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School district consolidation in North Carolina: Impacts on school composition and finance, crime outcomes, and educational attainment

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ABSTRACT

In this paper I analyze longitudinal data from North Carolina to study K-12 school district consolidations, or the merging of multiple districts into a single administrative unit. I use difference-in-differences and event study models to identify effects on theoretically related school- and district-level mechanisms as well as long-term youth outcomes. In contexts where districts consolidate, per-pupil expenditures on instruction and district support services decrease, and schools become more racially integrated. However, youth exposed to mergers are no more likely to be convicted of a crime in early adulthood than those unexposed. These null effects hold when looking at conviction rates by race and by type of crime and when analyzing another key outcome, educational attainment. My results thus provide causal evidence confirming prior research suggesting that consolidation may: 1) reduce the operational costs of schools without negatively affecting students, and 2) support efforts to address persistent racial isolation between schools.

1. Introduction

The number of public K-12 school districts serving students across the U.S. has shrunk from over 100,000 in the 1930s to approximately 14,000, despite substantial contemporaneous increases to student enrollment (De Brey et al., 2022). In the present day, public officials further incentivize or mandate the mergers of districts into single administrative units for economic efficiency reasons (e.g., Arkansas Act 60; Howley et al., 2011), especially in contexts experiencing declining enrollment. Many mid-century "consolidations" were indeed motivated by under-enrollment as birth rates fell after the generation of baby boomers, but policymakers also believed that bigger schools and districts could more cheaply improve educational outcomes via access to expanded course offerings taught by more specialized personnel (Guthrie, 1979). In a few high-profile instances, districts were also joined in efforts to address racial isolation between schools during the desegregation era following *Brown v. Board* (Holmes, 1973).

District consolidation has thus been one of the most significant and widely-used educational policy levers of the past century—one that is still employed today. Yet despite the pervasiveness of mergers and their potential to affect students' in-school experiences, we have relatively little rigorous evidence on their purported consequences. As such, in this study I ask:

1) Does school district consolidation impact theoretically related K-12 school- and district-level mechanisms, such as school finance, composition, and operational status?
2) Does consolidation impact the long-term outcomes of youth, specifically their criminal activity in early adulthood and educational attainment?

To answer these questions, I focus on longitudinal school-, district-, and county-level data collected from 18 counties in North Carolina where districts consolidated sometime after the 1989–1990 school year.[1] I contrast these data to those collected from districts in 59 counties that either consolidated considerably earlier than the period of this study—prior to 1966—or have never consolidated. To recover credibly causal effects, I estimate a series of difference-in-differences and event study models to compare outcomes before and after district consolidation in the "treatment" counties to those observed contemporaneously in "comparison" counties.

My analyses contribute three main findings. First, district consolidation does not lead to statistically significant enrollment shifts overall or by student race. However, I do observe reductions in the racial

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[1] For the remainder of the manuscript, I simply refer to school years using only the year of the spring semester.

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segregation of schools, with White students experiencing increased exposure to Non-White students. Second, consolidation does not change the total spent per-pupil by districts on capital outlays, though spending on both instruction and district support services significantly decreases over time. But no consistent impacts emerge for other interconnected and theoretically related school-level inputs, such as the average number of students per full time equivalent teacher ("class size"), school size, or the number of schools in operation. Similarly, despite changes to the resources available, consolidations do not affect students' high school diploma rates or their long-term involvement with the criminal justice system, even when subsetting by individuals' race and by the type or severity of the crime.

My results speak to three literatures. This study first contributes rigorous empirical evidence to existing explorations of district consolidation and its impacts. Advocates point to economic theory indicating that mergers can improve the cost effectiveness of school operations. But research on district size (and by extension, district consolidation) finds both economies *and* diseconomies of scale in education (Duncombe & Yinger, 2007). Cost effectiveness might improve as larger districts contract the services paid for by individual distinct districts (e.g., central administration), afford and employ more specialized equipment in schools, expand school size so that teachers can specialize in areas of instruction, benefit from bulk purchasing power of both literal school supplies and also labor, and become more productive and innovative because of collaboration among a broader pool of experienced staff. In contrast, cost effectiveness might decrease as larger district accrue higher costs when transporting students, face more powerful teachers' unions, and suffer from more vertical organizational structures with greater coordination costs, extra levels of bureaucracy, and weaker personal relationships, all of which ultimately affect community members' attitudes, motivation, engagement, and effort negatively.

Results from quantitative studies reinforce the difficulty of predicting how district size relates to spending and learning. This research generally finds that districts appear to benefit cost wise from enrollment growth up to a certain point before diseconomies of size begin to dominate, with more mixed effects being observed for student outcomes (Andrews et al., 2002; Collins, 2019; Duncombe & Yinger, 2007; Fox, 1981). Far fewer empirical investigations specifically focus on the effects of consolidation as opposed to overall enrollment. In his review, Collins (2019) finds three such published studies in the U.S. context, none of which demonstrate that mergers decreased costs and/or improved outcomes relative to cost.[2] Overall, most prior analyses exploring the importance of district size and/or consolidation do not leverage longitudinal data (Duncombe & Yinger, 2007) or identification strategies that would allow for causal claims to be made without relatively strong assumptions.

My results next complement existing findings on the importance of school resources for youth outcomes. Recent research leveraging quasi-experimental designs shows that providing additional dollars to schools improves educational measures such as test scores and attainment (e.g., Candelaria & Shores, 2019; Hyman, 2017, Lafortune et al., 2018), as well as the long-term outcomes of labor market success and involvement with the criminal justice system (e.g., Jackson et al., 2016; Johnson & Jackson, 2019). A smaller set of studies concurrently estimates the impacts of policy on different areas of school spending and how subsequent shifts across areas translate into student gains (e.g., Baron, 2022). Theoretically, school district consolidation most strongly links to decreased expenditure on district support services (e.g., through the contraction of central administration), which may be less vital than instructional spending for student outcomes. I indeed find some evidence of this after districts merge. Across spending categories, I observe the biggest decrease in support services and specifically show a sudden dip in dollars allocated to the services most likely to be duplicated and thus immediately unified when separate districts join, i.e., boards of education and/or superintendent offices.

But even with less financial support, I do not document significant worsening of long-term youth outcomes as might be expected given the extant literature, potentially because negative fiscal impacts are not solely concentrated in instructional expenditures. Moreover, shifts in dollars spent per pupil are not accompanied by changes in the associated K-12 mechanisms that would harm students. Class size does not consistently increase in consolidating contexts, nor does school size, as the total number of operational schools remains relatively stable over time. Existing studies on mergers (e.g., Collins, 2019) specifically consider whether district officials shutter under-enrolled schools to cut costs because an extensive, international literature suggests that school closure can negatively (and unequally) affect youth, especially if it severely disrupts a community and/or forces displaced students to attend relatively lower-performing options that also do not benefit from increased economies of scale (e.g., Beuchert et al., 2018; Bifulco & Schwegman, 2020; Brummet, 2014; de Haan et al., 2016; Engberg et al., 2012; Steinberg & MacDonald, 2019; Shaw, 2017; Tieken & Auldridge-Reveles, 2019).

Finally, by additionally investigating school segregation—as opposed to just cost effectiveness—and its link to school district boundaries and consolidation (Holmes, 1973; Siegel-Hawley, 2016), this study contributes to a third substantial body of work less directly connected to economic theory. Following the historic *Brown v. Board* Supreme Court decision, subsequent court orders to integrate, and the implementation of voluntary desegregation plans (Cascio et al., 2008; Welch & Light, 1987), racial isolation across schools was significantly reduced. But research has also found evidence of "White flight" or the disenrollment of White students from integrating districts (Reber, 2005), especially in contexts maintaining racially segregated suburban-city school systems where White families could avoid integration by exiting urban districts serving substantial populations of Black youth (Holmes, 1973). Efforts to combat this disenrollment and maximize the potential of integration involved desegregation plans that merged districts across entire metropolitan areas (e.g., Louisville-Jefferson County, Kentucky; Ayscue & Orfield, 2015; Diem et al., 2014; Orfield, 1995). At the same time, the *Milliken v. Bradley* Supreme Court decision limits what school district leaders can do to address inter-district segregation, arguably the most significant contributor to racial isolation across schools in the present day (Fiel, 2013); a series of more recent district secessions across the country have further exacerbated segregation by race across district lines (e.g., Taylor et al., 2019).

By changing the distribution of resources and peers across schools, integration has improved outcomes for Black youth, including in terms

[2] Cooley and Floyd (2016) explored rural school district mergers in Texas and calculated the following differences in outcomes and finances: between consolidating districts pre- and post-consolidation, and between consolidating and matched non-consolidating districts in post-consolidation years. They did not calculate a true difference-in-differences estimate. Streifel et al. (1991) investigated the pre- and post-consolidation finances for 19 districts across the country in the 1980s and benchmarked differences to districts' respective statewide trends. They did not look at youth outcomes. Duncombe and Yinger (2007) evaluated 12 rural district mergers in New York during the 1980s and 1990s, comparing pre-post spending outcomes with contemporaneous changes in spending occurring in approximately 200 non-consolidating districts. They projected long-term operational cost savings from mergers; student performance was treated as an input and not as an outcome. In an unpublished dissertation chapter, Collins (2019) explored consolidations resulting from Arkansas Act 60 using a difference-in-differences approach and did not find strong evidence that mergers induced by the law improved student outcomes or affected school finance. Outside of the U.S. context, Sandsør et al., 2022 found that mergers in Norway of local governmental entities in charge of primary and lower secondary education improved students' educational attainment and income, though these gains were not accompanied by commensurate changes in school resources. Notably, these consolidations did not explicitly affect schools and their catchment zones. Finally, in descriptive analyses Heinesen (2005) found economies to scale in the Denmark context, as students in larger districts similarly attain higher levels of education.

of educational attainment, labor market outcomes, and incarceration rates (e.g., Guryan, 2004; Johnson, 2011; Reardon & Owens, 2014)—without negatively affecting the long-term opportunities for White students. District consolidation thus holds enormous potential to help expand equity by addressing racial isolation between schools, which relates to segregation between neighborhoods in the same metropolitan area that are connected to separate school systems. At the same time, post-consolidation dynamics may replicate those seen during the school desegregation era if families—particularly those from more historically advantaged racial/ethnic and/or socioeconomic backgrounds—disinvest from public education either through disenrollment from merging contexts or by advocating for fewer dollars to be spent on supporting a unified school system.

In summary, prior research yields far more theoretical arguments for the effects of school district consolidation than rigorous empirical evidence, despite the policy importance of mergers and their potential to influence key school- and district-level mechanisms (e.g., school finance, composition, and operational status) that affect student outcomes and equity. I next describe the context for my study and the data, sample, and empirical approach I use to bring causal evidence on the impact of consolidations, before sharing results and discussing their implications.

2. The North Carolina context

For decades, school districts across North Carolina have been consolidating into single administrative agencies that serve entire counties, a common structure seen across the U.S. South. Prior to 1991, state law required the multiple school systems in a county involved in a merger (often a combination of city and county agencies) to each agree to consolidation (North Carolina General Statute 115C-67). In 1991, the state legislature enacted statutes that arguably made unifications easier. Instead of requiring approval from each separate district for consolidation—which often faced substantial resistance from at least one party—the act now created two additional paths to consolidation. The first path permitted county commissioners in North Carolina to force a merger of districts co-located in the same county to consolidate without a voter referendum (North Carolina General Statute 115C-68.1). The second path stated that if a city school district in the state dissolved itself, the encompassing countywide district would assume service of the dissolved agency's students (North Carolina General Statute 115C-68.2).

The extent to which (dis)economies of scale are realized following consolidation in any context depends in part on the structure of the system that finances schools. Districts in North Carolina receive most of their funding from the state government, who allocates dollars primarily based on districts' employee positions (determined by preestablished staff-to-pupil ratio requirements) as opposed to their students (i.e., weighted student funding). The statewide salary scale, in combination with the tendency for higher salaried teachers (e.g., those with more experience, advanced degrees, and National Board Certification) to sort into schools and districts serving more advantaged populations, means that wealthier contexts in North Carolina often receive more resources. And though the state offers additional dollars to educational agencies based on the wealth of the county and level of disadvantage in the student population, these allotments are far outpaced by the position allotments and are rarely used to increase pay. Finally, just about a quarter of total funds come from local sources (e.g., property and sales taxes), which themselves can be politically difficult to increase, will vary based on the wealth of a district, and are largely used (at least historically) for capital projects. The existing funding structure for education thus contributes to observed inequality across North Carolina in terms of schools' access to financial resources (Program Evaluation Division of the North Carolina General Assembly, 2016), and merging districts may be particularly exposed to shifts in available public dollars because of downstream effects following changes to school operations.

Additional detail on school finance in North Carolina can be found in Appendix C, as well as descriptions of specific consolidations that have occurred since the early 1990s. But it is worth highlighting here that in recent years, a "deconsolidation" movement has emerged that contends that larger districts in North Carolina are too big to efficiently improve outcomes and respond to community needs (Hood, 2022). Public officials did in fact withdraw a plan that incentivized the remaining counties with separate city and county districts to merge following statements from the North Carolina State Board of Education that were skeptical of whether consolidation saved money (Hieb, 2005). Yet proponents for mergers worry that deconsolidation will increase segregation and racial inequality (Ford, 2018; Gordon, 2020; Yeoman, 2018). A special legislative committee ultimately urged for more research on its effects before the state establishes rules for deconsolidation (Joint Legislative Study Committee on the Division of Local School Administrative Units, 2018). Indeed, evidence assembled by all sides on the nature of North Carolina's past consolidations and their impacts on outcomes largely emerges from other state contexts or focuses on specific, anecdotal cases within the state.

My study thus focuses on North Carolina because of the need for more rigorous statewide evidence to inform policymaking, the distinctness of the state's legislative environment, where mergers are potentially simpler to instigate and may subsequently result in more meaningful school integration policies otherwise obstructed by the *Milliken v. Bradley* decision, and because of the state's unique publicly available longitudinal data on youth outcomes, described next.

3. Method

3.1. Data

To investigate the impacts of school district consolidation in North Carolina, I first draw on data from the National Center for Education Statistics' (NCES) Common Core of Data (CCD). I primarily focus on the following information describing public K-12 schools and districts over time: demographic composition, including overall student enrollment and White/Non-White student enrollment; school segregation by race, including the commonly-used dissimilarity and exposure indices ;[3] per-pupil expenditures (all measured in 2020 dollars) on capital outlays, instruction, and support services; and other key educational inputs theoretically linked to school finance and consolidation, including the number of operational schools in a district, school size, and class size.[4]

[3] The dissimilarity index is a measure of balance and captures the proportion of White or Non-White students that would need to switch schools to achieve balanced racial representation across schools in a district. The exposure index is a measure of degree of potential contact and either captures the proportion of Non-White students in the average White student's school (White-Non-White exposure) or vice versa (Non-White-White exposure). Note that though both segregation indices are not specifically calculated at the school level, they capture how students from different racial backgrounds are distributed across schools in a given context.

[4] Though rates of missingness for these data at the school or district level are extremely low (i.e., less than a fifth of a percent of observations for any given measure), the temporal coverage of these measures varies slightly. School counts and size, as well as total enrollment counts, and are available from 1987 through 2021; segregation indices and enrollment counts by race are available from 1988 through 2021; class size data are available from 1987 through 2021 but I exclude data from 2007 from analyses because of anomalous levels of missingness; and finally, per-pupil expenditures are available from 1990 through 2019. In my main analyses, I leverage all data available when analyzing any given outcome. However, in Appendix Figures B2, B3, and B4, I show that results are robust when restricting analyses only to a set of years with full data coverage and a balanced panel of data for each observation.

When calculating these and other measures used in analyses, I treat merging districts as a single school district both before and after consolidation.[5]

My first key long-term youth outcome of interest is involvement with the criminal justice system in early adulthood. I focus on this particular measure because, as described in the literature review, crime outcomes have been linked to both school spending and segregation, two school- and district-level mechanisms directly associated with district consolidation. I collate longitudinal information on county-level birth rates overall and by race using publicly available reports from Bailey et al. (2018) and the North Carolina Department of Health and Human Services. I then use publicly available data from the North Carolina Department of Public Safety (NCDPS) on all individuals ever convicted of a crime by the NCDPS since 1972. This includes details on convicted individuals' race, age, and North Carolina county of birth (if applicable), and individual-conviction-level details on date of conviction and type of crime (see also, Barr & Smith, 2021).[6] With these two datasets, I estimate county-by-birth-cohort specific rates for having ever been convicted of a crime between the ages of 18 and 27, overall, by race, and for subsets of crimes including violent, property, and felony crimes.[7,8]

I supplement crime data with another long-term student outcome that research links to changes in school finance and segregation: educational attainment. Drawing on CCD data on high school diploma rates in North Carolina, I focus on attainment measured in 1991 and from 1998 through 2000 ("pre"- and "post"-consolidation periods, respectively). I do not evaluate earlier data because, following standard practice, I rescale district-level diploma receipts against eighth grade enrollment four years prior (Candelaria & Shores, 2019; Heckman & LaFontaine, 2010). Diploma rate data in North Carolina for years between these pre- and post-periods are also not complete for all counties with consolidating districts and/or are anomalous when validated against state reports. Finally, only in one county do districts consolidate after 2000.[9]

3.2. Sample

Two groups of school districts comprise my analytic sample. The first group includes the 43 districts from 18 different counties in North Carolina that consolidated into 18 districts between 1990 and the present day. I determine consolidation status based on the year-to-year presence of a district in the NCES Local Education Agency Universe Survey and/or the NCES Public Elementary/Secondary School Universe Survey and I confirmed mergers between districts using documents from the North Carolina State Board of Elections. I limit my treatment group to these observations because of when they consolidated relative to the presence of consistently collected annual data on school- and district-level measures, i.e., from 1987 through 2021.

My ideal empirical strategy for isolating the effect of school district mergers (described in more detail next) would leverage as a comparison for consolidating districts those that have not consolidated.[10] As noted above in the introduction, however, the total number of those never merging will be small as the number of consolidated school districts in the U.S. dramatically increased in the mid-1900s. As such, I include as comparison districts some that may have merged prior to 1966—24 years before the first set of districts in my treatment sample consolidate—based on publicly available reports on the universe of operating school districts in North Carolina generated by the Southern Education Reporting Service.[11] Thus, the second group in my analytic sample includes these 49 districts from 49 counties plus an additional 24 districts from 10 counties in North Carolina that I have confirmed to have not consolidated (i.e., 73 districts from 59 counties, total). All the details of these observations including district and county names, consolidation status, and year of consolidation can be found in Appendix Table A1.[12] A county-level map of consolidating, comparison, and excluded observations can be found in Appendix Figure A1; notably, the map shows that contexts experiencing district mergers are not geographically concentrated in any particular region of North Carolina.

In Table 1, I present summary statistics for the counties with and without merging districts in my analytic sample during a baseline year.

[5] Because nearly all consolidations that have occurred in North Carolina incorporate every district within county borders (the single educational agency exception, Mooresville Graded School District in Iredell County, is excluded from my analyses), this procedure essentially results in county-level outcomes being estimated for consolidated contexts, which also matches the level of the first key long-term youth outcome of interest.

[6] One benefit of the NCDPS is that with county of birth information for those criminally convicted, I can address potential concerns around mobility biasing estimates. Specifically, impact estimates for consolidation on youth long-term outcomes are not affected by potential movement into and away from contexts with merging districts. The assumption when analyzing this outcome, however, is that youth enroll in public schools where they are born (e.g., I am estimating intent-to-treat effects). The natality data yields supportive evidence; in North Carolina, the correlation for both White and Non-White births by county of birth and by county of residence is .96. Using data from the five-year American Community Surveys collected for 2005–2009, 2010–2014, and 2015–2019, I find that 75% to 80% of individuals (depending on survey administration, sex, and race/ethnicity) who are 18 to 27 and who were born in North Carolina still live in North Carolina, so criminal activity for most youth in my sample are not censored by out-of-state mobility. In results available on request, I analyze data from the 1990 and 2000 Decennial Censuses ("pre"- and "post"-consolidation periods, respectively) to investigate mobility at the sub-state level with difference-in-difference models. I do not find strong evidence of differential migration for those of school-going age from Public Use Microdata Areas (i.e., reported living in the same Public Use Microdata Area five years ago) in North Carolina where districts consolidate versus those where they do not.

[7] I focus on criminal convictions during this decadal age range for a few reasons. First, in North Carolina, those under 18 are considered juveniles and upwards of 94 percent of convictions in the NCDPS data are for persons over the age of 18. Second, all but one consolidating district in my sample consolidates by 1998. Youth who turn five years old (i.e., beginning school age) in 1998 who also live in contexts where districts consolidated that same year will thus be plausibly exposed to consolidation for all K-12 years. These youth will also turn 27 in 2020, the most recent year with available NCDPS data that does not completely coincide with the COVID-19 pandemic.

[8] I top-code criminal conviction rates so that they take a maximum value of one. Nearly all observations this affects have fewer than 10 births in the given cohort. Analyses of criminal conviction rates are weighted by birth cohort size, which further minimizes the influence of this decision rule.

[9] It is worth highlighting here that I do not use data from the North Carolina Education Research Data Center (NCERDC). The NCERDC houses student-level administrative data, but the outcome measures available largely span school years after the majority of consolidations in my panel have already occurred, i.e., there is no baseline data. Specifically, the earliest available student-level outcome of interest (end of grade test performance) is provided in 1995, and only three of 18 consolidations in North Carolina occurred after this year. Similarly, course-level data for students is available starting in 2006—after all consolidations have occurred—so within-school measures of racial segregation and intergroup exposure using NCERDC data would not be useful for this study.

[10] This restriction on the comparison sample mirrors that of synthetic control approaches (Abadie et al., 2010) and helps address concerns of bias in difference-in-differences and event study estimates raised by recent econometric advances (Goodman-Bacon, 2021). In North Carolina, I am constrained in my ability to compare consolidating districts to only those that merge at future periods because nearly all consolidations in my sample occur within a few years of each other (see Appendix Table A1), limiting the total number of post-period differences I can estimate.

[11] Notably, by this year, nearly all districts in North Carolina had begun desegregating schools by race (see also, Cascio et al., 2008), which sometimes entailed the merger of racially segregated school districts.

[12] Appendix Table A1 also shows that for most counties that are still served by multiple school districts, one agency specifically serves a relatively more urban center or city whereas the other serves the remainder of the county.

The primary difference between the two groups is that districts in the former are noticeably larger in terms of overall combined enrollment and enrollment by race as well as number of schools. However, the largest districts across these contexts are far more similar in terms of student counts, and I observe no significant differences in other key district- and school-level mechanisms, such as exposure indices, spending measures, or school and class size; one exception is the dissimilarity index measure of school segregation. Similarly, criminal conviction and high school diploma rates for baseline cohorts are comparable. Regardless of these similarities and differences, however, my analytic approach does not necessitate assuming baseline equivalence to identify the causal impact of district mergers on outcomes when comparing consolidating and comparison observations.

3.3. Empirical strategy

To arrive at credibly causal impacts of school consolidation on outcomes, I estimate a series of difference-in-differences (DD) and event study (ES) models. In concrete terms, for the first difference, I compare outcomes in counties with consolidating districts before and after consolidation. For the second difference, I compare outcomes over time between treatment and comparison observations. The first difference thus accounts for time-invariant differences between contexts with mergers and those without, while the second difference accounts for statewide shifts in outcomes over time.

To address concerns of potential bias in estimates derived from standard two-way fixed effects models of DD and ES (e.g., Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021), I employ the two-stage estimation framework recommended by Gardner (2021). In the first stage, I estimate on the sample of observations for which $Post_{it} = 0$:

$$Y_{it} = \lambda_i + \gamma_t + \varepsilon_{it} \tag{1}$$

Where i indexes county and t indexes year. $Post_{it}$ is a dichotomous variable equal to zero for all non-consolidating districts and equal to one for consolidating districts if year t falls on or after their year of consolidation. Y_{it} captures some outcome for county i at year t.[13,14] From the model I retain the county and time effects, $\hat{\lambda}_i$ and $\hat{\gamma}_t$, and then use these effects to adjust outcomes for regression analysis in the second stage:

$$Y_{it} - \hat{\lambda}_i - \hat{\gamma}_t = \beta \times Post_{it} \tag{2}$$

The point estimate on $Post_{it}$, β, thus captures the primary coefficient of interest. Importantly, because of the outcome residualization process, this "DD" estimate is not subject to concerns about bias related to analyzing staggered rollout of "treatments" (i.e., district mergers) in the DD framework. Put differently, counterfactual pre-trends for consolidating contexts are determined only using information from non-treated observations.

The two-stage process is easily extended to estimate both non-parametric and parametric ES models, i.e., by replacing the $Post_{it}$ indicator in the model represented by Eq. (2) with either a vector of years-to and years-after consolidation indicators, or with linear measures of lead and lag times. Results from these models can then be used to test the primary assumption necessary for β to recover a plausibly causal impact estimate of district mergers: that outcomes measured in treatment and comparison observations would have followed parallel trends absent consolidation. Because counterfactuals are never actually observed, I cannot directly test this assumption. But if the point estimates on the vector of years-to indicators or the linear measure of lead time are insignificant, this lends credence to this assumption, i.e., that outcomes for treatment and comparison observations would have followed parallel trends absent a merger.

To implement Gardner's (2021) approach, I use the *did2s* package in Stata 18.0. The program accounts for the two-step procedure to correct standard errors of DD and non-parametric ES estimates and allows me to cluster standard errors at the county level. Finally, I adapt the base package to estimate parametric ES models using the two-stage framework.

4. Results

4.1. Impacts on K-12 School- and district-level mechanisms

I first present the impact of school district mergers in North Carolina on the K-12 school- and district-level mechanisms that have been theoretically linked to consolidation.

In Fig. 1, I specifically plot the ES estimates from the model represented by Eq. (2) for consolidating observations. I focus on results for outcomes measured between three years prior to consolidation, up to 12 years post. I limit the temporal coverage because the earliest (latest) set of district consolidations in my panel happen during the 1990 (2005) school year, and most variables I consider are collected consistently between 1988 through 2019.[15] In Table 2, I present the associated point estimates for the ES (Panel A) and DD (Panel B) models (again limiting the number of leading and lagging periods included). Panel C of the table displays these results when estimating linear parametric ES models.

Several patterns emerge from Fig. 1 and Table 2. First, results from the ES models indicate that the parallel trends assumption is tenable (Fig. 1; Panel C of Table 2), as parametric and non-parametric differences in outcomes between consolidating and comparison districts are generally not statistically significant prior to consolidation. Second, following consolidation, I find sustained increases in the extent to which White students are exposed to Non-White students ("White-Non-White Exposure Index") and that districts spend less per pupil on current expenditures. For the per-pupil finance outcomes, the downward shift begins almost immediately for support services and over time for instruction. In contrast, there is no clear effect of district mergers on capital outlay spending, nor are differences in related mechanisms such as the number of operational schools, school size, and class size consistent in direction and/or statistical significance. Finally, though White-Non-White exposure increases post-consolidation, there is not a similar pattern for Non-White-White exposure, though there is a suggestive (but insignificant) decrease in the other commonly-used measure of segregation, the dissimilarity index. These segregation patterns may reflect overall enrollment shifts that are statistically insignificant; after districts merge, White (Non-White) enrollment decreases (increases) slightly.

[13] As noted above, for district-level outcomes, I treat all districts in the same county as a single unit, e.g., for districts that consolidate, school segregation indices before and after consolidation are calculated assuming all schools across the separate districts are in the same district. Because districts do not cross county borders in North Carolina, this calculation results in a county-level outcome measure.

[14] When the outcome is criminal conviction rates, time indexes the year that those from a given birth cohort turn 18, i.e., the final plausible age where a majority of the cohort would still be in K-12 schools. As such, when time is positive when analyzing these outcomes, I am comparing the criminal conviction rates in early adulthood of youth cohorts who turn 18 after district consolidation occurs (for treated contexts) in their contexts. When time is negative, these cohorts turn 18 before district consolidation occurs and thus are unexposed.

[15] Relatedly, the first year of district-level school finance data that I can analyze is available in 1990. Nearly three-quarter of the consolidations occur prior to or during 1994, which is also the modal consolidation year. Thus, by focusing on data describing up to three years prior to district mergers, I maximize for the majority of observations the number of pre-treatment years considered in ES models predicting expenditures, limiting the potential sensitivity of estimates due to random data censoring.

Table 1
County-level summary statistics at baseline.

	Comparison Counties (N = 59)		Consolidating Counties (N = 18)		
	Mean	SD	Mean	SD	Difference
Panel A. School Characteristics					
N Districts	1.237	(0.566)	2.389	(0.850)	1.152***
Total Enrollment - Largest District	7895.237	(10,898.175)	9859.556	(5605.398)	1964.319
Total Enrollment	8621.152	(11,283.565)	15,777.444	(10,919.694)	7156.292**
White Enrollment	5923.288	(7371.292)	9519.611	(6724.094)	3596.323*
Non-White Enrollment	2697.865	(4562.808)	6257.833	(5608.762)	3559.968**
N Schools in Operation	15.780	(16.333)	28.667	(18.790)	12.887**
Average School Size	493.794	(126.098)	534.199	(79.652)	40.405
Pupils per FTE	16.430	(1.444)	16.591	(1.241)	0.161
Dissimilarity Index	0.248	(0.143)	0.358	(0.112)	0.110**
White-Non-White Exposure Index	0.299	(0.216)	0.304	(0.138)	0.005
Non-White-White Exposure Index	0.630	(0.231)	0.538	(0.180)	−0.092
Capital Outlays PP	1030.102	(1130.563)	584.175	(403.571)	−445.927
Instructional Expenditures PP	5187.418	(414.463)	5061.948	(788.255)	−125.470
Support Services Expenditures PP	2654.375	(371.433)	2560.450	(388.380)	−93.925
Panel B. Student Outcomes					
Criminal Conviction Rates - All	0.107	(0.032)	0.112	(0.026)	0.005
Criminal Conviction Rates - Non-White	0.205	(0.052)	0.194	(0.029)	−0.011
Criminal Conviction Rates - White	0.069	(0.027)	0.070	(0.019)	0.001
High School Diploma Rates	0.745	(0.070)	0.761	(0.091)	0.016

Notes: The table provides average county-level summary statistics for observations in my study. "Comparison" counties are those whose districts never consolidate or consolidated prior to 1966. All school characteristics except for expenditures (including capital outlays) are measured in 1989, prior to the first consolidation in my sample. Expenditures (including capital outlays) are measured in 1990, the earliest available year with data. All expenditures outcomes (including capital outlays) are in 2020 dollars. FTE = full time equivalent teacher. PP = per pupil. Criminal conviction rates are measured for the birth cohort that turned 18 in 1989 and are weighted by the relevant county-level count of births in 1989. High school diploma outcomes are measured in 1991 and rescaled by eighth grade enrollment four years prior. The statistical significance of differences between consolidating and comparison counties are calculated using a *t-test*.

* $p<.1$,.
** $p<.05$,.
*** $p<.01$.

To help contextualize the size of the significant impacts, I focus specifically on the simpler, more straightforward DD estimates presented in Panel B of Table 2. From these results, I calculate that following a district merger, the proportion of Non-White students in the average White students' school significantly increases by 1.84 percentage points, or approximately a six percent (0.13-*SD*) increase on the baseline White-Non-White exposure index score (Table 1). Consolidation also leads to decreases of 93 and 203 dollars spent per-pupil on instruction and district support services, respectively, or two percent (0.12-*SD*) and eight percent (0.52-*SD*) decreases on the baseline averages (Table 1).

Results thus indicate that, overall, consolidation efforts can indeed help address between-district segregation and reduce schools' operational costs as theorized. The insignificant findings largely suggest that consolidation in North Carolina on average did not affect the number of schools in operation, nor did enrollment patterns change (at least immediately post-merger). In other contexts, unification could plausibly entail school closures and lead to the disenrollment of families preferring district independence and/or the segregation of schools. On the other hand, I do not find that capital outlays are affected, which would also be true in most merger cases.

In Fig. 2, I further explore finance impacts by plotting estimates from ES models analyzing more nuanced measures of school revenues and expenditures from the CCD. First, I consider how key subareas of educational expenditures shift after consolidations and find an immediate decrease (followed by a steady negative trend) in spending post-mergers on general administration, instructional support services, and pupil support services. These services comprise, for example, boards of education and executive administration (e.g., office of the superintendent), as well as supervisory units for curriculum development, instructional staff training, attendance, social work, and counseling support. Notably, the sudden dip in spending on these particular areas matches expectations, as the services duplicated across independent districts and least linked to the day-to-day operations of schools are most likely to immediately contract post-consolidation. Conversely, spending connected to school administrators experience a less pronounced over time and even reversed immediate change.

In Fig. 2I also show how district revenue sources shift after consolidation. Results indicate that expenditures on instruction and support services appear to decline commensurately with decreases to overall revenues, mostly from state but also local sources. Given the aforementioned structure of North Carolina's school finance system, these patterns suggest two potential pathways for why consolidation in the state translated to lower levels of district spending, both immediately and also monotonically over time. First, following a merger, local communities may have generated fewer tax dollars that could be used to fund schools. In North Carolina, county commissioners determine how revenue from property and sales tax get distributed to districts. If communities become poorer (e.g., growth in populations with historically fewer financial resources; see the positive Non-White enrollment trend following mergers in Fig. 1 and Table 2) or if they grow in opposition to supporting local public schools (as might be expected as consolidations in North Carolina become increasingly "forced" as described above), the declining pattern depicted in Fig. 2 where local revenues overall and specifically from other government entities will follow. In results available on request, I employ DD models similar to those used in this paper to analyze North Carolina county-level data on tax revenues from the U.S. Census Bureau's database on individual local government finances and find evidence further supporting this hypothesis, with decreases of approximately 115 tax revenue dollars per capita following school district consolidation.

The second pathway through which mergers may have translated into lower school expenditures pertains to the observed decreases in state revenue, which accounts for the majority of spending dollars available to North Carolina's local educational agencies. Nearly half of these specific school funds are appropriated to pay for teacher positions, so districts whose workforces have fewer years of experience, lower rates of advanced degree holding, or hold fewer National Board Certifications are allotted substantially fewer state dollars. This finance system combined with the sorting of teachers with "better" credentials in

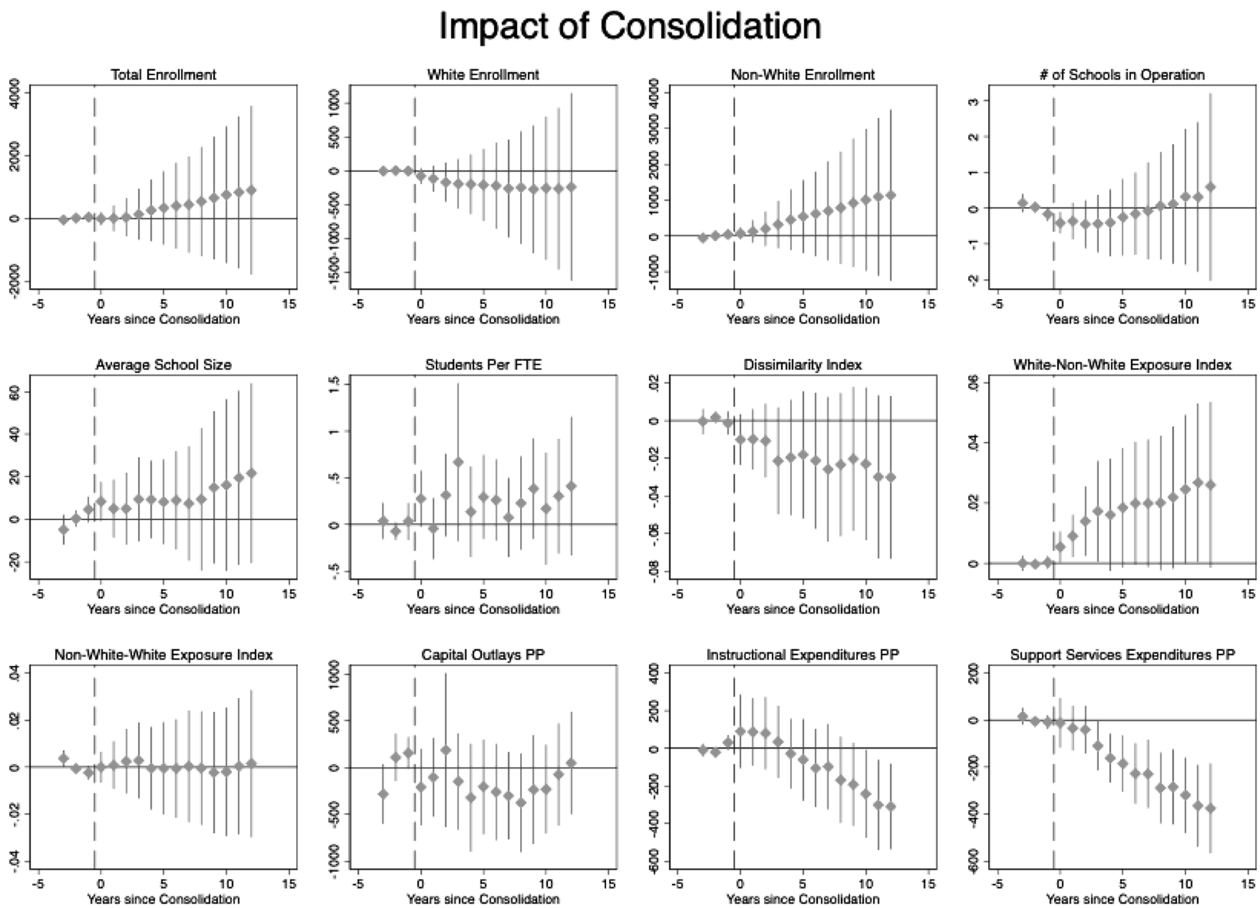

Fig. 1. Average impacts of school district consolidation on K-12 school- and district-level mechanisms with 95% confidence intervals.
Notes: The figure plots regression coefficient estimates for dummies that capture the years since school district consolidation (range from −3 to 12) using the two-stage estimation process recommended by Gardner (2021; see Eqs. (1) and (2). Each separate plot displays coefficients from a separate regression using all data available for the outcome named in the subtitle. Standard errors are clustered at the county level. All expenditures outcomes (including capital outlays) are in 2020 dollars. FTE = full time equivalent teacher. PP = per pupil. No specific period acts as a reference period because differences between treated and comparison observations are implicitly normalized to zero for pre-periods by Gardner (2021).

North Carolina to contexts serving more advantaged student populations (Program Evaluation Division of the North Carolina General Assembly, 2016) and the positive Non-White enrollment trend following mergers (see Fig. 1 and Table 2) thus predicts the monotonic decline in state revenues post consolidation.

4.2. *Impacts on crime in early adulthood and educational attainment*

In Fig. 3 (see Appendix Table A2 for regression point estimates), I present the ES estimates from models predicting my first long-term youth outcome, criminal conviction rates in early adulthood. Again, I do not find evidence of differential pre-trends in the outcomes, supporting the parallel trends assumption: I do not observe that birth cohorts in consolidating contexts that turn 18 (i.e., latest typical school age) in the years leading up to district mergers are convicted of crimes at divergent rates than those in comparison contexts. On the other hand, differences also do not clearly emerge between youth born in counties with and without consolidating districts for the cohorts who turn 18 after the district mergers occur (i.e., those still of plausible school age after treatment). Null effects hold for criminal conviction rates overall and by race, as well as by type of crime (i.e., violent, property, felony; see Appendix Figure A2).

To help contextualize the size of these main effects while factoring in the (im)precision of estimates, I focus on the simpler, more straightforward DD estimates and their standard errors presented in Panel B of Appendix Table A2. From these results, I calculate that the impact of district mergers on criminal conviction rates in early adulthood range from −0.0245 to 0.0155, −0.02 to 0.0062, and −0.0470 to 0.0303 percentage points for all, White, and Non-White youth, respectively. These values constitute changes on baseline averages (Table 1) anywhere from a 29% decrease (White youth, lower range) to a 16% increase (Non-White youth, upper range) in criminal conviction rates across populations. Yet plausible magnitudes for effects are generally negative despite the observed decreases in school spending and, for White students in particular, increases in exposure to Non-White students (Table 2).

In Table 3, I provide further evidence that consolidation had null impacts on long-term youth outcomes. Specifically, I find that high school diploma rates do not change post-consolidation, even after accounting for variation across contexts (i.e., county fixed effects) and contemporaneous statewide trends (i.e., time fixed effects) in educational attainment. When considering the 95% confidence intervals, plausible estimates are slightly more precise than those for crime outcomes and range from decreases (or increases) of approximately six percentage points, or about eight percent shifts on the pre-period mean for treatment counties.

Prior work suggests that fewer school resources (Jackson et al., 2016) and changes to the composition of peer groups (Billings et al., 2014) would increase criminal activity—not decrease it. These studies would also predict lower levels of educational attainment following mergers. One potential explanation for my divergent results is that, though related to racial school desegregation and school spending,

Table 2
Impacts of school district consolidation on K-12 school- and district-level mechanisms.

	Total Enrollment	White Enrollment	Non-White Enrollment	# of Operational Schools	Average School Size	Pupils per FTE	Dissimilarity Index	White-Non-White Exposure Index	Non-White-White Exposure Index	Capital Outlays PP	Instructional Expenditures PP	Support Services Expenditures PP
Panel A. Event Study Estimates												
Time=0	−9.977	−76.38	66.94	−0.412***	8.338*	0.276*	−0.0101	0.00546**	−3.53e-05	−208.2	89.97	−12.25
	(109.3)	(60.18)	(85.41)	(0.158)	(4.725)	(0.155)	(0.00696)	(0.00264)	(0.00341)	(209.1)	(100.0)	(54.53)
Time=1	6.231	−116.3	121.7	−0.364	4.996	−0.0436	−0.00991	0.00909**	0.000739	−102.7	86.66	−35.17
	(213.9)	(98.29)	(165.0)	(0.263)	(7.029)	(0.168)	(0.00823)	(0.00364)	(0.00521)	(216.0)	(92.33)	(48.95)
Time=2	32.95	−167.6	199.3	−0.451	5.001	0.316	−0.0107	0.0140**	0.00245	188.0	79.39	−41.04
	(314.1)	(147.2)	(251.3)	(0.343)	(8.716)	(0.226)	(0.0100)	(0.00592)	(0.00706)	(420.7)	(98.63)	(51.94)
Time=3	131.3	−191.3	321.2	−0.436	9.397	0.668	−0.0214	0.0173**	0.00282	−147.3	34.11	−109.8**
	(416.1)	(187.7)	(341.2)	(0.412)	(10.10)	(0.433)	(0.0146)	(0.00857)	(0.00828)	(263.8)	(97.76)	(53.50)
Time=4	254.9	−197.2	450.9	−0.409	9.297	0.136	−0.0196	0.0161*	−0.000464	−321.0	−28.64	−162.2***
	(504.6)	(226.4)	(436.1)	(0.477)	(9.331)	(0.249)	(0.0157)	(0.00957)	(0.00900)	(295.8)	(95.13)	(53.09)
Time=5	333.9	−208.4	540.3	−0.254	8.209	0.294	−0.0181	0.0185*	−0.000515	−204.3	−61.35	−185.3***
	(599.7)	(270.5)	(520.9)	(0.549)	(10.22)	(0.231)	(0.0173)	(0.0102)	(0.0100)	(260.2)	(111.0)	(61.44)
Time=6	401.2	−219.2	618.5	−0.157	8.942	0.262	−0.0213	0.0199*	−0.000611	−259.3	−105.7	−227.3***
	(698.6)	(326.5)	(604.2)	(0.590)	(11.88)	(0.223)	(0.0185)	(0.0105)	(0.0107)	(264.0)	(106.4)	(65.88)
Time=7	441.8	−259.4	699.2	−0.0750	7.455	0.0745	−0.0259	0.0199*	0.000394	−300.4	−97.75	−228.6***
	(786.2)	(368.2)	(706.7)	(0.696)	(13.81)	(0.217)	(0.0197)	(0.0109)	(0.0122)	(239.0)	(116.3)	(74.50)
Time=8	541.5	−247.1	786.6	0.0612	9.452	0.230	−0.0234	0.0201*	−0.000452	−375.9	−168.4	−288.8***
	(890.7)	(423.7)	(806.0)	(0.769)	(17.15)	(0.256)	(0.0194)	(0.0114)	(0.0124)	(269.7)	(117.3)	(76.95)
Time=9	652.5	−272.2	922.7	0.117	14.95	0.384	−0.0203	0.0219*	−0.00232	−235.3	−192.0*	−284.3***
	(999.1)	(481.6)	(917.6)	(0.854)	(18.31)	(0.276)	(0.0195)	(0.0121)	(0.0132)	(297.6)	(113.5)	(81.63)
Time=10	753.3	−256.9	1008	0.316	16.04	0.170	−0.0230	0.0246*	−0.00200	−230.3	−242.3**	−319.7***
	(1114)	(540.1)	(1016)	(0.969)	(20.64)	(0.308)	(0.0207)	(0.0126)	(0.0140)	(243.4)	(119.5)	(81.95)
Time=11	831.6	−263.5	1093	0.310	19.51	0.306	−0.0299	0.0268**	0.000338	−73.75	−301.1**	−364.4***
	(1238)	(608.4)	(1128)	(1.071)	(20.96)	(0.315)	(0.0221)	(0.0135)	(0.0148)	(280.1)	(122.4)	(88.98)
Time=12	898.3	−238.3	1135	0.590	21.71	0.411	−0.0302	0.0261*	0.00149	49.85	−309.3***	−375.6***
	(1375)	(707.5)	(1225)	(1.343)	(21.65)	(0.380)	(0.0221)	(0.0141)	(0.0160)	(280.6)	(116.4)	(98.00)
Panel B. Difference-in-Difference Estimates												
Post	405.3	−208.7	612.6	−0.0896	11.02	0.266	−0.0203	0.0184**	0.000141	−170.8	−93.57	−202.6***
	(703.6)	(333.1)	(626.9)	(0.612)	(11.92)	(0.192)	(0.0156)	(0.00934)	(0.00987)	(200.1)	(98.92)	(61.90)
Panel C. Parametric Event Study Estimates												
Post	−79.36	−127.7	47.71	−0.592**	3.995	0.233	−0.0110	0.00928**	0.000971	−156.0	124.5	−12.73
	(153.4)	(96.12)	(95.08)	(0.247)	(6.940)	(0.177)	(0.00903)	(0.00470)	(0.00533)	(221.7)	(100.7)	(51.85)
Pre-trend	6.520	0.0879	7.191	−0.0218	0.680	−0.000332	−7.65e-05	2.30e-05	−0.000435*	31.26**	2.992	−1.769
	(7.805)	(3.336)	(6.249)	(0.0162)	(0.460)	(0.0141)	(0.000468)	(0.000167)	(0.000240)	(15.88)	(2.683)	(2.382)
Post-trend	80.78	−13.50	94.14	0.0837	1.171	0.00570	−0.00155	0.00153	−0.000138	−2.471	−36.35***	−31.65***
	(105.2)	(54.45)	(96.80)	(0.0986)	(1.976)	(0.0315)	(0.00159)	(0.00101)	(0.00122)	(26.62)	(9.570)	(7.525)
Panel D. Number of Counties with Pre-Post Data												
Consolidated	18	18	18	18	18	18	18	18	18	15	15	15
Non-consolidated	59	59	59	59	59	59	59	59	59	59	59	59

Notes: Panels A, B, and C of the Table provide regression coefficient estimates showing the average impacts of school district consolidation on K-12 school- and district-level mechanisms in years following consolidation (e.g., Time=0 reflects the first year of consolidation; Post reflects all periods following consolidation) using the two-stage estimation process recommended by Gardner (2021; see Eqs. (1) and (2). Standard errors clustered at the county level in parentheses. FTE = full time equivalent teacher. PP = per pupil. All expenditures outcomes (including capital outlays) are in 2020 dollars. Each column displays coefficients from a separate regression with the column header as the outcome and uses all available data in estimation. For Panel A, B, and C, the range of leads and lags included include years since consolidation from −3 to 12.

* $p<.1$,.
** $p<.05$,.
*** $p<.01$.

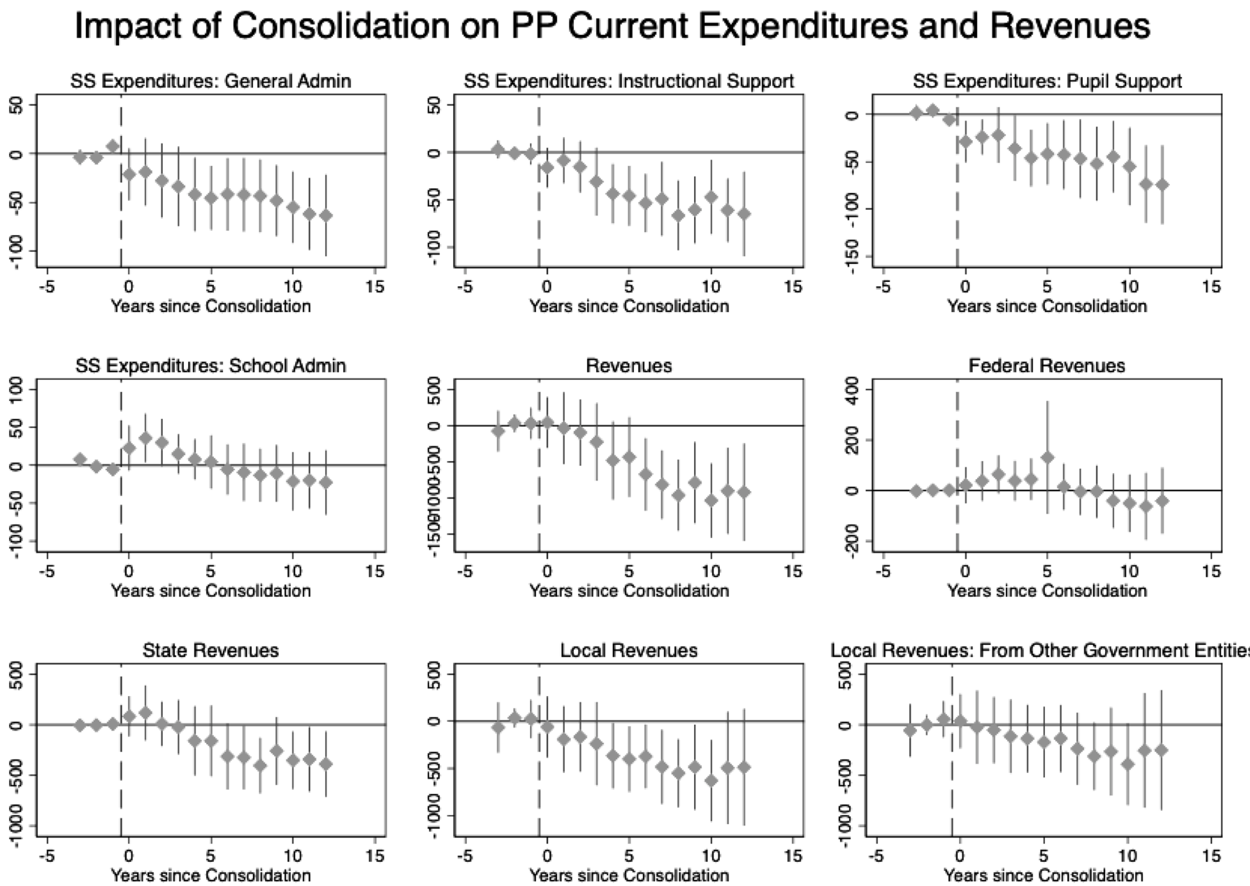

Fig. 2. Average impacts of school district consolidation on detailed K-12 school- and district-level finance measures with 95% confidence intervals.
Notes: The figure plots regression coefficient estimates for dummies that capture the years since school district consolidation (range from −3 to 12) using the two-stage estimation process recommended by Gardner (2021; see Eqs. (1) and 2). Each separate plot displays coefficients from a separate regression using all data available for the outcome named in the subtitle. Standard errors are clustered at the county level. All expenditure and revenue outcomes are in 2020 dollars. SS = support services. PP = per pupil. No specific period acts as a reference period because differences between treated and comparison observations are implicitly normalized to zero for pre-periods by Gardner (2021).

consolidation does not impact these mechanisms at the magnitude necessary to substantially shift students' in-school experiences. Indeed, the integrative influence of mergers in North Carolina is smaller than those observed from desegregation efforts following *Brown v. Board* (Reber, 2005) and even those documented following the release of school districts from desegregation orders (Reardon et al., 2012). Relatedly, historic efforts to reduce racial isolation across schools were not accompanied by negative effects for White youth (Johnson, 2011). Other evaluations of policies connected to influxes or effluxes of financial resources to districts similarly demonstrate larger raw effects on school spending (Jackson et al., 2016). Finally, by teasing out spending effects of district consolidation by type of current expenditure, I find that negative impacts are not solely concentrated in instruction, nor do they translate to other key school inputs such as larger class sizes or school closures, which may also explain the more bounded results regarding youth outcomes.

4.3. Robustness

In Appendix B, I display results to additional sensitivity tests, including: estimating alternative DD and ES approaches (i.e., standard two-way fixed effects models and the method recommended by Callaway & Sant'Anna, 2021); using log-transformed outcomes for the main set of K-12 school- and district-level mechanisms that might not be normally distributed (i.e., enrollment counts and per-pupil expenditures); analyzing only counties with fully balanced longitudinal data; addressing concerns of potential anticipation effects; and examining whether the main results can be explained by the impacts observed in individual counties with consolidating districts. Across all robustness checks my results remain qualitatively the same.

5. Conclusion

In this study, I show that the consolidation of school districts in North Carolina led to decreased expenditures per-pupil on instruction and district support services, while also expanding school integration by race. These impacts were not accompanied by commensurate effects on other theoretically related school- and district-level mechanisms, including class and school size or operation status. Nor do I find evidence that mergers negatively impacted the long-term involvement of youth with the criminal justice system or their educational attainment, despite changes to the demographics of students' schools and the financial resources expended on their education. I confirm that reasonable mechanisms inform school finance impacts. For example, capital outlays do not change, but investments in central office personnel dip immediately following unification. Finally, I demonstrate the robustness of my results and provide evidence that the assumption necessary to recover causal effects using my difference-in-differences approach is credible. All together, my findings yield among the first pieces of rigorous empirical evidence for the impacts of school district consolidation on both K-12 school- and district-level mechanisms and youth outcomes in the long term.

Subsequent research should explore the impact of mergers on other outcomes, as undocumented negative—or positive—effects may mask

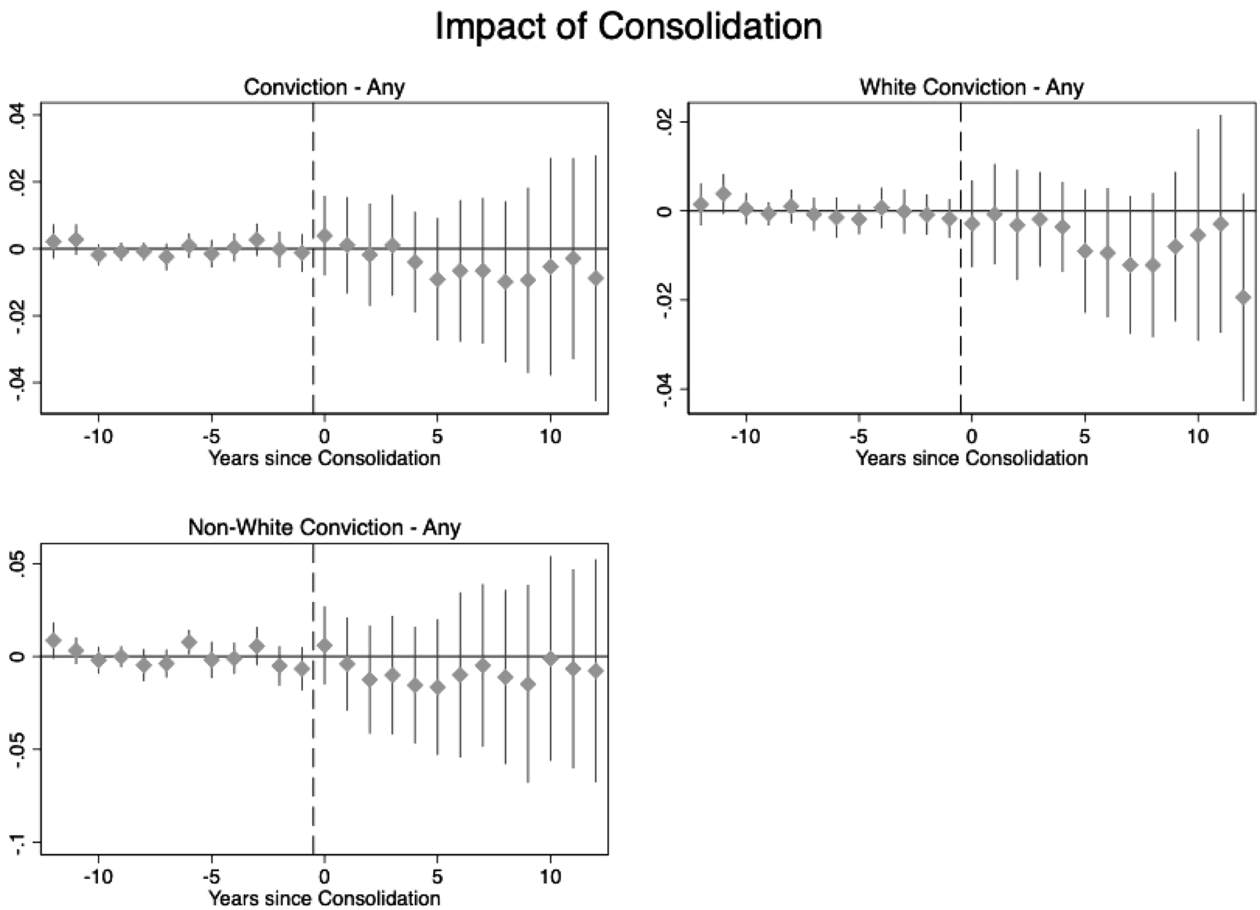

Fig. 3. Average impacts of school district consolidation on criminal conviction rates for youth in early adulthood with 95% confidence intervals.
Notes: The figure plots regression coefficient estimates for dummies that capture the years since school district consolidation (range from −12 to 12) using the two-stage estimation process recommended by Gardner (2021; see Eqs. (1) and 2). Year specifically describes the year a given birth cohort turned 18 (e.g., at −1, the birth cohort turned 18 one year prior to consolidation). Each separate plot displays coefficients from a separate regression using all data available for the outcome named in the subtitle. All outcomes are weighted by the relevant county-level count of births in 1989. Standard errors are clustered at the county level. No specific period acts as a reference period because differences between treated and comparison observations are implicitly normalized to zero for pre-periods by Gardner (2021).

Table 3
Impacts of school district consolidation on educational attainment.

	Diploma rate	Diploma rate	Diploma rate	Diploma rate
Post	0.00153	−0.0019	−0.00327	0.00976
	(0.0265)	(0.0278)	(0.0294)	(0.0242)
Treat Pre-Period Mean	0.745	0.745	0.745	0.745
Treat Pre-Period SD	(0.070)	(0.070)	(0.070)	(0.070)
Pre Year	1991	1991	1991	1991
Post Year(s)	1998–2000	1998	1999	2000

Notes: The Table provides regression coefficient estimates showing the average impacts of school district consolidation on high school diploma rates. The regressions estimated are two-way fixed effects models for standard two-period difference-in-differences: $DiplomaRate_{it} = \beta \times Post_{it} + \lambda_i + \gamma_t + \varepsilon_{it}$. Standard errors clustered at the county level in parentheses. Five treatment counties are dropped because they consolidated in 1990 or 2005 and/or do not have pre- or post-period data to analyze. Outcomes are weighted by county-level eighth grade enrollment in 1987. Each column displays coefficients from a separate regression with the column header as the outcome. *$p<.1$, **$p<.05$, ***$p<.01$.

the policy's true cost effectiveness. This work might draw on P-20 data systems to better tease out student-level mechanisms or link consolidations to measures of labor market success like wages and employment. Leveraging information from these sources gathered by other state agencies in particular may also help with the external validity of my findings, given the unique legislative context of North Carolina, as well as its distinct school funding formula.

In addition to building on the data used in this paper, future studies should investigate the extent to which impacts of school district consolidation on K-12 school- and district-level mechanisms as well as youth outcomes exhibit heterogeneity. The conclusion from my results is that, on average, systems that consolidate become more cost-effective—at least for the outcomes that I analyze—while also accruing other desirable benefits (i.e., reduced school segregation by race). However, this overall finding may mask important differences across contexts tied to what consolidated districts actually do following a merger. In Appendix C, I highlight substantial variation across counties in North Carolina in key characteristics that relate to implementation; for example, integration does not necessarily result from consolidation if district leaders do not enact new policies that change where students go to school (e.g., redrawing school boundaries; Siegel-Hawley, 2014). In other words, unification may be a necessary but insufficient condition for operational changes to occur in districts and research should thus elucidate the specific policies that consolidation allows public officials to effectively pursue.

For practitioners and policymakers, the results of my study show a promising avenue for remedying the greatest contributor to school segregation in the present day—the isolation of Black and White students across districts located in the same metropolitan area (Fiel, 2013). The *Milliken v. Bradley* Supreme Court decision limited the purview of inter-district integrative efforts. But state policymakers could potentially follow North Carolina's lead in enacting legislation that would facilitate district mergers and help establish the necessary conditions for more substantially addressing persistent segregation. Conversely, the extent to which state laws allow for district fragmentation should be limited (Diem et al., 2014). Of course, consolidation efforts may continue to face resistance from stakeholders with more socioeconomic and racial privilege (Howley et al., 2011), but my findings at least provide some evidence that district mergers can lead to arguably more cost-effective school systems—evidence that can be used to help build support for consolidation. Ultimately, the potential benefits of school district consolidation may not be outweighed by the costs and, as such, more policymakers should consider how to best employ it to expand equity when facing constrained resources.

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CRediT authorship contribution statement

Mark J. Chin: Conceptualization, Methodology, Software, Validation, Formal analysis, Investigation, Data curation, Writing – original draft, Writing – review & editing, Visualization.

Declaration of Competing Interest

None.

Data availability

Data will be made available on request.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econedurev.2023.102432.

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