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Virtual Illusion: Comparing Student Achievement and Teacher Characteristics in Online and Brick-and-Mortar Charter Schools*

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Abstract

As researchers continue to examine the growing number of charter schools in the U.S., they have focused attention on the significant heterogeneity of charter effects on student achievement. Our paper contributes to this agenda by examining the achievement effects of virtual charter schools vis-à-vis brick-and-mortar charters and traditional public schools and whether characteristics of teachers and classrooms explain the observed impacts. We found that students who switched to virtual charter schools experienced large, negative effects on mathematics and English/language arts achievement that persisted over time, and these effects could not be explained by observed teacher or classroom characteristics.

Keywords: charter schools; virtual schools; CMOs; EMOs; school effects; quasi-experimental methods

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Virtual Illusion: Comparing Student Achievement and Teacher and Classroom Characteristics in Online and Brick-and-Mortar Charter Schools

Introduction

The proliferation of charter schools has become one of the central features of contemporary education reform. As charter school options have expanded, researchers have focused on comparing outcomes between students attending charter schools and those in traditional public schools. Although researchers have started to concentrate on heterogeneity of learning opportunities across sectors by considering magnet and private schools alongside charter and traditional public schools (Berends & Waddington, 2018), they have paid less attention to differences within the charter school sector. Given the steady expansion of charter school enrollment across multiple types of operators (e.g., management organizations, independent schools), policymakers are looking for evidence of whether some types of charter school operators produce notably different results than others.

Depending on a state's law, different types of organizations may operate charter schools, including for-profit educational management organizations (EMOs), non-profit charter management organizations (CMOs), public school districts, and independently operated schools. In addition, these management organizations may operate traditional "brick-and-mortar" schools, virtual schools (i.e., e-schools), or blended versions of both. By design, then, the charter school sector has produced a wide variety of organizational forms that utilize a range of instructional and managerial strategies to impact student outcomes. Given this variation in operation, it is perhaps not surprising that researchers are beginning to identify heterogeneous effects across different operators. Most notably, virtually-operated EMOs have been found to have strong, negative effects on students who switch to these schools from traditional public schools (e.g., Ahn & McEachin, 2017; CREDO, 2015; Zimmer, Gill, Booker, Lavertu, & Witte, 2009). These negative outcomes have been found across multiple states with varying charter school laws regulating the authorization of these schools.

Despite the consistently negative effects of virtual charter schools on student achievement, we know very little about how these schools compare to other charter operators serving students in similar contexts. In addition, there is limited research on the extent to which characteristics of virtual charter school teachers are associated with student achievement. In the present study, we address the following research questions:

- What is the impact over time of switching from a traditional public school into a virtual charter school on mathematics and English/Language Arts achievement in Indiana? How do these impacts compare to those associated with switching into brick-and-mortar charter schools in the state?
- What proportion of the variation in virtual charter school students' achievement is concentrated at the teacher- and school-levels? How does this compare to traditional public schools and other types of charter schools in Indiana?
- What are the characteristics of teachers in virtual charter schools compared to traditional public schools and brick-and-mortar charter schools in Indiana? Do these characteristics explain the impacts of virtual charter schools on student achievement?

To address these questions, we analyze longitudinal administrative records from the state of Indiana. These data present a unique opportunity in that Indiana authorizes multiple types of charter school operators that each participate in the same state assessments as the traditional public schools.

Background and Literature Review

State laws that authorize the operation of charter schools vary widely in the procedures for authorization, operation, and expansion (among other things). In general, charter schools in the United States are operated by independent entities, EMOs, and CMOs. Both EMOs and CMOs function as autonomous school districts that are not constrained by traditional district boundaries (Berends, 2015). The primary difference between EMOs and CMOs is that the former operate as for-profit entities while the latter are non-profit. Most virtual charter schools in the United States operate under EMOs (Miron & Elgeberi, 2019).

Indiana began authorizing charter schools in 2002. State law permits authorization through multiple bodies: (1) state and other non-profit educational institutions that offer a four-year baccalaureate degree; (2) executive of a consolidated city (e.g., mayor of Indianapolis); and (3) the Indiana Charter School Board.[1] Further, state law permits three qualitatively distinct entities to operate charter schools: EMOs, CMOs, and independent operators. In addition, EMOs in Indiana operate both virtual (i.e., entirely online or hybrid)[2] and non-virtual (i.e., brick-and-mortar) charter schools.

For this paper, we grouped together all virtual charter schools and distinguish these schools from brick-and-mortar (B&M) charter schools. We did not distinguish between brick-and-mortar school operators. Enrollments in virtual charter schools has expanded rapidly since the first virtual charter opened in 2010. As of the 2016-17 school year, Indiana had 93 charter schools serving 43,135 K-12 students across the state. Among these were four virtual charter schools, enrolling 10,984 K-12 students—nearly one-fourth of all charter school students statewide.

Virtual Charter Schools and Student Outcomes

Previous research has demonstrated that charter schools produce heterogeneous outcomes relative to traditional public schools (for recent reviews, see Berends, 2015; Betts & Tang, 2019; Epple, Romano, & Zimmer, 2015; Ferrare, 2020). That is, on average, some charter schools are more effective than traditional public schools at improving student test scores and degree attainment, others are less effective, and many produce null effects. This has prompted researchers to consider which factors contribute to these varied outcomes. For instance, some scholars have begun to explore whether or not differences in authorizing organizations can impact student outcomes (Berends & Waddington, 2019; Carlson, Lavery, & Witte, 2012; Zimmer, Gill, Attridge, & Obenauf, 2014). Another emerging area of research focuses on the variation between different types of operators (CMOs, EMOs, etc.); the current study extends this field of research by focusing on virtual charter schools as a distinct source of heterogeneity within the charter sector.

Virtual schooling is not unique to the charter sector, but rather is situated within a broader space of full-time virtual and blended schools that are utilized by traditional public schools, homeschoolers, and charter schools. Traditional public schools, for instance, have long made use of online education to offer courses. Prior research in this area suggests that students enrolled in these courses experience similar outcomes as those in classroom-based environments (for an early meta-analysis, see Cavanaugh et al., 2004). In a meta-analysis conducted by researchers at SRI International, students taking courses in blended online settings performed better than those in traditional face-to-face settings, but the differences between those in purely online courses and face-to-face were null (Means, Toyama, Murphy, & Bakia, 2013).

Although there is a robust literature focusing on online course-taking, there is far less work that has sought to evaluate full-time and blended virtual schools. A research brief published by the National Education Policy Center (NEPC) details the national trends in virtual school enrollment (Miron & Elgeberi, 2019; see also Gill et al., 2015). According to the report, virtual schools tend to enroll students who are disproportionately white and less likely to qualify for free and reduced-price lunch (FRPL) compared to national averages. Approximately 79% of the estimated 433,000 students (300K full-time virtual and 133K blended) enrolled in virtual or blended schools are enrolled in schools operated by charters. Virtual schools operated within the charter sector are much more likely to be run by for-profit EMOs than virtual schools operated within public school districts, and also tend to have substantially larger enrollments (including teacher to student ratios) (Miron & Elgeberi, 2019).

Research focusing on virtual charter schools has increased in the past few years, but the types of evidence used to evaluate virtual charter schools varies considerably with the availability of data and estimation strategies. Numerous reports and research briefs have been disseminated, including annual reports by NEPC researchers since 2013. These briefs provide rich, descriptive accounts of the rapidly changing virtual schooling sector, but they offer limited information about how virtual charter schools impact student outcomes over time. Miron and Elgeberi (2019), for instance, used data from school report cards (math and ELA scores, graduation rates, and achievement gaps) to estimate the proportion of virtual charter schools (among other types of virtual schools) with academically acceptable and unacceptable ratings. They found that, in 2017/2018, 59.2% of the virtual charters in their sample had academically unacceptable ratings. While informative, it remains unclear if the unacceptable performance is an effect of the schools' practices or the selection process by which students elect into virtual charters.

In the absence of an experimental design, researchers attempting to estimate the impacts of virtual charter schools need to use models that rely on assumptions about the processes by which students select into them. For example, the Center for Research on Education Outcomes (CREDO, 2015) attempted to estimate such impacts among virtual charter schools across 18 states by using a strategy that matches students on observable characteristics, such as demographics, participation in specialized programs (e.g., FRPL), and a baseline test score. This approach has been critiqued for the assumption that students' observable characteristics serve as a sufficient control for selection into charter schools (see, e.g., Davis & Raymond, 2012). Using this approach to compare students in virtual charters to those in traditional public schools, CREDO researchers found that virtual students had annual achievement losses of -0.25 standard deviations (SD) in math and -0.10 SD in reading. The largest losses in mathematics occurred in Florida (-0.46), Texas (-0.39), Louisiana (-0.34), and California (-0.33). Some of the largest losses in reading occurred in Louisiana (-0.28), Florida (-0.19), Texas (-0.18), and Nevada (-0.17). More recent state-level follow-ups conducted by CREDO in Pennsylvania (CREDO, 2019a), Idaho (CREDO, 2019b), and Ohio (CREDO, 2019c) also found negative impacts for students when compared to their counterparts in traditional public schools.

CREDO's findings are consistent with what other studies have found in Ohio. In a study conducted by RAND, Zimmer et al. (2009) used a student fixed effects model to estimate changes in achievement growth among the subset of students who switched between traditional public and virtual charter schools. This widely used approach allows researchers to control for any unobservable time-invariant characteristics, but has been criticized due to the strong assumption that pre-treatment (i.e., prior to switching) achievement growth is a good predictor of future achievement growth (Hoxby & Murarka, 2008). Zimmer and colleagues found the impact estimate for Ohio's virtual charter schools was -0.44 SDs in mathematics and -0.25 SDs in reading. In a more recent study of virtual charter schools in Ohio, Ahn & McEachin (2017) found similar results even when disaggregating by achievement levels. In elementary and middle school math, for example, the losses ranged from -0.41 (SDs) for low achievers (i.e., those in the first tercile) to -0.30 for high achievers (third tercile). In reading, the effects ranged from -0.26 to -0.10, respectively. The study also found that virtual charter school students were less likely than traditional public students and those in other charter schools to pass Ohio's graduation exams.

Unlike most areas of the literature on student outcomes in charter schools, the research on virtual school student outcomes presents a consistent picture. Namely, students who switch into these schools experience negative impacts on their state-mandated standardized test performance. Despite the consistency of these results, we still know very little about why students experience these negative outcomes. The NEPC brief by Miron & Elgeberi (2019) suggests that virtual charter schools nation-wide have larger student-to-teacher ratios than traditional public schools, and that virtual charters run by EMOs have substantially larger enrollments than those run by CMOs. The extent to which characteristics of virtual charter school teachers explain variation in student achievement is essentially unknown. The results reported below seek to contribute to this gap in the literature by closely examining virtual charter schools run by EMOs in relation to brick-and-mortar charter schools operated in the state of Indiana.

Data and Measures

We used seven years (2010-11 school year through 2016-17) of longitudinal, student demographic and test score records from the Indiana Department of Education (IDOE). The records contain information about students in grades 3-8 attending traditional public, charter, and private schools that participate in the annual English/language arts (ELA) and mathematics assessments of the Indiana Statewide Testing for Educational Progress Plus (ISTEP+) program. The ISTEP+ is the state-mandated test for Indiana students in grades 3-8 and is aligned to the Indiana Academic Standards. The ISTEP+ tests students each spring in ELA and math.[4]

Our primary outcomes of interest are students' annual ELA and math ISTEP+ test score levels. We standardized each student's scores by the subject/grade/year mean and standard deviation of all test-taking students statewide. The standardized measures allow us to draw comparisons, in standard deviation (SD) units, between individuals in different types of charter and traditional public schools. We focus on these outcomes in grades 5-8, as grades 3 and 4 can only be used as pre-baseline and baseline years, as we describe in the next section.

We used several student-level demographic and academic background characteristics reported in the annual IDOE data. These characteristics include each student's sex, race/ethnicity, receipt of free or reduced-price lunch, grade level, English proficiency status, and special education status. We recoded each of these variables into binary indicators. We also constructed binary indicators for whether a student received an in-school or out-of-school suspension in a given year, as well as whether they were expelled.

The longitudinal records also contain information about students' school of record, including the school name and National Center for Education Statistics (NCES) unique identification number. Using the NCES ID, we linked the schools to the Common Core of Data

(CCD), and in addition to our own data collection, we created two binary indicators of charter school type, whether a virtual charter school or brick-and-mortar charter school.

In addition to the student-level data, we also used annual teacher/classroom-level data provided by the Indiana Department of Education for a mediation and moderation analysis of the main charter effects. These data were collected beginning in the 2010-11 school year and span through the 2016-17 school year, the same as our student-level panel. With these data, we were able to link 93% of students across all years to their math and ELA teacher of record in any given year.[5] Once linked, we incorporated several measures of teacher background and classroom characteristics into our analysis, including: number of years of teaching experience, class size, and dichotomous indicators for a teacher's sex, completion of a masters' degree, prior year employment (current school, another K-12 school, not in K-12), certification status (certified vs. uncertified) and whether a class was solo taught (vs. co-taught or by a computer program—these reference categories were combined due to small sample sizes).

Sample and Estimation Strategy

Our goal in this paper is estimate impacts of attending Indiana charter schools on student achievement. This is challenging, given that none of the virtual charter schools and very few brick-and-mortar charter schools were oversubscribed, and therefore did not implement enrollment lotteries. Thus, we cannot leverage a natural experiment as to estimate charter school effects in the same manner as many previous charter school studies (e.g., Abdulkadiroglu et al, 2011; Dobbie & Fryer, 2011; Angrist et al., 2012; Angrist et al., 2013; Clark et al., 2015).

In order to mitigate selection bias in our estimates of charter school effects, we used a nonexperimental matching approach that draws upon important lessons from the quasi-experimental design literature (Cook, Shadish, & Wong, 2008), within-study comparisons that use nonexperimental approaches to replicate the experimental estimates of school choice evaluations (Angrist, Pathak, & Walters, 2013; Bifulco, 2012; Dobbie & Fryer, 2013; Fortson et al., 2014), and the implementation of those lessons in the nonexperimental evaluation of charter schools (Dobbie & Fryer, 2017) and private school vouchers (Waddington & Berends, 2018). Specifically, we matched students who switch from public to charter schools on a finite set of student-level criteria with their public school peers from the same baseline cohort (school, grade, year, race/ethnicity, sex, and eligibility for free-or-reduced price lunch). Then we estimated the impact of attending a charter school by accounting for residual differences in prior achievement and other baseline characteristics as well as netting out unobserved differences between matched cells of students. We detail our process below.

Sample Construction and Description

We implemented several data restrictions prior to constructing our analytical sample, including requiring each student to have at least three years of test scores (including two pre-treatment years) (see Appendix A). We focused our analysis on a treatment sample of students moving from traditional public schools into virtual or brick-and-mortar charter schools serving students in grades 3-8 across the state of Indiana. Nearly two-thirds of all students who moved between public and charter schools first transitioned from a traditional public to a charter school.[6] From this group, we can more easily construct a public school comparison sample for this group of charter school switchers, as we describe below.

One of the important takeaways from the literature from which we drew our empirical strategy (see Bifulco, 2012; Cook, Shadish, & Wong, 2008; Fortson et al., 2014) is that treatment and comparison groups should be constructed from the same geographic location prior to receiving treatment (i.e., the same public school). Therefore, we constrained our public school comparison sample to include only public school students with the same grade, year, and school ("cohort") as a student who left the public school to attend a charter school the following year. This also establishes a baseline year from which we can draw posttreatment comparisons between charter and public school students after accounting for various pre-treatment factors that may have driven selection into a charter school.

In addition to constraining our comparison sample of public school students to include only those in the same baseline public school cohort as charter school switchers, we also exactly matched charter and public school students on a number of pre-treatment characteristics. These characteristics included a students' sex, race/ethnicity, and free-or-reduced price lunch status in the baseline year. After exact matching, we then matched charter and public students within a caliper of +/- 0.20 SD of their math baseline test scores (for the math achievement analysis) or ELA baseline test scores (for the ELA achievement analysis).[7] We chose to use caliper matching on test scores because it is highly unlikely we would find a public school student to match to a charter school student with the same demographic characteristics and the exact same test score. We net out any small remaining differences in our analytical model by controlling for baseline (and pre-baseline) achievement. Collectively, we refer to the matching of charter and public students within each baseline cohort by sex, race/ethnicity, baseline FRPL status, and baseline achievement as a "matching cell."

We believe that these characteristics further help to explain why a given student may select into a virtual or brick and mortar charter school. As we previously stated, there is no random assignment mechanism for the selection of students into these schools. There are myriad reasons why a given student may opt for a certain charter school, including proximity to a charter school, perceived school quality (of either public schools or charter schools), and a host of other factors. Of particular interest are the charter school options available to students and families, whether due to ease of enrollment, physical proximity, or available transportation (especially bussing) options. Matching on cohort and pre-treatment school is the best available way to compare students with similar alternatives to their current school. For example, in rural settings, students may be more likely to opt for a virtual charter school rather than a brick-and-mortar charter school simply because it is the only viable and cost-free alternative to their local traditional public school. Or as another example, an African American student from an urban public school may be more likely to opt for a specific brick-and-mortar charter school located within their neighborhood because of the values expressed by that school's leadership. We also match by certain background characteristics as specific types of students from within a given cohort may be more (or less) likely to switch to a specific charter school based on these unobserved factors. By comparing students who share nearly identical baseline characteristics whereby one switches to a charter school and another remains in a public school, we believe we have constructed an appropriate counterfactual for each individual student.

Our analysis takes place "within" each matching cell by incorporating matching cell fixed effects to our preferred model. Thus, we are individually comparing each charter school switcher to their same sex-race/ethnicity-FRPL status peers from the same baseline public school cohort, with similar test scores. The fixed effects help to net out differences between cohorts in order to account for unobserved differences in selection into treatment and subsequent outcomes. Also, each treated student is being compared with their similar public school peers (the counterfactual) before aggregating these effects by charter school type (either brick-and-mortar or virtual). Because we are matching on several fine-grained characteristics, it is highly unlikely that two students who experience different treatments end up in the same matching cell, thereby reducing concern about our estimates of multiple treatments being biased.

We opted for this approach of matching charter and public school students instead of propensity score matching (Rosenbaum & Rubin, 1983) for several reasons. First, although we chose a more limited set of criteria, matching directly on a limited set of variables is more precise. Second, we avoid issues associated with the propagation of errors generated from the imprecision of the matching process needing to be carried forward to the estimation of charter school effects (see Abadie & Imbens, 2006). Third, we believe the finite set of matching criteria are reasonable for explaining selection into a charter school, with further unobserved differences explained by our chosen matching characteristics netted out by the inclusion of fixed effects in our preferred model. Fourth, we believe this approach has greater empirical basis to reasonably approximate experimental estimates as described by Angrist, Pathak, & Walters (2013) and Dobbie & Fryer (2013).

To finalize our sample for analysis, we only included charter students who have a public school peer in their baseline cohort matched along the other baseline dimensions described above. Similarly, we only included public school students that share these same characteristics at baseline and have a peer who transfers to a charter school in the subsequent year. Charter school students can be matched to multiple public school students within their cohort. The matching was performed without replacement, so in the very small number of cases where two treated students occupy the same cohort, in the same school, and are similar in all other matching characteristics at baseline, control students were not matched to multiple treatment students. There were three pairs of treatment cases that were exactly the same on all variables upon which we exactly matched, and whose pretreatment scores were within 0.2 SD of one another. Because matching was done without replacement, one of each pair of cases was dropped at random. Our analytical sample includes 1,963 students in four virtual charter schools and 2,222 students in 67 brick-and-mortar charter schools who could be matched to at least one of 25,713 students in 931 traditional public schools. This represents a match rate of 81% of eligible charter school students with teacher data on record who switch from a public school and represents students drawn from 46% of all traditional public schools serving students in grades 3-8 statewide. For each student, we have achievement data from at least three years in at least one subject: pre-baseline, baseline, and at least one year post-baseline. As our approach requires at least three successive years of data, our outcomes are constrained to when students are enrolled in grades 5-8, given that grades 3 and 4 can only serve as pre-baseline and baseline years.

We compare the matched vs. unmatched charter and public school students in Appendix Table A1. We can only confidently generalize our findings to students entering charter schools from traditional public schools as there are few noticeable differences between our analytical sample of students and those who consistently attend charter schools or that were not part of our matched analytical sample due to exclusion restrictions. Our analytical sample slightly over-represents virtual charter students who are white, have higher achievement scores, and are more advantaged. We anticipate this would positively bias any impacts of attending a virtual charter school. As we describe in our results, we found a profoundly negative effect for attending a virtual charter school despite this overrepresentation. Less advantaged students are slightly overrepresented in the B&M charter student sample, so we anticipate any findings would be minimally downward biased.

Estimation Strategy

Although we have matched students who switched from public to virtual and brick-and-mortar charter schools with their peers in their prior public school, there are still differences between the two groups at baseline. We have only matched students on baseline achievement within a certain caliper; thus, we must net out any small remaining differences in baseline achievement. Further, we only matched on baseline achievement levels; there may be divergent pre-treatment *trends* in achievement between matched charter and public school students. In addition, student baseline special education status, English learner status, and experience with exclusionary discipline were excluded from the matching procedure, as each additional parameter reduced the matching rate. To correct for this, our modeling strategy conditions on those pre-treatment differences between students not already netted out of our estimate due to the matching procedure described above.

Main Effect Estimates

Our preferred model is an OLS regression with several covariates. We estimated this model for each individual year posttreatment, resulting in a total of three individual models to estimate the impacts of virtual and brick-and-mortar charter schools on student achievement in the first, second, and third year after switching to a charter school. In equation (1) below, t is fixed at 1, 2, or 3 for each year of estimation, except where otherwise specified. We also estimated different effects for each subject (math and ELA) as the outcome in separate models, though the structure of the equation remained the same. We display the model for the first-year estimates in equation (1) below.

$$Y_{icgt} = \alpha + \beta_1 Virtual_{icgt} + \beta_2 B\&M_{icgt} + \pi Y_{icg(t=0)} + \omega Y_{icg(t=-1)} + \delta \mathbf{X}_{icg(t=0)} + \theta_g + \tau_c + \upsilon_{icgt} \quad (1)$$

Here, the achievement level (Y) for each student (i) in matching cell (c), grade (g), and post-baseline year (t) is a function attending a virtual ($Virtual_{icgt}$) or brick-and-mortar ($B\&M_{icgt}$) charter school as well as a host of other covariates.

In this model, we controlled for a vector of student baseline academic characteristics ($\mathbf{X}_{icg(t=0)}$) including baseline classification as an English language learner and special education student and indicators for where these measures were missing. We also controlled for an indicator of whether a student was suspended in the baseline year, either in- or out-of-school, and whether the student was expelled at baseline. Grade fixed effects (θ_g) account for systematic differences in exams across grade levels. Matching cell fixed effects (τ_c) account for unobserved differences between match cells at baseline. These also account for systematic differences in exams across years as students within each cell take exams always within the same calendar year posttreatment. The term υ_{icgt} represents cluster-robust standard errors to account for correlation among students within the same baseline public school cohort, as higher rates of selection into charter schools may be correlated with a student's baseline school quality. This mirrors the preferred clustering approach used by Dobbie & Fryer (2017) and draws upon recent econometric literature suggesting standard errors should be clustered at the level which there may be correlation between subjects in the assignment to treatment (Abadie, Athey, Imbens, & Wooldridge, 2017).

Our preferred model also includes two measures of a student's prior achievement in the same subject as the outcome, one at baseline ($Y_{icg(t=0)}$) and one pre-baseline ($Y_{icg(t=-1)}$). Because lagged achievement scores are endogenous in the post-baseline years, these controls remain as the baseline and pre-baseline achievement measures for our estimates in the second and third years post-baseline. In Appendix B, we detailed three alternative model specifications regarding the inclusion of a student's prior achievement in addition to a host of other robustness checks of our main results.

After accounting for these pretreatment achievement differences between charter and public school students, we describe the charter school impacts as the value-added achievement gains (or losses) from baseline. Thus, we define our main estimates as the difference in the achievement gain (or loss) from baseline in a given post-baseline year between virtual charter school (β_1) or brick-and-mortar charter school (β_2) and public school comparison students within each matching cell. This estimate will be minimally biased if we have accounted for all covariates that could explain differences between the two groups.

By incorporating both baseline and pre-baseline achievement, we mitigate concerns regarding differing pretreatment trends between charter school and public students. This pretreatment phenomenon, known in the job-training literature as "Ashenfelter's Dip" (Ashenfelter, 1978), suggests that a substantial drop in student performance may be a signal to parents to have their child change schools. If this were the case, some students may be more likely to switch to a charter school than others, yielding biased estimates. By accounting for multiple years of pretreatment achievement in our models, we negate concerns with pretreatment trend differences between charter school students and their matched public school peers.

Because we disaggregated results by posttreatment year, we estimated two additional models, one with baseline achievement as the outcome and another with pre-baseline achievement as the outcome. These models contained the charter school indicator as well as the baseline student characteristics, matching cell fixed effects, grade fixed effects (for the pre-baseline model only; these are collinear in the baseline model with the matching cells) and cluster-robust standard errors. The estimate on the charter school indicators show the residual baseline and pre-baseline achievement level differences between charter school and public students within each matching cell and indicated level, insignificant differences between both virtual and brick-and-mortar charter school students and their public school peers.

Given the insignificant pretreatment differences between matched charter and public school students, our matching approach combined with covariate adjustment should produce internally valid estimates of the effect of switching to a charter school in Indiana to the degree that the observable factors contained in our approach accounts for selection on unobservable factors. We tested the robustness of our main effect estimates through a series of alternative model and sample specifications, all described in Appendix B. Our approach has improved external validity over studies that rely upon enrollment lotteries as we estimate effects in all charter schools across the state as opposed to only oversubscribed schools.

Variance Decomposition

The above analysis demonstrates that, on average, students attending virtual charter schools fare far worse than they would have had they remained in traditional public schools. That said, this finding should not be viewed as consistent for all students. Examining variation in student performance across teachers and schools within each sector can shed light on how consistently students follow the average trends. To do this, we employ unconditional multilevel models that identify how much variation in student achievement can be explained by the clustering of students within teachers and schools. Put another way, variance decompositions describe how much of the variation in student performance can be attributed to differences between teachers within schools and differences between schools, without constraining the description to measured variables (Jenkins & DiPrete, 2010).

We perform this analysis twice, one with unconditional hierarchical linear models, and once while controlling for pre-treatment test scores. The former makes no effort to net out selection into schools and teachers, while the second nets out some, but not all, of this selection process. We present the resulting intra-class correlations (ICCs), which describe the within cluster variance in student achievement as a proportion of the overall variance. Although instructive, ICCs describe variation across teachers and schools that cannot be explained using measured variables, and thus should be viewed as a purely descriptive analysis.

Mediation Analyses with Teacher and Classroom Characteristics

Virtual schooling represents an entirely unique learning environment, where daily interactions with teachers have little in common with that of a B&M school, charter or otherwise. Nevertheless, there are key differences between the virtual charter and traditional public teacher workforces, and especially the classroom circumstances experienced by students. Similar to other charter schools, the teachers working in virtual public schools are less experienced than those working in traditional public schools (9.5 years in virtual charters vs. 12.9 years in traditional public) and are less likely to have a master's degree (0.325 vs. 0.486). Most strikingly, and unlike in B&M charter schools, virtual schools have an average classroom size over four times that of comparable traditional public schools (100.6 students per classroom in virtual charters vs. 23.9). We performed a mediation analysis for the main results listed above in order to identify whether these differences explain some or all of the negative effect of attending a virtual charter school.

We measured mediation by incorporating several teacher and classroom characteristics in our preferred model. The mediation analysis is displayed in model (2) below.

$$Y_{icgt} = \alpha + \beta_1 Virtual_{icgt} + \beta_2 B\&M_{icgt} + \sigma \mathbf{T}_{icgt} + \pi Y_{icg(t=0)} + \omega Y_{icg(t=-1)} + \delta \mathbf{X}_{icg(t=0)} + \theta_g + \tau_c + \upsilon_{icgt} \quad (2)$$

This is an extension of model (1) where we now include a vector of teacher and classroom characteristics ($\mathbf{T}_{icgt}$) as additional covariates in the model. Collectively, the teacher and classroom covariates help to explain observable variation at the teacher/classroom-level that may explain virtual or brick-and-mortar charter school student outcomes. We specifically look for substantial changes in the magnitude and statistical significance of the main effect estimates (β_1 & β_2) to assess mediation.

We also tested for the moderating impacts of these teacher and classroom characteristics on the main effects of attending a charter school by including interactions between our main effects and each teacher characteristic. We theorized that the virtual nature of teacher-student interactions could dampen the relationship between teaching and schooling characteristics and student achievement. For instance, it is possible that the virtual setting prevents teachers from using their experience, and resulting skill, to improve instruction. These results were inconclusive; however, we have provided additional detail on our approach and describe our findings in Appendix C.

Results

Student and Teacher Mean Characteristics

Table 1 provides a general overview of the characteristics of students in each of the analytic samples (i.e., math and ELA). Students who transfer into virtual charter schools tend to come from more privileged backgrounds than those who switch into brick and mortar charter schools. White students make up 0.869 of students in virtual charter schools, compared to only 0.316 of students in brick and mortar charters. In addition, 0.545 virtual charter students qualify for FRPL, compared to 0.762 brick and mortar charters. Compared to the state population, which is 0.699 white and where 0.473 of students qualify for FRPL, the virtual school population is more likely to be white but also more likely to qualify for FRPL.

Relative to the state average of all ISTEP+ test takers (which, within a given grade level and year, is set to zero), students who switch into virtual charter schools have slightly lower baseline math and ELA scores on the magnitude of -0.151 and -0.059 standard deviations (SD), respectively. Students switching into brick-and-mortar charters do so with far lower baseline scores in math and ELA (-0.355 and -0.418 SD, respectively). These differences are nearly eliminated in the two corresponding control groups created by our matching routine. In both cases, the mean pre-treatment scores of the control groups are slightly closer to zero, the overall mean, than the treatment groups. This is the result of the normal distribution of test scores: within each student's 0.2 SD caliper, we would expect there to be more potential matches near the center of the distribution, as there are simply more cases in that direction. As both treatment groups have lower than average scores, the control groups have slightly higher average scores. These differences never exceed 0.06 SD; in addition, the inclusion of two years of pretreatment scores net out any remaining differences in pretreatment performance from the estimates presented below. Posttreatment, virtual charter students' scores decrease to -0.530 SD in math and -0.345 SD in ELA; brick-and-mortar charter students experience a less profound decrease to -0.487 SD in math and increase to -0.373 SD in ELA.

Table 1 also includes descriptive statistics of the teachers in each of the school types. Teachers in virtual and brick-and-mortar charter schools are less likely to have a master's degree and tend to have fewer years of experience than those in the traditional public school (TPS) sample. The most striking difference, however, is in regard to class size. On average, teachers in virtual charter schools tend to have 101 students, which is substantially higher than their counterparts in TPS (24) and treatment students in brick-and-mortar charters (22).

<Table 1 about here>

Main Effects of Virtual Charter School Attendance on Student Achievement

When compared to their matched peers in traditional public schools (TPS), on average, students who switched from a TPS into one of Indiana’s virtual charter schools experienced large, negative impacts on their student achievement scores. These changes are described in Table 2 and illustrated in Figure 1. Given that our analysis makes two distinct comparisons, the effect of attending either a virtual or B&M charter school compared to attending a traditional public school, we apply a Bonferroni correction to all tests of significance. As a result, all critical value thresholds are halved. In math, virtual switchers saw an average drop in their test scores of -0.414 SD during the first year after switching, and the impacts remained negative on average through Year 2 (-0.481 SD) and Year 3 (-0.500 SD). By comparison, students who switched into one of Indiana’s brick and mortar (B&M) charters experienced small to moderate decreases to their math test scores in Year 1 (-0.068 SD), but by Year 2 the differences were not significantly different from zero and remained null in Year 3.

<Table 2 about here>

<Figure 1 about here>

The results in English Language Arts (ELA) reflected a similar trend over time. Students who switched into a virtual charter school experienced an initial drop in ELA scores of -0.286 SD, on average, and these impacts remained negative through Year 2 (-0.264 SD) and Year 3 (-0.334 SD). Although the magnitudes of these impacts were less than those observed in math, they still reflect large decreases within the broader charter school effects literature described above. Meanwhile, students who switched into a brick-and-mortar charter school experienced no significant changes to their ELA scores in any of the observed years.

Overall, then, virtual charter schools in Indiana appear to have large, negative impacts to student achievement in math and ELA that are sustained across time, while brick-and-mortar charters generally have small to null effects. The following sections seek to unpack some of these differences by decomposing the variance between student, teacher, and school levels and by examining the mediating effects of teacher and classroom characteristics in these schools.

Variance Decomposition between Student-, Teacher/Classroom-, and School-Levels

The decomposition of variance suggests that nearly all of the variation in virtual student test scores occurred at the teacher and student levels (see Table 3). That is, when it comes to raw student achievement, it does not appear to matter which virtual charter school a student attended in Indiana, but rather who that student was assigned as a teacher and other student-level factors. For example, the school-level ICC for virtual charters was 0.02 in math, which indicates that only 2% of the total variation in student test scores can be attributed to the school-level. This is notably different than the school-level ICCs for TPSs and B&M charters, which were 21% and 11%, respectively. Meanwhile, 13% of the variation in the virtual student test scores was attributed to the teacher/classroom-level, compared to 20% in TPSs and 10% in B&M charters. The ICCs in ELA followed a similar pattern. Once again, little of the total variation in virtual student outcomes was explained at the school-level (4%) compared to TPSs (20%) and B&M charters (8%). A larger proportion of the variation was explained at the teacher/classroom-level (6%), but still less so than TPSs (14%) and B&M charters (7%).

<Table 3 about here>

While useful, the unconditional ICCs do not account for selection into schools and to different teachers (see Jenkins & DiPrete, 2010). To adjust for this, we include conditional ICCs, which include lagged student scores as a student-level control. Sorting accounts for much of the between-school and teacher variation in student achievement, especially in traditional public schools. However, in both ELA and math across both ICC estimation strategies, it is consistently the case that teachers account for less of the variation in student performance in virtual charters compared to both TPSs and B&M charters. This suggests that the virtual teaching format allows for less variation in instruction quality, and that there is far more variation in student achievement due to teacher differences in both traditional public schools and B&M charter schools compared to virtual charter schools.

Mediation Analysis of Teacher and Classroom Characteristics

Given that we find less variation in teacher effects in the virtual charter context, it stands to reason that sector-level differences in teacher and classroom characteristics could explain the negative impact of virtual charter schools on student achievement. The following mediation analysis aims to identify what proportion of our main finding is driven by various teacher and classroom characteristics. The main findings for models that include teacher and classroom characteristics can be interpreted as the effect of virtual and B&M charter schools net of differences in teacher and classroom characteristics. Any reduction in the main effect can be viewed as the portion of the relationship of interest explained by available teacher and classroom characteristics. We display the results of the mediation analysis for math and ELA outcomes across three years in Table 4.

<Table 4 about here>

In both ELA and math, we found that after accounting for teacher and classroom characteristics, the negative impacts of switching to a virtual charter school still remained large, negative, and statistically significant, despite being somewhat reduced. Specifically, the inclusion of teaching characteristics reduces the effect of attending a virtual charter by between 0% and 17%. Teaching characteristics explain far more of the virtual schooling effect on ELA achievement – the inclusion of teaching characteristics reduce the virtual charter coefficient by between 22% and 53% across three years. This indicates that the overall lower qualifications and higher student-teacher ratio of virtual charter teachers compared to traditional public teachers does explain some of the negative impact, yet the majority of this negative effect remains unexplained.

Discussion

This study builds upon a growing body of research that examines heterogeneity within the charter school sector by focusing on virtual charter schools operated by EMOs in Indiana. By making use of longitudinal administrative records in Indiana, we were able to estimate the effects of virtual and brick-and-mortar charter schools on elementary and middle school students' achievement over time. The results for virtual EMOs and brick-and-mortar charters were distinct. Notably, students who switched to virtual charter schools experienced large, negative effects in math and ELA that were sustained across time. These effects were consistently of magnitude that warrants serious concern and further investigation.

At first glance, virtual charter schools offer students and their families the ability to tailor learning experiences to specialized needs. For instance, some students may desire a different pace than a classroom that is standardized to students at a certain level of ability. Indeed, a study conducted by Mathematica Policy Research (Gill et al., 2015) found that virtual charter schools rely heavily on student-driven independent study and that they rarely experience teacher-guided synchronous instruction (median hours per week for 4th and 7th graders was 4 and 3 hours, respectively). However, if such advantages are available to students in virtual charters, there is no evidence that the benefits transfer into gains on student test scores. To the contrary, we find that virtual charter schools have a substantial negative impact on achievement for students in Indiana at magnitudes that are consistent with prior studies in Ohio (Ahn & McEachin, 2017; Zimmer et al., 2009) and across a broader sample of states (CREDO, 2015, 2019a, 2019b, 2019c). Furthermore, a recent public report by the IDOE reveals that the poor performance of virtual charter schools is not limited to test scores – among high school students, virtual charter schools have among the lowest graduation rates in the state, ranging from 2% to 59% of students (Indiana Department of Education, 2018).

For some parents, these negative outcomes may offset the benefits they receive from the autonomy afforded by virtual charter schools. Perhaps parents and children who chose virtual charter schools place a particular emphasis on autonomy or view their alternative schooling options as objectionable. We also should not discount the possibility that one benefit that families see in virtual schooling is the low risk of bullying or other concerns related to school safety. However, it is noteworthy that any systematic benefit that students experience from either an increase in autonomy or a decrease in physical or emotional threats they would face in their alternative school environment would bias our results towards zero. We encourage future work on why families choose these schools, and encourage efforts to eliminate any factor that "pushes" families to choose virtual schooling if they otherwise would not prefer it. That said, information about the poor performance of these schools should be available to families considering them.

From a public policy perspective, virtual charter schools are not translating public investment into outcomes that are consistent with the mandate of state and federal education policies (see Orfield, 2014). Indeed, the trace amount of variation in student performance at the school-level suggests that the entire virtual sub-sector in Indiana is performing well below expectations. This is especially concerning given the for-profit nature of these virtual charter operators, as these low-performing virtual schools continue to represent profitable ventures for investors at the taxpayers' expense. While the poor performance of these schools is no secret, with all virtual charter schools receiving an F rating from the IDOE in 2017 (Lindsay, 2018), their enrollment continues to increase. Perhaps this is due to the ability of virtual charter school operators to spend considerable sums lobbying state policymakers (Cavazos, 2019).

Ostensibly, authorizers are supposed to hold management organizations accountable to the learning objectives set forth in the charter. Yet, the findings from our study suggest a failure in the process by which virtual charter schools in Indiana are authorized and reauthorized. A report published by the Education Research Alliance for New Orleans (Bross & Harris, 2016) suggests that, in New Orleans, positive test score performance is predictive of reauthorization. However, the authors caution that the policy context in New Orleans is unique, and thus these findings cannot be inferred to other states such as Indiana. Yet, even if, on average, positive school performance is a driving factor behind reauthorization, it is clear that virtual charter schools in states across the country are the exception. Future research should examine the reauthorization process for virtual charters to better inform policymakers of ways to hold these management organizations accountable.

Aside from expanding the growing body of evidence concerning the negative effects of virtual charter schools, our study contributed a first glance at the extent to which the characteristics of teachers and classrooms in these schools impact student achievement. Overall, we found modest evidence that the negative impacts of virtual charter schools could be attributed to observable characteristics of teachers. Because the effects of teacher degrees and experience on student achievement have had mixed effects on student achievement, this finding may not be surprising (e.g., Clotfelter, Ladd, & Vigdor, 2007; Harris & Sass, 2011; Rivkin, Hanushek, & Kain, 2005). However, because we found the context (i.e., class size) varied significantly among virtual charters, TPS, and B&M charter teachers, we would have expected this social context to matter more (Kelly, Podgodzinski, & Zhang, 2018).

It is also possible that discussing classroom characteristics in a study of virtual charter schools reflects out of date thinking, as the typical understanding of the term “classroom” could be meaningless in the context of virtual schooling. After all, there are no rooms in which class takes place. This could explain why the vastly higher classroom size in virtual charter schools matters less than expected. This raises a wider question of how to conceptualize the structure of schooling when physical structures play no role. On the other hand, it could be that physical proximity is an essential ingredient to effective schooling. Although we do not assume that the failure of virtual schools in this setting indicates that they can never succeed, we also cannot entirely discount the idea that the virtual nature of these schools is inherently limiting. While prior research focusing on online courses offered in traditional public schools suggests that such courses can have a positive impact on student learning (Hart et al., 2019; Means et al., 2013), the literature on full-time virtual schools is far less optimistic (Miron & Elgeberi, 2019).

Given that virtual schooling represents a profoundly divergent setting for learning to take place, it is perhaps unsurprising that available data on teachers, classrooms, and schools failed to explain the negative effect of attending a virtual charter school. The administrative data used in the current study are collected by the state for the purpose of analyzing school performance, but the array of variables collected was decided upon before virtual charter schools were present in the state. We expect that alternative variables, especially time on schooling and time on instruction, would explain a substantial portion of the negative effect of attending a virtual charter school. Given that software mediates virtual schooling, collecting information on the amount of time students spend on schooling tasks should not be difficult, and is likely already collected by the companies that operate virtual charter schools. In addition, we currently do not have information on either the pedagogical practices of virtual schools or how they introduce students to the virtual school setting. We strongly recommend that researchers in the future collect data on student orientation to virtual schools, time spent on schooling, and instructional practices that would help to contextualize our virtual charter school findings.

In exploring the heterogeneity among charter operators, researchers have only scratched the surface of understanding the conditions through which charter schools impact student outcomes (Berends, 2015, 2020; Berends & Waddington, 2019). In addition to heterogeneity in the main effects of student achievement, it is also a policy imperative to estimate whether or not certain types of charter schools ameliorate gaps between racial and socioeconomic groups. The focus on gaps is important because one of the stated objectives of charter schools as a reform strategy is to offset the relative disadvantages that low income and students of color experience by being constrained to their neighborhood schools. Thus, in future research we intend to estimate whether or not different types of charters are more or less effective at ameliorating gaps that have persisted in traditional public schools for decades.

Notes

[1] The Indianapolis Mayor's Office is the only authorizer in the country that involves a consolidated city's executive. The recent charter law in Kentucky will allow for the mayors of Lexington and Louisville to authorize charter schools.

[2] One of the four virtual charter schools in Indiana, Indiana Cyber Charter School, operated as a hybrid school (i.e., time spent in both brick-and-mortar and online settings). This school closed after the 2014-15 school year due to financial mismanagement. We categorized it as a virtual school due to online instruction comprising a substantial proportion of the student's time. Although an analysis of hybrid schooling would be valuable, the existence of only a single school known to have had serious financial issues means that generalizing the findings of such a study would be impossible.

[3] The first virtual charter school opened in Indiana during the 2009-10 school year as a pilot program. A new one, Insight School of Indiana, opened prior to the 2016-17 school year as an offshoot of an existing virtual charter school, Hoosier Academy Virtual Charter School.

[4] The ISTEP+ is vertically equated across grades and consists of multiple-choice, constructed-response, and extended-response items scored using item response theory methods. Reliability coefficients range from 0.88 to 0.94 in ELA and 0.88 to 0.95 in math (Indiana Department of Education, 2011). Schools can administer the multiple-choice section online.

[5] In the elementary grade levels (i.e. 3-5), students most often have the same teacher for all subjects. In the middle grade levels (i.e. 6-8), students often have subject-specific teachers. This is common for all types of schools, whether virtual charter, brick-and-mortar charter, or traditional public.

[6] We omitted students transitioning from an Indiana private school into a charter school. Only 10% of students who switch into charter schools between grades 3-8 transition from private schools.

[7] By matching students separately by subject, we have in effect created two different analytical samples: one for the math analysis and one for the ELA analysis. We could have required that students be matched within the caliper on both subjects, therefore creating a consistent sample between the math and ELA analyses. However, we chose to match separately by subject to avoid a further 10 percentage point reduction in the analytical sample of charter school students. In addition, there are no meaningful differences between the math and ELA analysis samples.

Tables

Table 1. Descriptive comparison of matched analytic samples

	Math Analytic Sample			
	Virtual Charter		Brick & Mortar Charter	
	Control	**Treatment**	**Control**	**Treatment**
ISTEP+ Standardized Scores				
First Year Post-Treatment Math Scores	-0.063	-0.530	-0.390	-0.487
	(0.835)	(0.960)	(0.876)	(0.845)
Baseline Math Scores	-0.094	-0.151	-0.328	-0.355
	(0.781)	(0.894)	(0.786)	(0.847)
Pre-baseline Math Scores	-0.084	-0.116	-0.315	-0.350
	(0.833)	(0.899)	(0.851)	(0.860)
First Year Post-Treatment ELA Scores	-0.052	-0.345	-0.365	-0.373
	(0.872)	(0.984)	(0.867)	(0.865)
Baseline ELA Scores	-0.089	-0.059	-0.370	-0.418
	(0.723)	(0.796)	(0.704)	(0.750)
Pre-baseline ELA Scores	-0.069	-0.060	-0.349	-0.382
	(0.842)	(0.859)	(0.773)	(0.815)
Baseline Student Characteristics				
White	0.907	0.869	0.336	0.316
African American	0.042	0.048	0.478	0.483
Latino	0.025	0.041	0.166	0.164
Asian/Pacific Islander	0.006	0.007	0.003	0.000
Female	0.574	0.561	0.509	0.503
Free or Reduced Lunch	0.516	0.545	0.770	0.762
Limited English Proficiency	0.013	0.008	0.103	0.085
Special Education	0.125	0.151	0.112	0.095
Received an In-School Suspension	0.061	0.043	0.089	0.063
Received an Out-of-School Suspension	0.063	0.053	0.141	0.250
Expelled	0.001	0.010	0.003	0.005
Teacher/Classroom Characteristics in First Post-Treatment Year				
Class Size	23.950	100.649	29.214	22.009
	(8.267)	(83.265)	(30.873)	(10.201)
Years of Teaching Experience	12.858	9.477	11.876	6.163
	(10.104)	(6.566)	(10.423)	(6.747)
Master's Degree	0.486	0.325	0.411	0.294
Female	0.679	0.805	0.698	0.734
Solo Taught Classroom	0.892	0.894	0.804	0.904
Students	7,492	1,644	4,351	1,511

Note: Cells contain variable means, with standard deviations in parentheses where appropriate.

Table 2. Annual impacts of charter school attendance on student achievement

A. Math achievement

	Pre-baseline	Baseline	First year at a charter school	Second year at a charter school	Third year at a charter school
Virtual Charter	0.012	-0.001	-0.414***	-0.481***	-0.500***
	(0.014)	(0.002)	(0.025)	(0.038)	(0.064)
Brick & Mortar Charter	-0.003	0.001	-0.068**	-0.018	0.005
	(0.018)	(0.002)	(0.023)	(0.025)	(0.058)
Baseline covariates	Y	Y	Y	Y	Y
Baseline & pre-baseline achievement	N	N	Y	Y	Y
Grade fixed effects	Y	N	Y	Y	Y
Matching cell fixed effects	Y	Y	Y	Y	Y
Observations	17,581	17,581	17581	7407	2829
r^2	0.022	0.004	0.199	0.148	0.121

B. ELA achievement

	Pre-baseline	Baseline	First year at a charter school	Second year at a charter school	Third year at a charter school
Virtual Charter	0.005	-0.001	-0.286***	-0.264***	-0.334**
	(0.014)	(0.001)	(0.024)	(0.047)	(0.092)
Brick & Mortar Charter	0.000	-0.004**	0.005	-0.005	0.026
	(0.017)	(0.002)	(0.026)	(0.027)	(0.056)
Baseline covariates	Y	Y	Y	Y	Y
Baseline & pre-baseline achievement	N	N	Y	Y	Y
Grade fixed effects	Y	N	Y	Y	Y
Matching cell fixed effects	Y	Y	Y	Y	Y
Observations	17,466	17,466	17466	7504	2802
r^2	0.018	0.003	0.175	0.131	0.104

Note: *p≤0.025; **p≤0.005; ***p≤0.0005 after applying Bonferroni correction for multiple comparisons. ISTEP+ math and ELA achievement measured in standard deviation units, relative to the Indiana state mean and standard deviation within each grade and year. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses.

Table 3. Intra-class correlations within teachers and students by public school type

	Unconditional			Conditional		
A. Math Achievement						
	Traditional Public	**Virtual Charter**	**Brick & Mortar Charter**	**Traditional Public**	**Virtual Charter**	**Brick & Mortar Charter**
School ICC	0.212	0.020	0.108	0.074	0.033	0.118
Unique Teacher ICC	0.201	0.130	0.104	0.149	0.021	0.123
N - Schools	769	4	63	769	4	63
N - Teachers	3,396	74	415	3,396	74	415
N - Students	13,938	1,651	1,720	13,938	1,651	1,720
B. ELA Achievement						
	Unconditional			**Conditional**		
	Traditional Public	**Virtual Charter**	**Brick & Mortar Charter**	**Traditional Public**	**Virtual Charter**	**Brick & Mortar Charter**
School ICC	0.201	0.035	0.075	0.040	0.060	0.056
Unique Teacher ICC	0.142	0.058	0.072	0.052	0.022	0.043
N - Schools	764	4	64	764	4	64
N - Teachers	3,814	56	413	3,814	56	413
N - Students	13,904	1,677	1,722	13,904	1,677	1,722

Note: The ICCs listed above were derived from random effects models of student tests scores from the first year posttreatment. Year and grade fixed effects are included in all models, and the conditional model included a control for pre-treatment test score.

Table 4. Mediating impacts of teacher traits

	First year at a charter school		Second year at a charter school		Third year at a charter school	
A. Math achievement						
Virtual Charter	-0.414***	-0.396***	-0.481***	-0.401***	-0.500***	-0.499**
	(0.025)	(0.034)	(0.038)	(0.073)	(0.064)	(0.150)
Brick & Mortar Charter	-0.068**	-0.065**	-0.018	-0.015	0.005	-0.005
	(0.023)	(0.022)	(0.025)	(0.026)	(0.058)	(0.057)
Master's Degree		0.012		-0.006		0.023
		(0.011)		(0.020)		(0.033)
Class Size (x10)		-0.000		-0.000		0.000
		(0.000)		(0.000)		(0.000)
Years of Teaching Experience		0.001*		0.003		0.001
		(0.001)		(0.001)		(0.002)
Female		0.002		0.078**		0.034
		(0.011)		(0.023)		(0.032)
Solo Taught Classroom		0.036		0.025		0.165**
		(0.018)		(0.029)		(0.045)
Observations	17,581	17,581	7407	7407	2829	2829
r^2	0.199	0.201	0.148	0.154	0.121	0.131
Virtual Coefficient Change		-4%		-17%		0%

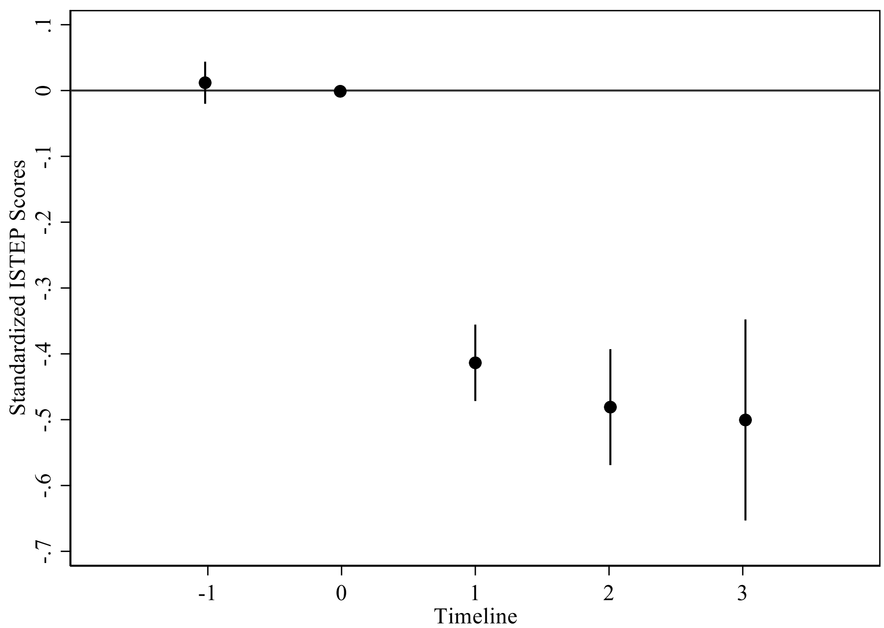
	First year at a charter school		Second year at a charter school		Third year at a charter school	
B. ELA achievement						
Virtual Charter	-0.286***	-0.222***	-0.264***	-0.162**	-0.334**	-0.157
	(0.024)	(0.027)	(0.047)	(0.055)	(0.092)	(0.175)
Brick & Mortar Charter	0.005	0.016	-0.005	0.006	0.026	0.018
	(0.026)	(0.025)	(0.027)	(0.028)	(0.056)	(0.061)
Master's Degree		0.003		0.001		-0.028
		(0.013)		(0.019)		(0.033)
Class Size (x10)		-0.000**		-0.000*		-0.000
		(0.000)		(0.000)		(0.000)
Years of Teaching Experience		0.002**		0.002		0.001
		(0.001)		(0.001)		(0.001)
Female		-0.011		0.018		0.011
		(0.012)		(0.025)		(0.048)
Solo Taught Classroom		-0.007		-0.037		-0.021
		(0.018)		(0.035)		(0.045)
Observations	17,466	17,466	7504	7504	2802	2802
r^2	0.175	0.178	0.131	0.133	0.104	0.106
Virtual Coefficient Change		-22%		-39%		-53%

Note: *p≤0.025; **p≤0.005; ***p≤0.0005 after applying Bonferroni correction for multiple comparisons. ISTEP+ math and ELA achievement measured in standard deviation units, relative to the Indiana state mean and standard deviation within each grade and year. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses. All models include baseline covariates, baseline and pre-baseline achievement, grade fixed effects, and matching cell fixed effects.

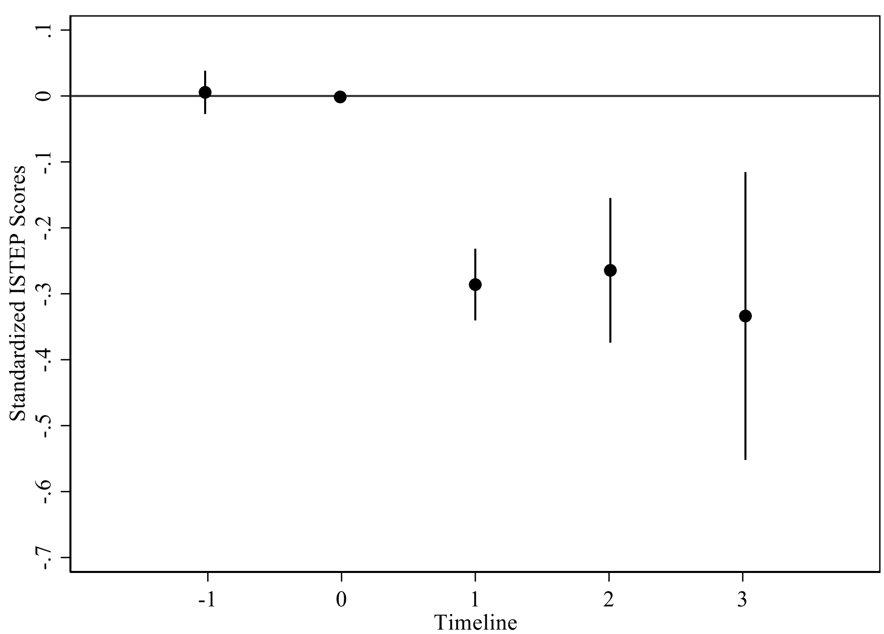
Figures

Figure 1. Annual effect of virtual charter school attendance

A. Math achievement



B. ELA achievement



Note: Each point represents the virtual charter coefficient from a distinct model from Table 2. Timeline=0 corresponds to the baseline year. 97.5% confidence intervals are displayed in accordance with the Bonferroni adjustment.

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Appendix A: Data Restrictions for the Analytic Sample

Before excluding students based on whether they could be linked to teacher data and whether they could be matched to peers at baseline, we made multiple decisions that paired down our sample. Between the 2011-2012 and 2016-2017 school years, there were 11,240 students who attended a virtual charter school and 36,020 students who attended a B&M charter school in grades 3-8 across the state of Indiana. We removed all students who changed schools during the school year or had duplicate observations (245), as we do not have information on which school these students attended first or how long they attended each school. We then removed charter students with less than three years of data, as our modeling strategy requires two years of data pre-treatment. This included students with only one or two years of data (6,398 and 7,272 respectively, mostly 3^{ed} and 4th graders in 2016-2017). We also removed any student who are missing both math and ELA test scores in any given year because of the achievement trajectory gap this represents (2,879).

Because of these decisions, we begin with a sample of 30,631 eligible unique charter school students (7,217 virtual and 23,893 B&M charter). Because of similar pairing of the sample, we begin with 531,866 unique traditional public school students who remain in traditional public schools throughout our dataset. We describe additional sample reductions in the body of our data. The end result is a sample of 1,963 virtual charter and 2,222 B&M charter school students that are used in our analysis. We compare this sample to eligible students who were excluded from our sample in Table A1.

Our analytic sample slightly over-represents virtual charter students who are white, have higher achievement scores, and are more advantaged. The opposite trend exists for the B&M

charter student sample. While noteworthy, the differences between both groups in the sample and the population are quite small.

Appendix Table A1. Comparison of analytic sample to all other charter students

	Virtual Charter		Brick & Mortar Charter	
	Analytic Sample	Absent from Sample	Analytic Sample	Absent from Sample
ISTEP+ Math Standardized Score	-0.511	-0.644	-0.470	-0.430
	(0.927)	(1.072)	(0.867)	(0.948)
ISTEP+ ELA Standardized Score	-0.285	-0.399	-0.372	-0.343
	(0.963)	(1.108)	(0.825)	(0.920)
White	0.884	0.775	0.293	0.259
African American	0.048	0.096	0.511	0.549
Latino	0.039	0.061	0.170	0.126
Asian/Pacific Islander	0.005	0.013	0.000	0.006
Female	0.571	0.500	0.479	0.503
Free or Reduced Lunch	0.486	0.401	0.766	0.743
Limited English Proficiency	0.015	0.014	0.092	0.070
Special Education	0.134	0.154	0.100	0.129
Received an In-School Suspension	0.033	0.018	0.062	0.062
Received an Out-of-School Suspension	0.041	0.029	0.250	0.192
Expelled	0.010	0.007	0.002	0.002
N - Students	1,963	5,284	2,222	21,869
N - Cases	2,552	9,014	3,542	58,009

Appendix B: Robustness Checks for the Virtual Charter Finding

Our aim here is to detail the five alternate model specifications that act as robustness checks for our preferred model. Each set of estimates presented here have the same specifications as the preferred model, with the exception of the alterations described below. We display these results in Table B1 for math and B2 for ELA. For the sake of simplicity, we only display the coefficients for the effect of attending a virtual charter school, though these alternative specifications also provide evidence for the robustness of our estimates of the effect of brick and mortar charter schools.

Our first two checks involve manipulation of our specification of baseline and pre-baseline student achievement. The first includes first-, second-, and third-order polynomial terms, mimicking the specification used by Dobbie & Fryer (2017). This specification reveals does not differ meaningfully from the preferred model. The second check includes off subject (e.g., pretreatment math achievement math achievement in the ELA models and vice versa) pre-treatment controls. This was done in response to recent value-added modeling literature (for review see Koedel, Mihaly, & Rockoff, 2015). This specification leads to small but significant changes in both directions for the post-treatment covariates, but these changes would in no way alter our conclusion concerning the negative impact of virtual charter schools on student outcomes.

The third robustness check includes two covariates that indicate whether a student changed schools from between the previous and current school year. The first covariate indicates that a student changed schools due to normal grade progression, and the second indicates that a student changed for any other reason. Given the negative association between student mobility and achievement (Schwartz, Stiefel, & Cordes, 2017), these indicators could explain part of the first-year negative effect of virtual charters. While the first post-treatment year coefficient is slightly smaller with the inclusion of these controls, this change amounts to 0.03 SD in math and a 0.02 SD decrease in ELA.

Our next check constrains our sample to only those students who entered a virtual charter school due to a structural move, where students where both treatment and control students changed schools entering the first post-treatment year by necessity. While this strategy greatly decreases the analytic sample, the results of these models grant the same conclusion about the effect of virtual schooling, with only small deviations in the coefficients.

Finally, we ran models where students who leave virtual charter schools after the first post-treatment year in the treatment group as opposed to returning them to the control group. This affects a profound number of students, as 41% of students who enter a virtual charter school in the first year post-treatment exit the next year; this compares to only 26% of B&M charter students. There are a number of unobserved and potentially confounding reasons a student would exit a virtual charter school, and these factors could threaten the validity of our estimates. This approach is the result of work by Robins, Hernán, & Brumback (2000) and Sobel (2012). Perhaps unsurprisingly, the negative effect of virtual charters in the second and third years post-treatment are reduced for both math and ELA by up to 50% using this specification. However, given that the majority of the students viewed as treated in these models have returned to traditional public schools, we view these still-profound and negative coefficients as strengthening our main findings rather than detracting from it.

Appendix Table B1. Robustness checks of main virtual charter effects on math achievement

	Pre-Baseline	Baseline	1st Year in virtual charter	2nd Year in virtual charter	3rd Year in virtual charter
Preferred model	0.012	-0.001	-0.414***	-0.481***	-0.500***
	(0.014)	(0.002)	(0.025)	(0.038)	(0.064)
	[1,651]	[1,651]	[1,651]	[367]	[100]
Polynomial prior ach.	0.012	-0.001	-0.414***	-0.479***	-0.498***
	(0.014)	(0.002)	(0.025)	(0.036)	(0.062)
	[1,651]	[1,651]	[1,651]	[367]	[100]
Off-Subj. prior ach. incl.	0.012	-0.001	-0.409***	-0.429***	-0.635***
	(0.014)	(0.002)	(0.028)	(0.071)	(0.103)
	[1,651]	[1,651]	[1,651]	[367]	[100]
Mobility indicators incl.	0.012	-0.001	-0.379***	-0.492***	-0.478***
	(0.014)	(0.002)	(0.025)	(0.036)	(0.063)
	[1,651]	[1,651]	[1,651]	[367]	[100]
Structural changes only	0.016	0.003	-0.356***	-0.440***	-0.382***
	(0.031)	(0.004)	(0.035)	(0.061)	(0.080)
	[411]	[411]	[411]	[159]	[49]
Keep exit stud. in treat.	0.012	-0.001	-0.414***	-0.366***	-0.232***
	(0.014)	(0.002)	(0.025)	(0.027)	(0.055)
	[1,651]	[1,651]	[1,651]	[660]	[230]

Note: *p≤0.025; **p≤0.005; ***p≤0.0005 after applying Bonferroni correction for multiple comparisons. ISTEP+ Math achievement measured in standard deviation units. Number of virtual charter students contributing to each yearly estimate in brackets. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses.

Appendix Table B2. Robustness checks of main virtual charter effects on ELA achievement

	Pre-Baseline	Baseline	1st Year in virtual charter	2nd Year in virtual charter	3rd Year in virtual charter
Preferred model	0.005	-0.001	-0.286***	-0.264***	-0.334**
	(0.014)	(0.001)	(0.024)	(0.047)	(0.092)
	[1,677]	[1,677]	[1,677]	[360]	[102]
Polynomial prior ach.	0.005	-0.001	-0.287***	-0.265***	-0.334**
	(0.014)	(0.001)	(0.024)	(0.048)	(0.093)
	[1,677]	[1,677]	[1,677]	[360]	[102]
Off-Subj. prior ach. incl.	0.005	-0.001	-0.246***	-0.218**	-0.489***
	(0.014)	(0.001)	(0.027)	(0.072)	(0.110)
	[1,677]	[1,677]	[1,677]	[360]	[102]
Mobility indicators incl.	0.005	-0.001	-0.270***	-0.264***	-0.350**
	(0.014)	(0.002)	(0.025)	(0.048)	(0.100)
	[1,677]	[1,677]	[1,677]	[360]	[102]
Structural changes only	0.012	-0.004	-0.290***	-0.222**	-0.350**
	(0.042)	(0.004)	(0.045)	(0.065)	(0.125)
	[338]	[388]	[388]	[128]	[39]
Keep exit stud. in treat.	0.005	-0.001	-0.286***	-0.149***	-0.187***
	(0.014)	(0.001)	(0.024)	(0.030)	(0.045)
	[1,677]	[1,677]	[1,677]	[648]	[238]

Note: *p≤0.025; **p≤0.005; ***p≤0.0005 after applying Bonferroni correction for multiple comparisons. ISTEP+ ELA achievement measured in standard deviation units. Number of virtual charter students contributing to each yearly estimate in brackets. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses.

Appendix C: Moderation Analysis with Teacher and Classroom Characteristics

In this appendix, we describe our empirical approach and findings for a moderation analysis of teacher and classroom characteristics on the impacts of attending a charter school. Our model for testing the moderating impacts of individual teacher and classroom characteristics is displayed in equation (3).

$$\mathrm{Y_{icgt}} = \alpha + \beta_1 \mathrm{Virtual_{icgt}} + \beta_2 \mathrm{B\&M_{icgt}} + \sigma \mathrm{T_{icgt}} + \varphi_1 \mathrm{Virtual} * \mathrm{TC_{icgt}} + \varphi_2 \mathrm{B\&M} * \mathrm{TC_{icgt}} + \pi \mathrm{Y_{icg(t=0)}} + \omega \mathrm{Y_{icg(t=-1)}} + \delta \mathrm{X_{icg(t=0)}} + \theta_g + \tau_c + \upsilon_{icgt} \quad (3)$$

This model an extension of model (2), containing all teacher and classroom characteristics used for the mediation analysis, whereby we now include an interaction of a single teacher covariate with the virtual ($\mathrm{Virtual} * \mathrm{TC_{icgt}}$) or brick-and-mortar ($\mathrm{B\&M} * \mathrm{TC_{icgt}}$) main effects. If the estimates on these terms are statistically significant, it would indicate the presence of a moderating impact of a given teacher characteristic on the impact of switching to a virtual (φ_1) or brick-and-mortar (φ_2) charter school. For ease of estimation and interpretation, we tested for the moderating impacts of one teacher characteristic at a time within any one year posttreatment. In Table C1, we display the moderating impacts on math achievement and in Table C2 for ELA achievement. The interaction coefficients in each model indicate the extent to which the relationship between teacher/ school characteristics and student achievement change depending on the schooling context. Additional years post-treatment are available upon request.

The results of this analysis are inconsistent from year to year and yield little evidence that charter type profoundly moderates the relationship between teacher/classroom characteristics and school achievement. We originally theorized that the drawbacks of virtual communication would reduce the relationship between teacher qualifications and student achievement in the virtual charter setting but found little evidence that this is the case. The only teacher characteristic with a consistently positive relationship with student achievement is a teacher's years of teaching experience, and this relationship is consistent across TPS, B&M, and virtual charter schools. We recommend additional research into how virtual schooling affects teaching and learning.

Appendix Table C1: Moderating effects of teacher traits on Math achievement – First year post treatment

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Virtual Charter	-0.414***	-0.396***	-0.439***	-0.409***	-0.410***	-0.384***	-0.467***
	(0.025)	(0.034)	(0.038)	(0.044)	(0.039)	(0.030)	(0.047)
Brick & Mortar Charter	-0.068**	-0.065**	-0.076**	-0.123***	-0.052	-0.106*	-0.133*
	(0.023)	(0.022)	(0.024)	(0.033)	(0.031)	(0.038)	(0.058)
Master's Degree		0.012	-0.008	0.012	0.012	0.012	0.011
		(0.011)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)
Class Size (x10)		-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years of Teaching Experience		0.001**	0.002**	0.001**	0.002**	0.001**	0.001**
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female		0.002	-0.002	0.002	0.002	-0.002	0.001
		(0.011)	(0.011)	(0.011)	(0.011)	(0.013)	(0.011)
Solo Taught Classroom		0.036*	0.035*	0.035*	0.035*	0.035*	0.018
		(0.018)	(0.017)	(0.018)	(0.017)	(0.017)	(0.018)
Virtual Charter * Master's Degree			0.121*				
			(0.044)				
B & M Charter * Master's Degree			0.038				
			(0.047)				
Virtual Charter * Class Size (x10)				0.000			
				(0.000)			
B & M Charter * Class Size (x10)				0.000*			
				(0.000)			
Virtual Charter * Years of Teaching Experience					0.002		
					(0.003)		
B & M Charter * Years of Teaching Experience					-0.002		
					(0.003)		
Virtual Charter * Female Teacher						-0.016	
						(0.033)	
B & M Charter * Female Teacher						0.058	
						(0.044)	
Virtual Charter * Solo Taught Classroom							0.083
							(0.050)
B & M Charter * Solo Taught Classroom							0.076
							(0.066)
Constant	1.467***	1.402***	1.413***	1.410***	1.398***	1.388***	1.427***
	(0.236)	(0.233)	(0.237)	(0.231)	(0.236)	(0.229)	(0.234)
N	17,581	17,581	17,581	17,581	17,581	17,581	17,581
r2	0.199	0.201	0.202	0.201	0.201	0.201	0.201

Note: *$p \leq 0.025$; **$p \leq 0.005$; ***$p \leq 0.0005$ after applying Bonferroni correction for multiple comparisons. ISTEP+ math and ELA achievement measured in standard deviation units, relative to the Indiana state mean and standard deviation within each grade and year. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses. All models include baseline covariates, baseline and pre-baseline achievement, grade fixed effects, and matching cell fixed effects. There are few differences between these models and those run in additional posttreatment years; those models are available upon request.

Appendix Table C2: Moderating effects of teacher traits on ELA achievement – First year post treatment

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Virtual Charter	-0.286***	-0.222***	-0.242***	-0.249***	-0.257***	-0.033	-0.262***
	(0.024)	(0.027)	(0.034)	(0.030)	(0.037)	(0.035)	(0.047)
Brick & Mortar Charter	0.005	0.016	-0.005	-0.060	0.040	0.004	-0.018
	(0.026)	(0.025)	(0.025)	(0.070)	(0.026)	(0.035)	(0.056)
Master's Degree		0.003	-0.011	0.002	0.001	0.007	0.002
		(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Class Size (x10)		-0.000**	-0.000**	-0.000**	-0.000**	-0.000***	-0.000**
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years of Teaching Experience		0.002**	0.002**	0.002**	0.002**	0.002**	0.002**
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female		-0.011	-0.012	-0.011	-0.011	0.016	-0.011
		(0.012)	(0.013)	(0.012)	(0.013)	(0.011)	(0.012)
Solo Taught Classroom		-0.007	-0.009	-0.008	-0.007	-0.010	-0.017
		(0.018)	(0.018)	(0.018)	(0.018)	(0.017)	(0.018)
Virtual Charter * Master's Degree			0.067				
			(0.053)				
B & M Charter * Master's Degree			0.071				
			(0.045)				
Virtual Charter * Class Size (x10)				0.000*			
				(0.000)			
B & M Charter * Class Size (x10)				0.000			
				(0.000)			
Virtual Charter * Years of Teaching Experience					0.005		
					(0.003)		
B & M Charter * Years of Teaching Experience					-0.005		
					(0.003)		
Virtual Charter * Female Teacher						-0.215***	
						(0.041)	
B & M Charter * Female Teacher						0.017	
						(0.043)	
Virtual Charter * Solo Taught Classroom							0.047
							(0.059)
B & M Charter * Solo Taught Classroom							0.037
							(0.066)
Constant	1.252**	1.206**	1.204**	1.210**	1.199**	1.156**	1.214**
	(0.375)	(0.377)	(0.377)	(0.377)	(0.377)	(0.373)	(0.376)
N	17,466	17,466	17,466	17,466	17,466	17,466	17,466
r2	0.175	0.178	0.178	0.178	0.178	0.180	0.178

Note: *p≤0.025; **p≤0.005; ***p≤0.0005 after applying Bonferroni correction for multiple comparisons. ISTEP+ math and ELA achievement measured in standard deviation units, relative to the Indiana state mean and standard deviation within each grade and year. Robust standard errors clustered by baseline cohort (year-grade-school) are in parentheses. All models include baseline covariates, baseline and pre-baseline achievement, grade fixed effects, and matching cell fixed effects. There are few differences between these models and those run in additional posttreatment years; those models are available upon request.