August 18, 2025

PUBLIC COMMENT IN ADVANCE OF AUGUST 19 MEETING OF THE ACT 73 REDISTRICTING TASK FORCE

Dear Members of the Task Force:

This is a follow-up to our letter to you of July 29 in which we attached two documents developed by the Rural School Community Alliance (RSCA) for your consideration: *Community, Democracy and Education: The Case for Voluntary Collaboration and Supervisory Unions Position Statement*; and *School Districts and Governance Models in Vermont: Overview.* These two documents were accompanied by a cover letter. We are re-sending this material as a single PDF since we find none of it listed on the website for the Task Force, nor any other materials submitted as public comment. **We respectfully request that these documents and all others submitted as public comment be timely submitted to Task Force members for their review, and that they also be posted on the Task Force website and included for the record with the Task Force's final recommendations.**

With today's letter, we also are attaching a report of a 2022 study, **"The Effect of School District Consolidation on Student Achievement," by Josh B. McGee, Jonathan N. Mills and Jessica S. Goldstein (AERA, 2022. https://journals.sagepub.com/home/epa).** This study describes outcomes resulting from recent consolidation efforts in the State of Arkansas—similar to what is envisioned by Vermont's Act 73.

We commend this study to your attention because of its relevant findings in a rural state. The study reports that there were no significant cost savings through economies of scale achieved through school district consolidations, and little to no improvements in measurable academic gains for students. Crucially, however, there were a notable number of detrimental impacts including school closures. Among those negative impacts, the report cites research that found diminished populations, fewer community schools, reduced property values and other negative consequences in the affected areas as a result of implementation of Arkansas' school district consolidation law, Act 60.

While the RSCA normally emphasizes the importance of the Redistricting Task Force using Vermont-specific data in developing its recommendations, there are sufficient relevant parallels with the impact of Arkansas' Act 60 that we ask for a review of its outcomes as part of your deliberations. We also bring the study to your attention in part as a response to Senator Gulick's comment during the first meeting of the Task Force on August 1 in which she stated, "we don't even know if consolidation does save money, we don't even know that." We are submitting this evidence in partial response to the Senator's implied question.

Research demonstrates there is no clear evidence that school district consolidation does save money, especially in rural areas. There is counter-evidence that it can and often does cause harm. As the RSCA has cited about potential savings in various testimony concerning H.454 (now Act 73), according to University of Vermont Professor Daniella Hall Sutherland (2025): "There is over 100 years of research on the outcomes of school and district consolidation, yet there is no empirical consensus that consolidation results in reduced educational costs in rural areas. In Vermont, where 71% of our schools are rural, this research should not be taken lightly. In rural contexts, projected savings are offset by increased transportation costs, staff salaries, and infrastructure needs."

Where research does support savings through economies of scale and increased student performance, it tends to come from research in urban and suburban settings, not from research within rural states like Vermont. That may be the case in the comments made by Task Force member Jay Badams on August 1 when he referenced prior consolidation efforts.

That leads us to the RSCA's summary recommendations at this time:

Stand on Vermont-specific evidence. If resting on other evidence, make sure it is relevant to Vermont, including its rural areas.

Question assumptions and test them with evidence. Senator Gulick was questioning assumptions when she said "we don't even know if consolidation does save money."

Question assertions. When statements are made as if they are fact, make sure those assertions are supported by Vermont-specific and/or relevant evidence.

As ever, we appreciate the thoughtfulness by which Task Force members are approaching this important and consequential task.

Sincerely,

Cheryl Charles

Cheryl Charles, Ph.D., Chair, RSCA Steering Committee
On behalf of RSCA Steering Committee Members, Jeanne Albert, Ph.D., Dan MacArthur, Margaret MacLean, David Schoales and representatives of RSCA's more than 100 member towns in Vermont

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Research Article

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The Effect of School District Consolidation on Student Achievement: Evidence From Arkansas

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University of Arkansas

School district consolidation is one of the most widespread education reforms of the last century, but surprisingly little research has directly investigated its effectiveness. To examine the impact of consolidation on student achievement, this study takes advantage of a policy that requires the consolidation of all Arkansas school districts with enrollment of fewer than 350 students for two consecutive school years. Using a regression discontinuity model, we find that consolidation has either null or small positive impacts on student achievement in math and English Language Arts (ELA). We do not find evidence that consolidation in Arkansas results in positive economies of scale, either by reducing overall cost or by allowing for a greater share of resources to be spent in the classroom.

Keywords: *achievement, economics of education, educational policy, policy, restructuring, rural education, econometric analysis, quasi-experimental analysis, regression discontinuity, consolidation, district size*

District consolidation has been one of the most prevalent education reforms over the last century, and it continues to be debated in statehouses across the country (Howley et al., 2011).[1] As a result of consolidation efforts, the number of public school districts in the United States declined from 117,108 to 13,551 between 1940 and 2018.[2] Despite the scale of this reform effort, relatively little rigorous research explores the effect of district and school consolidation on student achievement. For example, a 2002 literature review by Andrews, Duncombe, and Yinger shows most prior research focuses on the financial costs and benefits of consolidation rather than student achievement. The studies included in this review find some evidence that district consolidation results in positive economies of scale.

While much of the early research exploits existing variation on school or district enrollment to estimate the effects of district size, several recent papers have looked more specifically at consolidation as an intervention. Here too, many authors have chosen to focus on outcomes other than achievement such as costs, housing prices, and teacher reactions (see Berry & West, 2008; Duncombe & Yinger, 2007; Hu & Yinger, 2008; Nitta et al., 2010). The few papers that investigate the impact of consolidation on student achievement have found mixed results regarding the direction, magnitude, and longevity of effects (see Beuchert et al., 2018; Brummet, 2014; Cooley & Floyd, 2013; De Haan et al., 2016; Engberg et al., 2012; Humlum & Smith, 2015; Liu et al., 2010).

TABLE 1

Districts Affected by Act 60

First year after consolidation	Consolidated due to enrollment	Consolidated and receiving districts
2005	58	99
2006	2	4
2007	4	14
2008	–	–
2009	–	–
2010	1	2
2011	4	15

Source. Arkansas Department of Education.

Unfortunately, the majority of these studies, both those examining existing variation in district and school size and those examining consolidation-induced changes in size, are susceptible to endogeneity issues. Existing variation in district and school enrollment is not randomly assigned. For example, some districts and schools are larger or smaller as a function of their academic quality, a correlation implying biased estimation of the impacts of scale on achievement. In addition, districts and schools are rarely selected at random for consolidation. In such cases, unobserved characteristics associated with their academic quality are likely to be associated with the probability that a district or school is consolidated.

This article leverages a natural experiment to understand the effects of consolidation on student achievement. Arkansas Act 60, enacted in 2004, requires the consolidation of all districts with enrollment of less than 350 students for two consecutive years (Arkansas Department of Education, 2005; Arkansas 84th General Assembly, 2003; Holley, 2015).[3] We leverage the exogenous variation created by this policy change and the policy's clear enrollment threshold to learn about the effects of consolidation on student outcomes and district finances.

The remainder of this article proceeds as follows. The "Arkansas Act 60" section provides some of the history and important details about Arkansas Act 60, which implemented mandatory district consolidation. The "Data" section outlines the data used in this study. The "Empirical Model" section describes our empirical method, and the "Results" section provides our results. The article concludes with the "Conclusion" section.

Arkansas Act 60

School district consolidation has long been debated in Arkansas (Ledbetter, 2006). The latest wave of consolidation in Arkansas arose in response to school finance litigation that occurred throughout the late 1990s and early 2000s. The decade-long litigation culminated in 2003 with the Arkansas Supreme Court ruling that the state's school funding system was unconstitutional in *Lake View School District vs. Huckabee*.[4]

Governor Mike Huckabee responded to the court's decision by convening the State Legislature in the Second Extraordinary Session of 2003. Governor Huckabee proposed large-scale school district consolidation to reduce district administrative costs and provide greater educational opportunity for students. Governor Huckabee's original proposal would have resulted in threefold reduction in the number of school districts in Arkansas. Compromise legislation was enacted in early 2004. The Public Education Reorganization Act, Arkansas Act 60, required the consolidation of any district with average daily attendance of fewer than 350 students for two consecutive school years (Arkansas Department of Education, 2005; Holley, 2015).[5]

The final enrollment threshold of 350 students, while not as drastic as the Governor's original proposal, did result in a substantial number of district consolidations in the years that followed. Table 1 presents the number of district

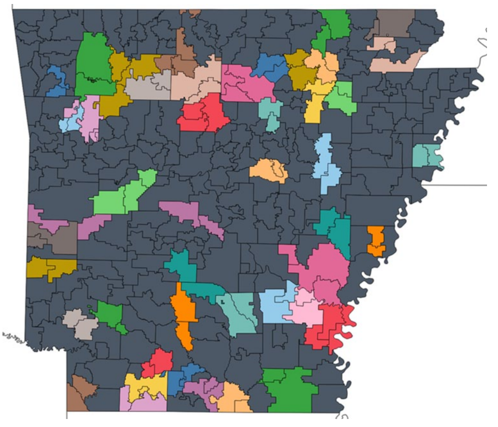

FIGURE 1. *Map of districts consolidated due to Act 60.*

consolidations occurring each year beginning with the 2004–2005 school year. In the first year the law was in effect, 58 school districts were required to consolidate. These school districts will serve as the treatment group in our analysis (see the Empirical Model section).

Although Act 60 continues to have an impact as enrollments decline in rural districts, only a few districts have been required to consolidate since the initial wave in 2005. Recent legislation has also limited the ongoing impact of Act 60. Pushback against mandatory consolidation led the legislature to pass and Governor Asa Hutchinson to sign Act 377 of 2015 which allows school districts that fall below the 350 threshold to apply for a waiver from the State Board of Education.[6]

Figure 1 shows the geographic location and district borders of the 99 districts that, in the 2004–2005 school year, were either consolidated due to Act 60 (58 districts) or merged with or annexed one of the Act 60 districts (41 districts).[7] For the remainder of this article, we refer to the latter group as receiving districts. The map in Figure 1 depicts district borders in the year prior to consolidation and districts are color-coded to indicate which districts combined in the 2004–2005 school year because of Act 60. The initial round of consolidations was relatively widespread across the state, affecting districts in every region. Districts subject to Act 60 enjoyed some autonomy in determining which other district to merge with; however, the overwhelming majority merged with or were annexed by adjoining district.

While Act 60 does not specifically require school closure following a consolidation, closures often occur to eliminate redundant course offerings and take advantage of potential economies of scale. School closures can have either a negative or a positive effect on student performance depending on which schools are closed and how closures are implemented (Beuchert et al., 2018; Brummet, 2014; Engberg et al., 2012). Table 2 presents the number of school closures that occurred following a district consolidation. Unsurprisingly, consolidation has had a nontrivial impact on school closures. In total, 105 school closures occurred between 2005 and 2011.

Data

Our analysis uses a rich panel of demographic and academic performance data for all students who took the Arkansas mathematics and English Language Arts (ELA) Benchmark exams between the 2003–2004 and 2007–2008 school years. We also have data on district and school enrollment collected from the Arkansas Department of Education (ADE) as well as information on district consolidation and school closure compiled with the help of ADE and district officials. We merge these data to create a master panel that includes roughly 200,000 records per year across Grades 3 through 8, with multiple records for students across school years.

While consolidation occurs at the district level, our analysis is conducted using student-level data. We identify students as affected by consolidation if they were in a district that was forced to consolidate due to Act 60 in the 2004–2005 school year, the first year the law was in effect, and continue to identify them as such for the remainder of their appearance in the data.

Arkansas did not begin testing third-grade students until the 2004–2005 school year. Since our empirical model, described in the section below, controls for previous year's test score, students in the fourth grade in 2003–2004 are the youngest cohort in our analysis. Our study sample includes data from the 2003–2004 school year through the 2007–2008 school year, allowing us to follow all consolidation-affected

TABLE 2
School Closures in Consolidated Districts

	Number of schools closed			
First year since consolidation	Elementary	Middle	Secondary	Total
2005	5	–	11	16
2006	8	–	13	21
2007	16	4	15	35
2008	2	–	4	6
2009	8	–	1	9
2010	5	1	3	9
2011	4	–	5	9

Source. Arkansas Department of Education.

students through their last year of testing (i.e., eighth grade).[8]

Empirical Model

Endogeneity concerns prevent us from directly examining the effects of consolidation on student outcomes. In most cases, district consolidation occurs through selection—districts voluntarily choose to consolidate for any number of reasons such as perceived cost benefits or to take advantage of state financial incentives. Unfortunately, the very nature of this selection makes it likely that results in simple models comparing consolidated districts to unaffected districts will be biased. For example, if a poor performing district consolidates with a larger, higher performing district to take advantage of fiscal incentives, one might expect a decline in the overall average performance in the resulting district. This decline will be reflected in the estimated coefficients from a standard ordinary least squares (OLS) model but cannot be attribute to the causal effects of the consolidation itself. Fortunately, the natural experiment created by Act 60 allows us to use a regression discontinuity (RD) model to mitigate endogeneity concerns.

Estimating the Achievement Impacts of Consolidation via RD Design

We examine the impacts of consolidation on student performance using a standard RD approach.[9] The explicit enrollment cutoff designated by Act 60 allows us to employ a sharp RD model whereby students in districts with enrollment of less than 350 in the 2 years immediately prior to the passage of Act 60 (i.e., the 2001–2002 and 2002–2003 school years) are assigned to the treatment group and students in the remaining districts represent the control group. If districts were able to game the forcing variable by either purposely avoiding or making themselves subject to the consolidation mandate, then it would undermine the sharp RD design. By limiting our analysis to consolidations that occurred in 2005, we eliminate the possibility of gaming because these consolidations were determined shortly after the law enacted in 2004 and were based on 2001–2002 and 2002–2003 enrollment data, which districts had no ability to alter.

Figure 2 shows district assignment to treatment (i.e., consolidation) based on enrollment in the 2001–2002 and 2002–2003 school years. Three types of districts are represented in Figure 2: Red circles represent districts forced to consolidate by Act 60; blue dots represent districts that merged with or annexed the Act 60 districts (“receiving” districts); and black circles represent districts that were not affected by Act 60. Districts in the lower left quadrant were below the Act 60 enrollment cutoff in both the 2001–2002 and 2002–2003 school years, and all the red circles are in this quadrant. The clear division between the red circles and the other two types of districts in Figure 2 illustrates our ability to implement a sharp RD design.

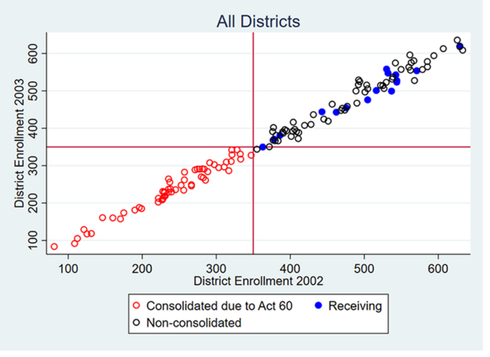

FIGURE 2. *Consolidation treatment assignment by 2-year enrollment.*

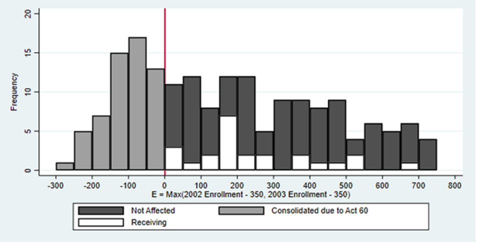

FIGURE 3. *Distribution of forcing variable (**E**) district types.*

Note. The figure presents histograms of forcing variable, ***E***, for three district types: (a) districts that were not affected by Act 60 in 2005 (dark gray), (b) districts forced to consolidate (light gray), and (c) districts who merged with or annexed an Act 60 district (i.e., "receiving districts"; white). Histogram bar width = 50.

We estimate the achievement impacts of consolidation via RD using the following model:

$$Y_{it} = \beta_0 + \beta_1 Y_{i,t-1} + \beta_2 C_{it} + \boldsymbol{E}\theta + \mathbf{X}\gamma + \rho + \varepsilon_{it}, \quad (1)$$

where *Y* represents student *i*'s standardized scale score on a state exam, *C* is an indicator variable for district consolidation due to Act 60, **X** is a vector of individual-level controls (ethnicity, gender, etc.), ρ contains a set of grade and year indicators, and ε is random error. ***E*** is the "forcing" or "assignment" variable that determines whether or not one is subject to the treatment (Imbens & Lemieux, 2008; Reardon & Robinson, 2012).

In our model, the forcing variable, ***E***, is a binding score based on student *i*'s district enrollment in the 2 years prior to the passage of Act 60 (i.e., the 2001–2002 and 2002–2003 school years, Reardon & Robinson, 2012). The binding score is the maximum of each year's enrollment minus the 350 cutoff. Students with a negative binding score attended a district that had enrollment below the 350 cutoff in both the 2001–2002 and the 2002–2003 school years. If a student's binding score falls below zero, then their district was forced to consolidate under Act 60 and they are in the treatment group. Specifically, the binding score is as follows:

$$E_i = \max(2002\,\text{Enrollment} - 350, \; 2003\,\text{Enrollment} - 350). \quad (2)$$

To fully capture any potential curvature in the relationship between district enrollment and achievement, ***E*** includes both linear and quadratic terms. β_2 represents our estimate of the effect of consolidation on student achievement. We estimate this model using a multilevel analysis in which students are nested within districts.

A key component of RD models is defining the local neighborhood around the forcing variable cutoff within which we can reasonably assume local randomization (i.e., the bandwidth). We face an important trade-off when setting the enrollment band that will define our sample: the wider the chosen band, the less appropriate the control group; the smaller the band, the less generalizable the results. We employ two data-driven algorithmic bandwidth selection procedures to help inform our decision. Supplementary Table A2 in the online version of the journal provides both the mean-squared-error optimal bandwidth and the coverage-error-probability optimal bandwidth for both math and ELA (Calonico et al., 2014a, 2014b, 2017). Based on these results, the optimal bandwidth is between 200 and 290. We provide results for bandwidths of 200, 245, and 290.

Figure 3 presents the distribution of districts by values of the forcing variable, ***E***. The figure shows no evidence of a discontinuity in the forcing variable around the cutoff. We formally test for a discontinuity using a McCrary (2008) test

TABLE 3

Descriptive Statistics for Students in Consolidated and Nonconsolidated Districts

	Distance ≤|200|				Distance ≤|290|			
	Consolidated districts (treatment) (N = 2,848)	Other districts (control) (N = 5,484)			Consolidated districts (treatment) (N = 2,995)	Other districts (control) (N = 7,961)		
Variable	Avg.	Avg.	Differences	*SE*	Avg.	Avg.	Differences	*SE*
Female	0.51	0.48	0.02*	0.01	0.51	0.49	0.02*	0.01
Ethnicity								
Black	0.17	0.16	0.01	0.05	0.17	0.13	0.04	0.05
Hispanic	0.01	0.03	−0.02**	0.01	0.01	0.03	−0.02**	0.01
White	0.80	0.81	0.00	0.05	0.80	0.84	−0.04	0.05
Other	0.02	0.01	0.01	0.01	0.02	0.01	0.01	0.01
Grade								
4	0.31	0.32	−0.01	0.01	0.31	0.32	−0.01	0.01
6	0.35	0.33	0.02	0.01	0.35	0.33	0.01	0.01
8	0.34	0.35	0.00	0.01	0.35	0.35	0.00	0.01
Standardized achievement								
Math	−0.04	0.00	−0.05	0.07	−0.05	0.03	−0.07	0.06
ELA	−0.11	−0.01	−0.11	0.06	−0.12	0.02	−0.13**	0.05
Forcing variable: ***E***	−78.45	114.21	−192.67***	10.49	−86.08	153.29	−239.36***	11.64

Note. Analysis restricted to students in the 2003–004 school year. Achievement is standardized within grade and year across all students in the testing data. ELA = English Language Arts; *SE* = standard errors that account for clustering of students within districts.
*$p < .10$. **$p < .05$. ***$p < .01$.

and do not find statistically significant discontinuity ($p = .82$). Figure 3 also shows that there are a meaningful number of districts close to the threshold. It is worth noting that the larger algorithmic bandwidth selection recommendations include most of the districts affected by Act 60 in 2005.

Study Sample

Using an RD procedure requires that we assume the exogenous Act 60 enrollment cutoff approximates random assignment of districts to the consolidation treatment in the immediate neighborhood of the cutoff. If this assumption is accurate, districts just above the Act 60 enrollment cutoff should be essentially the same as the consolidated districts just below the cutoff. We test this hypothesis, presenting averages for several key demographic variables for students in both the treatment and control groups for the ±200 and ±290 bandwidths in Table 3. The table also provides differences and the standard errors for those differences.

White students represent the majority (80%) in both samples and bandwidths, whereas Black students are the second largest demographic group (17%). For both bandwidths, the treatment group is 2 percentage points more female ($p < .1$) and two percentage points more Hispanic ($p < .05$) than the control group. Both the treatment and control groups are relatively similar on the remaining variables for both bandwidths. The lone exception is ELA for the largest bandwidth, where baseline treatment group performance is −0.13 *SD* units compared with the control group ($p < .05$).

Even if our RD model approximates random assignment at the level of school districts, it may not necessarily do so at the student level. Each district has a particular demographic profile and those just above and below the consolidation threshold may differ at the student level. While the differences between treatment and control

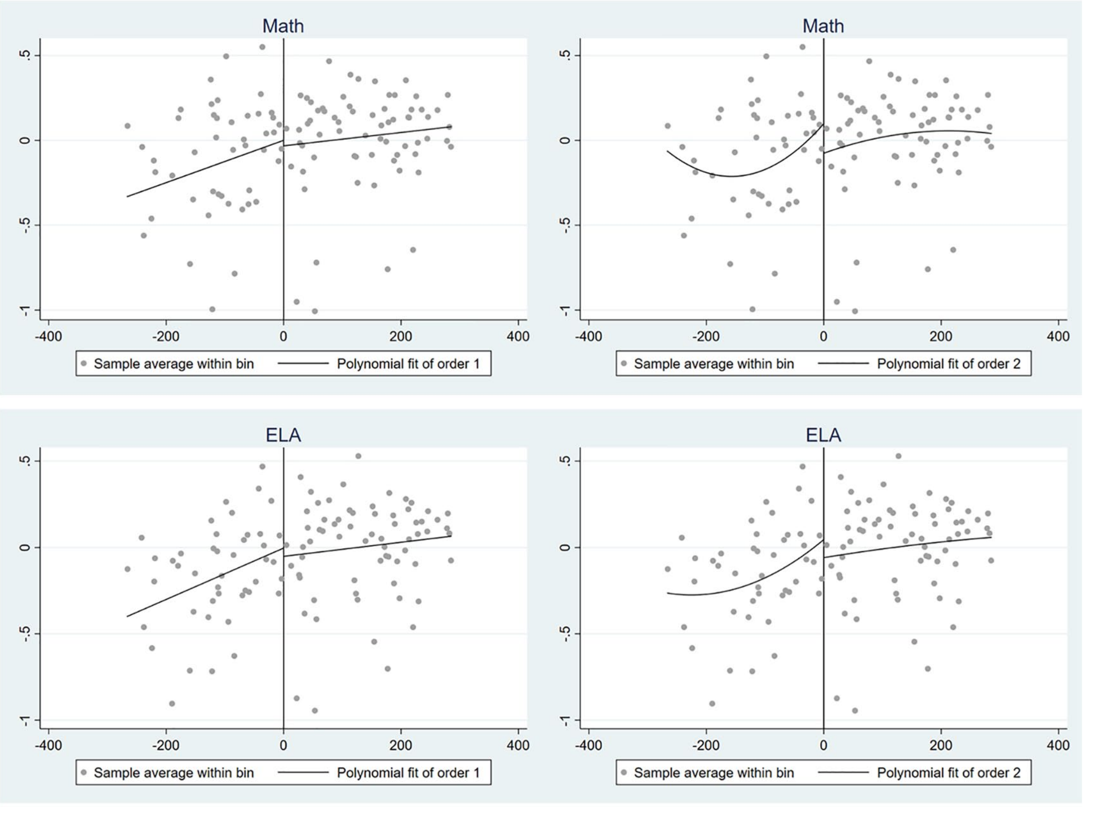

FIGURE 4. *District achievement by forcing variable.*
Note. The figure presents distribution of standardized achievement in 2005 (i.e., 1-year post consolidation) by forcing variable, ***E***. ELA = English Language Arts.

group documented in Table 3 suggest caution in interpreting the RD results, especially the ELA results at larger bandwidths, they are not large enough to undermine the entire analysis. The 2 percentage point difference for gender and Hispanic students, though statistically significant, is not meaningfully large in practical terms. While the ELA difference is somewhat larger, it is only significant for the largest bandwidth.

Results

Achievement Impacts

First, we investigate whether there is graphical evidence of a discontinuity in student achievement around the enrollment threshold 1 year after consolidation was implemented. Figure 4 displays both linear and quadratic fits of 2005 district-level math and ELA achievement by our forcing variable. We generated similar plots for 2004–2005 achievement gains that were quite similar to these plots (see Supplementary Figure A1 in the online version of the journal). While Figure 4 shows some evidence of a slight positive discontinuity at the enrollment threshold for both subjects, the difference in all cases appears relatively modest (i.e., less than 0.2 *SD*s).

To investigate the magnitude of these discontinuities, we use the multilevel mixed-effects model discussed above which allows students to be nested within districts. Table 4 presents estimates of the average impact of consolidation on student mathematics and ELA achievement, respectively. Coefficient estimates are presented with standard errors in parentheses. For each bandwidth, we include three separate models: (a) a simple model that only includes a consolidation indicator and linear version of the forcing variable, ***E***, (b) a more complex model that adds lagged achievement and student-level controls,

TABLE 4

Impacts of District Consolidation on Student Achievement

	E ≤|200|			***E*** ≤|245|			***E*** ≤|290|		
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Impacts on standardized math scores									
Consolidated	−0.062*	0.014	0.015	−0.036	0.015	0.037	−0.040	0.009	0.040*
	(0.033)	(0.017)	(0.028)	(0.030)	(0.015)	(0.024)	(0.029)	(0.015)	(0.022)
Panel B. Impacts on standardized ELA scores									
Consolidated	−0.045	0.027*	0.059**	−0.036	0.009	0.068***	−0.035	0.010	0.049**
	(0.035)	(0.016)	(0.025)	(0.032)	(0.014)	(0.022)	(0.031)	(0.014)	(0.020)
Unique students	20,970	17,141	17,141	25,307	20,686	20,686	27,444	22,413	22,413
Unique districts (2003–2004 school year)	108	108	108	126	126	126	133	133	133
Student characteristics		X	X		X	X		X	X
Linear forcing variable	X	X		X	X		X	X	
Quadratic forcing variable			X			X			X

Note. Unit of analysis is student-year. All models include grade and year fixed effects. Multilevel standard errors account for nesting of students within districts. ELA = English Language Arts.
Source. Authors' calculations.
*$p < .10$. **$p < .05$. ***$p < .01$.

and (c) a preferred specification which also adds the quadratic form of the forcing variable. Each of the regressions presented in Table 4 includes grade and year fixed effects to control for systematic differences in student performance across these domains.[10] Table 4 includes students attending receiving districts (i.e., those that merged with or annexed Act 60 districts) in the control group if they are within the specified bandwidth. As a robustness check, we estimated models that exclude students from receiving districts and find substantively similar results (see Supplementary Table A4 in the online version of the journal).

The estimated effect of consolidation on math achievement is statistically insignificant for most specifications. However, the effect on math is positive and marginally significant for the largest bandwidth (±290) in the model that includes the quadratic form of the forcing variable. Despite being marginally significant, the point estimate is relatively small (0.04 *SD* units).

For ELA, the estimated effect of consolidation in nearly all specifications that include the linear form of the forcing variable is statistically insignificant with point estimates approaching zero. However, in the specifications including the quadratic form of the forcing variable, the estimated impact of consolidation on ELA achievement is positive and statistically significant. The estimates are practically small, ranging from 5% of an *SD* to nearly 7% of an *SD*. We recommend some caution when interpreting the ELA results given that we see a statistically significant gap in baseline ELA performance between treatment and control for the largest bandwidth (see Table 3).

To investigate whether consolidation impacts varied by demographic subgroups, we perform a secondary analysis crossing subgroup dummy variables with the treatment indicator. Tables 5 and 6 provide the results of this subgroup analysis for math and ELA achievement, respectively. We find small, negative, statistically significant impacts on math test score outcomes for Black students relative to White students. The negative impacts for Black students are similar in magnitude or slightly larger than the positive impacts for White students but are more consistently statistically significant. We also find marginally significant, positive impacts for students in the Other ethnicity category relative to White students.[11] Based on these results it appears consolidation may have had a small positive impact on the math achievement of White students and students in the Other ethnicity category. For Black students, the math impacts appear to be either null or small and negative. We found no evidence of differential impacts by subgroup on ELA achievement.

It is also possible that the impacts of consolidation vary over time especially if students require several years to adjust to their new surroundings. Table 7 presents results from models

TABLE 5

Impacts of District Consolidation on Student Subgroup Math Achievement

	E ≤|200|		***E*** ≤|245|		***E*** ≤|290|	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Consolidated	0.030	0.030	0.033*	0.052**	0.030*	0.055**
	(0.020)	(0.029)	(0.018)	(0.026)	(0.018)	(0.024)
Black × Consolidated	−0.049**	−0.048**	−0.063***	−0.061***	−0.062***	−0.059***
	(0.020)	(0.020)	(0.018)	(0.019)	(0.018)	(0.018)
Hispanic × Consolidated	−0.022	−0.022	−0.031	−0.033	−0.015	−0.016
	(0.052)	(0.052)	(0.047)	(0.047)	(0.046)	(0.046)
Other × Consolidated	0.056*	0.056*	0.060*	0.062**	0.056*	0.058*
	(0.033)	(0.033)	(0.031)	(0.031)	(0.031)	(0.031)
Female × Consolidated	0.004	0.004	0.007	0.006	0.007	0.006
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
FRL × consolidated	−0.019	−0.019	−0.019	−0.019	−0.023*	−0.023*
	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)
ELL × Consolidated	−0.031	−0.030	0.031	0.029	0.020	0.019
	(0.116)	(0.116)	(0.101)	(0.101)	(0.100)	(0.100)
Unique students	17,141	17,141	20,686	20,686	22,413	22,413
Unique districts (2003−2004 school year)	108	108	126	126	133	133

Note. Unit of analysis is student-year. All models include grade and year fixed effects. Multilevel standard errors account for nesting of students within districts. FRL = Free or Reduced-Price Lunch; ELL = English Language Learners.
Source. Authors' calculations.
**p* < .10. ***p* < .05. ****p* < .01.

TABLE 6

Impacts of District Consolidation on Student Subgroup ELA Achievement

	E ≤|200|		***E*** ≤|245|		***E*** ≤|290|	
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Consolidated	0.033*	0.066**	0.022	0.079***	0.023	0.061***
	(0.018)	(0.027)	(0.017)	(0.024)	(0.016)	(0.022)
Black × Consolidated	−0.004	−0.005	−0.014	−0.012	−0.012	−0.009
	(0.018)	(0.018)	(0.017)	(0.017)	(0.017)	(0.017)
Hispanic × Consolidated	−0.021	−0.022	−0.014	−0.016	0.003	0.003
	(0.049)	(0.049)	(0.044)	(0.044)	(0.044)	(0.044)
Other × Consolidated	0.003	0.004	−0.001	0.001	−0.001	0.002
	(0.031)	(0.031)	(0.029)	(0.029)	(0.029)	(0.029)
Female × Consolidated	0.004	0.004	0.000	−0.001	0.001	0.001
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
FRL × Consolidated	−0.013	−0.013	−0.017	−0.017	−0.019	−0.019
	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)
ELL × Consolidated	0.007	0.005	0.027	0.023	0.017	0.016
	(0.109)	(0.109)	(0.095)	(0.095)	(0.095)	(0.095)
Unique students	17,141	17,141	20,686	20,686	22,413	22,413
Unique districts (2003−2004 school year)	108	108	126	126	133	133

Note. Unit of analysis is student-year. All models include grade and year fixed effects. Multilevel standard errors account for nesting of students within districts. ELA = English Language Arts; FRL = Free or Reduced-Price Lunch; ELL = English Language Learners.
Source. Authors' calculations.
**p* < .10. ***p* < .05. ****p* < .01.

TABLE 7

Do Results Vary Over Time?

	E ≤|200|		Distance ≤|245|		Distance ≤|290|	
	Linear model	Quadratic model	Linear model	Quadratic model	Linear model	Quadratic model
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Impacts on standardized math scores						
First year of consolidation	−0.034	−0.031	−0.034	−0.012	−0.035*	−0.005
	(0.024)	(0.032)	(0.022)	(0.028)	(0.021)	(0.027)
Second year of consolidation	0.036*	0.038	0.041**	0.064**	0.037**	0.067***
	(0.020)	(0.029)	(0.018)	(0.025)	(0.018)	(0.024)
Third year of consolidation	0.003	0.005	−0.001	0.022	−0.010	0.020
	(0.020)	(0.029)	(0.018)	(0.026)	(0.018)	(0.024)
Fourth year of consolidation	0.022	0.024	0.024	0.047*	0.020	0.050**
	(0.020)	(0.029)	(0.018)	(0.026)	(0.018)	(0.024)
Panel B. Impacts on standardized ELA scores						
First year of consolidation	0.057***	0.089***	0.031	0.089***	0.031	0.070***
	(0.022)	(0.029)	(0.020)	(0.026)	(0.020)	(0.025)
Second year of consolidation	0.016	0.048*	0.008	0.067***	0.006	0.045**
	(0.018)	(0.027)	(0.017)	(0.023)	(0.016)	(0.022)
Third year of consolidation	0.034*	0.066**	0.012	0.070***	0.011	0.050**
	(0.018)	(0.027)	(0.017)	(0.024)	(0.016)	(0.022)
Fourth year of consolidation	0.018	0.050*	−0.002	0.057**	0.003	0.043*
	(0.018)	(0.027)	(0.017)	(0.024)	(0.016)	(0.022)
Unique students	17,141	17,141	20,686	20,686	22,413	22,413
Unique districts (2003–2004 school year)	108	108	126	126	133	133

Note. Unit of analysis is student-year. All models include grade and year fixed effects. Multilevel standard errors account for nesting of students within districts. ELA = English Language Arts.
Source. Authors' calculations.
$^{*}p < .10$. $^{**}p < .05$. $^{***}p < .01$.

in which we use a series of dummy variables to examine nonlinear consolidation impacts across time.

As the results in Table 7 show, the consolidation impacts do not vary dramatically over time and do not differ substantially from the average results in Table 4. The results for math are mostly insignificant, except in the second year after consolidation where there is a pattern of small, positive, and significant results across bandwidths and models, varying from 3.6% of an *SD* to 6.7% of an *SD*.

For ELA, however, the coefficients are largely positive and significant in the first year after Act 60 was implemented. Results fade in magnitude and significance in subsequent years and are generally larger and statistically significant in models that include the quadratic form of the forcing variable. Here too the coefficients are modest. Statistically significant first-year estimates range from 0.057 and 0.09 deviation units and these estimates fade to be between 0.043 and 0.057 S*D* units by the fourth year in the largest bandwidths in models that include the quadratic form of the forcing variable.

In general, our preferred models—which include controls for student demographics—indicate impacts that are either null or small and positive. Math results are largely insignificant. On the other hand, we find a pattern of small, positive, and statistically significant results for ELA especially in models that include the quadratic form of the forcing variable. Overall, school district consolidation does not appear to have had a large measurable impact, either positive or negative, on students' math and ELA performance.

Economies of Scale

The primary motivation that policymakers articulated for consolidating smaller school

TABLE 8

Summary of District Financial Information

	2004				2005		2006		2007		2008	
	Act 60	Receiving (merged or annexed)	All affected	AR avg.	All affected	AR avg.	All affected	AR avg.	All affected	AR avg.	All affected	AR avg.
Summary statistic	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Number of districts	58	41	99	308	46	254	46	252	46	245	46	245
Median area	114	193	148	147	343	166	343	166	351	171	351	171
Total enrollment	13,037	50,675	63,712	422,787	62,917	426,136	62,885	432,371	62,252	433,952	62,139	433,333
Median enrollment	230	852	288	678	946	887	911	891	920	943	909	924
Expenditures/pupil	$7,586	$6,049	$6,363	$6,475	$7,335	$7,307	$7,575	$7,687	$7,883	$7,992	$8,057	$8,256
Certified teachers share	40.8%	45.6%	44.5%	45.4%	43.1%	43.4%	42.6%	42.4%	42.6%	42.5%	42.4%	42.1%
Other certified (e.g., administration) share	7.7%	5.8%	6.3%	5.6%	6.3%	5.8%	6.0%	5.7%	6.2%	6.1%	6.2%	6.1%

Source. Arkansas Department of Education and authors' calculations.

districts in Arkansas was to achieve cost savings through economies of scale. Even if consolidation only had null-to-small positive effects on achievement, Act 60 would still be considered a success if consolidation reduced administrative and other spending outside of the classroom, freeing up resources for additional classroom spending or to be redirected toward other important public purposes.

Unfortunately, we cannot leverage the sharp enrollment threshold used in the previous analysis because districts subject to Act 60, in many cases, merged with districts above the threshold, and it is impossible to disentangle the finances of one from the other after consolidation. A difference-in-difference analysis has the potential to help us estimate causal impacts, but we need to secure better preconsolidation finance trend data before that is feasible.

Instead, to investigate whether districts affected by consolidation experienced positive economies of scale we descriptively compare district-level spending trends before and after consolidation occurred. Table 8 presents a summary of financial information for districts affected by consolidation and Arkansas averages for the 2004–2008 school years.

In 2004, prior to consolidation, districts that would be forced to consolidate by Act 60 spent more on average, a lesser share on classroom teachers, and a greater share on other certified staff like administrators than did other school districts in Arkansas (see Columns 1–4 of Table 8). On the surface, this supports the argument that consolidation had the potential to deliver improvements through greater economies of scale, and Act 60 districts saw their spending converge to statewide averages after consolidating. However, it is unclear how much of this convergence was driven by the existing spending patterns in larger receiving districts.

The appropriate counterfactual is to compare expenditure trends for districts affected by consolidation (both consolidated and receiving) with unaffected districts to see if affected districts exhibit substantial changes after consolidation that deviate from broader state trends. Columns 5 to 12 of Table 8 show that Act 60–affected districts exhibit consistent resource allocation over time to both classroom staff and other certified staff (see last two rows). While affected districts experienced increased spending per pupil, that trend did not deviate meaningfully from the overall state trend. Based on this simple, descriptive analysis we do not find evidence that Act 60 resulted in substantial positive economies of scale for affected districts.[12]

Conclusion

This article adds to our understanding of the effects of consolidation on student achievement and district finances by taking advantage of a natural experiment in Arkansas which occurred when policymakers required the consolidation of all districts with fewer than 350 students for 2 consecutive years. We focus on the first year of implementation when districts were unable to manipulate enrollment in response to the policy, thus providing an exogenous source of variation that we exploit using an RD design.

We find that consolidation had either null or small positive effects on the achievement of students whose districts were forced to consolidate due to Act 60. In addition, while schools that were forced to consolidate did see their spending converge to the statewide average, we find no evidence that Act 60 resulted in meaningful positive economies of scale for affected districts. Overall, Act 60 does not appear to have helped Arkansas students much, but on the other hand, it did not harm them either.

As Act 60 was being debated and implemented many stakeholders raised concerns about the broader social and community impacts which could result from school district consolidation. For example, stakeholders worried that consolidation would result in lower parental involvement, longer bus rides, and less opportunities for extracurricular and after-school activities, and that consolidation would eliminate community schools as the center of public life in small-town, rural Arkansas (Holley, 2015). In addition, shortly after implementation of Act 60 began, a report by the Rural School and Community Trust raised concerns that consolidation and resulting school closures were having a disproportionate impact on rural, Black communities (Jimerson, 2005).

Similar concerns are echoed in the academic literature in which researchers provide evidence that consolidation can negatively impact communities' economic prospects, erode social/cultural capital, among other negative effects (Howley et al., 2011; Schafft, 2016). A recent study of the impact of Act 60 on affected communities found that consolidation had significant negative impacts on population, availability of community schools, and property values and that communities of color may be disproportionately affected (Smith & Zimmer, 2022). Given that we do not find large positive impacts on achievement or meaningful positive economies of scale, which were the primary motivations behind consolidation in Arkansas, the potential negative impacts on communities should carry considerable weight in future policy debates on this issue.

While this article adds to the literature by providing causal estimates of the impact of consolidation on student performance, several important questions remain. A limitation of our analysis is that it treats all consolidations as homogeneous, rather than heterogeneous, events. This is largely due to our identification strategy that leverages a statewide mandatory district-level consolidation policy to identify causal effects. Other research has shown that the impact of consolidation can be heterogeneous, and understanding what factors lead to positive/negative impacts has important policy implications (Beuchert et al., 2018; Brummet, 2014; Engberg et al., 2012). While we provide a secondary analysis of the differential impact of Act 60 on student subgroups, much is left to be done to fully understand the various context-dependent impacts of consolidation.

In addition, it is possible that reducing the number of administrative units (i.e., districts) will pay dividends in the future, but it is also possible that larger districts are less responsive to the needs of individual communities, harming students down the line. These long-term effects are yet unknown, making this a fruitful area for future study.

Other areas for future work include building out a causal analysis around economies of scale, investigating the effect of consolidations on receiving districts, and investigating the impact of school closures that result from consolidation.

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Supplemental Material

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Notes

1. https://www.washingtonpost.com/outlook/2021/08/19/more-than-century-policymakers-have-mishandled-rural-schools/

2. Table 214.10 in NCES's 2019 Digest of Education Statistics.

3. https://www.arkleg.state.ar.us/Acts/FTPDocument?type=PDF&file=60&ddBienniumSession=2003%2FS2

4. https://encyclopediaofarkansas.net/entries/lake-view-school-district-no-25-v-huckabee-4167/

5. For the remainder of this article, when we refer to Act 60 or an enrollment level of 350, we are specifically referring to a district's enrollment in the 2 years prior to the enactment Act 60—the 2001–2002 and 2002–2003 school years.

6. https://www.arkleg.state.ar.us/Acts/FTPDocument?type=PDF&file=377&ddBienniumSession=2015%2F2015R

7. See Supplementary Table A1 in the online version of the journal for a list of Act 60 and receiving districts. Lists of consolidated districts can also be found on the Arkansas Department of Education website: https://web.archive.org/web/20100924075952/http://arkansased.org/about/excel/consolidaton_annexation_070110.xls https://dese.ade.arkansas.gov/Files/20201126123548_asr_2004-05_ed.xls

8. Ending our study sample in 2007–2008 drops a small number of observations for students who appear in the data in subsequent years for various unknown reasons (e.g., grade repeaters). However, including these observations does not affect our results.

9. For a more general treatment of regression discontinuity models, see Imbens and Lemieux (2008).

10. The estimated coefficients have been omitted from our reported results for the sake of space and are available upon request. Estimated coefficients for demographic variables and forcing variables are available in Supplementary Tables A3a and A3b in the online version of the journal.

11. The Arkansas administrative data systems have changed ethnicity categories over time. In this article, the Other category includes students not categorized as Black, Hispanic, or White. In Arkansas, this generally includes students who identify as two or more ethnicities, Asian, Native American/Alaskan Native, and Native Hawaiian/Pacific Islander.

12. Supplementary Tables A5 and A6 in the online version of the journal provide separate descriptive financial analyses for districts that merged with other districts and those that were annexed by a larger district. The results are similar to the overall results in Table 7.

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