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Moving On Up? A Virtual School, Student Mobility, and Achievement

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Virtual charter schools provide full-time, tuition-free K-12 education through internet-based instruction. Although virtual schools offer a personalized learning experience, most research suggests these schools are negatively associated with achievement. Few studies account for differential rates of student mobility, which may produce biased estimates if mobility is jointly associated with virtual school enrollment and subsequent test scores. We evaluate the effects of a single, large, anonymous virtual charter school on student achievement using a hybrid of exact and nearest-neighbor propensity score matching. Relative to their matched peers, we estimate that virtual students produce marginally worse ELA scores and significantly worse math scores after one year. When controlling for student mobility during the outcome year, estimates of virtual schooling are slightly less negative. These findings may be more reliable indicators of the independent effect of virtual schooling if matching on mobility proxies for otherwise unobservable negative selection factors.

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Abstract

Virtual charter schools provide full-time, tuition-free K-12 education through internet-based instruction. Although virtual schools offer a personalized learning experience, most research suggests these schools are negatively associated with achievement. Few studies account for differential rates of student mobility, which may produce biased estimates if mobility is jointly associated with virtual school enrollment and subsequent test scores. We evaluate the effects of a single, large, anonymous virtual charter school on student achievement using a hybrid of exact and nearest-neighbor propensity score matching. Relative to their matched peers, we estimate that virtual students produce marginally worse ELA scores and significantly worse math scores after one year. When controlling for student mobility during the outcome year, estimates of virtual schooling are slightly less negative. These findings may be more reliable indicators of the independent effect of virtual schooling if matching on mobility proxies for otherwise unobservable negative selection factors.

Keywords: virtual schools, charter schools, mobility, matching

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Introduction

Covid-19 entirely disrupted brick-and-mortar education for most students in the United States. As traditional public schools shifted toward virtual instruction, many commentators concluded that virtual education was an ineffective substitute for in-person instruction.[1] It is too early to know the extent of learning loss during the pandemic, but it seems likely that some students indeed fell behind. By and large, virtual schooling in 2020 was delivered by public school districts with little experience in online education. Not only did teachers and administrators contend with a new instructional mechanism, but they also had to grapple with a deadly pandemic. Accordingly, conclusions about the efficacy of virtual instruction during Covid-19 may not be generalizable to virtual schooling as we knew it prior to 2020—or as we will know it once the pandemic has been contained.

Having said that, much of the existing literature on virtual instruction offered by full-time online education professionals still suggests a negative association with student achievement. Much of this research has been conducted with quasi-experimental designs, since random assignment is often unfeasible in the context of virtual schooling. Accordingly, it is unclear whether prior studies have adequately controlled for differences between virtual and comparison students. Our evaluation of a single, large, anonymous virtual charter school leverages data on student mobility, at least in the outcome year of evaluation. We provide evidence that failing to account for school transfers may modestly bias the effects of virtual schooling in a negative direction.

[1] See Gould, E. (2020). "Remote Learning Is a Bad Joke." *The Atlantic*. Retrieved from https://www.theatlantic.com/ideas/archive/2020/08/kindergartener-virtual-education/615316/; Natanson, H. & Meckler, L. (2020). "Remote school is leaving children sad and angry." *The Washington Post*. Retrieved from: https://www.washingtonpost.com/education/2020/11/27/remote-learning-emotional-toll/?arc404=true; and Hobbs, T.D. & Hawkins, L. (2020). "The results are in for remote learning: It didn't work." The Wall Street Journal. Retrieved from: https://www.wsj.com/articles/schools-coronavirus-remote-learning-lockdown-tech-11591375078

Background on Virtual Schooling

Virtual schools, also referred to as cyber schools or online schools, provide full-time, tuition-free K-12 education to nearly 300,000 students in 35 states (Molner et al., 2019). Although virtual students usually receive education from their home residence, virtual schooling is distinct from homeschooling. The defining feature of virtual schooling is the web-based delivery mechanism. Virtual schools often provide computers and software, and students are connected to instructors through the internet, video-conference, and email. Lessons may be synchronous or asynchronous, depending on the school and course, allowing students to interact with teachers and peers in real-time. As of 2018, one-fifth of virtual students were enrolled in district-managed virtual schools (Molner et al., 2019). One of the first district-managed virtual schools was established by Florida's legislature in 1997.[2]

Virtual charter schools, one form of virtual schooling, are state-funded, state-regulated public schools that educate students through internet-based communication. As of January 2020, virtual charter schools were authorized in 27 states.[3] Virtual charters are required to administer annual state assessments like brick-and-mortar charter schools.

Pazhouh and colleagues (2015) underscore the heterogeneous nature of virtual schools in lesson delivery. Students are required to be online at different times, experience a mixture of online and in-person discussions, and have varying amounts of teacher interaction. Gill and colleagues (2015) document that Black students and students with disabilities enroll in virtual schools at similar levels to those found in other public schools, although Hispanic students and non-native English speakers are somewhat underrepresented. Beck and colleagues (2014) present

[2] Florida Virtual School. "About Florida Virtual School." Retrieved from: https://www.flvs.net/about/flvs
[3] Education Commission of the States. (2020). "Charter Schools: Does state law explicitly allow virtual charter schools?" Retrieved from: https://www.crpe.org/sites/default/files/crpe-policy-framework-online-charter-schools-final_0.pdf

evidence that virtual schools serve disproportionately high numbers of students with emotional or academic problems relative to traditional public schools.

Advocates argue that virtual schools reimagine education through an innovative approach, personalized to the unique needs of each student. Virtual schooling has potential to offer instruction for children in remote areas who lack access to certain coursework (Heppen et al., 2011). In addition, virtual schools can hire teachers from anywhere in each state, accommodate teachers who prefer non-traditional work schedules, and employ high teacher-student ratios without sacrificing personalized attention for students (Pazhouh et al., 2015). Virtual schools are also attractive for students with rare illnesses or unusual travel schedules.

Parents may pursue non-traditional educational models for various reasons, including academic quality, civic or religious values, concerns about safety, curriculum, class size, extracurriculars, school facilities, and longer school hours, among others. School choice theory suggests student achievement will improve when parents exercise increased autonomy over the education of their children (Chubb & Moe, 1990).

Nonetheless, virtual charter schools are controversial. Critics point to virtual schools' poor track record of increasing achievement relative to traditional public schools. Researchers question whether "the poor performance of virtual charter schools reflects a disadvantage inherent to online instruction, the unique dysfunction of this particular sector of schools, or some combination of the two factors."[4] In several states, a combination of unions,[5] school board

[4] Fitzpatrick, B. R., Berends, M., Ferrare, J.J., & Waddington, J. (2020). "Virtual charter schools and online learning during COVID-19." Retrieved from Brookings Institution: https://www.brookings.edu/blog/brown-center-chalkboard/2020/06/02/virtual-charter-schools-and-online-learning-during-covid-19/
[5] "Oregon's Coronavirus education lockdown." (2020). Retrieved from *Wall Street Journal*: https://www.wsj.com/articles/oregons-coronavirus-education-lockdown-11585697080

organizations,[6] and district leaders[7] have called for virtual school moratoriums, enrollment limits, and spending cuts. But it is not just opponents of school choice who oppose virtual schooling. In 2016, three national organizations that support brick-and-mortar charter schools issued a report highlighting the need to improve the quality of virtual charter schools.[8]

Virtual Schooling Literature Suggests Negative Association with Achievement

There have been few rigorous evaluations comparing outcomes of virtual students to their traditional brick-and-mortar public school peers (Barbour & Reeves, 2009; U.S. Department of Education, 2010). Even fewer studies account for the type of students who select into virtual schooling. Virtual students may be, on average, negatively selected based on prior test scores, disability status, and poor experience in a previous school. A virtual student may also be positively selected if he or she is unchallenged academically in traditional schools.

Existing evaluations conclude that virtual schooling has significant and large negative effects on student achievement, especially in math. None of the studies of virtual schooling have been experimental, since online instruction can be provided to most students who desire it. It is not clear if the quasi-experimental studies to date have adequately controlled for observable and unobservable factors that might bias estimates of virtual schooling effects.

A research team with the Center for Research on Education Outcomes at Stanford University systematically analyzed the effects of virtual charter schools on student achievement

[6] "PSBA supports governor taking steps to address charter funding issues." (2019). Retrieved from Pennsylvania School Boards Association: https://www.psba.org/2019/08/psba-supports-governor-taking-steps-to-address-charter-funding-issues/

[7] Mezzacappa, D. (2020). "District leaders call for moratorium on new charters until law is changed." Retrieved from *Philadelphia Public School Notebook*: https://thenotebook.org/articles/2020/01/27/district-leaders-call-for-moratorium-on-new-charters-until-law-is-changed/

[8] "A call to action to improve the quality of full-time virtual charter public schools." (2016). Retrieved from National Association of Public Charter Schools: https://www.publiccharters.org/sites/default/files/migrated/wp-content/uploads/2016/06/Virtuals-FINAL-06202016-1.pdf

in multiple states (Woodworth et al., 2015). Researchers collaborated with 18 state departments of education to obtain data on virtual charter and traditional public school students. For each observed virtual charter student, a match was generated by drawing on the records of public school students with identical traits and similar prior achievement. Although the researchers provided descriptive statistics on student mobility, they did not include mobility as a control variable in their statistical models. The study found large negative results for virtual charter students compared to peers in traditional public schools. Virtual charter students produced 25 percent of a standard deviation lower gains in math and 10 percent of a standard deviation lower gains in English language arts (ELA).

Other quasi-experimental research also found negative associations between virtual charter attendance and student achievement. For example, Fitzpatrick and colleagues (2020) used longitudinal student records in Indiana to compare students who switch from traditional public schools to virtual charters against matched peers with similar observable characteristics. The authors found that virtual students perform one-third of a standard deviation worse in ELA and one-half of a standard deviation worse in math compared to their peers. Ahn and McEachin (2017) found virtual students in Ohio perform 40 percent of a standard deviation worse than comparable students in math and 20 percent of a standard deviation worse in ELA. Additionally, Bueno (2020) found that virtual students in Georgia demonstrated achievement losses ranging from 10 percent to 40 percent of a standard deviation in multiple subjects. An evaluation of a virtual charter school in a southern state, using a matching design, found that participating students experienced statistically significant negative achievement effects in ELA and math during their first three years of virtual schooling (Lueken et al., 2015).

Our interest is in public charter schools that deliver instruction through a virtual modality. The intervention is a composite of charter schooling and virtual schooling. A separate research literature exists on virtual schooling delivered by traditional public schools (e.g., Evergreen Education Group, 2017; Schwerdt & Chingos, 2015). As has been made clear during the Covid-19 crisis, charter schools and traditional public schools deliver virtual schooling differently, whether under planned or unplanned circumstances (Kingsbury, 2020; Vanourek, 2020). Students also vary in the extent to which they use computer technology at home to complete schoolwork, with some studies indicating more home computer use is associated with lower student achievement (Agasisti, Gil-Izquierdo & Han, 2020; Fairlie, Beltran & Das, 2010). The general research literature on education technology is vast and variegated. Thus, we focus on the narrower research literature specifically on virtual charter schooling.

Student Mobility: A Predictor of Low Achievement

A student may be considered mobile if he or she transfers from one school to another for reasons other than a grade promotion. In our study, student mobility is measured by counting the number of transfers that occur during a school year. School transfers during the academic year are likely to harm student learning since they interrupt the sequencing of concepts and skills. Mobility, also referred to as churn or transience, can occur for many reasons. Families may seek academic programming at a different school, a parent may assume a new job in a new location, or the student may suffer from bullying or dissatisfaction in the previous school (Sparks, 2016).

A meta-analysis by Mehana and Reynolds (2004) found student mobility was a strong, negative predictor of achievement. Similarly, Hanushek & colleagues (2004) estimated that student learning losses, particularly those experienced in the first year, had spillover effects on other students. Kerbow (1996) found that high rates of student mobility had large negative implications for schools in addition to the students themselves. South and colleagues (2007) used the National Longitudinal Study of Adolescent Health to estimate that mobile students were twice as likely as non-mobile peers to drop out of school, although mobile students differed on observable variables that may have influenced both mobility and persistence.

Virtual students are highly mobile. Gill and colleagues (2015) found that the average student in a virtual school enrolled for only two years. As a result, some researchers studying virtual schools argue that student mobility should be included in statistical models that compare achievement of virtual students to traditional public school students (Gatti, 2018).

Data

We study a full-time, accredited virtual charter school. Any K-12 student residing within the anonymous state is eligible to attend the school, tuition-free. Students receive synchronous and non-synchronous instruction, complete state assessments, and are eligible to participate in extracurricular activities with their resident school district. The school employs full-time, licensed counselors to assist with personal development as well as college and career planning.

We use statewide student-level achievement and demographic data from the 2014-15 through 2017-18 school years. The outcomes of interest are ELA and math scores, standardized by year and grade. The data include indicators for gender and race, as well as free- or reduced-price lunch (FRPL) eligibility, English as a Second Language (ESL) status, and special education (SPED) status.

Treatment is defined as being enrolled in the virtual school of interest at any time during the outcome year. The comparison condition is defined as not being enrolled in the virtual school of interest during the outcome year. We observe if a student transferred to a different school during the school year but not specifically when the transfer occurred. Thus, we code any student exposed to the virtual charter school during the outcome year as "treated."

This school educates a non-trivial percentage of the nation's virtual sector. Its large enrollment permits a well-powered analysis and increases the salience of our research. Our data include 12,498 unique students in the virtual school and 1,061,165 other public school students with whom the virtual students may be compared. We limit the sample to grades 3 through 8, reducing the analytic sample to 6,054 virtual students and 574,633 potential comparison students since these are the grades assessed by the state. Then, because students must have baseline and outcome test scores to be included in our analysis, the sample is further reduced to slightly more than 3,500 virtual students and slightly less than 400,000 comparison students (Table 1). Student counts in ELA are slightly different from student counts in math because of variation in missing test score data, explained in more detail below.

Table 1: Unique Students Who Meet Criteria for Inclusion in Analytic Samples

	ELA		Math	
	Virtual	Comparison	Virtual	Comparison
Total Unique Students	12,498	1,061,165	12,498	1,061,165
% of total unique students	100%	100%	100%	100%
In Grades 3-8	6,054	574,633	6,054	574,633
% of total unique students	48%	54%	48%	54%
With baseline test scores	3,888	406,177	3,890	407,106
% of total unique students	31%	38%	31%	38%
With outcome test scores	3,505	393,632	3,509	393,824
% of total unique students	28%	37%	28%	37%

To quantify mobility, we create a variable identifying each student's number of mid-year school switches during the outcome year. For example, a student who remains in the same school for an entire school year is coded as "0" mid-year switches, while a student who changes schools twice during one school year is coded with "2." Less disruptive transfers between school years—over the summer, for example—are not captured by this variable.

Table 2 presents baseline descriptive statistics for our analytic sample. Students in the virtual school have statistically significant and practically meaningful observable differences from other public school students. Virtual students demonstrate higher lagged ELA scores and lower lagged math scores than their peers. Unsurprisingly, the average virtual student has more mid-year school transfers than his or her peers. Virtual students are substantially more likely to be white and less likely to be Black. Students in the virtual school are less likely to be identified with ESL, SPED, or FRPL status.

Since the virtual school does not operate a traditional school lunch program, it is possible some low-income families did not complete the necessary paperwork for their child to be deemed eligible for the program. However, FRPL eligibility can be the basis for other benefits, such as receiving free education technology. It is therefore unclear whether parents of students in the virtual school are less likely to submit FRPL paperwork. Absent clear evidence to the contrary, we assume that FRPL rates are not systematically biased across the treatment and comparison conditions, though we acknowledge such bias is possible.

Table 2: Descriptive Baseline Statistics (2015-16)

	Virtual	Comparison	Difference	
Lagged ELA (normed)	0.09	0.00	0.09	***
Lagged Math (normed)	-0.14	0.00	-0.14	***
Mid-year School Switches	0.35	0.06	0.29	***
Female	0.52	0.49	0.03	**
FRPL	0.57	0.60	-0.03	**
ESL	0.01	0.05	-0.04	***
Special Education	0.12	0.12	0.00	
White	0.74	0.52	0.22	***
Black	0.12	0.34	-0.22	***
Hispanic	0.06	0.08	-0.02	***
Asian	0.01	0.02	-0.01	
	n= 1,532	n= 267,658		

Notes: The number of virtual students, 1,532, is less than the number of virtual students in Table 3A and 3B because Table 3A and 3B include virtual students from multiple outcome years, not just 2015-16. The 2015-16 school year is the first year for which outcome data can be observed, because 2014-15 serves as the baseline measure of the outcome variable.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Missing Data

In administrative records with over one million students spanning multiple years, missing data are to be expected. Fortunately, missingness is relatively rare among both dependent and independent variables. Less than five percent of ELA or math scores in the entire sample are missing. Conditional on observable characteristics, virtual students are six percentage points more likely than potential comparison group students to report missing achievement data, in both subjects. Given the overall low rate of missingness, this differential attrition is consistent with the quality standards of the What Works Clearinghouse.[9]

Less than one half of one percent of the values of independent variables such as race, gender, FRPL, and ESL indicators are missing. We used multiple imputation by chained equations to address these missing cells within our matrix of independent variables. The results we present include imputed values for the independent variables. All points estimates are robust to the sample without imputed data, which is plausible given the overall low rates of missingness.

Empirical Strategy

The ideal research design to evaluate the virtual school would be an experiment with random assignment. Unlike brick-and-mortar charter schools constrained by limits on physical space, virtual schools typically can accommodate all students who seek to enroll, so random lotteries are less likely to occur. The anonymous school in our analysis does not utilize lotteries to determine enrollment.

Given that random assignment is not feasible, a credible analysis of student achievement hinges on the strategy for reducing selection bias. As is evident from the descriptive statistics in Table 2, the average virtual student differs from students in other public schools. Accordingly, it is likely virtual students and their peers differ on unobservable characteristics influencing both selection into virtual schooling and future achievement.

To reduce selection bias, we use a hybrid of exact and nearest-neighbor propensity score matching to match students by grade-year cohort, prior achievement, and other demographic characteristics (Rosenbaum & Rubin, 1983). These variables control for observable factors that

[9] U.S. Department of Education, Institute for Education Sciences, What Works Clearinghouse, Standards Handbook, Version 4.1, pg. 12.

might otherwise confound estimation of the effects of the virtual school and serve as proxies for unobservable differences between the average treated and comparison student. Our matching protocol generates a comparison group equal to the number of virtual students in the analytic sample. One-to-one matching produces an analytic sample sufficiently sized to generate a well-powered analysis. Matching rarely eliminates omitted variables bias, but it can reduce such bias to trivial levels (Bifulco, 2012).

Our analysis includes four components. The first evaluates one-year outcomes of virtual students relative to a matched comparison group in other public schools. In this specification, we do not control for student mobility. Second, we explore heterogeneous one-year effects based on gender, race, FRPL status, and school level. Third, to investigate the role of student mobility, we include a mobility variable in our matching protocol and compare these estimates to those generated from the specification that did not match on mobility. We hypothesize that failing to account for student mobility will downwardly bias the estimate of virtual charter schooling. Our assumption, grounded in theory and research on school mobility, is that students with greater numbers of school transfers will be negatively selective, on average, compared to other students. Fourth, we analyze outcomes from a restricted sample of students who do not transfer schools in the outcome year.

Generating Comparison Group through Matching

We use logit models to predict the likelihood that a student enters the virtual charter school:

$$Pr\ (D_{it}) = \delta_0 + \delta_1 A_{\text{it-1}} + \delta_2 M_{\text{it}} + \boldsymbol{X}_{\text{it}}\delta_3 + \varepsilon_{\text{it}}$$

Our models compare students within cohort cells, ensuring that they are matched to peers in the same grade and year. The probability of attending the virtual school (D) is a function of prior year test scores in ELA and math (A_{it-1}), and student-level covariates ($\boldsymbol{X}$) including gender, race, FRPL status, ESL status, and SPED status. We exact-match on a band of baseline proficiency, gender, and race to limit observable differences on these crucial factors. When exploring the role of student mobility, we exact-match on an ordinal variable (*M*) indicating the number of mid-year school switches for each student.[10]

To limit differences in baseline achievement, virtual students are matched with non-virtual students who scored within a 5-percentage point band on the prior year's state assessment in the subject of the relevant dependent variable. Students are within-band matched on lagged ELA scores and lagged math scores separately. Thus, different comparison groups are constructed for math and ELA, respectively. Separate comparison groups are necessary because there are not enough comparison students who share similar lagged test scores in both subjects. In our preferred specification, we control for prior year performance in both subjects.

After limiting the pool of potential comparison students to those with the same cohort, baseline proficiency, gender, and race, students are matched with their nearest neighbor based on propensity scores. We use a caliper of 0.10 and match without replacement.[11] The protocol generates one comparison student for each virtual student in the final analytic sample. For each student pair, the outcome year is the second year that a student appears in the data, with the first appearance in the data serving as the baseline year.

Once the comparison group is identified, we estimate the effects of the virtual school on student achievement in a multiple regression framework, using the model:

[10] Students with two or more mid-year transfers are matched to other students with two or more transfers, but the exact number of transfers may differ. In other words, a virtual student with three transfers may be matched to a student with two transfers.

[11] Ideally, we would identify a comparison student who is identical on observable characteristics for each virtual student in the analytic sample. In practice, there is a trade-off between match quality and the number of virtual students included in the analysis. Virtual students are dropped on the rare occasion when no suitable comparison students exist who meet the exact-matching and propensity score criteria. In the pages that follow, we report the percentage of virtual students who dropped from each matching protocol.

$$Achievement_{it} = \alpha + \beta_1 D_{it} + \beta_2 A_{it-1} + \boldsymbol{X}_{it}\beta_3 + \epsilon_{it}$$

Standardized test scores in ELA or math ($Achievement_{it}$) for student *i* in year *t* are a function of an indicator variable for virtual school treatment (*D*); prior year achievement in both subjects (A_{it-1}); and a vector (***X***) of student-level covariates, including gender, race, FRPL, ESL, and special education status; and an error term (ϵ). The coefficient of interest is β_1.

Results

One-Year Outcomes

Tables 3A and 3B present observable differences, after matching, between virtual students and their peers. The sample size is so large, at nearly 7,000 students, that even small differences between the student groups are statistically significant. However, many of the differences that existed between the groups in Table 2 are eliminated after matching.

Note that in the ELA outcome sample, virtual students have similar prior ELA scores but worse prior year math scores. We prioritize baseline equivalence in the subject of the relevant dependent variable. The observable differences in the math outcome sample (Table 3B) are similar to those found in the ELA outcome sample (Table 3A) with the exception of lagged test scores. In Table 3B, the groups are indistinguishable on prior year math performance, but the virtual students remain positively selected on baseline ELA scores.[12] In both analytic samples, virtual students are marginally more likely to have FRPL status and marginally less likely to have ESL status.

[12] Fewer than two percent of virtual students from the final analytic sample are dropped from this matching protocol.

Table 3A: Observable Differences After Matching (ELA Outcome)

	Virtual	Comparison	Difference	
Lagged ELA (normed)	0.08	0.08	0.00	
Lagged Math (normed)	-0.08	0.11	-0.19	***
Female	0.53	0.53	0.00	
FRPL	0.58	0.52	0.06	***
ESL	0.01	0.04	-0.03	***
Special Education	0.08	0.08	0.00	
White	0.73	0.73	0.00	
Black	0.15	0.15	0.00	
Hispanic	0.07	0.07	0.00	
Asian	0.01	0.01	0.00	
	n=3,467	n=3,467		

Notes: See appendix for student counts by grade and year.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3B: Observable Differences After Matching (Math Outcome)

	Virtual	Comparison	Difference	
Lagged ELA (normed)	0.07	-0.02	0.09	***
Lagged Math (normed)	-0.08	-0.08	0.00	
Female	0.53	0.53	0.00	
FRPL	0.58	0.55	0.04	***
ESL	0.01	0.04	-0.03	***
Special Education	0.09	0.09	0.00	
White	0.73	0.73	0.00	
Black	0.15	0.15	0.00	
Hispanic	0.07	0.07	0.00	
Asian	0.01	0.01	0.00	
	n=3,473	n=3,473		

Notes: See appendix for student counts by grade and year.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4 displays achievement differences among virtual and comparison students. In the preferred specification, we control for all observable characteristics, including prior year test scores. Virtual students perform 4 percent of a standard deviation worse in ELA and 21 percent of a standard deviation worse in math than their matched peers. These point estimates are robust to a simple specification that only includes an indicator variable for virtual charter schooling. We are particularly interested in whether these estimates differ from our mobility-matched estimate, which is presented later in Table 7.

Table 4: Virtual School, One-Year Outcomes

	ELA		Math	
	Simple	Preferred	Simple	Preferred
Virtual	-0.08***	-0.04***	-0.19***	-0.21***
	(0.02)	(0.01)	(0.02)	(0.01)
Lagged Math (normed)		0.21***		0.51***
		(0.01)		(0.01)
Lagged ELA (normed)		0.61***		0.27***
		(0.01)		(0.01)
Black		-0.05***		-0.11***
		(0.02)		(0.02)
Hispanic		0.04		-0.02
		(0.03)		(0.03)
Asian		0.10*		0.19***
		(0.06)		(0.07)
Mix		0.01		-0.05
		(0.03)		(0.04)
ESL		-0.18***		-0.06
		(0.04)		(0.04)
Female		0.12***		-0.08***
		(0.01)		(0.01)
Special Education		-0.25***		-0.15***
		(0.03)		(0.03)
FRPL		-0.11***		-0.11***
		(0.01)		(0.01)
n=	6,934	6,934	6,946	6,946

Notes: Heteroskedastic-robust standard errors in parenthesis.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Heterogeneous Effects

Heterogeneous effects of the virtual school are presented in Table 5. Negative results in math are consistent across gender, race, FRPL status, and school level. In ELA, however, we estimate statistically significant heterogeneous effects for females relative to males and for elementary schoolers (grades 3-5) relative to middle schoolers (grades 6-8). The estimate of virtual schooling on ELA for females is 6 percent of a standard deviation more negative than for males.

We speculate that females may be, on average, more social than males and less naturally inclined to thrive in the independent environment of online education. The estimate of virtual charter schooling on ELA for elementary schoolers is 7 percent of a standard deviation more negative than for middle schoolers. We speculate that middle school students are more comfortable using computers and maintaining concentration for long periods than elementary school students.

Table 5: Heterogeneous Effects

	ELA	Math
Female*Virtual	-0.06**	-0.04
	(0.03)	(0.03)
White*Virtual	-0.02	-0.03
	(0.03)	(0.03)
FRPL*Virtual	0.02	0.03
	(0.03)	(0.03)
Elementary*Virtual	-0.07**	-0.02
	(0.03)	(0.03)

Notes: Heteroskedastic-robust standard errors in parenthesis. Control variables include lagged test scores, race, gender, ESL, FRPL, and SPED status. Each point estimate comes from a separate regression.
*** p<0.01, ** p<0.05, * p<0.1

One-Year Outcomes, Controlling for Mobility

The third analysis uses the same matching protocol to generate a comparison group for the virtual students, with one exception: we include mobility in the matching process and as a control variable.[13] We match on school transfers that occur during the outcome year. Matching on pre-program mobility would be superior to our approach here, as some unknown

[13] These sample sizes are marginally smaller than those presented in Tables 3A and 3B because including mobility in the matching protocol causes more virtual students to be dropped from the analysis. Fewer than three percent of virtual students from the final analytic sample are dropped from this matching protocol.

component of outcome-year mobility likely is induced by the treatment of virtual schooling. Unfortunately, we cannot reliably determine prior-year student mobility from the data.

Tables 6A and 6B present observable differences between the samples of virtual students and matched peers. The groups are nearly identical on prior test scores in the subject of the dependent variable, gender, and race. Virtual students are less likely to be identified as FRPL and ESL and are marginally more likely to have SPED status. After matching, virtual and comparison students have similar mobility rates.

Table 6A: Observable Differences After Matching, With Mobility (ELA Outcome)

	Virtual	Comparison	Difference	
Lagged ELA (normed)	0.07	0.07	0.00	
Lagged Math (normed)	-0.09	0.01	-0.10	***
Mid-year School Switches	0.51	0.51	0.00	
Female	0.53	0.53	0.00	
FRPL	0.58	0.65	-0.08	***
ESL	0.01	0.04	-0.03	***
Special Education	0.09	0.08	0.01	*
White	0.73	0.73	0.00	
Black	0.15	0.15	0.00	
Hispanic	0.06	0.06	0.00	
Asian	0.01	0.01	0.00	
	n= 3,412	n= 3,412		

Notes: See appendix for student counts by grade and year.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6B: Observable Differences After Matching, With Mobility (Math Outcome)

	Virtual	Comparison	Difference	
Lag ELA (normed)	0.07	-0.06	0.13	***
Lag Math (normed)	-0.09	-0.09	0.00	
Mid-year School Switches	0.51	0.51	0.00	
Female	0.53	0.53	0.00	
FRPL	0.58	0.67	-0.08	***
ESL	0.01	0.04	-0.03	***
Special Education	0.08	0.08	0.00	
White	0.73	0.73	0.00	
Black	0.15	0.15	0.00	
Hispanic	0.06	0.06	0.00	
Asian	0.01	0.01	0.00	
	n= 3,390	n= 3,390		

Notes: See appendix for student counts by grade and year. The number of virtual students is smaller for math outcomes than ELA outcomes because there are more missing outcome data for math than ELA.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7 compares ELA and math achievement of virtual students after one year against their matched comparison group. Notably, the estimated effects on ELA are indistinguishable between the groups. In math, virtual students' achievement lags their matched peers by 18 percent of a standard deviation. These point estimates are robust to a simple specification that only includes an indicator variable for virtual charter schooling.

Table 7: Virtual School, One-Year Outcomes with Mobility

	ELA		Math	
	Simple	Preferred	Simple	Preferred
Virtual	-0.01	0.00	-0.14***	-0.18***
	(0.02)	(0.01)	(0.02)	(0.01)
Lagged Math		0.20***		0.50***
		(0.01)		(0.01)
Lagged ELA		0.59***		0.28***
		(0.01)		(0.01)
Mid-year Switches		-0.06***		-0.06***
		(0.01)		(0.01)
Black		-0.07***		-0.10***
		(0.02)		(0.02)
Hispanic		0.03		-0.01
		(0.03)		(0.03)
Asian		0.12**		0.18**
		(0.06)		(0.08)
ESL		-0.11**		-0.01
		(0.05)		(0.04)
Female		0.12***		-0.09***
		(0.01)		(0.01)
Special Education		-0.29***		-0.13***
		(0.03)		(0.03)
FRPL		-0.10***		-0.09***
		(0.02)		(0.02)
n=	6,824	6,824	6,780	6,780

Notes: Heteroskedastic-robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

The findings in Table 7, which account for outcome-year mobility, are 4 percentage points of a standard deviation less negative in ELA and 3 percentage points of a standard deviation less negative in math relative to the findings in Table 4, which do not account for mobility. The ELA estimate in Table 7 is statistically significantly different than the ELA estimate in Table 4 at the 99 percent confidence level. The math estimate in Table 8 is statistically significantly different than the math estimate in Table 4 at the 95 percent confidence level. Thus, failing to account for mobility may modestly attenuate the point estimate of virtual charter schooling.[14] While the bias appears downward, it is smaller in magnitude than we hypothesized. Still, these results may demonstrate the importance of controlling for differential rates of school mobility. Had we not accounted for this difference in our primary analysis, we would have falsely concluded that the virtual school had a significantly negative effect on ELA outcomes when the true effect may be null.

Limiting Sample to Non-Switchers

In the previous section, treated students are matched to comparison students who had the same number of school transfers during the outcome year. In both the ELA and math samples, roughly 48 percent of virtual students received instruction in both the virtual school and another public school during the outcome year, which could obscure the treatment effect. To address this concern, we conduct a separate analysis restricted to the 52 percent of students who did not change schools during the outcome year. These estimates, presented in Table 8, explore something akin to a dosage analysis of an entire year of virtual schooling.

[14] The point estimates in Table 4 and Table 7 are obtained from different samples. We conduct significance tests between the estimates by merging both samples, eliminating duplicate student observations, and running our preferred specification with separate indicator variables for comparison students in each of the two samples. The regression coefficients for each comparison group indicator have a variance and a covariance because they are estimated in the same equation, allowing us to test for statistical significance between the two estimates.

Table 8: Virtual School Outcomes, Students Without Switches

	ELA		Math	
	Simple	Preferred	Simple	Preferred
Virtual	-0.04	0.00	-0.16***	-0.21***
	(0.03)	(0.02)	(0.03)	(0.02)
Lagged Math		0.22***		0.52***
		(0.01)		(0.02)
Lagged ELA		0.58***		0.29***
		(0.01)		(0.02)
Black		-0.06**		-0.09***
		(0.03)		(0.03)
Hispanic		0.06		-0.03
		(0.04)		(0.04)
Asian		0.17***		0.18**
		(0.06)		(0.09)
ESL		-0.19***		-0.06
		(0.06)		(0.06)
Female		0.12***		-0.10***
		(0.02)		(0.02)
Special Education		-0.28***		-0.12***
		(0.03)		(0.03)
FRPL		-0.10***		-0.09***
		(0.02)		(0.02)
n=	3,534	3,534	3,506	3,506

Notes: See appendix for student counts by grade and year. Heteroskedastic-robust standard errors in parenthesis. Limiting the sample to students without switches in the outcome year produces differences in observable characteristics much like those presented in Tables 6A and 6B, with one exception: differences in lag test scores in the non-dependent variable subject are exacerbated. In the ELA outcome sample, virtual students are more negatively selected with respect to prior year Math test scores. In the Math outcome sample, virtual students are more positively selected with respect to prior year ELA scores.
*** p<0.01, ** p<0.05, * p<0.1

Results that exclude switchers are consistent with the findings from the previous section. In ELA, the virtual schooling estimate remains nearly zero. In math, the estimated negative effect of virtual schooling is 21 percent of a standard deviation, which is 3 percentage points of a standard deviation more negative in magnitude than the math estimate in Table 7.

Limitations

We acknowledge this analysis relies on non-random selection of students into the virtual school of interest. We think such selection bias, endemic to virtual schooling research, is mitigated by identifying comparison students with the same cohort, race, gender, and prior achievement as treated students. Although we cannot make causal claims about the impact of virtual charter schooling on achievement, we argue that controlling for student mobility in the outcome year represents an improvement on other quasi-experimental or observational studies of virtual schooling.

Our study is limited by the fact that we cannot reliably determine prior-year student mobility. Some component of our mobility measure might be a product of virtual schooling. We attempt to mitigate this obstacle by employing a sample restriction to students who do not change schools during the outcome year and find similar results employing both the restricted and unrestricted one-year samples.

Absent random assignment, which is rare in virtual schooling, finding an appropriate comparison group for virtual students remains a challenge. Two otherwise identical students who only differ in virtual school enrollment are likely to have meaningful unobservable differences. Moreover, we do not know why students in our study change schools. Different types of mobility likely have different impacts on achievement. For example, a transfer due to a parent getting promoted to a new job is not the same as a child transferring because of bullying. Future research should explore those mobility-related issues.

Conclusion

Our findings cannot be generalized to school years interrupted by a global pandemic, but we hope to shed light on the heterogeneous nature of virtual charter schooling as well as the role of student mobility. Although most existing research finds virtual schools have deeply negative achievement effects, our analysis of a single, large, anonymous virtual charter school suggests that different virtual schools vary in their influence on student learning growth. We estimate students in this virtual charter school demonstrate slightly less ELA growth than similar students in other public schools. Estimated math effects, while still quite negative, are less negative in magnitude than found in other research. We cannot say for sure why our achievement findings are less negative than those from most prior studies of virtual schooling. It is possible that this school implements an effective combination of personalized learning and rigorous curriculum relative to other virtual charter schools. It also may have more experience delivering virtual schooling than the virtual providers in prior studies.

The effects of this virtual charter school on student achievement are not monolithic. Students who are female or younger experience more negative virtual schooling effects on ELA than their virtually-schooled classmates who are male or older. Our heterogeneous effects suggest that virtual charter schooling may be somewhat more effective for boys and older students, at least regarding ELA growth.

We provide evidence that omitting controls for student mobility may downwardly bias the effects of virtual schooling on student achievement. Granted, part of the inherent promise of virtual schooling is the ability of such schools to capably transition students into the world of online education. Regardless of whether virtual schools are fulfilling this promise, however, measuring a highly-mobile virtual student against a less-mobile non-virtual student does not produce an apples-to-apples comparison. We suggest researchers account for mobility when studying virtual schools, especially using pre-program measures if possible.

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Appendix

The following tables identify the grade and outcome year for students included in each unique analytic sample. All grade by year cells include an equal number of virtual students and matched comparison peers in other public schools. Although students are only assessed in grades 3-8, and students must have a lagged test score to be included in the analysis, a third grade student can be included in the samples below if he or she is repeating third grade and has a baseline test score from the previous year.

ELA (No Mobility Control) Cohort, Table 3A

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	10	312	376	538	692	820	2,748
2017	24	342	230	352	420	476	1,844
2018	10	498	280	484	518	552	2,342
Total	44	1,152	886	1,374	1,630	1,848	6,934

Math (No Mobility Control) Cohort, Table 3B

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	10	314	376	538	692	820	2,750
2017	22	342	228	352	428	484	1,856
2018	12	500	284	484	510	550	2,340
Total	44	1,156	888	1,374	1,630	1,854	6,946

ELA (Mobility Control) Cohort, Table 6A

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	10	306	374	528	680	812	2,710
2017	22	342	226	350	416	470	1,826
2018	10	488	268	480	504	538	2,288
Total	42	1,136	868	1,358	1,600	1,820	6,824

Math (Mobility Control) Cohort, Table 6B

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	10	310	372	534	678	796	2,700
2017	22	334	222	338	410	464	1,790
2018	12	492	268	460	506	552	2,290
Total	44	1,136	862	1,332	1,594	1,812	6,780

ELA (No Transfers) Cohort, Table 8

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	6	218	262	366	456	586	1,894
2017	22	228	132	194	178	238	992
2018	4	250	62	82	102	148	648
Total	32	696	456	642	736	972	3,534

Math (No Transfers) Cohort, Table 8

	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Total
2016	6	220	260	364	452	586	1,888
2017	20	226	130	190	172	232	970
2018	4	250	56	80	104	154	648
Total	30	696	446	634	728	972	3,506