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The Impacts of School District Consolidation on Rural Communities: Evidence from Arkansas Reform

Sarah Ausmus Smith
University of Kentucky

Ron Zimmer
University of Kentucky

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Sarah Ausmus Smith
University of Kentucky

Ron Zimmer
University of Kentucky

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Abstract: Over the past fifty years, school districts have consolidated in an effort to achieve economies of scale. While the determinants and effects of district mergers on operations have been studied (Gordon and Knight 2006; Duncombe and Yinger 2007; Jones et al 2008), the impact on communities has not. In small towns, schools not only educate, but also provide stable employment and are a cornerstone for community engagement and local identity. In this article, we examine whether district mergers have adverse effects on the community at large. We evaluate the effects of rural school district consolidations on town population size, number of schools, and property values using a propensity score matched difference-in-differences design, leveraging a 2003 Arkansas state law requiring reorganization using an enrollment cutoff. We estimate that the reform led to reductions in population, community schools, and property value assessments.

Introduction

Governance in the United States is increasingly decentralized as a result of New Public Management reforms to increase competition and local responsiveness. However, public school districts are an exception— in the past fifty years, the number of public school districts has declined drastically in the United States, from over 40,000 to under 14,000 (de Brey et al. 2019). Faced with demographic pressures, many state governments have used incentives and sanctions to induce rural school districts in particular to consolidate, effectively centralizing administration, in an effort to increase economic efficiency and performance (Gordon and Knight 2006, Gordon and Knight 2008). Yet, very little research focuses on the unique problems faced by rural policymakers with regards to public education and its reform, even as a quarter of America's schoolchildren are attending schools in towns or rural places today (de Brey et al. 2019). In this paper, we estimate the impact of school district consolidation on rural communities using a novel policy change in Arkansas. Rural schools are not only educating students; they are a source of economic activity, local identity, culture, and civic engagement

(Schafft 2016). We argue that when districts are forced to consolidate, it signals the removal of residential amenities leading to loss of population and housing values.

Using a propensity score matched difference-in-differences research design which capitalized on a state policy to induce consolidation, we find that district reorganization led to a population reduction equivalent to 13 to 15 percent pre-reform levels. Furthermore, communities of color were disproportionately impacted—for every ten percentage point increase in racial minority population share, there is an associated 38-person reduction in population. The inducement to consolidate also led to a 0.19 to 0.25 reduction in community schools and $1,300 reduction in assessed property values. Taken together, these results indicate that residents of rural communities value local governance of public education, and when faced with the potential loss of local responsiveness, "vote with their feet."

The Ongoing Decentralization Debate

Scholarly and political debate regarding local government fragmentation, and possible reforms aimed at reducing it, often revolve around the "optimal" number of local governments. Proponents of fragmentation often invoke the Tiebout hypothesis—interjurisdictional competition between many local governments for residents promotes economic efficiency and provides for a plurality of preferences in local goods and services (Howell-Moroney 2008). More jurisdictions in a decentralized system means more choice. Advocates for consolidation point out that self-sorting can have negative consequences, such as creating and perpetuating racial segregation and income inequality; consolidation could increase economies of scale in public

administration, decrease racial segregation, and enhance economic development (Leland and Thurmaier 2005, Jimenez and Hendrick 2010).

The debate surrounding school district consolidation echoes these broader themes. State financial incentives, economies of scale, distance, and the degree of match between districts regarding educational spending preferences are all important factors to rural districts contemplating consolidation (Gordon and Knight 2006). Consolidation could reduce the burden of fixed administrative costs and allow for more specialized classes and facilities, but conversely, there are also concerns larger districts could be less responsive to their local communities (Leach et al. 2010).

However, district reorganization has also been viewed as a policy tool to professionalize and "urbanize" rural schools, with the goal of reducing the out-migration of high-achieving young adults from their rural farm communities (Cubberley 1922, Theobald 2021). Moreover, these century-old anxieties were racialized—"If the schools offered students an urban education, they could stay home instead of moving to cities where the racial stock of the country threatened to be diluted" (Theobald 2021). The public administration literature contextualizes this by placing it squarely in the ongoing debate regarding the interplay between politics and administration (Demir and Nyhan 2008). In local government, 20th century institutional reforms such as the introduction of city manager roles, at-large representation, and nonpartisan elections were intended to improve the rationality and efficiency of government—moving away from politics based on "community cleavages—class, ethnic, racial, or religious" (Morgan and Pelissero 1980). Scholars have reasoned that the reforms aimed at enhancing expertise and professionalism in administration may come at the expense of political

responsiveness, particularly for minority groups within a community. For example, such administrative reforms are associated with lower levels of political participation via elections (Hajnal and Lewis 2003), providing support for the notion that reforms aimed at de-politicizing and professionalizing government may also undermine democratic processes. School districts, like municipalities, are local governments leveraging institutional reforms to increase their expertise, the theory of action being increased administrative capability would lead to service quality rivaling that of urban centers.

While the phenomenon is rampant, there have been relatively few studies of school district consolidations in the education policy and public administration literature; the existing scholarship has focused on student and district financial outcomes. For example, Leach, Payne et al. (2010) found consolidation led to modest improvements in test scores, but that the effects differed by student socioeconomic status and may have been driven by simultaneous funding changes. A recent working paper leverages the same Arkansas state policy cutoff that we use here to examine student achievement using a regression discontinuity design, concluding there is little evidence of impacts on ELA and math test scores (McGee et al. 2021). Duncome and Yinger (2007) found that for a given level of performance in New York state, rural school district consolidation led to lower operating costs by achieving economies of scale. In their estimations using 12 consolidations, they predict savings on the order of more than 20 percent for the consolidation of two 300-student districts, with declining savings as districts get larger. Rural district consolidations in Iowa were not associated with decreases in quality measures— pupil-teacher ratio, mean school size, dropout rate (Gordon and Knight 2008).

In examining alternative outcomes, scholars found that district and school size do impact student outcomes. In Texas, Jones, Toma, and Zimmer (2008) found that larger districts are associated with lower average daily attendance, as individual schools have less direct financial incentive to improve student attendance than in a smaller district. Increasing school size leads to reductions in student achievement as there is less competitive pressure with fewer schools to compete for students, more bureaucracy, and potentially less parental engagement as larger schools can be intimidating for parents (Eberts et al. 1990, Borland and Howsen 1992, Brasington 1999, Hoxby 2000, Driscoll et al. 2003, Foreman-Peck and Foreman-Peck 2006, Kuziemko 2006). When school districts consolidate, school closures are often expected by constituents, as district leadership may choose to close schools for both academic or financial reasons (De la Torre and Gwynne 2009). Closures can have short term negative impact on scholastic achievement (Engberg et al. 2012, Brummet 2014, Beuchert et al. 2018), but the effects of school closures also extend beyond the immediate class cohort—post-closure cohorts can benefit high performing students (Bifulco and Schwegman 2020).

The limited scholarship that does consider the effects of school district reorganizations on local communities has often relied on qualitative or cross-sectional quantitative methods. Sell and Leistritz (1997) use a mixed-methods approach including descriptive statistics, interviews, and a survey of parents in eight consolidated North Dakotan districts to assess population loss, civic participation, quality of life, and economic activity. The survey respondents report a decline in civic organization participation and quality of life after consolidation for communities “losing” their school, relative to respondents in “host” communities. Brasington (2004) studies the relationship between 1991 home sales in Ohio with

district consolidation; they find that consolidation is associated with a decrease in housing value. However, neither of these papers address the endogeneity of reorganization—integrating two or more districts is often the result of declining populations. They also rely on cross-sectional or self-reported assessments at a single point in time. In sum, this literature provides insights into the theoretical debate surrounding school consolidation as well evidence on the effects of costs and performance, but it does not fully consider the effects school consolidation has on communities over time accounting for selection bias. In this paper, we address this void in the literature.

Theoretical Framework

There is a dearth of scholarship measuring the impacts these reforms, directly or indirectly (district size, school size, and school closure), have on their local communities outside of the classroom. We argue that district reorganization changes the calculus for rural populations' residential choices, despite the theory of action held by some reformers in pursuit of increasing desirability through professionalization. Local schools are a highly visible cornerstone of small towns; they are key actors in community development not only as employers and educators, but also as civic, cultural, and economic institutions, around which communities can organize and progress towards shared community development goals (Schafft 2016). Schools provide a conduit for the creation of social capital, which may be important to economic mobility and public administration performance (Coffé and Geys 2005, Andrews 2012, Chetty et al. 2018). Fischel (2009) argues that the spillovers from the "network of social

capital that is fostered by public schools" benefits adults without school-age children within a community.

Consolidation could signal community disinvestment in a publicly provided service, as it could lead to the local school closing, which is important to local life (Schafft 2016). When districts reorganize to achieve economies of scale, the perceived risk of school closure increases. From an economic perspective, reorganization signals a reduction in the local amenities by both removing an important institution and increasing travel time to get to a new school. The public school system is also a source of stable employment—relocating the district office, for example, could incent employees to move to be closer to work. Often these communities are already experiencing population decline, and the merging of school districts could accelerate the decline by removing or relocating preferred amenities.

Through the public administration lens, centralizing districts through consolidation weakens the link between the community served and its government in an effort to professionalize management; administrators serving a larger base have less incentive to be responsive to minority group interests, for instance. In contrast, centralizing bureaucratic functions could mean a reduction in local oversight (Whitford 2002). Decentralization can allow for greater responsiveness to local preferences for public services according to Oates (1993); it is also associated with perceptions of increased performance, as well (Moynihan and Pandey 2005). We argue that rural communities could value their local districts for these reasons as well as the local amenities they create. Consolidation attenuates these perceived benefits.

Previously literature has established a strong link between school quality and housing values. In their review, Nguyen-Hoang and Yinger (2011) conclude that the willingness to pay

for a one standard deviation increase in test scores hovers around four percent. These estimates are consistent in international contexts (Machin 2011). More recent work has attempted to isolate the channels through which school quality is capitalized—using data from England in a regression discontinuity design, Gibbons et al. (2013) estimate that schools' "value-added" achievement can increase housing prices at a magnitude similar to that of overall indicators of school quality. Conversely, home values in Los Angeles did not respond in a similar manner to a public accountability campaign disseminating information on school and teacher "value-added" data (Imberman and Lovenheim 2016). However, these studies narrowly focus on the effects school quality have on housing values in urban areas and do not consider the effects schools, as the heart of many these rural communities, has on housing values. To our knowledge, this paper is the first to assess the housing market's response to district reorganization reforms in a rural context, acknowledging rurality may shape communal preferences in ways that are distinct from urban areas.

We study the impact of school district reorganizations on rural communities by using a statewide reform in Arkansas mandating small districts reorganize on the basis of an enrollment threshold. This cutoff allows us to reduce bias in our estimates by constructing a more plausible control group. We present results from several models, using the same difference-in-differences framework. First, we estimate impacts of reorganization on town population, finding that consolidation led to a 62 to 70-person reduction in town population overall. We also show some evidence that these effects disproportionately impact communities with a higher proportion of people of color. Because the public discourse often couples consolidation with closure, we assess the relationship between consolidation and school counts, finding as

expected that towns affected by consolidation had 0.19 to 0.25 fewer schools post-treatment. Third, we use data obtained from Zillow (2021) to estimate the effects of consolidation on assessed property values. Our results indicate district reorganization reduces property values by over a thousand dollars which suggests that the reduced level of local amenities associated with a nearby school has been capitalized into the values of homes. While policymakers may pursue government reforms to increase cost effectiveness, enhance client outcomes, or increase administrative professionalism, our results show that these reforms can have broader implications for communities at large and may work at cross purposes to their intents.

Policy Background: Education Reform in Arkansas

Studying the effects of school district consolidation on rural communities is challenging due to the inherent endogeneity. Communities with declining populations or economic activity may choose to consolidate for those exact reasons, for example. To address this problem, we leverage a plausibly exogenous policy change in Arkansas to reduce this source of bias. In 2002, the Arkansas state Supreme Court ruled public education funding unconstitutional in a suit brought by a school district arguing the existing system did not guarantee equitable access. The Supreme Court called upon the legislature to address deficiencies (Loyd 2019). In response, the governor called a 2003 special session, during which the legislature passed the Public Education Reorganization Act (Holley 2015). Widespread administrative consolidation was the governor's original intent, but the resulting legislation took a narrower approach (McGee, Mills et al. 2021). Under Act 60, school districts must reorganize only if enrollment falls below an average annual enrollment of 350 students; the affected districts may voluntarily submit a plan for

reorganization for the state board of education's approval, or the state board may impose one per regulation (2020).

To comply, districts could consolidate or be annexed. In a consolidation, two or more school districts dissolve to re-form a new one, and in an annexation, an existing district (often larger) expands its boundaries to incorporate the smaller dissolving district (Johnson 2006). In practice, a consolidation results in a new school board, but in an annexation the annexing district often maintains control of the school board (Johnson 2006). There was an immediate impact of this policy— almost 90 percent of the resulting reorganizations occurred within two years.

While many states may offer financial incentives to encourage consolidation, the Arkansas reform exogenously imposes an enrollment cutoff that can be used to provide a more plausible counterfactual. In addition, Arkansas provides an excellent test case for our research question because much of the state is rural—there are only three major metropolitan statistical areas with estimated populations above 100,000—Little Rock, Northwest Arkansas (home to the University of Arkansas and several major corporations including Walmart), and Jonesboro. Overall, nearly 85 percent of Arkansas school districts are in rural areas.

Data Collection

Our analyses focus on the impacts of school district consolidation on their communities, apart from impacts on student achievement or district finances. We have three dependent variables of interest: total population counts, number of community schools, and total assessed property value. For the models focusing on community population and schools, we obtained

town population counts and an array of covariates from the decennial censuses.[1] To measure the potential impact of district reorganization on property value assessments, we obtained historical property assessment data from the Zillow Transaction and Assessment Database (ZTRAX). Our assessment data cover years from 2000 to 2015. We use the property address to identify parcels that fall within the same towns identified for the other analyses.

For all analyses, we started by identifying school districts that were subject to Act 60 per publicly available documents from the Arkansas Department of Education. At this stage, we had a list of 74 treated districts to match to Census-designated places. We limited our list to districts that were treated before 2010. For a control group, we selected districts that had an average enrollment between 350 and 700 students for the years 1999 to 2003 and were not party to an annexation or consolidation in the future.[2] We used this cutoff to identify districts that would decrease biases in our estimation on unobservable factors relating to district size. We used the location addresses for individual schools and district offices from the Common Core Data file from 2000 to identify corresponding towns, and GIS boundary files from the Census to identify other towns that fell completely within the boundaries of our treatment and control districts. We excluded towns that had both a treatment and control school, and/or schools from an outside district; some district boundaries split towns—there were ten towns that fell into this category that were removed from our dataset. After these cleaning processes, we had a pool of 77 treatment and 71 control towns; 20 treatment and 22 control towns did not have a school in

[1] At the time of this writing, 2020 estimates breaking down age, poverty, and educational attainment were not available. So, we carried forward data from the 2015-2019 American Community Survey for covariates as needed.
[2] Which is to say, we excluded districts as control that were incorporating the treatment districts due to this policy change.

the year 2000 but fell within the school district borders. To further increase the comparability of the control group, we also employ a propensity match procedure on pre-trends.

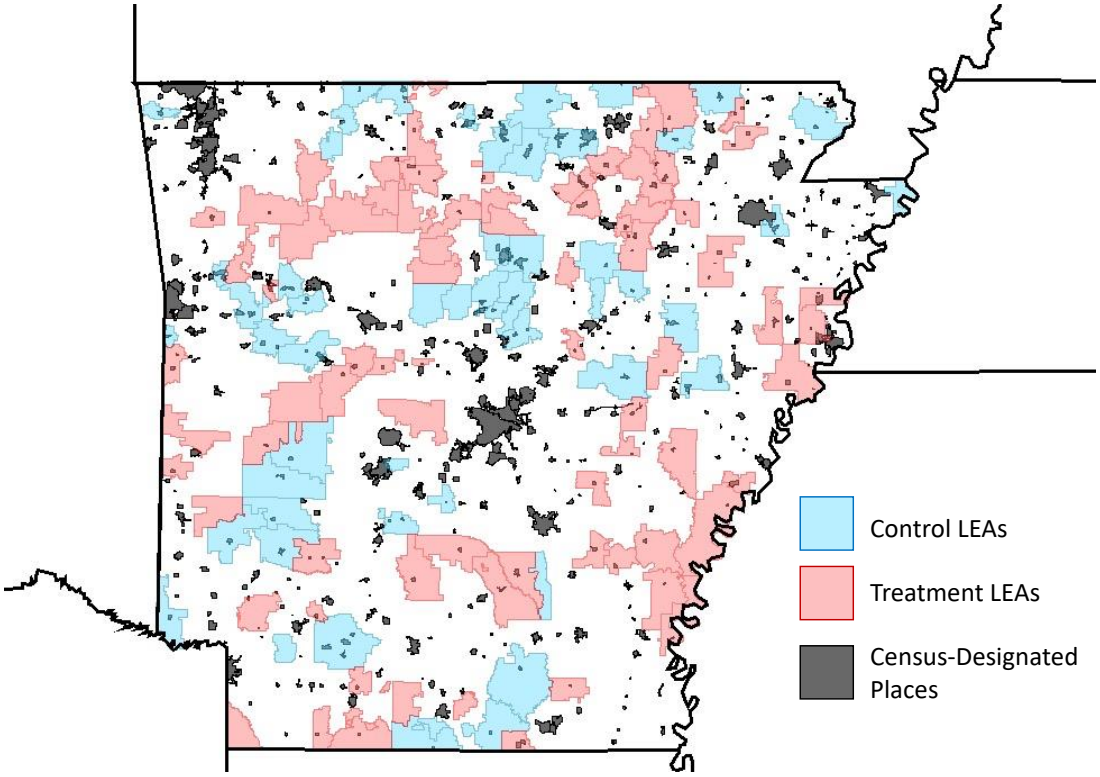

Figure 1: Map of Municipalities, Treatment School Districts, and Pool of Potential Control Districts

Figure 1 shows the treatment districts (red), potential control districts (blue), and incorporated places in Arkansas (grey). This map illustrates the distribution of treatment and the pool of potential control districts from which the matching procedure draws—they are both dispersed throughout the state, and their boundaries do not overlap. The biggest urban commercial areas are Little Rock in central Arkansas, and the cluster of towns in northwest Arkansas, which is home to the flagship public university and several corporate headquarters. Most of the areas studied do not fall in the metro area for either.

To create a strong control group with similar demographics and similar pre-trends for our population an school count analysis, we used propensity score matching to identify the two nearest neighbors (with replacement) in the potential pool of control towns for each treatment community. We matched on pre-treatment town population and nonwhite population percent (years 1990 and 2000) to construct two control groups—the first of the two control groups is reflected in column II in Table 1 and a secondary control group that focuses on creating a strong control group by race is reflected in the third column. In all analyses, we use the frequency weights produced through the nearest neighbor matching procedure.

Table 1: Treatment & Control Places Weighted Descriptive Statistics, 2000

	(I) Treatment	(II) Control (NN=2) Pop. Matched	(III) Control (NN=2) Pop. Matched Race Matched
Total Population	477.29	462.58	441.75
	(33 – 1,867)	(74 – 2278)	(74 – 1850)
College Attainment	8.07%	8.23%	8.68%
	(0 – 34.62)	(0 – 23.73)	(0 – 23.73)
Labor Force Participation	76.39%	78.20%	79.51%
	(50.94 – 87.90)	(52.25 – 90.97)	(56.76 – 90.97)
Poverty Rate	22.84%	18.04%	20.41%
	(0.81 – 63.64)	(4.28 – 39.30)	(4.28 – 39.30)
Under 18 Share	26.13%	24.73%	26.52%
	(9.54 – 33.33)	(11.88 – 38.64)	(11.88 – 38.64)
Over 65 Share	16.99%	18.01%	16.54%
	(9.54 – 33.33)	(5.68 – 45.70)	(5.68 – 32.67)
Racial Minority Population	23.81%	8.01%	20.21%
Share	(0 – 99.39)	(0 – 58.11)	(0 – 58.11)
Number of Schools	1.42	1.29	1.37
	(0 – 4)	(0 – 3)	(0 – 2)
	N = 77	N = 58 unweighted	N = 50 unweighted
		N = 77 weighted	N = 77 weighted

Mean (Min – Max). Source: NHGIS Census Data. Analytical weights applied.

In Table 1, we compare demographics for the towns in the final analyses, prior to treatment. The process of identifying comparable untreated towns produced a control group

that looks very similar to the treated towns in the pre-period with respect to total population, education levels, labor force participation, poverty, age, and schools (columns I and II). The biggest observable difference in these data is the racial composition. The treated towns have roughly three times more people of color than the control towns. Because of this difference, we deployed a propensity match to create an additional control group; the descriptive statistics for this alternate control group are in column III. Adding this additional match variable had the intended effect of increasing pre-treatment comparability with respect to racial composition but reduces the sample a control communities a bit. Therefore, we consider this a secondary control group to serve as a sensitivity analysis.

We used a similar approach to the analysis of property values—we propensity score matched the nearest neighbor on assessed value per square foot and total town population prior to reform. Because there were inconsistent gaps in the parcel panels, we matched on the first observed value prior to 2004. We matched on the population count for the year 2000, the assessed value per square foot, and the assessment year. Because we started with half a million observations, we opted to use only the single nearest neighbor with replacement. Using the pre-trend years as a baseline limits our analysis to those parcels that show up in both. While the strategy will significantly decrease our sample size, we believe this process increases the quality of our estimates.

Table 2: Treatment & Control Property Value Assessments, Pre-Treatment

	Treatment	Control (NN=1)
Total Assessed Value	$9,212.07	$11,949.52
	(100 – 192,950)	(60 – 89,940)
Lot Size (Sq Ft)	744,072.20	522,040.90
	(2,178 – 27,900,000)	(2,178 – 35,000,000)
Residential – Multi Unit	0.04%	0.33%
	(0 – 1)	(0 – 1)

Residential – Apartments	0.01%	0.00%
	(0 – 1)	(0 – 1)
Residential – Single Family	90.70%	89.83%
	(0 – 1)	(0 – 1)
Residential – Mobile Home(s)	7.61%	9.85%
	(0 – 1)	(0 – 1)
	N = 6,731	N = 4,812 unweighted N = 6,754 weighted

Mean (Min – Max). Source: Zillow (2021). Authors' analysis.

The properties in the matched control group on average have higher assessed values and smaller lot sizes. However, roughly 9 out of 10 parcels across groups are zoned single-family residential. The remainder almost exclusively consist of mobile homes. The historical property value data was limited in its detail. We introduce additional controls for local housing stock, which we discuss in greater detail in the next section.

Research Design

We use population counts, number of schools, and property value assessments as dependent variables in a propensity score matched difference-in-differences (DID) event analysis design. While the enrollment cutoff specified in the legislation forcing treatment would lend itself to a regression discontinuity design, that was not feasible due to the small number of observations near the cutoff; comparing towns at the cut-point would have significantly decreased power. For example, only five of our control districts had average enrollments under 400 students from the pre-adoption years, which covered nine towns. Therefore, we conceptually take advantage of the intuition of a regression discontinuity to construct the control group, as outlined in the data section, and follow the same logic for all analyses. This

policy change is exogenous; while communities have some say in what their reorganization will look like, they still must reorganize with another district per state law.

An identifying assumption of the DID framework is the need for pre-treatment parallel trends. We presented some descriptive statistics comparing treated and control communities in the data section to attest to comparability. In the figure below, we show historic trends in population by treatment status to highlight general trends without controlling for covariates. The control group in this figure are the weighted nearest neighbors. While the levels are slightly different, they follow the same trend over time—a dip in the 1990 census, with an increase in the year 2000.

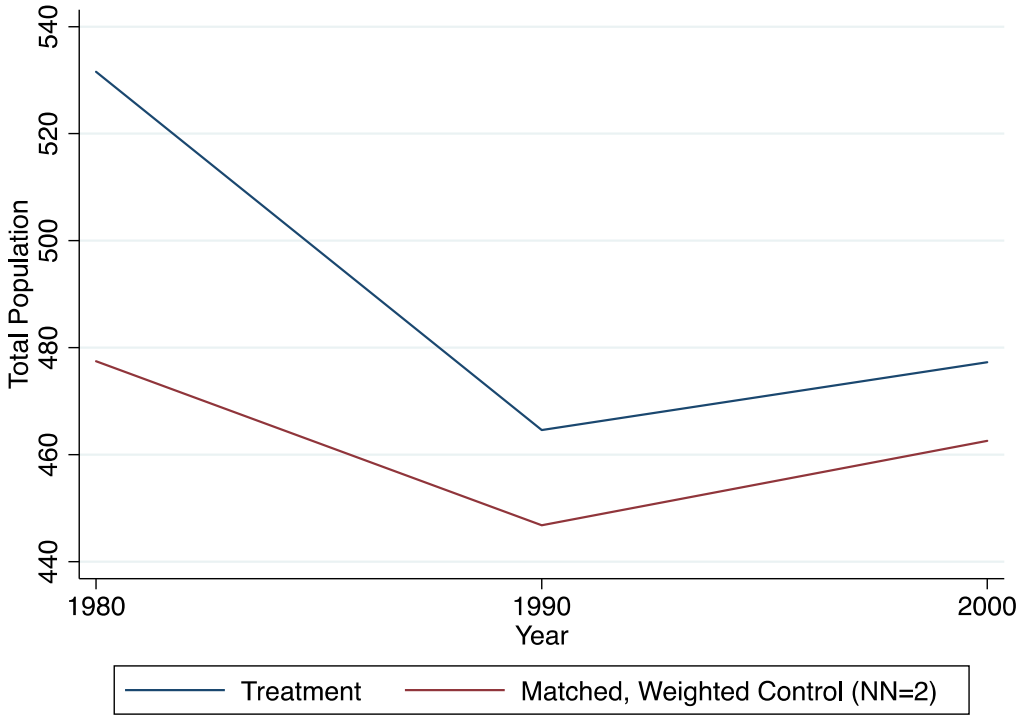

Figure 2: Pre-Treatment Population Means

We have two choices in designating treatment timing in our model—using the year of the universal policy lever, the passage of the 2003 special session reform, or allowing for

staggered treatment timing by using the consolidation date instead. We chose to use a universal treatment lever, which could be considered an intent-to-treat analysis (ITT). We chose this designation for two reasons. First, our conceptual argument rests, in part, on perceived risks. For small rural communities, the passage of this bill was likely highly visible. An increased sense of risk regarding reorganization and losing their schools is an important signal to residents considering leaving. Therefore, the ITT analysis avoids an anticipation effect. Second, the overwhelming majority of our test cases were reorganized shortly after policy adoption— of our treatment towns, 60 were treated (i.e. consolidated or annexed) before the 2005 school year. We applied this same framework to all dependent variables in a panel regression:

$$(1a) \quad Pop_{pt} = TreatPost_{pt} + Treat_p + Post_t + Demo_{pt} + Econ_{pt} + Year_t + Place_p + e$$

$$(1b) \quad Pop_{pt} = TreatPost_{pt} * POC_{pt} + TreatPost_{pt} + Treat_p + Post_t + Demo_{pt} + Econ_{pt} + Year_t + Place_p + e$$

$$(2) \quad SchoolCount_{pt} = TreatPost_{pt} + Treat_p + Post_t + Demo_{pt} + Econ_{pt} + Year_t + Place_p + e$$

$$(3) \quad Value_{lt} = TreatPost_{lt} + Treat_l + Post_t + Features_{lt} + Community_{lt} + Year_t + Parcel_l + e$$

In equations 1a, 1b, and 2, *p* is place and *t* denotes time. Equation 1b interacts treatment ($Treat_p$) with racial minority population share (POC_{pt}) to examine the treatment effect by race. For all the models, we include a vector of demographic ($Demo_{pt}$) and economic controls ($Econ_{pt}$), alongside year ($Year_t$) and Census-designated place fixed effects ($Place_p$), to estimate the effect of this reform on population counts (Pop_{pt}) and school counts ($SchoolCount_{pt}$). For the third equation, the unit of observation is property parcel. Parcel value is a function of similar controls but includes property features ($Features_{lt}$) and parcel fixed effects ($Parcel_l$) in lieu of town fixed effects. To compensate for the lack of property-level data on building features, we include variables from the Census on town housing stock, including total

number of housing units, median number of rooms, occupation density, and owner-occupied ratios ($Community_{lt}$).

Results

We estimated the effects of this government reform on community population first using the weights produced from the matching procedure in panel regressions. We present the results below for two nearest neighbors in Table 3. In the base model (column I), the treated communities had an average population loss of 62 people post-reform. This is equal to roughly 13 percent of the population of the average treatment town in the 2000 Census. In our sensitivity analysis using pre-treatment population counts and racial composition (column II), the magnitude of treatment is slightly larger—nearly 70 people.

Table 3: The Effects of School District Reorganization on Community Population

	(I) Pop. Matched	(II) Pop. Matched Race Matched
Treatment*Post	**-61.87****	**-69.56***
	(21.23)	(27.51)
Treatment Group	0	0
	(.)	(.)
Post Period	-62.4	-85.82
	(31.63)	(44.85)
	Community Characteristics	
Under 18 Percent	**610.9****	**561.4****
	(186.5)	(171.7)
Over 65 Percent	35.9	-139.5
	(100.9)	(130.4)
Racial Minority Percent	-95.41	112.7
	(161.8)	(195.9)
Educational Attainment	155	**340.2***

	(79.56)	(130.9)
Labor Force Participation	-39.04	-121.3
	(57.47)	(86.06)
Poverty Rate	22.56	66.77
	(44.18)	(59.19)
School Count	11.37	29.47
	(20.29)	(17.83)
Year Fixed Effects	Y	Y
Place Fixed Effects	Y	Y
Constant	**329.4*****	**327*****
	(85.23)	(86.21)
Observations	672	632
R^2	0.285	0.300

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We used the same covariates in event analyses, omitting the year 2000 as a baseline (Figures 3 and 4). The pre-treatment coefficients are not statistically different from zero in either specification, providing support for the identifying assumptions of our model. The event studies also reveal that the magnitude of population loss is steady over time in the base model, but that the effects may be short lived in the second model matching on both pre-treatment nonwhite percent and total population.

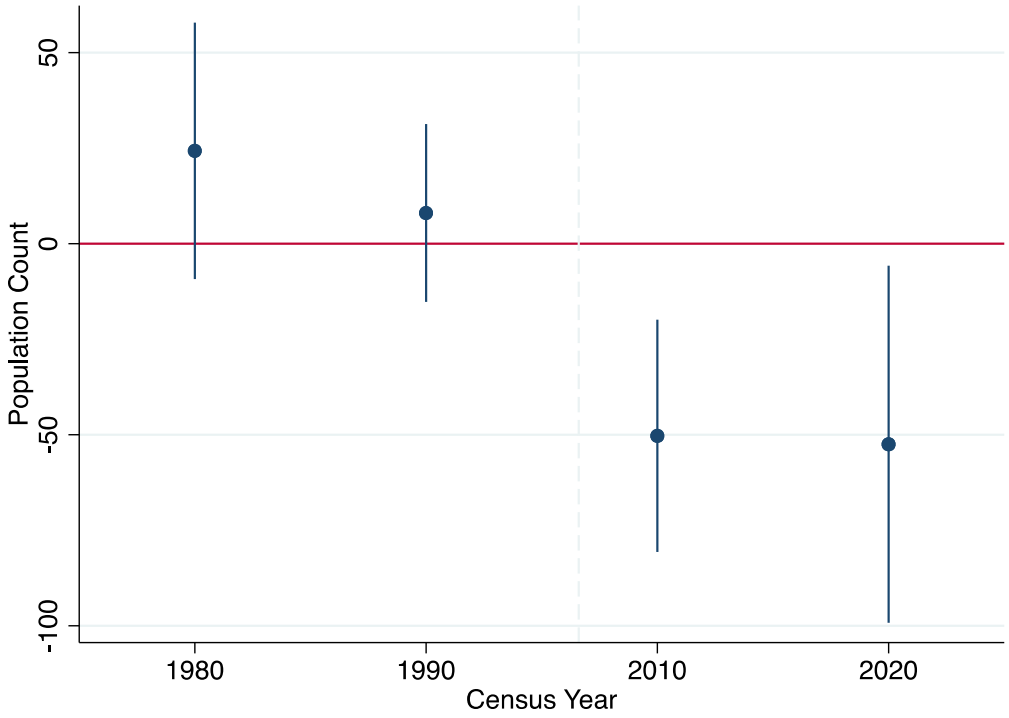

Figure 3: Population Event Study Using Matching (NN=2, Population Count)

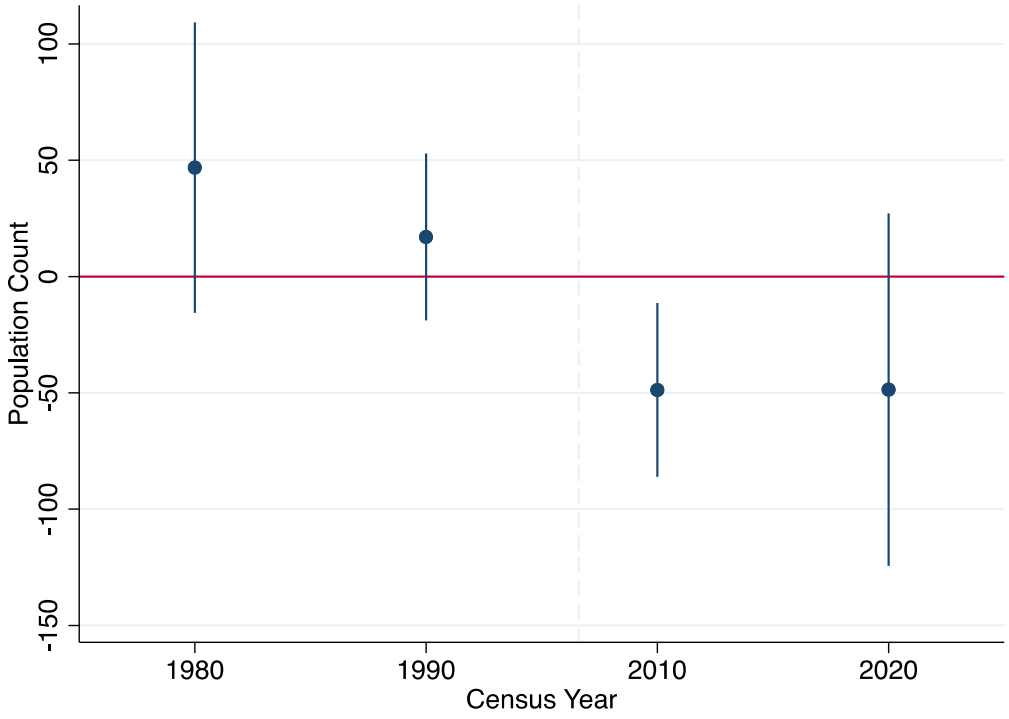

Figure 4: Population Event Study Using Matching (NN=2, Population Count and Nonwhite Share)

To understand if there were racially-disparate impacts of this reform, we interact the nonwhite population percentage with treatment status (Table 4). We use the same matching as above—only total population in the pre-period for two nearest neighbors in the first column, and adding nonwhite population share as an additional criteria in the second column. In the first regression, a ten percentage point increase in the racial minority population share is associated with a 38-person reduction in population for treated towns. The post-reform treatment term is no longer significant in this specification. This implies that communities with larger racial minority populations had a disparate response to consolidation. This result is mirrored in the second regression (column II).

Table 4: The Racially-Disparate Effects of School District Reorganization on Community Population

	(I) Pop. Matched	(II) Pop. Matched Race Matched
Treatment*Post	32.69	31.9
	(18.85)	(24.11)
Treatment*Post*POC	**-378.9*****	**-381.3*****
	(76.83)	(79.32)
Treatment Group	0	0
	(.)	(.)
Post Period	**-75.59****	**-102.3***
	(28.52)	(43.13)
	Community Characteristics	
Under 18 Percent	**374***	**332.5***
	(146.9)	(129.4)
Over 65 Percent	-71.25	**-240.5***
	(87.93)	(117.6)
Racial Minority Percent	36.17	224.1
	(130.5)	(181.5)
Educational Attainment	126.8	**306.3***

	(69.79)	(128.4)
Labor Force Participation	-26.73	-98.13
	(53.39)	(78.17)
Poverty Rate	.8143	39.99
	(39.91)	(57.53)
School Count	5.235	24.38
	(16.36)	(15.15)
Year Fixed Effects	Y	Y
Place Fixed Effects	Y	Y
Constant	**407.8*****	**394.1*****
	(76.34)	(75.11)
Observations	672	632
R^2	0.427	0.423

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

One of the perceived risks of district consolidation is loss of community schools. In Table 5, we show that towns in consolidated districts did see a statistically significant reduction in their number of schools, but that this effect was not magnified by the racial composition of the community using either control group. So, while towns in consolidated districts had roughly 0.19 to 0.25 fewer schools post-reform, that impact was not disproportionally distributed based on race.

Table 5: The Effects of School District Reorganization on Community Schools

	Pop. Matched		Pop. Matched Race Matched	
	(I)	(II)	(III)	(IV)
Treatment*Post	**-.2478*****	**-.2175***	**-.1916***	-.1583
	(.07114)	(.08888)	(.08055)	(.09646)
Treatment*Post*POC		-.1197		-.1238
		(.2038)		(.2023)
Treatment Group	0	0	0	0
	(.)	(.)	(.)	(.)
Post Period	**-.1682***	**-.172***	**-.3037****	**-.3085****
	(.06682)	(.06621)	(.1151)	(.1145)
Community Characteristics				

Under 18 Percent	-.03371	-.1085	-.3871	-.4608
	(.3144)	(.3114)	(.4618)	(.4554)
Over 65 Percent	-.0938	-.1275	-.2148	-.2473
	(.3411)	(.3385)	(.3503)	(.3443)
Racial Minority Percent	**-.6939***	**-.651***	-.7233	-.6859
	(.3288)	(.306)	(.5811)	(.5795)
Educational Attainment	.3508	.3412	.5149	.503
	(.2541)	(.2526)	(.3042)	(.3047)
Labor Force Participation	-.2849	-.2804	-.3426	-.3345
	(.1985)	(.1948)	(.2574)	(.2553)
Poverty Rate	-.1116	-.1183	.1473	.1383
	(.1485)	(.1486)	(.1805)	(.1811)
Year Fixed Effects	Y	Y	Y	Y
Place Fixed Effects	Y	Y	Y	Y
Constant	**1.776*****	**1.797*****	**2.032*****	**2.051*****
	(.2391)	(.2412)	(.3723)	(.3724)
Observations	672	672	632	632
R^2	0.176	0.178	0.197	0.198

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figures 4 and 5 report coefficients for event studies on the number of schools in treatment and control communities. The trends in both regressions tell a consistent story. The coefficients for the two periods prior to the reform are not statistically significant from zero in both cases, again providing support for the parallel trends assumption. The effect of consolidation on the number of schools within a community is also not particularly responsive to time; the reduction hovers around 0.2 for both 2010 and 2020.

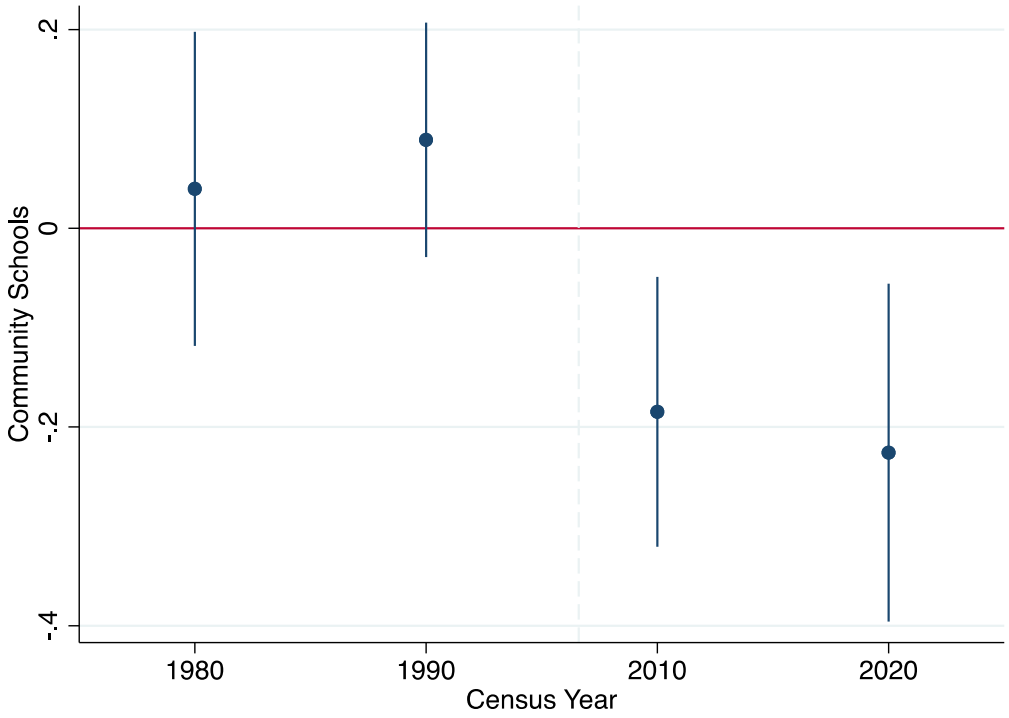

Figure 5: Number of Schools Event Study (NN=2, Population Count)

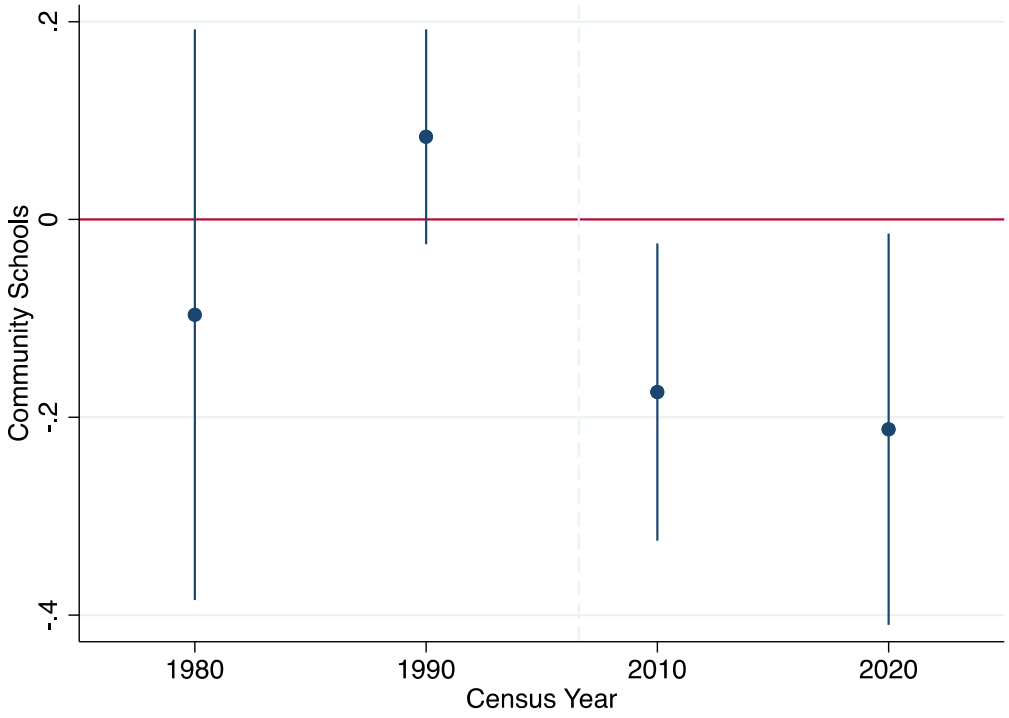

Figure 6: Number of Schools Event Study (NN=2, Population Count and Nonwhite Share)

Next, we turn to property values. The table below shows the average treatment effects of the reform on property assessments using the matching procedure described in the data section. The full complement of covariates is included in the regression in column I, while column II reports results for an additional interaction with racial composition. District reorganization led to a decline in assessed value of roughly 1,300 dollars. While larger nonwhite population shares were associated with lower assessed values, there is no evidence that the interaction between community racial composition and consolidation augmented that effect.

Table 6: The Effects of School District Reorganization on Assessed Value

	(I) Base Model	(II) Racial Minority Interaction
Treatment*Post	**-1314*****	**-1384*****
	(144.3)	(190.7)
Treatment*Post*POC		337.1
		(405.2)
Treatment Group	-260.5	-207.5
	(594.4)	(604.3)
Post Period	**4155*****	**4066*****
	(480.5)	(526.4)
	Community Characteristics	
Percent Racial Minority	**-6468****	**-6897****
	(2405)	(2659)
School Count	**152.7***	**169***
	(74.81)	(77.4)
Poverty Rate	**7373*****	**7430*****
	(1569)	(1569)
Educational Attainment	**3163***	**3191***
	(1552)	(1551)
Under 18 Percent	**7.08****	**7.223****
	(2.327)	(2.341)
Over 65 Percent	**10.4***	**10.64***
	(5.145)	(5.197)
Total Housing Units	**-9.13***	**-9.253***
	(3.695)	(3.739)
Median Number of Rooms	**1643*****	**1636*****
	(386.4)	(386.7)
<1 Occupant Per Room Share	1372	1178

	(780.1)	(914.9)
Owner-Occupied Ratio	**-1977*****	**-2012*****
	(493.9)	(507.5)
	Property Characteristics	
Lot Size (Sq Ft)	**.001732*****	**.001732*****
	(.0002548)	(.0002547)
Multi-Family Residence	**8207*****	**8208*****
	(711.1)	(711.1)
Apartments	1518	1514
	(3244)	(3244)
Single-Family Residence	**6514*****	**6512*****
	(486.1)	(486)
Mobile Home(s)	**5629*****	**5630*****
	(462.3)	(462.3)
Year Fixed Effects	Y	Y
Parcel Effects	Y	Y
Constant	**-6260****	**-5929***
	(2360)	(2565)
Observations	46,599	46,599
R^2	0.179	0.179

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To examine the effects of consolidation on property values more closely, we also deploy an event study using the full array of covariates (Figure 7). The year 2003 is used as a baseline, and is omitted. Relative to 2003, the pre-treatment years are somewhat noisy and there is not a clear bias in the property values on the basis of treatment group status. However, after the implementation of the reform, there is a clear downward trend in property values after 2005, ceteris paribus. After a few years the magnitude decreases.

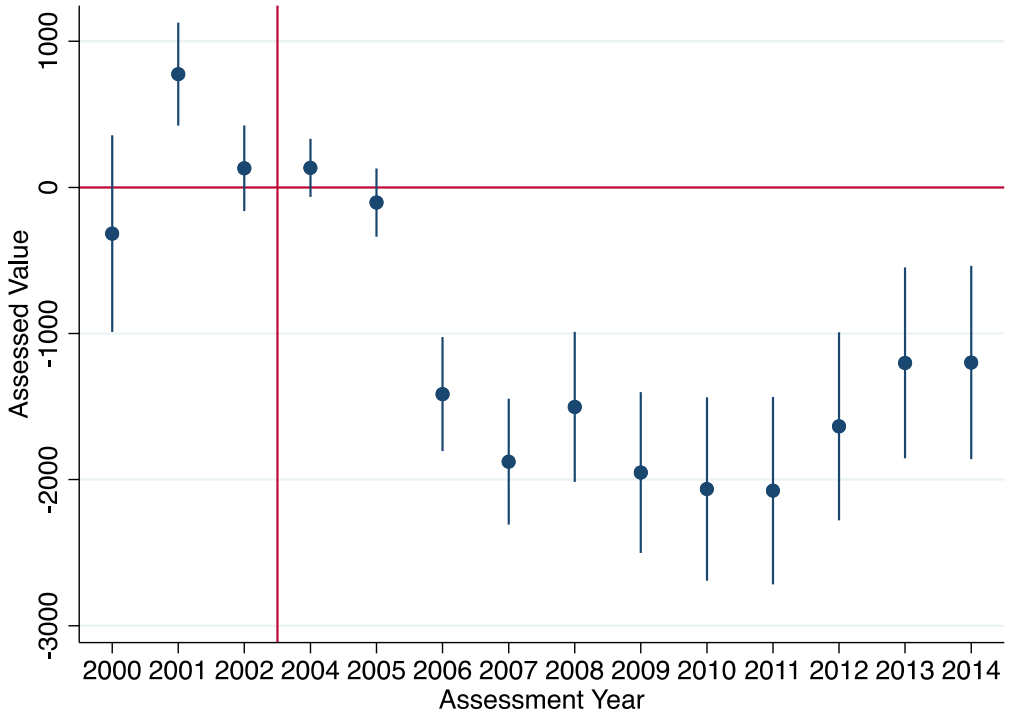

Figure 7: Property Assessments Event Study (NN=1)

Discussion & Conclusion

In this paper, we examine the impact of rural school district reform on the communities they serve. We find that consolidation decreases town population, community schools, and property values. We also find some evidence that communities with larger racial minority populations may be disproportionately affected by this reform. Taken together, these results indicate that local communities value local institutions in rural settings. Our study does have limitations due to data constraints; for example, we could not include un-incorporated places for study, so generalizability is reduced. Our analysis of property value assessments also lacks fine-grained parcel information and does not have as many pre-treatment years as we would prefer. We combat this problem through including town-level housing data and deploying a

nearest neighbor matching strategy. Despite this approach, our pre-trend is limited in length; further work is needed to confirm our findings in other contexts.

Despite these limitations, we contribute to scholarship in three ways: we explore new ways efforts to increase capacity in local governments may impact constituents; we increase our understanding of the impacts of a common education reform on community outcomes; and we focus our study on the rural context. Ultimately, our empirical work highlights potential tradeoffs in government centralization and professionalization. Local districts contribute to the character of their communities and provide a public service beyond the immediate clients. Future work could further examine how such professionalization reforms impact democratic processes, accessibility, and equity.

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Appendix

Table A1: Data Sources for All Models

Variable	*Description*	*Source*
Total population	Census data	(Manson et al. 2021)
Treatment	Dummy variable	Arkansas Department of Education
Bachelor's degree rate*	Census data	(Manson, Schroeder et al. 2021)
Labor force participation*	Census data	(Manson, Schroeder et al. 2021)
Poverty rate*	Census data	(Manson, Schroeder et al. 2021)
Population <75% FPL*	Census data	(Manson, Schroeder et al. 2021)
Under 18 share*	Census data	(Manson, Schroeder et al. 2021)
Over 65 share*	Census data	(Manson, Schroeder et al. 2021)
Non-white share	Census data	(Manson, Schroeder et al. 2021)
School count	Scaled per capita	National Center for Education Statistics
School closure	Indicator	National Center for Education Statistics
Lot size (sq ft)	Calculated	(Zillow 2021)
Multi-family residence	Indicator	(Zillow 2021)
Apartments	Indicator	(Zillow 2021)
Single-family residence	Indicator	(Zillow 2021)
Mobile home(s)	Indicator	(Zillow 2021)
Total housing units	Census data	(Manson, Schroeder et al. 2021)
Median number of rooms	Census data	(Manson, Schroeder et al. 2021)
<1 Occupant Per Room Share	Census data	(Manson, Schroeder et al. 2021)
Owner-Occupied Ratio	Census data	(Manson, Schroeder et al. 2021)

*Values for 2020 carried forward from latest available ACS estimates.