLSY Summary Statistics for Selected Covariates - Both Genders

Covariate	Public	Private	T-Stat	N	5% Sig
Demographics					
Female	0.50	0.54	1.84	3819	
Age in 1979	17.70	17.65	-0.45	3819	
Race/Ethnicity					
Asian-Pacific Islander	0.01	0.01	0.40	3819	
Black	0.11	0.11	-0.07	3819	
Hispanic	0.04	0.02	-2.19	3819	*
Native American	0.04	0.03	-1.90	3819	
Other	0.12	0.11	-0.49	3819	
Born outside US	0.80	0.84	2.06	3798	*
Childhood					
Parental structure at 14					
Both parents	0.80	0.84	2.06	3798	*
Father only	0.01	0.01	0.04	3798	
Mother only	0.12	0.10	-1.31	3798	
Other	0.02	0.01	-0.80	3798	
Never knew mother	0.00	0.00	-0.59	3231	
Never knew father	0.01	0.01	-1.15	3,106	
Library card at 14	0.85	0.88	1.78	3807	
Magazine subscription at 14	0.79	0.83	1.97	3798	*
Newspaper subscription at 14	0.90	0.92	1.62	3808	
Education					
ASVAB - principal component 1	0.77	0.90	2.84	3819	*
ASVAB - principal component 2	0.15	0.28	3.02	3819	*
High school curriculum type					
Vocational	0.08	0.06	-1.31	3775	
Commercial	0.02	0.01	-0.74	3775	
College Prep	0.51	0.64	4.75	3775	*
General	0.39	0.28	-4.61	3775	*
High school statistics					
% teachers w/ advanced degree	50.24	53.40	1.83	2649	
Dropout rate (%)	12.62	10.95	-1.24	2630	
Enrollment	1,330	1,420	1.61	2659	
Library volumes (thousands)	16.04	19.26	3.65	2428	*
Base teacher salary (thousands)	10.78	10.84	0.82	2638	
# clubs in high school	2.09	2.39	2.56	3778	*
Year started college	1979.8	1979.5	-1.97	3818	*

Parents					
Mother's highest grade	12.60	13.25	3.53	3668	*
Father's highest grade	13.21	13.95	2.43	3479	*
Geographic					
Per-capita inc of county at age 14 (thousands)	8,368	8,986	3.74	3586	*
Region					
New England	0.03	0.14	1.71	3586	
Mid-Atlantic	0.15	0.29	3.15	3586	*
Great Lakes	0.25	0.21	-1.09	3586	
Plains	0.09	0.08	-0.25	3586	
Southeast	0.23	0.17	-1.89	3586	
Southwest	0.10	0.04	-2.91	3586	*
Mountain	0.05	0.02	-1.35	3586	
Pacific	0.10	0.05	-2.37	3586	*

Statistics are weighted by customized weights. See paper text for details. Standard errors & T-statistics are robust to heteroskedasticity & clustered by primary sampling unit.

in pooled sample none of the wage and earnings measures for any of the age groups are statistically significant.   However, when we split the sample we start to see some interesting differences.   First, for women we see that there are no statistically significant wage or earnings returns to attending a private college.   If anything, the point estimates suggest the returns for women may, in fact, be negative, though they are well within a 95% confidence interval, and thus the negative sign could be due to imprecision.

For men, the story is considerably different.   At ages 24 – 29, both wage and earnings returns are very small and statistically insignificant, though the point estimate is indeed positive at about 1%.   Then we start to see a gradual increase in the returns.   The wage return estimates for males increase to a still statistically insignificant 4% at ages 30 – 35, and then increases to a statistically significant 11% for ages 36 – 41.   For the earnings measures, the estimated returns are roughly similar for 30-35 year old males & 36-41 year old males at 15% and 12%, respectively. However, these estimates are less precise than the wage estimates, and thus are not necessarily inconsistent with the wage results.   The earnings measures for the HSB,

Table 3.4 - HSB Summary Statistics For Selected Covariates - Both Genders

	Public	Private	Difference	T-Stat	N	5% Sig
Demographics						
Female	0.512	0.561	0.049	2.10	3526	*
Race/Ethnicity						
Hispanic or Spanish	0.063	0.063	0.001	0.07	3526	
Native American	0.006	0.002	-0.004	-2.64	3526	*
Asian or Pacific Islander	0.020	0.010	-0.010	-2.70	3526	*
Black	0.085	0.076	-0.009	-0.70	3526	
White	0.826	0.848	0.022	1.35	3526	
Family Income in 1982						
$0 - $7,999	0.029	0.034	0.005	0.49	3526	
$8,000 - $14,999	0.096	0.062	-0.034	-2.91	3526	*
$15,000 - $19,999	0.103	0.102	-0.001	-0.10	3526	
$20,000 - $24,999	0.129	0.122	-0.008	-0.49	3526	
$25,000 - $29,999	0.145	0.138	-0.008	-0.48	3526	
$30,000 - $39,999	0.188	0.177	-0.011	-0.59	3526	
$40,000 - $49,999	0.104	0.085	-0.019	-1.36	3526	
> $50,000	0.137	0.192	0.055	3.00	3526	*
Academic Performance						
Average Test Percentile[†]	0.694	0.724	0.030	2.58	3526	*
Average Grades						
Mostly A's	0.205	0.288	0.083	3.73	3526	*
Mostly A's & B's	0.317	0.292	-0.025	-1.20	3526	
Mostly B's	0.241	0.211	-0.030	-1.49	3526	
Mostly B's & C's	0.171	0.143	-0.027	-1.54	3526	
Mostly C's	0.056	0.046	-0.010	-0.93	3526	
Mostly C's & D's or Lower	0.010	0.020	0.010	0.92	3526	
Motivation						
# of Extracurricular Activities in HS	3.902	4.273	0.371	2.80	3526	*
# of Extracurricular Activities in HS						
Where Student Was a Leader	1.232	1.369	0.137	1.90	3526	
How Often Reads For Pleasure						
Rarely or Never	0.225	0.193	-0.032	-1.55	3526	
Less Than Once Per Week	0.202	0.162	-0.040	-2.14	3526	*
1 or 2 Times A Week	0.304	0.350	0.046	2.05	3526	*
Every Day	0.246	0.272	0.025	1.17	3526	

Parents
  Mother's Education

High School or Lower	0.528	0.481	-0.047	-1.89	3526	
Some College	0.196	0.172	-0.024	-1.24	3526	
Bachelor Degree	0.148	0.167	0.019	1.16	3526	
Advanced Degree	0.081	0.131	0.050	3.29	3526	*

  Father's Education

High School or Lower	0.423	0.380	-0.043	-1.71	3526	
Some College	0.145	0.134	-0.011	-0.61	3526	
Bachelor Degree	0.169	0.197	0.028	1.58	3526	
Advanced Degree	0.198	0.220	0.022	1.14	3526	

Family Structure

Both Parents	0.751	0.782	0.031	1.44	3526	
Father Only	0.035	0.030	-0.005	-0.55	3526	
Mother Only	0.181	0.168	-0.013	-0.65	3526	
Other	0.034	0.018	-0.016	-2.33	3526	*

High School Type/Quality
  Type of High School

Regular Public	0.861	0.707	-0.154	-5.73	3526	*
Alternative	0.005	0.007	0.002	0.53	3526	
Catholic	0.102	0.184	0.082	3.86	3526	*
Other Private	0.032	0.102	0.070	3.14	3526	*

  High School Program

General	0.221	0.161	-0.061	-3.17	3526	*
College Prep	0.695	0.766	0.071	3.14	3526	*
Vocational	0.078	0.068	-0.010	-0.72	3526	

  High School Quality Measures

College Attendence Rate	0.500	0.597	0.096	6.59	3439	*
Dropout Rate	0.075	0.058	-0.018	-4.35	3422	*
Enrollment (in hundreds)	12.5	12.0	-0.5	-1.02	3324	
Disadvantaged Student Rate	0.119	0.084	-0.035	-4.58	3306	*
Tearcher Adv. Degree Rate	0.475	0.512	0.037	2.80	3424	*

Geographics of High School in 1982

Per Capita Income of County	10.359	11.266	0.906	7.04	3526	*

  Region

New England	0.069	0.133	0.064	3.35	3526	*
Mid Atlantic	0.126	0.277	0.151	5.86	3526	*
South Atlantic	0.136	0.136	0.000	0.01	3526	
East South Central	0.059	0.024	-0.035	-3.52	3526	*
West South Central	0.128	0.069	-0.059	-3.07	3526	*
East North Central	0.217	0.205	-0.012	-0.54	3526	
West North Central	0.107	0.073	-0.034	-2.19	3526	*
Mountain	0.056	0.014	-0.042	-4.04	3526	*
Pacific	0.102	0.070	-0.033	-2.25	3526	*

† Mean of percentile score on test batteries given in 1980 & 1982.
Means based on weighted sample. T-statistics are adjusted for clustering at the high-school level. Note that means for some categorical variables may not sum to 1 due to missing data.

Table 3.5 - Estimates of *Private* on Outcomes under Different Covariate Sets, NLSY - Both Genders Pooled

	1	2	3	4	5	6	7	8	9	Δ Coef
Log(Wage) - Age 24-29										
Coefficient	0.092*	0.096*	0.066*	0.043*	0.042	0.042	0.039	0.034	0.021	-77.2%
Standard Error	0.031	0.028	0.024	0.022	0.022	0.022	0.023	0.022	0.020	
R-Sqared	0.01	0.06	0.11	0.13	0.15	0.15	0.15	0.16	0.17	
Log(Wage) - Age 30-35										
Coefficient	0.101*	0.109*	0.066	0.046	0.040	0.039	0.036	0.027	0.010	-90.1%
Standard Error	0.045	0.040	0.035	0.033	0.030	0.031	0.030	0.030	0.030	
R-Sqared	0.01	0.09	0.17	0.18	0.20	0.21	0.21	0.22	0.23	
Log(Wage) - Age 36-41										
Coefficient	0.111*	0.126*	0.079*	0.068*	0.057	0.057	0.056	0.044	0.036	-67.6%
Standard Error	0.041	0.037	0.032	0.030	0.031	0.031	0.031	0.030	0.030	
R-Sqared	0.01	0.10	0.19	0.20	0.21	0.22	0.22	0.23	0.24	
Highest Grade Completed										
Coefficient	0.723*	0.719*	0.473*	0.466*	0.402*	0.401*	0.390*	0.360*	0.361*	-50.1%
Standard Error	0.140	0.136	0.107	0.107	0.102	0.097	0.095	0.096	0.098	
R-Sqared	0.03	0.05	0.24	0.24	0.28	0.32	0.32	0.33	0.34	
Bachelor Degree Attainment (Probit)										
Coefficient	0.495*	0.508*	0.423*	0.415*	0.403*	0.411*	0.409*	0.381*	0.372*	-24.8%
Standard Error	0.094	0.091	0.091	0.090	0.092	0.096	0.094	0.094	0.097	
Avg Marginal Effect	0.183	0.185	0.130	0.128	0.117	0.115	0.114	0.104	0.101	
Pseudo R-Sqared	0.022	0.053	0.201	0.201	0.250	0.279	0.283	0.294	0.301	

* denotes significance at the 5% or lower level. Covariates are dummies (for binary variables) or entered linearly (if continuous) unless specified otherwise. Standard errors are robust to heteroskedasticity and clustered by primary sampling units. Regressions weighted by customized weights. Each regression contains all of the covariates in the previous regression plus the following: 1 - Private only. 2 - Race, age, gender. 3 - ASVAB. 4 - Quadratic in per-capita income of county of residence at age 14. 5- Parents Info - occupation (Census 1 digit), years of education, age, whether student ever knew a parent. 6 - Quartic in year started college. 7 - Quadratic in the number of clubs participated in in high school. 8 - HS Info - curriculum type, fraction of teachers w/ advanced degrees, dropout rate, enrollment, # of books in library, teacher base salary. 9 - Region, urbanicity.

Table 3.6 - Estimates of *Private* on Outcomes under Different Covariate Sets, HSB - Both Genders Pooled

	1	2	3	4	5	6	7	8	9	Δ Coef
Log Annual Earnings in 1991†										
Coefficient	0.049	0.035	0.018	0.026	0.025	0.015	0.014	-0.022	-0.011	-120.4%
Standard Error	0.036	0.035	0.036	0.036	0.036	0.036	0.036	0.037	0.036	
R Squared	0.00	0.03	0.05	0.07	0.07	0.09	0.09	0.11	0.15	
Attempt Advanced Degree (Probit)										
Coefficient	0.406*	0.332*	0.294*	0.285*	0.278*	0.250*	0.255*	0.238*	0.202*	-50.2%
Standard Error	0.070	0.071	0.072	0.074	0.075	0.076	0.076	0.078	0.078	
Pseudo-R Squared	0.02	0.07	0.09	0.12	0.12	0.13	0.13	0.14	0.18	
Avg Marginal Effect	0.113	0.086	0.076	0.071	0.069	0.061	0.062	0.057	0.046	
Bachelor Degree Attainment (Probit)										
Coefficient	0.307*	0.289*	0.250*	0.259*	0.259*	0.242*	0.254*	0.222*	0.254*	-17.3%
Standard Error	0.070	0.068	0.070	0.073	0.073	0.074	0.075	0.077	0.078	
Pseudo-R Squared	0.01	0.09	0.11	0.15	0.15	0.17	0.18	0.19	0.21	
Avg Marginal Effect	0.099	0.086	0.073	0.072	0.071	0.065	0.068	0.059	0.065	
Participated in Labor Force for all of 1991 (Probit)										
Coefficient	-0.162*	-0.132	-0.121	-0.096	-0.082	-0.099	-0.092	-0.113	-0.065	-62.5%
Standard Error	0.077	0.074	0.076	0.077	0.079	0.079	0.080	0.081	0.081	
Pseudo-R Squared	0.00	0.02	0.03	0.09	0.09	0.10	0.11	0.11	0.15	
Avg Marginal Effect	-0.035	-0.028	-0.026	-0.019	-0.016	-0.019	-0.017	-0.021	-0.012	

† Adjusted from reported earnings by dividing earnings reported by fraction of year in labor force. * denotes significance at the 5% or lower level. Covariates are dummies (for binary variables) or entered linearly (for continuous) unless specified otherwise. Standard errors are robust to heteroskedasticity and clustering at the high school level. Each regression includes all variables in the previous regression and the following: 1 - Private only; 2 - Quartic in average test percentile, average grades in high school; 3 - Race, income, county per-capita income in 1982; 4 - Family structure, parents' education, parents' occupations; 5 - Type of high school; 6 - High school program, dropout rate, college attendence rate, enrollment, % of teachers with advanced degree, disadvantaged student rate; 7 - Year started college; 8 - Region of high school; 9 - Read for pleasure, homework completion in high school, quadratics in extracurriculars participated, extracurricular leadership, & interaction, intended major as of 1982, religion...

Table 3.7 - NLSY OLS/Probit Estimates of "Private" on Outcomes

	Both				Males				Females			
	Coef	SE	N	AME†	Coef	SE	N	AME†	Coef	SE	N	AME†
Log Wages By Age												
24-29	0.023	0.021	3614	-	0.014	0.030	1687	-	0.032	0.031	1927	-
30-35	0.014	0.030	3179	-	0.039	0.036	1480	-	-0.017	0.040	1699	-
36-41	0.042	0.032	2642	-	0.104**	0.035	1257	-	-0.007	0.053	1385	-
Log Earnings by Age												
24-29	0.002	0.027	3615	-	0.027	0.041	1694	-	-0.008	0.047	1921	-
30-35	0.026	0.059	3155	-	0.137*	0.066	1472	-	-0.049	0.089	1683	-
36-41	0.013	0.055	2577	-	0.110*	0.054	1237	-	-0.043	0.087	1340	-
Out of Labor Force by Age (Probit)												
24	-0.062	0.081	3657	-0.012	0.135	0.111	1695	0.023	-0.248	0.112	1935	-0.047
27	0.075	0.082	3396	0.013	0.206	0.120	1520	0.022	-0.052	0.101	1821	-0.011
30/31	0.049	0.095	3161	0.008	-0.191	0.159	1264	-0.013	0.050	0.110	1700	0.012
33/34	0.090	0.093	2890	0.015	0.028	0.179	1219	0.002	0.080	0.115	1534	0.020
36/37	-0.038	0.107	1930	-0.006	-0.206	0.266	613	-0.014	-0.057	0.144	1043	-0.012
Unemployed by Age (Probit)												
24	-0.073	0.098	3620	-0.008	-0.229	0.150	1672	-0.024	0.082	0.105	1837	0.009
27	0.084	0.114	3151	0.006	0.177	0.160	1354	0.013	0.064	0.169	1615	0.004
30/31	0.002	0.096	3148	0.000	-0.270	0.196	1153	-0.017	0.155	0.128	1625	0.012
33/34	0.111	0.123	2833	0.007	-0.087	0.207	1186	-0.004	0.279	0.145	1479	0.020
36/37	-0.137	0.152	1621	-0.008	6.985**	0.191	573	0.059	-0.435	0.259	738	-0.025
Enrolled by Age (Probit)												
24	-0.072	0.074	3686	-0.021	#REF!	#REF!	#REF!	#REF!	-0.137	0.106	1931	-0.036
27	0.092	0.072	3426	0.022	#REF!	#REF!	#REF!	#REF!	0.177	0.105	1615	0.041
30/31	-0.064	0.072	3227	-0.012	0.022	0.122	1469	0.004	-0.177	0.103	1723	-0.034
33/34	0.126	0.099	2943	0.020	-0.171	0.132	1290	-0.019	0.315*	0.134	1583	0.055
36/37	-0.024	0.104	1936	-0.003	0.216	0.198	718	0.018	-0.197	0.124	1025	-0.028

Degrees & Grad School

	Coef	SE	N	AME	Coef	SE	N	AME	Coef	SE	N	AME
Complete Bachelors' (Probit)	0.396**	0.095	2312	0.107	0.545**	0.129	1070		0.135		1237	0.089
Post-Bac Schooling (Probit)	0.136	0.072	2517	0.037	0.129	0.122	1148		0.032		1357	0.046
Graduate Degree (Probit)	-0.005	0.092	2460	-0.001	0.131	0.132	1023		0.020		1333	-0.016
Highest Grade Completed	0.380**	0.099	2520	-	0.473**	0.150	1155		0.364*		1365	-

Table 3.8 - HSB OLS/Probit Estimates of *Private* on Outcomes

	Male				Female			
	Coef	SE	N	AME	Coef	SE	N	AME
Log(Earnings) in 1991†	-0.053	0.055	1517	-	0.028	0.047	1605	-
Attempt Advanced Degree	0.434**	0.110	1657	0.087	0.058	0.109	1813	0.01
Obtained Bachelor Degree	0.442**	0.110	1654	0.108	0.264*	0.111	1853	0.06
LFP For All 1991	-0.089	0.124	1626	-0.010	-0.070	0.109	1803	-0.01
Employed For All of 1991††	-0.359*	0.149	1338	-0.036	0.038	0.159	1502	0.00
Enrolled in School in 1991	0.042	0.123	1652	0.006	0.099	0.121	1828	0.02

shown in Table 3.8, are consistent with the wage earnings measures in the NLSY, with statistically insignificant earnings returns for both males and females 11 years after finishing their sophomore year of high school (i.e. around age 27).

One concern about these results is that, since they are only based off of people who are employed, they may be hiding some substantial differences in labor force participation and employment. To alleviate these concerns I show the relationship between *private* and both unemployment and labor force participation. For the NLSY sample, I test this at multiple ages and do not find any statistically significant impact of the private variable on these outcomes except for men at age 36 or 37. However, this result is suspect since only a small portion of the original sample remains in the data up to this age level and a large number of observations are dropped from the probit analysis due to perfect prediction. A linear probability model of the same specification generates a slightly negative and statistically insignificant estimate of the employment effect. LPM models for male unemployment at other ages provide estimates nearly identical to the marginal effects of the probits, suggesting that the probit estimate for age 36/37 males is inaccurate.

Nonetheless, in the HSB sample, I do find a statistically significant drop in employment for private males of 3.6 percentage points. How this may affect the HSB earnings estimate is unclear. However, since that estimate is consistent with the NLSY estimates, the effect of the employment differential is likely small. Another concern is that if one type of student is more likely to be enrolled in classes while working then their wages and earnings may be artificially low, i.e. they may be working part-time. To address this, table 3.5 shows that there is no statistically significant effect of private on whether or not a person is enrolled in school at a given age. This holds for the HSB results as well.

For educational outcomes, the results differ much less across genders, though once again the men seem to garner higher returns. Attending a private school is

associated with statistically significant increases in the likelihood of finishing bachelor degrees for both males and females. These results hold in both datasets. For the NLSY data, the average marginal effect of *private* on completing a bachelor's degree is 13.5 percentage points for males and an 8.9 percentage points for females . In light of this, it is also not surprising that private students are also found to complete more years of education. Also, in the HSB data, private males appear more likely to attempt post-bachelor degrees, but the NLSY results suggest there is no statistically significant difference for post-bachelor schooling or graduate school completion.

3.5   Modeling Selection and Alternative Specifications

3.5.1   Evidence for Positive Selection

In the previous section, I showed that there are substantial drops in the *private* coefficients when covariates are added, providing some evidence that the selection into private schools is positively related to unobservables that co-vary positively with wage and educational outcomes, such as ability and motivation. Table 3.9 provides some additional evidence that the selection works in this way. Here, I run a probit model of whether or not a student's main school is a private school on the covariates used in the outcome regressions. In all cases, the coefficients are either statistically insignificant or are statistically significant in the direction that we would expect if there is positive selection. Students who attend private colleges have higher test scores (particularly females), higher incomes, participate in more clubs in high school (males only), are more likely to be in a college preparatory curriculum in high school, attend high schools with more library books, have mothers with more education (females), and are more likely to come from the northeast section of the country. Women are also more likely to come from a two-parent household. One concern about these results is that, since the region dummies are so highly statistically significant, they may be

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Table 3.9 - Probits of Covariates on Private, NLSY

Variable	Both Genders		Males		Females	
	Coef	*SE*	*Coef*	*SE*	*Coef*	*SE*
Female	0.055	(0.066)	-	-	-	-
ASVAB						
1st Principla Comp	0.093*	(0.054)	0.023	(0.076)	0.167**	(0.076)
2nd Principal Comp	0.074*	(0.040)	0.002	(0.059)	0.149***	(0.055)
Age 14 - County Income †	0.770***	(0.224)	0.726**	(0.289)	0.867**	(0.352)
# of High School Clubs	0.030	(0.022)	0.101***	(0.033)	-0.026	(0.026)
HS Curric (College Prep omitted)						
Vocational	-0.052	(0.106)	-0.055	(0.147)	-0.031	(0.141)
Commercial	-0.178	(0.172)	-0.320	(0.424)	-0.181	(0.188)
General	-0.189***	(0.072)	-0.245**	(0.106)	-0.146	(0.091)
# of Books in HS Library †	0.154***	(0.038)	0.150***	(0.051)	0.141**	(0.058)
Mother's Highest Grade Completed	0.045***	(0.011)	0.030	(0.019)	0.057***	(0.020)
Father's Highest Grade Completed	0.002	(0.013)	0.020	(0.017)	-0.013	(0.016)
Parents/Guardians (Both omitted)						
One Natural, One Stepparent	-0.034	(0.133)	-0.034	(0.200)	-0.111	(0.178)
Father Only	0.092	(0.242)	0.444	(0.339)	-1.039***	(0.398)
Mother Only	-0.071	(0.080)	-0.034	(0.118)	-0.118	(0.120)
Other	-0.069	(0.192)	-0.137	(0.278)	-0.097	(0.235)
Region (New-England omitted)						
Mid-Atlantic	-0.480**	(0.216)	-0.473**	(0.230)	-0.412*	(0.225)
Great Lakes	-0.877***	(0.208)	-0.824***	(0.217)	-0.865***	(0.222)
Plains	-0.835***	(0.212)	-0.788***	(0.233)	-0.822***	(0.214)
Southeast	-0.864***	(0.218)	-0.893***	(0.233)	-0.783***	(0.266)
Southwest	-1.239***	(0.210)	-1.288***	(0.234)	-1.250***	(0.244)
Mountain	-1.175***	(0.348)	-1.053***	(0.351)	-1.274***	(0.396)
Pacific	-1.196***	(0.204)	-1.284***	(0.260)	-1.139***	(0.235)

† denotes coefficient & standard errors are multiplied by 10,000. ***, **, * denote significance at the 1%, 5%, & 10% levels, respectively. Robust standard errors clustered by primary sampling unit in parentheses. Regressions also include religion, fraction of high school teachers with advanced degrees, high school dropout rate, base salary of high school teacher, high scholl enrollment, age, whether someone in the person's family at age 14 had a library card, magazine subscription, or newspaper subscription, and race dummies. All coeficients for these variables were insiginficant except for "Native American" in the female regression. Weighted by 1979 NLSY weights.

absorbing a lot of variation. Nonetheless, regressions without the region dummies provided similar results.

3.5.2   Specification Checks

Table 3.10 shows that the main NLSY results are robust to a number of different specification checks. Column 1 provides the main results found in Table 3.5. In column 2, I restrict the sample to only those who complete college. Since there is such a large correlation between attending a private school and completing college, we may think that any wage returns would come from that extra education. Column 2 shows little change in the estimates when making this restriction. This suggests that any positive returns are not being completely driven by degree completion. I will explore this issue in depth in the next section.

Column 3 restricts the sample to students who are 14 – 17 in 1979. The concern here is that, since the ASVAB test is given to individuals at different ages, this could create a measurement error bias. While I age adjust the ASVAB scores before doing the factor analysis, the larger concern is that some students may take the test after attending college. Thus the scores will reflect both pre-college and during-college human capital accumulation. By restricting to people who are 14 – 17 at the start of the survey, when the test is administered, we can see if this is a problem. Once again, the wage and earnings estimates do not appear to change much with this restriction. While log wages at ages 36 – 41 becomes statistically insignificant, this is only due to the reduced precision since the coefficient actually increases. While the coefficients of the other estimates jump around a bit, these are in line with the increased standard errors and thus, significance does not change. Educational outcomes are another matter. For males there is a substantial increase in the bachelor degree coefficient and increases in post-bachelor schooling for both genders. Nonetheless, while the magnitude of the education results for this group is

Table 3.10 - Alternative Estimations of *Private* on Outcomes - NLSY

A: Males	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Wages By Age								
24-29	0.014	0.051	0.058	0.023	0.017	0.031	0.017	0.012
	(0.030)	(0.041)	(0.054)	(0.032)	(0.030)	(0.037)	(0.035)	(0.032)
30-35	0.039	0.063	0.039	0.039	0.048	0.044	0.036	0.048
	(0.036)	(0.048)	(0.052)	(0.040)	(0.037)	(0.038)	(0.038)	(0.037)
36-41	0.104**	0.103*	0.110	0.110**	0.127**	0.121**	0.111**	0.100**
	(0.035)	(0.051)	(0.064)	(0.035)	(0.037)	(0.038)	(0.038)	(0.037)
Log Earnings by Age								
24-29	0.027	0.064	0.096	0.024	0.028	0.058	0.068	0.039
	(0.041)	(0.044)	(0.072)	(0.041)	(0.041)	(0.049)	(0.042)	(0.042)
30-35	0.137*	0.218*	0.156*	0.161*	0.162*	0.168*	0.143*	0.142*
	(0.066)	(0.088)	(0.080)	(0.072)	(0.064)	(0.081)	(0.071)	(0.063)
36-41	0.110*	0.110	0.065	0.130*	0.137*	0.132*	0.121*	0.078
	(0.054)	(0.062)	(0.080)	(0.053)	(0.054)	(0.054)	(0.056)	(0.050)
Degrees & Grad School								
Complete Bachelors' (Probit)	0.545**	-	0.923**	0.423**	0.410**	-	0.496**	0.433**
	(0.129)		(0.231)	(0.097)	(0.097)		(0.113)	(0.102)
Post-Bac Schooling (Probit)	0.129	-0.053	0.339*	0.171*	0.159*	-	0.133	0.026
	[0.135]	[0.148]	[0.183]	[0.110]	[0.107]		[0.124]	[0.107]
Graduate Degree (Probit)	0.131	-0.016	0.054	-0.009	0.024	-	0.199	0.123
	(0.132)	(0.146)	(0.242)	(0.097)	(0.094)		(0.129)	(0.114)
Highest Grade Completed	0.473**	0.076	0.431*	0.462**	0.545**	-	0.457**	0.349**
	[0.020]	[-0.004]	[0.007]	[-0.001]	[0.004]		[0.031]	[0.019]
	(0.150)	(0.145)	(0.211)	(0.157)	(0.154)		(0.140)	(0.123)

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B: Females

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Wages By Age								
24-29	0.032 (0.031)	-0.016 (0.040)	0.033 (0.054)	0.039 (0.032)	0.028 (0.033)	0.027 (0.033)	0.016 (0.034)	0.039 (0.033)
30-35	-0.017 (0.040)	-0.045 (0.054)	0.024 (0.050)	-0.025 (0.040)	-0.011 (0.042)	-0.013 (0.041)	-0.019 (0.041)	-0.008 (0.042)
36-41	-0.007 (0.053)	-0.088 (0.078)	0.007 (0.084)	-0.009 (0.051)	-0.003 (0.053)	0.010 (0.051)	-0.001 (0.053)	0.010 (0.054)
Log Earnings by Age								
24-29	-0.008 (0.047)	-0.047 (0.054)	-0.056 (0.086)	-0.011 (0.048)	-0.015 (0.048)	-0.045 (0.051)	-0.061 (0.052)	-0.016 (0.049)
30-35	-0.049 (0.089)	-0.160 (0.120)	0.044 (0.123)	-0.017 (0.080)	-0.037 (0.083)	-0.076 (0.090)	-0.070 (0.095)	-0.036 (0.096)
36-41	-0.043 (0.087)	-0.210 (0.131)	0.083 (0.135)	0.012 (0.092)	-0.009 (0.081)	-0.024 (0.089)	-0.042 (0.088)	-0.023 (0.090)
Degrees & Grad School								
Complete Bachelors' (Probit)	0.340** (0.117) [0.089]	-	0.451* (0.216) [0.090]	0.423** (0.097) [0.110]	0.410** (0.097) [0.107]	-	0.405** (0.119) [0.104]	0.398** (0.113) [0.102]
Post-Bac Schooling (Probit)	0.170 (0.097) [0.046]	0.135 (0.111) [0.045]	0.538** (0.157) [0.128]	0.171* (0.082) [0.045]	0.159* (0.072) [0.042]	-	0.110 (0.090) [0.029]	0.104 (0.084) [0.028]
Graduate Degree (Probit)	-0.107 (0.140) [-0.016]	-0.240 (0.154) [-0.059]	0.284 (0.240) [0.037]	-0.009 (0.097) [-0.001]	0.024 (0.094) [0.004]	-	-0.148 (0.138) [-0.022]	-0.111 (0.124) [-0.017]
Highest Grade Completed	0.364* (0.142)	0.127 (0.149)	0.532* (0.226)	0.399** (0.152)	0.365* (0.151)	-	0.353* (0.140)	0.307* (0.134)

*, ** Denotes significance at 5% & 1% levels, respectively. Robust standard errors clustered by primary sampling unit in parentheses for regressions. Average marginal effects are in brackets. Wage and earnings regressions weighted by customized weights unless specified otherwise. See paper for description. Full regressions include same covariates as described in table 5. Specifications are as follows: 1 - Main regressions from table 5. 2 - Limited to persons who attained a bachelor degree. 3 - Limited to persons aged 14 - 17 in 1979. 4 - Control for state of college. 5 - Control for state at age 14 rather than region. 6 - 2002 weights. 7 - 1996 weights. 8 - 1990 weights.

different, it does not change the overall pattern of the results.

Columns 4 and 5 add controls for state of college and replace region of residence with state of residence controls, respectively. In both cases the results are very similar to the main results in column 1. Columns 6 – 8 provide estimates under alternative weights, using the NLSY provided weights based on the 2002, 1996, and 1990 samples. The results are robust across all three weighting schemes with respect to the main results.

3.6 Extensions

3.6.1 Role of Additional Education in Wage/Earnings Estimates

As the results in Table 3.7 show, there is a large relationship between attending a private school and additional schooling for both genders. Thus, it seems plausible that the positive wage returns found for males could be solely due to increased education stock and not necessarily due to a higher quality education. Table 3.11 investigates this by accounting for multiple combinations of different educational outcome variables for males.[5]   Arguably, the private estimates here reflect the wage and earnings returns to attending a private college net of the improved educational outcomes. Column 2 simply controls for whether or not the student completes his bachelor degree. Two interesting results come out of these regressions. First, the private coefficients at younger ages (24-29 and 30-35) barely change. On the other hand, the private coefficient for both wages and earnings at ages 36 – 41 both fall by 33%. This is a rather surprising result considering that 75% of degree recipients in the sample finish before age 24 and 93% finish before age 30 . Nonetheless, the estimates for the returns to obtaining a bachelor's degree appear to increase with age, suggesting that much of the increase with age in the wage returns to attending a

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[5] Running these estimates for females generates no changes in the estimated wage effects. These results are provided in Appendix Table 11

Table 3.11 - OLS Regressions of *Private* on Wages and Earnings While Controlling For Educational Outcomes - Males, NLSY

A. Log(Wage) ages 24-29

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private	0.014	0.017	-0.010	0.018	0.017	0.019	0.042	0.045
	(0.030)	(0.032)	(0.048)	(0.033)	(0.030)	(0.033)	(0.033)	(0.035)
Bachelor Degree	-	0.082*	0.073*	0.090*	-	0.213**	-	0.165**
		(0.037)	(0.037)	(0.038)		(0.044)		(0.052)
Private*Bachelor Degree	-	-	0.040	-	-	-	-	-
			(0.070)					
Bachelor Degree*Graduate Degree	-	-	-	-0.055	-	-	-	-
				(0.044)				
Highest Grade Completed	-	-	-	-	-0.008	-0.041**	-	-0.018
					(0.009)	(0.011)		(0.013)

B. Log(Wage) ages 30-35

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private	0.039	0.033	-0.001	0.032	0.026	0.033	0.050	0.046
	(0.036)	(0.039)	(0.049)	(0.038)	(0.036)	(0.039)	(0.038)	(0.041)
Bachelor Degree	-	0.179**	0.169**	0.161**	-	0.170**	-	0.148**
		(0.034)	(0.034)	(0.036)		(0.047)		(0.055)
Private*Bachelor Degree	-	-	0.049	-	-	-	-	-
			(0.066)					
Bachelor Degree*Graduate Degree	-	-	-	0.123*	-	-	-	-
				(0.052)				
Highest Grade Completed	-	-	-	-	0.031**	0.003	-	0.014
					(0.009)	(0.013)		(0.015)

C. Log(Wage) ages 36-41

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private	0.104**	0.070	0.063	0.069	0.071*	0.067	0.122**	0.087*
	(0.035)	(0.038)	(0.052)	(0.038)	(0.036)	(0.038)	(0.035)	(0.039)
Bachelor Degree	-	0.299**	0.297**	0.275**	-	0.182**	-	0.168*
		(0.052)	(0.055)	(0.052)		(0.067)		(0.066)
Private*Bachelor Degree	-	-	0.010	-	-	-	-	-
			(0.070)					
Bachelor Degree*Graduate Degree	-	-	-	0.164*	-	-	-	-
				(0.073)				
Highest Grade Completed	-	-	-	-	0.065**	0.036*	-	0.049**
					(0.012)	(0.017)		(0.017)

D. Log(Earnings) ages 24-29

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private	0.027 (0.041)	0.021 (0.042)	-0.051 (0.095)	0.023 (0.043)	0.032 (0.042)	0.025 (0.044)	0.053 (0.045)	0.046 (0.049)
Bachelor Degree	-	0.182* (0.071)	0.159* (0.074)	0.214** (0.074)	-	0.468** (0.090)	-	0.393** (0.095)
Private*Bachelor Degree	-	-	0.104 (0.104)	-	-	-	-	-
Bachelor Degree*Graduate Degree	-	-	-	-0.215** (0.072)	-	-	-	-
Highest Grade Completed	-	-	-	-	-0.016 (0.014)	-0.089** (0.016)	-	-0.059** (0.020)

E. Log(Earnings) ages 30-35

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private	0.137* (0.066)	0.136 (0.070)	-0.005 (0.099)	0.134* (0.068)	0.118 (0.065)	0.135 (0.069)	0.153* (0.063)	0.156* (0.066)
Bachelor Degree	-	0.215** (0.061)	0.172** (0.061)	0.189** (0.063)	-	0.164 (0.092)	-	0.130 (0.099)
Private*Bachelor Degree	-	-	0.200 (0.134)	-	-	-	-	-
Bachelor Degree*Graduate Degree	-	-	-	0.173 (0.097)	-	-	-	-
Highest Grade Completed	-	-	-	-	0.045** (0.017)	0.016 (0.024)	-	0.040 (0.025)

E. Log(Earnings) ages 36-41

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private	0.110* (0.054)	0.074 (0.055)	0.084 (0.082)	0.072 (0.054)	0.079 (0.055)	0.073 (0.054)	0.147** (0.054)	0.105 (0.054)
Bachelor Degree	-	0.342** (0.054)	0.345** (0.058)	0.321** (0.055)	-	0.307** (0.095)	-	0.269** (0.091)
Private*Bachelor Degree	-	-	-0.015 (0.101)	-	-	-	-	-
Bachelor Degree*Graduate Degree	-	-	-	0.147* (0.070)	-	-	-	-
Highest Grade Completed	-	-	-	-	0.059** (0.012)	0.011 (0.021)	-	0.038 (0.022)

*, ** Denotes significance at 5% & 1% levels, respectively. Robust standard errors clustered by primary sampling unit are in parentheses. Regressions weighted by customized weights. See appendix for description. All regressions include include same covariates as described in table 5. Specifications 7 & 8 also include dummies for major fields.

private school may be related to educational attainment. Column 3 interacts degree completion with attending a private school to see if the returns to degree completion differ across college types. While, not surprisingly, the private coefficient falls in some cases, the interaction is never statistically significantly different from 0. Columns 4-6 add in highest grade completed and advanced degree completion, neither of which demonstrably changes the results from column 2.

In columns 7 and 8, I address another concern with how colleges affect educational decisions by controlling for choice of major. One can make the argument that private colleges may encourage students to participate in majors that lead to lower paying careers or, alternatively, students who are interested in these careers may be attracted to private colleges. This brings up one potential flaw in my argument for positive selection. If the latter argument is true, then there may be elements of unobserved selection bias that reduce, rather than increase, the estimates. It does indeed appear that when controlling for major the private estimates increase. However, the increase is small and does not change the pattern or significance of results. Thus, if the choice of major is being driven by selection, its impacts on the estimates are likely to be small.

3.6.2   *Spousal Earnings and the Marriage Market*

Since we can only take these estimates to be upper bounds on the returns to attending a private college, it is not necessarily clear for men if there are any wage or earnings returns. However, for women it seems clear that there are little to no pecuniary returns to attending a private college. If anything, the returns may be negative. This begs the question of what did women from the time period covered in the data gain from attending a private college. Just like males, they do appear more likely to graduate and to complete more years of schooling. However, it seems unlikely that this is the entire story, particularly since the estimated returns for males

are higher for both wages and educational attainment. One possibility is that women get improved marriage market outcomes, such as higher spousal income and higher quality marriages.

Table 3.12 investigates these possibilities. For spousal earnings, when one just looks at differences in means, it does appear that both men and women who attend private colleges marry wealthier people than their public counterparts. This is particularly true at younger ages. However, once the covariates are added the spousal earnings returns from ages 24 - 29 fall to insignificance. It is also unlikely that this result is due to differences in the probability of entering into marriage, since the likelihood of ever getting married does not seem to be related to attending a private school. What is striking is that women who attend private schools are statistically significantly less likely to get divorced than their public counterparts. When covariates are added the magnitude of the effect falls, but remains statistically significant at about 5.7 percentage points. This suggests that there could be substantial quality of life benefits to women from attending a private college. Interestingly, I find nearly the exact opposite effect for men, though due to larger standard errors it is not statistically significant.

3.6.3 Accounting for Heterogeneity in College Quality

Other questions one may ask about these results is how much of the estimates can be explained by differences in observable college quality, and whether there is heterogeneity with respect to observable quality. In Table 3.13 I consider this issue by looking at how robust the estimates are to controls for and interactions with observed quality. I use the school's average SAT score or, if SAT score is not available, the SAT equivalent of their average ACT score as a proxy for college quality. The data for the SAT/ACT measure comes from *Barron's Profiles of American Colleges 1982*. While using SAT/ACT scores is not ideal, there are two main reasons I use them. First

Table 3.12 - Spousal Earnings and Marriage Outcomes

	Means				Regressions					
	Male		Female		Males			Females		
	Public	Private	Public	Private	(1)	(2)	(3)	(1)	(2)	(3)
Log(Spouse's Earnings) by Age										
24-29	9.78	9.93	10.45	10.59	0.148*	0.000	0.017	0.144*	0.049	0.059
	(0.83)	(0.77)	(0.67)	(0.64)	(0.074)	(0.076)	(0.073)	(0.057)	(0.046)	(0.046)
30-35	10.03	10.11	10.84	10.92	0.075	-0.010	-0.020	0.081	0.001	0.008
	(0.88)	(0.99)	(0.70)	(0.71)	(0.075)	(0.067)	(0.069)	(0.058)	(0.046)	(0.045)
36-41	9.89	9.97	10.67	10.80	0.075	-0.099	-0.112	0.137	0.072	0.064
	(0.96)	(1.06)	(0.77)	(0.84)	(0.088)	(0.083)	(0.084)	(0.080)	(0.066)	(0.065)
Ever Married (Probit)	0.82	0.80	0.87	0.86	-0.072	0.016	-	-0.030	-0.040	-
	(0.38)	(0.40)	(0.34)	(0.34)	(0.103)	(0.114)		(0.106)	(0.108)	
					[-0.019]	[0.004]	-	[-0.006]	[-0.008]	-
Ever Divorced (Probit)	0.19	0.20	0.31	0.22	0.029	0.195	0.247	-0.288**	-0.245**	-0.213*
	(0.40)	(0.40)	(0.46)	(0.41)	(0.114)	(0.122)	(0.139)	(0.094)	(0.092)	(0.095)
	-	-	-	-	[0.008]	[0.050]	[0.062]	[-0.093]	[-0.069]	[-0.057]

*, ** Denotes significance at 5% & 1% levels, respectively. Robust standard errors clustered by primary sampling unit in parentheses for regressions, standard deviations in parentheses for means. Average marginal effects in brackets. Earnings regressions weighted by customized weights. See appendix for description. Marriage/divorce regressions limited to people in sample in 1990 and weighted by 1990 weights. Specification 1 includes only the *private* dummy. Specifications 2 & 3 include same covariates as described in table 5. Specification 3 also includes include a quartic in the age at which person first reported being married or reported a spousal income.

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Table 3.13 - Heterogeneous Returns by SAT Score

	Male (1) Private	Male (2) Private	Male (3) Private	Male (4) Private	Male (4) Priv*SAT	Female (1) Private	Female (2) Private	Female (3) Private	Female (4) Private	Female (4) Priv*SAT
Log Wages By Age										
24-29	0.014 (0.030)	0.014 (0.042)	-0.009 (0.043)	0.188 (0.395)	-0.021 (0.044)	0.032 (0.031)	0.017 (0.037)	0.020 (0.038)	0.478 (0.303)	-0.050 (0.033)
30-35	0.039 (0.036)	0.034 (0.043)	0.029 (0.043)	0.199 (0.409)	-0.018 (0.044)	-0.017 (0.040)	-0.053 (0.043)	-0.048 (0.043)	0.370 (0.391)	-0.045 (0.041)
36-41	0.104** (0.036)	0.088* (0.044)	0.065 (0.043)	-0.322 (0.381)	0.041 (0.044)	-0.007 (0.040)	-0.067 (0.045)	-0.054 (0.043)	0.369 (0.391)	-0.046 (0.041)
Log Earnings by Age										
24-29	0.027 (0.035)	0.047 (0.048)	0.050 (0.050)	-0.722 (0.445)	0.082 (0.047)	-0.008 (0.053)	-0.020 (0.060)	-0.016 (0.060)	0.763 (0.455)	-0.084 (0.049)
30-35	0.137* (0.041)	0.144 (0.081)	0.142 (0.085)	-0.105 (0.603)	0.026 (0.067)	-0.049 (0.047)	-0.122 (0.060)	-0.126 (0.060)	1.514 (0.843)	-0.177 (0.092)
36-41	0.110* (0.066)	0.084 (0.081)	0.078 (0.085)	0.083 (0.524)	0.000 (0.067)	-0.043 (0.087)	-0.069 (0.099)	-0.043 (0.094)	0.785 (0.930)	-0.090 (0.101)
Degrees & Grad School										
Complete Bachelors' (Probit)	0.545** (0.129)	0.416** (0.138)	0.319* (0.148)	4.463* (1.753)	-0.455* (0.192)	0.340** (0.117)	0.147 (0.149)	0.113 (0.152)	4.320** (1.617)	-0.461** (0.171)
Post-Bac Schooling (Probit)	0.129 (0.122) [0.032]	0.066 (0.142) [0.017]	-0.023 (0.142) [-0.006]	0.113 (1.056) [0.029]	-0.014 (0.112) [-0.004]	0.170 (0.097) [0.047]	-0.003 (0.123) [-0.001]	0.009 (0.134) [0.003]	2.036 (1.156) [0.459]	-0.218 (0.124) [-0.059]
Graduate Degree (Probit)	0.131 (0.132) [0.020]	0.161 (0.166) [0.024]	0.028 (0.178) [0.004]	-2.089 (1.559) [-0.213]	0.216 (0.155) [0.030]	-0.107 (0.140) [-0.107]	-0.318 (0.171) [-0.318]	-0.397* (0.194) [-0.397]	1.738 (1.623) [1.738]	-0.226 (0.170) [-0.226]
Highest Grade Completed	0.473** (0.150)	0.267 (0.147)	0.159 (0.146)	1.848 (1.233)	-0.180 (0.131)	0.364* (0.142)	0.091 (0.177)	0.057 (0.187)	4.907** (1.596)	-0.525** (0.168)

*, ** Denotes significance at 5% & 1% levels, respectively. Robust standard errors clustered by primary sampling unit are in parentheses. Average marginal effects are in brackets. Wage and earnings regressions weighted by customized weights unless specified otherwise. See appendix for description. Full regressions include same covariates as described in table 5. ACT scores are converted to SAT scores following Marco & Abdel-Fattah (1991) and conversion tables provided by ACT. Interaction coefficients multiplied by 100. Specifications are as follows: 1 - Main regressions from table 5. 2 - Regressions with no SAT/ACT scores, but on SAT/ACT school sample. 3 - Control for quartic in SAT/ACT scores. 4 - Interaction of private with school SAT/ACT score & control for quartic in SAT/ACT score.

of all, I could arguably take multiple quality measures and use principal component analysis to generate a composite quality index as is done in Black and Smith (2004) and Black, Daniel, and Smith (2005). However, there are two drawbacks to this method - it would make interpretation of the quality estimate difficult and since many schools do not report some measures, I would have to drop a substantial number of schools. Secondly, Black and Smith (2005) show that SAT scores are a relatively accurate measure of overall college quality.

Since many of the students studied in the main sample do not attend colleges that report average SAT or ACT scores, the sample for this analysis is different from previous ones. Thus, in column 2, I run the regression without any college quality measures but only on the quality sub-sample. The wage and earnings measures stay relatively consistent; however the educational outcome coefficients drop for both males and females. This in and of itself is an interesting result. In order to be included in the sample, your school not only needs to be included in the *Barron's* book, but it also needs to have reported test scores. It is plausible to believe that higher quality schools are more likely to be included in the book and are more likely to report scores. Thus, these results suggest that much of the impact of private schools on completion derives from the lower portion of the college quality distribution. As I will discuss a bit, the results from column 4 support this argument.

Column 3 shows the private coefficient after controlling for a quartic in SAT/ACT scores. For both males and females, most of the estimates change very little from those in column 2. This suggests that much of the returns to attending private colleges for males are due to unobserved quality differences between the schools, and observed quality gives us little information in this regard. For women, since the estimates in column 2 were all statistically insignificant to begin with, it is not surprising that controlling for SAT/ACT score generates no substantial changes with respect to column 2. I should note though that the graduate degree completion

and post-baccalaureate schooling estimates in both columns 2 and 3 fall considerably with respect to the main results in column 1. This is also true, albeit to a lesser extent, for males. For women, the graduate degree estimate becomes statistically significantly negative.

The most important results in this table from a policy perspective are those provided in column 4. In this column the regressions include the full set of covariates along with a quartic in SAT/ACT score, and an interaction term of private and the SAT/ACT scores. For the wage/earnings results, the standard errors are too large to draw any conclusions. However, for bachelor degree completion, it is clear that private schools with lower observed quality have higher bachelor completion rates relative to public schools of the same quality, while this is does not appear to be the case for schools with higher observed quality. If one takes the estimates at face value, the completion rate impact of attending a private school is very high for schools with low SAT scores and falls as the school's SAT score reaches 900 – 1000. Coincidentally, the median SAT score for private schools in the *Barron's* guide is 920, so the returns would be positive for about half of the schools. Of course, it is unlikely that the true relationship is linear. Indeed, it seems implausible that attending a private school with an average SAT score of 1300 would negatively affect completion relative to public schools of the same quality. Thus, it is likely that the returns drop at a diminishing rate as SAT scores increase. Unfortunately, adding higher order interactions causes the standard errors to become too large to accurately assess this argument. Nonetheless, in light of the linear interaction results, it is reasonable to believe that the returns would fall as school quality rises, but are unlikely to fall below 0. I should note though, that there may be a mechanical reason for this result. As school quality increases, the completion rate reaches 1, leaving less room for improvement. However, a cursory look at graduation rates in *Barron's* for 1982 suggests that graduation rates for public schools, while they increase with SAT scores,

never get that close to 1. Ninety percent of public colleges have graduation rates less than or equal to 0.7 and half of public colleges have rates less than or equal to 0.5. Considering that the average marginal effect of attending a private school for men is .135 and for women is .089, very few private schools would get close to the absolute maximum.

3.7   Conclusion

In this chapter I examine whether attending a private college provides both monetary and non-pecuniary benefits over attending a public college. In order to account for positive selection of higher ability and more motivated students into private schools I control for a large set of observable characteristics, including a number of unique variables that proxy for motivation and ability such as the number of clubs a student participates in during high school. However, since some bias may remain, I interpret these as upper-bound estimates. I find that wage and earnings returns start out small for men, but then rise to about 11% as they age. I find little to no wage and earnings returns for women. Some of the male wage increases come from higher bachelor completion rates. Both men and women who attend private schools are much more likely to complete their degree even after controlling for a large number of observable characteristics. Completion rates for males attending private colleges are 13.5 percentage points higher than their public counterparts off of a 55.9% baseline, while the female rate is 8.9 percentage points higher off of a 53.9% baseline. I find no statistically significant impact of attending a private school on post-bachelor degree schooling or graduate school attendance, though there are some statistically significant effects on highest grade completed. In addition to the results just described, I show that women may get some benefits in the marriage market from attending a private school. Private women have a 5.7 percent lower divorce rate than their public counterparts after controlling for a large set of observable variables.

I also show that the returns to attending a private college appear to be heterogeneous with respect to observable college quality. In particular, the private premium with respect to completing a bachelor's degree is larger for schools that have low SAT/ACT scores. This suggests that a person would be much more likely to graduate college by attending the local private college rather than the local public college, but his or her likelihood of graduating will not increase much by attending private college of similar quality to a high SAT public school. In addition, I show that the results described above are likely due to unobserved characteristics of private colleges rather than observed quality measures.

While the results provided here shed light on the average impact of attending a private college rather than a public college, future work should consider the mechanism through which institutional control affects student outcomes. Additionally, more work is needed on how the effects vary by student characteristics and by school quality. Future studies should also look into the sources of any heterogeneous effects.

4. APPENDIX

A.1   Chapter 1: Severe Disciplinary Infractions

One potentially important concern with the discipline results is that, since they are based on punishments rather than observed behavior, one explanation for the drop in behavior problems is that charter schools may be more lenient when determining punishments. One way to address this concern is to find disciplinary infractions for which any leniency is highly unlikely to result in punishments less severe than an in-school suspension. Thus, in Appendix Table 7, I consider two types of these "severe" infractions. The first is a substance abuse infraction. This includes the use of any drugs or alcohol on school grounds. The second is a criminal infraction, which includes any behavior that could warrant arrest and prosecution. In the first two columns of the table I use the model provided in section 4 of chapter 1 and split the charter indicator by conversion and startup status. In levels, there are statistically significant drops in substance abuse infractions and criminal infractions in both types of charters. For value-added specifications, the estimates are still statistically significant for conversion charters, and substance abuse infraction impacts are statistically significant at the 10% level for startups. In the second two columns I drop the two periods immediately prior to entering a charter school or having a non-structural switch to account for endogenous entry. In levels, the impacts for conversion charters are statistically significant at the 10% level while impacts for startups are statistically significant at the 1% level. Value-added measures are not statistically significant except for criminal behavior in startup charters. Thus, overall,

it seems that there are substantial reductions in disciplinary infractions for these severe misbehaviors in startup charters. In addition, while some of the measures are not statistically significant, the point estimates are always negative. Thus, this would suggest that at least part of the estimated decline in discipline problems are from real behavior modifications.

A.2 Chapter 1: Imputations for Attrition Scenarios

In order to impute data for the attrition scenarios described in section 1.5.2 and Table 1.8 I use a few procedures. First the student's grade level is imputed as described for the Kyriazidou (1997) procedure in section 1.5.2. Free lunch, reduce-price lunch, other economic disadvantage, and parents being migrant workers are imputed as the student's last observation for those variables. Recent immigration status is imputed to be one if the student was less than four years from their first observation as an immigrant and zero otherwise. Attrited students are imputed to undergo a non-structural switch only in the first year after leaving ALUSD. If their predicted grade is six or nine and they do not undergo a non-structural switch, then attrited students are imputed to have undergone a structural switch.

To determine transition probabilities for attrited students between charter and non-charter schools in panels C and D I run a multinomial logit regression on non-attrited observations where the dependent variable is whether the student attends a conversion, startup, or non-charter and the independent variables are gender, race, free lunch status, reduced-price lunch status, other economic disadvantage, recent immigration status, whether a parent is a migrant worker, a set of grade-by-year indicators, a set of indicators for adjusted zip-codes, and whether the student is in the highest grade for his or her school. The adjusted zip-code is the student's zip-code of residence where any zip-codes with less than 500 students in the base sample from 1993 - 2004 are grouped into a single catch-all zip code. This is done to limit

the size of the variable set and to avoid multicollinearity issues. Attrited students have adjusted zip-codes imputed to be their last observed adjusted zip-code and have the highest grade variable imputed to be one if the student is in fifth or eighth grade. This variable is set to 0 if a student is in twelfth grade since the purpose of the variable is to capture forced school movement.

After I conduct this regression, I impute the type of school the attrited student attends. First I use the predicted values for the regression as transition probabilities for the first year of attrition. Then I generate a random number from a uniform distribution on [0,1]. This was done in Stata$^{\text{TM}}$with the seed set to 1090195. I then impute the type of school attended based off the transition probability and the random number. Then I repeat this process for each additional year the student is attrited until all attrited students have complete data through twelfth grade

A.3 Chapter 3: List of Variables Used in Main Regression Analyses

NLSY

Age in 1979

Armed Services Vocational Aptitude Battery - First 2 principal components of standardized scores.

Base salary for high school teacher.

Census region of residence.

Father's age.

Father's occupation - Census 1 digit.

Father's years of education.

Fraction of high school teachers with advanced degrees.

Gender.

High school curriculum - general, college prep, vocational, or commercial.

High school dropout rate.

High school enrollment.

Lived in urban, suburban, or rural area at age 14.

Mother's age.

Mother's occupation - Census 1 digit.

Mother's years of education.

Number of books in high school library.

Number of high school clubs participated in.

Parental structure - both parents, father only, mother only, one parent & one step-parent, other.

Per-capita income of county of residence at age 14.

Race.

Religion.

Whether somebody in household had library card when student was 14.

Whether somebody in household had magazine subscription when student was 14.

Whether somebody in household had newspaper subscription when student was 14.

Whether student ever knew father.

Whether student ever knew mother.

Year started college.

HSB

Average grades in high school.

Average HSB test percentile (1980 & 1982).

Census region of high school.

Expected major in 1982.

Family Income in 1982.

Father's education.

Father's occupation.

Fraction of high school teachers with advanced degrees.

Fraction of students in high school who are economically disadvantaged.

Gender.

High school curriculum - general, college prep, or vocational.

High school dropout rate.

High school enrollment.

How often student completes homework for class in high school.

How often student read for pleasure in high school.

Mother's education.

Mother's occupation.

Number of high school extracurricular activities participated in.

Number of high school extracurricular activities was leader in.

Parental structure - both parents, father only, mother only, other.

Per capita income of county of high school in 1982.

Race.

Religion.

Type of high school - public, alternative, Catholic, other private.

Year started college.

A.4 Chapter 3: Weighting

The NLSY is set up with weights based on years. However, the wage and earnings estimates in this chapter are based on the age of the person, not the calendar year. In order to best approximate the true representativeness of each subject, I use the following procedure to create wage and earnings weights:

(1) The sample is cut to include only those people who attended a 4-year college.

(2) Weights for each calendar year are normalized to sum to 1.

(3) Each individual weight is calculated to be the average of the weights in each year the person is

      (a) within the appropriate age range and

      (b) reports the wage or earnings measure to be greater than 0.

For educational outcome measures in the NLSY, I use the provided weights for the 2002 sample. This is to ensure that all individuals included in the analyses had sufficient time to complete their education. For High School and Beyond, I use the provided weights for the fourth (1992) follow-up survey.

Table A1 - Description of Data Elements Used in Chapters 1 and 2

At risk	At risk classification varies by grade: K-3: Student fails a state reading exam or is LEP. 4-12: Student fails any section of state exam on most recent attempt, is LEP, or is overrage for grade. A student is also classified "at-risk" if he/she is pregnant, abused, a parent, homeless, has previously dropped out, resides in a residential placement facility, attends an alternative education program, is on conditional release from juvenile corrections, or has previously been expelled.
Attendance rate	Percent of days the student is enrolled during which the student attends class.
Average grade	Annual average of quarterly (grades 1-5) or biannual (grades 6-12) grades in mathematics, reading, English, science, and social studies courses.
Bilingual education	Student is enrolled in bilingual education classes. LEP students only.
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.
English as a second language	Student is enrolled in ESL classes. LEP students only.
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 grade	Annual average of quarterly (grades 1-5) or biannual (grades 6-12) grades in mathematics courses.
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 agriculture or fishing
Reading/English grade	Annual average of quarterly (grades 1-5) or biannual (grades 6-12) grades in reading and English courses.
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.
Retention	Whether or not a student was held back one or more grades.
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 - Sample Selection Process by Year

Baseline: Students Enrolled in ALUSD Grades 1-12 in Late October

	1994	1995	1996	1997	1998	1999
Included in Base Sample	**99.9%**	**99.8%**	**99.9%**	**99.9%**	**99.9%**	**99.9%**
Reason for Base Sample Exclusion						
Missing Attendence Data	0.1%	0.2%	0.1%	0.1%	0.1%	0.1%
% of Base Sample Included in Test Sample	**-**	**-**	**-**	**-**	**71.5%**	**72.0%**
% of Base Sample Excluded from Test Sample	**-**	**-**	**-**	**-**	**28.5%**	**28.0%**
Reason for Test Sample Exclusion						
Enrolled in 12th Grade	-	-	-	-	5.4%	4.4%
Student Took Spanish Language Exam	-	-	-	-	11.6%	12.1%
Other Missing Math, Reading, or Language Score	-	-	-	-	11.5%	11.5%
% of Base Sample in Grades 1-11 Excluded from Test Sample	**-**	**-**	**-**	**-**	**24.4%**	**24.7%**

	2000	2001	2002	2003	2004
Included in Base Sample	**99.9%**	**100.0%**	**100.0%**	**100.0%**	**100.0%**
Excluded from Base Sample	**0.1%**	**0.0%**	**0.0%**	**0.0%**	**0.0%**
Reason for Base Sample Exclusion					
Missing Attendence Data	0.1%	0.0%	0.0%	0.0%	0.0%
% of Base Sample Included in Test Sample	**72.5%**	**73.0%**	**74.4%**	**74.7%**	**74.3%**
% of Base Sample Excluded from Test Sample	**27.5%**	**27.0%**	**25.6%**	**25.3%**	**25.7%**
Reason for Test Sample Exclusion					
Enrolled in 12th Grade	4.4%	4.3%	4.6%	4.7%	5.2%
Student Took Spanish Language Exam	12.5%	12.5%	11.8%	12.1%	12.0%
Other Missing Math, Reading, or Language Score	10.6%	10.1%	9.1%	8.5%	12.0%
% of Base Sample in Grades 1-11 Excluded from Test Sample	**24.2%**	**23.7%**	**21.9%**	**21.6%**	**21.6%**

Table A3- Sample Selection Process by Grade

Baseline: Students Enrolled in ALUSD Grades 1-12 in Late October	Elementary (1-5)	Middle (6-8)	High (9-11)	Twelfth (12)
Included in Base Sample	99.9%	99.9%	99.9%	99.8%
Excluded from Base Sample	0.1%	0.1%	0.1%	0.2%
Reason for Base Sample Exclusion				
Missing Attendance Data	0.1%	0.1%	0.1%	0.2%
% of Base Sample Included in Test Sample (1998 & Later Only)	69.6%	91.1%	77.5%	
% of Base Sample Excluded from Test Sample (1998 and Later Only)	30.4%	8.9%	22.5%	-
Reason for Test Sample Exclusion				
Missing Scored due to Spanish Language Exam	24.3%	0.4%	0.1%	-
Other Missing Math, Reading, or Language Score	6.0%	8.5%	22.5%	-

Note: Exclusion categories for base sample are not mutually exclusive although test sample exclusions are.

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Table A4 - OLS Regressions of Charter Impacts

	(1)	(2)	
	Any Charter	Conversion	Startup
# of Infractions	-0.238**	-0.170	-0.453**
	(0.082)	(0.104)	(0.068)
Attendance Rate (%)	0.284	0.580	-0.654
	(0.501)	(0.486)	(1.214)
Δ # of Infractions	-0.148*	-0.058	-0.465**
	(0.068)	(0.052)	(0.180)
Δ Attendance Rate (%)	0.408	0.140	1.347
	(0.294)	(0.134)	(1.059)
Likelihood of Being Retained	-0.006	-0.009#	0.007
	(0.006)	(0.005)	(0.026)
Δ Mathematics NPR	0.404	0.425	0.332
	(0.457)	(0.501)	(1.027)
Δ Reading NPR	-0.574	-0.612	-0.441
	(0.359)	(0.433)	(0.403)
Δ Language NPR	-0.117	-0.106	-0.157
	(0.275)	(0.338)	(0.363)

Robust standard errors clustered by school in parentheses. Behavior and attendance regressions contain over 1,200,000 observations. Retention regressions contain over 1,000,000 observations. Test score regressions contain over 500,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also include the following covariates: free or reduced price lunch status, other economic disadvantages, peer mobility rate, whether student undergoes a 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 A5 - Specification Tests

A. Test Score Levels

	(1)	(2)	
	Any Charter	Conversion	Startup
Math NPR Level	1.925**	2.000**	1.642
	(0.551)	(0.593)	(1.381)
Reading NPR Level	0.633	0.708	0.348
	(0.412)	(0.477)	(0.897)
Language NPR Level	0.850#	0.893	0.686
	(0.470)	(0.560)	(0.756)

B. Regressions Weighted by Number of Days Enrolled

	(1)	(2)	
	Any Charter	Conversion	Startup
# of Infractions	-0.349**	-0.222*	-0.770**
	(0.082)	(0.090)	(0.127)
Attendance Rate (%)	0.571	0.111	2.095*
	(0.342)	(0.148)	(0.978)
Likelihood of Being Retained	0.005	-0.002	0.035
	(0.010)	(0.009)	(0.038)
Δ Mathematics NPR	1.481**	1.811**	0.093
	(0.440)	(0.462)	(0.916)
Δ Reading NPR	-0.659*	-0.582#	-0.985
	(0.324)	(0.345)	(0.844)
Δ Language NPR	0.494	0.474	0.573
	(0.298)	(0.335)	(0.573)

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 800,000 observations in panel A and over 500,000 observations in panel B.   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 A6 - Fixed Effects Regressions of Charter Impact Excluding Students Who Attend Magnet Conversion Charter

	(1)	(2)	
	Any Charter	Conversion	Startup
# of Infractions	0.598	-0.067	-0.784**
	(0.114)	(0.090)	(0.107)
Attendance Rate (%)	0.598	0.110	1.490
	(0.518)	(0.220)	(1.180)
Δ # of Infractions	-0.247*	-0.038	-0.631**
	(0.117)	(0.065)	(0.202)
Δ Attendance Rate (%)	0.955	0.101	2.532#
	(0.618)	(0.140)	(1.301)
Likelihood of Being Retained	-0.001	-0.016**	0.041
	(0.013)	(0.006)	(0.042)
Δ Mathematics NPR	0.636	1.302#	-0.684
	(0.598)	(0.777)	(0.946)
Δ Rehing NPR	-0.226	0.330	-1.328
	(0.502)	(0.584)	(0.895)
Δ Language NPR	0.879*	1.178*	0.286
	(0.407)	(0.538)	(0.602)

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

Table A7 - Linear Fixed Effects Estimates of Effect of Charter Status on "Severe" Disciplinary Infractions

	Full Base Sample		Drop g - 1 & g - 2	
	Conversion	Startup	Conversion	Startup
# Substance Abuse Infractions	-0.003**	-0.013**	-0.002#	-0.012**
	(0.001)	(0.004)	(0.001)	(0.004)
# Criminal Infractions	-0.003*	-0.011**	-0.002#	-0.010**
	(0.001)	(0.002)	(0.001)	(0.002)
Δ # Substance Abuse Infractions	-0.002*	-0.007#	-0.002	-0.004
	(0.001)	(0.004)	(0.001)	(0.002)
Δ # Criminal Infractions	-0.003*	-0.005	-0.002	-0.007*
	(0.001)	(0.004)	(0.001)	(0.003)

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, and grade-by-year dummies. **, *, # denote significance at the 1%, 5%, and 10% levels, respectively.

Table A8 - Difference in Difference Estimates of Startup Charter Impact

	Startup	Postswitch	Postswitch*Startup
# of Infractions	0.176	-0.304**	-0.718**
	(0.111)	(0.042)	(0.144)
Attendance Rate (%)	-2.023**	0.322	1.640
	(0.576)	(0.352)	(1.335)
Δ # of Infractions	-0.041	-0.422**	-0.670*
	(0.073)	(0.105)	(0.276)
Δ Attendance Rate (%)	-1.066**	1.145***	3.713*
	(0.341)	(0.173)	(1.733)
Likelihood of Being Retained	0.082**	-0.065**	-0.060
	(0.021)	(0.007)	(0.044)
Δ Mathematics NPR	-0.249	1.052**	-0.066
	(0.600)	(0.279)	(1.997)
Δ Reading NPR	0.653	0.484#	-0.464
	(0.540)	(0.269)	(1.176)
Δ Language NPR	0.126	0.464#	0.775
	(0.720)	(0.262)	(1.025)

Sample is limited to observations on students in years t and t+1 that meet the following conditions: (1) student is observed undergoing a non-structural switch in year t, (2) student is in a non-charter school in year t - 1, (3) student does not undergo a non-structural switch in year t - 1. Robust standard errors clustered by school in parentheses. Behavior and attendence regressions contain over 250,000 observations. Retention regressions contain over 240,000 observations. Test score regressions contain over 90,000 observations. Exact sample sizes cannot be revealed due to confidentiality restrictions. Regressions also include the following covariates: gender, race, free or reduced price lunch status, other economic disadvantages, recent immigration status, whether a parent is a migrant worker, and grade-by-year dummies. **, *, and # denote significance at the 1%, 5%, and 10% levels, respectively.

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Table A9 - Description of Census Tract Data

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

Table A10 - Sample Reductions

NLSY79

Reason for Reduction	Observations Left
NLSY Sample	12686
Base Sample (Attended 4-Yr College)	4595
Missing ASVAB Scores	4392
No FICE or UNITID Code Provided	4338
Finished College Before 1979	4313
Main School Could Not Be Determined or Matched	3875
Not Public or Private-NFP	3823
Main School is Military Academy	3819
Final Sample	**3819**

HSB-Sophomore Cohort

Reason for Reduction	Observations Left
HSBSO Sample	14825
Did Not Participate in All Follow-ups	11142
Did not Graduate High School	10642
Base Sample (Attended 4–Yr Public or Private NFP College by 1986 & Attempted Bachelor Degree)	4237
Missing Test Battery Scores	3842
Listed a 4-Yr School with No FICE	3841
Control not Public or Private NFP	3823
Not Able to Match FICE	3788
Did Not Graduate HS in 1982	3651
Did Not Start College From 1982-1984	3544
Race Defined as "Other"	3543
HS Grades Missing	3542
Main School Military Academy	3526
Final Sample	**3526**

Table A11 - OLS Regressions of Wages and Earnings on *Private* While Controlling For Educational Outcomes - Females, NLSY

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Log(Wage) ages 24-29								
Private	0.014	0.023	0.107	0.025	0.018	0.023	0.020	0.007
	(0.030)	(0.037)	(0.066)	(0.037)	(0.033)	(0.037)	(0.033)	(0.039)
Bachelor Degree	-	0.186**	0.221**	0.175**	-	0.143**	-	0.163**
		(0.039)	(0.047)	(0.042)		(0.052)		(0.049)
Private*Bachelor Degree	-	-	-0.128	-	-	-	-	-
			(0.072)					
Bachelor Degree*Graduate Degree	-	-	-	0.063	-	-	-	-
				(0.047)				
Highest Grade Completed	-	-	-	-	0.039**	0.014	-	0.015
					(0.009)	(0.013)		(0.012)
B. Log(Wage) ages 30-35								
Private	-0.017	-0.045	-0.011	-0.042	-0.038	-0.048	-0.016	-0.056
	(0.036)	(0.043)	(0.053)	(0.042)	(0.038)	(0.042)	(0.037)	(0.038)
Bachelor Degree	-	0.219**	0.233**	0.197**	-	0.086	-	0.063
		(0.045)	(0.049)	(0.045)		(0.058)		(0.055)
Private*Bachelor Degree	-	-	-0.052	-	-	-	-	-
			(0.072)					
Bachelor Degree*Graduate Degree	-	-	-	0.125*	-	-	-	-
				(0.050)				
Highest Grade Completed	-	-	-	-	0.059**	0.043**	-	0.064**
					(0.012)	(0.016)		(0.016)
C. Log(Wage) ages 36-41								
Private	-0.007	-0.061	-0.006	-0.055	-0.033	-0.065	-0.009	-0.073
	(0.035)	(0.058)	(0.073)	(0.056)	(0.053)	(0.057)	(0.052)	(0.055)
Bachelor Degree	-	0.200**	0.222**	0.156**	-	-0.015	-	0.010
		(0.049)	(0.058)	(0.054)		(0.085)		(0.083)
Private*Bachelor Degree	-	-	-0.084	-	-	-	-	-
			(0.093)					
Bachelor Degree*Graduate Degree	-	-	-	0.242**	-	-	-	-
				(0.068)				
Highest Grade Completed	-	-	-	-	0.068**	0.069**	-	0.081**
					(0.011)	(0.021)		(0.023)

D. Log(Earnings) ages 24-29

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private	#REF! (0.041)	-0.058 (0.053)	-0.077 (0.103)	-0.056 (0.053)	-0.024 (0.047)	-0.058 (0.053)	-0.005 (0.043)	-0.058 (0.048)
Bachelor Degree	-	0.328** (0.055)	0.320** (0.052)	0.315** (0.055)	-	0.361** (0.080)	-	0.354** (0.079)
Private*Bachelor Degree	-	-	0.029 (0.121)	-	-	-	-	-
Bachelor Degree*Graduate Degree	-	-	-	0.067 (0.054)	-	-	-	-
Highest Grade Completed	-	-	-	-	0.049** (0.012)	-0.011 (0.019)	0.005	0.005 (0.020)

E. Log(Earnings) ages 30-35

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private	-0.049 (0.066)	-0.128 (0.093)	0.012 (0.103)	-0.118 (0.092)	-0.082 (0.088)	-0.133 (0.092)	-0.058 (0.082)	-0.151 (0.085)
Bachelor Degree	-	0.335** (0.063)	0.392** (0.072)	0.264** (0.066)	-	0.060 (0.100)	0.021 (0.101)	-
Private*Bachelor Degree	-	-	-0.212 (0.145)	-	-	-	-	-
Bachelor Degree*Graduate Degree	-	-	-	0.391** (0.091)	-	-	-	-
Highest Grade Completed	-	-	-	-	0.091** (0.019)	0.088** (0.028)	-	0.124** (0.031)

E. Log(Earnings) ages 36-41

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private	-0.043 (0.054)	-0.127 (0.096)	0.028 (0.113)	-0.119 (0.096)	-0.082 (0.086)	-0.134 (0.095)	-0.041 (0.082)	-0.134 (0.087)
Bachelor Degree	-	0.380** (0.079)	0.443** (0.087)	0.328** (0.085)	-	0.098 (0.119)	0.121 (0.132)	-
Private*Bachelor Degree	-	-	-0.236 (0.149)	-	-	-	-	-
Bachelor Degree*Graduate Degree	-	-	-	0.277** (0.102)	-	-	-	-
Highest Grade Completed	-	-	-	-	0.106** (0.016)	0.089** (0.025)	-	0.101** (0.033)

*, ** Denotes significance at 5% & 1% levels, respectively. Robust standard errors clustered by primary sampling unit are in parentheses. Regressions weighted by customized weights. See appendix for description. All regressions include include same covariates as described in table 5. Specifications 7 & 8 also include dummies for major fields.

Table A12 - Eigenvectors of First Two Principal Components of ASVAB
Scores

	Component 1	Component 2
Science	0.345	-0.129
Arithmetic	0.339	0.030
Word Knowledge	0.347	0.064
Paragraph Comprehension	0.327	0.179
Numeric Operations	0.288	0.455
Coding Speed	0.261	0.516
Auto & Shop Information	0.285	-0.475
Mathematics Knowledge	0.324	0.123
Mechanical Comprehension	0.313	-0.350
Electronics Information	0.323	-0.331

Table A13 - Eigenvalues of Principal Components of ASVAB Scores

Component	Eigenvalue
1	6.60
2	1.15
3	0.51
4	0.46
5	0.28
6	0.25
7	0.22
8	0.22
9	0.17
10	0.15

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