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CHARTER SCHOOLS AND LABOR MARKET OUTCOMES

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ABSTRACT

We estimate the impact of charter schools on early-life labor market outcomes using administrative data from Texas. We find that, at the mean, charter schools have no impact on test scores and a negative impact on earnings. No Excuses charter schools increase test scores and four-year college enrollment, but have a small and statistically insignificant impact on earnings, while other types of charter schools decrease test scores, four-year college enrollment, and earnings. Moving to school-level estimates, we find that charter schools that decrease test scores also tend to decrease earnings, while charter schools that increase test scores have no discernible impact on earnings. In contrast, high school graduation effects are predictive of earnings effects throughout the distribution of school quality. The paper concludes with a speculative discussion of what might explain our set of facts.

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I. Introduction

Charter schools are publicly funded, but privately managed, educational institutions that have grown in popularity across the U.S. and U.K. over the past 20 years. Currently, five percent of all American public students attend charter schools, and school districts such as New Orleans, Detroit, Camden, and District of Columbia are now majority charter. There is an intense political debate over the further expansion of charter schools. Many believe that charter schools are the most important education reform of the 21st century, and the National Alliance for Public Charter Schools estimates that there are over 1 million children on charter wait lists across America. Yet, 23 out of the 43 states that permit charter schools also impose a quantity constraint on their growth.

There are several arguments both for and against constraining charter growth (e.g. Bettinger 2000, Miron and Nelson 2001, Holmes, DeSimone, and Rupp 2003, Schneider and Buckley 2003). For example, some charter critics argue that charter schools redirect funds and students from regular public schools. According to this view, while charters may benefit their own students, they hurt those left behind by reducing district budgets and increasing the concentration of disadvantaged students. Others believe that charter schools are a risky and unproven gamble with children's lives and the government's resources. A third argument is that charter schools can only increase test scores through intense test prep (Haladyna, Nolen, and Haas 1991, Haladyna 2006), a paternalistic environment (Whitman 2008), strategic resource allocation, or blatant cheating, without instilling long-term or general knowledge in even their own students.

The case for charter expansion relies, at least in part, on the idea that high-performing charter schools can increase long-term outcomes such as employment and earnings. There is a robust literature that certain charter schools – particularly those that implement the "No Excuses" approach – increase test scores (Abdulkadiroğlu et al. 2011, Dobbie and Fryer 2011, Angrist, Pathak, and Walters 2013, Tuttle et al. 2013) and college enrollment (Dobbie and Fryer 2015, Angrist et al. 2016). Both test scores and college enrollment are correlated with labor market outcomes such as employment and earnings (e.g. Griliches and Mason 1972, O'Neill 1990, Neal and Johnson 1996, Currie and Thomas 2001, Chetty et al. 2011, Chetty, Friedman, and Rockoff 2014). Thus, charter schools – particularly No Excuses charter schools – seem likely to increase employment and earnings and potentially reduce intergenerational poverty. Consistent with this argument, Dobbie and Fryer (2015) show that students who were admitted by lottery into the Promise Academy Charter School in the Harlem Children's Zone have lower rates of female teen pregnancy and male incarceration.[1] If high-performing charters can increase long-term outcomes, then the argument for constraining charter school growth at the margin seems incongruent with increasing equality of opportunity. If,

[1]The effects of the Promise Academy on these medium-run outcomes is larger than would have been expected from the test score increases alone, suggesting that charter schools may develop non-tested forms of intelligence or change students' social networks that independently impact longer-term outcomes (Heckman and Rubinstein 2001, Heckman, Stixrud, and Urzua 2006, Segal 2008, Whitman 2008, Chetty et al. 2011, Jackson 2012). There is also evidence that students assigned to high test score value-add teachers are more likely to attend college, earn higher salaries as adults, and are less likely to become pregnant as teenagers (Chetty, Friedman, and Rockoff 2014). Additionally, attending a high-quality public school can reduce crime and increase college enrollment even when there is little impact on state test scores (Cullen, Jacob, and Levitt 2006, Deming 2011, Deming et al. 2014).

on the other hand, charter schools have no detectable long-term benefit, then there is an argument for constraining their growth until we better understand what types of schools benefit students in the long run.

In this paper, we estimate the impact of charter schools on early-life labor market outcomes using administrative data from the state of Texas. The combination of high-stakes accountability and a large and varied charter school sector makes Texas an archetypal laboratory to measure the effect of charter schools on labor market outcomes. Texas introduced high-stakes accountability in 1993 – eight years before the No Child Left Behind Act – and, two years later in 1995, enacted legislation that allowed for the opening of charter schools. The Texas charter sector has subsequently grown into one of the largest in the nation, with approximately 3.5 percent of Texas public students now enrolled in a charter school. Texas also boasts several of the most successful charter school networks. The Knowledge is Power Program (KIPP) and YES Prep schools – both winners of the Broad prize for most effective charter networks – have their flagship schools in Houston, and the IDEA Public Schools – another exemplar of the charter community – opened its first school in the lower Rio Grande Valley in 2000. Conversely, there are a relatively large number of charter schools in Texas that have been closed due to under enrollment, low student achievement, or fiscal mismanagement (Baude et al. 2014).

Ideally, we would use admission lotteries to identify the effect of charter schools on earnings. Unfortunately, Texas charter schools are only required to retain admissions lottery records for two years, and none of the schools in our sample that we were able to successfully contact had admissions lottery data for the relevant cohorts.[2] Moreover, even if these data were available for the schools in our sample, estimates using admissions lotteries are unlikely to yield sufficiently precise estimates on earnings to be informative. For example, consider if we had lottery data for all of the approximately 5,000 students in our Texas charter sample and another 5,000 lottery losers. If we assume an intra-cluster correlation 0.2 – a typical correlation observed in the charter lottery school data in other districts – we would only be able to reliably detect treatment effects of about $7,000 per year on a base of $16,515, a 42 percent increase. Even if we assume an intra-cluster correlation of zero, we could still only observe treatment effects of $1,490 per year, a 9 percent increase.

In our analysis, we therefore use a combination of matching and regression to adjust for baseline differences between charter and non-charter students. Our primary specification controls for elementary school by race by gender fixed effects and for a rich set of background characteristics including third-order polynomials in baseline math and reading test scores. We identify school-specific effects by comparing the outcomes of students who attended the same non-charter elementary school, but different middle or high schools. This specification yields relatively precise earnings estimates while

[2]We successfully contacted 28 of the 45 schools in our analysis sample. Two of the 28 schools initially reported that they had lottery data available. However, both schools discovered that the data did not actually extend to our sample period when they were preparing the lottery data for the research team. The other 26 schools we were able to contact reported not having lottery data for more than a few years or not having binding lotteries during our sample period.

controlling for any observable differences between charter and non-charter students.

The key identifying assumption of our empirical design is that gender-race-cohort-school effects and baseline controls account for all observed and unobserved differences between charter and non-charter students. Put differently, we assume unobserved determinants of students' labor market outcomes are orthogonal to our school value-added measures. Abdulkadiroğlu et al. (2011) and Dobbie and Fryer (2013) find that this empirical design yields similar test score estimates as lottery-based designs for oversubscribed charter schools in Boston and New York City, respectively. Deming (2014) demonstrates similar results using a less restrictive set of controls for regular public schools in Charlotte-Mecklenburg that have oversubscribed choice lotteries. Abdulkadiroğlu et al. (2015) show that this empirical approach works less well in Denver, with observational estimates yielding treatment effects of 0.3 standard deviations (hereafter σ) while lottery based estimates are closer to 0.5σ. In Section IV, we provide a partial test of our identifying assumption in our setting, showing that selection into Texas charter schools is remarkably similar to selection in environments in which lottery and observational strategies yield similar point estimates. Nevertheless, our estimates should be interpreted with this strong identifying assumption in mind.

A second limitation of our analysis is that we are only able to observe earnings outcomes for individuals employed in the state of Texas. For the approximately 36 percent of students in our sample with missing earnings outcomes, we do not know if they are unemployed or employed in another state. We consider the extent to which out-state migration may threaten our estimates by (1) examining the characteristics of individuals with missing earnings outcomes, (2) estimating results leaving these observations as missing, and (3) imputing missing earnings data using several different approaches. None of these results suggest that selective out-state migration significantly biases our main results.

We begin our analysis by estimating the mean impact of charter schools in our sample on test scores, educational attainment, and labor market outcomes. We find that, at the mean, charter schools in Texas are no more effective at increasing test scores or educational attainment than regular public schools. This is a recurring theme in the charter literature (e.g., Gleason et al. 2010, Baude et al. 2014). We estimate that attending a Texas charter school for one year increases state test scores by a statistically insignificant 0.006σ (se=0.005). Similarly, charter attendance increases high school graduation by 1.2 (se=0.2) percentage points, two-year college enrollment by 1.5 (se=0.3) percentage points, and four-year college enrollment by 0.3 (se=0.3) percentage points. Turning to labor market outcomes, the focus of our analysis, we find that charter attendance is associated with a \$163 (se=98) *decrease* in annual earnings, with no detectable impact on employment rates. Taken together, these results suggest little positive impact of the average charter school in Texas.

However, investigating charter effects at the mean masks considerable heterogeneity by charter type. No Excuses charter schools – schools that tend to have higher behavioral expectations, stricter disciplinary codes, uniform requirements, and an extended school day and year – are effective at increasing human capital on almost every dimension we are able to measure in our data. State test scores increase by 0.097σ (se=0.008) per year of attendance, high school graduation increases by 2.5 (se=0.3) percentage points, and enrollment in two- and four-year colleges increases by 1.2 (se=0.5) and 2.8 (se=0.5) percentage points, respectively. We also find that attending a No Excuses charter school increases persistence in both two- and four-year colleges. Yet, despite these short-run human capital benefits, the impact of attending a No Excuses charter school on earnings is only a statistically insignificant $101 (se=176) per year of attendance.

Regular charters (defined as charters not implementing the No Excuses approach) decrease state test scores by 0.054σ (se=0.006) per year of attendance, increase high school graduation by only 0.4 (se=0.3) percentage points, and decrease four-year college enrollment by 1.3 (se=0.3) percentage points. Two-year college enrollment increases by 1.6 (se=0.3) percentage points, suggesting regular charters may move students from four- to two-year colleges. Moreover, the impact of enrollment in regular charter schools on earnings is -$322 (se=114) per year of attendance.

Estimates by race yield similar anomalies. No Excuses charter schools are particularly effective at increasing the human capital of minority students. No Excuses charter schools increase the test scores of black and Hispanic students by 0.169σ (se=0.010), similar to the treatment effects observed in No Excuses schools in other districts (Abdulkadiroğlu et al. 2011, Dobbie and Fryer 2011, Angrist, Pathak, and Walters 2013). Black and Hispanic children in No Excuses charter schools are also significantly more likely to graduate from high school or enroll in a two- or four-year college. Yet, the impact on earnings is only $154 (se=215) for minority students. In other words, while there are economically and statistically significant effects of attending a No Excuses school on the test scores and educational attainment of minority students, the earnings effect is both small and measured with considerable noise.

In the second part of the paper, we examine the correlation between school-level education effects and school-level labor market effects. These estimates provide information on the effect of charter schools on labor market outcomes at other points in the distribution, not just the mean. We also allow the correlation between the school-level effects to differ above and below zero to examine trends in both the left and right tails of the distribution.

Separately estimating the school-level correlation between test scores and earnings effects above and below zero yields another set of surprising results. Below zero, a 0.1σ increase in a school's state test score effect is associated with a $984 (se=232) increase in the school's earnings effect. Above zero, however, a 0.1σ increase in a school's test score effect is associated with a statistically insignificant $169 (se=439) increase in earnings. Similar to the test score results, schools that have positive impacts on two- or four-year college enrollment have little impact on earnings, while schools that have negative effects on college enrollment also tend to have negative effects on earnings.

In sharp contrast, there is a robust positive correlation of high school graduation effects with labor market outcomes throughout the distribution. Below zero, a ten percentage point increase in a school's high school graduation effect is associated with a $912 (se=272) increase in earnings. Similarly, above zero, a ten percentage point increase in a school's high school graduation effect is associated with a $2,175 (se=761) increase in earnings. These results are consistent with the seminal work in Heckman, Lochner and Todd (2008), who argue that the internal rate of return on high school completion is between 33 percent and 52 percent for white men and between 38 percent and 56 percent for black men between 1960 and 2000.[3] These estimates also suggest that high school graduation may be an additional short-run instrument along with state test scores to evaluate the efficacy of charter schools, particularly in the right tail of the test score distribution.

We conclude with a more speculative discussion designed to help interpret our set of facts, though we are quite limited in the breadth of hypotheses we can test due to data constraints. First, we show that the age of the sample is unlikely to be driving the reported results. Estimates using only a subset of older cohorts are, if anything, stronger than the main results. Moreover, our estimates are remarkably stable over the time horizons we are able to examine. Second, we show that our results do not appear to be driven by the negative effects of high dropout rates observed among some charter schools. Estimates on program completers suggest the same qualitative conclusions. Third, we consider the extent to which one might predict our earnings effects given the observed changes across our set of human capital outcomes. Using the cross-sectional relationship between human capital outcomes and earnings in our data, we find that regular charters have smaller effects on earnings than their test score and attainment effects would have suggested. However, No Excuses schools have earnings effects that are approximately equal to what their score and attainment effects would have suggested. A similar pattern emerges at the school level. The smaller than anticipated earnings in non-No Excuses charters may be driven by at least three channels: (a) compensating differentials for students who attend negative test score schools (e.g. a terrific art program); (b) parents lack adequate information about which schools are negative test score value-added; or (c) selection bias – the types of students who knowingly attend schools with negative test score value-added are negatively selected on unobservables that are also predictive of earnings.

Our results are also consistent with the classic substitution effect in models of multitasking. Unfortunately, our ability to directly test this hypothesis is also severely limited by the data. We provide indirect evidence using detailed data on school policies and practices from Dobbie and Fryer (2013) – there is some evidence that schools that increase test scores spend less time on art, history, and foreign language. To the extent that these skills are important either directly or through the acquisition of future skills, they might explain our results. This theory, however, is unlikely to explain why students in negative value-add schools have lower than expected earnings unless, in a Lazear (2006) way, teaching to the test builds human capital among low achieving students.

In parallel work, Sass et al. (2016) estimate the impact of attending a charter high school on

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[3] These results are related to an important literature estimating the impact of school quality on labor market earnings. Changes in school inputs, such as pupil teacher ratios, annual teacher pay, and term length, help explain differences in state-specific returns to education (Card and Krueger 1992a) and the narrowing of the black-white earnings gap between 1960 and 1980 (Card and Krueger 1992b). There is also evidence of large gains of Catholic school attendance for urban minorities that would have otherwise attended poor public schools (Neal 1997, Grogger and Neal 2000). Recent work suggests students assigned to high-quality kindergarten classrooms or high test score value-add teachers in grades 4-8 are also less likely to become pregnant as teenagers, more likely to attend college, and earn higher salaries as adults (Chetty et al. 2011, Chetty, Friedman, and Rockoff 2014). There is also evidence that smaller class sizes increase educational attainment and earnings in Sweden (Fredriksson, Ockert, and Oosterbeek 2013).

college persistence and age 23-25 earnings in Florida. Their empirical design compares students who attended both a charter middle and high school to students who attended a charter middle school but non-charter high school. Using this empirical design, they find that attending a charter high school increases maximum annual earnings by over $2,000. In the specification most similar to ours where both charter and non-charter middle school students are included, the effect of attending a charter high school falls to $493. Beyond the impact of charter schools on mean earnings, there is not much overlap between Sass et al. (2016) and our approach.[4]

The remainder of the paper is structured as follows. Section II discusses the institutional setting of education reform in Texas. Section III describes our data. Section IV discusses our research design and its potential limitations. Section V presents student-level results on human capital and earnings. Section VI estimates the correlation between a school's human capital effects and its labor market effects. Section VII discusses potential interpretations of our results, and Section VIII concludes. There are three online appendices. Online Appendix A provides additional results. Online Appendix B is a data appendix that details our sample and variable construction. Online Appendix C provides additional details on the empirical Bayes procedure we use to adjust our estimated school effects for estimation error.

II. Education Reform in Texas

Texas introduced both charter schools and high-stakes accountability in the early 1990s, making it a rich setting for our set of research questions. In this section, we briefly discuss both the charter sector and the high-stakes accountability system in Texas during our sample period.

A. The Texas Charter School Sector

Texas enacted legislation allowing for the establishment of charter schools in 1995. The Texas charter sector has subsequently grown into one of the largest in the nation. Today, there are more than 600 charter schools in Texas educating approximately 3.5 percent of public school students.

The vast majority of charter schools in Texas are open-enrollment charters granted by the Texas State Board of Education.[5] Open-enrollment charter schools receive public funding but are not subject to the regulatory restrictions of regular public schools. For example, charter schools have almost no restrictions on hiring and firing teachers outside of the requirements for teachers in core areas imposed by the No Child Left Behind legislation. In practice, open-enrollment charters often hire teachers who currently lack certification or bring skills and experiences that may not

[4]Unfortunately it is not possible to replicate the Sass et al. (2016) empirical specification in our data. During our sample period, there are only two students who graduate from a charter middle school and attend a different charter high school. This result is due, at least in part, to the fact that the majority of charter schools serve both middle and high school students. See Appendix Table 1 for additional details on the charter schools in our sample.

[5]There are four types of charter schools operating in Texas: open-enrollment charters, university/college campus charters, independent school district charters, and home-rule district charters. University charters operate similarly to open-enrollment charters. Independent district charters are established by and accountable to the school districts in which they reside. Texas also allows for home-rule district charters, although none of them were established as of 2015.

be rewarded in conventional public schools (Baude et al. 2014). Open-enrollment charters are subject to the same accountability and testing requirements as regular public schools. However, these schools are accountable to the Texas State Board of Education, not the school district in which the school is located.

From 1995 to 2000, there was no statutory limit on the number of open-enrollment charters as long as 75 percent of enrolled students were classified as at risk of dropping out. Following reports of poor performance and mismanagement at some open-enrollment schools, the legislature relaxed the constraint on the number of at risk students and put a cap on the number of open-enrollment charters in 2001. Consistent with these reports, Baude et al. (2014) find that the test score value-added of Texas charter schools in the early 2000s was highly variable and, on average, lower than the regular public schools. However, by 2011 the test score value-added of Texas charter schools was roughly equal to regular public schools due to the closure of ineffective charters, improvements among existing charters, and the opening of new charters by successful charter management organizations such as the Knowledge is Power Program (KIPP), Yes Prep, and IDEA Public Schools.

We make three sample restrictions to the charter schools examined in our analysis. First, we restrict our analysis to open-enrollment charter schools that target the general population of public school students but are not run by the regular public school system. We exclude both district charters that are operated by the public school districts, and alternative charter schools that typically work with non-traditional students such as high-school dropouts and operate under different accountability standards. We also exclude charter schools for abused students, autistic students, shelters, residential treatment centers, juvenile detention centers, juvenile justice alternative education programs, virtual charter schools, and sports academies. Second, we restrict our analysis to charter schools whose oldest cohort graduated high school in or before 2005-2006. This restriction ensures that students in our sample are approximately 25 years old or older in the most recent earnings data. Third, we drop schools who have fewer than ten students enrolled during our sample period. These sample restrictions leave us with 128 school by cohort observations from 45 different charter schools. Appendix Table 1 provides additional details on our sample charter schools.

Throughout the text, we present results for three categories of charter schools: all charter schools, No Excuses charter schools, and regular charter schools. All charters refers to the complete set of charter schools in our estimation sample. No Excuses charters have higher behavioral expectations, stricter disciplinary codes, are more likely to have uniform requirements, and are more likely to have an extended school day and year (e.g. Thernstrom and Thernstrom 2003). Regular charters are defined as all charters in Texas that are not No Excuses schools. These partitions are motivated by the literature which demonstrates small, if any, gains in student achievement from attending average charter schools but a large achievement effect of attending schools that adopt the No Excuses approach. Cheng et al. (2015) conduct a meta analysis of seven studies and report that No Excuses charters improve math scores by 0.25σ and literacy achievement by 0.16σ. They also conclude that students who attend No Excuses charter schools have 0.15σ higher math achievement and 0.07σ higher reading achievement than students attending a more general sample of random assignment charter schools. We classify No Excuses schools using information from school mission statements, charter applications, and public statements. Appendix Table 1 provides a complete list of the No Excuses and regular charter schools in our sample, and Appendix B contains additional information on how we coded No Excuses and regular charter schools.

B. High-Stakes Accountability in Texas

In 1993, Texas implemented a high-stakes accountability system in order to rate both school districts and individual schools. Under the high-stakes system, school accountability ratings are based on school-wide and subgroup specific performance on mandated state tests, and school-wide and subgroup specific dropout rates if applicable. School ratings are determined by the lowest scoring test-subgroup combination (e.g math for whites), giving some schools strong incentives to focus on particular students, subjects, and grade cohorts.[6] Test-subgroup rates were calculated for African American, Hispanic, white, and economically disadvantaged students. School ratings were then published in full page spreads in local newspapers, and the lowest rated schools were forced to undergo an evaluation process with the possibility of being reconstituted or otherwise sanctioned, including an allowance for students to transfer to better-performing schools inside or outside the district. The highest rated schools were also exempt from some regulations and requirements, and in many years there have been financial awards for schools that are either high performing or showed substantial improvement (Texas Education Agency 1994, Haney 2000, Cullen and Reback 2006). No Child Left Behind incorporated most of the main features of the Texas system, including reporting and rating schools based on exam pass rates, additional reporting requirements, an increased focus on performance among poor and minority students, and raising standards over time.

There was a rapid rise in high-stakes test scores following the introduction of the high-stakes accountability system in Texas (Klein et al. 2000, Haney 2000). For example, pass rates on the 8th grade math exam rose from 58 percent for the 1994 cohort to 91 percent in the 2000 cohort. Pass rates on the 10th grade exam, a high-stakes exit exam for students during this period, rose from 57 percent to 78 percent over the same time period. Reading test scores also increased following the introduction of the high-stakes accountability system, although the magnitudes were smaller.

However, there is also evidence that the accountability system led schools to narrow their curriculum and instructional practices at the expense of low-stakes subjects, students, and grade cohorts (Haney 2000, McNeil and Valenzuela 2001, Jacob 2005, Cullen and Reback 2006, Figlio 2006, Figlio and Getzler 2006, Vasquez Heilig and Darling-Hammond 2008, McNeil et al. 2008, Jennings and Beveridge 2009). Finally, recent work suggests that there is no overall impact of

[6]The high-stakes accountability system categorized all schools as exemplary, recognized, acceptable, or low performing. In the first year of the accountability system, schools were rated as exemplary if 90 percent of each student subgroup passed the mandated state tests and the school drop-out rate did not exceed 1 percent, recognized if 65 percent of each student subgroup passed the mandated state tests and the school drop-out rate did not exceed 3.5 percent, and acceptable if 25 percent of each student subgroup passed the mandated state tests and the school drop-out rate did not exceed 6.0 percent. The standards for recognized and acceptable ratings have slowly increased over time. See Haney (2000) and Cullen and Reback (2006) for additional details.

increased pressure to achieve a higher accountability rating on postsecondary attainment and early-life earnings, with large declines in both for low-scoring students, who typically have little impact on a school's accountability rating (Deming et al. 2014). These findings are consistent with a large literature suggesting that high-stakes performance incentives may have distortionary effects (e.g. Holmstrom and Milgrom 1991, Baker 1992).

III. Data

We use administrative data from the Texas Education Research Center (ERC) that allows us to follow all Texas public school students from kindergarten to college through to the labor market. The data include information on student demographics and outcomes from the Texas Education Agency, college enrollment records from the Texas Higher Education Coordinating Board, and administrative earnings records from the Texas Workforce Commission. Appendix B contains all relevant information on the data and coding of variables. This section summarizes the most relevant information from the appendix.

A. Data Sources

The Texas Education Agency (TEA) data include information on student gender, a mutually exclusive and collectively exhaustive set of race dummies, and indicators for whether a student is eligible for free or reduced-price lunch or other forms of federal assistance, whether a student receives accommodations for limited English proficiency, whether a student receives special education accommodations, or whether a student is categorized as "at risk". The TEA data also include information on each student's grade, school, state math and reading test scores in each year, and graduation year. These data are available for all Texas public school students for the 1994-1995 to 2012-2013 school years.

Information on college outcomes comes from the Texas Higher Education Coordinating Board (THECB). The THECB collects and centralizes data for students attending Texas public universities, private universities, community colleges, and health related institutions. The data includes information on each student's enrollment, graduation, and grade in each year. All students missing from these files are assumed to have not enrolled in or graduated from college. The THECB data are available for the 2004-2005 to 2012-2013 school years.

An important limitation of the THECB data is that it only contains students who attend Texas colleges or universities. If charter schools increase the probability that a student attends out-of-state four year universities, for instance, our estimates using the THECB will be biased. To explore the robustness of our college results and measure the effect of charters on out-of-state college attendance, we supplement our analysis with data from the National Student Clearinghouse (NSC) that contain information on student enrollment for over 90 percent of all colleges and universities in the United States. The NSC data is only available from 2008 to 2009. In practice, the estimated effects of charter school attendance on college-going are almost identical in the NSC and THECB data in the years where we have both. This provides some confidence that differential out-of-state migration to attend college is not driving our results.

Employment and earnings outcomes are measured using data from the Texas Workforce Commission (TWC). The TWC data record quarterly earnings for all Texas employees, with information on approximately 12 million individuals each year. The data include information on each individual's earning, number of employers, and size of each employer. The TWC data are available from 2002 to 2014.

We assume that individuals with no reported earnings in a given year are unemployed. In Section V, we report results showing that our results are robust to excluding all zero earnings outcomes, imputing zero earnings outcomes using baseline covariates, and imputing zero earnings outcomes using both baseline covariates and realized educational outcomes.

The TEA, THECB, NSC, and TWC data are housed at the Texas ERC. Using a unique identifier based on an individual's social security number to link the data from these four sources, these data allow us to follow each Texas student from Kindergarten to college to the job market as long as this individual resides in Texas. These data are not publicly available, but interested researchers can apply to the Texas Education Research Center.

B. Sample Restrictions

We make six sample restrictions to the student data with the overarching goal of having a valid comparison sample. Table 1 provides details on the number of students dropped by each sample restriction. With no restrictions, there are 1,420,877 students in regular public schools, 1,358 students in No Excuses charter schools, and 4,905 students in regular charter schools. Column 2 omits students who did not attend a public elementary school in 4th grade. This decreases the sample by 7,646 students in non-charters, but only by 13 students in No Excuses Charters and 75 in regular charters. Column 3 leaves out students with missing baseline covariates such as gender or race. Column 4 drops students with no middle or high school test score. Column 5 drops students who transferred to an out-of-state primary or secondary school. Column 6 drops charter schools with a cohort size fewer than ten. In our final estimation sample – which includes all students for which there is a match cell on 4th grade school, cohort, gender, and race – there are 188,666 students in non-charters, 1,039 in No Excuses charters, and 3,860 students in regular charter schools. The majority of the non-charter sample was dropped due to not matching individuals in the charter sample, primarily because these students attend schools in districts without a charter school.

C. Summary Statistics

Table 2 presents summary statistics for non-charter students, students enrolled in No Excuses charter schools, and students enrolled in regular charter schools – for both the full sample (columns 1-3) and the estimation sample (columns 4-6). In the full sample, relative to the non-charter sample, regular charter schools are overwhelmingly minority, more likely to enroll students who are free or reduced-price lunch eligible or classified as needing special education accommodations, and have students with lower baseline test scores in reading and math. No Excuses charters have a higher fraction of Hispanic students, which might be driven by the IDEA public schools in the lower Rio Grande Valley, are less likely to enroll special education students, and have students with higher baseline test scores.

The summary statistics between the full sample and the estimation sample are strikingly similar on most dimensions. In the estimation sample, No Excuses charter schools are more likely to be female, more likely to be free lunch, and have higher baseline test scores than students in non-charters. The average number of years in a Texas charter school is three years for No Excuses schools and two years for regular charter schools. Students in any charter are more likely to be labeled at risk of dropping out. Hispanics are more represented in charter schools than non-charter schools. Black students in Texas are less likely to attend No Excuses schools relative to regular charters or non-charter schools.

Putting these pieces together, the summary statistics paint a familiar portrait of the characteristics of charter school enrollees. Students in charter schools are more likely to be minority, more likely to be on free lunch (a measure of poverty), and more likely to be labeled at risk of dropping out, and yet those in No Excuses charter schools enroll with higher test scores. Consistent with this, Allen and Consoletti (2007, 2008) state that charter schools attract minority students who are more probable of receiving free lunch and being at risk.

IV. Research Design

Our empirical analysis has two objectives: (1) to estimate the effect of attending charter schools on labor market outcomes such as earnings and employment, and (2) to estimate the correlation between a school's effect on labor market outcomes and its effect on human capital outcomes such as test scores. This section discusses our empirical strategy for each objective.

A. Estimating the Effect of Charter Schools on Labor Market Outcomes

Estimation Framework: We model the effect of a charter school on student outcomes as a linear function of the number of years spent at the school:

$$y_{it} = \gamma \mathbf{X}_i + \sum_s \beta_s Charter_{its} + \varepsilon_{it} \tag{1}$$

where y_{it} is the outcome of interest for student i in year t, $\mathbf{X}_i$ is a vector of baseline demographic controls such as baseline test scores, gender, race, special education status, free and reduced-price lunch eligibility, limited English proficiency, gifted designation, at risk designation, and the number of years spent at charter schools not included in our analysis sample, and ε_{it} is noise. $Charter_{its}$ is the number of years student i has attended school s by year t.

The effect of attending charter school s is β_s. Prior research has provided a set of causal estimates of this parameter for short and medium run outcomes using admissions lottery data (e.g.

Abdulkadiroğlu et al. 2011, Dobbie and Fryer 2011, Angrist, Pathak, and Walters 2013, Dobbie and Fryer 2015, Angrist et al. 2016). Unfortunately, Texas charter schools are only required to retain admissions lottery records for two years. As a result of this requirement, none of the charter schools in our sample have admissions lottery data for cohorts in our sample period. Moreover, as discussed in the introduction, using admissions lotteries are unlikely to yield sufficiently precise estimates on earnings even if these data were available for our sample.

We therefore identify the effect of each charter school using a combination of matching and regression analysis to partially control for selection into schools in our sample. Specifically, we follow Angrist, Pathak, and Walters (2013) and Dobbie and Fryer (2013) and match students attending sample charters to a control sample of regular public school students using "cells" consisting of the 4th grade school, gender, race, and cohort. Charter students are included in the estimates if they are matched to a cell with at least one regular public school student. Traditional school students are included if they are matched to a cell with at least one charter student.

We then include these "matched cell" fixed effects when estimating equation (1). We also control for third-order polynomials in 4th grade math and reading scores, 4th grade special education status, 4th grade free and reduced-price lunch eligibility, 4th grade limited English proficiency, 4th grade gifted designation, 4th grade at risk designation, and the number of years spent at charter schools not included in our analysis sample. Standard errors are clustered at the matched cell level to account for serial correlation in outcomes.

Our matching and regression approach semi-parametrically controls for any differences between gender-race-cohort-school cells that may bias our estimates by comparing the outcomes of observationally similar students who attended the same elementary school, but attended different middle or high schools. Any differences in human capital or labor market outcomes are attributed to differences in the number of years spent at each charter school.

Selective Charter Enrollment: The key identifying assumption of our approach is that our gender-race-cohort-school effects and baseline controls account for all observed and unobserved differences between charter and non-charter students. We therefore assume that unobserved determinants of students' labor market outcomes are orthogonal to our school value-added measures.

Consistent with this identifying assumption, Abdulkadiroğlu et al. (2011), Angrist, Pathak, and Walters (2013) and Dobbie and Fryer (2013) find that a similar observational empirical design yields similar test score estimates as lottery-based designs for oversubscribed charter schools in Boston and New York City, respectively. Deming (2014) finds similar results using a less restrictive set of controls for regular public schools in Charlotte-Mecklenburg that have oversubscribed choice lotteries. However, it is possible that the selection processes are different for Texas charter schools than in charter schools in Boston or New York City or regular public schools in Charlotte-Mecklenburg.

It is also possible that the selection processes for test scores and labor market outcomes may be different. For example, Chetty, Friedman, and Rockoff (2014) find that while controlling for lagged test scores effectively absorbs most unobserved determinants of student achievement on how students are sorted to classrooms, it does not account for unobserved determinants of earnings.

Specifically, Chetty, Friedman, and Rockoff (2014) show substantial "effects" of earnings value-added estimates on baseline parent income and family characteristics, indicating that their set of baseline controls is unable to fully account for sorting when estimating earnings value added. Unfortunately we do not have information on parent income or family characteristics, and are therefore unable to replicate the Chetty, Friedman, and Rockoff (2014) tests in our context.

We partially test for selection bias on observable characteristics in our data in three ways. First, in Panel A of Appendix Table 2A, we regress each baseline characteristic on the number of years at the indicated charter school type, gender-race-cohort-school effects, and all baseline controls other than the indicated dependent variable. Column 1 reports the mean and standard deviation for non-charter schools in our estimation sample. Column 2 reports results pooling all charter schools in our sample. Columns 3-4 report results for No Excuses and regular charter schools separately. Students who attend charter schools are more likely to have reached 4th grade on time – 0.8 (0.2) percentage points on a base of 83.2 percent. Yet, due to the precision of our estimates, this difference is statistically significant. Similarly, both 4th grade LEP and math scores differ between students in charter and non-charter schools. As before, they are statistically significant but do not seem economically meaningful.

Second, Panel B of Appendix Table 2A conducts a number of falsification tests using outcomes that we do not directly control for: 3rd grade math and reading scores, and an indicator for having been held back before 3rd grade. On all but one outcome – 3rd grade math scores for No Excuses charters – there is no relationship between charter attendance and these baseline characteristics. Students who attend No Excuses charters have 0.023σ (se=0.012) higher math test scores. This is substantively small and marginally significant.

Finally, Panel C of Appendix Table 2A conducts a similar exercise using predicted earnings and employment for ages 24-26. We predict earnings using the relationship between actual earnings and employment with the baseline controls used in equation (1). Consistent with the previous results, we find statistically significant but economically small differences between those who attend charters and those who attend non-charters. The predicted difference in earnings between charter and non-charter students is 0.001 percent (a $28.68 difference on a non-charter mean of $22,478.66). It therefore appears that, because of our large sample, several coefficients are statistically significant but none of them are economically large.

To better understand how to interpret these results, we conduct an identical exercise in an environment where we believe both lottery-based and observational estimates of charter effectiveness have been shown to be highly correlated. Appendix Table 2B replicates our specifications from Appendix Table 2A using information from NYC charter schools where Dobbie and Fryer (2013) have shown that lottery-based and observational estimates are highly correlated. If anything, Appendix Table 2B reveals more selection on charter attendance in NYC than in Texas. We interpret these results as suggesting that there is some modest selection into charter schools based on observable characteristics, but that our estimates from equation (1) are unlikely to be significantly biased.

Selective Attrition from the Earnings Data: Another concern is that charter students may be either more or less likely to leave the state, and hence more or less likely to be missing from our earnings data. If charter students are more or less likely to migrate out of Texas, or the types of charter students that migrate out of Texas are different than the types of non-charter students who migrate, estimates of equation (1) may be biased.[7]

Unfortunately we are unable to directly observe out-state migration in our data. We therefore explore attrition from of our sample in three ways. First, Appendix Table 3 examines the characteristics of charter and non-charter students with no observed earnings outcomes. While far from an ideal test, these results help us understand the types of individuals for whom we do not observe earnings, and whether selective attrition is likely to be a serious concern in our setting. Similar to the test of selective attrition into charter schools, there are small differences in six out of seventeen variables that are statistically significant but substantively small. Female students who attend non-charter schools are three percent less likely to be in the earnings data than male students. Among charter students this number is 2.8 percentage points – the p-value of the difference is 0.001. There is a similar pattern among the other variables that show statistical differences.

Second, we test whether charter students are more likely to attend an out-of-state college in the two cohorts where NSC data – which include college enrollment outcomes from all states – is available. Appendix Table 4 presents these results. At the mean, charter students are no more likely to attend two- or four-year schools in Texas or two-year colleges outside of Texas. They are, however, 0.9 (se=0.2) percentage points more likely to attend out-of-state four-year colleges. The largest coefficients in the table are from No Excuses students who attend out-of-state colleges. They are 1.8 (se=0.03) percentage points more likely to attend an out-of-state four-year college compared to a non-charter mean of 4.4 percentage points.

We also show in Section V that our earnings results are robust to (1) excluding all zero earnings outcomes, (2) imputing zero earnings outcomes using baseline covariates, (3) and imputing zero earnings outcomes using both baseline covariates and observed attainment outcomes. We interpret these results as suggesting that any selective out-state migration is likely to be modest in our sample.

B. Correlation of School Effects on Earnings and Academic Outcomes

Estimation Framework: We estimate the correlation between a school's effect on labor market outcomes and its effect on short-run outcomes such as test scores using the following specification:

$$\beta^y_{cs} = \lambda\beta^t_{cs} + \varepsilon_{cs} \tag{2}$$

[7] More generally, one can compare the types of attrition observed in our data with other well-known datasets. For instance, in the Current Population Survey (CPS), we find that 8.4 percent of 23-26 year olds had migrated out of Texas sometime during the five years prior to taking the CPS. Individuals that attended at least some college, served in the armed forces, and were 23-26 in 2005 (as compared to 2015) were more likely to migrate out of Texas. We also find that the employment rate among 23-26 year olds in Texas is 70.8 percent in the CPS. For minority youth in Texas, the rate is 65.5 percent. In comparison, we observe non-zero earnings for 64.1 percent of individuals in our Texas data. This is strikingly consistent with our data.

where β_{cs}^{y} is a school's effect for cohort c on labor market outcomes y, and β_{cs}^{t} is a school's predicted effect on short-run outcomes such as test scores. We report results using a simple linear relationship, and a linear spline with a change in slope when the short-run effect is equal to zero. The linear spline results will help us understand whether low- and high-performing schools (as measured by short-run test score or attainment outcomes) have different effects on long-run outcomes.[8] We estimate equation (2) at the school-cohort level and cluster standard errors at the school level.

Mechanical Bias in Student-Level Errors: Following Chetty, Friedman, and Rockoff (2014), we calculate our academic school effects using a leave-cohort-out measure. Specifically, school effects in a given cohort c are predictions of school quality for cohort c based on outcomes from all cohorts excluding outcomes from cohort c. For example, when predicting a school's effects on the outcomes of students graduating in 2002-2003, we estimate β_{cs} based on academic outcomes from students in all cohorts of the sample except 2002-2003. Further, we maximize precision by calculating these leave-out school effects estimates using data from all cohorts graduating high school, not just the subset of older cohorts for which we observe earnings outcomes.

Using a leave-cohort-out estimate of β_{cs} is necessary to obtain unbiased estimates of equation (2) because of correlated errors in students' short-run outcomes and later outcomes. Intuitively, if a school is randomly assigned unobservably high-ability students, its estimated impact on short-run outcomes will also tend to be higher. The same unobservably high-ability students are likely to have high levels of earnings, generating a mechanical correlation between short-run impacts and earnings impacts even if the school has no causal effect. The leave-cohort-out approach eliminates this correlated estimation error bias because β_{cs} is estimated using a sample that excludes the observations on the left hand side of equation (2).

Attenuation Bias from Estimation Error: A final concern is estimation error. The median school in our sample has fewer than 70 students in the relevant cohorts, and we observe fewer than 50 students in the relevant cohorts for 38.6 percent schools in our sample. The stochastic nature of our outcomes combined with the relatively small number of students in some schools means that some of our school effects will be estimated with considerable error, leading to attenuation bias in our analysis of the relationship between these effects and outcomes.

We apply an empirical Bayes procedure to adjust for estimation error in our estimates of β_{cs} (e.g. Morris 1983). The empirical Bayes procedure is based on the idea that there is likely to be positive (negative) estimation error if a school's estimated effect is above (below) the mean school effect. The expected school effect is therefore a convex combination of the estimated school effect and the mean of the underlying distribution of school effects. The relative weight on the estimated school effect is proportional to the precision of the estimate, which is based on the standard error

[8] We formally test for the location of the trend breaks in Appendix Figure 1. Specifically, we plot the R^2 from equation (2) estimated for every possible break point in the data. The natural break point is slightly below zero for most specifications – though simply assuming zero is a reasonable approximation. We prefer to use zero because of ease of interpretation and consistency across outcomes.

of the coefficient estimate. Online Appendix C provides a detailed description of this procedure in our context.

V. The Impact of Charter Schools on Human Capital and Labor Market Outcomes

Below, we provide a series of estimates of the impact of charter schools on human capital outcomes such as test scores and college enrollment, and labor market outcomes such as earnings and employment.

A. Human Capital Outcomes

Table 3 presents estimates of equation (1) for math scores, reading scores, and both math and reading scores together. The odd numbered columns control for the baseline characteristics in Table 2, third-order polynomials in 4th grade math and reading state test scores, number of years spent at charter schools not included in our analysis sample, and 4th grade school x cohort fixed effects. The even numbered columns add 4th grade school x cohort x race x gender fixed effects – the specification that aligns with the lottery estimates in Abdulkadiroğlu et al. (2011), Angrist, Pathak, and Walters (2013), and Dobbie and Fryer (2013). We report the coefficient on the number of years attended at the indicated charter school and standard errors clustered at the 4th grade x cohort level. Appendix Tables 5-7 report results using an indicator for having ever attended the indicated charter school as an alternative.

Consistent with the prior literature, the mean impact of charter schools on test scores is roughly zero (e.g. Gleason et al. 2010, Baude et al. 2014). In our preferred specification with 4th grade school x cohort x race x gender fixed effects, we find that the impact of attending a charter school for one year is -0.009σ (se=0.006) on math scores and 0.022σ (se=0.005) on reading scores. Stacking both math and reading test scores, we find that attending a charter school for one year increases test scores by 0.006σ (se=0.005). None of the estimates suggest economically large impacts of charter attendance on test scores at the mean.

However, and again consistent with the prior literature (e.g. Abdulkadiroğlu et al. 2011, Angrist, Pathak, and Walters 2013, Dobbie and Fryer 2013), the test score estimates differ markedly for No Excuses and non-No Excuses charter schools. In our preferred specification, the impact of attending a No Excuses charter school for one year is 0.095σ (se=0.009) in math, 0.099σ (se=0.008) in reading, and 0.097σ (se=0.008) stacking both math and reading scores. In contrast, the impact of attending a regular, or non-No Excuses charter school, is -0.078σ (se=0.007) in math, -0.029σ (se=0.007) in reading, and -0.054σ (se=0.006) stacking scores from both subjects.[9]

[9] Appendix Figure 2 plots school-specific estimates of the test score effects for both No Excuses and regular charter schools. We estimate the school-specific estimates using equation (1) and adjust the coefficients for estimation error using the procedure outlined in Online Appendix C. The reported means are weighted by the number of students at each school in the earnings effects estimation sample. The distribution of regular charter school effectiveness is similar to distribution of charter school effectiveness in Gleason et al. (2010), providing more evidence that the Texas charter sector is not an outlier. Another interesting feature of Appendix Figure 2 is the consistency of the No Excuses

Table 4 presents similar estimates for high school graduation, two-year college enrollment, and four-year college enrollment. Appendix Table 8 presents analogous results for the number of years enrolled at two- and four-year colleges.[10] At the mean, the effect of attending a charter school is 1.2 (se=0.2) percentage points for high school graduation, 1.5 (se=0.3) percentage points for two-year college enrollment, and 0.3 (se=0.3) percentage points for four-year college enrollment. Consistent with the test score results from Table 3, the effects differ by charter type, particularly for four-year college enrollment. No Excuses charters increase four-year college enrollment by 2.8 (se=0.5) percentage points, compared to -1.3 (se=0.3) percentage points for regular charters. High school graduation effects are also larger for No Excuses and regular charters, while two-year college enrollment effects are similar. These results are consistent with No Excuses charters increasing the number of students attending all types of colleges, while regular charters shift students who otherwise would have attended a four-year school to a two-year school.[11]

The consistency between our results and the previous literature – much of which employs a lottery-based design – for the test scores and attainment results provides a bit of confidence that our matched cell research design is valid in our setting. Moreover, if anything, our test score effects for No Excuses charters are smaller than those found in much of the literature. This too, is a similar feature of analyses that have employed both lottery-based and matched-cell designs. In Dobbie and Fryer (2013), the matched cell specification estimates are biased downwards and the correlation between lottery based estimates and observational estimates is 0.768 for math test scores and 0.526 for reading test scores.

B. Labor Market Outcomes

Table 5 presents estimates of equation (1) for average earnings and employment for ages 24-26.[12] Columns 1-2 present earnings results using our baseline set of controls and with matched cell fixed effects, respectively, mirroring the specifications used in Tables 3-4. At the mean, the effect of attending a charter school for one year is -\$163.65 (se=98.86). Thus, if a student attended a charter school for 5 years, expected annual earnings would be over \$800 lower. Consistent with our test score and attainment results, No Excuses charters have better outcomes. The impact of attending a No Excuses charter for one year is a statistically insignificant \$101.04 (se=176.12). Regular charters

school test score effects, with all of the point estimates concentrated between zero and 0.25σ. An important caveat to these results is that the distribution adjusted for estimation error has lower variance than the true distribution of school-specific estimates. See Jacob and Rothstein (forthcoming) for additional discussion of this issue.

[10] Deming et al. (forthcoming) estimate that less than nine percent of the graduating students in the Texas ERC data attend out of state colleges or universities and their test scores are drawn from the top deciles of the academic distribution – even conditional on college enrollment. In Appendix Table 4 we use data from the National Student Clearinghouse to demonstrate the robustness of the college enrollment results to out-state migration.

[11] Following our test score results from Appendix Figure 2, Appendix Figure 3 plots school-specific estimates of the attainment effects for both No Excuses and regular charter schools. There is significant variation in the school-specific estimates, with the effects centered at or below zero for regular charters. For No Excuses charters, the effects are centered around zero for high school graduation and two-year college enrollment, and above zero for four-year college enrollment.

[12] Appendix Table 9 presents results for the maximum observed earnings for ages 24-26. The results are nearly identical to the average earnings presented in Table 5.

have a surprisingly negative impact on earnings of -\$322.28 (se=114.52).[13] Results for employment are less precise and are not statistically distinguishable from zero for either No Excuses or regular charters.[14]

As discussed in Section III, an important limitation of our data is that we only observe the earnings of individuals working in the state of Texas. If No Excuses charter schools increase or decrease the probability of leaving Texas, our estimates may be biased. This problem is analogous to the well-known missing earnings problem in labor economics (see Blundell and MaCurdy 1999 for a review). Columns 3-5 of Table 5 explore the robustness of our earnings results to various assumptions about missing earnings observations. Column 3 presents results dropping all zero earnings observations. In this scenario, the effect that is being estimated is the impact of charters on earnings, conditional on employment. Column 4 imputes the missing earnings observations using the baseline characteristics in Table 2, third-order polynomials in 4th grade math and reading state test scores, the number of years spent at charter schools not included in our analysis sample, and 4th grade school x cohort x race x gender fixed effects. Column 5 imputes the missing earnings observations using the same baseline characteristics and the observed test score and academic attainment outcomes from Tables 3-4. Specifically, for both imputation procedures, we regress non-missing earnings on all characteristics. We then take the median predicted earnings in each 4th grade school x cohort x race x gender cell. Results are similar using the 25th or 75th percentile of each 4th grade school x cohort x race x gender cell instead.

Our earnings results are broadly similar regardless of how we deal with missing earnings. The estimated effect of No Excuses charters is modestly more positive when dropping missing earnings observations or imputing outcomes, while the estimated effects of regular charters is somewhat more negative. The largest estimates (in absolute value) suggest that No Excuses charters increase earnings by a statistically insignificant \$237.44 (se=152.79) and that regular charters decrease earnings by \$443.56 (se=138.42). In results available upon request, we find nearly identical results if we impute earnings at different percentiles of the predicted earnings distribution. We also estimate results using a grouped Heckit procedure (e.g. Gronau 1974, Heckman 1979). Specifically, for each 4th grade school x cohort we compute the fraction with valid earnings data. We then include the implied control function for each group as a control variable to re-center the residuals in our sample. Using this approach, we find nearly identical results as those reported in Table 5.

Broadly, any selection correction or imputation method that uses the differential attrition from earnings data between charters and non-charters will lead to qualitatively similar results because, as discussed in Section IV, there is little differential attrition on average or across observable characteristics. Importantly, however, any "worse case" type bound that assumes the missing

[13] Regular charters can be further subdivided into three categories: college preparatory charters, special mission charter schools (e.g. religious or STEM education), and the remaining we categorize as miscellaneous. In results available upon request, we find that the negative earnings effects are driven almost entirely by special mission and miscellaneous charter schools.

[14] Appendix Figure 4 plots school-specific estimates of the earnings effects for both No Excuses and regular charter schools. There is significant variation in the school-specific estimates, with the effects centered at or below zero for regular charters and just above zero for No Excuses charters.

observations from non-charter schools are significantly lower earning earners will substantially alter the results. For example, our estimates will significantly understate the true effect of charter schools if all missing charter observations are due to out-state migration for high paying jobs and all missing non-charter observations are due to incarceration. Our robustness results should be interpreted with this caveat in mind.

C. Subsample Results

Appendix Tables 10A-10C report estimates by gender, baseline test scores, and race. At the mean, charter schools are equally (in)effective at educating male and female students and high- and low-skill students. For gender, the only variable in which there is a statistical difference is high school graduation – charter schools and, in particular No Excuses charter schools, have a larger impact on the likelihood that male students will graduate from high school. There is no difference in the impact of charter schools on earnings by gender, however. For baseline test scores, high-skill students are also more likely to experience gains in high school graduation. Earnings effects are also larger for high-skill students, but the difference is not statistically significant.

More interesting results emerge when we divide the sample by ethnicity. Of the four education outcomes we consider, three are statistically larger for black and Hispanic students. For the average charter school, the impact on test scores is 0.030σ (se=0.006) for blacks and Hispanics and -0.040σ (se=0.007) for whites and Asians. The difference, 0.070σ, is statistically significant at conventional levels. Treatment effects on the attainment outcomes are similar. The only academic outcome for which charter schools do not produce better results for blacks and Hispanics is two-year college enrollment. Consistent with these markedly different test score and attainment results, the impact on average earnings is \$41 (se=114) for blacks and Hispanics and -\$509 (se=196) for white and Asians.

No Excuses schools display a similar pattern for educational outcomes, though the effect sizes are larger. For example, the impact of No Excuses charter schools on test scores is 0.169σ (se=0.010) for black and Hispanic students and -0.001σ (se=0.009) for white and Asian students. Our estimates imply that if a black or Hispanic student spends 5 years in a No Excuses charter school, she or he would have 0.845σ higher test scores. These effects are similar in size to estimates of No Excuses schools in urban environments (e.g. Angrist, Pathak, and Walters 2013) and efforts to transport the best practices from these schools (Fryer 2014).

However, the positive human capital benefits of No Excuses schools do not translate into measurable improvements in earnings or employment for blacks or Hispanics, though the effects are estimated with considerable error. For blacks and Hispanics, the coefficient on earnings from No Excuses charters is \$154.35 (se=215.09). For whites and Asians, the earnings effect from No Excuses charters is \$30.37 (se=319.62). The p-value on the difference is 0.757 for No Excuses charters. Of course, the 95 percent confidence interval of these estimates contains modest effect sizes, but these results are surprisingly small compared to the rhetoric on the power of charter schools to increase intergenerational mobility among poor minority students.

VI. Correlation of School Effects and Labor Market Outcomes

Our results thus far have used individual-level data to estimate the relationship between charter school attendance at the mean and human capital and labor market outcomes. In this section, we generalize this approach by exploring the correlation between school-specific effects on human capital and labor market measures.

Figure 1 plots school-specific estimates for labor market outcomes and test scores. Each point represents the mean effect (across all available cohorts) for a school adjusted for estimation error as described in Online Appendix C. Figure 1 also presents estimates of equation (2) where we allow the relationship between labor market effects and test score effects to differ above and below zero. Equation (2) is estimated at the school x cohort level using the "leave-out" procedure described in Section IV. Standard errors are clustered at the school level.

Estimating the correlation between test scores effects and earnings effects yields starkly different results above and below zero. For schools with negative value-added on test scores, a 0.1σ increase in the school's test score effect is associated with a \$984.91 (se=232.94) increase in the school's earnings effect. For schools with positive value-added on test scores, however, the correlation between a school's test score effect and earnings effect is statistically zero. Specifically, a 0.1σ increase in a school's test score effect, above zero, is associated with a \$169.40 (se=439.07) increase in earnings. Figure 1B suggests a similar, if more muted, pattern for employment effects, and Appendix Figure 5 shows identical results when math and reading scores are considered separately. Taken at face value, these results suggest that negative test score effects are a strong indicator of school failure, but positive test score effects are a poor indicator of school success.

Figure 2 presents analogous results for high school graduation and two- and four-year college enrollment. For both two- and four-year college enrollment, the patterns are identical to those for test scores. Schools that have negative impacts on these post-secondary attainment measures also tend to have negative impacts on earnings and employment. For example, for schools with negative value-added on test scores, a ten percentage point increase in a school's four-year college enrollment effect is associated with a \$2,104.36 (402.35) increase in the school's earnings effect. For schools with positive value-added on four year-college enrollment, however, a ten percentage point increase in a school's four-year college enrollment is associated with only a \$145.09 (se=777.88) increase in the earnings effect.

The only academic outcome with a positive correlation with earnings (or employment) both above and below zero is high school graduation. Below zero, a ten percentage point increase in a school's high school graduation effect is associated with a \$912.67 (se=272.10) increase in the earnings effect. Above zero, a ten percentage point increase in a school's graduation effect is associated with a \$2,175.15 (se=761.02) increase in the earnings effect. These results are consistent with Heckman, Lochner, and Todd (2008), who argue that the internal rate of return on high school effect is between 33 percent and 52 percent for white men and between 38 percent and 56 percent for black men between 1960 and 2000. Moreover, taken at face value, our results suggest that high school graduation may be a better short-run instrument, at least as compared to state test scores, to evaluate the efficacy of charter schools, particularly in the right tail of the achievement distribution.

VII. Interpretation

Our analysis has established six facts. First, at the mean, charter schools in Texas have little impact on test scores, educational attainment, or earnings. Second, No Excuses charter schools increase test scores and educational attainment, but have a small and statistically insignificant effect on earnings. Third, regular charters modestly increase two-year college enrollment but decrease test scores, four-year college enrollment, and earnings. Fourth, the impact of charter schools on employment is small and statistically insignificant throughout. Fifth, at the school level, charter schools that decrease test scores or college enrollment also tend to decrease earnings and employment, while charter schools that increase test scores or college enrollment demonstrate no measurable earnings or employment benefits. Sixth, there is a robust positive correlation of school-level high school graduation effects with school-level labor market effects throughout the distribution.

In this section, we provide a speculative discussion of the potential mechanisms that could explain these six facts. Unfortunately, data limitations prevent us from directly testing a large set of potential mechanisms. For example, it is possible that the null effect of No Excuses charter schools on earnings is due to the fact that neighborhood quality and social networks are left unchanged. Consistent with this idea, Chetty, Hendren, and Katz (2016) show that moving disadvantaged youth to lower poverty neighborhoods has a significant impact on future earnings, despite little measurable effect on human capital outcomes. Our data do not allow us to observe students' home addresses or neighborhood quality, making it impossible to test whether students living in better neighborhoods benefit more from charter attendance. These data limitations mean that we are more confident with our set of facts than our ability to credibly identify the mechanisms that generated them.

Yet, we can still make some progress by exploiting the kink around zero in the correlation between school human capital effects and school earnings effects. Many intuitive theories conflict with this result, allowing us to make at least some progress in identifying potential mechanisms. For example, it is unlikely that neighborhood quality is an important mechanism driving our results, as the effects of better neighborhoods are likely to be monotonic through zero. Specifically, while it is possible that the importance of better neighborhoods can explain why the correlation between school test score effects and school earnings effects is zero for schools that have a positive value-add on test scores, it seems difficult to explain the strong correlation for these effects among schools that have negative test score value-add.

Thus, any potential mechanism must have different predictions for schools that increase and decrease test scores. Below, we explore four such potential explanations: (1) the relative young age of our sample, (2) the negative effects of high dropout rates at high-performing charter schools, (3) low returns to human capital in Texas, and (4) multitasking. Another theory potentially consistent with the data is that Texas labor markets do not reward high test scores. This is inconsistent with our estimates of the return to test scores calculated with Texas data that are strikingly consistent with those calculated in Chetty, Friedman, and Rockoff (2014).

A. Age of the Sample

One potential explanation for our results is that the individuals in our sample are too young for us to accurately measure their earnings. It is possible that students who attended charter schools with high test score value-added will eventually earn more, but we observe them in our data too soon after schooling to capture these increased earnings. This concern is particularly reasonable given the fact that earning trajectories are typically increasing in years of education, and that No Excuses charters increase both two- and four-year college enrollment. If No Excuses students are on an upward trajectory relative to the comparison group, then we may underestimate the charter effect on earnings.

We explore the robustness of our results to this concern in three ways. First, we explore the typical earning trajectories of students in our sample. Appendix Figure 6 plots average earnings by educational attainment for students in our sample who are at least 30 years old. We plot results both with and without zero earnings observations included. Not surprisingly, earnings for individuals with at least four years of college are relatively low for ages 19-22 when these individuals are likely still enrolled in school. Earnings for these individuals sharply increase for ages 22-26, leveling off for ages 26-30. In contrast, earning trajectories are relatively stable over all ages for individuals with some college, only a high school diploma, or less than a high school diploma. Importantly, average earnings for college educated students exceed the average earnings of other students by age 23, providing some assurance that our sample is not too young.

These results suggest that since students at No Excuses and other high test score value-added charter schools are more likely to enroll in a four-year college, their earnings schedule is likely flatter than regular charter students through age 22. Their earnings are then likely to increase sharply until about age 26. All else equal, this suggests that measuring earnings outcomes for ages 24-26, as we do in our analysis, is likely to modestly understate the earnings benefits of attending a high test score value-added charter school. We also find that the correlation of age 26 earnings with age 30 earnings is 0.673 if zeros are included and 0.613 if zeros are not included (see Appendix Table 11). These results are again consistent with our main earnings measure accurately measuring labor market outcomes.

Appendix Table 12 presents additional evidence on this issue by presenting results for earnings at ages 28-30, when observed earnings are more indicative of lifetime earnings (Neal and Johnson 1996, Chetty et al. 2014). Columns 1-2 present results using true earnings for the subset of individuals we observe at ages 28-30. Columns 3-4 present results for our full sample using predicted earnings at ages 28-30. We calculate predicted earnings using indicators for high school graduation, two-year college enrollment, four-year college enrollment, and employment from ages 24-26; cubic polynomials in grades 5-11 math and reading scores, years of two-year college, years of four-year college, earnings from ages 24-26, and median industry earnings from ages 24-26; and the baseline controls used in all other specifications. If anything, the results are more exacerbated when estimating our earnings effects on older cohorts.

The coefficient on any charter is twice as large as the full sample, driven by large negative results from attendance in regular charter schools. The impact of attending a regular charter school for one year for age 28-30 earnings is -$753.76 (se=229.68). The coefficient on No Excuses attendance is positive but measured with considerable noise at $308.09 (se=477.68). Appendix Figure 7 presents results separately for each year relative to high school graduation. Consistent with the results from Appendix Table 12, earnings and employment effects are constant for No Excuses charter schools from years 5 to 10. The effect of regular charters is, if anything, becoming more negative from years 5 to 10. None of the results suggest that our main results understate the effects of No Excuses charters.

Finally, we investigate whether charter school students are more likely to be employed in high growth industries that may not be reflected in their early-life earnings. Consider the following thought experiment. Imagine that 26-year-olds in the biotech industry earn similar earnings as 26-year-old managers of McDonald's. In our analysis thus far, we mask these differences. Yet, the expected lifetime earnings of an entry-level biotech employee are higher than the lifetime earnings of a McDonald's manager. Investigating industry earnings at different percentiles will capture these differences. Appendix Table 13 presents estimates of the effect of charter school attendance on industry earnings measured at the 25th, 50th, and 75th percentiles. Of the eighteen coefficients estimated in the table, not one of them is statistically significant. If anything, all charter students seem to be in lower paying industries.

B. High Dropout Rates Among High-Performing Charter Schools

A second potential explanation for our results is that high-performing charter schools only help the select subset of students that are able to endure a more rigorous education program. In this scenario, our estimates combine the positive effects of "completing" a charter education with the negative effects of dropping out early. While our empirical design accounts for the number of years at each charter school, it is possible that students do particularly poorly after leaving a particular charter school, and that this masks the true potential of these schools.

We provide evidence on this potential mechanism in Appendix Table 14. For each human capital and earnings outcome, we estimate the effects separately for students who completed a charter (i.e. those who enrolled in the highest grade offered by a particular charter school) and those that failed to complete (i.e. those who never enrolled in the highest grade offered by a particular charter school). On almost every dimension of human capital, students who complete No Excuses schools have better results than those who did not complete the charter program through the last grade offered. Yet, again, although the coefficients are markedly different and even of opposite sign, earnings are measured with such error that we fail to reject that the average earnings of students who do and do not complete are the same. Regular charters display the opposite pattern – students who complete have lower human capital and lower earnings, though we fail to reject the null of no difference on earnings or employment.

C. Cross-Sectional Correlations Between Human Capital and Labor Market Outcomes

A third, very simple, explanation for our results is that the estimates are consistent with the returns to human capital observed in cross-sectional data. To put the magnitude of our estimates in perspective, Appendix Table 15 describes the cross-sectional correlations between academic achievement in grades 5-10 and various adult outcomes in our data.[15] There is a strong correlation between academic achievement and high school graduation, college attendance, earnings, and employment both with and without additional controls. With no additional controls, a one σ increase in grade 5-10 reading scores is associated with a \$3,545.89 (se=42.99) increase in earnings at ages 24-26, and one σ increase in grade 5-10 math scores is associated with a \$4,129.76 (se=41.74) increase. Conditional on our standard set of the baseline variables and matched cell fixed effects, a one σ increase in reading scores is associated with a \$1,547.60 (se=58.93) increase in earnings, and one σ increase in math scores is associated with a \$2,343.50 (se=56.87) increase. These estimates represent 9.3 and 14.1 percent increases from the sample mean, respectively. These cross-sectional estimates are strikingly consistent with the cross-sectional correlations between test scores and earnings estimated in other settings. For instance, using the estimates from Chetty, Friedman, and Rockoff (2014), a one standard deviation gain in test scores is associated with an 11 percent increase in earnings.

In our data, students who attend No Excuses schools demonstrate an increase in test scores of approximately 0.1σ per year. Thus, based on the cross-sectional estimates from Appendix Table 15, we would expect an approximately 0.9 to 1.4 percent increase in earnings for each year a student attends a No Excuses school. In practice, we estimate that attending a No Excuses charter school increases earnings by about 1.1 percent for each year of attendance, or about the same as we might have expected given our cross-sectional results. For regular charter schools, we estimate that test scores decrease by approximately 0.05σ per year of attendance, suggesting an earnings decrease of about 0.45 to 0.7 percent per year. In practice, we estimate effects of approximately negative 1.95 percent per year, larger than we would have expected.

Comparing these results at the mean may mask interesting heterogeneity that is captured in our school level results. Figure 3 plots the relationship between school-level pooled test score effects and school-level earnings effects (similar to Figure 1), but imposes a line which represents the cross-sectional relationship estimated between test scores and earnings in Chetty, Friedman, and Rockoff (2014) and our internal calculations using data from Texas. For charter schools that have negative effects on student achievement, the correlation between test score and earnings effects is steeper than the cross-sectional correlation between test scores and earnings reported by Chetty, Friedman, and Rockoff (2014) or computed with data from Texas – implying that their students have more negative earnings than one would expect given the test score decrease. In contrast, the

[15] Appendix Figure 8 presents analogous non-parametric results graphically. The main conclusions remain the same in these results.

correlation between test score and earnings effects is approximately equal to the cross-sectional prediction. The smaller than anticipated earnings in non-No Excuses charters may be driven by any of three channels: (a) compensating differentials for students who attend negative test score schools (e.g. a terrific art program); (b) parents lack adequate information about which schools are negative test score value-added; or (c) selection bias – the types of students who knowingly attend negative test score value-added schools are worse on unobservables that are predictive of earnings. Unfortunately, we cannot test between these mechanisms.

D. Multitasking

It is also plausible that the positive value-added charter schools have learned to improve test scores, but (un)intentionally substituted away from other non-tested skills that have value in the labor market (i.e. creativity or adaptation to language). In this case, schools that are ineffective at increasing test scores are unwilling to tradeoff the skills they believe are important for long-term success to demonstrate short term gains on particular measured skills. Conversely, schools that are effective at increasing short-run measured test scores are willing to make that tradeoff. It is also plausible that schools that increase test scores simply work harder or smarter and their gains will have no deleterious impacts on other non-measured skills. This theory may have difficulty explaining why students in negative value added schools have lower than anticipated earnings unless "teaching to the test" can potentially build human capital if a school is very low performing (Lazear 2006) and they are actively choosing to avoid this strategy by continuing to focus on a more holistic approach.

Unfortunately, we cannot directly test the multitasking theory with our data as it relies on important, but subtle, changes in curriculum or the management of schools. For instance, one might want to compare the scope and content of lessons in high-test-score schools versus low-test-score schools. In low-test-score schools, under this theory, one would expect more lessons that were not correlated with the content on the state test but which one could argue might be correlated with labor market success.

In an effort to make modest progress on this theory, we explore detailed data on the inner-workings of charter schools in New York City, described in Dobbie and Fryer (2013). An enormous amount of information was collected from each school. A principal interview asked about teacher development, instructional time, data-driven instruction, parent outreach, and school culture. Teacher interviews asked about professional development, school policies, school culture, and student assessment. Student interviews asked about school environment, school disciplinary policy, and future aspirations. Lesson plans were used to measure curricular rigor and the scope and sequence of instruction. Importantly for this paper, the instruction time variables in the principal interview gleaned the amount of time that each school spends per week on both tested (e.g. math and reading) and non-tested subjects (e.g. art, history, foreign language).

Appendix Table 16 investigates differences across a wide set of variables – using the NYC charter data from Dobbie and Fryer (2013) – that might be consistent with multitasking. At the mean, charters that increase test scores spend 4.5 percent more time on math and reading relative to charters that decrease test scores. The p-value on the difference – 0.569 – is not significant. Moreover, they spend 6.3 percent more time on non-tested subjects. The p-value is 0.413. These data do not seem consistent with multitasking.

Digging deeper, however, there are some differences between achievement-increasing charter schools in New York City and those that decrease achievement that may be applicable to our earnings results for charters in Texas. For instance, achievement-increasing charter schools spend significantly less time on foreign languages and history. This is consistent with Jacob (2005). Whether this is important for labor market earnings is unknown – but it does provide some evidence of differences in time focus for schools that increase versus decrease short-run test scores. Some argue that familiarity with a foreign language, adeptness with social studies, and immersion in the arts are important elements of a liberal arts education that instill creativity, problem-solving skills, grit, and other non-cognitive skills that are important for labor market success (Bialystok and Martin 2004, Mindes 2005, Elpus 2013, Elpus 2014, Catterall 2009, Catterall, Dumais, and Hampden-Thompson 2012, Bradley, Bonbright, and Dooling 2013). Others believe that these skills are essentially a "luxury good" and students (particularly those who are low-income), would be better served by focusing on basic math and reading.

Settling this debate is beyond the scope of this paper. In the end, there is some evidence that schools that increase achievement do so at the expense of subjects such as foreign language and history. Whether that can explain the patterns in our data is unknown.

VIII. Conclusion

In this paper, we estimate the impact of charter schools on early-life labor market outcomes using administrative data from Texas. We find that, at the mean, charter schools have no impact on test scores and a negative impact on earnings. No Excuses charter schools increase test scores and four-year college enrollment, but have a small and statistically insignificant impact on earnings, while regular charter schools decrease test scores, four-year college enrollment, and earnings. Using school-level estimates, we find that charter schools that decrease test scores also tend to decrease earnings, while charter schools that increase test scores have no discernible impact on earnings. In contrast, high school graduation effects are predictive of earnings effects for both low- and high-value added schools.

The underlying mechanism that drives these results is elusive. We test four hypotheses. Students in our main specifications are in their mid-twenties, but investigating older cohorts of students only strengthens the results. High attrition rates of achievement-increasing charters also fails to explain the results. The final two mechanisms are, at least, generally consistent with the data. Some – though not all – of the estimates reported are consistent with the impact on earnings one might expect given the cross-sectional correlation between test scores and earnings documented in the literature. Finally, there is some evidence that schools may put subjects such as art and history on the back burner when they increase test scores and the effects of this practice on labor market outcomes is unknown.

Charter schools, in particular No Excuses charter schools, are considered by many to be the most important education reform of the past quarter century. At the very least, however, this paper cautions that charter schools may not have the large effects on earnings many predicted. It is plausible this is due to the growing pains of an early charter sector that was "building the plane as they flew it." This will be better known with the fullness of time. Much more troubling, it seems, is the possibility that what it takes to increase achievement among the poor in charter schools deprives them of other skills that are important for labor markets.

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Table 1
Students in Estimation Sample

	Full Sample	Trad. Elem.	Baseline Covars	Test Scores	In Texas	Cohort Size	Matched Cell
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Non-Charters	1420877	1413231	1319580	1226527	1162148	1162148	188666
No Excuses Charters	1358	1345	1192	1164	1051	1039	1039
Regular Charters	4905	4830	4633	4365	4090	3860	3860

Notes: This table details the number of students in our estimation sample. All rows are restricted to Texas public school students expected to graduate high school in or before 2005-2006. Column 1 is the total number of students with no additional restrictions. Column 2 drops students who did not attend a traditional elementary school in 4th grade. Column 3 drops students with missing gender and race. Column 4 drops students with no middle or high school test scores. Column 5 drops students who transferred to an out-of-state school. Column 6 drops charter school cohorts of fewer than 10 students. Column 7 drops students who are not in a matched cell of 4th grade school, cohort, gender, and race.

Table 2
Summary Statistics

	Full Sample			Estimation Sample		
	Non-Charters	No Excuses	Regular Charters	Non-Charters	No Excuses	Regular Charters
Baseline Characteristics	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.488	0.539	0.487	0.511	0.546	0.494
Black	0.140	0.108	0.329	0.224	0.116	0.337
Hispanic	0.363	0.586	0.344	0.288	0.525	0.332
Asian	0.022	0.063	0.010	0.006	0.075	0.009
Free Lunch	0.518	0.627	0.647	0.476	0.576	0.635
4th On Time	0.813	0.827	0.786	0.833	0.832	0.806
4th Grade Spec. Ed	0.138	0.063	0.157	0.103	0.066	0.129
4th Grade Gifted	0.090	0.106	0.063	0.100	0.120	0.068
4th Grade LEP	0.140	0.371	0.147	0.115	0.321	0.133
4th Grade At Risk	0.425	0.545	0.509	0.392	0.504	0.494
4th Grade Math	0.010	0.206	-0.329	0.030	0.217	-0.304
4th Grade Reading	0.007	0.193	-0.234	0.064	0.215	-0.214
Missing 4th Math	0.216	0.346	0.284	0.176	0.313	0.245
Missing 4th Reading	0.225	0.352	0.300	0.185	0.321	0.259
Treatment						
Years Any Charter	0.000	2.871	1.858	0.000	2.919	1.892
Years No Excuses	0.000	2.803	0.000	0.000	2.835	0.000
Years Regular Charters	0.000	0.068	1.858	0.000	0.084	1.892
Outcomes						
5th-11th Grade Math	-0.089	0.251	-0.560	-0.085	0.243	-0.532
5th-11th Grade Reading	-0.089	0.224	-0.419	-0.050	0.242	-0.391
High School Graduation	0.713	0.736	0.623	0.761	0.828	0.663
Any Two-Year College	0.310	0.279	0.264	0.322	0.337	0.302
Years Two-Year College	0.929	0.895	0.814	0.971	1.090	0.937
Any Four-Year College	0.238	0.262	0.138	0.291	0.320	0.159
Years Four-Year College	0.944	1.012	0.478	1.155	1.236	0.546
Avg. Earnings (24-26)	14816.180	13411.150	11188.850	16598.530	15986.780	12563.630
Avg. Employment (24-26)	0.582	0.503	0.551	0.642	0.592	0.599
N Schools	9983	5	40	7290	5	40
N Students	1420877	1358	4905	188666	1039	3860

Notes: This table reports descriptive statistics for Texas public school students in our data graduating high school by 2005-2006. Columns 1-3 report means for all Texas public school students in the indicated schools. Columns 4-6 report means for students who are in the final estimation sample described in Table 1. See Online Appendix B for additional details on the variable definitions and sample.

Table 3
Charter School Attendance and Test Scores

	Math Scores		Reading Scores		Pooled Scores	
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)
Any Charter	-0.011^{*}	-0.009	0.021^{***}	0.022^{***}	0.005	0.006
	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Panel B: By Charter Type						
No Excuses	0.092^{***}	0.095^{***}	0.098^{***}	0.099^{***}	0.095^{***}	0.097^{***}
	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)
Regular Charter	-0.080^{***}	-0.078^{***}	-0.030^{***}	-0.029^{***}	-0.055^{***}	-0.054^{***}
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	No	Yes	No	Yes
N Students x Years	903281	903281	900712	900712	1803993	1803993
Dep. Variable Mean	-0.006	-0.006	0.030	0.030	0.012	0.012

Notes: This table reports OLS estimates of the effect of charter attendance on test scores. We report the coefficient and standard error on the number of years spent at the indicated charter school type. Odd columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Even columns add 4th grade school x cohort x race x gender effects. All specifications stack 5th-11th grade test score outcomes and cluster standard errors by student. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable definitions and sample.

Table 4
Charter School Attendance and Academic Attainment

	High School Grad.		Two-Year Enrollment		Four-Year Enrollment	
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)
Any Charter	0.012***	0.012***	0.015***	0.015***	0.002	0.003
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Panel B: By Charter Type						
No Excuses	0.024***	0.025***	0.012**	0.012**	0.026***	0.028***
	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.005)
Regular Charter	0.004	0.004	0.016***	0.016***	−0.013***	−0.013***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	No	Yes	No	Yes
N Students	193565	193565	193565	193565	193565	193565
Dep. Variable Mean	0.760	0.760	0.322	0.322	0.289	0.289

Notes: This table reports OLS estimates of the effect of charter attendance on academic attainment. We report the coefficient and standard error on the number of years spent at the indicated charter school type. Odd columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Even columns add 4th grade school x cohort x race x gender effects. All specifications include one observation per student and cluster standard errors at the 4th grade school by cohort level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable definitions and sample.

Table 5
Charter School Attendance and Labor Market Outcomes at Ages 24-26

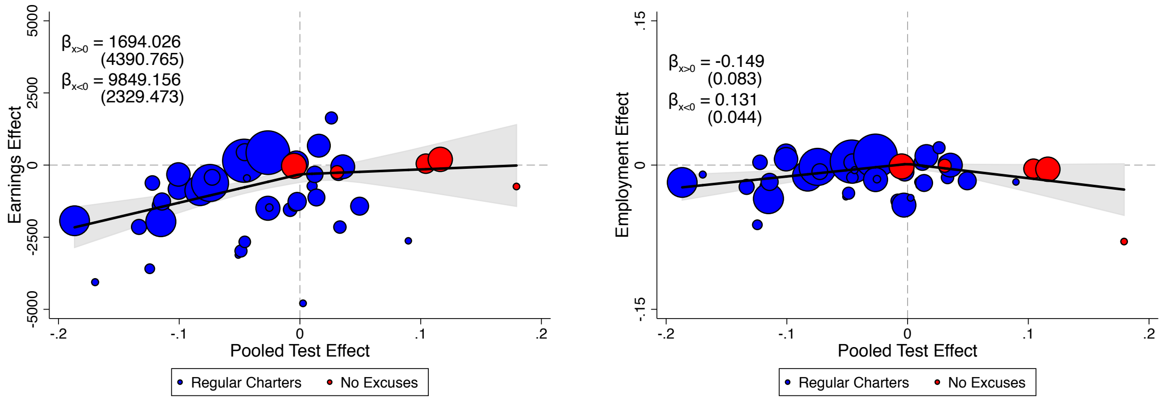
	Average earnings					earnings > 0	
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any Charter	−184.184*	−163.653*	−199.477*	−143.209*	−102.408	−0.002	−0.001
	(97.681)	(98.860)	(119.275)	(85.091)	(85.352)	(0.003)	(0.003)
Panel B: By Charter Type							
No Excuses	56.175	101.043	211.058	167.098	237.441	−0.002	−0.002
	(172.743)	(176.117)	(218.020)	(150.790)	(152.787)	(0.004)	(0.004)
Regular Charter	−329.780***	−322.278***	−443.561***	−329.167***	−306.078***	−0.001	−0.001
	(113.763)	(114.515)	(138.418)	(99.516)	(100.599)	(0.003)	(0.003)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	Yes	Yes	Yes	No	Yes
Non-Zero earnings Only	No	No	Yes	No	No	No	No
Baseline Imput.	No	No	No	Yes	No	No	No
Output Imput.	No	No	No	No	Yes	No	No
N Students	193565	193565	141340	193565	193565	193565	193565
Dep. Variable Mean	16514.79	16514.79	22616.99	21097.92	20996.20	0.641	0.641

Notes: This table reports OLS estimates of the effect of charter attendance on earnings eight years after high school graduation. We report the coefficient and standard error on the number of years spent at the indicated charter school type. All columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Columns 2-5 and 7 add 4th grade school x cohort x race x gender effects. All specifications include one observation per student and cluster standard errors at the 4th grade school by cohort level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable definitions and sample. See the text for additional details on the imputation procedures.

Figure 1
Correlation of Labor Market and Test Score Effects

(A) Earnings and Test Scores

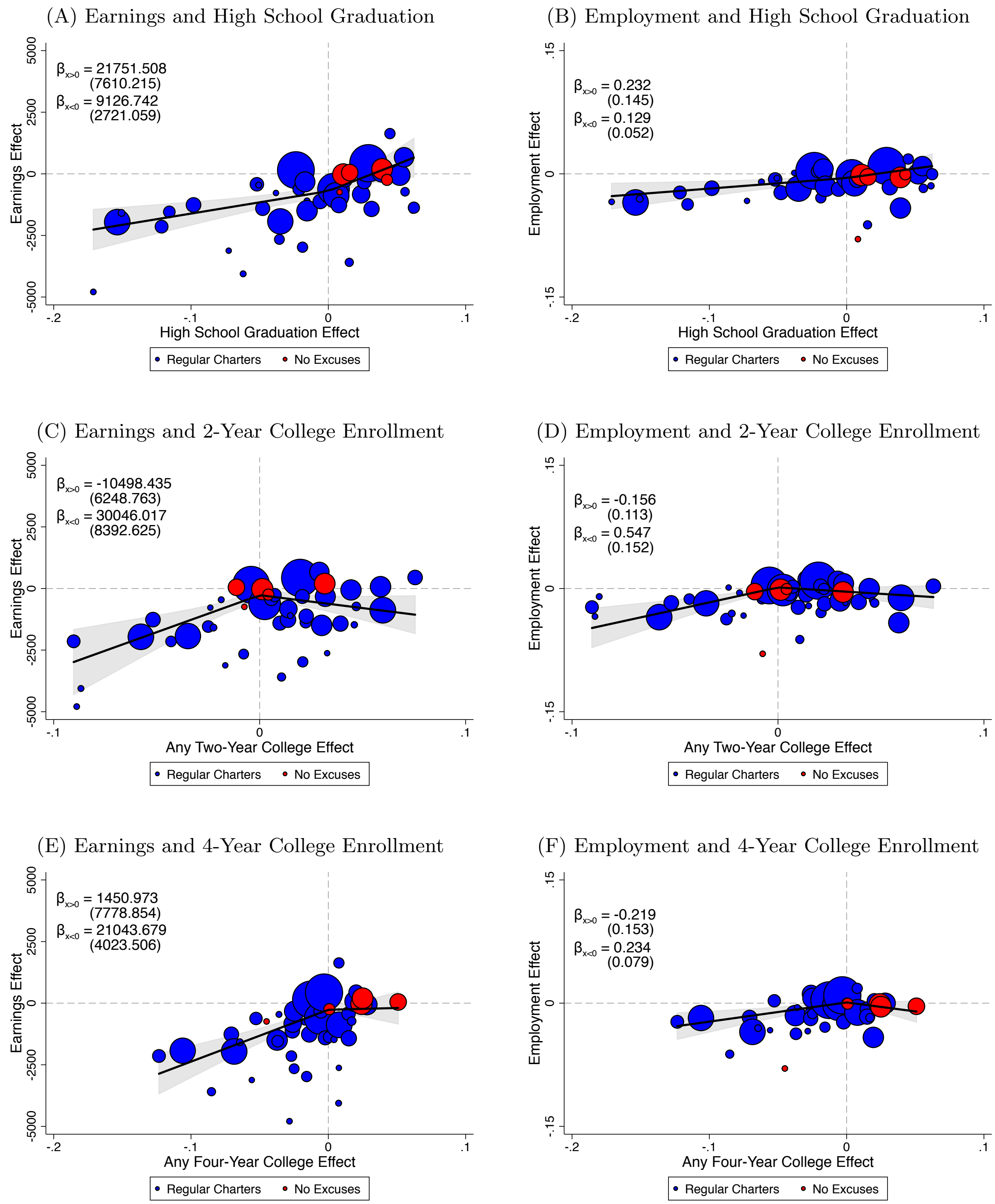
(B) Employment and Test Scores



Notes: These figures plot the correlation between school-level labor market effects and school-level test score effects. Observations are weighted by the number of students at each school in the earnings estimation sample. The solid line is estimated at the school-cohort level. See Table 2 notes for details on the sample and variable construction and Online Appendix C for details on estimation of the school effects.

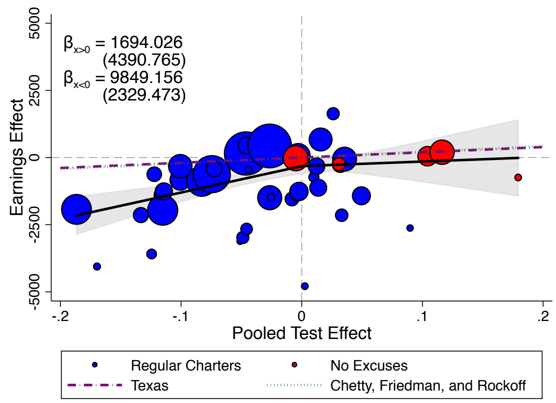
Figure 2

Correlation of School Labor Market and Academic Attainment Effects



Notes: These figures plot the correlation between school labor market effects and academic attainment effects. Observations are weighted by the number of students at each school in the earnings estimation sample. We estimate the labor market and academic effects using non-overlapping samples of students. See Online Appendix B for details on the sample and variable construction and Online Appendix C for details on estimation of the school effects.

Figure 3
Correlation of Earnings and Test Score Effects



Notes: This figure plots the correlation between school-level earnings effects and school-level test score effects. Observations are weighted by the number of students at each school in the earnings estimation sample. The solid line is estimated at the school-cohort level. The dotted and dashed lines are student-level correlations estimated in the given sample. See Table 2 notes for details on the sample and variable construction and Online Appendix C for details on estimation of the school effects. See Appendix Table 14 notes for details on the estimation of the student-level correlations in the Texas sample. See Chetty, Friedman, and Rockoff (2014) for details on their estimation of the correlation in a sample of students from a large urban school district.

Online Appendix A: Supplemental Results

Appendix Table 1
Charter Schools in Estimation Sample

	District	Years Open	Grades	Cohorts	Students
Panel A: No Excuses Charters	(1)	(2)	(3)	(4)	(5)
Harmony Science Academy	Houston	2001-2014	6-12	2	95
IDEA College Prep	Donna	2001-2014	5-12	1	24
KIPP Academy*	Houston	1996-2014	5-12	5	211
Yes College Preparatory	Houston	1999-2014	5-12	5	331
Uplift Education - North Hills Prep	Irving	1998-2014	5-12	5	351
Panel B: Regular Charter - College Prep					
A+ Academy	Dallas	2001-2014	5-12	2	72
Arlington Classics Academy	Arlington	2000-2014	5-9	3	43
Bright Ideas Charter	Wichita Falls	1999-2014	5-12	2	33
Chaparral Star Academy	Austin	1999-2014	5-12	4	71
Girls & Boys Prep Academy	Houston	1997-2014	5-12	5	465
Katherine Anne Porter	Wimberly	2000-2014	9-12	5	221
Life Charter School of Oak Cliff	Lancaster	1999-2014	5-12	3	130
Pineywoods Community Academy	Lufkin	2000-2014	5-9	3	83
Rick Hawkins HS*	San Antonio	2005-2014	5-12	5	324
The Ehrhart School	Beaumont	2002-2014	5-12	3	56
Treetops School International	Fort Worth	1999-2014	5-12	5	137
Two Dimensions Preparatory*	Houston	1999-2014	5-8	1	10
Universal Academy*	Irving	1999-2014	5-12	3	141
Panel C: Regular Charter - Special Mission					
Aristoi Academy	Arlington	1997-2014	5-6	5	144
Eden Park Academy	Austin	1999-2014	5-8	2	36
Guardian Angel Performance Arts	San Antonio	2000-2010	6-8	3	68
Heritage Academy	Dallas	2000-2000	9-11	1	16
Inspired Vision	Dallas	2002-2014	5-9	1	12
Kaleidoscope/Caleidoscopio	Houston	1998-2012	6-8	3	80
Nyos Charter School	Austin	1999-2014	5-12	3	67
Oak Cliff Academy	Dallas	2000-2014	5-8	1	12
Odyssey Academy	Galveston	2000-2014	5-8	2	143
Tekoa Academy	Port Arthur	2000-2014	5-9	1	12
Texas Empowerment Academy	Austin	1999-2014	5-12	3	59
Waxahachie Faith Family Academy	Desoto	2000-2014	5-12	4	105
West Houston Charter 2	Katy	1999-2007	6-12	1	14
XXI Century Academy	Corpus Christi	2001-2001	9-11	1	11
Panel D: Regular Charter - Misc.					
Accelerated Intermediate Charter	Houston	2002-2010	6-8	2	38
Crossroad Community Charter	Houston	2001-2006	9-12	5	225
CSAS Academy of Beaumont	Beaumont	2000-2009	5-8	1	25
CSAS Academy of Houston	Houston	1999-2003	5-8	2	62
CSAS Academy of San Antonio	San Antonio	2000-2014	5-8	1	25
Emma L Harrison Charter	Waco	1999-1999	5-9	4	52
Education Center International	Rowlett	2002-2014	5-12	4	73
Fruit of Excellence School	Bastrop	2000-2010	5-12	3	46
Mainland Preparatory Academy	Texas City	1999-2014	5-8	2	36

Rameses School	San Antonio	1999-1999	5-10	1	10
Raul Yzaguirre School for Success*	Houston	1997-2014	5-12	5	475
Renaissance Charter HS	Irving	1997-2000	5-11	5	228

Notes: This table describes the charter schools in our estimation sample. Column 2 reports the first and last dates of the school operation in our data. Column 3 reports the largest grade span attended by students in our estimation sample. Column 4 reports the number of distinct entry cohorts in the estimation sample. Column 5 reports the total number of students in the estimation sample. * indicates schools with multiple campus IDs.

Appendix Table 2A

Charter Attendance and Baseline Characteristics

	Non-Charter Mean	Any Charter	No Excuses	Regular Charters
Panel A: Leave-Out Controls	(1)	(2)	(3)	(4)
Free Lunch	0.480	−0.002	−0.014***	0.005
		(0.003)	(0.004)	(0.003)
4th Grade On Time	0.832	0.008***	0.014***	0.005*
		(0.002)	(0.003)	(0.003)
4th Grade Spec. Ed	0.103	0.003*	0.001	0.005**
		(0.002)	(0.002)	(0.002)
4th Grade Gifted	0.100	−0.001	0.003	−0.003*
		(0.002)	(0.004)	(0.002)
4th Grade LEP	0.116	0.004**	0.010***	0.000
		(0.002)	(0.003)	(0.002)
4th Grade At Risk	0.395	0.001	0.001	0.002
		(0.002)	(0.003)	(0.003)
4th Grade Math	0.021	−0.010**	0.016***	−0.025***
		(0.004)	(0.005)	(0.005)
4th Grade Reading	0.049	0.003	0.004	0.003
		(0.003)	(0.004)	(0.004)
Panel B: Characteristics not in Controls				
3rd Grade On Time	0.853	0.001	0.001	0.001
		(0.001)	(0.001)	(0.001)
3rd Grade Math	0.047	0.003	0.023*	−0.009
		(0.006)	(0.012)	(0.007)
3rd Grade Reading	0.060	0.004	0.014	−0.002
		(0.006)	(0.010)	(0.007)
Panel C: Predicted Outcomes				
Predicted Earnings	22478.66	−28.679***	44.315***	−72.397***
		(9.322)	(10.618)	(12.616)
Predicted Employment	64.108	−0.038***	0.005	−0.064***
		(0.014)	(0.021)	(0.019)
N Students	188666	4899	1039	3860

Notes: This table reports OLS estimates of the correlation between charter attendance and baseline variables. Column 1 reports the mean of the indicated variable for students at non-charter schools. Column 2 reports the coefficient and standard error on the number of years at any charter school controlling for the baseline controls listed in Table 2 and 4th grade school x cohort x race x gender effects. Columns 3-4 report the coefficient and standard error on the number of years at the indicated charter school type controlling for the baseline controls listed in Table 2 and 4th grade school x cohort x race x gender effects. In Panel A, the controls do not include the indicated dependent variable. In Panels B and C all controls from Table 2 are used. Predicted earnings and employment are calculated in the full estimation sample using the baseline controls listed in Table 2 and 4th grade school x cohort x race x gender effects. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable construction and sample. See the text for additional details on the specification.

Appendix Table 2B

Charter Attendance and Baseline Characteristics in NYC Data

	Non-Charter Mean	Any Charter	No Excuses	Regular Charters
Panel A: Leave-Out Controls	(1)	(2)	(3)	(4)
Free Lunch	0.924	−0.001	−0.005**	0.003
		(0.001)	(0.002)	(0.002)
4th Grade On Time	0.851	0.005***	0.006***	0.003
		(0.001)	(0.002)	(0.002)
4th Grade Spec. Ed	0.132	−0.003***	−0.004***	−0.002
		(0.001)	(0.001)	(0.002)
4th Grade LEP	0.164	−0.002**	−0.000	−0.004***
		(0.001)	(0.002)	(0.002)
4th Grade Math	−0.278	0.002	0.007*	−0.002
		(0.003)	(0.004)	(0.004)
4th Grade Reading	−0.246	0.005*	0.013***	−0.003
		(0.003)	(0.003)	(0.004)
Panel B: Characteristics not in Controls				
3rd Grade On Time	0.868	0.001	−0.000	0.002
		(0.001)	(0.001)	(0.001)
3rd Grade Math	−0.225	−0.001	0.004	−0.005
		(0.003)	(0.004)	(0.004)
3rd Grade Reading	−0.223	0.000	0.007*	−0.007
		(0.003)	(0.004)	(0.004)
N Students	70898	8036	2678	5358

Notes: This table reports OLS estimates of the correlation between charter attendance and baseline variables in the NYC data used by Dobbie and Fryer (2013). Specifically, we focus on the sample of charter schools with experimental estimates in Dobbie and Fryer (2013). Column 1 reports the mean of the indicated variable for students at non-charter schools. Column 2 reports the coefficient and standard error on the number of years at any charter school in the sample controlling for free lunch status, if a student reached 4th grade on time, 4th grade special education status, 4th grade Limited English Proficiency status, 4th grade math and ELA test scores, and 4th grade school x cohort x race x gender effects. Columns 3-4 report the coefficient and standard error on the number of years at the indicated charter school type with the same controls as Column 2. In Panel A, the controls do not include the indicated dependent variable. In Panel B all controls are used. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix Table 3
Zero Earnings and Baseline Characteristics

	Non-Charter	Any Charter	(1)-(2)	Non-Charter	No Excuses	Regular Charters	(4)-(5)	(4)-(6)	(5)-(6)
Panel A: Baseline Vars.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Female	-0.031^{***}	-0.028^{**}	0.001	-0.031^{***}	-0.033	-0.028^{*}	0.790	0.937	0.856
	(0.003)	(0.014)		(0.003)	(0.025)	(0.016)			
Black	0.048	-0.063	0.758	0.048	0.713^{***}	-0.235	0.000	0.420	0.003
	(0.105)	(0.290)		(0.105)	(0.068)	(0.309)			
White	0.090	-0.012	0.727	0.090	0.744^{***}	-0.184	0.000	0.431	0.003
	(0.105)	(0.291)		(0.105)	(0.050)	(0.309)			
Hispanic	0.005	-0.131	0.724	0.005	0.635^{***}	-0.300	0.000	0.381	0.003
	(0.105)	(0.290)		(0.105)	(0.059)	(0.308)			
Asian	0.145	0.086	0.779	0.145	0.823^{***}	-0.038	0.000	0.587	0.008
	(0.103)	(0.288)		(0.103)	(0.060)	(0.312)			
Free Lunch	0.003	-0.008	0.323	0.003	-0.056	0.005	0.112	0.922	0.138
	(0.003)	(0.015)		(0.003)	(0.037)	(0.017)			
4th Grade On Time	-0.019^{***}	-0.015	0.108	-0.019^{***}	-0.036	-0.012	0.698	0.644	0.578
	(0.003)	(0.017)		(0.003)	(0.041)	(0.019)			
4th Grade Spec. Ed	-0.062^{***}	-0.091^{***}	0.006	-0.062^{***}	-0.020	-0.097^{***}	0.564	0.148	0.296
	(0.005)	(0.024)		(0.005)	(0.068)	(0.026)			
4th Grade Gifted	0.035^{***}	0.012	0.682	0.035^{***}	0.010	0.010	0.683	0.443	0.996
	(0.004)	(0.029)		(0.004)	(0.062)	(0.032)			
4th Grade LEP	-0.090^{***}	-0.095^{***}	0.244	-0.090^{***}	-0.079^{*}	-0.099^{***}	0.848	0.698	0.694
	(0.005)	(0.025)		(0.005)	(0.043)	(0.031)			
4th Grade At Risk	-0.024^{***}	-0.024	0.267	-0.024^{***}	-0.048	-0.020	0.560	0.761	0.507
	(0.003)	(0.016)		(0.003)	(0.040)	(0.017)			
4th Grade Math	-0.011^{***}	-0.005	0.031	-0.011^{***}	-0.017	-0.003	0.844	0.343	0.582
	(0.002)	(0.009)		(0.002)	(0.025)	(0.010)			
4th Grade Reading	-0.007^{***}	-0.015	0.007	-0.007^{***}	-0.009	-0.015	0.892	0.407	0.818
	(0.002)	(0.010)		(0.002)	(0.025)	(0.011)			
Missing 4th Math	0.198^{***}	0.208^{***}	0.620	0.198^{***}	0.222	0.200^{***}	0.883	0.971	0.896
	(0.008)	(0.044)		(0.008)	(0.165)	(0.046)			
Missing 4th Reading	0.110^{***}	0.154^{***}	0.005	0.110^{***}	0.181	0.149^{***}	0.632	0.328	0.850
	(0.007)	(0.044)		(0.007)	(0.162)	(0.046)			
Panel B: Predicted Outcomes									
Predicted Earnings	-0.004^{***}	-0.005^{***}	0.223	-0.004^{***}	-0.012^{***}	-0.004^{**}	0.070	0.785	0.078
	(0.000)	(0.001)		(0.000)	(0.004)	(0.002)			
Predicted Employment	-0.011^{***}	-0.013^{***}	0.000	-0.011^{***}	-0.014^{***}	-0.012^{***}	0.000	0.021	0.088
	(0.000)	(0.001)		(0.000)	(0.001)	(0.001)			
N Students	188666	4899		188666	1039	3860			

Notes: This table reports OLS estimates of the correlation between zero observed earnings between ages 24-26 and baseline variables. Columns 1-2 and 4-6 reports the coefficient and standard error on the indicated variable in the indicated school type. Columns 3 and 7-9 report the p-value on the F-test that the indicated coefficients are equal. Predicted earnings and employment are calculated using the baseline controls listed in Table 2 and 4th grade school x cohort x race x gender effects. All specifications control for 4th grade school x cohort effects and cluster standard errors at the 4th grade school by cohort level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable construction and sample.

Appendix Table 4

Charter School Attendance and In- and Out-of-State College Attendance

	Texas Colleges				Non-Texas Colleges			
	Any Two-Year		Any Four-Year		Any Two-Year		Any Four-Year	
Panel A: Pooled Results	(1)	(2)	(3)	(4)				
Any Charter	−0.003	−0.004	0.002	0.002	0.000	0.000	0.009***	0.009***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)
Panel B: By Charter Type								
No Excuses	−0.013***	−0.014***	0.017***	0.016***	0.000	0.000	0.018***	0.019***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.001)	(0.001)	(0.003)	(0.003)
Regular Charter	0.008**	0.007**	−0.015***	−0.015***	0.000	0.000	−0.002**	−0.002**
	(0.004)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	No	Yes	No	Yes	No	Yes
N Students	127898	127898	127898	127898	127898	127898	127898	127898
Dep. Variable Mean	0.268	0.268	0.230	0.230	0.006	0.006	0.044	0.044

Notes: This table reports OLS estimates of the effect of charter attendance on in- and out-of-state college attendance measured using the National Student Clearinghouse. The sample is restricted to Texas public school students in our estimation sample who graduated high school between 2008 and 2009. We report the coefficient and standard error on the number of years spent at the indicated charter school type. Odd columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Even columns add 4th grade school x cohort x race x gender effects. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable construction and sample.

Appendix Table 5
Ever Attended Results: Test Scores

	Math Scores		Reading Scores		Pooled Scores	
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)
Any Charter	-0.071^{***}	-0.067^{***}	0.012	0.014	-0.029^{***}	-0.027^{**}
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Panel B: By Charter Type						
No Excuses	0.246^{***}	0.254^{***}	0.262^{***}	0.264^{***}	0.254^{***}	0.259^{***}
	(0.023)	(0.023)	(0.021)	(0.020)	(0.020)	(0.020)
Regular Charter	-0.188^{***}	-0.185^{***}	-0.081^{***}	-0.079^{***}	-0.135^{***}	-0.132^{***}
	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	No	Yes	No	Yes
N Students x Years	903281	903281	900712	900712	1803993	1803993
Dep. Variable Mean	-0.006	-0.006	0.030	0.030	0.012	0.012

Notes: This table reports OLS estimates of the effect of charter attendance on test score outcomes. We report the coefficient and standard error on ever attending the indicated charter school type. Odd columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Even columns add 4th grade school x cohort x race x gender effects. All specifications stack 5th-11th grade test score outcomes and cluster standard errors by student. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable construction and sample.

Appendix Table 6

Ever Attended Results: Academic Attainment

	High School Grad.		Two-Year Enrollment		Four-Year Enrollment	
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)
Any Charter	−0.016***	−0.015***	0.020***	0.020***	−0.030***	−0.029***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
Panel B: By Charter Type						
No Excuses	0.070***	0.072***	0.027*	0.025	0.048***	0.053***
	(0.013)	(0.014)	(0.016)	(0.016)	(0.014)	(0.015)
Regular Charter	−0.037***	−0.037***	0.020***	0.020***	−0.051***	−0.051***
	(0.008)	(0.008)	(0.007)	(0.007)	(0.006)	(0.007)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	No	Yes	No	Yes
N Students	193565	193565	193565	193565	193565	193565
Dep. Variable Mean	0.760	0.760	0.322	0.322	0.289	0.289

Notes: This table reports OLS estimates of the effect of charter attendance on academic attainment outcomes. We report the coefficient and standard error on ever attending the indicated charter school type. Odd columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Even columns add 4th grade school x cohort x race x gender effects. All specifications include one observation per student and cluster standard errors at the 4th grade school by cohort level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable construction and sample.

Appendix Table 7

Ever Attended Results: Labor Market Outcomes at Ages 24-26

	Average Earnings					Earnings > 0	
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any Charter	−1648.677***	−1586.602***	−1733.941***	−1292.095***	−1281.249***	−0.023***	−0.024***
	(239.087)	(238.635)	(300.099)	(207.782)	(210.686)	(0.007)	(0.007)
Panel B: By Charter Type							
No Excuses	−1004.666*	−825.059	−278.536	−265.292	−87.198	−0.028*	−0.029*
	(571.899)	(584.244)	(713.703)	(474.229)	(482.066)	(0.015)	(0.015)
Regular Charter	−1779.449***	−1748.870***	−2075.884***	−1539.423***	−1574.041***	−0.022***	−0.021***
	(250.338)	(248.979)	(321.789)	(223.785)	(227.641)	(0.007)	(0.007)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	Yes	Yes	Yes	No	Yes
Non-Zero Earnings Only	No	No	Yes	No	No	No	No
Baseline Imput.	No	No	No	Yes	No	No	No
Output Imput.	No	No	No	No	Yes	No	No
N Students	193565	193565	141340	193565	193565	193565	193565
Dep. Variable Mean	16514.79	16514.79	22616.99	21097.92	20996.20	0.641	0.641

Notes: This table reports OLS estimates of the effect of charter attendance on earnings eight years after high school graduation. We report the coefficient and standard error on ever attending the indicated charter school type. All columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Columns 2-5 and 7 add 4th grade school x cohort x race x gender effects. All specifications include one observation per student and cluster standard errors at the 4th grade school by cohort level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable construction and sample.

Appendix Table 8

Charter School Attendance and Years of College

	Years Two-Year			Years Four-Year		
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)
Any Charter	0.053***	0.051***	0.005	−0.009	−0.006	−0.052**
	(0.009)	(0.009)	(0.013)	(0.013)	(0.013)	(0.022)
Panel B: By Charter Type						
No Excuses	0.029**	0.028**	−0.032*	0.093***	0.101***	−0.006
	(0.014)	(0.014)	(0.016)	(0.022)	(0.023)	(0.027)
Regular Charter	0.067***	0.065***	0.031*	−0.070***	−0.070***	−0.116***
	(0.011)	(0.011)	(0.018)	(0.012)	(0.012)	(0.033)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	Yes	No	Yes	Yes
Ever Enrolled Only	No	No	Yes	No	No	Yes
N Students	193565	193565	86815	193565	193565	61088
Dep. Variable Mean	0.971	0.971	2.729	1.143	1.143	3.901

Notes: This table reports OLS estimates of the effect of charter attendance on academic attainment. We report the coefficient and standard error on the number of years spent at the indicated charter school type. Odd columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Columns 2, 3, 5, and 6 add 4th grade school x cohort x race x gender effects. All specifications include one observation per student and cluster standard errors at the 4th grade school by cohort level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable definitions and sample.

Appendix Table 9

Charter School Attendance and Labor Market Outcomes at Ages 24-26

	Max Earnings				
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)
Any Charter	-211.365^{*}	-196.244^{*}	-231.837^{*}	-169.857^{*}	-134.625
	(113.861)	(114.578)	(133.532)	(94.207)	(94.038)
Panel B: By Charter Type					
No Excuses	71.157	107.83	242.255	197.053	264.586
	(195.141)	(198.259)	(233.803)	(159.338)	(160.802)
Regular Charter	-382.502^{***}	-378.466^{***}	-513.708^{***}	-389.736^{***}	-373.871^{***}
	(135.998)	(136.155)	(157.911)	(112.58)	(113.588)
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	Yes	Yes	Yes
Non-Zero Earnings Only	No	No	Yes	No	No
Baseline Imput.	No	No	No	Yes	No
Output Imput.	No	No	No	No	Yes
N Students	193565	193565	141340	193565	193565
Dep. Variable Mean	21507.26	21507.26	29454.18	28000.59	27835.08

Notes: This table reports OLS estimates of the effect of charter attendance on earnings eight years after high school graduation. We report the coefficient and standard error on the number of years spent at the indicated charter school type. All columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Columns 2-5 add 4th grade school x cohort x race x gender effects. All specifications include one observation per student and cluster standard errors at the 4th grade school by cohort level. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable definitions and sample. See the text for additional details on the imputation procedures.

Appendix Table 10A

Results by Student Gender

	Pooled Scores	HS Grad	2-Year College	4-Year College	Average Earnings	Earnings>0
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)
Male x Any Charter	0.003	0.014***	0.017***	0.000	−153.975	0.000
	(0.007)	(0.003)	(0.004)	(0.004)	(152.426)	(0.004)
Female x Any Charter	0.011	0.010***	0.012***	0.005	−163.845	−0.003
	(0.007)	(0.003)	(0.004)	(0.004)	(126.182)	(0.003)
p-value	0.443	0.337	0.379	0.399	0.961	0.466
Panel B: By Charter Type						
Male x No Excuses	0.099***	0.035***	0.020***	0.029***	−81.548	−0.010
	(0.012)	(0.005)	(0.007)	(0.007)	(317.817)	(0.006)
Female x No Excuses	0.095***	0.017***	0.006	0.027***	256.301	0.003
	(0.010)	(0.004)	(0.006)	(0.007)	(193.375)	(0.005)
p-value	0.837	0.003	0.097	0.851	0.368	0.080
Male x Regular Charter	−0.050***	0.004	0.015***	−0.013***	−189.568	0.005
	(0.009)	(0.004)	(0.004)	(0.004)	(173.618)	(0.004)
Female x Regular Charter	−0.056***	0.005	0.017***	−0.012**	−468.044***	−0.008
	(0.009)	(0.004)	(0.005)	(0.005)	(159.515)	(0.005)
p-value	0.656	0.904	0.806	0.747	0.255	0.042
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
N Students	193565	193565	193565	193565	193565	193565

Notes: This table reports results for male and female students. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable definitions and Tables 3-5 notes for details on the estimation framework.

Appendix Table 10B
Results by Student Baseline Test Score

	Pooled Scores	HS Grad	2-Year College	4-Year College	Average Earnings	Earnings>0
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)
Low Scores x Any Charter	−0.017***	0.006**	0.015***	−0.005	−207.361	0.000
	(0.007)	(0.003)	(0.005)	(0.005)	(206.656)	(0.004)
High Scores x Any Charter	−0.001	0.016***	0.021***	0.009**	−2.752	0.002
	(0.009)	(0.005)	(0.005)	(0.004)	(146.563)	(0.004)
p-value	0.114	0.051	0.380	0.042	0.437	0.690
Panel B: By Charter Type						
Low Scores x No Excuses	0.029***	0.013**	0.004	0.024***	173.203	0.003
	(0.008)	(0.003)	(0.006)	(0.007)	(309.895)	(0.006)
High Scores x No Excuses	0.118***	0.037**	0.041***	0.036***	425.189	0.003
	(0.015)	(0.008)	(0.010)	(0.009)	(322.342)	(0.008)
p-value	0.000	0.001	0.000	0.284	0.592	0.962
Low Scores x Regular Charter	−0.069***	−0.002	0.027***	−0.034***	−607.823**	−0.002
	(0.010)	(0.005)	(0.007)	(0.006)	(248.965)	(0.005)
High Scores x Regular Charter	−0.044*	0.009	0.014***	0.000	−139.855	0.002
	(0.010)	(0.005)	(0.005)	(0.005)	(176.374)	(0.005)
p-value	0.085	0.133	0.140	0.000	0.127	0.517
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
N Students	193565	193565	193565	193565	193565	193565

Notes: This table reports results for students with below and above median baseline test scores. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable definitions and Tables 3-5 notes for details on the estimation framework.

Appendix Table 10C
Results by Student Ethnicity

	Pooled Scores	HS Grad	2-Year College	4-Year College	Average Earnings	Earnings>0
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)
Black/Hispanic x Any Charter	0.030***	0.016***	0.017***	0.006**	41.150	0.000
	(0.006)	(0.003)	(0.003)	(0.003)	(114.733)	(0.003)
White/Asian x Any Charter	−0.040***	0.005	0.011**	−0.004	−509.073***	−0.004
	(0.007)	(0.004)	(0.005)	(0.005)	(196.619)	(0.004)
p-value	0.000	0.020	0.284	0.076	0.019	0.384
Panel B: By Charter Type						
Black/Hispanic x No Excuses	0.169***	0.034***	0.023***	0.032***	154.348	−0.003
	(0.010)	(0.004)	(0.005)	(0.007)	(215.088)	(0.005)
White/Asian x No Excuses	−0.001	0.012**	−0.004	0.022***	30.369	−0.001
	(0.009)	(0.005)	(0.007)	(0.007)	(319.615)	(0.007)
p-value	0.000	0.000	0.000	0.257	0.757	0.817
Black/Hispanic x Regular Charter	−0.042***	0.006	0.013***	−0.007**	−17.790	0.002
	(0.007)	(0.004)	(0.004)	(0.003)	(129.015)	(0.004)
White/Asian x Regular Charter	−0.081***	0.001	0.023***	−0.023***	−923.750***	−0.007
	(0.011)	(0.005)	(0.006)	(0.006)	(224.460)	(0.005)
p-value	0.003	0.421	0.188	0.015	0.001	0.161
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
N Students	193565	193565	193565	193565	193565	193565

Notes: This table reports results for white/Asian and black/Hispanic students. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable definitions and Tables 3-5 notes for details on the estimation framework.

Appendix Table 11
Correlation of Earnings Over the Life Cycle

	Texas Data	
Age	Correlation with Age 30 Earnings Including Zeros	Correlation with Age 30 Earnings Excluding Zeros
(1)	(2)	(3)
21	0.316	0.183
22	0.391	0.272
23	0.502	0.413
24	0.580	0.511
25	0.614	0.544
26	0.673	0.613
27	0.742	0.689
28	0.822	0.775
29	0.922	0.887
30	1.000	1.000

Notes: This table reports the correlation between individual earnings at the indicated age with age 30 earnings. The sample includes students in our estimation sample graduating high school in 2002 - 2003. See text for additional details.

Appendix Table 12

Charter School Attendance and Realized and Predicted Earnings at Ages 28-30

	Realized Earnings		Predicted Earnings	
Panel A: Pooled Results	(1)	(2)	(3)	(4)
Any Charter	-423.736^{*}	-399.507^{*}	5.639	11.306
	(230.377)	(229.871)	(13.302)	(13.122)
Panel B: By Charter Type				
No Excuses	273.501	308.086	65.192^{***}	82.141^{***}
	(485.044)	(477.682)	(23.961)	(23.400)
Regular Charter	-776.191^{***}	-753.757^{***}	-30.270^{**}	-30.943^{**}
	(230.060)	(229.681)	(15.239)	(14.984)
Baseline Controls	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	No	Yes
Output-Based Imputation	No	No	Yes	Yes
N Students	90590	90590	193514	193514
Dep. Variable Mean	21211.070	21211.070	12902.940	12902.940

Notes: This table reports OLS estimates of the effect of charter attendance on realized and predicted earnings at ages 28-30. The dependent variable for columns 1-2 is realized average earnings for ages 28-30 for students graduating between 2002-2004. The dependent variable for columns 3-4 is predicted earnings for ages 28-30 Texas students graduating between 2002-2006. We predict earnings in the sample of students graduating between 2002-2004 with non-zero earnings using indicators for high school graduation, two-year college enrollment, four-year college enrollment, and employment from ages 24-26; cubic polynomials in grade 5-11 math and reading scores, years of two-year college, years of four-year college, earnings from ages 24-26, and median industry earnings from ages 24-26; and the baseline controls used in all other specifications. We report the coefficient and standard error on the number of years attended at the indicated charter school type. Odd columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Even columns add 4th grade school x cohort x race x gender effects. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable construction and sample.

Appendix Table 13

Charter Attendance and Industry Earnings Distributions

	25th Percentile Earnings		50th Percentile Earnings		75th Percentile Earnings	
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Any Charter	−86.936	−73.062	−148.317	−124.010	−239.748	−200.832
	(61.340)	(61.937)	(104.685)	(105.309)	(168.991)	(170.017)
Panel B						
No Excuses	−39.654	−26.549	−41.417	−24.402	−93.199	−75.438
	(101.512)	(102.803)	(178.504)	(179.958)	(295.632)	(297.496)
Regular Charter	−115.577	−100.936	−213.072*	−183.702	−328.520	−275.977
	(74.979)	(76.101)	(125.671)	(127.340)	(200.696)	(203.142)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	No	Yes	No	Yes
N Students x Years	193565	193565	193565	193565	193565	193565
Dep. Variable Mean	9696.404	9696.404	18835.530	18835.530	31525.880	31525.880

Notes: This table reports OLS estimates of the effect of charter attendance on labor market outcomes. We report the coefficient and standard error on the number of years attended at the indicated charter school type. Odd columns control for the number of years spent at charter schools not in our main sample, the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort effects. Even columns add 4th grade school x cohort x race x gender effects. All specifications stack 5th-11th grade test score outcomes and cluster standard errors by student. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable construction and sample.

Appendix Table 14
Results by Charter Completion

	Pooled Scores	HS Grad	2-Year College	4-Year College	Average Earnings	Earnings>0
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)
Completed x Any Charter	0.056^{***}	0.007^{***}	0.011^{***}	0.007^{*}	-161.781	-0.002
	(0.012)	(0.003)	(0.004)	(0.004)	(121.661)	(0.003)
Not Completed x Any Charter	0.027^{*}	0.022^{***}	0.022^{***}	-0.007^{*}	-167.499	0.000
	(0.014)	(0.004)	(0.004)	(0.004)	(178.378)	(0.005)
p-value	0.073	0.001	0.043	0.016	0.979	0.674
Panel B: By Charter Type						
Completed x No Excuses	0.140^{***}	0.020^{***}	0.011^{*}	0.039^{***}	269.335	-0.000
	(0.014)	(0.004)	(0.006)	(0.006)	(210.614)	(0.005)
Not Completed x No Excuses	0.092^{***}	0.037^{***}	0.013^{*}	0.001	-308.968	-0.008
	(0.017)	(0.006)	(0.008)	(0.008)	(323.402)	(0.008)
p-value	0.119	0.010	0.846	0.000	0.137	0.398
Completed x Regular Charter	-0.023	-0.001	0.011^{**}	-0.014^{***}	-446.104^{***}	-0.003
	(0.016)	(0.004)	(0.004)	(0.004)	(142.255)	(0.004)
Not Completed x Regular Charter	-0.023	0.015^{**}	0.026^{***}	-0.011^{**}	-94.756	0.004
	(0.015)	(0.005)	(0.005)	(0.005)	(202.783)	(0.005)
p-value	0.980	0.012	0.021	0.643	0.163	0.238
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
N Students	193565	193565	193565	193565	193565	193565

Notes: This table reports results separately for charter students who did and did not attend until the last offered grade by the school. We report the coefficient on the number of years attended at the indicated school type. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level. See Online Appendix B for additional details on the variable construction and sample and Tables 3-5 notes for details on the estimation framework.

Appendix Table 15

Cross-Sectional Correlations Between Outcomes in Adulthood and Test Scores

	HS Grad	Two-Year College	Four-Year College	Average Earnings	Earnings>0
Panel A: Reading	(1)	(2)	(3)	(4)	(5)
Reading No Controls	0.159***	0.090***	0.190***	3545.888***	0.019***
	(0.001)	(0.001)	(0.001)	(42.989)	(0.001)
Reading With Controls	0.138***	0.057***	0.100***	1547.598***	−0.005***
	(0.002)	(0.001)	(0.001)	(58.925)	(0.002)
Panel B: Math					
Math No Controls	0.157***	0.083***	0.183***	4129.756***	0.021***
	(0.001)	(0.001)	(0.001)	(41.744)	(0.001)
Math With Controls	0.146***	0.051***	0.106***	2343.502***	0.002
	(0.002)	(0.001)	(0.001)	(56.870)	(0.002)
Dep. Variable Mean	0.760	0.321	0.288	16514.790	0.641
Observations	193565	193565	193565	193565	193565

Notes: This table reports results from an OLS regression of adult outcomes on average test scores for grades 5-11. The sample is the estimation sample detailed in Table 2. The control specifications include the baseline controls listed in Table 2, cubic polynomials in grade 4 math and reading scores, and 4th grade school x cohort x race x gender effects. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

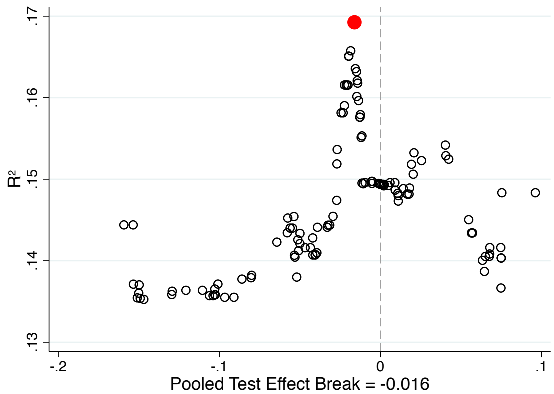
Appendix Table 16
Average Practices of Achievement-Increasing Versus Achievement-Decreasing NYC Charters

	Negative Impacts	Postive Impacts	p-value
	(1)	(2)	(3)
Panel A: Weekly Minutes in Tested Subjects			
ELA	558.63	680.80	0.050
	19	25	
Math	433.00	361.36	0.010
	21	33	
Pooled Minutes	1000.89	1046.20	0.569
	19	25	
Panel B: Weekly Minutes in Non-Tested Subjects			
Art	124.76	135.59	0.566
	21	31	
Foreign Language	126.43	51.52	0.012
	21	31	
History	128.10	64.52	0.018
	21	31	
Music	100.48	145.27	0.195
	21	31	
Physical Ed	103.33	145.71	0.037
	21	33	
Science	190.71	178.94	0.468
	21	33	
Social Studies	146.75	184.79	0.148
	20	24	
Pooled Minutes	880.50	935.75	0.413
	20	20	
Panel C: Other Inputs			
Frequency of Student Assessments	2.75	3.43	0.247
	22	28	
Number of Ways Use Assessments	3.40	4.41	0.165
	15	27	
Non-Academic Summer Programs	0.27	0.22	0.669
	22	36	

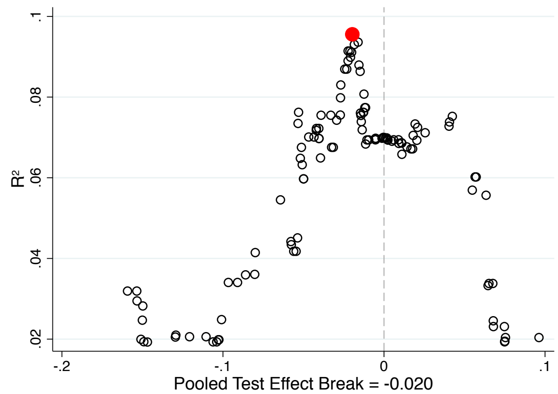
Notes: This table reports the average inputs for charter schools in the NYC data used by Dobbie and Fryer (2013). Specifically, we focus on the sample of charter schools with experimental estimates in Dobbie and Fryer (2013) and compare charters with negative impacts on test scores to charters with positive impacts. Column 1 reports the mean of the indicated variable for schools with negative impacts. Column 2 reports the mean of the indicated variable for schools with positive impacts. Column 3 reports a two-sided p-value from a two-sample t test. Beneath each mean, we also report the number of schools that had non-missing responses for the given survey question. The last row of Panel A and Panel B report results from summing all the other variables in the given panel. Note that this pooled variable only exists for schools that had non-missing responses for all variables being summed together.

Appendix Figure 1
Location of Trend Breaks

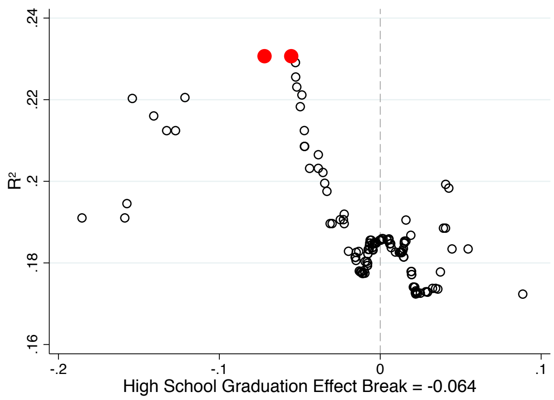
(A) Pooled Scores and Earnings



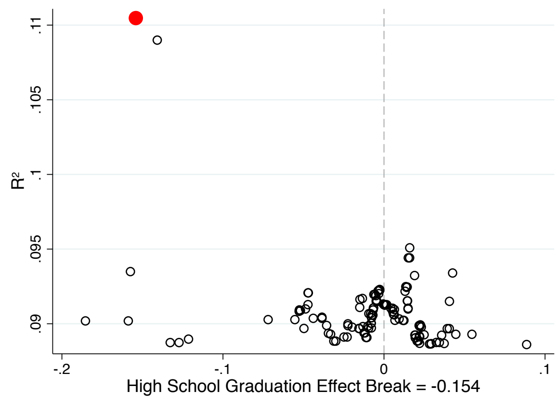
(B) Pooled Scores and Employment



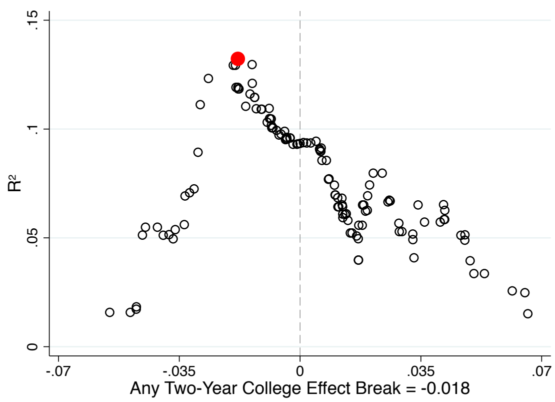
(C) High School Graduation and Earnings



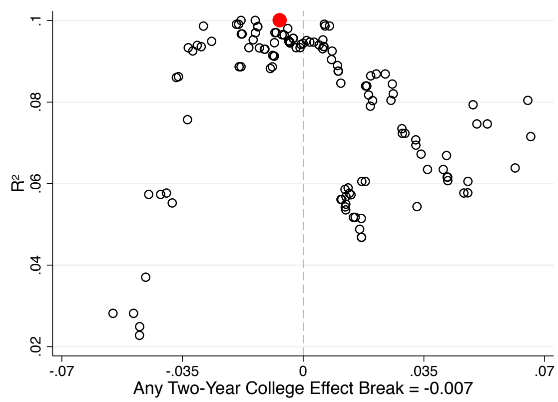
(D) High School Graduation and Employment



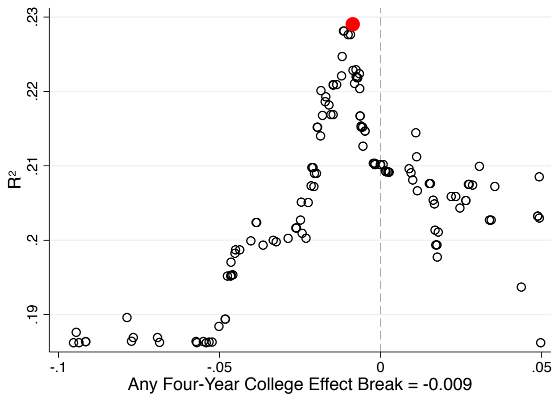
(E) Two-Year College and Earnings



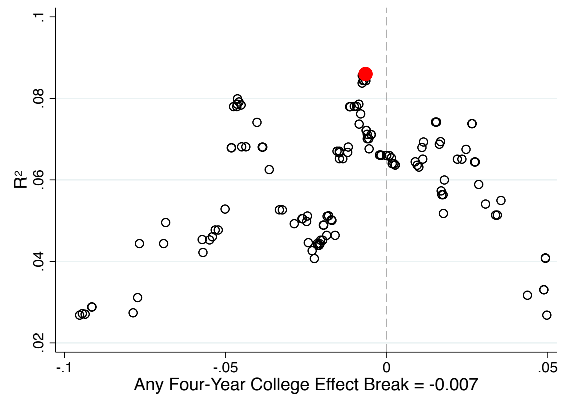
(F) Two-Year College and Employment



(G) Four-Year College and Earnings

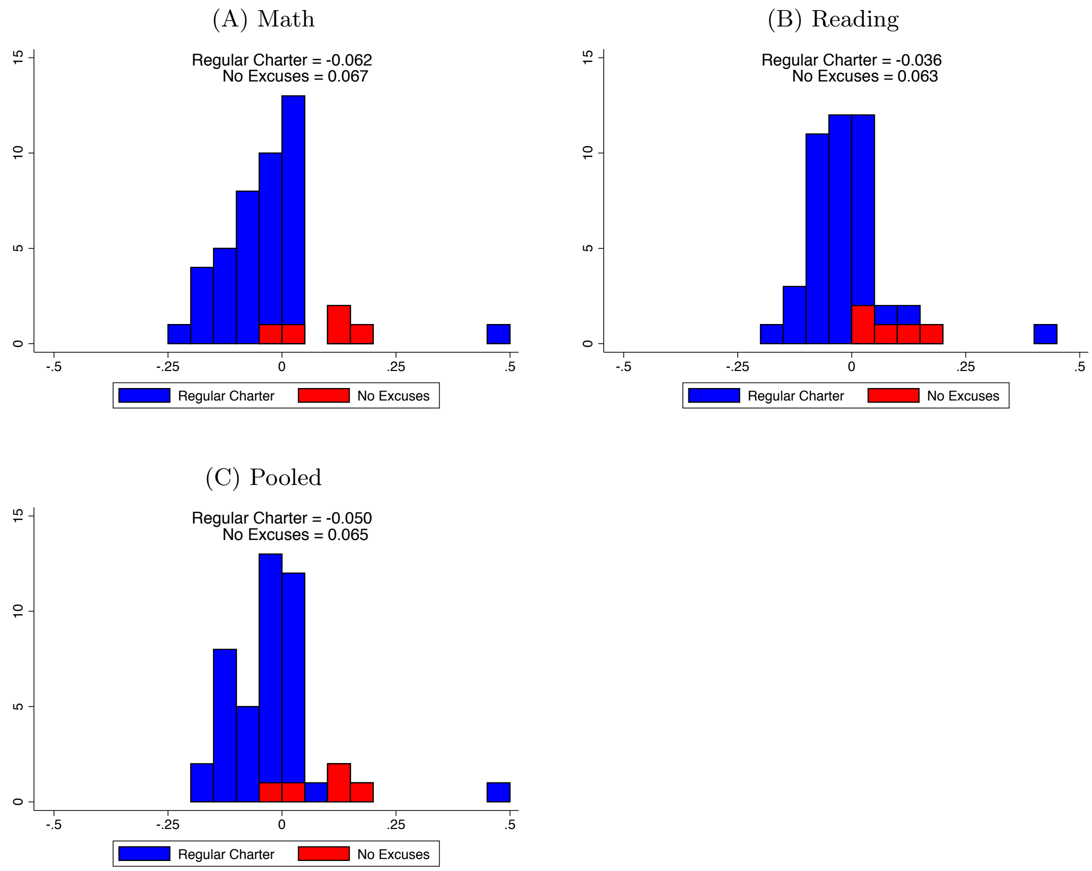


(H) Four-Year College and Employment



Notes: These figures plot the R^2 associated with different trend break locations. We report the value of the x variable for the maximum R^2. See text for details.

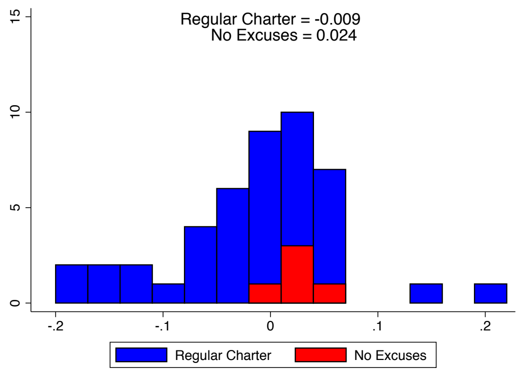
Appendix Figure 2
School Test Score Effects



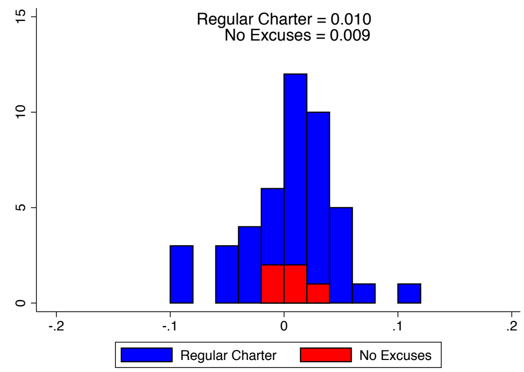
Notes: These figures plot school-level test score effects by charter school type. The reported means are weighted by the number of students at each school in the earnings effects estimation sample. See Online Appendix B for details on the sample and variable construction and Online Appendix C for details on estimation of the school effects.

Appendix Figure 3
School Academic Attainment Effects

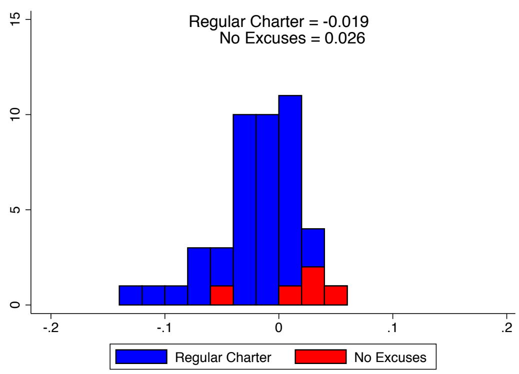
(A) High School Graduation



(B) Two-Year College Enrollment



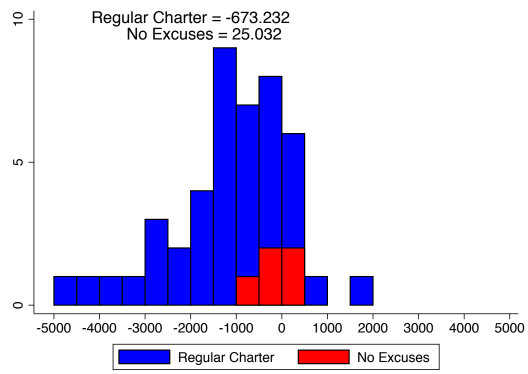
(C) Four-Year College Enrollment



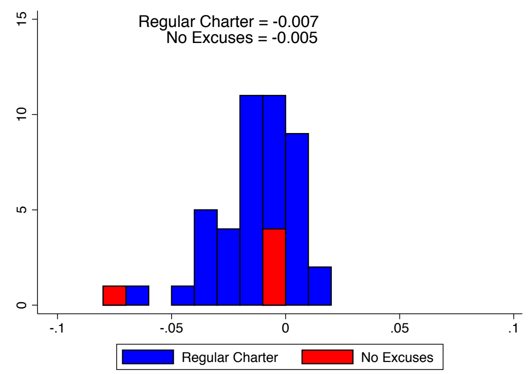
Notes: These figures plot school-level academic attainment effects by charter school type. The reported means are weighted by the number of students at each school in the earnings effects estimation sample. See Online Appendix B for details on the sample and variable construction and Online Appendix C for details on estimation of the school effects.

Appendix Figure 4
School Labor Market Effects

(A) Average Earnings 24-26

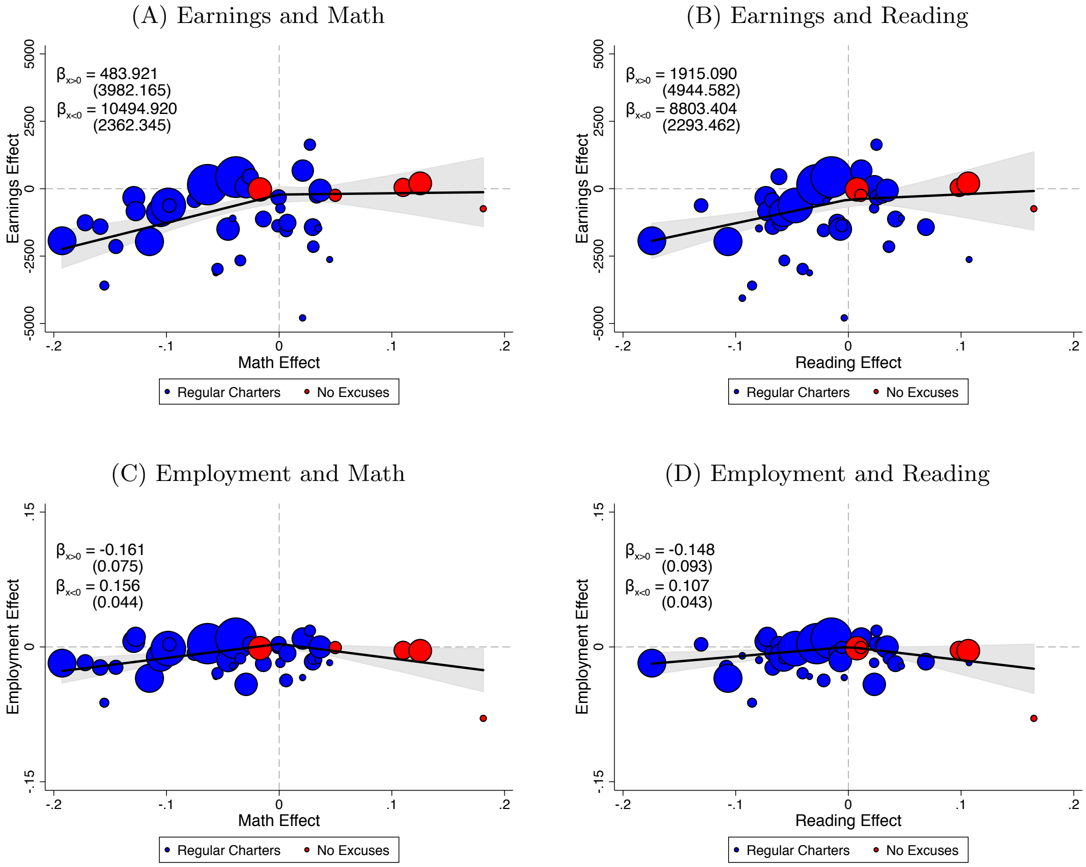


(B) Average Employment 24-26



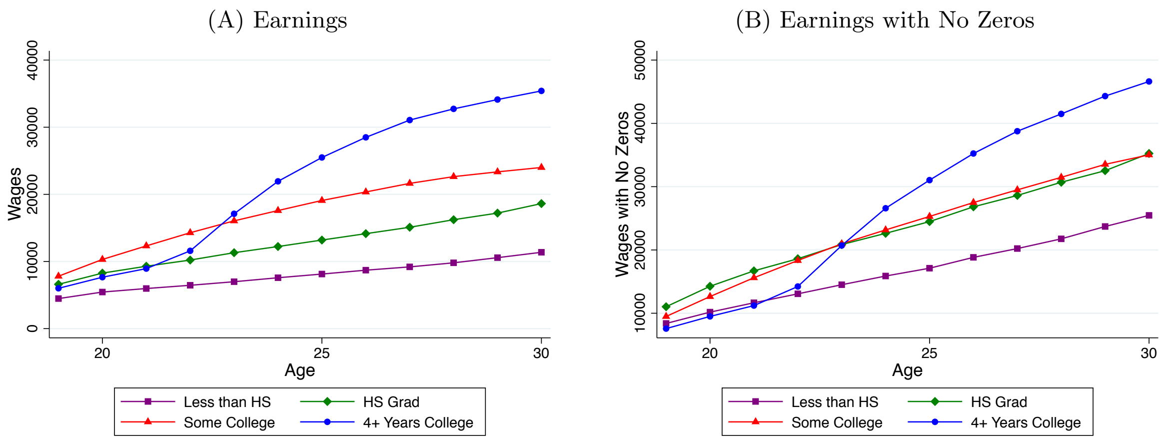
Notes: These figures plot school-level earnings and employment effects by charter school type. Earnings and employment are measured eight years after high school graduation. The reported means are weighted by the number of students at each school in the earnings effects estimation sample. See Online Appendix B for details on the sample and variable construction and Online Appendix C for details on estimation of the school effects.

Appendix Figure 5
Correlation of Labor Market and Math and Reading Score Effects



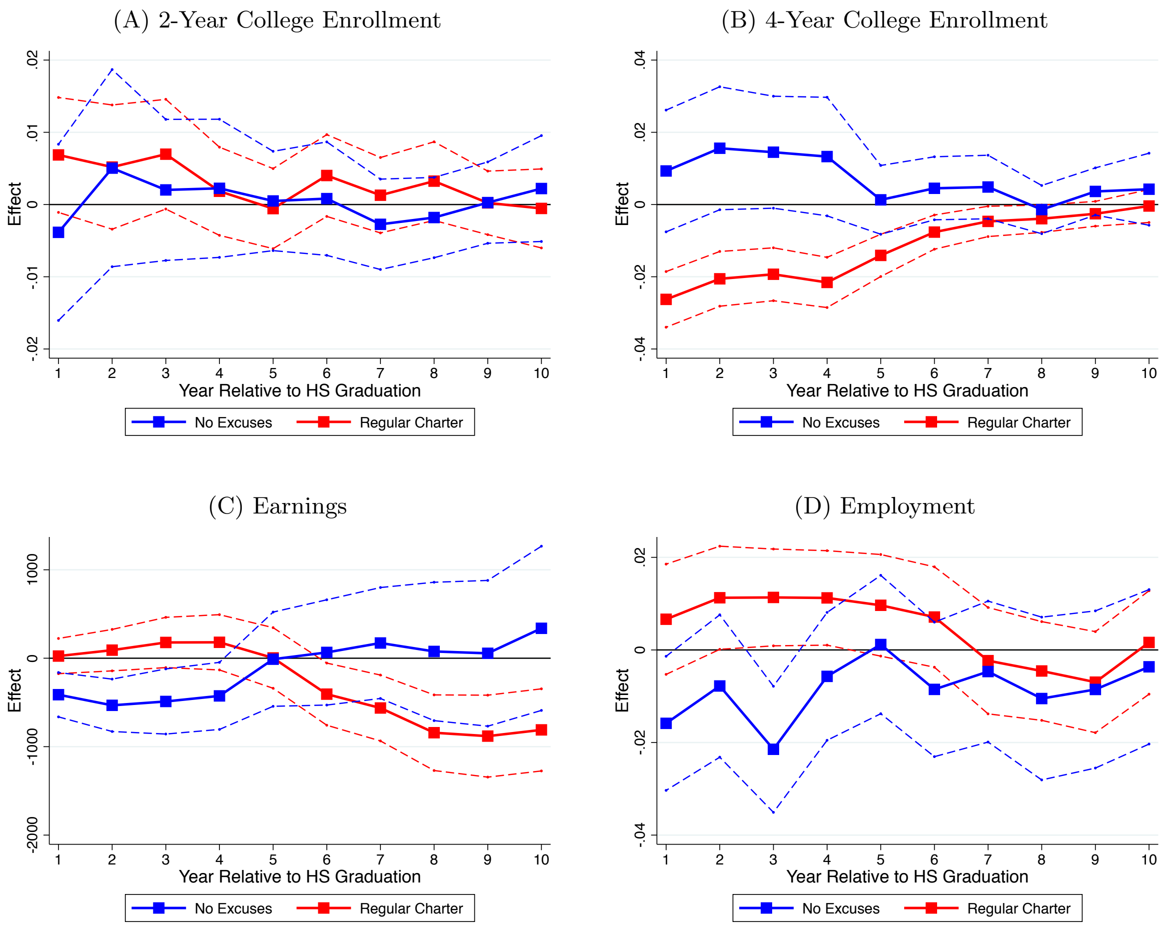
Notes: These figures plot the correlation between school-level labor market effects and school-level test score effects. Observations are weighted by the number of students at each school in the earnings estimation sample. The solid line is estimated at the school-cohort level. See Online Appendix B for details on the sample and variable construction and Online Appendix C for details on estimation of the school effects.

Appendix Figure 6
Earning Trajectories by Educational Attainment



Notes: These figures plot average earnings with and without zero earnings by educational attainment level. The sample includes students in our estimation sample graduating high school in 2002 - 2003.

Appendix Figure 7
Results by Year in the 2002-2004 Cohorts

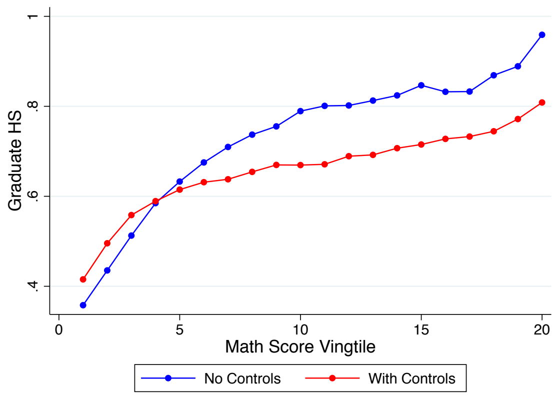


Notes: These figures plot charter school effects and 95 percent confidence intervals by school type in the 2002-2004 graduating cohorts. See Tables 3-5 notes for details on the sample and estimation framework.

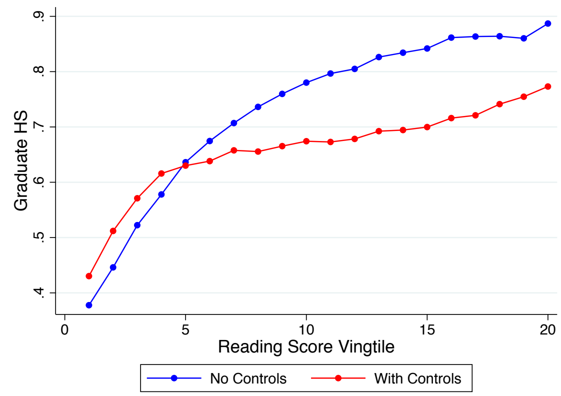
Appendix Figure 8

Correlations Between Outcomes in Adulthood and Test Scores

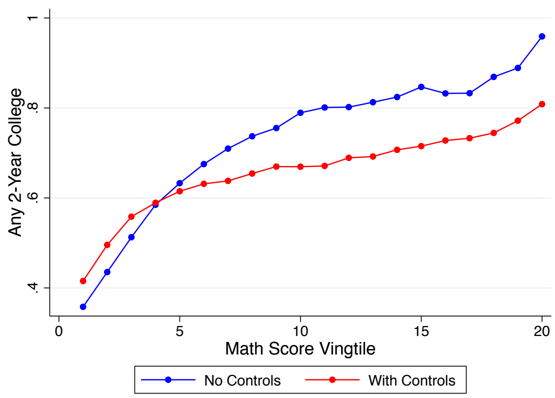
(A) Math and High School Graduation



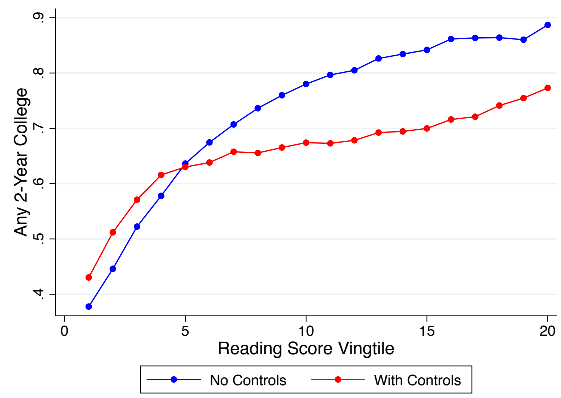
(B) Reading and High School Graduation



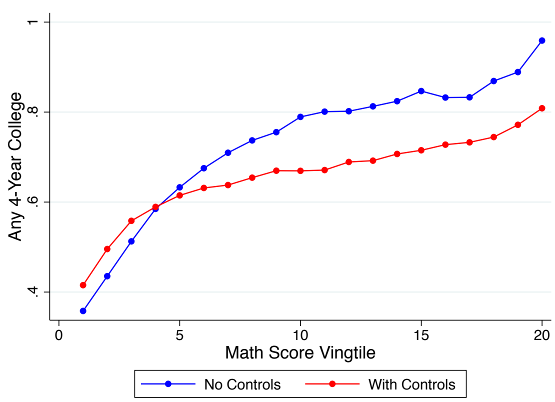
(C) Math and Two-Year College Enrollment



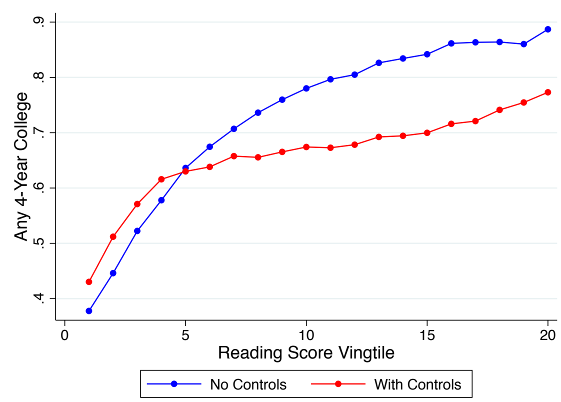
(D) Reading and Two-Year College Enrollment



(E) Math and Four-Year College Enrollment



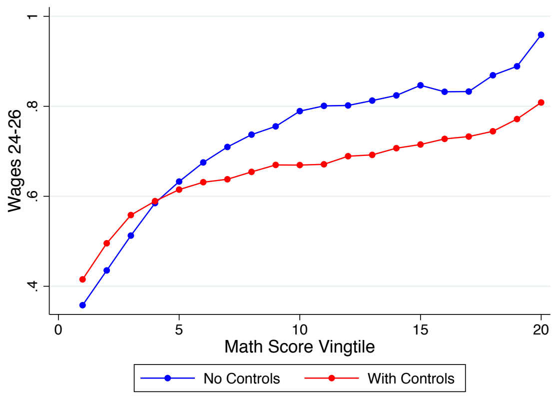
(F) Reading and Four-Year College Enrollment



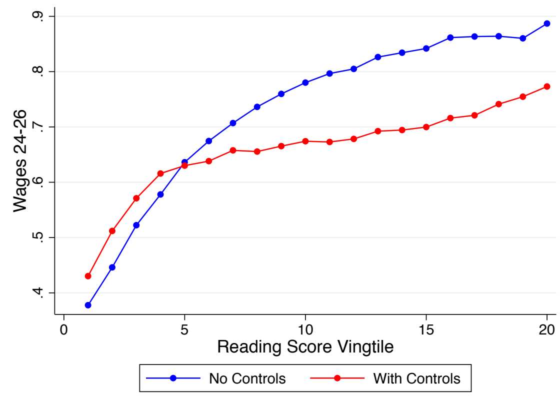
Appendix Figure 8 Continued

Correlations Between Outcomes in Adulthood and Test Scores

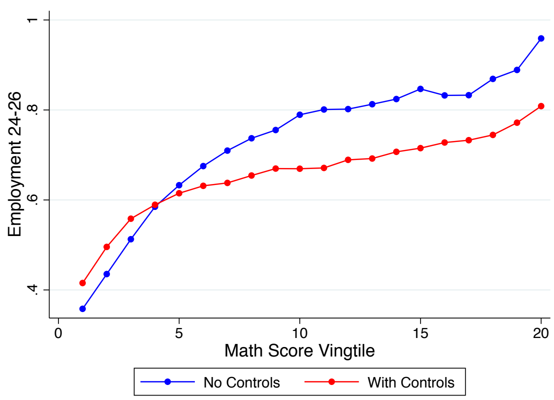
(G) Math and Earnings 24-26



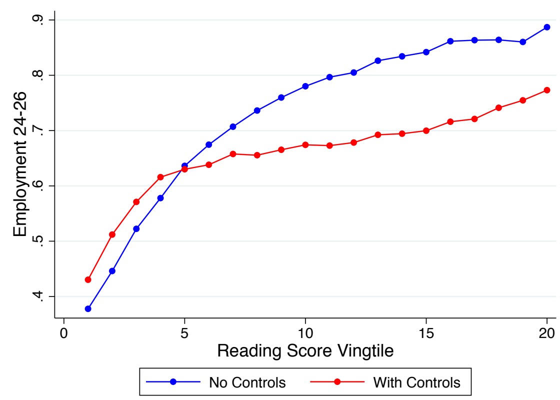
(H) Reading and Earnings 24-26



(I) Math and Employment 24-26



(J) Reading and Employment 24-26



Notes: These figures present binned scatter plots corresponding to the cross-sectional regressions of outcomes in adulthood on test scores. See notes to Table 2 and Online Appendix B for further information on the variable definitions and sample specification. In each panel, the blue series corresponds to the estimates without controls. The red series corresponds to the estimates with controls. To construct the blue series, we bin raw test scores into twenty equal-sized groups (vingtiles) and plot the means of the outcome within each bin against the mean test score within each bin. To construct the red series, we first regress both the test scores and adult outcomes on the individual controls and compute residuals of both variables. We then divide the test score residuals into twenty equal-sized groups and plot the means of the outcome residuals within each bin against the mean test score residuals within each bin. Finally, we add back the unconditional mean of the adult outcome in the estimation sample to facilitate interpretation of the scale.

Online Appendix B: Data Appendix

We use administrative data from the Texas Education Research Center (ERC) that allows us to follow all Texas public school students from Kindergarten to college to the labor market. The ERC data include information on student demographics and outcomes from the Texas Education Agency (TEA), college enrollment records from the Texas Higher Education Coordinating Board (THECB), and administrative earnings records from the Texas Workforce Commission (TWC). This appendix describes these data sets and details the procedures used to clean and match them.

Texas Education Agency

Overview: The TEA data include information on student race, gender, free and reduced-price lunch eligibility, limited English proficiency, special education status, at risk designation, and graduation year. The TEA data also include information on each student's grade, school, and state math and reading test scores in each year. These data are available for all Texas public school students for the 1994-1995 to 2012-2013 school years.

State Assessments: Mathematics and reading assessments come from two statewide criterion-referenced achievement tests that were administered during our period of study. From 1993-2003, the Texas Assessment of Academic Skills (TAAS) was administered each spring to eligible students enrolled in grades three through eight. An exit level test was also administered in grade 10 in reading, writing, and mathematics as a requirement for graduation. In 2003, Texas introduced a new exam called the Texas Assessment of Knowledge and Skills (TAKS). TAKS expanded the number of subjects that students were required to demonstrate proficiency in and elevated the level of difficulty of the tests. TAKS was administered to grades 3-10 in reading and mathematics. An exit level test was also administered in grade 11 in English language arts, mathematics, science, and social studies as a requirement for graduation. Spanish versions of the TAKS test were offered for students with limited English proficiency in grades 3-6. TAKS assesses grade-specific content in grades 3-8. In grades 9-11, TAKS assesses content from specific courses. In our analysis, we normalize all math and reading scaled scores to have a mean of zero and a standard deviation of one in each year and grade level for the entire state of Texas. Since TAAS and TAKS are taken in different years, they are standardized separately.

High School Graduation Variables: We code a student as having graduated from high school if the Texas graduation files indicate that (1) she received a valid diploma or (2) if she enrolled in a two- or four-year college in any subsequent year. All students who are missing from both the graduation files and the college enrollment files are assumed to have not graduated from high school.

Transfer Variables: We code students as having transferred to an out-of-state school if they reenrolled outside of Texas, intended to reenroll outside of Texas, returned to their home country, or graduated from another state for the military. We also code a small number of students who are deceased as having transferred to an out-of-state school.

Demographic Variables: Demographic variables that should not vary from year to year (race, gender, immigrant status) were pulled from enrollment files, with precedence given to the most recent files. Race consisted of the following categories: white, black, Hispanic, Asian, Native American, and other race. These categories were considered mutually exclusive. Gender was coded as male, female, or missing. Demographic variables that may vary from year to year (free lunch status, English Language Learner status, at-risk status, gifted status, and special education designation) were pulled from the relevant enrollment file.[16,17]

<u>Texas Higher Education Coordinating Board</u>

Overview: Information on college outcomes come from the THECB. The THECB collects and centralizes data for students attending Texas public universities, private universities, community colleges, and health related institutions. The data include information on each student's enrollment, graduation, and grade in each year. The THECB data are available for the 2004-2005 to 2012-2013 school years.

Enrollment Variables: We code a student as having enrolled in college if she ever attends a school in the THECB data. Two-year and four-year college results are coded similarly. All students missing from these files are assumed to have not enrolled in college.

<u>Texas Workforce Commission</u>

Overview: Employment and earnings outcomes are measured using data from the TWC. The TWC data record quarterly earnings for all Texas employees, with information on approximately 12 million individuals each year. The data include information on each individual's earnings, number of employers, and size of each employer. The TWC data are available from 2002 to 2014.

[16] A student is income-eligible for free lunch if her family income is below 130 percent of the federal poverty guidelines, or categorically eligible if (1) the student's household receives assistance under the Food Stamp Program, the Food Distribution Program on Indian Reservations (FDPIR), or the Temporary Assistance for Needy Families Program (TANF); (2) the student was enrolled in Head Start on the basis of meeting that program's low-income criteria; (3) the student is homeless; (4) the student is a migrant child; or (5) the student is identified by the local education liaison as a runaway child receiving assistance from a program under the Runaway and Homeless Youth Act. Determination of special education or ELL status is done by HISD Special Education Services and the HISD Language Proficiency Assessment Committee.

[17] Texas Education Code Section 29.081 defines a student as at-risk of dropping out if any of the following is true: (1) the student was held back in one grade level; (2) the student is in grades 7-12, did not maintain an average equivalent to 70 on a scale of 100 in two or more subjects in the foundation curriculum during a semester in the preceding or current school year, or is not maintaining such an average in two or more subjects in the foundation curriculum in the current semester; (3) did not perform satisfactorily on an assessment, and who has not in the previous or current school year subsequently performed on that instrument or another appropriate instrument at a level equal to at least 110 percent of the level of satisfactory performance on that instrument; (4) if the student is in PK-3 and did not perform satisfactorily on a readiness test or assessment instrument administered during the current school year; (5) is pregnant or is a parent; (6) has been placed in an alternative education program during the preceding or current school year; (7) has been expelled during the preceding or current school year; (8) is currently on parole, probation, deferred prosecution, or other conditional release; (9) was previously reported as having dropped out of school; (10) is a student of limited English proficiency; (11) is in the custody or care of the Department of Protective and Regulatory Services or has been referred to the department during the current school year; (12) is homeless; or (13) currently or in the past school year resided in a residential placement facility.

Earnings and Employment Variables: We assume that individuals with no reported earnings in a given year are unemployed. Employment is an indicator for having nonzero earnings in the relevant year. We also find that our main results are similar to dropping individuals with no reported earnings.

National Student Clearinghouse

Overview: To explore the robustness of our college results, we also use data from the National Student Clearinghouse (NSC) that contain information on student enrollment for over 90 percent of all colleges and universities in the United States. The NSC data is only available from 2008 to 2009.

Enrollment Variables: We code a student as having enrolled in college if she ever attends a school in the NSC data. Two-year and four-year college results are coded similarly. All students missing from these files are assumed to have not enrolled in any college.

Sample Restrictions

School Level: We employ three sample restrictions at the school level. First, we restrict our analysis to open-enrollment charter schools that target the general population of public school students. We therefore exclude both district charters that are operated by the traditional public school system, and "alternative instruction" charter schools that operate under different accountability standards and typically work with non-traditional students such as high-school dropouts. We also exclude charter schools for abused students, autistic students, shelters, residential treatment centers, juvenile detention centers, juvenile justice alternative education programs, virtual charter schools, and sports academies. Second, we drop schools who have fewer than ten students enrolled during our sample period. In the school x cohort level analysis, we also drop cohorts with fewer than 10 students enrolled during our sample period. Third, we restrict our primary analysis sample to charter schools whose oldest cohort graduated high school in or before 2005-2006. This restriction ensures that students in our sample are approximately 26 years old or older in the most recent earnings data.

Student Level: We also make six sample restrictions to the student data with the overarching goal of having a valid comparison sample. Table 1 provides details on the number of students dropped by each sample restriction. With no restrictions, there are 1,420,877 students in regular public schools, 1,358 students in No Excuses charter schools, and 4,905 students in regular charter schools. Column 2 omits students who did not attend a public elementary school in fourth grade. This decreases the sample by 7,646 students in non-charters, but only by 13 students in No Excuses Charters and 75 in regular charters. Column 3 leaves out students with missing baseline covariates such as gender or race. Column 4 drops students with no middle or high school test score. Column 5 drops students who transferred to an out-of-state primary or secondary school. Column 6 drops charter schools with a cohort size fewer than ten. In our final estimation sample – which includes all students for which there is a match cell on fourth grade school, cohort, gender, and race – there are 188,666 students in non-charters, 1,039 in No Excuses charters, and 3,860 students in regular charter schools. The majority of the non-charter sample was dropped due to not matching individuals in the charter sample, primarily because these students attend schools in districts without a charter school.

Classifying Charter Schools

We use information from the Texas Charter School Association and school websites to classify eligible charters as No Excuses schools, college preparatory schools, specialized mission schools, or regular charters. The Texas Charter School Association classifies schools as college preparatory, specialized mission, or regular/unclassified. College preparatory schools have a stated mission to prepare students for a 4-year college degree. Most college preparatory schools also have dedicated college placement offices and track students through college graduation. Specialized mission charters have distinctive focus areas such as religious study, fine arts, STEM, or classics. These schools may also have strong college readiness programs. Regular charter schools are schools that do not fall into any of the above categories.

Charter school classifications are available for 30 out of the 45 schools in our sample. For the 15 schools with missing classifications, we determined school type using mission statements from each school's website. For two schools, mission statements were unavailable. We coded both of these schools as regular charters. Results are robust to coding all 28 schools with missing information as regular charters, or coding all 28 schools as a separate group.

College preparatory charters are further classified as either No Excuses schools or regular college preparatory charters. Compared to regular college preparatory charters, No Excuses charters have higher behavioral expectations, stricter disciplinary codes, are more likely to have uniform requirements, and are more likely to have an extended school day and year. We classify No Excuses schools using information from school mission statements, charter applications, and public statements. We verified our No Excuses categorizations with numerous school administrators in Texas. The No Excuses classification in this paper largely follows the classification system used by Dobbie and Fryer (2013) and Angrist, Pathak, and Walters (2013), but is somewhat more strict than the classification system used by Baude et al. (2014). We use this stricter definition of No Excuses to focus on exemplar schools in the category.

Online Appendix C: Empirical Bayes Procedure

This appendix describes the empirical Bayes (EB) procedure that we use to adjust our estimated school effects for estimation error. The EB procedure is based on Morris (1983). Jacob and Lefgren (2007), Dimick et al. (2009), and Chandra et al. (forthcoming) provide additional examples of the EB procedure in other contexts.

The EB procedure is based on the idea that there is likely to be positive (negative) estimation error if a school's estimated effect is above (below) the mean school effect. Thus, the expected school effect is a convex combination of the estimated school effect and the mean of the underlying distribution of school effects. The relative weight on the estimated school effect is proportional to the precision of the estimate, which is based on the standard error of the coefficient estimate.

To fix ideas, suppose that we have a noisy but unbiased estimate of the effect of attending school s, $\hat{\beta}_s = \beta_s + \eta_s$, where β_s is the true effect of attending school s and η_s is a school-specific mean zero error term. We assume that the estimated school effect is independently normally distributed around the true school effect with known variance of π_s^2. In this context, π_s^2 can be thought of as the variance of the estimation error. We also assume that the true school effect β_s is independently normally distributed with an underlying mean of $\bar{\beta}$ and variance of σ^2 for the full distribution of schools.

The EB adjusted estimate is equal to the expected value of the school effect conditional on the estimated effect $\hat{\beta}_s$ and the parameters π_s^2, $\bar{\beta}$, and σ^2 is:

$$E[\beta_s|\hat{\beta}_s, \pi_s^2, \bar{\beta}, \sigma^2] = \lambda_s \hat{\beta}_s + (1 - \lambda_s)\bar{\beta}$$
$$\lambda_s = \frac{\pi_s^2}{\pi_s^2 + \sigma^2}$$

As discussed above, the EB adjusted estimate attenuates the unadjusted estimated school effect $\hat{\beta}_s$ toward the underlying mean of the full distribution of school effects $\bar{\beta}$. As the variance of the estimation error π_s^2 increases, the EB adjusted estimate increasingly converges to the underlying mean of the school effects $\bar{\beta}$.

In practice, the parameters needed to construct the EB adjusted estimate are unknown and must be estimated. The estimated school effects $\hat{\beta}_s$ are the unadjusted coefficient estimates from our main student-level estimating equation described in the text. The standard errors on these unadjusted coefficient estimates are an estimate of the standard deviation of the asymptotic distribution of $\hat{\beta}_s$. We estimate π_s^2 by squaring these standard errors. We estimate the mean of the distribution of school effects $\bar{\beta}$ and the variance of the error term σ^2 using the method proposed by Morris (1983), which uses an iterative process to calculate the feasible best estimate of the appropriate shrinkage factor λ_s. This method also incorporates a degrees of freedom adjustment to account for the fact that we are estimating the mean and variance parameters. The above EB procedure yields unbiased estimates of the expected effect of attending each school in our sample for any particular outcome.