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, due to imprecision, have a statistically insignificant impact on earnings – though the coefficient is almost identical to what one would expect given the correlation between test scores and wages. Other types of charter schools decrease test scores, four-year college enrollment, and earnings, and, surprisingly, the decrease in wages is more negative than one would anticipate. Using school-level estimates, we find that charter schools that decrease average test scores also tend to decrease earnings, while charter schools that increase average test scores have no discernible impact on earnings. In contrast, high school graduation effects and four-year college enrollment 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 United States and the United Kingdom over the past 20 years. Currently, five percent of all U.S. public students attend charter schools, and school districts such as New Orleans, Detroit, Camden, and District of Columbia are now majority charter. The National Alliance for Public Charter Schools estimates that there are over 1 million children on charter wait lists across America.

There is a burgeoning 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). As a result, charter schools seem likely to increase employment and earnings and potentially reduce intergenerational poverty. Consistent with this argument, Dobbie and Fryer (2015) estimate 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]

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 Texas charter sector is 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 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, one 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 that we were able to successfully contact had admissions lottery data for the relevant cohorts.[2]

[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 which 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).

[2] We also attempted to validate the observational versus lottery design on short-run test scores using oversubscribed lotteries. We successfully contacted 28 of the 57 schools in our analysis sample. Only 2 of the 28 schools initially reported that they had lottery data available. Unfortunately, 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

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 4th grade school x cohort x race x 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 controlling for any observable differences between charter and non-charter students.

The key identifying assumption of our empirical design is that school x cohort x race x gender 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. (2017) show that this empirical approach works less well in Denver, with observational estimates yielding treatment effects of 0.3 standard deviations (hereafter σ) and lottery based estimates yielding effects closer to 0.5σ.

In Section V and Online Appendix C, 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 settings in which lottery and observational strategies yield similar point estimates. There is also no evidence that our earnings estimates are more sensitive to the inclusion of baseline controls compared to our test score estimates. Nevertheless, our estimates should be interpreted with this important identifying assumption in mind.

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 only slightly 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 0.023σ (se=0.003). Similarly, charter attendance increases high school graduation by 1.4 (se=0.1) percentage points, two-year college enrollment by 0.8 (se=0.2) percentage points, and four-year college enrollment by 0.7 (se=0.2) percentage points.

Turning to labor market outcomes, the focus of our analysis, we find that charter attendance is associated with a \$143.55 (se=64.50) *decrease* in annual earnings in the state of Texas, with no detectable impact on employment in Texas. Taken together, these results suggest negative impacts of the average charter school in Texas.

However, investigating charter effects at the mean masks potential heterogeneity by charter

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.

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 0.093σ (se=0.004) per year of attendance, high school graduation increases by 2.3 (se=0.2) percentage points, and enrollment in four-year colleges increases by 2.8 (se=0.3) percentage points.

At first glance, our estimates may seem small relative to some empirical estimates in Abdulkadiroğlu et al. (2011) and Dobbie and Fryer (2013). Recall, these estimates are for oversubscribed schools. Similar estimates on all charters are significantly smaller. For example, in Abdulkadiroğlu et al. (2011), observational estimates for middle school reading (respectively, math) effects are 0.174σ (respectively, 0.316σ) for charters with lotteries, but 0.098σ (respectively, 0.148σ) for charters without lotteries. In the same work, the observational estimates for high school reading (respectively, math) effects are 0.258σ (respectively, 0.269σ) for charters with lotteries, but 0.169σ (respectively, 0.122σ) for charters without lotteries. Any comparison of our test score effects with lottery-based estimates for oversubscribed charters should take this caveat into account.

We also find that attending a No Excuses charter school increases persistence in four-year colleges. Yet, despite these short-run human capital benefits, the impact of attending a No Excuses charter school on earnings in Texas is a statistically insignificant $129.27 (se=102.23) per year of attendance, in line with the cross-sectional correlation between test scores and average earnings observed in Texas. Given the academic impacts on math (respectively, reading) scores, one would expect a $320.88 increase (respectively, $159.41) based on the cross-sectional relationship between test scores and average earnings.

Regular charters (defined as charters not implementing the No Excuses approach) decrease state test scores by 0.034σ (se=0.003) per year of attendance, increase high school graduation by only 0.7 (se=0.2) percentage points, and decrease four-year college enrollment by 1.0 (se=0.2) percentage points. Two-year college enrollment increases by 1.8 (se=0.2) 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 in Texas is -$369.51 (se=82.14) per year of attendance. Surprisingly, this is $210.60 (respectively, $338.02) less than anticipated given the cross-sectional correlations between math (respectively, reading) scores and average earnings.

Subsample estimates by race yield similar results. 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 0.122σ (se=0.004), similar to the treatment effects estimated by observational research designs from No Excuses charter 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 four-year college. The impact on earnings in Texas is $212.49 (se=111.12) for minority students, about what we expect. In other words, there are economically and statistically significant effects of attending a No Excuses school on the test scores and educational attainment of minority students, and this is reflected in the earnings effect, but measured with considerable noise.[3]

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 for schools with above and below median effects to examine trends in both the left and right tails of the distribution.

Separately estimating the school-level correlation between pooled test scores and earnings effects above and below the median test score effect yields another set of surprising results. Below median, a 0.1σ increase in a school's state pooled test score effect is associated with a \$1,510.04 (se=206.57) increase in the school's earnings effect. Above median, however, a 0.1σ increase in a school's pooled test score effect is associated with a marginally significant \$624.47 (se=332.90) increase in earnings, with the p-value on the difference in the below and above median associations (\$885.57) equaling 0.08. Similar to the test score results, schools that have above median impacts on two-year college enrollment have small or no impact on earnings, while schools that have below median effects on two-year college enrollment tend to have negative effects on earnings. Four-year college enrollment effects, however, are predictive of earnings effects throughout the distribution.

There is also a robust positive correlation of high school graduation effects with labor market outcomes throughout the distribution. Below median, a ten percentage point increase in a school's high school graduation effect is associated with a \$1,629.81 (se=482.96) increase in the school's earnings effect. Similarly, above median, a ten percentage point increase in a school's high school graduation effect is associated with a \$2,647.80 (se=\$564.09) increase in the school's earnings effect. 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.[4] These estimates also suggest that high school graduation may be an additional short-run

[3]To put these results into perspective, we estimate how much our No Excuses effects would have to be to close the earnings gap between Blacks and whites in Texas. Using CPS data for individuals aged 25-27 living in Texas between 2009-2016 (7-9 years after our cohorts leave high school), we find a raw gap in average personal income of \$3,534.55, out of a sample mean of \$30,180.35. Assuming 7 years of charter attendance (middle and high school), the yearly No Excuses effect on earnings would have to be \$504.94 to close this gap. Based on the cross-sectional correlations between test scores and earnings in our sample, the math (respectively, reading) test score effects would have to be 0.165σ (respectively, 0.257σ). Note that these are lower bounds on the required effect sizes. The average number of years spent in a No Excuses charters is 3.3 years in our estimation sample. The required earnings effect size to close the gap would then be \$1,071.08, and the required math (respectively, reading) effects would be 0.350σ (respectively, 0.544σ). We obtain similar conclusions when (i) using only wage income instead of total income, and (ii) estimating the wage gap controlling for education, sex, marital and armed forces statuses, and fixed effects for survey year, respondent age, and metropolitan area.

[4]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,

instrument along with state test scores to evaluate the efficacy of charter schools, particularly in the right tail of the test score distribution.

We explore three threats to the internal validity of our estimates: selective enrollment into charters, selective attrition from our earnings data, and alternative clustering of standard errors. We explore the extent to which selection bias into charters may threaten our results by examining whether baseline characteristics differ by charter type, and conducting falsification tests using outcomes that we do not directly control for. We find economically small but statistically significant differences between charter and non-charter students. We repeat these exercises in the Dobbie and Fryer (2013) data. We find more selection on charter attendance in NYC – where observational and lottery estimates closely mirror each other – than in Texas. Together, we take these results as suggesting that there is modest selection into charters based on observables, but that our estimates are unlikely to be significantly biased. Or, at least, less biased than other analyses which demonstrate the similarity between observational and lottery-based estimates.

A second caveat is that we are only able to observe earnings outcomes for individuals employed in the state of Texas. For the approximately 26 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-of-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-of-state migration significantly biases our main results. Last, we find that our main results are robust to alternative ways of clustering our standard errors.

In parallel work, Sass et al. (2016) estimate the impact of attending a charter high school on 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 is \$493.15 (se=987.30), and is statistically insignificant. Beyond the impact of charter schools on mean earnings, there is not much overlap between Sass et al. (2016) and our approach.[5]

We conclude with a more speculative discussion designed to help better interpret our set of facts, although we are admittedly quite limited in the breadth of hypotheses we can test due to data constraints. First, we show that neither the age of the sample nor returns to work experience

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).

[5]Unfortunately, it is not possible to replicate their research design in our data. During our sample period, there are only 31 students who graduate from a charter middle school and attend a non-charter high school. This result is due, at least in part, to the fact that the majority of charter schools in our sample serve both middle and high school students. See Appendix Table 1 for additional details on the charter schools in our sample.

for non-college students are likely 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, indicating that our results are not driven by returns to early work experience among non-college goers. 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. Finally, we discuss the extent to which multitasking may explain our results.

The remainder of the paper is structured as follows. Section II discusses the charter sector 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 and discusses potential threats to the validity of our estimates. Section VI estimates the correlation between a school's human capital effects and its labor market outcomes. Section VII discusses potential interpretations of our results, and Section VIII concludes. There are four 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 tests of our identifying assumptions. Online Appendix D provides additional details on the empirical Bayes procedure we use to adjust our estimated school effects for estimation error.

II. 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.[6] Open-enrollment charter schools receive public funding but are not subject to the regulatory restrictions of traditional 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 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

[6] 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.

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 who operate under different accountability standards. We also exclude charter schools for abused and autistic students; schools housed in shelters, residential treatment centers, and 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 2008-2009. 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 262 school x cohort observations from 57 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. (2017) 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 charter 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.

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. Online 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, receives accommodations for limited English proficiency, is categorized as "at-risk," or receives special education accommodations. 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 for the 2008-2009 and 2009-2010 academic years. 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 datasets. 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 2017.

We assume that individuals with no reported earnings in a given year are unemployed in Texas. 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 2,305,979 students in regular public schools, 3,300 students in No Excuses charter schools, and 12,324 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 13,412 students in non-charters, but only by 178 students in No Excuses Charters and 1,586 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 scores. 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 376,208 students in non-charters, 2,550 in No Excuses charters, and 8,537 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, 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 charter 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 for the full sample and the estimation sample are strikingly similar on most dimensions. In the estimation sample, students enrolled in 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 charter schools and two years for regular charter schools. Students enrolled in any charter school 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 charter schools relative to regular charters or non-charter schools.

Putting these pieces together, the summary statistics are consistent with previous work describing the characteristics of charter school enrollees. Students enrolled in charter schools are more likely to be minority, more likely to be eligible for free or reduced-price lunch (a measure of poverty), and more likely to be labeled at-risk of dropping out. Students enrolled in No Excuses charter schools also have somewhat higher baseline test scores than students enrolled in either regular charter schools or non-charter schools. Consistent with these descriptive statistics, Allen and Consoletti (2007, 2008), for example, find that students enrolled in charter schools are more likely to be a minority and more likely to receive free lunch and be considered 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

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.

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 4th grade school x cohort level when outcomes are observed only once in a student's career. In test score regressions where we observe multiple outcomes per student, we cluster at the student level.

Our matching and regression approach semi-parametrically controls for any differences between school x cohort x race x gender 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. The key identifying assumption of our approach is that our school x cohort x race x gender 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. In Section V.D and Appendix C, we discuss threats to validity and provide support for this identifying assumption in our data.

B. Correlation of School Effects on Earnings and Academic Outcomes

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:

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

where β^y_{cs} is a school's effect for cohort c on labor market outcomes y, and β^t_{cs} 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 its median. 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. We estimate Equation (2) at the school x cohort level and cluster standard errors at the school level.

Following Chetty, Friedman, and Rockoff (2014), we estimate Equation (2) using a leave-cohort-out design that eliminates any mechanical correlation between β^y_{cs} and β^t_{cs}. Specifically, we calculate β^t_{cs} for cohort c using the short-run outcomes from all other cohorts, excluding the outcomes from cohort c itself. For example, when estimating a school's effects on the labor market outcomes of students graduating in 2002-2003, we estimate β^t_{cs} based on short-run outcomes from students in all cohorts of the sample except 2002-2003. We maximize precision by calculating these leave-cohort-out estimates using data from all cohorts graduating high school, not just the subset of older cohorts for which we observe earnings outcomes.

We adjust for estimation error in β^t_{cs} using an empirical Bayes procedure when estimating Equation (2) (e.g. Morris 1983, Jacob and Rothstein 2016). 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 when using unadjusted estimates as right hand side variables.[7] The empirical Bayes procedure is based on the idea that there is likely to be positive (respectively, negative) estimation error if a school's estimated effect is above (respectively, 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 of the coefficient estimate. Online Appendix D 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 in Texas.

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 replace 4th grade school x cohort fixed effects with 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 student level (test scores) or 4th grade school x cohort level (all other outcomes).[8]

[7]This may be an issue theoretically, but empirically it does not make a difference to our results (see Appendix Figures 1-2 in comparison to Figures 1-2).

[8]Appendix Tables 2-4 report results using an indicator for having ever attended the indicated charter school as an alternative. The significant test score and educational attainment effects by charter type are robust to this alternative design, except for the high school graduation effect for regular charters. The same holds for regular charter effects on average earnings. No Excuses effects on earnings become negative, but insignificant, whereas effects on employment in Texas remain negative and become significant.

Consistent with the prior literature, the mean impact of charter schools on test scores is relatively small in economic terms (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 0.019σ (se=0.003) on math scores and 0.028σ (se=0.003) on reading scores. Stacking both math and reading test scores, we find that attending a charter school for one year increases test scores by 0.023σ (se=0.003). 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 0.105σ (se=0.005) in math and 0.081σ (se=0.004) in reading. This test score effect is 78% as large in math and 61% as large in reading as the average observational estimates of middle and high school charters in Abdulkadiroğlu et al. (2011). In contrast, the impact of attending a regular (non-No Excuses) charter school is -0.052σ (se=0.004) in math and -0.016σ (se=0.004) in reading.[9]

Table 4 presents similar estimates for high school graduation, two-year college enrollment, and four-year college enrollment.[10] At the mean, the effect of attending a charter school is 1.4 (se=0.1) percentage points for high school graduation, 0.8 (se=0.2) percentage points for two-year college enrollment, and 0.7 (se=0.2) 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=0.3) percentage points, compared to -1.0 (se=0.2) percentage points for regular charters.[11] High school graduation effects are also larger for No Excuses and regular charters, while two-year college enrollment effects

[9] Appendix Figure 3 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 D. The reported means are weighted by the number of students at each school in the earnings effects estimation sample. The reported standard deviations are for the adjusted school-specific estimates, and therefore represent a lower bound for the true standard deviation of the school effects. The distribution of regular charter school effectiveness is similar to the 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 3 is the consistency of the No Excuses school test score effects, with almost 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 (2016) for additional discussion of this issue.

[10] Deming et al. (2016) 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 Tables 5-6 we use data from the National Student Clearinghouse to demonstrate the robustness of the college enrollment results to out-of-state migration.

[11] Appendix Table 7 presents analogous results for the number of years enrolled at two- and four-year colleges. These results are consistent with the ever enrolled results. No Excuses charters decrease years enrolled in two-year colleges and increase years enrolled in four-year colleges, while regular charters increase years enrolled in two-year colleges and decrease years enrolled in four-year colleges. While we do not observe reliable measures of college graduation, Appendix Table 8 presents results for being enrolled two or more years in a two-year college and four or more years in a four-year college. We find the same patterns as in our ever enrolled and years enrolled models. Taking these outcomes as proxies for graduation, these results suggest that No Excuses charters may also be effective at increasing four-year college graduation rates.

are approximately zero for No Excuses charter and positive for regular charters. These results are consistent with No Excuses charters only increasing the number of students attending four-year colleges, while regular charters shift students who otherwise would have attended a four-year school to a two-year school.[12]

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. If anything, our test score effects for No Excuses charters are smaller than those found in much of the literature. This is also 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 0.946 for math test scores and 0.842 for reading test scores.

B. Labor Market Outcomes

Table 5 presents estimates of Equation (1) for average earnings and employment in the state of Texas for ages 25-27.[13] 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 -\$143.55 (se=64.50). Thus, if a student attended a charter school for 5 years, expected annual earnings in the state of Texas would be about \$700 lower. Consistent with our test score and attainment results, No Excuses charters have less negative outcomes. The impact of attending a No Excuses charter for one year is a statistically insignificant \$129.27 (se=102.23), in line with the cross-sectional correlation between test scores and average earnings observed in Texas. Given the academic impacts on math (respectively, reading) scores, one would expect a \$320.88 increase (respectively, \$159.41) based on the cross-sectional relationship between test scores and average earnings in Appendix Table 9. Regular charters have a surprisingly negative impact on earnings of -\$369.51 (se=82.14).[14] This is \$210.60 (respectively, \$338.02) less than anticipated given the cross-sectional correlations between math (respectively, reading) scores and average earnings.

Results for employment in the state of Texas are less precise and not statistically distinguishable from zero for both No Excuses charters or regular charters.[15]

[12]Following our test score results from Appendix Figure 3, Appendix Figure 4 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. For regular charters, the effects are centered just below zero for high school graduation, above zero for two-year college enrollment, and below zero for four-year college enrollment. For No Excuses charters, the effects are centered above zero for high school graduation and four-year college enrollment, and just below zero for two-year college enrollment.

[13]Dobbie and Fryer (2016) show that results using maximum, instead of average earnings yields virtually identical results.

[14]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.

[15]Appendix Figure 5 plots school-specific estimates of the earnings and employment effects for both No Excuses

C. Subsample Results

Appendix Tables 10A-10C report estimates by gender, baseline test scores, and race, respectively. At the mean, charter schools are equally effective at educating male and female students. No Excuses schools have slightly larger effects on high school graduation and four-year college enrollment for male students, however, but smaller effects for earnings in Texas. For baseline test scores, low-skill students are also more likely to experience gains across educational outcomes. Earnings effects are also larger for low-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 0.042σ (se=0.003) for blacks and Hispanics and -0.039σ (se=0.005) for whites and Asians. The difference, 0.081σ, 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 in Texas is $36.67 (se=70.81) for blacks and Hispanics and -$619.91 (se=138.24) for whites and Asians.[16]

No Excuses charter 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 0.122σ (se=0.004) for black and Hispanic students and -0.007σ (se=0.007) 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 0.610σ higher test scores. These effects are similar in size to estimates of No Excuses charter 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 charter 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 in Texas from No Excuses charters is $212.49 (se=111.11). For whites and Asians, the earnings effect from No Excuses charters is -$153.26 (se=227.68). The p-value on this difference is 0.147 for No Excuses charters.

D. Threats to Validity

In this section, we discuss three potential threats to the internal validity of our estimates: (1) selective enrollment in charter schools, (2) selective attrition from our earnings data, and (3) alternative clustering of standard errors.

Selective Charter Enrollment: As discussed above, the key identifying assumption of our approach

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 below zero for No Excuses charters.

[16] We also find that, for educational outcomes, Hispanics perform better than blacks in No Excuses charters, but worse in regular charters [results not shown in tabular form]. The earnings effects are insignificant for both groups.

is that our school x cohort x race x gender 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 this type of 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 for charter schools in Boston or New York City or for regular public schools in Charlotte-Mecklenburg.

In Online Appendix C, we discuss three partial tests for selection bias on observable characteristics in our data. First, we regress each baseline characteristic on the number of years at the indicated charter school type, school x cohort x race x gender effects, and all other baseline controls. Second, we conduct 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. Finally, we conduct a similar exercise using predicted earnings and employment for ages 25-27. We predict earnings using the relationship between actual earnings and employment with the baseline controls used in Equation (1). In all three tests, we find economically small but statistically significant differences between charter and non-charter students.

To better understand how to interpret these results, we also 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. Specifically, we replicate our specifications from Online Appendix C using information from NYC charter schools where Dobbie and Fryer (2013) have shown that lottery-based and observational estimates are highly correlated. If anything, these falsification tests reveal 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. Or, at least, less biased than other analyses which demonstrate the similarity between observational and lottery-based estimates.

A second concern is 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, these lagged scores do 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 lagged test scores are 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 data.

To explore the importance of this issue in our context, Appendix Figure 6 tests the sensitivity of both our test score and earnings estimates to the regression specification. We construct each panel by estimating the effect of each school using both our baseline specification controlling for school x cohort x race x gender effects and the full set of baseline controls, and a less-restrictive specification dropping either the school x cohort x race x gender effects, the cubic in baseline test scores, or both. We then plot the school rankings for each specification, as well as the best-fit line estimating using the number of students as weights. In all cases, we find that our test score and earnings estimates are equally sensitive to the regression specification. In Panel A, for example, we find that regressing school test score ranks from our baseline specification on school test score ranks from a specification with matched cells but only linear controls for baseline test scores yields a coefficient of 0.991. In Panel B, we find that the same regression for school earnings ranks yields a coefficient of 0.984. Comparing our baseline specification to a specification with no matched cells and a cubic for test scores, we find regression coefficients of 0.900 and 0.914 for school test score and school earnings ranks, respectively. In a final comparison of our baseline specification to a specification with no matched cells and only linear controls for baseline test scores, we find regression coefficients of 0.899 and 0.916 for school test score and school earnings ranks, respectively. None of the results suggest that the selection processes for test scores and labor market outcomes are systematically different in our context.[17]

A final concern is that our estimates are not only identified by students moving from regular to charter schools at "regular" transition grades such as the 6th and 9th grades, but also students moving at "irregular" transition grades such as the 7th and 10th grades. (See Appendix Table 11 for the distribution of grades that students first switched into charter schools for our sample.) The selection processes for students moving in "irregular" grades may be very different than for students moving in "regular" grades, potentially biasing our estimates using the full sample of charter students. Appendix Tables 12-14 explores the robustness of our results to this concern by presenting estimates where we limit the charter sample to students moving into a charter school at a "regular" transition point for that school, defined as the lowest grade served by that school and/or the 6th and 9th grades. The results are similar to our preferred estimates using the full sample of charter students, suggesting that our estimates are unlikely to be biased due to the inclusion of students transitioning at "irregular" grades.

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. Missing earnings is a well-known problem for labor economists (see Blundell and MaCurdy 1999 for a review). If charter students are more or less likely to migrate out of Texas, or the types

[17] Our results from Appendix Figure 6 also confirm the importance of controlling for school x cohort x race x gender effects in our main regression specification. The school test score and earnings ranks are extremely stable across specifications once these fixed effects are included in the regression specification, but unstable across otherwise similar specifications when these fixed effects are not included.

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.[18]

Unfortunately we are unable to directly observe out-of-state migration in our data. We therefore explore attrition from of our sample in three ways. First, we examine 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 economically small.[19] 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. At the mean, charter students are no more likely to attend two-year schools in or outside of Texas. They are, however, 0.5 (respectively, 0.8) percentage points more likely to attend a four-year college in Texas (respectively, outside of Texas). Third, using data on the same NSC cohorts, we test whether the average earnings of students that attend out-of-state colleges and return to Texas are meaningfully different from students that attend colleges in Texas. At the mean, out-of-state two-year (respectively, four-year) college attendees earn $1,736.03 (respectively, $2,898.35) less than in-state two-year (respectively, four-year) college attendees.

With these caveats in mind, 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 in Texas. 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 x free lunch status cell in column (4); and the median predicted earnings in each 4th grade school x cohort x race x gender x free lunch status x

[18] 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 8.7 percent of 24-27 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 24-27 in 2005 (as compared to 2015) were more likely to migrate out of Texas. We also find that the employment rate among 24-27 year olds in Texas is 72.1 percent in the CPS. For minority youth in Texas, the rate is 67.5 percent. In comparison, we observe non-zero earnings for 68.0 percent of individuals in our Texas data. This is strikingly consistent with our data.

[19] A related concern is that undocumented students may be particularly likely to leave our earnings sample. While we do not observe whether or not a student is undocumented in our data, other data sources suggest that the number of undocumented students in our sample is likely to be small. Data from the Migration Policy Institute for 2014 suggests that there are roughly 88,000 undocumented Latino students aged 11-17 in the Texas school system. In contrast, the Texas Education Agency shows that there are about 1,326,000 enrolled Latino students for roughly the same ages. These numbers suggest that only about 6.6% of Latino students are undocumented in Texas.

educational attainment cell in column (5). Our educational attainment variables groups students that either (i) have less than a high school degree, (ii) completed a high school degree, (iii) had some college attendance, or (iv) spent over four years in college. Results are similar using the 25th or 75th percentile of each cell instead.

Our earnings results are broadly similar regardless of how we deal with missing earnings, though some of the estimates are now significant. 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 significant \$259.84 (se=119.42) and that regular charters decrease earnings by a statistically significant \$530.77 (se=95.85). We find nearly identical results if we impute earnings at different percentiles of the predicted earnings distribution or if we estimate results using a grouped Heckit procedure (e.g. Gronau 1974, Heckman 1979) [not shown in tabular form].[20] We interpret these results as suggesting that any selective out-of-state migration is likely to be modest in our sample. Taken together, our results consistently show that the earnings effect of regular charters is negative while the earnings effect of No Excuses charters are weakly positive.

Alternative Clustering: In the specifications above, we cluster standard errors at the student level in the test score regressions, and at the 4th grade school x cohort level in all other regressions. These standard errors do not allow for heterogeneity of errors within the two groups of charters. To account for this potential issue, we cluster our standard errors at alternative levels. For test scores, we cluster (1) at the school x year level, and (2) at the student and 4th grade school levels. All our test score results are robust to these two alternative ways of clustering [results not shown in tabular form].

For all other specifications, we cluster (1) at the last school attended level, and (2) at the last school x 4th grade cohort level. Most educational attainment effects are robust to these two alternative ways of clustering [results not shown in tabular form]. The only exception is the No Excuses effect on high school graduation, which becomes insignificant when clustering at the last school level. A similar pattern holds for the earnings results. Regular charter (respectively, No Excuses) effects remain significant (respectively, insignificant) when clustering standard errors at either of those two levels. Effects on average earnings for all charters are insignificant when clustering at the last school level, but significant when clustering at the last school x 4th grade cohort level. We take these results as suggesting that our effects are robust to alternative ways of clustering that allow for errors to vary within charter types.

[20]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.

E. Comparing our Results with Sass et al. (2016)

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

At first glance, these results may seem strikingly different from our findings. However, 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 decreases to \$493.15 (se=987.30), and is statistically insignificant. Given these standard errors, the range of possible earnings effects in Sass et al. (2016) would be much larger than the range of earnings effects we observe for the average charter in our sample.

Beyond the impact of charter schools on mean earnings, there is not much overlap between Sass et al. (2016) and our approach. Unfortunately, it is not possible to replicate their research design in our data. During our sample period, there are only 31 students who graduate from a charter middle school and attend a non-charter high school. This result is due, at least in part, to the fact that the majority of charter schools in our sample serve both middle and high school 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 in Texas 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 D. 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 the median of the school valued-added distribution. Equation (2) is estimated at the school x cohort level using the leave-cohort-out procedure described in Section IV. School effects being used as right hand side variables are adjusted for estimation error using the empirical Bayes procedure described in Section IV. Standard errors are clustered at the school level.

Estimating the correlation between test scores effects and earnings effects yields different results above and below the median. For schools with below median value-added on average test scores, a 0.1σ increase in the school's test score effect is associated with a \$1,510.04 (se=206.57) increase in the school's earnings effect. For schools with above median value-added on average test scores, however, the correlation between a school's test score effect and earnings effect is only marginally significant. Specifically, a 0.1σ increase in a school's test score effect, above zero, is associated with a marginally significant \$624.47 (se=332.90) increase in earnings. The same pattern holds for math and reading effects when estimated separately. Panel B of Figure 1 suggests a similar but more muted pattern for employment effects. 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 less indicative of school success.

Figure 2 presents analogous results for high school graduation and two- and four-year college enrollment. For two-year college enrollment, the patterns are identical to those for test scores. Schools that have below median impacts tend to have negative impacts on earnings and employment, but schools with above median impacts are again, only marginally significant at conventional levels. In stark contrast, four-year college enrollment effects are predictive of earnings effects throughout the distribution. For schools with below median value-added in four-year college enrollment, a ten percentage point increase in a school's four-year college enrollment effect is associated with a $2,998.90 (se=399.15) 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 a significant $982.40 (se=226.45) increase in the earnings effect.

Similar to four-year college enrollment, high school graduation exhibits an economically and statistically significant correlation with earnings both above and below median. Below median, a ten percentage point increase in a school's high school graduation effect is associated with a $1,629.81 (se=482.96) increase in the earnings effect. Above median, a ten percentage point increase in a school's graduation effect is associated with a $2,647.80 (se=564.09) increase in the earnings effect.[21] 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.[22]

One potential source of the heterogeneity in school effects is the quality of the counterfactual schools that students would attend if not enrolled in a charter school (Neal 1997, Chabrier et al. 2016). Holding charter school quality constant, a low quality counterfactual school will increase the estimated effect of a charter school. Following Chabrier et al. (2016), we explore heterogeneity

[21]The empirical Bayes adjustment shrink estimates uniformly towards the average school effect, and could theoretically mask nonlinearities in the relationship between effects on labor market and effects on educational outcomes. In Appendix Figures 1-2, we present results using unadjusted effects. Our results are robust to using unadjusted estimates, and the scatterplots do not suggest nonlinear relationships.

[22]In results available upon request, we conduct a series of permutation tests to test the statistical significance of the mean No Excuses and regular charter effects. Specifically, we regress school x cohort level effects (adjusted using our empirical Bayes procedure) on indicators for the No Excuses and regular charter labels that are randomly re-labeled 10,000 times. This procedure gives us a simulated distribution of No Excuses and regular charter effects. The results from the permutation test are consistent with our main results. The mean effect for regular charters is statistically significant for test scores, two- and four-year college enrollment, and average earnings, but not high school graduation and employment. The mean No Excuses charter effect in our sample is statistically significant for pooled scores, reading scores, and four-year college enrollment, but not math scores, high school college graduation, two-year college enrollment and labor market outcomes.

in counterfactual school quality by plotting our school level effects against counterfactual school quality. We measure counterfactual school quality for each charter school cohort using a two-step calculation. First, we find the average math and reading test score for grades 5-11 of all non-charter students in a matched cell. We then find the average test score across all matched cells for a charter school cohort, using the number of charter students in each cell as weights.

Appendix Figure 7 presents our school-level effects versus these counterfactual school measures. We also present estimates of the correlation between our charter school effects and counterfactual quality at the school x cohort level. We find a negative correlation between the charter school effects and counterfactual school quality for all outcomes except two-year college enrollment. For example, we find that a 0.1 increase in counterfactual school quality measure is associated with a \$65.37 (se=40.14) lower school earnings effect. Appendix Figure 7 also shows that most No Excuses effects are clustered in the left tail of the distribution of counterfactual quality, together with most of the regular charter effects. These results suggest that the larger No Excuses effects are not due to worse quality of the fallback options relative to regular charters.

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, though we are underpowered to detect modest but meaningful effects. Third, regular charters increase two-year college enrollment but decrease test scores, four-year college enrollment, and earnings. Fourth, the impact of charter schools on employment in Texas is small and statistically insignificant throughout. Fifth, at the school level, charter schools with below median test scores or two-year college enrollment effects tend to decrease earnings and employment, while charter schools with above median test scores or two-year college enrollment demonstrate no measurable earnings or employment benefits. Sixth, there is a robust positive correlation of school-level high school graduation and four-year college enrollment 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 instance, it is plausible – though not testable in our data – that charter schools do not increase networks, and networks are important for increasing test scores. 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. Many intuitive theories conflict with these results, allowing us to make at least some progress in identifying potential mechanisms.

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

A. Age of the Sample and Returns to Early Work Experience

One potential explanation for our results is that the individuals in our sample are too young for us to accurately measure their earnings, particularly if non-charter students accumulated significant work experience while charter students were in college. It is possible that students who attended charter schools with high test score value-added will eventually earn more, but we observe them 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 earnings trajectories of students in our sample. Figure 3 plots average earnings by educational attainment for students in our estimation sample. 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, earnings trajectories are relatively stable over all ages for individuals with some college, only a high school diploma, or less than a high school diploma. Average earnings for college educated students also exceed the average earnings of other students by age 23, providing some assurance that our sample is not too young. In results available upon request, we repeat this analysis by school type, finding nearly identical results.

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 to be 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 25-27, 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 27 earnings with age 30 earnings is 0.742 if zeros are included and 0.691 if zeros are not included (see Appendix Table 15). These results are again consistent with our main earnings measure accurately measuring labor market outcomes.

Appendix Table 16 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 3-6 present results for our full sample using predicted earnings at ages 28-30. In columns 3-4, we calculate predicted earnings using indicators for high school graduation, two-year college enrollment, and four-year college enrollment; cubic polynomials in grades 5-11 math and reading scores, years of two-year college, and years of four-year college; and the baseline controls used in all other specifications. Columns 5-6 add cubic polynomials on earnings from ages 25-27, and on median industry earnings from ages 25-27 to the prediction. If anything, the results are 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 -\$461.46 (se=109.48). The coefficient on No Excuses attendance is positive but measured with considerable noise at \$246.82 (se=171.61). Appendix Figure 8 presents results separately for each year relative to high school graduation. Consistent with the results from Appendix Table 16, earnings and employment effects are constant for No Excuses charter schools from years 5 to 10. The effect of regular charters on earnings 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. Appendix Table 17 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, none are both positive and statistically significant. If anything, charter students seem to be working 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 18. 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 charter schools have better results than those who did not complete the charter program through the last grade offered. Students who complete No Excuses charter schools also have considerably higher earnings results than those who did not complete. In contrast, there are no significant differences in the earnings or employment effects for regular charter students.

C. Multitasking

It is also plausible that the positive value-added charter schools improve test scores by, either intentionally or unintentionally, substituting away from other non-tested skills that have value in the labor market (e.g. creativity or adaptation to language). 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. Principal interviews 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 19 investigates differences across a wide set of variables that might be consistent with multitasking using the NYC charter data from Dobbie and Fryer (2013). At the mean, charters that increase test scores spend a statistically insignificant 4.5 percent more time on math and reading relative to charters that decrease test scores. Moreover, they spend a statistically insignificant 6.3 percent more time on non-tested subjects with a p-value of 0.413. These data do not seem consistent with multitasking. There are, however, some differences between achievement-increasing and achievement-decreasing charter schools in New York City that may be applicable to our earnings results for charters in Texas. For instance, consistent with Jacob (2005) achievement-increasing charter schools spend significantly less time on foreign languages and history.

Whether these input changes are important for labor market earnings is unknown, but these results 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 math and reading achievement do so at the expense of other 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 in Texas. No Excuses charter schools increase test scores and four-year college enrollment, but have a small and statistically insignificant impact on earnings – about what we would expect given the size of the test score effects – while regular charter schools decrease test scores, four-year college enrollment, and earnings. Using school-level estimates, we find that charter schools with below median value-added in test scores also tend to decrease earnings, while charter schools with above median value-added in test scores have no discernible impact on earnings. In contrast, high school graduation effects and four-year college enrollment effects are predictive of earnings effects for both low and high value-added schools.

The underlying mechanism that drives these results remains unclear. We test four hypotheses. Students in our main specifications are in their mid-twenties, but investigating older cohorts of students only strengthens the results. The 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 increase math and reading scores at the expensive of subjects such as art and history, although the exact effects of this practice on labor market outcomes is unknown.

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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	2305979	2292567	2145143	1993841	1880024	1880024	376208
No Excuses Charters	3300	3122	2826	2779	2569	2550	2550
Regular Charters	12324	10738	10260	9685	9083	8537	8537

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 2008-2009. 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	0.489	0.528	0.505	0.515	0.532	0.516
Black	0.140	0.165	0.374	0.234	0.167	0.363
Hispanic	0.375	0.597	0.334	0.325	0.573	0.339
Asian	0.023	0.060	0.012	0.007	0.065	0.012
Free Lunch	0.524	0.655	0.636	0.506	0.637	0.650
4th On Time	0.826	0.864	0.806	0.847	0.876	0.821
4th Grade Spec. Ed	0.137	0.060	0.140	0.095	0.060	0.119
4th Grade Gifted	0.092	0.131	0.065	0.106	0.145	0.074
4th Grade LEP	0.143	0.320	0.136	0.126	0.288	0.135
4th Grade At Risk	0.405	0.496	0.457	0.386	0.477	0.482
4th Grade Math	0.014	0.111	-0.498	-0.006	0.139	-0.383
4th Grade Reading	0.011	0.112	-0.352	0.021	0.141	-0.277
Missing 4th Math	0.209	0.286	0.259	0.162	0.266	0.227
Missing 4th Reading	0.219	0.295	0.272	0.172	0.278	0.239
Treatment						
Years Any Charter	0.000	3.371	2.091	0.000	3.404	1.993
Years No Excuses	0.000	3.291	0.000	0.000	3.325	0.000
Years Regular Charters	0.000	0.080	2.091	0.000	0.079	1.993
Outcomes						
5th-11th Grade Math	-0.077	0.213	-0.534	-0.086	0.214	-0.517
5th-11th Grade Reading	-0.075	0.171	-0.399	-0.058	0.183	-0.384
High School Graduation	0.716	0.779	0.638	0.762	0.836	0.666
Any Two-Year College	0.315	0.279	0.277	0.327	0.320	0.305
Years Two-Year College	0.909	0.822	0.806	0.941	0.941	0.901
Any Four-Year College	0.236	0.293	0.150	0.283	0.338	0.160
Years Four-Year College	0.897	1.040	0.494	1.064	1.199	0.526
Avg. Earnings (25-27)	16912.67	15937.90	12964.50	18856.76	18220.97	14248.3
Avg. Employment (25-27)	0.611	0.585	0.613	0.681	0.646	0.648
N Schools	9983	8	49	7290	8	49
N Students	2305979	3300	12324	376208	2550	8537

Notes: This table reports descriptive statistics for Texas public school students in our data graduating high school by 2008-2009. 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	0.018***	0.019***	0.028***	0.028***	0.023***	0.023***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Panel B: By Charter Type						
No Excuses	0.103***	0.105***	0.080***	0.081***	0.092***	0.093***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Regular Charter	−0.052***	−0.052***	−0.016***	−0.016***	−0.034***	−0.034***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	No	Yes	No	Yes
N Students x Years	2076898	2076898	2077867	2077867	4154765	4154765
Dep. Variable Mean	-0.017	-0.017	0.011	0.011	-0.003	-0.003

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 replace 4th grade school x cohort effects with 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	0.014***	0.014***	0.008***	0.008***	0.007***	0.007***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Panel B: By Charter Type						
No Excuses	0.022***	0.023***	−0.003	−0.003	0.027***	0.028***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Regular Charter	0.007***	0.007***	0.018***	0.018***	−0.010***	−0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Cell FE	No	Yes	No	Yes	No	Yes
N Students	387295	387295	387295	387295	387295	387295
Dep. Variable Mean	0.761	0.761	0.326	0.326	0.281	0.281

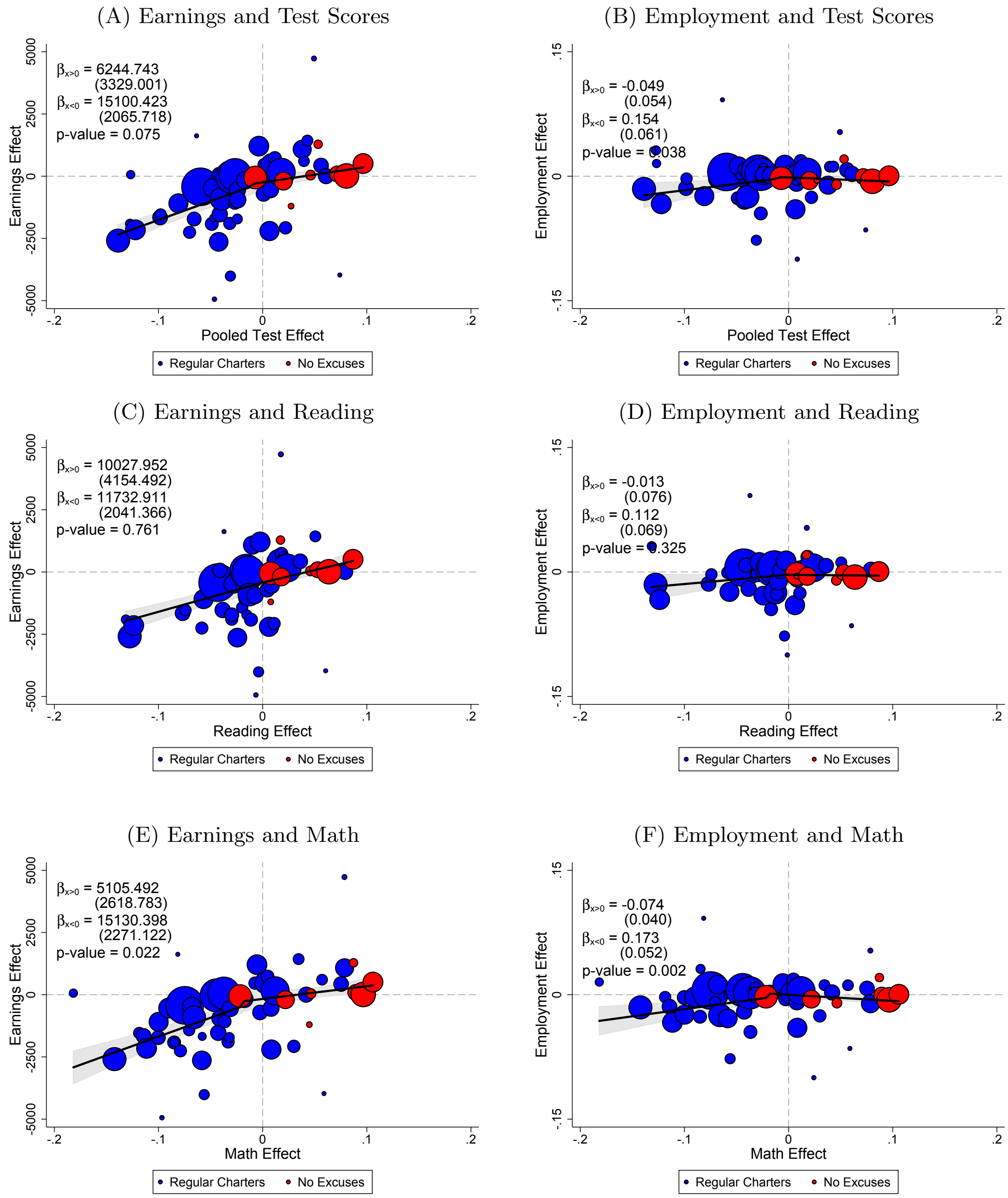
Notes: This table reports OLS estimates of the effect of charter attendance on academic attainment. We report the coefficient and standard error on the number of years spent at the indicated charter school type. Odd columns control for the number of years spent at charter schools not in our main sample, 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 replace 4th grade school x cohort effects with 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 25-27

	Average Earnings					Earnings > 0	
Panel A: Pooled Results	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any Charter	−139.270**	−143.547**	−177.068**	−130.102**	−91.004	−0.002	−0.002
	(63.711)	(64.496)	(76.853)	(55.216)	(55.811)	(0.001)	(0.001)
Panel B: By Charter Type							
No Excuses	136.865	129.270	259.843*	190.293**	244.267***	−0.002	−0.002
	(101.481)	(102.230)	(119.415)	(83.787)	(84.940)	(0.002)	(0.002)
Regular Charter	−368.746***	−369.513***	−530.773***	−395.476***	−368.700***	−0.002	−0.001
	(80.885)	(82.144)	(95.849)	(70.162)	(70.832)	(0.002)	(0.002)
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	387295	387295	284723	387295	387295	387295	387295
Dep. Variable Mean	18750.99	18750.99	25506.07	23991.66	24053.54	0.680	0.680

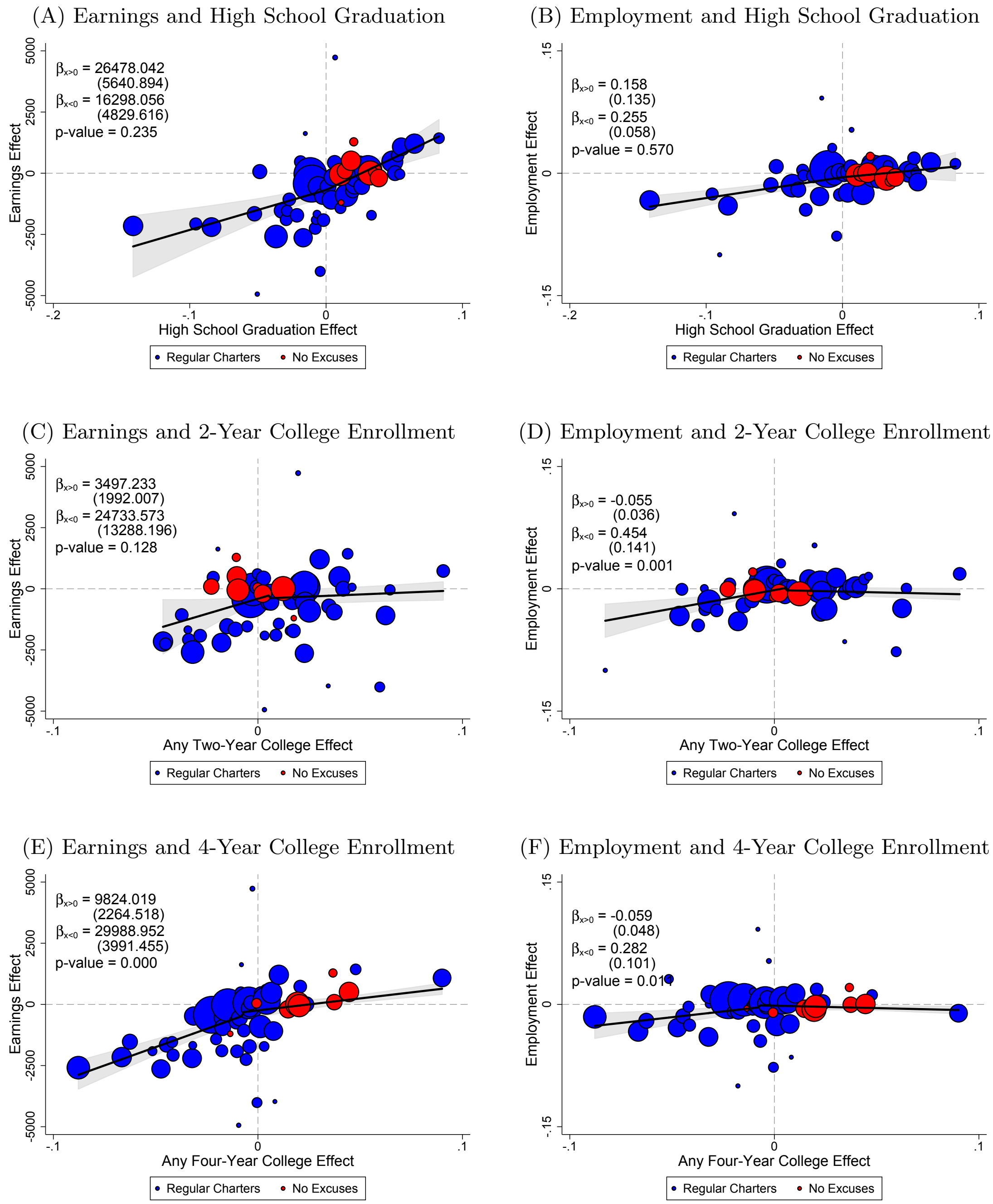
Notes: This table reports OLS estimates of the effect of charter attendance on earnings nine years after high school graduation. We report the coefficient and standard error on the number of years spent at the indicated charter school type. All columns control for the number of years spent at charter 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 replace 4th grade school x cohort effects with 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



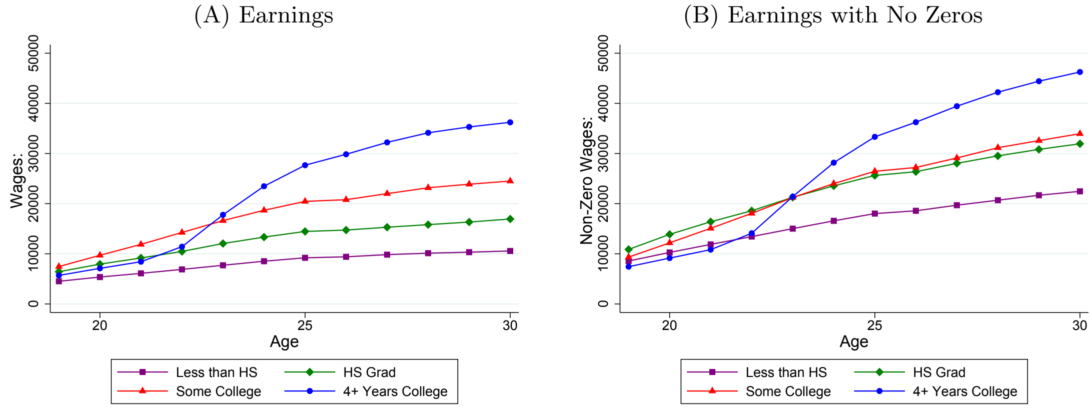
Notes: These figures plot the correlation between school-level labor market effects and school-level empirical Bayes adjusted test score effects. We allow the correlation between effects to vary below and above the median school-level test score effect. Observations are weighted by the number of students at each school in the earnings estimation sample. The solid line is estimated at the school x cohort level, with standard errors clustered at the school level. See Table 2 notes for details on the sample and variable construction and Online Appendix D 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 school-level empirical Bayes adjusted academic attainment effects. We allow the correlation between effects to vary below and above the median school-level attainment effect. Observations are weighted by the number of students at each school in the earnings estimation sample. The solid line is estimated at the school x cohort level, with standard errors clustered at the school level. See Online Appendix B for details on the sample and variable construction and Online Appendix D for details on estimation of the school effects.

Figure 3: Earning Trajectories by Educational Attainment



Notes: These figures plot average earnings with and without zero earnings for students in our estimation sample by educational attainment level, pooling across charter types. See Online Appendix B for details on the sample and variable construction.