Indiana Charter School Performance During and After the COVID-19 Pandemic

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Abstract

The COVID-19 pandemic disrupted schooling nationwide and contributed to substantial declines in student achievement. At the same time, enrollment patterns shifted and charter school sectors expanded in several states, raising questions about whether charter schools were better positioned to support student learning during and after the pandemic. This study estimates the effect of attending a charter school on student achievement growth in Indiana during and after the pandemic using statewide longitudinal administrative data from 2017-18 through 2023–24. We compare charter school students with observationally similar traditional public school students using regression models with inverse probability weighting based on propensity scores and controls for prior achievement and student characteristics. During the 2020–21 school year, charter school students performed similarly to traditional public school peers in mathematics and modestly outperformed them in English/language arts. In post-pandemic years, charter school students experienced greater achievement growth in both subjects, with moderate effect sizes. These gains were concentrated among Black, Hispanic, economically disadvantaged, and lower-performing students. Differences in chronic absenteeism or instructional modality do not explain these patterns. The results are similar in magnitude to a companion study in Tennessee, suggesting charter schools may have contributed to stronger post-pandemic academic recovery.

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Introduction

Few events have had such a disruptive impact to large scale education as the COVID-19 pandemic. Beginning in early spring 2020, schools around the country began transitioning to remote or hybrid forms of instruction, and many oscillated between in-person and remote for much of the next academic year. It is well documented that students experienced learning loss during this period (Fahle et al, 2023; Goldhaber et al., 2023; Relyea et al., 2022; Sass and Goldring, 2022), and that student enrollment patterns changed (Dee, 2023), including a surge of students into charter schools (Jacobs & Veney, 2022).

Studies of COVID-19 learning loss and recovery have generally focused on geographic variation as well as demographic subgroups, finding highly uneven recovery across states and more adverse impacts to disadvantaged students (Goldhaber et al., 2022; Kuhfeld, Soland, & Lewis, 2023). However, very little is known about learning loss and recovery across different sectors of the education system. Given that there may have been considerable variation in the ways that traditional public schools (TPSs) and charter schools responded to the pandemic, examining the impact of these sectors on student outcomes may offer educational leaders and policymakers insight into student recovery from learning loss.

To our knowledge, Kho, Smith, and Zimmer's (2025) working paper of Tennessee charter schools is the only analysis using student-level data to examine the impact of charter schools during and after the pandemic. They found that charter students performed on par with TPS students during the pandemic but outperformed TPS students post-pandemic. In this paper, we expand the scope of Kho et al.'s (2025) analysis in two ways. First, we not only examine the impact Indiana charter schools had on test scores but also student absenteeism, which has become a major challenge in addressing learning loss (Dee, 2024; Malkus, 2024; Fuller et al., 2024; Diliberti et al., 2025; Swiderski et al., 2025). According to the U.S. Department of Education, chronic absenteeism reached nearly 31% in the 2021-2022 school year, dropping slightly to 28% in the 2022-23 school year.[1] Given these high rates, it is critical to examine the impact charter schools are having on absenteeism as consistent student attendance is important to academic outcomes, including achievement, grade progression, and high school graduation (Allensworth & Easton, 2007; Gershenson, Jacknowitz, & Brannegan, 2017; Gottfried & Kirksey, 2017).

Second, because prior research has shown that minority and low-performing students experienced greater learning loss during the pandemic (Goldhaber et al., 2022; Kuhfeld, Soland, & Lewis, 2022), we examine the impact charter schools have on these outcomes by subpopulations, including racial/ethnic groups and by prior academic performance. Taken together, our analysis offers a deeper look at how these two facets of the public sector (traditional and charter) have impacted student recovery from learning loss and absenteeism, particularly among students from disadvantaged groups. To do so, we use an administrative dataset comprising longitudinal student-level records provided by the Indiana Department of Education.

Literature Review

While there is a large body of literature examining the impact of charter schools on student outcomes,[2] there has been almost no research evaluating charter school performance since the pandemic. The two exceptions are an analysis of Ohio (Lavertu, 2024) using school-level data, which makes it difficult to account for changing student populations over time, and Kho et al.'s (2025) working paper evaluating Tennessee charter schools. In the case of Tennessee, the authors

[1] https://www.ed.gov/teaching-and-administration/supporting-students/chronic-absenteeism
[2] See Harris (2025) and Zimmer et al. (2019) for recent literature reviews

employed student-level data using a quasi-experimental approach and found that charter schools were performing on par with TPS during the pandemic period but outperforming TPSs in the post-pandemic period. Kho et al. did not explore other student outcomes, including absenteeism and performance by subpopulations.

These gaps in our knowledge are important to explore. For instance, previous research has shown that absenteeism can directly (through reduced instruction time) (Auceio & Romano, 2016; Gershenson et al., 2017; Gottfried, 2011; Liu et al., 2021) and indirectly (disruptive environment) affect student achievement (Gottfried & Ansari, 2022), which likely slowed post-pandemic academic recovery. In addition, research has shown that the COVID-19 pandemic had heterogeneous effects across racial, income, and ability groups, with students of color, students in low-income schools, and lower-performing students more adversely affected (Goldhaber et al., 2022). In a study of over 5 million US students in grades 3-8, test score gaps between students in high-poverty and low-poverty schools grew by 0.20 SD in math and 0.13 SD in reading (Kuhfeld, Soland, & Lewis, 2022). Test score gaps by race/ethnicity grew in both subjects as well, but especially in math. For example, the gap between Hispanic third graders and the national average grew from 0.30 SDs prior to the pandemic to 0.61 SDs in the fall of 2021. Similar changes were observed among Black students.

In this current study, we examine the performance of Indiana charter schools relative to TPS both in student achievement and absenteeism across all students and subgroups. The results have implications for whether absenteeism can explain any variation in performance we observe across charter and TPSs, as well as whether charters have had an impact in ameliorating the achievement gaps by race/ethnicity and income that have widened since the pandemic.

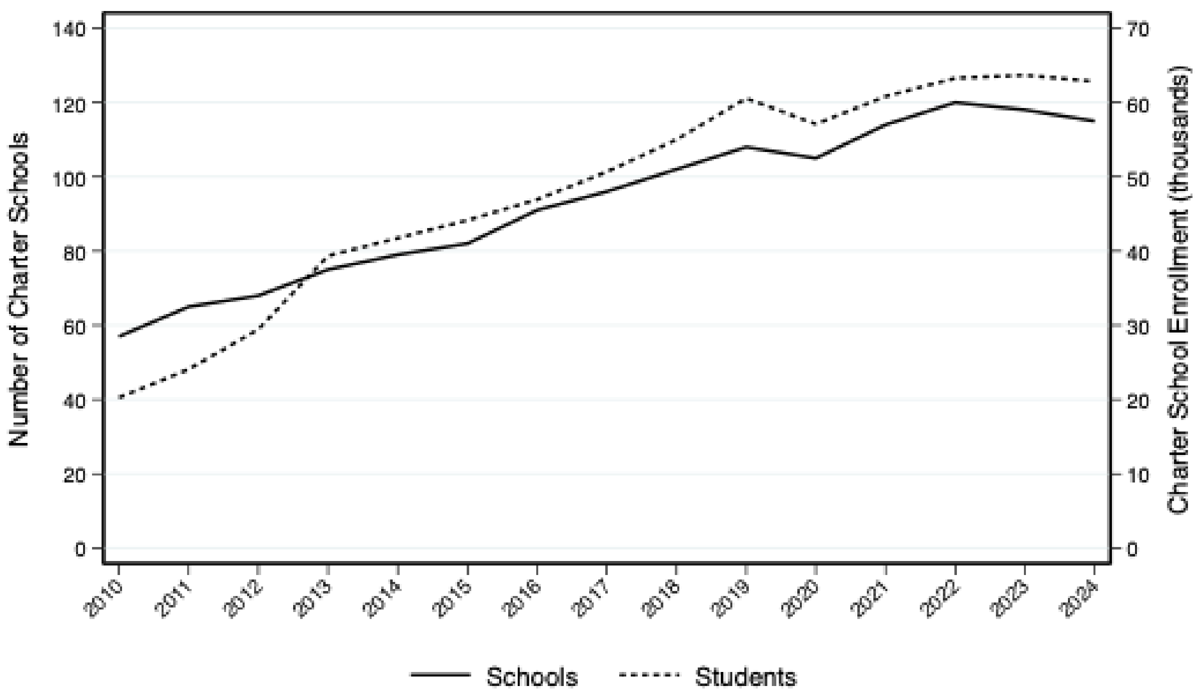
Conceptual Discussion

There is an extensive body of research on the impacts of charter schools relative to TPSs, and the primary takeaway is that those impacts are highly variable [for reviews, see Harris (2025) and Zimmer et al. (2019)]. Ostensibly, charter schools, which are publicly funded schools of choice, have greater autonomy over curricular and instructional decisions in hopes of creating schools that are more responsive to families' needs through innovation (Finn, 2000). Therefore, it is possible that charter school autonomy led them to adapt more quickly to the pandemic challenges. Boast et al. (2020) examined publicly available plans of charter schools and TPSs and found that charter schools were more likely to set expectations that teachers provide real-time instruction, regular check-ins with students, and monitor attendance. Other studies have also found that charter schools had preexisting mentoring models and quickly adapted communication systems to conduct daily check-ins with students (CREDO, 2022; Neugebauer, Schoettler, & Marshall, 2024; Vanourek, 2020). A CREDO study (2022), surveying charter schools in California, New York, and Washington, found that it only took, on average, 3.5 days to transition to online instruction and noted that within two months of the onset of the pandemic, 94% had devices with internet connectivity, which highlights the malleability that comes from autonomy. On the flipside, charter schools may not have had the staff capacity to easily apply for federal support, implement best practices, offer summer programs, and support the social and emotional well-being of students, which could minimize their ability to mitigate and address learning loss. With these advantages and disadvantages as context, we examine the performance of Indiana's charter schools.

Background of Indiana Charter Schools

In 2001, Indiana's charter legislation was signed into law, with the first charter school established in 2002. Over the last 15 years, as shown in Figure 1, charter schools and enrollment have grown with a small dip before the pandemic and a rebound of growth during the pandemic. Part of the growth of charter schools and enrollment could be potentially explained by the fact that charter schools in Indiana can be authorized by a wide array of authorizers, including local school boards, higher education institutions, cities, and state charter school boards.[3]

Figure 1
Charter Schools and Student Enrollment in Indiana



Previously, a few studies have evaluated Indiana charter school performance, including a national study of charter schools by CREDO, which disaggregated results by states and found null

[3] National Conference of State Legislatures: https://www.ncsl.org/education/education-choice-state-policy-scan-charter-schools#:~:text=Charter%20schools%20operate%20under%20the,application%20may%20appeal%20the%20decision.

effects for Indiana's charter schools (Raymond et al., 2023). In a study examining Indiana's charter, magnet, and private schools, Berends and Waddington (2018) also found no effects for charter schools. Despite these results, more recent studies have found negative effects associated with virtual charter schools in Indiana. For example, Fitzpatrick et al. (2020) examined Indiana's virtual charter schools and found negative effects on test scores. In a follow-up study, Ferrare et al. (2025) focused on virtual charter high schools' effect on long-term outcomes, including high school graduation, dropout, and college enrollment and found that these schools produced negative effects across these outcomes. In contrast, both of these studies found null effects of Indiana's brick-and-mortar charters. Related to the flexibility of authorizers mentioned previously, Ferrare et al. (2023) explored variation in the charter sector, finding positive achievement effects for the brick-and-mortar schools authorized by the Indianapolis Mayor's Office but negative effects for other authorizers–particularly those choosing to authorize virtual charters. However, none of these studies examine the performance of charter schools during or after the pandemic.[4] We build off this research by examining the performance of Indiana's charter schools both during and after the pandemic.

Indiana COVID Policies and Implications for Charter Schools

Like other states, both Indiana's charter and TPSs switched to remote instruction in March 2020 for the rest of the 2019-20 academic year. In June of that same year, the state released guidelines for schools to consider before reopening, which included requiring face coverings and social distancing rules. With these rules in place, Indiana schools were allowed to reopen for in-

[4] Dallavis & Ponisciak (2025) examined student performance in Catholic and public schools during and after the pandemic using a similar empirical approach to this paper, finding Catholic school students outperformed their public school peers, largely explained by the in-person instruction in Catholic schools during 2020-21.

person instruction beginning July 1, 2020.[5] At the beginning of the 2020-21 school year, the majority of schools were open for in-person instruction with periods of school switching to remote instruction throughout the year based on COVID spread.[6] Charter schools, much like TPSs, varied in the amount of instructional modality throughout the year.[7] Using data provided by the Indiana Department of Education,[8] we examined the portion of days in which charter schools provided in-person instruction relative to TPS in the 2020-21 school year.

Schools were grouped into the three categories based on the average percentage of students receiving in-person each week. These three categories were less than 70% in-person instruction, 70-90% in-person instruction and 90-100% in-person instruction. Charter schools had lower incidence of in-person instruction in the 2020-21 school year as 18.6% of students had 90-100% in-person instruction compared to 35.4% for TPSs. Similarly, 23.3% charter school students had 70-90% in-person instruction, compared to 30.9% for TPS. These comparisons suggest that charter schools had fewer in person instructional days. However, this comparison does not take into account that charter schools were disproportionately located in urban areas and schools in urban areas were more likely to provide remote or hybrid instruction. While not necessarily an apples to apples comparison, it provides some insights into the instructional modality of charter schools, which may have affected student learning.

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[5] https://ballotpedia.org/Documenting_Indiana%27s_path_to_recovery_from_the_coronavirus_(COVID-19)_pandemic,_2020-2021#School_reopenings_and_closures

[6] https://cdn.ymaws.com/www.indiana-asbo.org/resource/collection/13F09444-C611-4280-AAB9-5D2BA0559AA0/PandemicImpactEducation1-22-21.pdf#:~:text=The%202020%2D2021%20school%20year%20began%20with%20school,and%20virtual%20students%20at%20the%20same%20time.

[7] https://www.in.gov/sboe/files/ICSB-Annual-Report_2021.pdf#:~:text=During%20the%202020%2D2021%20school%20year%2C%20attendance%20reporting,submitted%20DAILY%20to%20IDOE%20based%20upon%20the

[8] https://www.in.gov/doe/it/data-center-and-reports/data-reports-archive/#Attendance

Data

For the analysis, we used statewide, longitudinal student-level data provided by the Indiana Department of Education. The data spans from the 2017-18 school year through 2023-24 school year and includes a unique student identifier with the school(s) students attend, the respective grades, math and English/language Arts (ELA) test scores (annually in grades 3-8), number of days in attendance, as well as student characteristics including gender, race/ethnicity, special education, and free and reduced-price lunch status. Because the state of Indiana did not administer a test in the spring of 2020 and because attendance data may not have been reliable during the last two months of remote instruction in that same spring, we do not analyze the impact of charter schools for the COVID period of the 2019-20 school year. Therefore, we examine outcomes of the 2020-21 school year as the "during the pandemic" period. The 2021-22, 2022-23, and 2023-24 school years are defined as "post-pandemic" years.[9] It should be noted that while virtual charter schools are present in Indiana, they were not open long enough to be included in this analysis– only two were in existence long enough to have sufficient longitudinal data; they are at the same address and have the same principal, so they may be effectively one school (which covers different grade levels).

For the achievement analysis, the math and ELA test scores for each year are standardized using statewide test score data. More specifically, we standardize the math and ELA scores by grade and year, with a mean of zero and standard deviation of one. For the absenteeism analysis, we create two "chronically absent" variables—one for unexcused absences and one for total days absent. The rationale for a focus on chronically absent students is that many districts and states

[9] We defined 2020-21 as a pandemic year because there were periods during the school year in which schools were using remote instruction. We define the subsequent years as post pandemic because schools were in person nearly all days during the school year.

have adopted policies to address chronically absent students, which is defined as missing 10% or more days.[10] Therefore, we create a dummy variable of *unexcused* absences with a value of 1 in cases in which a student has 10% or more of *unexcused* school days absent, and 0 otherwise. Similarly, we create a dummy variable of *total* absences with a value of 1 in cases in which a student has 10% or more of *total* school days absent, and 0 otherwise.

In estimating student achievement and absenteeism effects for the 2020-21 school year, we use the respective baseline measures in 2018-19. For the analysis of outcomes for the post-pandemic period of 2021-22 through 2023-24 school years, we use test scores and attendance measures in the 2020-21 school year as the baseline.[11] Given these lags, we include different grades for the different years of analysis. We include students in grades 5-8 in the 2020-21 and 2022-23 analyses, students in grades 4-8 in the 2021-22 analysis, and grades 6-8 in the 2023-24 analysis.

Finally, we should note, while the two outcome variables for absenteeism are dichotomous variables measuring whether a student is chronically absent, we followed the literature (Kirksey & Gottfried, 2021) in creating baseline categorical dummy variables for the baseline measures of absenteeism employed as independent variables: absent not at all, >0% to <5%, 5% to <10%, and 10% or more. This allows us to capture greater variance and better distinguish between students who are near (5% to 10%) and above the cutoff (10% or more) from those who are less of a concern (0% and between 0% and 5%).

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[10] This measure is consistent with the literature (e.g. Kirksey & Gottfried, 2021) and the measure the U.S. Department of Education uses: https://www.ed.gov/teaching-and-administration/supporting-students/chronic-absenteeism#:~:text=Chronic%20absenteeism%20%E2%80%94%20defined%20as%20students,absenteeism%20from%20coast%20to%20coast

[11] Later, in a sensitivity analysis, we use a consistent baseline year of 2018-19 test scores for both the pandemic and post-pandemic periods. We find similar results.

Research Design

When examining charter school performance in general, selection bias is a major concern. Selection bias can result from unobserved differences between students who choose to enroll in charter schools versus those students who do not. For instance, students and families who enroll in charter schools may be more motivated to find a strong school match, which could lead to biased results if not adequately addressed. Our focus in this study is on student performance in charter schools during and after the pandemic. Because COVID occurred as an exogenous shock after many students had already opted to attend a charter school, we must dually address concerns about selection bias related to sector enrollment decisions both prior to and during the pandemic. We detail these procedures below.

First, we restrict our sample of treated charter students to those who were in charter schools both before and after the treatment period. Similarly, we restrict our sample of comparison students to those who were in a TPS both before and after the treatment period. These restrictions eliminate the possibility of students sorting themselves across charter and TPSs based on the pandemic conditions or school practices, including a school's decisions to open for in-person instruction. Second, baseline measures of the outcomes (test scores and absenteeism) for each student are included in the generation of weights (more below) as well as the subsequent weighted regression analysis. The inclusion of the outcome measures at baseline captures much of the student's unobserved characteristics that can affect the outcome measures in non-experimental designs (Bifulco, 2012). Third, we employ a doubly robust approach using propensity scores to construct a weighted comparison group of TPS students. While this approach is not experimental, studies have shown that when approaches addressing selection using observables incorporate baseline outcomes, the results can be consistent with the results from a randomized approach (Abdulkadiroglu et al., 2011; Bifulco, 2012; Cook et al., 2008; Fortson et al., 2015).

While we view these three steps as the most important in mitigating bias, we take additional steps to increase confidence in the validity of our estimate. First, to guard against our results being skewed by the performance of opening or closing schools, our primary sample includes only schools that were open for all academic years for our analysis (2017-18[12] through 2023-24). Second, to focus on the performance of charter students relative to TPS students, we exclude students in magnet, virtual, alternative, and optional enrollment public schools. In subsequent robustness checks, we test the sensitivity of our primary findings to these sample restrictions. We first adjust the samples to include students in schools open in any year during the analysis window (thereby removing the exclusion criteria requiring schools to be open for all six years). We then include students from other school choice sectors in the pool of comparison students.

To construct the weighted sample, we assign inverse probability weights based on propensity score estimates of the likelihood of TPS students attending charter schools (Imbens & Woolridge, 2009; McEachin et al., 2020; Willet & Murnane, 2011). By using weights generated from a propensity score approach, we have a set of TPS control students who, at any given propensity score, have, on average, similar measured baseline outcome measures and demographic characteristics as the treatment set of charter students. These probabilities were obtained through the estimation of the following logistic regression:

$$\text{Charter}_{it} = \beta_0 + \beta_1\text{math}_{it-n} + \beta_2\text{ELA}_{it-n} + \beta_3\text{absenteeism}_{it-n} + \beta_4 X_i + \varepsilon_{it} \qquad \text{(Equation 1)}$$

[12] While our main analyses do not include outcomes from 2017-18, we use this sample criteria to ensure that we are not capturing charter school effects within their first year, in which lower performance is expected (Sass, 2006; Booker et al., 2009; Zimmer, et al, 2009).

The model includes treatment as the outcome (i.e., a student attending a charter school) and student characteristics that predict attending a charter school as covariates, including students' baseline standardized test scores and unexcused absenteeism measures. Additional covariates (X_i) include a student's gender, race/ethnicity, economic disadvantage, special education status, ELL status, grade, and county of school attendance, which serves as a rough proxy for the local schooling market. Propensity score estimation was limited to students who had complete data on all covariates, including baseline measures of outcomes for each analytic sample (i.e., student achievement and absenteeism).[13] We should note that, for our absenteeism analysis, we drop the small number of students who were enrolled for less than 90 days to reduce the influence of outliers and because these students are not functionally attending school.

These propensity scores ($P(X_i)$) were used in the following equation to estimate inverse probability weights for TPS students:

$$w_i = \frac{P(x_i)}{1 - P(x_i)}$$

This estimation procedure gives more weight to TPS students who have larger propensity scores—i.e., look more like the treatment set of charter school students. All charter school students were assigned a weight equal to 1.

To assess the weighting approach, we conducted a balance check in Table 1, which shows student characteristics before and after weighting for the statewide sample included in the student achievement analysis of the COVID period. Similarly, Table 2 shows the statewide sample

[13] To ensure reliability of our estimates, we employed a trimming procedure that excludes students, both TPS and charter, who have an estimated propensity score greater than 0.9. This cut-off is consistent with the upper bound from Crump et al.'s (2009) rule of thumb for trimming. We additionally tested our analysis with trimming at the recommended lower-bound of 0.1 and found that the results were not sensitive to the specification change.

included in the absenteeism analysis during the COVID period. In both tables, values bolded in the unweighted sample are statistically different between the charter and TPS populations. As designed, the weighting approach reduced all observable differences in student characteristics among the charter school and comparison TPS samples. For both the achievement and absenteeism analyses, the balances look similar for the post-COVID period.[14]

Table 1. Baseline Covariate Balance of Charter and TPS Students for Statewide During-COVID Student Achievement Analysis

Characteristic	Unweighted Sample		Math – Weighted Sample		ELA - Weighted Sample	
	Charter	TPS	Charter	TPS	Charter	TPS
Female	49.2%	49.1%	48.1%	48.2%	48.1%	48.2%
Race						
Hispanic	19.8%	16.6%	21.5%	20.9%	21.4%	20.9%
Black	**46.1%**	**18.7%**	48.6%	47.3%	48.6%	47.3%
Asian	1.7%	3.7%	1.5%	1.5%	1.5%	1.5%
Economically Disadvantaged	**73.5%**	**53.3%**	77.7%	77.0%	77.7%	77.0%
Special Education	15.8%	15.6%	17.9%	17.9%	17.9%	17.9%
English Language Learner	11.9%	9.7%	11.6%	11.4%	11.7%	11.4%
Baseline Math Score	**-0.43**	**-0.01**	-0.43	-0.42	-0.43	-0.42
Baseline ELA Score	**-0.39**	**-0.02**	-0.40	-0.39	-0.40	-0.39
Baseline Unexcused Absences	**6.01**	**3.07**	5.24	4.89	5.25	4.90
Baseline Total Absences	**9.39**	**7.06**	8.31	7.94	8.31	7.94
>0 to <5% Unexcused Absences	51.2%	46.8%	53.8%	53.4%	53.6%	53.4%
5% to <10% Unexcused Absences	**16.8%**	**7.7%**	15.0%	14.9%	15.0%	14.9%
>=10% Unexcused Absences	**7.5%**	**2.8%**	5.3%	5.1%	5.4%	5.2%

Notes: Baseline characteristics from 2018-19 are displayed for the respective student pool in the "During-COVID" (2020-21) analysis. Statistically significant differences at the 5% level are bolded. Results for binary variables are represented as a percentage of the sample with the given characteristics. Results for continuous variables represent mean values.

[14] Tables of the balance checks for post-pandemic periods are available upon request.

Table 2. Baseline Covariate Balance of Charter and TPS Students for Statewide During-COVID Absenteeism Analysis

Characteristic	Unweighted Sample		Absenteeism – Weighted Sample	
	Charter	TPS	Charter	TPS
Female	49.1%	49.1%	48.4%	48.1%
Race				
Hispanic	19.8%	16.6%	20.4%	20.9%
Black	**46.1%**	**18.7%**	47.2%	48.1%
Asian	1.7%	3.7%	0.6%	1.4%
Economically Disadvantaged	73.5%	53.3%	76.7%	77.3%
Special Education	15.8%	15.6%	18.3%	17.8%
English Language Learner	11.9%	9.7%	10.7%	11.6%
Baseline Math Score	**-0.43**	**-0.02**	-0.42	-0.44
Baseline ELA Score	**-0.39**	**-0.02**	-0.41	-0.40
Baseline Unexcused Absences	**6.01**	**3.07**	5.59	4.97
Baseline Total Absences	**9.39**	**7.06**	8.81	8.01
>0 to <5% Unexcused Absences	51.2%	46.8%	56.2%	53.4%
5% to <10% Unexcused Absences	**16.8%**	**7.7%**	16.1%	15.1%
>=10% Unexcused Absences	**7.5%**	**2.8%**	5.8%	5.4%

Notes: Baseline characteristics from 2018-19 are displayed for the respective student pool in the "During-COVID" (2020-21) analysis. Statistically significant differences at the 5% level are bolded. Results for binary variables are represented as a percentage of the sample with the given characteristics. Results for continuous variables represent mean values.

Preferred Analytic Model

For all analyses, we use the weighted sample of charter and TPS students in a doubly robust approach by integrating the inverse probability weights into the following regression:

$$Y_{igsdt} = \beta_0 + \beta_1 T_{is} + \beta_2 Y_{igt-n} + \beta_3 Z_{st-n} + \beta_4 X_i + \text{⎳}_g + \text{™}_d + \varepsilon_{igsdt} \qquad \textit{(Equation 2)}$$

We examine four outcomes Y_{igsdt}: standardized math test score, standardized ELA test scores, chronically unexcused absences, and chronically total absences. In the cases of the dichotomous dependent variables (for our absenteeism measures), these models are linear probability models.[15] For each analysis, the outcomes are measured for student i in grade g in school s in county d in year t. In each analysis, student i in school s receives a 1 for the treatment variable (T_{is}) if the student attends a charter school and 0 otherwise. For each analysis, the baseline outcome (Y_{igt-n}) is included as a predictor along with an indicator for a school's average outcome in the baseline year (Z_{st-n}). For the achievement analyses, the baseline outcomes are the math and ELA test scores. For attendance analyses, the baseline outcome is the categorical variables of attendance described in the previous data section. Additionally, the estimations include the same vector of student characteristics (X_i) used in the estimation of propensity scores for the weights. Finally, we employ grade ($\angle_g$) and county (™$_d$) fixed effects to ensure that treatment effects reflect a comparison among students in the same grade and county. The treatment effect of attending a charter school is provided by the coefficient of T_{is} in each analysis and can be interpreted as the difference in charter school students' outcomes attributable to attending a charter school rather than TPS. In all models, standard errors are clustered at the school level.

Results

In Table 3, we display the estimates of interest for the math and ELA achievement analyses, while Table 4 displays the estimates for absenteeism. The first set of columns shows the performance of students in charter schools relative to similar TPS students for the 2020-21 school year, which we define as the during-pandemic period. The second, third, and fourth columns show

[15] As a sensitivity analysis, we employ a logistic regression model. In general, the results are very similar to the linear probability model.

the same comparisons for the 2021-22, 2022-23, and 2023-24 school years, which we define as the post-pandemic periods. The full results are shown in Table A-1 in the appendix.

Table 3. Student Achievement Performance of Charter School Students Relative to Traditional Public School (TPS) Students During and Post-Pandemic

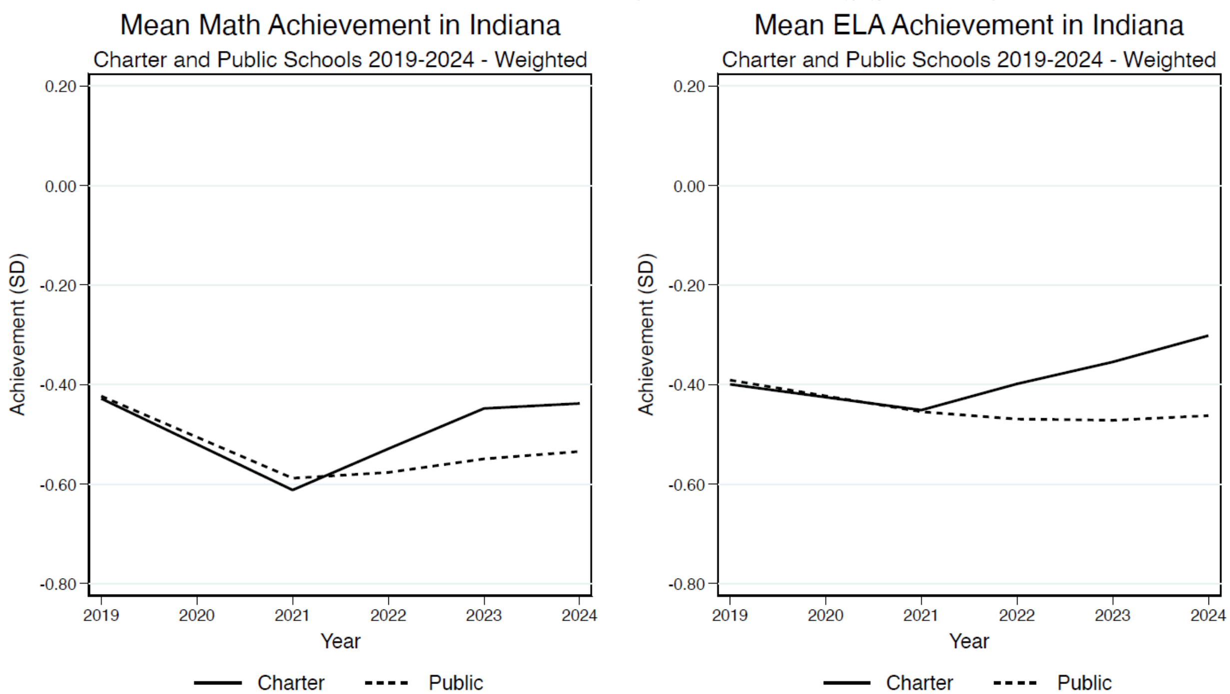
	COVID (2021)		**Post-COVID (2022)**		**Post-COVID (2023)**		**Post-COVID (2024)**	
	Math	ELA	Math	ELA	Math	ELA	Math	ELA
Charter Schools	0.027 (0.024)	0.083*** (0.025)	0.070* (0.029)	0.097*** (0.025)	0.133** (0.050)	0.145*** (0.032)	0.116 (0.069)	0.173*** (0.042)
N	95912	95974	103333	103396	75101	75140	46880	46933

Notes: All models include grade and county fixed effects. Standard errors are clustered at the school level and are reported in parentheses. * for p<0.05, ** for p<0.01, and *** for p<0.001

Table 3 suggests that charter school student achievement was mixed during the pandemic period (2021) as charter students performed on par with TPS students in math, while outperforming TPS students in ELA with a modest effect size of 0.08 standard deviations. In the post-pandemic period, charter schools outperformed TPS students in ELA across all years. In math, charter students also outperformed TPS students in all years except the 2023-24 school year, when the effect was marginally insignificant despite a similar effect size (0.12 standard deviations) to the previous year of 2023. The effect sizes are modest in size in the 2021-22 school year but reach moderate to large magnitudes in the 2022-23 and 2023-24 school years. As a source of comparison, Kho et al. (2025) found no effects during the COVID period but found effects of similar magnitudes across the post-pandemic periods, which suggests that charter schools may have been more successful in increasing student achievement gains in post-pandemic periods.

It should be noted that during the COVID period, all schools experienced learning loss, including charter schools. Therefore, it is not clear whether the observed positive effects for student achievement gains in the post-pandemic periods would necessarily lead to pre-pandemic levels of achievement. To gain a better sense of the overall achievement trends of charter and TPSs, in Figure 2, we graph the descriptive achievement levels of both sectors using the same sample restrictions and weighting approach in Table 3. However, for the graph, while we standardize the test scores by grade (with a mean of zero and standard deviation of 1), we do not standardize them by year so that we can examine achievement trends over time. As can be seen in the figure, math and ELA achievement levels are similar across the two sectors during the COVID period, with slightly better trends in ELA. However, in post-pandemic periods, charter schools outperformed TPSs. Charter schools regain pre-pandemic achievement levels, while TPSs do not.

Figure 2
Math and ELA Achievement Levels of Charter and TPS Schools Over time



In Table 4, we show the estimates of interest for our analysis of absenteeism rates of charter school students relative to TPS students. Table A-2 in the appendix shows the full results, including all covariate estimates. The results indicate that charter schools had no effect on absenteeism across both measures and all periods, which suggests that improved attendance in charter schools is not a possible explanation for the positive achievement effects.

Table 4. Excessive Absenteeism Rate of Charter School Students Relative to Traditional Public School (TPS) Students During and Post-Pandemic

	COVID (2021)		**Post-COVID (2022)**		**Post-COVID (2023)**		**Post-COVID (2024)**	
	Unexcused	All	Unexcused	All	Unexcused	All	Unexcused	All
Charter Schools	0.008 (0.023)	-0.002 (0.025)	0.007 (0.015)	0.023 (0.018)	0.000 (0.016)	0.020 (0.017)	-0.001 (0.016)	-0.004 (0.019)
N	89495	89495	102594	102594	76382	76382	47262	47262

*Notes: All models include grade and county fixed effects. Standard errors are clustered at the school-level and are reported in parentheses. * for p<0.05, ** for p<0.01, and *** for p<0.001*

In addition to absenteeism, modality of instruction could also mediate our estimates. As noted in the background discussion of Indiana charter schools, charter schools had less in-person instruction. In order to examine the potential impact of the mode of instruction, we utilized data collected by the state of Indiana that measured the frequency with which schools provided remote, in-person, or hybrid instruction during the 2020-21 school year. State IDOE staff aggregated daily student attendance by week; each day, students could be listed as virtual due to COVID, virtual, in attendance, tardy, or exempt.[16] In each week, the student's dominant mode was defined as in-person (in-person at least 75%), hybrid (in-person 30-67%), virtual due to COVID (at least 60%), or virtual (at least 60%). These dominant student modes of instruction were aggregated to the school level for each week. The dominant model for the school was then defined as the mode that had at least 50% of students in that week. These results were then aggregated to provide the number of weeks for each mode for each school, which was then categorized into ten categories (0-10, 10-20,..., 90-100). In our sample, about 34 percent of schools were listed as providing in-person instruction in at least 90% of weeks, while another 31 percent met this criterion in 70 to 90 percent

[16] Mode of instruction data was available for 72 percent of schools (both charter and TPS).

of weeks, leaving the remaining schools providing in-person instruction in less than 70 percent of weeks.

Using these data, we reestimate our preferred model using only schools that were included in the data for mode of instruction, to provide an accurate baseline measurement, and subsequently estimated models that included 2020-21 mode of instruction indicators into one of three categories of 0-70% in-person instruction, 70-90% in-person instruction, with 90-100% in-person instruction as the omitted category. We estimated the models for both the 2020-21 school year analysis as well as analyses for 2020-21 through 2023-24 school years. Our results were minimally affected - on average by less than 4% - by the inclusion of these controls. Therefore, like absenteeism, instructional modality does not appear to explain the positive charter school student achievement results.

Heterogenous Effects

In addition to understanding the overall impact of charter schools during and after the pandemic, it is also important to examine the performance of subpopulations. Because we only find a statistically significant effect for student achievement, we focus the heterogeneous analysis only on student achievement and do not examine absenteeism. To examine the heterogeneous effects, we build on our preferred estimation approach (see equation 1) by including an interaction term between the treatment status of attending a charter school and individual student characteristics of gender, ESL, special education, and economically disadvantaged status, as well as race/ethnicity and the student's achievement level at baseline.

We estimate the interaction term coefficients in separate models for math and ELA for each student characteristic. For instance, we estimate the interactions for race/ethnicity by interacting the race/ethnicity indicators of White, Black, and Hispanic students with charter status in two separate analyses, one for math and one for ELA. Similarly, for baseline student achievement level, we group students into the tercile categories of top, middle, and low achievement levels using categorical variables. These categorical variables are interacted with charter status in separate analyses for each subject. In total, across the student characteristics, 12 models are estimated for the six types of student characteristics. For each model, we compute the difference between estimates for charter and TPS students with the respective levels of each variable, and the associated standard error. We show these estimates in Table 5.

In general, the results suggest that Black, economically disadvantaged, and the lowest-performing students had higher achievement in charter schools relative to TPSs across both subjects and academic years, both during and after the pandemic. Hispanic students had higher achievement during the pandemic and in some years and subjects, post-pandemic. In contrast, White, non-economically disadvantaged, and the highest-performing students did not, in most years, suggesting that disadvantaged students largely drive the positive effects observed in the main analysis.

Table 5. Estimates for Subpopulations

	COVID (2021)		Post-COVID (2022)		Post-COVID (2023)		Post-COVID (2024)	
	Math	**ELA**	**Math**	**ELA**	**Math**	**ELA**	**Math**	**ELA**
Black	0.047	0.088**	0.160***	0.162***	0.224***	0.227***	0.250***	0.274***
	(0.029)	(0.034)	(0.026)	(0.029)	(0.048)	(0.037)	(0.055)	(0.055)
Hispanic	0.098**	0.125**	0.002	0.051	0.098	0.118**	0.067	0.149*
	(0.034)	(0.042)	(0.036)	(0.030)	(0.062)	(0.040)	(0.096)	(0.066)
White	-0.064	0.042	-0.043	0.014	0.015	0.034	-0.049	0.038
	(0.037)	(0.027)	(0.041)	(0.034)	(0.061)	(0.042)	(0.076)	(0.045)
N	95912	95974	103333	103396	75101	75140	46880	46933
ESL	0.060	0.089	0.018	0.067	0.127*	0.133**	0.113	0.180**
	(0.049)	(0.058)	(0.039)	(0.038)	(0.060)	(0.040)	(0.079)	(0.058)
non-ESL	0.023	0.082*	0.080**	0.102***	0.135**	0.147***	0.117	0.171***
	(0.024)	(0.025)	(0.031)	(0.026)	((0.051)	(0.034)	(0.070)	(0.044)
N	95912	95974	103333	103396	75101	75140	46880	46933
Special Ed	0.050	0.104**	0.004	0.075*	0.039	0.116***	0.048	0.152***
	(0.027)	(0.032)	(0.025)	(0.031)	(0.048)	(0.029)	(0.059)	(0.043)
Non-Spec Ed	0.022	0.078**	0.085**	0.101***	0.154**	0.152***	0.132	0.178***
	(0.026)	(0.026)	(0.032)	(0.027)	(0.053)	(0.037)	(0.074)	(0.047)
N	95912	95974	103333	103396	75101	75140	46880	46933
Male	0.040	0.099***	0.086**	0.116***	0.146**	0.165***	0.127	0.209***
	(0.025)	(0.026)	(0.031)	(0.026)	(0.050)	(0.031)	(0.065)	(0.039)
Female	0.013	0.065*	0.054	0.076**	0.120*	0.123***	0.105	0.133**
	(0.025)	(0.027)	(0.029)	(0.027)	(0.053)	(0.037)	(0.075)	(0.051)
N	95912	95974	103333	103396	75101	75140	46880	46933
Econ Disadv	0.045	0.097***	0.089**	0.107***	0.164**	0.160***	0.151*	0.190***
	(0.025)	(0.028)	(0.029)	(0.024)	(0.051)	(0.034)	(0.074)	(0.048)
Non-Econ	-0.037	0.032	0.010	0.065*	0.034	0.097**	0.000	0.115**
Disadv	(0.032)	(0.025)	(0.035)	(0.032)	(0.051)	(0.035)	(0.060)	(0.038)
N	95912	95974	103333	103396	75101	75140	46880	46933
Lowest	0.055*	0.112***	0.078**	0.118***	0.137***	0.183***	0.150**	0.211***
Tercile	(0.023)	(0.030)	(0.025)	(0.027)	(0.037)	(0.031)	(0.047)	(0.039)
Middle	0.042	0.077**	0.078	0.095**	0.170*	0.134**	0.087	0.158**
Tercile	(0.033)	(0.029)	(0.041)	(0.034)	(0.068)	(0.045)	(0.091)	(0.058)
Top	-0.053	0.037	0.034	0.038	0.068	0.054	0.051	0.074
Tercile	(0.032)	(0.028)	(0.045)	(0.023)	(0.079)	(0.045)	(0.120)	(0.066)
N	95912	95974	103333	103396	75101	75140	46880	46933

Notes: All models include grade and county fixed effects. Standard errors are clustered at the school-level and reported in parentheses. * for p<0.05, ** for p<0.01, and *** for p<0.001

Robustness Checks

Next, we examine whether our main achievement results are sensitive to our methodological choices. Because the coefficients in our main absenteeism results and the respective robustness checks are not statistically significant, we do not include the robustness check results here, other than to say that the robustness checks are very similar to the main results in magnitude and conclusions. The summary table of the results from the absenteeism robustness checks can be viewed in Appendix Table A-3. Table 6 highlights the results of six robustness checks for the primary student achievement analysis. We display results for each school year in separate panels. The results from the main analysis are the first set of rows.

Our first set of robustness checks examines the sensitivity of our estimates to our sample restrictions. Our first restriction in the primary analyses limited our comparison group to only TPS students to ensure the effect of charter schools was estimated against traditional schooling environments rather than alternative educational interventions. However, in many urban environments, magnet and optional enrollment schools are common and could be considered a strong counterfactual. Therefore, we conduct a robustness check that includes magnet and open enrollment school students as part of the comparison group, shown in the second set of rows in Table 8.[17] As evident in the table, the magnitudes of the coefficient estimates, along with the substantive conclusions, are similar to the main analysis, which suggests that our results are not sensitive to the exclusion restriction.

[17] We continue to omit alternative and virtual TPSs in our comparison pool as students in these settings are likely different from those selecting into charter schools. Students in alternative schools may have been assigned to, rather than selected into, such schools based on prior low academic performance or behavioral concerns. Students in virtual TPSs likely have circumstances such that they are seeking for a learning environment completely online.

Table 6. Robustness Achievement Analyses

	COVID (2021)		**Post-COVID (2022)**		**Post-COVID (2023)**		**Post-COVID (2024)**	
	Math	ELA	Math	ELA	Math	ELA	Math	ELA
Main Analysis	0.027	0.083***	0.070*	0.097***	0.133**	0.145***	0.116	0.173***
	(0.024)	(0.025)	(0.029)	(0.025)	(0.050)	(0.032)	(0.069)	(0.042)
N	95912	95974	103333	103396	75101	75140	46880	46933
Magnet Students	0.027	0.085***	0.070*	0.096***	0.135**	0.139***	0.119	0.170***
	(0.024)	(0.024)	(0.030)	(0.025)	(0.050)	(0.032)	(0.067)	(0.041)
N	99962	100004	106775	106830	78166	78195	49176	49233
ITT	0.015	0.064**	0.057*	0.076***	0.095**	0.103***	0.085*	0.103***
	(0.019)	(0.021)	(0.023)	(0.019)	(0.034)	(0.023)	(0.039)	(0.028)
N	101750	101806	121979	122065	90559	90609	64182	64267
Opening / Closing Schools	0.031	0.096***	0.076**	0.098***	0.125**	0.145***	0.107	0.167***
	(0.022)	(0.023)	(0.027)	(0.023)	(0.045)	(0.029)	(0.067)	(0.041)
N	119845	119895	124957	125114	91388	91422	62839	62901
1:1 Matching	0.016	0.079**	0.054	0.089***	0.125*	0.160***	0.127	0.203***
	(0.026)	(0.025)	(0.028)	(0.025)	(0.049)	(0.032)	(0.068)	(0.043)
N	11721	11759	11729	11741	7450	7440	4452	4461
Baseline 2018-19	0.027	0.083***	0.087*	0.159***	0.100	0.192***	0.089	0.212***
	(0.024)	(0.025)	(0.043)	(0.034)	(0.055)	(0.043)	(0.070)	(0.049)
N	95912	95974	58371	58426	35238	35247	15159	15171
Common Support	0.027	0.083***	0.070*	0.097***	0.134**	0.145***	0.116	0.173***
	(0.024)	(0.025)	(0.029)	(0.025)	(0.050)	(0.032)	(0.069)	(0.042)
N	93809	93870	102978	103041	73966	74004	46520	46573

Notes: All models include grade and county fixed effects. Standard errors are clustered at the school-level and reported in parentheses. * for p<0.05, ** for p<0.01, and *** for p<0.001

The second set of robustness checks evaluates our choice to restrict our analysis to students who made nonstructural school changes after the pandemic. As noted previously, we chose to exclude "switchers" because the choice to switch schools during and after the pandemic may have been partially driven by school performance or other reasons related to the COVID-19 pandemic, which could introduce endogeneity. To examine this sample restriction, we employ an intent-to-treat (ITT) approach in which students' enrollment is based on their enrollment at the baseline year of 2018-19. In other words, even if a student switches from a charter school (or TPS), that student's enrollment will be assigned to a charter school (or TPS) across all periods for the sake of our analysis. The results are shown in the third set of rows in Table 5. While the coefficient estimates are more modest across the pandemic and post-pandemic periods for the student achievement results, the substantive conclusions remain.

Next, we checked the robustness of our results based on our decision to exclude schools that opened, closed, or did not have achievement data for all years during our analytic period. The decision to restrict these schools is based on the fact that these schools cannot contribute to both pandemic and post-pandemic estimates, limiting the comparability of our results across periods. The results are shown in the fourth set of rows, which shows that the magnitudes of the coefficient estimates are similar to the main results. For our next check, we use a 1-to-1 matching approach, a common approach used in charter school effectiveness research, instead of inverse probability analysis weights. The coefficient estimates, as shown in the fifth set of rows, generally decrease in magnitude, but the substantive conclusions remain across time periods. Our fifth robustness check tests the choice of using separate baseline years for the pandemic and post-pandemic years. The choice to use the 2020-21 school year as the baseline for the post-pandemic year allows our analysis to increase our sample size but comes with the possible bias of students switching because of COVID and COVID school practices. In this robustness check, we use a constant baseline year of 2018-19 across both the pandemic and post-pandemic analyses, which reduces any possible bias from endogenous switching of schools, but reduces our sample size substantially, especially for the last two years of the post-pandemic analysis, which include students in grades 7 and 8 in 2022-23, and only those in grade 8 in 2023-24. The coefficient estimates shown in the sixth set of results are similar in magnitude to our main set of results. Finally, we test the robustness of the main results to the use of common support. The results are shown in the final set of rows and are almost identical to our main set of estimates.

Across the robustness checks, our results are robust to our analytical and data restriction choices, providing greater confidence in our main results.

Selection on Unobservable Characteristics

As a result of concerns about whether quasi-experimental analyses eliminate all possible selection bias, researchers have developed approaches to estimate how important unobservable covariates would have to be to nullify estimated statistically significant results. We employ two popular approaches by Oster (2019) and Frank et al. (2013) to quantify the issue of selection on unobservables for our statistically significant results from the achievement analysis. We do not apply these postestimation analyses to the absenteeism results because no statistically significant effects were observed. The results for these analyses on student achievement effects are presented in Table 7.

We first employ Oster's (2019) approach, which refines the assumptions of earlier work by Altonji et al. (2005) on unobservable selection bias and coefficient stability for linear models. Oster's (2019) approach includes two measures: (1) the ratio of unobservable to observable characteristics (δ) that would be needed to invalidate the conclusion and (2) a lower bound estimate of the treatment effect (β) assuming that $\delta = 1$. Given that this approach is solely validated for linear models, we estimate the measures for our inverse-probability weighted regressions. While selection bias is a concern when estimating first-stage propensity scores, the predicted values from the logistic regressions are incorporated into the second-stage models that are assessed for sensitivity to unobservable characteristics. As seen in Table 6, for statistically significant results in our achievement analysis, the estimated δ values for our models range from -11.834 to -5.670. The estimated δ values are negative, indicating negative selection into charter schools based on observed characteristics. Under this pattern, unobserved factors would need to be positively correlated with both charter enrollment and achievement—and sufficiently strong relative to selection on observables—to fully attenuate the estimated effects. Consistent with this interpretation, the Oster (2019) bounds—computed under the assumption that selection on unobservables is proportional to selection on observables—exceed our main estimates, suggesting that our estimated effects may be conservative.

Complementing these measures, we also employ Frank et al.’s (2013) approach, which quantifies (as a percentage) the extent of bias necessary in a given estimate to nullify the causal inference. For our statistically significant results, we find that as low as 18% and as high as 56% of the estimates would have to be attributed to bias to invalidate our results. While there is no standard benchmark of this measure that constitutes a robust inference, this measure has been estimated for all studies included in the What Works Clearinghouse, which represent the highest quality of causal inferences. When compared to the distribution of this measure for quasi-experimental studies with the same outcome, our estimates for math outcomes are found to be more robust than at least 16% of studies included in the What Works Clearinghouse (KonFound-It, n.d.). When assessing our estimated effects on ELA standardized test scores, our estimates are more robust than at least 35% of studies. These comparisons suggest that our models meet current standards for accounting for selection bias.

Table 7. Robustness to Selection on Unobservable Characteristics

	COVID (2021)		**Post-COVID (2022)**		**Post-COVID (2023)**		**Post-COVID (2024)**	
	Math	ELA	Math	ELA	Math	ELA	Math	ELA
Main Estimate	0.027	0.083***	0.070*	0.097***	0.133**	0.145***	0.116	0.173***
δ (unobs./obs. for null result)	-1.906	-5.670	-8.019	-10.365	-7.947	-11.360	-7.629	-11.834
Lower bound of β at δ = 1	0.039	0.094	0.075	0.103	0.140	0.152	0.121	0.178
% bias for null result	NA	40.93%	18.08%	49.82%	26.63%	56.25%	NA	52.57%

Discussion and Conclusion

This study expands upon emerging work focused on the effects of charter schools in the post-pandemic era. As we noted above, there are plausible reasons why charter schools may have been both limited or poised to meet students' academic needs during and after the COVID-19 pandemic. On the one hand, charter schools may not have had the same level of administrative infrastructure to apply for federal funds, implement best practices, offer summer programs, or support the social and emotional well-being of students. On the other hand, they may have the advantage of greater autonomy to adapt quickly to changing modalities and pivot to new practices to address learning loss. An analysis that provides insights into the effectiveness of charter schools is important, as both TPSs and charter schools are still grappling with the learning loss from the pandemic. To the degree that charter schools are having greater success, it could create an opportunity to learn more about their practices to inform all schools in addressing learning loss.

While there has been a large set of studies evaluating the performance of charter schools, almost no studies have evaluated the effectiveness of charter schools during and after the pandemic. The only two exceptions we are aware of are a working paper examining the impact of charter schools in Tennessee (Kho et al., 2025) and a school-level analysis of Ohio charter schools

(Lavertu, 2024). Kho et al. (2025) found no effects during the COVID period but found positive effects of similar magnitudes across the post-pandemic periods.

Overall, we found mixed results for charter schools during the pandemic as charter schools had no statistically significant effect in math but moderate, positive impacts on student achievement in ELA (0.08 standard deviations). Post-pandemic, we found more consistent positive and statistically significant effects ranging from a small-to-moderate effect size of 0.07 to a moderate-to-large effect size of 0.17. These findings are notable given research on Indiana's brick-and-mortar charters prior to the pandemic that showed null impacts on student achievement (Fitzpatrick et al., 2020). The findings in this analysis suggest a shift occurred during the pandemic and its aftermath. Not only are students in Indiana's charter schools experiencing more accelerated test score gains than their TPS peers, but these impacts appear to be growing over time in the post-pandemic period. This recent growth trajectory of students in charter schools represents a departure from the pre-pandemic era trends (null). Further, the positive effects we observed overall were driven by students from historically marginalized groups (Black, Hispanic, and low-income) and those students scoring within the lowest tercile of the achievement distribution.

A second contribution of our study was to evaluate two potential mechanisms that could shed light on the main effects. In both cases, our analysis yielded counter-intuitive results. Chronic absenteeism, for example, has been widely considered a major challenge for educational leaders as they work to address students' learning loss resulting from the pandemic (Dee, 2024; Diliberti et al., 2025). If charter schools were having greater success in reducing chronic absenteeism, this could at least partially explain the positive impacts they are having on student achievement. However, we found no differences between charter and TPSs, which suggests that absenteeism is an unlikely explanation for the difference in student achievement gains we observed for Indiana's charter schools.

We also tested for differences in school instructional modality across the pandemic period, as the positive impacts we observed among charters may have been a function of charters offering more in-person instruction. However, during the 2020-2021 academic year, charter schools actually offered less in-person instruction than TPSs, and controlling for this difference did little to explain the estimated differences in achievement. Taken together, the positive effects of charter schools in the post-pandemic period does not appear to be a simple case of students attending school more consistently or having experienced more in-person instruction during the lockdown periods of 2020-2021. Thus, further research is needed to understand the extent to which specific educational and operational practices of charter schools may have driven the positive effects.

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Appendix

Table A-1: Full Results of Primary Achievement Analysis

	COVID (2021)		Post-COVID (2022)		Post-COVID (2023)		Post-COVID (2024)	
	Math	ELA	Math	ELA	Math	ELA	Math	ELA
Charter	0.027 (0.024)	0.083*** (0.025)	0.070* (0.029)	0.097*** (0.025)	0.133** (0.050)	0.145*** (0.032)	0.116 (0.069)	0.173*** (0.042)
Female	-0.025** (0.008)	0.090*** (0.009)	-0.021** (0.008)	0.053*** (0.009)	-0.030* (0.012)	0.059*** (0.010)	-0.015 (0.015)	0.072*** (0.016)
Hispanic	-0.000 (0.023)	0.020 (0.017)	-0.008 (0.026)	0.012 (0.022)	-0.011 (0.028)	0.018 (0.024)	0.001 (0.034)	0.060 (0.035)
Black	-0.157*** (0.026)	-0.147*** (0.023)	-0.036 (0.027)	-0.065** (0.022)	-0.059* (0.029)	-0.060* (0.027)	-0.005 (0.032)	-0.006 (0.037)
Asian	0.140** (0.043)	0.158*** (0.040)	0.029 (0.038)	0.132*** (0.040)	0.097 (0.061)	0.090 (0.060)	0.109 (0.069)	0.169*** (0.051)
Native American	0.061 (0.068)	-0.238 (0.174)	0.051 (0.053)	-0.139 (0.127)	0.001 (0.062)	-0.139 (0.196)	-0.177 (0.159)	-0.225 (0.230)
Native Hawaiian or Pacific Islander	0.044 (0.099)	-0.056 (0.159)	0.033 (0.053)	-0.161 (0.107)	0.194** (0.059)	0.224*** (0.063)	-0.207 (0.173)	-0.026 (0.087)
Economically Disadvantaged	-0.097*** (0.013)	-0.105*** (0.012)	-0.019 (0.013)	-0.057*** (0.010)	-0.014 (0.020)	-0.068*** (0.015)	-0.040 (0.031)	-0.086*** (0.024)
Special Education	-0.098*** (0.013)	-0.228*** (0.016)	-0.164*** (0.014)	-0.255*** (0.019)	-0.172*** (0.018)	-0.345*** (0.019)	-0.160*** (0.024)	-0.369*** (0.024)
English Learner	-0.066** (0.022)	-0.184*** (0.026)	-0.046* (0.021)	-0.140*** (0.019)	-0.065** (0.024)	-0.147*** (0.021)	-0.104*** (0.031)	-0.171*** (0.035)
Baseline Math Score	0.682*** (0.010)		0.784*** (0.008)		0.720*** (0.011)		0.700*** (0.013)	
Baseline ELA Score		0.626*** (0.007)		0.688*** (0.009)		0.650*** (0.009)		0.608*** (0.012)
School Avg. Baseline Math	0.226*** (0.029)		0.138*** (0.033)		0.172* (0.075)		0.113 (0.106)	
School Avg. Baseline ELA		0.239*** (0.041)		0.166*** (0.035)		0.159*** (0.045)		0.158* (0.061)
Constant	-0.039 (0.037)	-0.133** (0.050)	0.018 (0.037)	-0.039 (0.071)	0.037 (0.091)	0.082 (0.075)	0.024 (0.064)	0.067 (0.067)
N	95912	95974	103333	103396	75101	75140	46880	46933

Notes: All models include grade and county fixed effects. Standard errors are clustered at the school-level. * for p<0.05, ** for p<0.01, and *** for p<0.001

Table A-2: Full Results of Primary Absenteeism Analysis

	COVID (2021)		Post-COVID (2022)		Post-COVID (2023)		Post-COVID (2024)	
	Unexcused	All Absences	Unexcused	All Absences	Unexcused	All Absences	Unexcused	All Absences
Charter	0.008	-0.002	0.007	0.023	0.000	0.020	-0.001	-0.004
	(0.023)	(0.025)	(0.015)	(0.018)	(0.016)	(0.017)	(0.016)	(0.019)
Female	-0.015*	-0.012	0.001	0.005	0.004	0.010	0.005	0.004
	(0.008)	(0.008)	(0.004)	(0.005)	(0.005)	(0.006)	(0.007)	(0.009)
Hispanic	0.020	0.035	-0.006	-0.010	-0.003	-0.015	-0.040**	-0.047**
	(0.016)	(0.018)	(0.010)	(0.013)	(0.009)	(0.012)	(0.015)	(0.016)
Black	0.097***	0.118***	0.023	0.002	0.007	-0.028	-0.039*	-0.061***
	(0.021)	(0.022)	(0.014)	(0.017)	(0.013)	(0.017)	(0.015)	(0.015)
Asian	0.008	-0.007	-0.028	-0.019	-0.019	-0.076***	-0.053**	-0.084*
	(0.029)	(0.030)	(0.019)	(0.041)	(0.011)	(0.020)	(0.017)	(0.033)
Native American	-0.072	-0.099	-0.019	-0.080*	-0.061*	-0.100**	0.081	-0.002
	(0.054)	(0.063)	(0.037)	(0.040)	(0.024)	(0.037)	(0.092)	(0.080)
Native Hawaiian or Pacific Islander	-0.036	0.039	0.157	0.215	-0.068*	-0.120*	0.021	-0.006
	(0.047)	(0.052)	(0.085)	(0.132)	(0.029)	(0.054)	(0.041)	(0.048)
Multiracial	0.061***	0.082***	0.012	0.022	0.029*	0.032*	-0.028	-0.008
	(0.016)	(0.018)	(0.011)	(0.017)	(0.012)	(0.015)	(0.022)	(0.018)
Economically Disadvantaged	0.086***	0.108***	0.016*	0.047***	0.021**	0.038***	0.026***	0.065***
	(0.008)	(0.010)	(0.006)	(0.008)	(0.007)	(0.009)	(0.006)	(0.009)
Special Education	0.014	0.026**	0.036***	0.044***	0.013	0.034***	0.020*	0.035**
	(0.009)	(0.009)	(0.008)	(0.010)	(0.008)	(0.009)	(0.008)	(0.011)
English Language Learner	0.063**	0.069**	-0.017	-0.020	-0.031**	-0.052***	-0.024*	-0.032
	(0.021)	(0.021)	(0.009)	(0.013)	(0.011)	(0.014)	(0.011)	(0.017)
Baseline Unexcused Absences >0 to <5%	0.109***		0.018**		0.027**		0.029***	
	(0.012)		(0.006)		(0.008)		(0.007)	
Baseline Unexcused Absences 5 to <10%	0.346***		0.115***		0.109***		0.098***	
	(0.016)		(0.010)		(0.014)		(0.015)	
Baseline Unexcused Absences 10% or more	0.470***		0.334***		0.239***		0.285***	
	(0.024)		(0.016)		(0.017)		(0.020)	
Baseline Any Absences >0 to <5%		0.065***		0.052***		0.069***		0.068***
		(0.018)		(0.011)		(0.015)		(0.013)
Baseline Any Absences 5 to <10%		0.251***		0.207***		0.190***		0.184***
		(0.021)		(0.015)		(0.020)		(0.017)
Baseline Any Absences 10% or more		0.413***		0.433***		0.373***		0.381***
		(0.021)		(0.019)		(0.021)		(0.022)
School Average Baseline Unexcused Absences 5 to <10%	0.078		0.048***		0.036		0.063***	
	(0.044)		(0.012)		(0.022)		(0.015)	

School Average Baseline Unexcused Absences 10% or more	0.255***		0.051**		0.005		0.050*	
	(0.029)		(0.018)		(0.026)		(0.021)	
School Average Baseline Any Absences 5 to <10%		0.084**		0.027*		0.015		0.007
		(0.028)		(0.013)		(0.027)		(0.024)
School Average Baseline Any Absences 10% or more		0.030		0.026		-0.015		-0.010
		(0.034)		(0.018)		(0.032)		(0.027)
4th Grade			0.018*	0.017				
			(0.008)	(0.010)				
6th Grade	0.057**	0.049**	0.002	-0.008	0.013	0.003	0.006	-0.008
	(0.018)	(0.018)	(0.011)	(0.013)	(0.008)	(0.010)	(0.013)	(0.013)
7th Grade	0.052**	0.042*	0.001	-0.001	0.012	0.003	-0.008	-0.019
	(0.020)	(0.019)	(0.017)	(0.019)	(0.013)	(0.015)	(0.011)	(0.012)
8th Grade	0.048*	0.038	-0.010	-0.005	-0.002	0.008	0.000	0.000
	(0.024)	(0.024)	(0.017)	(0.018)	(0.013)	(0.016)	(.)	(.)
Constant	-0.019	-0.058	-0.076**	0.059	-0.027	0.043	0.002	0.042
	(0.028)	(0.036)	(0.024)	(0.064)	(0.027)	(0.098)	(0.042)	(0.028)
N	89495	89495	102594	102594	76382	76382	47262	47262

Notes: Standard errors are clustered at the school-level. * for p<0.05, ** for p<0.01, and *** for p<0.001

Table A-3. Robustness Absenteeism Analyses

	COVID (2021)		**Post-COVID (2022)**		**Post-COVID (2023)**		**Post-COVID (2024)**	
	Unexcused	All	Unexcused	All	Unexcused	All	Unexcused	All
Main Analysis	0.008	-0.002	0.007	0.023	0.000	0.020	-0.001	-0.004
	(0.023)	(0.025)	(0.015)	(0.018)	(0.016)	(0.017)	(0.016)	(0.019)
N	89495	89495	102594	102594	76382	76382	47262	47262
Magnet	0.008	0.000	0.005	0.022	-0.000	0.019	-0.001	-0.005
Students	(0.022)	(0.025)	(0.014)	(0.017)	(0.016)	(0.017)	(0.016)	(0.020)
N	93159	93159	105734	105734	79463	79463	49286	49286
ITT	0.014	0.004	0.004	0.016	0.006	0.014	0.001	0.005
	(0.015)	(0.018)	(0.011)	(0.013)	(0.012)	(0.013)	(0.010)	(0.013)
N	97266	97266	124748	124748	95567	95567	67127	67127
Opening / Closing	-0.008	-0.019	0.000	0.024	-0.002	0.019	0.001	0.001
Schools	(0.027)	(0.030)	(0.015)	(0.018)	(0.016)	(0.017)	(0.017)	(0.020)
N	104291	104291	123946	123946	92542	92542	61111	61111
1:1 Matching	0.019	0.009	0.001	0.020	0.002	0.018	-0.001	-0.006
	(0.023)	(0.024)	(0.016)	(0.019)	(0.017)	(0.018)	(0.020)	(0.022)
N	9834	9834	12240	12240	7574	7574	4620	4620
Baseline 2018-19	0.008 (0.023)	-0.002 (0.025)	-0.015 (0.021)	-0.015 (0.026)	-0.007 (0.017)	-0.026 (0.021)	0.018 (0.021)	0.001 (0.024)
N	89497	89497	57947	57947	35943	35943	15276	15276
Common	0.008	-0.002	0.007	0.023	0.000	0.020	-0.001	-0.004
Support	(0.023)	(0.025)	(0.015)	(0.018)	(0.016)	(0.017)	(0.016)	(0.019)
N	87830	87830	102230	102230	75246	75246	46898	46898

Notes: All models include grade and county fixed effects. Standard errors are clustered at the school-level and reported in parentheses. * for p<0.05, ** for p<0.01, and *** for p<0.001