Wesleyan Economic Working Papers

http://repec.wesleyan.edu/
N°: 2025-008

Bricks and Mortar versus Computers and Modems: The Influences of Enrollment in K–12 Virtual Schools

Carycruz Bueno

March 2025

WESLEYAN
UNIVERSITY

Department of Economics
Public Affairs Center
238 Church Street
Middletown, CT 06459-007

Tel: (860) 685-2340
Fax: (860) 685-2301
http://www.wesleyan.edu/econ

Bricks and Mortar versus Computers and Modems: The Influences of Enrollment in K–12 Virtual Schools

Carycruz Bueno*

Brown University†

March 5, 2025

Abstract

The COVID-19 pandemic has placed virtual schooling at the forefront of policy concerns, as millions of children shift to virtual schooling. Given this shift, and the corresponding increase in demand for virtual education, it is imperative to explore its impacts on student outcomes. Using panel data, I find that attending a full-time virtual school in Georgia led to a reduction of 0.1 to 0.4 standard deviations in achievement test scores among elementary and middle school students. These results are robust to using multiple approaches to account for selection. I also find a negative relationship between attending a virtual school and graduation. (*JEL* I21, I24, I28)

*I thank Tim Sass for guidance and mentorship, and Barry Hirsch, Tom Mroz, and Kevin Fortner. I also appreciate mentorship from Jonathan Smith and Marie Mora. I am grateful for comments on earlier versions of this paper from Cruz Caridad Bueno, Jarod Apperson, Daniel Kriesman, Garth Heutel, Kevin Stange, Jonathan Meer, Susanna Loeb, Sarah Cohodes, participants of the American Economic Association (AEA) CSWEP session, the Association for Education Finance and Policy Conference, the AEA Summer Pipeline Conference, and the Southern Economic Association Conference. I am grateful for financial support from the National Science Foundation and the AEA Mentoring Program. All errors and views are my own.

†Annenberg Institute, Brown University, Providence, RI. E-mail: carycruz_bueno@brown.edu

The COVID-19 pandemic forced institutions to treat in-person services and online delivery of services as substitutes. Despite this substitution, the question of whether online delivery of services is comparable to in-person delivery is salient during health crises that mandate social distancing. In education, online delivery of instruction grew exponentially during the last decade, and full-time virtual schools are the fastest-growing type in the United States (Watson et al., 2010; Miron and Gulosino, 2016). However, little evidence exists regarding their influence on students' academic outcomes. Examining full-time virtual schools—in which all classes are online—although different from an emergency shift to online learning, offers insights into the influences of this education context on student performance. The COVID-19 pandemic, which forced millions of students into virtual school settings, coupled with increased demand for full-time virtual schools, makes answering how attending a full-time virtual school influences student performance in comparison to brick-and-mortar schools paramount.

This paper assesses the impact of attending a full-time virtual school on students' cognitive and behavioral outcomes, including test scores, graduation, and attendance. Two challenges are acquiring comprehensive data and accurately measuring the influence since students and families self-select into virtual schools. Such self-selection is problematic for finding causal estimates because unobserved student characteristics might confound the true influence of full-time virtual schools on student outcomes. Students who self-select into virtual schools might also have performed the same at brick-and-mortar schools, whereas such outcomes might be attributed to full-time virtual school attendance. I overcome these challenges by using a novel data set—Georgia's Academic and Workforce Analysis and Research Data System (GA•AWARDS), and implementing panel and quasi-experimental designs to account for selection. I use an individual-fixed-effects approach, which uses students who switched between virtual and brick-and-mortar schools for identification. The method yields causal estimates of the influence of virtual school enrollment as long as students switching between school types does not correlate with unobserved factors that affect student outcomes. I address potential problems with this strategy by using an interrupted panel method (Imberman, 2011), which removes the tests scores from the year prior to switching since those scores might drive unobserved factors. I also use semi-parametric cell analysis to compare outcomes for students who were in the same 4^{th} grade school cohort, are the same sex, and are the same race/ethnicity but had different amounts of full-time virtual school enrollment after the fourth grade. The approach produces treatment effect estimates similar to those derived from random assignment enrollment lotteries (Angrist et al., 2013; Dobbie and Fryer, 2013; Deming, 2014).

I find that attending a full-time virtual school leads to a reduction of between 0.1 and 0.4 standard deviations in English language arts (ELA), mathematics, science, and social studies for students in elementary and middle schools, equivalent to a loss of approximately one to two school years of learning (Center for Research on Education Outcomes, 2015). This influence is large relative to other education programs and policies studied in education economics literature. For example, Angrist (2014) find that "no-excuse" charter schools, which emphasize high expectations for students both academically and behaviorally, have an influence of 0.1 standard deviations in ELA. Results in the current paper are in the same negative direction as that found in the Center for Research on Education Outcomes (2015) report. I show descriptive evidence that students who return to brick-and-mortar schools after attending a full-time virtual school almost fully recover from their initial reduction in test scores. Regarding behavioral outcomes, I find that ever attending a full-time virtual school associates with a 10 percentage point reduction in ever graduating high school.

Although many reports provide cross-sectional comparisons of virtual and traditional schools with mixed findings (Rittner (2012); Center for Research on Education Outcomes (2012)), these studies cannot account for selection into virtual schooling (U.S. Department of Education (2009); Barth et al. (2012); Hubbard and Mitchell (2011); Miron et al. (2012); Sass (2016)). A full-time, virtual charter school is the newest school choice available, in comparison to homeschooling, brick-and-mortar charters, and private school vouchers. Much causal evidence suggests a positive influence of No Excuses brick-and-mortar charters schools on inner-city and historically marginalized students (Angrist et al., 2013, 2016b,a; Dobbie and Fryer, 2013), and the viability and positive influences of expanding such schools (Abdulkadiroğlu et al. (2016); Cohodes et al. (2020)). In contrast, brick-and-mortar charters schools not characterized by No Excuse have mixed influences on students' academic outcomes (Zimmer et al. (2012)). Evidence also suggests that brick-and-mortar charter schools have positive effects on earnings during adulthood (Sass et al., 2016). For private school vouchers, a modest, positive spillover influence is evident among students who remain in public schools in Florida (Figlio and Hart, 2014), and a negative influence is evident regarding math test scores among students in Louisiana (Abdulkadiroğlu et al., 2018). Part-time, virtual K–12 literature finds that students who attend one class online perform better or about the same as those in traditional schools (Chingos and Schwerdt, 2014), largely due to positive selection of students. Hart et al. (2019) find that later academic outcomes are negative among students who take a virtual class for the first time, but a positive influence among students who take a course for credit recovery. The Center for Research on Education Outcomes (2015) is the most comprehensive report on full-time virtual schools[1], studying 158 full-time virtual charter schools in 17 states and the District Columbia. Using a peer-matching method, they find that students who attend full-time virtual schools do worse with math and English than students who attend brick-and-mortar schools do.

This paper contributes to school choice education literature by demonstrating a causal link between student performance and full-time virtual school attendance. Extant papers, such as Chingos and Schwerdt (2014) and Hart et al. (2019), analyze only a single institution, the Florida Virtual School, which is a part-time virtual school at which students also take classes in brick-and-mortar schools. It is important to know how students perform when they take more than one class virtually, which is what full-time virtual schools offer. Unlike part-time virtual schools, full-time virtual schools are less likely to attract a positive selection of students (Chingos and Schwerdt, 2014). Findings from this paper thus inform about the influence of virtual education on a broader population of students. This study examines multiple full-time virtual schools, and unlike extant research, its data provide a more complete record of students' K–12 education history, including school settings, disciplines, and detailed attendance, allowing panel and semi-parametric methods. This study advances discussions on the influences of school choice and the causal influence of full-time virtual school attendance on student outcomes in Georgia, suggesting that students perform worse in online settings in comparison to in-person education.

Section I. provides background information on full-time virtual schools in Georgia, and describes the data. Sections II. and III. explain the theoretical foundation and econometric methods that I use. Section IV. reports results, and Section V. discusses policy implications of the findings.

[1]Concurrent with my research, Fitzpatrick et al. (2020) study the impact of full-time charter virtual schools in Indiana, finding large, negative influences on students' English and mathematics test scores.

I. Virtual Schooling in Georgia: Background and Data

A. Background

In Georgia, schools can be chartered by local school districts and by the State Charter Schools Commission (SCSC). Students can take virtual classes either through a part-time program or a full-time virtual state charter school. There are eight fully accredited, district-run, virtual part-time programs whose focus is to supplement the education of students in their districts by offering online classes.[2] Besides the eight district-run virtual programs, one statewide virtual education program exists, Georgia Virtual School,[3], which supplements students' education regardless of whether they are in public or private schools, or are home-schooled. During 2014–2015, the GVS served 30,000 students who took one or more courses. Part- and full-time virtual schools serve many students in Georgia, but most of the state's students who take full-time, online classes do so through charter schools under the authority of the SCSC.

During the period of this study, there were three full-time virtual state charter schools in Georgia—Georgia Cyber Academy (GCA), Georgia Connections Academy, and Graduation Achievement Charter (formerly Provost Academy).[4] Like all charter schools in Georgia, full-time virtual schools are overseen by nonprofit governing boards. The board holds the charter or contract, and it can contract with companies, such as K12 Inc., Pearson Inc., or EdisonLearning Inc., to provide services to the school. At full-time virtual schools, students attend classes remotely five times a week using off-site computers. Teachers at virtual schools have the same certification requirements as brick-and-mortar charter teachers in Georgia. Teachers communicate regularly with their students in virtual classes and through online communication tools, phone calls, e-mail, and face-to-face meetings. The schools offer aid to qualifying students in the form of loaner computers and Internet subsidies, since they represent barriers to entry at virtual schools. This context allows time flexibility for both students and their families.

[2]The eight district-run virtual programs are Fulton Virtual, Atlanta Virtual Academy, Cobb Virtual, Dekalb Virtual, Forsyth iAchieve Virtual Academy, Gwinnett Online Campus, Henry County Impact Academy, and Rockdale Virtual Campus. Georgia Virtual School (GVS) is a Georgia Department of Education's Office of Technology Services program that serves 6–12^{th} graders statewide. GVS serves as an education supplement for public, private, and home school students who are seeking additional courses or remedial classes. Information on GVS was taken from http://www.gavirtualschool.org/.

[3]This institution is comparable to Florida Virtual School, as assessed by Chingos and Schwerdt (2014)

[4]The Graduation Achievement Charter closed SY 2017–2018 due to poor academic performance.

Table 1 reports enrollment for each full-time virtual school, and by school type, over the years of the panel—2007 to 2016. Full-time charter virtual schools entered the public school market during the 2009/2010 school year, and by 2016, enrollment increased to over 21,000 students. Despite the large increase in demand and Georgia having one of the largest full-time virtual charter school enrollments in the country, full-time virtual school students still represent a small portion of the total student population; from 2015 to 2016, full-time virtual charter students represented a little over one percent of the Georgian student population (i.e., 1.8 million students) who attended public schools.

The first and largest full-time virtual state charter school, Georgia Cyber Academy (GCA), was created in 2009. GCA's board contracts management to the for-profit education company K12 Inc. Table 1 reports that the academy had 13,837 students enrolled in kindergarten through 12^{th} grade. Unlike other virtual schools, which typically serve high school students (Barth et al., 2012), only 35 percent of GCA's students are in high school. Before the 2014/2015 school year, GCA was part of the Odyssey School, a brick-and-mortar state charter school, and thus school-level statistics during that period include students enrolled in both online and traditional classrooms. Odyssey students represented a small portion of the GCA population. The second virtual school, Georgia Connection Academy, opened in the Fall of 2011, with an initial enrollment of 863 students and serving grades kindergarten through 12^{th} grade. GCA's board contracts with Connections Education, owned by the for-profit company Pearson Inc., for management. Table 1 shows that enrollment increased nearly five-fold to 4,241 by the 2014/2015 school year. The third virtual school, Graduation Achievement Charter High School, serves only high school students. Graduation Achievement's board first contracted with EdisonLearning for management, but later switched to Edgenuity Inc. Although the school's student population has fluctuated since its first year of operation (2013/2014), during 2016, 2,386 students were enrolled.

Table 2 reports that 66 percent of full-time virtual school students between 2010 and 2016 came from a Georgia district, brick-and-mortar school. The second largest group was students coming from home-schooling. About 4 percent of first-time virtual students were in kindergarten or first grade (i.e., they had never previously attended school). Shown in Table 3, students come from various school districts across the state of Georgia. Table 4a reports summary statistics for the number of years that students attended full-time virtual schools. On average, students attended virtual schools in Georgia for 2 years, and their attendance ranged from one to seven years.[5] Table 4a shows how many years that students attended virtual schools, of which the majority, 32,399 students, attended for only one year. Table 4b breaks down the distribution of students who attended a virtual school for one year. Eighty-four percent of those students went to a virtual school for one year and then back to a brick-and–mortar school. Ten percent attended a Georgian public school for only one year and left to attend a non-public school in Georgia, thus leaving the sample. Five percent attended for one year because they were enrolled only during the last year in the panel, 2016 (i.e., they are right-censored, and I am unsure whether they will continue to attend a virtual school in the future).

B. Data

To evaluate the performance of Georgia's virtual state charters, I use individual-level information on students and teachers in both full-time virtual charter schools and brick-and-mortar public schools (both charter and traditional) throughout Georgia. Data come from the state's longitudinal database, Georgia's Academic and Workforce Analysis and Research Data System (GA•AWARDS), which includes data from education agencies that span K-20, and Georgia's Department of Labor.[6]

GA•AWARDS includes teachers' demographics, pre-service credentials, years of experience, certification, and unemployment insurance records from the Department of Labor from 2006/2007 through 2015/2016. Student-level data include demographics, grade levels, course enrollments, course grades, standardized test scores across four subjects (i.e., ELA, math, science, and social studies), attendance, discipline, education attainment, and program participation (e.g., special education, English-language learner, free or reduced-price lunch, gifted, and homeless).

Table 1 shows enrollment by year and school type in Georgia. Annual public school enrollment in the state is approximately 1.8 million students. Although Georgia Cyber Academy opened during school year 2009/2010, it was part of a brick-and-mortar school, the Odyssey School, until 2014/2015. During that time, the Georgia Department of

[5]Data are right-censored, and I have data only until the 2016 school year

[6]Education agencies include Bright from the Start: Department of Early Care and Learning, Georgia Department of Education, State Charter Schools Commission, Georgia Student Finance Commission, University System of Georgia, Technical College System of Georgia, Georgia Independent College Association, Georgia Professional Standards Commission, and Governor's Office of Student Achievement.

Education (GaDOE) did not differentiate students who attended the brick-and-mortar and virtual programs.[7] Table 5 reports basic demographics for students in Georgia, split into virtual and non-virtual school attendance for the 2016 school year. Full-time virtual schools have a slightly higher proportion of females, a smaller portion of Hispanic students, lower average state test scores, and lower attendance rates.

II. Conceptual Framework

A. Selection into Virtual Schools

One impediment to generating causal estimates of the influence of attending a full-time virtual school on student outcomes is that students self-select into virtual schools. If unmeasured factors that determine the type of schools that students select also affect student outcomes, estimated effects of attending a virtual school will be biased. For example, if a student's parents get divorced and this shock leads the student to both attend a virtual school and do poorer while there, I would be overestimating the effect of attending a virtual school on student outcomes by attributing the effect solely to the student's attendance at a virtual school when in reality the influence is at least partially due to the divorce. Hence, modeling selection into a virtual school is paramount.

Little literature formally models the choice between charters and traditional public schools (Walters (2017); Ferreyra and Kosenok (2015); Mehta (2017)).[8] Such research on charter school choice does not apply directly to the virtual school selection problem due to several factors that distinguish full-time virtual charter schools from brick-and-mortar charter schools, which are used to model selection of traditional charters. Since virtual schools experience little to no capacity constraints, potential students do not incur any costs associated with applying to and attending admission lotteries that applicants over-subscribed to brick-and-mortar charter schools incur. Without over-subscription, application data are unavailable to identify student/family preferences. Given no spatially defined sub-statewide market area for virtual schools, general equilibrium effects are extremely difficult to identify. Peer effects in virtual schools are difficult to characterize

[7] Students who attended Odyssey, the brick-and-mortar school associated with Cyber, from 2010 to 2014 were coded as attending Georgia Cyber since most students who were enrolled attended that school

[8] Other studies focus on the supply side of the market, modeling entry into charter schools. See (Glomm et al., 2005; Singleton, 2017)

or identify, since students do not necessarily participate simultaneously and do not have face-to-face interactions with one another.

Although extant charter school choice models do not apply directly to the decision to enroll in a virtual school, I use Walters' general framework as a starting point. I model school type selection as a family that maximizes its expected utility over various school options while incurring information costs. In reality, families have a variety of school options, including private, traditional public, public charter, and virtual charter schools, and homeschooling.[9] To simplify the model, I ignore private school and homeschooling options, focusing on choices among public school alternatives. I also do not distinguish traditional and charter brick-and-mortar schools.[10] I assume that brick-and-mortar charters are close substitutes to brick-and-mortar traditional schools, arguing that families primarily choose on the margin of the type of instructional setting (i.e., virtual versus brick-and-mortar) rather than charter status. I also assume that there exists a single virtual charter school. These assumptions simplify the problem to a binary choice between enrolling in a public brick-and-mortar school and a public full-time virtual school, and families choose the school setting that yields the greatest expected utility.

Families select a virtual school during year t if the expected utility they receive is greater than the expected utility from a brick-and-mortar school. Uncertainty in the utility associated with each choice is due to imperfect information on school quality, the "fit" of the learning environment with a child's educational needs, and parental time costs associated with supporting their child in each type of school. As in Walters (2017), family preferences for schools depend in part on expected academic achievement. Expected test score Y_{ij} for student i in school j is represented by:

$$Y_{ij} = y_j(X_i, S_{it}, \epsilon_i), \tag{1}$$

where X_i are student demographics, S_{it} are school quality, and ϵ_i is unobserved academic

[9]Due to tuition costs, private schools are not an option for many families, and thus their choice set is limited to public schools, though some cities and states have vouchers that make the option viable. Homeschooling involves no tuition costs, but the option remains small. As I show later, most movement is in and out of virtual schools

[10]This assumption is reasonable if the choice between traditional public schools and full-time virtual charter schools is independent of the availability of local brick-and-mortar charter schools. I argue that differences between virtual and brick-and-mortar learning environments are far greater that differences between traditional and charter brick-and-mortar schools, and thus having the option of brick-and-mortar charter schools in the model does not alter the conclusions derived.

ability.

In addition to student achievement, families can consider a variety of school characteristics, including distance to the school (which is zero in the case of a virtual school), school schedule, non-academic peer interactions, cost of school materials (e.g., notebooks, computers, Internet access, etc.), availability of extra-curricular activities, time cost associated with supporting the child's education, and unobserved heterogeneity. The utility for attending the virtual school, v, is:

$$U_{iv} = u(Y_{iv}, X_i, S_{vt}, Int_{ivt}, TC_{ivt}, \omega_{iv}), \tag{2}$$

where X_i is a vector of observable student demographics that determine a student's/family's preferences, S_t is a vector of characteristics of the virtual school other than test scores. Int_{it} is Internet accessibility and TC_{ivt} is the expected time costs parents must invest to assist the student in the virtual school. ω_{iv} is unobserved heterogeneity of students' preference for virtual schools, and unobserved heterogeneity regarding the school. Distance is excluded from the utility function since no travel costs associate with attending a virtual school. Peer characteristics are similarly excluded since it is assumed that peer interactions in the virtual environment are negligible.

The utility associated with attending a brick-and-mortar school ,b, is:

$$U_{ib} = u(Y_{ib}, X_i, S_{bt}, P_{bt}, D_{ibt}, TC_{ibt}, \omega_{ib}), \tag{3}$$

where P_{bt} is a vector of peer characteristics at the brick-and-mortar school and D_{bt} is the distance to the brick-and-mortar school that reflects the travel costs of attendance. Internet access is excluded, based on the assumption that instruction occurs at the brick-and-mortar school site and at-home Internet access is thus unessential. The difference in utility between the virtual and brick-and-mortar schools is:

$$\begin{aligned} U_{iv} - U_{ib} &= u(Y_{iv}(X_i, S_{it}\epsilon_i), X_i, S_{vt}, Int_{ivt}, TC_{ivt}, \omega_{iv}) - u(Y_{ib}(X_i, S_{it}\epsilon_i), X_i, S_{bt}, P_{bt}, D_{ibt}, TC_{ibt}, \omega_{ib}) \\ &= u_j(X_i, S_{vt}, S_{bt}, Int_{ivt}, P_{bt}, D_{ibt}, TC_{ivt}, TC_{ibt}, \Omega_i), \end{aligned} \tag{4}$$

where Ω_i represents the effects of both academic ability and unobserved preferences for school characteristics. Families choose a virtual school during year t if u_j is positive.

Students who expect greater achievement at a virtual school are more likely to attend one. The relationship between student demographics and selection into a virtual school is unclear. It Students of different races, special education statuses, and social-economic backgrounds select differently into virtual school; the more negative the environment or the lower the school quality in a student's local school, the more likely he/she chooses to attend a full-time virtual school. Independent of school quality, peers at local schools might influence selection of a virtual school; the worse the peers at a local school (e.g., bullies), the more likely a student is to attend a virtual school. I predict that the relationship between distance to a local brick-and-mortar school and selection of a virtual school is positive; the farther away the local school, the more likely utility is gained from going to a virtual one. Costs associate with attending a virtual school. Students need a home at which there is a computer,[11] a good Internet connection, time to find out about the schools, and time parents spend with the children to ensure they are doing the work. The higher these costs, the less likely students have a home with these resources, making it less likely they will attend a virtual school. Other reasons also drive why a student wants to attend a virtual school that a researcher cannot observe.

B. Performance

I assess the influence of virtual schools on student performance as an input to the education production function.[12]

The education production function measures student achievement as a function of the individual, family, peer, and school inputs (Hanushek 1979). In its most general form, achievement of student i during period t is $A_{it} = f(I_i, F_i, P_i, S_i)$, where A_{it} represents student outcomes, which can be either cognitive (i.e., test scores) or non-cognitive (i.e., attendance, graduation, and behavior). Student outcome is a function of four vectors—student i's individual abilities, I_i, his/her family background over a lifetime, F_i, peer effects, P_i, and cumulative school inputs, S_i. I thus argue that virtual schools primarily

[11] Full-time virtual schools provide a loaner computer if the family does not own one, but families that do not own a computer have the cost of applying for financial aid to receive one.

[12] One issue is what virtual schools do to the effectiveness or performance of traditional brick-and-mortar schools, a question that lies outside the scope of this paper and is almost impossible to answer, since the market is statewide, the influence on a single school is small, and virtual school students come from many schools.

influence student achievement through school and non-peer inputs.

Full-time virtual schools can lead to either positive or negative effects on student achievement. If virtual schools offer an individualized learning experience and students receive targeted education, these lead to positive academic outcomes. Virtual schools that do not offer in-person contact, and if students need it to learn and master material, student achievement suffers. These positive and negative mechanisms might operate simultaneously, and this study assesses which is stronger, on average. Another input in which virtual school attendance might influence student achievement is peer composition, P_i. As students leave traditional schools, where peers have direct negative and positive influences on their achievement, for virtual schools, where they do not directly have peer influence, the relationship between the effect of peers and student achievement is inverted. For example, suppose a student's peers at a traditional school have positive effects on him/her (e.g., working in pairs). If the student switches to a virtual school, at which students must work independently, the student's academic achievement might be affected negatively. The opposite could also be true. For example, if a student is being bullied and does not do well because of this negative peer effect, changing to a virtual school might lead to positive academic achievement for the student.

III. Estimation Framework

A. Selection into Virtual Schools

Identifying correlates of virtual school attendance is important for two reasons. First, policymakers who determine the funding for virtual schools must know whom the schools serve. Second, since selection into virtual schools is non-random, understanding determinants of virtual school attendance allows creation of instruments that can be used during two-stage, least-squares analyses to combat selection bias during estimation of the influences of virtual schools on student outcomes. From equation 4 above, the choice between virtual and brick-and-mortar schools depends on expected achievement at each school type, Y_{iv} and Y_{ib}, student/family characteristics, (X_i), school characteristics (other than their effect through test scores), S_{vt} and S_{bt}, peer characteristics at the brick-and-mortar school that affect non-academic outcomes (e.g., bullying), P_{bt}, distance to the brick-and-mortar school, D_{bt}, availability of Internet access, Int_{ivt}, and the parental time costs of supporting a child in a virtual school (TC_{ivt}) in comparison to a brick-and-mortar school (TC_{ibt}).

I use descriptive analyses to characterize students who attend virtual schools. I consider only student/family characteristics, estimating:

$$VirtualSch_{igt} = \alpha_0 + \alpha_1 \mathbf{X}_i + \alpha_2 \mathbf{A}_{it-1} + \epsilon_{igt}, \tag{5}$$

where VirtualSch is an indicator variable of whether a student attended a virtual school during year t, X_i is a vector of student demographics, A_{it-1} is a vector of student outcomes from the previous year, and ϵ_{igt} is the normally distributed error term.

B. Performance

I use a value-added framework in which current achievement A_{it} is a function of student characteristics and prior-year test scores A_{it-1} represents prior educational inputs. I begin with naïve ordinary least squares (OLS) estimation of :

$$A_{it} = \alpha_0 + \alpha_1 X_i + \alpha_2 VirtualSch_{igt} + \alpha_3 A_{it-1} + \epsilon_{igt}, \tag{6}$$

where A_{it} is the outcome variable for student i at the end of t^{th} school year. X_i is a vector of student demographics such as race/ethnicity, sex, lunch status, special education status, and limited English proficiency (LEP) eligibility. A_{it-1} is a student's prior-year achievements, which captures both innate abilities, family characteristics and prior schooling inputs (Sass, Semykina, and Harris, Sass et al.). $VirtualSch$ is an indicator variable of whether a student attended a virtual school.[13] ϵ_{igt} is the normally distributed error term. The coefficient of interest is α_2, which represents the relationship between attending a virtual school and achievement. Given non-random selection into virtual schools, OLS estimates of equation 6 are likely biased.

Discussed in the conceptual model, unmeasured attributes of students and their families likely influence both student achievement (equation 1) and preferences for school attributes that determine the choice of school type (equations 2 and 3), which

[13] In addition to the binary definition of attending a virtual school, I also report results from when "virtual" is defined as the number of years a student attended a virtual school up to year t, when the outcome was measured

lead to biased estimates during naïve OLS estimation. To control for unmeasured, time-invariant student/family characteristics, I estimate an individual fixed effects model, in which a student's performance at a virtual school is compared to his/her performance at a brick-and-mortar school:

$$A_{it} = \alpha_0 + \alpha_1 VirtualSch_{igt} + \delta_i + \epsilon_{igt}, \tag{7}$$

$$A_{it} = \alpha_0 + \alpha_1 VirtualSch_{igt} + \alpha_2 A_{it-1} + \delta_i + \epsilon_{igt}, \tag{8}$$

where δ_i is the individual or student fixed effect. As Imberman (2011) explains, it is important to use individual fixed effects with and without lags to bound the influence of charter attendance on achievement. The drawbacks of student fixed effects are that identification relies on students who switched between school types, which might not represent the population, and such students self-select to enter virtual schools and can also self-select to leave them. Individual fixed effects also do not consider selection due to time-varying factors or shocks that correlate with dependent and independent variables; it is possible that switchers experienced a reduction to their academic achievement that motivated them to change schools, and they thus naturally performed better later (i.e., the Ashenfelter Dip).

Following the semi-parametric matching methods from Angrist et al. (2013), I match virtual students with non-virtual students at the cell level, where a cell consists of 4^{th}-grade school, sex, race, and cohort. Although the method does not address the bias of students who self-select into virtual charter schools completely, it does control for differences among the four dimensions and also unmeasured characteristics associated with the neighborhood in which a student attended elementary school. Research demonstrates that this method produces results similar to those from experimental studies (i.e., studies based on randomized enrollment lotteries; Angrist et al. (2016a, 2013)). I estimate:

$$A_{it} = \alpha_0 + \sum_{m} \alpha_2 VirtualSch_{itv} + \alpha_3 A_{it-1} + \sigma_{cell} + \epsilon_{it}, \tag{9}$$

where VirtualSch is the number of years student i attended school v by year t (Dobbie and Fryer, 2016) and α_2 is the effect of attending a virtual charter school, v. σ_{cell} is a cell fixed effect. Like Dobbie and Fryer (2016), I cluster standard errors at the matched cell level since it then considers correlation of errors among observationally equivalent students who attended the same elementary school.

IV. Results

A. Predicting attendance into Virtual school

Table 6a reports estimates for selection into virtual schools outlined in equation 5. Column one includes only prior English language arts (ELA) test scores as a regressor on attending a virtual school in the current year. Prior ELA scores do not alone predict whether a student will attend a virtual school during the following year. The second column shows only prior mathematics test scores as a predictor, suggesting that a one-standard-deviation increase to the prior year's scores associates with a decrease of 0.114 percentage points in the likelihood of attending a virtual school that year. A student with a better score last year is thus less likely to go to a virtual school this year. Column 3 includes last year's student's percent of attendance, showing that the higher a student's attendance last year, the less likely he/she will attend virtual school the following year. The last column includes the prior year's ELA scores, mathematics scores, and attendance, and student demographics, suggesting negative selection into virtual schools based on prior performance.

One problem with selection into a virtual school is that students' previous schools might been virtual or non-virtual, and thus some of the estimate includes the influence of already being at a virtual school. To address this issue, Table 6b limits analysis to students who during the previous year attended a non-virtual school, predicting whether a student attends a virtual school the next year. As in table 6a, negative selection into virtual schools but a slightly smaller association are evident. For example, a one-standard-deviation increase to the prior year's mathematics test scores associates with a decrease of 0.04 percentage points in the likelihood of attending a virtual school that year, whereas the previous table estimates 0.114 percentage points.

B. Ordinary Least Squares

Table 7a shows estimates from ordinary least squares, which suggest that attending a virtual school associates with lower test scores across all four subjects, while controlling for same-subject lagged test scores and demographics. Attending a virtual school also associates with a reduction of 0.011 standard deviations in ELA, 0.169 standard deviations in mathematics, 0.107 standard deviations in science, and 0.190 standard deviations in social studies. Thee last three subjects had large decreases in test scores. Excepting Sass (2016), no studies assess social studies and science scores in the context of full-time virtual schools. These associations suggest that students who attend full-time virtual schools are performing worse than their counterparts in science and social studies, in addition to the two more researched subjects, mathematics and ELA.

When I limit the population to those who during the previous year attended a non-virtual school (Table 7b), the influence is stronger, suggesting that moving directly from a non-virtual school to a virtual school has a larger influence on students, or that the first transition year is the most difficult. Attending a virtual school while controlling for a student's previous non-virtual school test scores and demographics associates with a reduction of 0.06 standard deviations in ELA, 0.26 standard deviations in mathematics, 0.21 standard deviations in science, and 0.34 standard deviations in social studies

Table 8 restricts the population to only charter school students to assess whether this is a virtual or charter school effect. Results are similar to those in Table 7a; students who attend full-time virtual charter schools do 0.18 and 0.03 standard deviations worse than charter brick-and-mortar students across the four subjects. These results suggest that the relationship does not derive from a charter school effect, but are instead a virtual school effect. However, these results are associations that do not directly address selection into virtual schools, which the next models assess.

C. Student Fixed Effects

One way to mitigate selection is by implementing individual fixed effects and thus controlling for time-invariant characteristics. Identification relies on students who switch between school types but are not switching because of the outcome. I report estimates for individual fixed effects both with and without last year's lagged scores, which represent a bound of the influence of virtual schools on student test scoresImberman (2011). Table

9 shows that when controlling for time-invariant characteristics, students who attend a virtual school perform worse than the OLS regression suggests. For each subject, I report estimates from individual fixed effects without a test lag first, and in the following column, I control for same-subject-lagged test scores. Attending a virtual school leads to a reduction of 0.12 standard deviations in ELA, 0.31 standard deviations in mathematics, 0.27 standard deviations in science, and 0.4 standard deviations in social studies. To place these numbers in context, a teacher with ten or more years of experience increases a student's reading test scores by about 0.17 standard deviations (Rockoff, 2004), meaning that these students would need 1 to 2 years with an experienced teacher to recover from the negative effects of attending a virtual school.

One issue with individual fixed effects is time-varying shocks that influence the outcome being unable to be controlled for. One way to test this is by examining test score trends pre- and post-entry into a virtual school. Figures 3 through 6 show regression coefficients plotted on the y-axis for the periods before and after a student's first year in a virtual school across populations of students who have ever attended a virtual school and entered a virtual school in grades 3 through 8. Figure 3 shows students who have ever attended a virtual school and after entry, never exited a virtual school. Both ELA and math students experienced a slight reduction before entering a virtual school. During the first year that they attended a virtual school, they suffered a further reduction, more so in math, and then improved slightly after being at a virtual school for three years. Figure 4 shows a similar result, though the population excludes those who attended a virtual school for only one year. Figure 5 shows the trend for students who attended a virtual school for only one year and returned to a brick-and-mortar. They had a more dramatic decline in both test scores before entering a virtual school, but once they returned to a brick-and-mortar school, they experienced recovery to their previous performance. Similarly, Figure 6 shows that students who attended a virtual school for two years and returned to a brick-and-mortar experienced a reduction and recovery once they returned. Given the drastic difference between students who only attended one year versus those who attended more than one year and remained at a virtual school, I perform sub-sample analysis for the two groups. Table 11 shows that students who attended a full-time virtual school for only one year and returned to brick-and-mortar did 0.16 to 0.44 standard deviations worse than during their non-virtual years across the four subjects. Those who attended a full-time virtual school for at least two years and did not leave did 0.079 to

0.364 standard deviations worse than while attending virtual schools, in comparison to their performance in brick-and-mortar schools. These two samples cannot be compared directly due to selection, such that families might have selected to attend only one year versus staying at a full-time virtual school.

One way extant papers such as Imberman (2011) deal with the Ashenfelter Dip is estimating an interrupted panel. Like Imberman (2011), I remove the year before entering a virtual school and use the average of two-year gains as a lagged score. One disadvantage to not using direct lagged scores is a loss of sample size. Table 10 reports interrupted panel estimates of attending a virtual school on student test scores. I find that attending a virtual school leads to a reduction of 0.14 to 0.08 standard deviations in ELA, 0.35 to 0.20 standard deviations in mathematics, 0.30 to 0.14 standard deviations in science, and 0.43 to 0.19 standard deviations in social studies.

Tables 12 through 16 report heterogeneous effects across demographics and grade level. Table 12 shows the influence on four sub-samples—females only, males only, ever FRL, and non-white students. Both females and males performed worse while attending a full-time virtual school, though males did slightly worse on ELA tests. Students who have ever been on free or reduced lunch, and non-white students, did 0.1 to 0.4 standard deviation worse while at a full-time virtual school. Table 13 shows whether results are driven by entering a full-time virtual school during a typical transition grade, kindergarten, and 6^{th} or 8^{th} grade versus an atypical year. I find that when entering full-time virtual schools during an atypical year, students did 0.03 to 0.07 standard deviations worse than those who entered during a typical year in English, mathematics, and science. Table 14 limits the sample to students who had ever been home schooled. I find similar negative influences that range from 0.12 to 0.42 standard deviations worse while in a full-time virtual school, providing more evidence of negative influences across sub-samples.

Tables 15 and 16 report heterogeneous effects across grade level. These influences might be driven by either elementary or middle school students. Table 15 shows the individual fixed effects for students in 4^{th} and 5^{th} grade. I find that attending a virtual school leads to a reduction of 0.16 standard deviations in ELA, 0.32 standard deviations in mathematics, 0.31 standard deviations in science, and 0.32 standard deviations in social studies in elementary grades. Middle school students performed slightly better than elementary students did , but a negative influence is still evident. Table 16 shows that attending a virtual school leads to a reduction of 0.05 standard deviations in ELA, 0.24 standard deviations in mathematics, 0.27 standard deviations in science, and 0.36 standard deviations in social studies for students in middle schools grades 6 through 8.

D. Semi-Parametric Cell Model

To measure the causal influence of attending a full-time virtual school on student outcomes, I would ideally randomize which students attended a virtual school. Since this and over-subscription are nearly impossible, the next best method is semi-parametric cell analysis(Angrist et al., 2013), where full-time virtual school students are compared to non-virtual school students who were in the same 4^{th} grade school, gender, race, and cohort. Table 17 reports that the influence of attending a virtual school is different from zero across the four subjects, with a 0.02 to 0.2 standard deviation decline in test scores in comparison to students who went to the same brick-and-mortar school as students in 4^th grade and having the same sex and race.

E. Probit Model-Graduation and Attendance

Table 18 reports results of the relationship of attending a virtual school and graduating high school. The first column defines the independent variables as ever attending a virtual school in Georgia, suggesting it associates with a 10 percentage point reduction in ever graduating high school. The second column shows that an additional year of attending a virtual school associates with a 2 percentage point decline in ever graduating high school. Table 19 reports results of the relationship of virtual school attendance and percent of attendance in a school year. Across the three definitions of virtual school—total number of years virtual, ever virtual, and years of virtual enrollment by year t—all indicate no relationship; attending a virtual school associates with no worse attendance. I caution against relying on the last result since attendance is measured differently at full-time virtual schools.

V. Summary and Conclusion

One of the most debated contemporary education issues is school choice, and the fastest growing option is full-time virtual school. The debate centers on parents' education choices for their children and whether the alternatives are better for students than the existing public school system. The COVID-19 crisis made the debate more salient, given the rush to online instruction in hopes of slowing the spread of infection. Essential to framing the debate and creating sound education policies is understanding whether full-time virtual school attendance will have a positive influence on students, due to their individualized structures, or a negative influence, due to a lack of in-person instruction. Using individual fixed effects and semi-parametric cell analysis, this paper suggests that a lack of in-person instruction and classroom socialization influence students' outcomes negatively.

I find that attending a full-time virtual school in Georgia leads to a negative influence on student test scores of 0.1 to 0.4 standard deviations across four subjects—English, mathematics, science, and social studies—where the magnitudes depends on the model implemented. These results are robust to implementing an interrupted panel method that mitigates the Ashenfelter Dip that students experienced prior to enrolling in a virtual school and semi-parametric cell analysis used in charter school literature. I also conduct sub-sample analyses, finding that those who attended a full-time virtual school for one year performed worse than expected in comparison to how they performed in non-virtual schools. However, when the students returned to a brick-and-mortar school, their outcomes improved to the degree of pre-virtual school attendance. These negative influences hold during sub-sample analysis across gender, FRL status, and race. I also find that elementary students preformed slightly worse than middle school students did. Among high school students, attending a full-time virtual school associates with a reduction in their graduation rate of about 2 to 10 percentage points. These influences are large and both socially and economically significant. These results corroborate Center for Research on Education Outcomes (2015)'s conclusion that full-time virtual schools, on average, have a negative influence on student outcomes.

The health pandemic COVID-19 has influenced the world in various areas, including education. Unlike with full-time virtual schools, parents did not have a choice to go to a virtual school and teachers were unprepared to instruct students online. Given current circumstances, parents and education leaders want to know how transitioning to virtual schooling influences students' academic and behavioral outcomes. I demonstrate that full-time virtual schooling leads to negative influences on elementary and middle school students, suggesting that students influenced by COVID-19 and forced to switch to virtual learning will lose educational attainment given the abrupt transition to virtual schooling. Inferred from sub-sample analysis of non-white students, virtual schooling increases inequality, especially given the disproportional influences that COVID-19 has had on the non-white population's health, and since Blacks and Latinx parents are more likely to be essential workers. Negative influences of COVID-19 on Black and Latinx populations, in addition to negative influences of full-time virtual schools, suggest a need for more funding and resources among these populations, both in and out of education. One positive piece of evidence suggests that students who reenter brick-and-mortar schools nearly fully recover, and therefore education policy should prioritize returning students to brick-and-mortar schools.

Given the results suggested in this study and the money invested in these schools, it appears that full-time virtual schools, as a school choice, is not the best alternative for average parents and their children. From the little research conducted on full-time virtual schools, evidence suggests that these schools are harmful to students' learning and future economic opportunities, and represent sub-optimal use of taxpayer money in Georgia. When parents apply to these schools, more information on student performance should be given to them so that they can choose a school that maximizes their expected utility, given their situation. For some students, this choice might be beneficial, especially if the alternative is the student dropping out or other negative outcomes, such as committing a crime. If full-time virtual charter schools are not reaching their accountability targets, the schools should be closed.[14] This paper assesses only Georgian, full-time virtual schools, and thus more research is needed to determine whether its results apply to other states, and to assess the influence of abrupt transitions on virtual learning due to COVID-19. Research should also examine long-term outcomes, such as college enrollment, persistence, and labor force participation.

[14]The State Charter School Commission closed Graduation Achievement Charter during this study since it was not reaching academic goals

References

Abdulkadiroğlu, A., J. D. Angrist, P. D. Hull, and P. A. Pathak (2016). Charters without lotteries: Testing takeovers in new orleans and boston. *American Economic Review 106*(7), 1878–1920.

Abdulkadiroğlu, A., P. A. Pathak, and C. R. Walters (2018, January). Free to choose: Can school choice reduce student achievement? *American Economic Journal: Applied Economics 10*(1), 175–206.

Angrist, J. D. (2014). The perils of peer effects. *Labour Economics 30*, 98–108.

Angrist, J. D., S. Cohodes, S. Dynarski, P. A. Pathak, and C. R. Walters (2016a). Stand and deliver: Effects of boston's charter high schools on college preparation, entry, and choice. *Journal of Labor Economics 34*(2), 275–318.

Angrist, J. D., S. R. Cohodes, S. M. Dynarski, P. A. Pathak, and C. R. Walters (2016b). Stand and deliver: Effects of Boston's charter high schools on college preparation, entry, and choice. *Journal of Labor Economics 34*(2), 275–318.

Angrist, J. D., P. A. Pathak, and C. R. Walters (2013). Explaining charter school effectiveness. *American Economic Journal: Applied Economics 5*(4), 1–27.

Barth, P., J. Hull, and R. St. Andrie (2012). Searching for the reality of virtual schools. Technical report.

Center for Research on Education Outcomes, C. (2012). Charter school performance in pennsylvania. Technical report.

Center for Research on Education Outcomes, C. (2015). Online charter school study. Technical report.

Chingos, M. and G. Schwerdt (2014). Virtual schooling and student learning: Evidence from the florida virtual school. Working paper.

Cohodes, S., E. Setren, and C. Walters (2020). Can successful schools replicate? scaling up boston's charter school sector. *American Economic Journal: Economic Policy, forthcoming*.

Deming, D. (2014). Using school choice lotteries to test measures of school effectiveness. *American Economic Review 104*(5), 406–11.

Dobbie, W. and R. Fryer (2016). Charter schools and labor market outcomes. Working paper.

Dobbie, W. and R. G. Fryer (2013). Getting beneath the veil of effective schools: Evidence from New York City. *American Economic Journal: Applied Economics 5*(4), 28–60.

Ferreyra, M. M. and G. Kosenok (2015). Charter school entry and school choice: the case of washington, d.c. *Policy Research Working Paper Series 7383*, –.

Figlio, D. and C. M. D. Hart (2014, January). Competitive effects of means-tested school vouchers. *American Economic Journal: Applied Economics 6*(1), 133–56.

Fitzpatrick, B. R., M. Berends, J. J. Ferrare, and R. J. Waddington (2020). Virtual illusion: Comparing student achievement and teacher and classroom characteristics in online and brick-and-mortar charter schools. *Educational Researcher 49*(3), 161–75.

Glomm, G., D. Harris, and T. Lo (2005). Charter school location. *Economics of Education Review 24*(4), 451–457.

Hart, C., D. Berger, B. Jacob, S. Loeb, and M. Hill (2019). Online learning, offline outcomes: Online course taking and high school student performance. *AERA Open 5*(1), 1–17.

Hubbard, B. and N. Mitchell (2011, October 4). roubling questions about online education. *Education News Colorado*.

Imberman, S. A. (2011). Achievement and behavior in charter schools: Drawing a more complete picture. *The Review of Economics and Statistics 93*(2), 416–435.

Mehta, N. (2017). Competition in public school districts: Charter school entry, student sorting, and school input determination. *International economic review. 58*(4), 1089–1116.

Miron, G. and C. Gulosino (2016). Virtual schools report 2016: Directory and performance review. Technical report.

Miron, G., A. Urschel, and Dailey (2012). Profiles of for-profit and nonprofit education management organization. Technical report, University of Colorado, Boulder.

Rittner, G. W. (2012). Internal evaluation of the arkansas virtual academy school. Technical report.

Rockoff, J. E. (2004, May). The impact of individual teachers on student achievement: Evidence from panel data. *American Economic Review 94*(2), 247–252.

Sass, T. (2016). The performance of state charter schools in georgia, 2014-2015. state charter schools performance evaluation. Technical report.

Sass, T., A. Semykina, and D. Harris. Value-added models and the measurement of teacher productivity. *Economics of Education Review* (38), 9–23.

Sass, T. R., R. W. Zimmer, B. P. Gill, and T. K. Booker (2016). Charter high schools' effects on long-term attainment and earnings. *Journal of Policy Analysis and Management 35*(3), 683–706.

Singleton, J. (2017). Incentives and the supply of effective charter schools. Working paper.

U.S. Department of Education, Office of Planning Evaluation Policy Development, D. (2009). Evaluation of evidence-based practices in online learning: A meta-analysis and review of online learning studies. Technical report.

Walters, C. R. (2017, October). The demand for effective charter schools. Working Paper 20640, National Bureau of Economic Research.

Watson, J., B. Gemin, J. Ryan, and M. Wicks (2010). Keeping pace with k-12 online and blended learning: An annual review of state-level policy and practice. Technical report.

Zimmer, R., B. Gill, K. Booker, S. Lavertu, and J. Witte (2012). Examining charter student achievement effects across seven states. *Economics of Education Review 31*(2), 213–224.

Figure 1: Grade Level of Initial Enrollment in Georgian, Full-Time Virtual Schools, 2010–2016

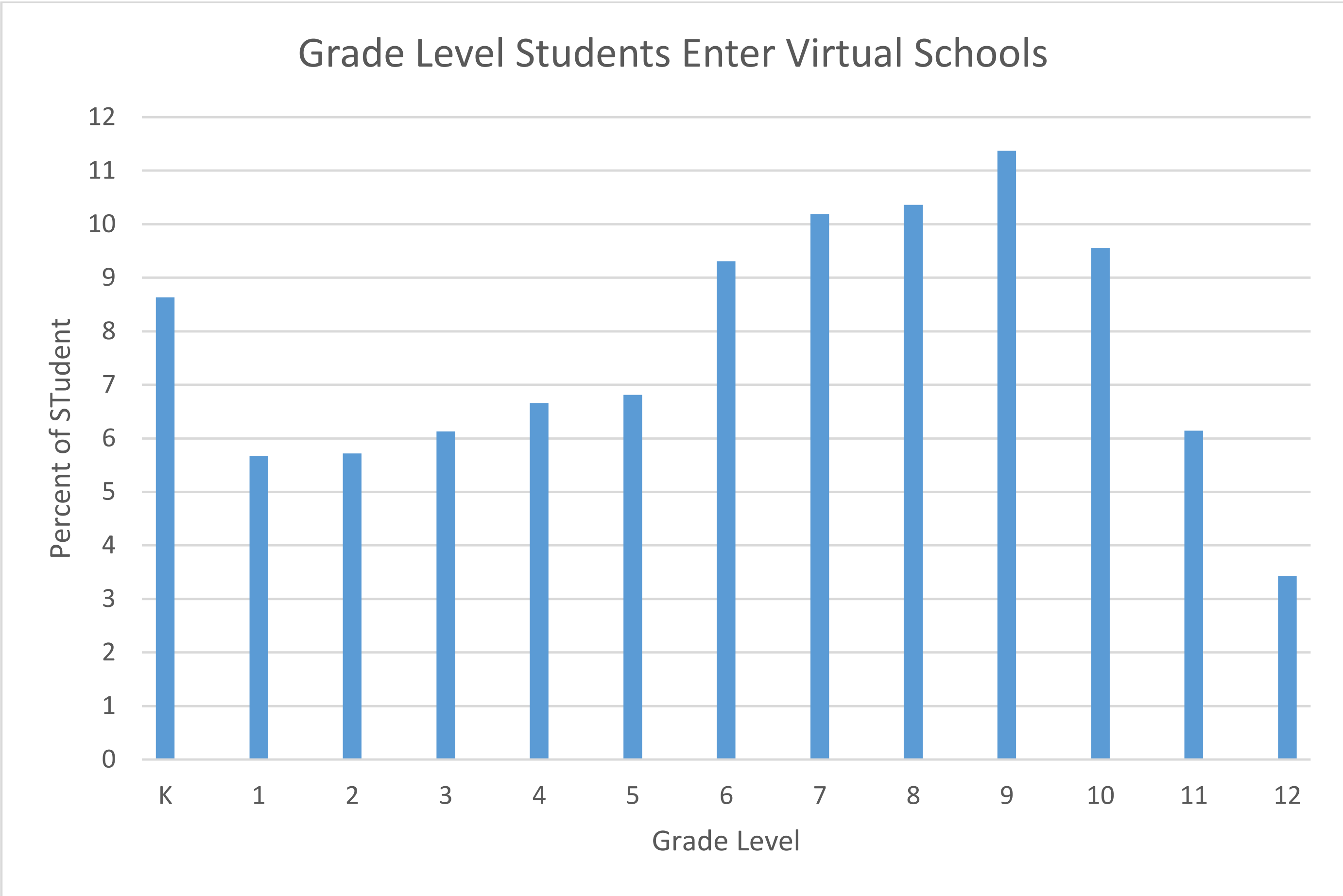


Figure 2: Grade Level at Which First-Time Georgian, Virtual School Students Exit a Full-Time Virtual School, 2010–2016

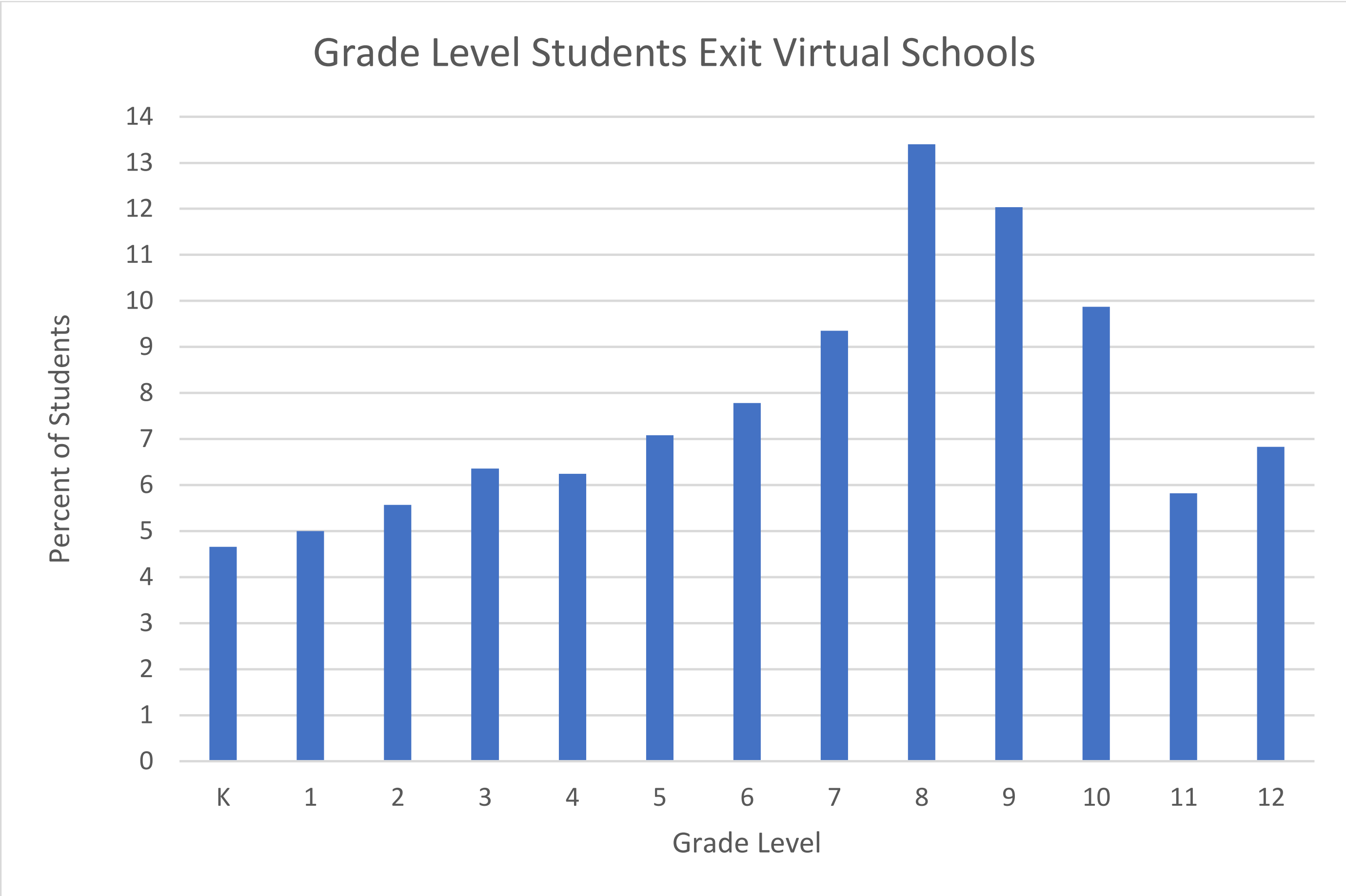
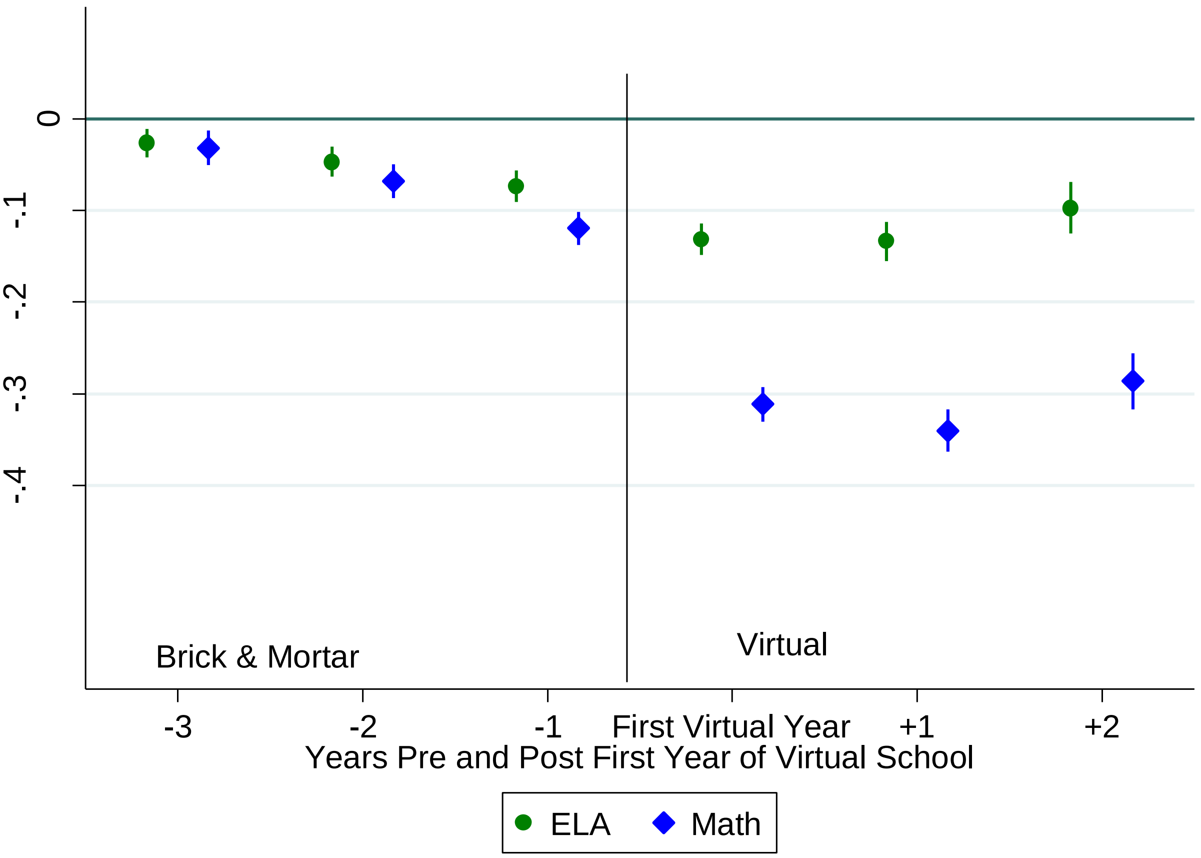
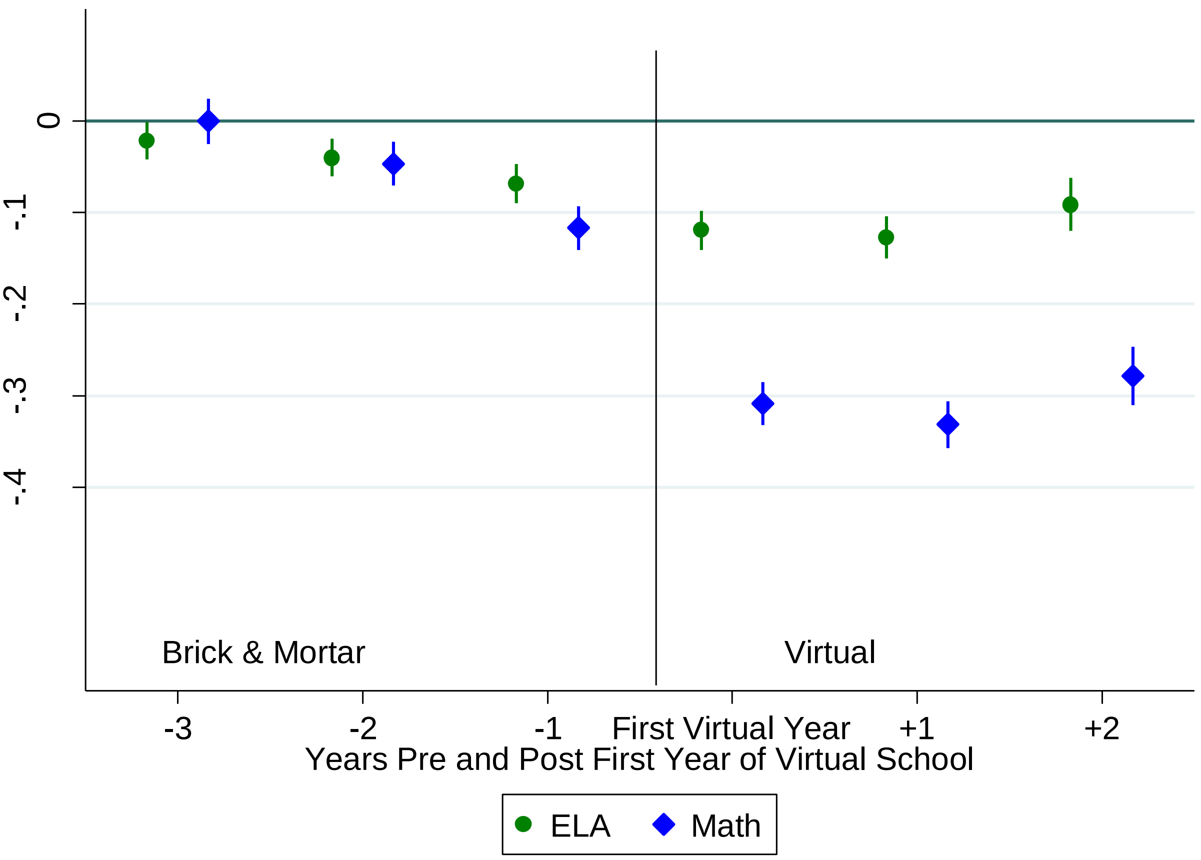


Figure 3: Within-Student Normalized Student Test Scores by School Type for Brick-and-Mortar School Students who Transition to a Full-time Virtual School in Grades 3–8 and Do Not Return to a Brick-and-Mortar School.



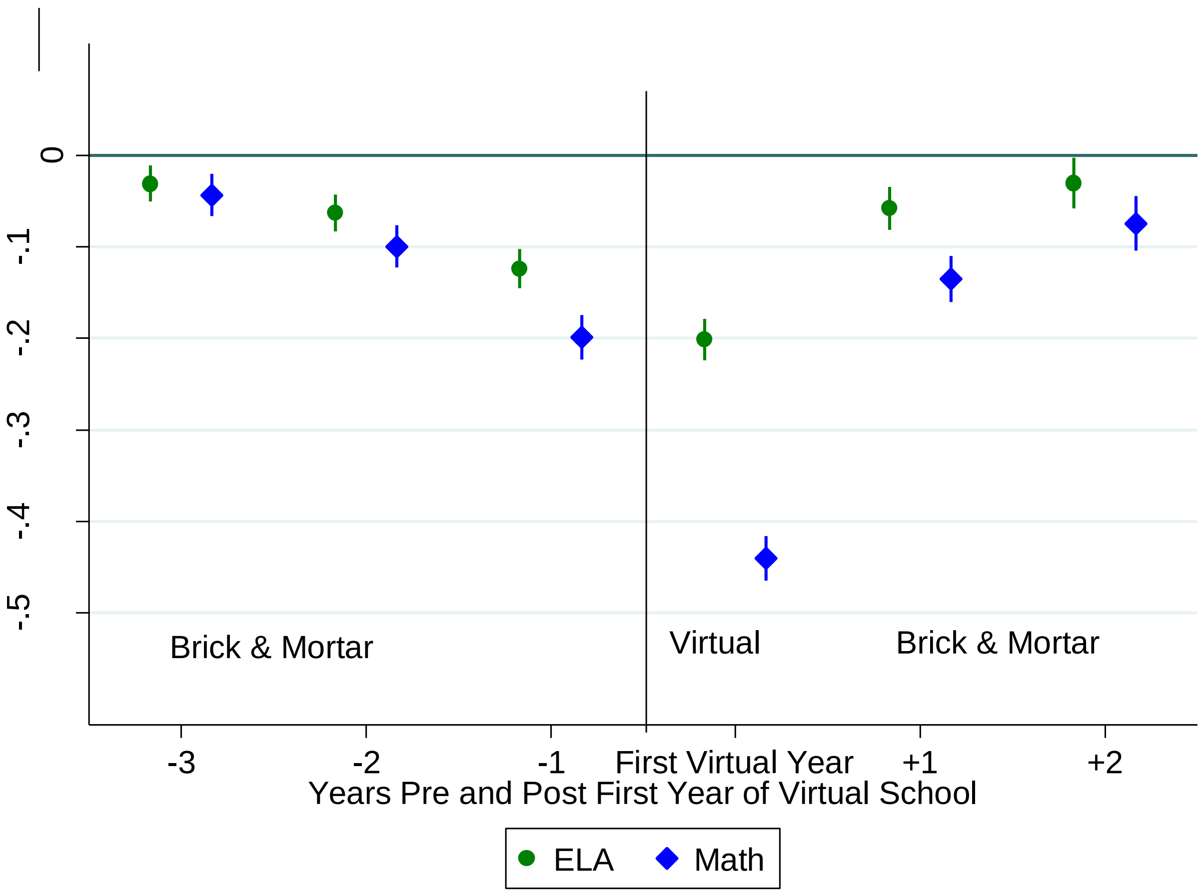
Notes: Coefficients of indicator variables are plotted on the graph, where the variable equals one if they were x years before or after entering a virtual school. Vertical band represent +/- 1.96 confidence intervals. The sample is limited to students who enter a virtual school in grades 3 through 8. English Language Arts has 55,691 Student-Year observations and Mathematics has 55,250 Student-Year observations

Figure 4: Within-Student Normalized Student Test Scores by School Type for Brick-and-Mortar School Students who Transition to a Full-time Virtual School in Grades 3–8, and Attended a Virtual School for More than One Year and Did Not Exit.



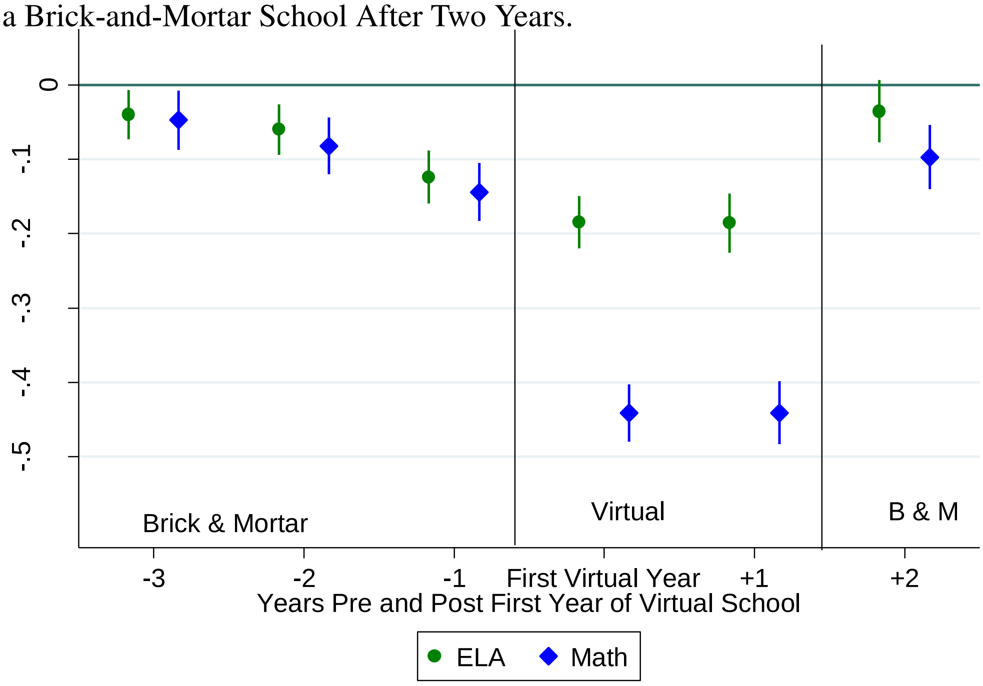
Notes: Coefficients of indicator variables are plotted on the graph, where the variable equals one if they were x years before or after entering a virtual school. Vertical band represent +/- 1.96 confidence intervals. The sample is limited to students who enter a virtual school in grades 3 through 8. English Language Arts has 33,976 Student-Year observations and Mathematics has 33685 Student-Year observations

Figure 5: Within-Student Normalized Student Test Scores by School Type for Brick-and-Mortar School Students who Transition to a Virtual School in Grades 3-8 and Return to a Brick-and-Mortar School After One Year.



Notes: Coefficients of indicator variables are plotted on the graph, where the variable equals one if they were x years before or after entering a virtual school. Vertical band represent +/- 1.96 confidence intervals. The sample is limited to students who enter a virtual school in grades 3 through 8. English Language Arts has 32,541 Student-Year observations and Mathematics has 32,286 Student-Year observations

Figure 6: Within-Student Normalized Student Test Scores by School Type for Brick-and-Mortar School Students who Transition to a Virtual School in Grades 3-8 and Return to a Brick-and-Mortar School After Two Years.



Notes: Coefficients of indicator variables are plotted on the graph, where the variable equals one if they were x years before or after entering a virtual school. Vertical band represent +/- 1.96 confidence intervals. The sample is limited to students who enter a virtual school in grades 3 through 8. English Language Arts has 11,728 Student-Year observations and Mathematics has 11,649 Student-Year observations

VI. Tables

Table 1: Number of Students Enrolled in Georgia per Year by School Type

School Year	Georgia Cyber	Georgia Conn.	Grad. Ach.	Virtual Schools Enrollment	Non-Virtual Schools Enrollment	Total Enrollment
2007	0	0	0	0	1708156	1708156
2008	0	0	0	0	1722093	1722093
2009	0	0	0	0	1724994	1724994
2010	6418	0	0	6418	1728364	1734782
2011	6738	0	0	6738	1735161	1741899
2012	11345	863	0	12208	1737150	1749358
2013	11782	2269	1179	15230	1748500	1763730
2014	13506	3571	2195	19272	1766868	1786140
2015	13837	4241	2767	20845	1786754	1807599
2016	14530	4142	2386	21058	1801315	1822373
				(1.16)	(98.84)	(100.00)

Notes:Numbers in parentheses are percentages for school year 2016.

Table 2: Previous School Attended by Full-time Virtual School Student from 2010-2016

Reason for Entering	First Time Virtual	Percent
Re-enter Other	16	0.03
GA District	36232	64.68
Homeschool	8574	15.31
Other	10	0.02
Never Attend	3887	6.94
Out State	1948	3.48
Private	3075	5.49
Re-enter After Withdrawal	77	0.14
Unknown	2195	3.92
Total	56014	100

Notes: Table reports entry code for the first time a student enters a full-time virtual school. If a student did not have an entry code they were coded as unknown. The category Other includes: Illness, Incarcerated, School Choice, and Within the School System

Table 3: Top 24 Previous Districts Attended by Full-Time Virtual School Student.

Sending District	Freq.	Percent
Gwinnett County	1,207	7.71
DeKalb County	1,090	6.96
Cobb County	923	5.9
Fulton County	725	4.63
Clayton County	625	3.99
Henry County	487	3.11
Cherokee County	439	2.8
Richmond County	379	2.42
Chatham County	357	2.28
Coweta County	350	2.24
Paulding County	346	2.21
Atlanta Public Schools	328	2.09
Douglas County	306	1.95
Newton County	285	1.82
Columbia County	261	1.67
Houston County	253	1.62
Bibb County	221	1.41
Forsyth County	215	1.37
Rockdale County	213	1.36
Muscogee County	194	1.24
Bartow County	163	1.04
Fayette County	162	1.03
Walton County	146	0.93
Carroll County	142	0.91

Notes: Percent indicates the portion of first-time virtual school students that attended that district in the previous year from school years 2010-2016.
Percent indicates the percent of first time students that attended that district in the previous year

Table 4a: Summary Statistics and Number of Years Students Attend a Full-time Virtual School

Number of Years	Student Observations
1	32399
2	12080
3	5774
4	2875
5	1533
6	748
7	605
Total	56014
Mean	1.82
SD	1.27

Table 4b: Count of Students Who Attend a Full-time Virtual School for One Year Broken Down by Reason

Classification of One Year in a Full-time Virtual School	Count	Percent
One Year in Virtual School: Enter and Exit a Virtual School	27,333	84.4%
One Year in the Ga. Public School Sys. Panel and School Year is not 2016	3,344	10.3%
One Year in Virtual School: During the Last Available Year of the Panel-2016	1,722	5.3%
Total	32,399	100%

Table 5: Summary Statistics for School Year: 2016

	All Student Observations	Non-Virtual Student Observations	Virtual Student Observations	
Demographics	Mean	Mean	Mean	Difference
Female	0.50	0.49	0.52	0.03***
Black	0.37	0.37	0.37	0.00
White	0.50	0.49	0.50	0.02***
Native American	0.031	0.033	0.0038	-0.03***
Asian	0.039	0.038	0.016	-0.02***
Pacific Islander	0.002	0.002	0.0010	0.00*
Multi-racial	0.045	0.047	0.074	0.03***
Hispanic	0.14	0.15	0.067	-0.08***
Ever SPED	0.16	0.16	0.17	0.01***
Ever LEP	0.023	0.023	0.0013	-0.02***
Ever Migrant	0.0054	0.0056	0.00062	0.00***
Ever Homeless	0.065	0.066	0.069	0.00
Ever Free or Red. Lunch	0.67	0.68	0.83	0.15***
Percent Present	87.9	88.1	78.9	-9.11***
ELA Mean Score	507.5	507.5	501.0	-6.54***
Math Mean Score	514.5	514.7	496.1	-18.59***
Science Mean Score	506.2	506.3	496.2	-10.17***
Social Std. Mean Score	507.1	507.3	489.2	18.09***
Observations	2135827	1801274	21058	1822332

Table 6a: Predictors of Full-time Virtual School Attendance

	(1)	(2)	(3)	(4)
Lagged ELA Score	-0.0000386			0.00253
	(0.0000302)			(0.0000503)
Lagged Math Score		-0.00114		-0.00264
		(0.0000286)		(0.0000471)
Lagged Percent Present			-0.000129	-0.0000544
			(0.00000110)	(0.00000165)
Female				-0.000502
				(0.0000580)
Black				-0.00484
				(0.0000677)
Asian				-0.00229
				(0.000166)
Hispanic				-0.00722
				(0.0000944)
Ever SPED				0.0000775
				(0.0000821)
Ever LEP				-0.00568
				(0.000928)
Ever Migrant				-0.00229
				(0.000398)
Ever Homeless				-0.00407
				(0.000112)
Ever Free or Red. Lunch				0.00646
				(0.0000735)
Year FE	✓	✓	✓	✓
Grade FE	✓	✓	✓	✓
Observations	6383112	6369127	13956527	6349054

Notes:Standard errors in parentheses. Virtual school is defined as 1 if student attended a full-time virtual school that year. I estimated selection using Linear Probability Model and each column is a different regression

Table 6b: Predictors of Virtual School Attendance Conditional on not Attending a Virtual School the Previous Year.

	(1)	(2)	(3)	(4)
ELA Lagged	-0.00007 (0.00002)			0.0009 (0.00004)
Math Lagged		-0.0004 (0.00002)		-0.0008 (0.00003)
Lagged Percent Present			-0.0001 (0.0000008)	-0.00008 (0.000001)
Female				0.000005 (0.00004)
Black				-0.00225 (0.00005)
Asian				-0.00169 (0.000116)
Hispanic				-0.00321 (0.0000659)
Ever SPED				0.000293 (0.0000574)
Ever LEP				-0.00227 (0.000647)
Ever Migrant				-0.00131 (0.000277)
Ever Homeless				-0.00175 (0.0000782)
Ever Free or Red. Lunch				0.00274 (0.0000513)
Year FE	✓	✓	✓	✓
Grade FE	✓	✓	✓	✓
Observations	6359033	6345149	13893707	6325188

Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Sample is limited to those who in the previous year were not in a virtual school and virtual equals to one if they attended a virtual school that year. I estimated selection using Linear Probability Model and each column is a different regression

Table 7a: Ordinary Least Square Estimates of the Effect of Virtual School Attendance on Normalized End-of-Grade Achievement Test Scores, Grades 4-8

	ELA	Mathematics	Science	Social Studies
Virtual	-0.011 (0.003)	-0.169 (0.003)	-0.107 (0.003)	-0.190 (0.003)
ELA Lagged	0.770 (0.0003)			
Math Lagged		0.713 (0.0003)		
Science Lagged			0.719 (0.0003)	
Social Studies				0.717 (0.0003)
Year FE	✓	✓	✓	✓
Grade FE	✓	✓	✓	✓
Observations	6355086	6303140	5397484	4610350

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Demographics include race, Hispanic, sex, special education eligibility, ever free and reduced lunch, ever homeless and ever migrant. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 7b: Ordinary Least Square Estimates of the Effect of Virtual School Attendance on Test Score Grades 4-8 and Conditional on Not Attending a Virtual School the Previous Year.

	ELA	Mathematics	Science	Social Studies
Virtual	-0.061	-0.266	-0.208	-0.348
	(0.004)	(0.005)	(0.005)	(0.005)
ELA Lagged	0.767			
	(0.0003)			
Math Lagged		0.702		
		(0.0003)		
Science Lagged			0.718	
			(0.0003)	
Social Studies				0.717
				(0.0003)
Year FE	✓	✓	✓	✓
Grade FE	✓	✓	✓	✓
Observations	6326573	6244083	5358227	4572734

Notes: Standard errors in parentheses. Sample is limited to those who in the previous year were not in a virtual school. Virtual school is defined as 1 if student attended a virtual school that year. Demographics include race, Hispanic, sex, special education eligibility, ever free and reduced lunch, ever homeless and ever migrant. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 8: Ordinary Least Square Estimates of the Effect of Virtual School Attendance on Test Score Grades 4-8 and Conditional on Students Attending a Charter School Scores, Grades 4-8.

	ELA	Mathematics	Science	Social Studies
Virtual	-0.028 (0.003)	-0.165 (0.004)	-0.084 (0.004)	-0.177 (0.004)
ELA Lagged	0.763 (0.001)			
Math Lagged		0.714 (0.001)		
Science Lagged			0.717 (0.001)	
Social Studies				0.715 (0.001)
Year FE	✓	✓	✓	✓
Grade FE	✓	✓	✓	✓
Observations	297621	295828	254095	227753

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Demographics include race, Hispanic, sex, special education eligibility, ever free and reduced lunch, ever homeless and ever migrant. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 9: Student Fixed Effects Model Estimates of the Effect of Virtual School Attendance on Test Score Grades 4-8.

	ELA		Math		Science		Social Studies	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Virtual	-0.119	-0.116	-0.309	-0.307	-0.265	-0.271	-0.396	-0.400
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
ELA Lagged		-0.030						
		(0.000)						
Math Lagged				-0.037				
				(0.000)				
Science Lagged						-0.126		
						(0.000)		
Social Std. Lagged								-0.063
								(0.000)
Observations	8538404	6351128	8466631	6268306	7519158	5381632	7039285	4596128

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Demographics include race, Hispanic, sex, special education eligibility, ever free and reduced lunch, ever homeless and ever migrant. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 10: Interrupted Panel Student Fixed Effects Model Estimates of the Effect of Virtual School Attendance on Test Score Grades 4-8.

	ELA		Math		Science		Social Studies	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Virtual	-0.139 (0.004)	-0.079 (0.003)	-0.345 (0.004)	-0.197 (0.004)	-0.299 (0.004)	-0.143 (0.004)	-0.426 (0.004)	-0.186 (0.004)
ELA 2 Year Avg. Gain		0.961 (0.001)						
Math 2 Year Avg. Gain				-0.928 (0.001)				
Science 2 Year Avg. Gain						0.966 (0.001)		
Social Std. 2 Year Avg. Gain d								-0.963 (0.001)
Observations	8523317	4590145	8488905	4537333	7521251	3678212	7042552	3252875

Notes: Standard errors in parentheses. The year before students enter a virtual school is dropped. The lagged score is an average gain over two years, specifically it is the norm score in year t minus norm score in year t-2 divided by 2. Virtual school is defined as 1 if student attended a virtual school that year. Demographics include race, Hispanic, sex, special education eligibility, ever free and reduced lunch, ever homeless and ever migrant. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 11: Student Fixed Effects Model Estimates of the Effect of Virtual School Attendance on Test Score Grades 4-8.

	ELA	Mathematics	Science	Social Studies
One Year and Exit	-0.167	-0.367	-0.332	-0.444
	(0.007)	(0.008)	(0.009)	(0.009)
Two Plus Never Exit	-0.079	-0.277	-0.213	-0.364
	(0.005)	(0.006)	(0.005)	(0.007)
Observations	48783	48444	42002	40379
Observations	62163	61772	54821	51628

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 12: Sub-sample Analysis of Student Fixed Effects Model Estimates of the Effect of Virtual School Attendance on Test Score Grades 4-8.

	ELA	Mathematics	Science	Social Studies
Female	-0.087	-0.305	-0.278	-0.405
	(0.005)	(0.005)	(0.005)	(0.005)
Male	-0.150	-0.324	-0.258	-0.393
	(0.005)	(0.005)	(0.006)	(0.006)
Ever FRL	-0.121	-0.319	-0.273	-0.402
	(0.003)	(0.003)	(0.005)	(0.004)
Non-White	-0.095	-0.261	-0.223	-0.356
	(0.005)	(0.005)	(0.006)	(0.006)
Observations	4200434	4184935	3700684	3467688
Observations	4342950	4323818	3839160	3592236
Observations	6114599	6091236	5395140	5060702
Observations	4310336	4294181	3815159	3572701

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Prior year test score is not included. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 13: Student Fixed Effects Model Estimates of the Effect of Virtual School Attendance on Test Score Grades 4-8 .for Students who Enter a Full-time Virtual School in a Atypical Grade

	(1)	(2)	(3)	(4)
	ELA	Math	Science	Social Studies
Virtual	-0.093	-0.260	-0.222	-0.441
	(0.007)	(0.008)	(0.008)	(0.008)
Atypical Grade * Virtual	-0.033	-0.072	-0.060	0.056
	(0.008)	(0.009)	(0.009)	(0.009)
Constant	0.005	0.017	0.002	0.007
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	8544363	8509731	7540775	7060835

Notes: Standard errors in parentheses. Prior year test score is not included. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 14: Sub-sample Analysis of Student Fixed Effects Model Estimates of the Effect of Virtual School Attendance on Test Score Grades 4-8 Limited Sample to Students Who Have Ever previously Been Home-schooled

	(1)	(2)	(3)	(4)
	ELA	Math	Science	Social Studies
Virtual	-0.124	-0.344	-0.297	-0.424
	(0.006)	(0.007)	(0.008)	(0.008)
Observations	178098	176803	154936	145465

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Sample is limited to student who had ever previously been in home school setting. Prior year test score is not included. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 15: Student Fixed Effects Model Estimates of the Effect of Virtual School Attendance on Test Score Elementary Grades 4-5.

	ELA		Math		Science		Social Studies	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Virtual	-0.155 (0.010)	-0.156 (0.009)	-0.328 (0.012)	-0.323 (0.011)	-0.312 (0.012)	-0.305 (0.011)	-0.321 (0.012)	-0.317 (0.011)
ELA Lagged		-0.441 (0.001)						
Math Lagged				-0.383 (0.001)				
Science Lagged						-0.410 (0.001)		
Social Std. Lagged								-0.379 (0.000)
Observations	2518540	2166281	2512907	2143836	2538624	2173490	2529964	2164159

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 16: Student Fixed Effects Model Estimates of the Effect of Virtual School Attendance on Test Score Middle Grades 6-8.

	ELA		Math		Science		Social Studies	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Virtual	-0.050	-0.0594	-0.230	-0.244	-0.223	-0.232	-0.361	-0.359
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
ELA Lagged		-0.273						
		(0.001)						
Math Lagged				-0.231				
				(0.001)				
Science Lagged						-0.268		
						(0.001)		
Social Std. Lagged								-0.254
								(0.000)
Observations	3717074	3200862	3684888	3146992	3707999	3187422	3241132	2411426

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 17: Cell Analysis Model Estimates of the Effect of Virtual School Attendance on Test Score Grades 5-8.

	ELA	Mathematics	Science	Social Studies
Virtual	-0.023	-0.183	-0.129	-0.238
	(0.005)	(0.006)	(0.006)	(0.007)
ELA Lagged	0.759			
	(0.0001)			
Math Lagged		0.698		
		(0.001)		
Science Lagged			0.714	
			(0.001)	
Social Studies				0.716
				(0.001)
Year FE	✓	✓	✓	✓
Grade FE	✓	✓	✓	✓
Observations	6176875	6095220	5237106	4470491

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Demographics include race, Hispanic, sex, special education eligibility, ever free and reduced lunch, ever homeless and ever migrant. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 18: Probit Model Estimates of the Effect of Virtual School Attendance on Graduation– Conditional on Being In High School at least Four Years.

	Graduation	
Ever Virtual	-0.104 (0.002)	
Number Years Virtual		-0.026 (0.001)
Demographics	✓	✓
Year FE	✓	✓
Grade FE	✓	✓
Observations	611854	611854

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Demographics include race, Hispanic, sex, special education eligibility, ever free and reduced lunch, ever homeless and ever migrant. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.

Table 19: Probit Model Estimates of the Effect of Virtual School Attendance on Daily Attendance

	Daily Attendance		
Ever Virtual	-0.000 (0.0000)		
Total Number Years Virtual		-0.000 (0.000)	
Number Years Virtual by Year t			-0.000 (0.000)
Lagged Percent Present	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Demographics	✓	✓	✓
Year FE	✓	✓	✓
Grade FE	✓	✓	✓
Observations	13956517	13956517	13956517

Notes: Standard errors in parentheses. Virtual school is defined as 1 if student attended a virtual school that year. Demographics include race, Hispanic, sex, special education eligibility, ever free and reduced lunch, ever homeless and ever migrant. Science and social studies samples are smaller because the state stopped giving theses exams in all grades.