Monarrez, T., Kisida, B., & Chingos, M. (2022). The Effect of Charter Schools on School Segregation. American Economic Journal: Economic Policy, 14(1), 301-340.
https://www.aeaweb.org/articles?id=10.1257/pol.20190682

We identify the impact of the charter share of enrollment on segregation using the following econometric specification

$$Y_{igt} = \beta E_{igt} + X_{igt}^{\prime}\Gamma + \tau_{ig} + \delta_{it} + \gamma_{s(i)gt} + \epsilon_{igt}, \tag{2}$$

where Y_{igt} is the segregation of school system i in grade g for school year t; E_{igt} is the percent of school system enrollment going to charter schools in that grade and year; and X_{igt} is a vector of characteristics that vary at the system-grade-year level, including log of total enrollment, the fraction of students from a given racial group, and the number of schools serving a given system-grade-year. The model also includes system-by-grade fixed effects τ_{ig}, system-by-year fixed effects δ_{it}, and state-by-grade-by-year fixed effects $\gamma_{s(i)gt}$. Finally, ϵ_{igt} is a structural residual that may threaten the validity of the assumptions necessary to interpret β causally if correlated with E_{igt}.

This specification can be interpreted as a triple differences model, with identification relying primarily on the inclusion of system-year fixed effects, but also accounting for state-year-grade and system-grade variation.[17] The system-year effects δ_{it} serve an important role because they account for unobserved time-varying shocks at the school system level that have equal impact on segregation across all grade levels. For instance, we can rule out that our estimates are driven by districts enacting a policy that applies to all grade levels and impacts segregation, and whose timing coincides with the rise of the charter school sector in this locality. Additionally, system-year effects flexibly absorb the impact that uneven urban change and gentrification may have on stratification patterns. Nonetheless, by themselves the system-year fixed effects cannot account for important between-grade grade differences in the determinants of school segregation.

The inclusion of system-grade fixed effects τ_{ig} restricts comparisons to the same grade level within a single school system, which has a twofold use in the case for causal identification. First, they difference out time-fixed variation in segregation across school grade levels, which have been documented empirically on a national scale, but may vary by place (Erica Greenberg and Tomas Monarrez 2019). Second, both the system-grade and system-year effects get rid of time-fixed confounding variation in segregation across the geography of the country. For instance, school segregation is higher in southern school systems than in western ones. Charter penetration also happens to be higher in the West than in the South, but we wouldn't want to attribute this correlation to the causal effect of the charter sector. Finally, the state-grade-year effects $\gamma_{s(i)gt}$ ensure our estimates flexibly account for differences in segregation varying by grade, year and state, which could be driven by state-specific cohort effects like the secular growth of Hispanic enrollment in certain areas of the country over recent decades.

Intuitively, equation (2) identifies average effects by aggregating variation in charter enrollment *dynamics* across grade levels within each school system and year. For a given school system, if in year t the charter share of grade g enrollment grew relatively more than in other grades and there was a corresponding relative increase in the change rate of segregation, our model would attribute this to a causal association between charter sector growth and increased segregation. Our national estimate of the average effect β can be interpreted as a weighted average of these types of adjusted comparisons within system-years, across all school systems fitting our analysis requirements over the period 1998-2018.[18]

Bravata, D., Cantor, J. H., Sood, N., & Whaley, C. M. (2021). Back to School: The Effect of School Visits During COVID-19 on COVID-19 Transmission. National Bureau of Economic Research Working Paper Series, No. 28645. https://www.nber.org/system/files/working_papers/w28645/w28645.pdf

4. EMPIRICAL APPROACH

With these data, we estimate the effect of school reopenings using within-county variation in household structure. We estimate the effects of changes in school visits on COVID-19 outcomes using a triple-differences model of the form:

$$
\begin{aligned}
covid_{igt} = \alpha + \gamma_1 child_i + \gamma_2 \Delta school_{gt-2} + \delta child_i \times \Delta school_{gt-2} \\
+ \tau \boldsymbol{week_t} + \psi \boldsymbol{county_g} + \varepsilon_{igt}
\end{aligned}
\tag{3}
$$

In this model, $covid_{igt}$ represents the dichotomous COVID-19 related outcome of interest—a COVID-19 diagnosis for household i in county g during week t. The $child_i$ term indicates that the household has a school-age child. The $\Delta school_{gt-2}$ represents the week and county-specific measure of visits to schools from the SafeGraph data. Given the incubation period of the SARS-CoV-2 virus, we use a two-week lag between changes in the number of visits to schools and household-level outcomes. As a sensitivity test, we also use a one-week lag and find similar results (Appendix Table A2).

Our primary coefficient of interest, δ, captures the differential change in COVID-19 outcomes between households with and without children following changes in county-level visits to school. In all specifications, we include fixed effects for week and county. Thus, the δ coefficient measures the change in COVID-19 infections between households with and without children in the same county, and relative to all other households in that week. Due to these fixed effects and because our school visits measures are county and week specific, we do not include main post-reopening or treatment county indicators. We estimate all models using ordinary least squares and cluster standard errors at the county level.

Across all specifications, the δ coefficient measures the intent-to-treat effect of changes in county-level school visits on COVID-19 diagnoses and other COVID-19 related outcomes. We are unfortunately unable to link data on household-specific school visits and infections, which would allow us to fully estimate the first-stage effect that would allow for a local-average treatment effect calculation of school visits on COVID-19 cases. We are also unable to account for differences in school-specific policies designed to limit infection spread (e.g., mask and social distancing stringency), which are important contributors to mitigating the spread of COVID-19 (Falk et al. 2021; Lessler et al. 2021; Dawson et al. 2021; Volpp et al. 2021). Instead, this estimate captures both the direct effect of school-related COVID-19 transmission (e.g., children becoming infected at school) and the potential indirect effect of risk avoidance by households and schools.