



A National Measure of School District Partisanship for the 2016 to 2024 Presidential Elections

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A National Measure of School District Partisanship for the 2016 to 2024 Presidential Elections

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Abstract

Scholars increasingly interested in the governance of local school districts, which invites curiosity about the partisanship of their voting constituencies. Presently, researchers have to translate precinct-level or county-level votes to school district geographies using imprecise imputation strategies as the geographies are not coterminous. We review the recent methods researchers use to allocate votes to school districts for partisan proxy measurements and then propose a nationwide measure using ancillary land cover data. We provide a novel, nationwide database of vote counts and partisanship for the roughly 13,000 school districts for the 2016, 2020, and 2024 presidential elections via dasymetric interpolation, which allocates precinct votes to districts in proportion to developed land rather than geometric area. We illustrate common trends of interest to data users: within-state variation, partisanship change over time, and disaggregation on district locale. We then use our measure to replicate Hartney and Finger's (2022) study exploring the relationship between district partisanship and pandemic school reopening, which assigns each district the vote share of its parent county. We compare our measure of votes against the county-level Ohio measure they assembled, finding closely similar distributions of school district partisanship. We improve both the sample size and interpretability of their study, finding that their headline Ohio result also holds nationally while illustrating that a national measure allows for broader generalizability and more precise estimates.

Key Words: school district partisanship, dasymetric interpolation, education politics, education policy, voting

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Data Availability: The data and analysis for this article can be found on the Harvard Dataverse at Almes and Arnzen (2026) via <https://doi.org/10.7910/DVN/XTWTXH>

1 Introduction

American public education is heavily decentralized. Roughly thirteen thousand school districts, most governed by elected boards, set budgets, hire superintendents, adopt curricula, and draw attendance boundaries. In recent years, accelerated by the COVID-19 pandemic, school districts have become sites of national, often partisan, political conflict. Recently, an increased national spotlight has illuminated school board meetings that have erupted in heated conflict over pandemic closures, curricular content, library holdings, and the treatment of transgender students. These recent battles have become intertwined with national partisan conflict over how schools should function in society.

While school districts have long been sites of political conflict (e.g., school prayer, teaching evolution, segregation), this conflict has largely existed outside of the partisan fray in part because of nonpartisan ballots, off-cycle elections, and appointed rather than elected superintendents (Henig, 2013; Tyack, 1974). Considerable work has, however, documented how partisanship nevertheless seeps into local education politics (e.g., Hartney and Finger 2022; Grossmann et al. 2021; Shah et al. 2024; Arnzen and Jacobsen 2025). This reflects a broader nationalization of American politics (Henig et al., 2019; Hopkins, 2018), in which local contests increasingly take on the tone and division of national partisan politics. If school district governance is no longer insulated from partisan politics, but is instead being reshaped by it, then a district's partisan composition is a variable that education politics research increasingly needs to consider.

The behaviors and attitudes of voters are central to understanding both school district governance and education politics. In most American elections votes are counted in precincts and typically reported as statewide or county totals, but precinct boundaries are drawn by county clerks and state election officials, often with no reference to school district lines. While school board elections themselves may yield some insight into the attitudes of a school district's voters, most school board elections are formally nonpartisan and occur off-cycle (Hill et al., 2026) preventing the

aggregation of any meaningful, national measure of district partisanship. Thus, attempts to explore how partisanship and polarization have influenced education politics have required some method of moving votes across boundaries that were never meant to align (e.g., Kitchens 2021; Shah et al. 2024).

A methodological literature has evolved around this boundary misalignment problem (see Amos et al. (2017)), and precise translation of precincts-to-other geographies often requires ancillary data, exemplified by Fekrazad (2025) who allocated precinct votes to census block groups, tracts, and ZIP code tabulation areas using regionalized land cover regression, validated against more than eight million census blocks and against geocoded voter records in North Carolina. Though Fekrazad (2025) articulated a methodological road map for translating precinct-to-administrative geographies, there is still no nationwide federal election in which school district partisanship is directly observed, and the district-level measures are commonly estimates derived by moving votes across a boundary mismatch.

2 Previous Measures of School District Partisanship

Existing work on the politics of education uses one of three geographic units to translate votes to school districts, the predominant being assigning county-level voting composition to school districts. Recent studies include Shah et al. (2024) who identified the county containing each Wisconsin district and assigned it that county's Republican vote share, while Grossmann et al. (2021) similarly linked county-level Democratic vote share to school closings during COVID in Michigan. County-level returns are universally available and require no spatial processing, but this means that every district in a county receives an identical value, flattening within-county variation. In states with substantial county-school district fragmentation (the breaking of a county into a high volume of districts, see Bischoff 2008) we expect substantial measurement error as each district loses its heterogenous contribution. For example, Shah et al. 2024 compress roughly 420

Wisconsin districts into 72 county values, assigning each district its county's average Republican two-party share across the 2016 and 2020 presidential elections. In our dasymetric estimates, 47 Wisconsin school districts are assigned the wrong majority partisanship in 2016, 48 in 2020, and 44 in 2024.¹ Attributing county partisanship to Wisconsin districts moves the average school district 6 points more Republican in 2016 and 6.2 points more in 2020 than using precinct-to-district dasymetric measures. In Figure 1, Panel A, we illustrate misattributing districts across party lines using Milwaukee County, which under the Shah et al. 2024 method applies a static 30.5 percent Republican to each of 17 districts² (unified and elementary), while our method increases the between-district variation, illustrating the presence of Republican-majority districts in the southern region of the county.

A second approach used by scholars is areal interpolation (or spatial join), which allocates precinct characteristics to school districts and apportions votes by overlapping area (see Kitchens 2021; Jacob 2024). While offering more granularity than assigning county-level values, this technique carries other spatial biases, particularly in the potential misattribution of voters to land without residents, or therefore, land without voters (Flowerdew and Green, 1992). Because the resulting error scales with the share of a district's area that is undeveloped, areal weighting is least reliable in places where districts are large and sparsely settled, and it can distort comparisons between, for example, a compact district and the rural district beside it. For studies examining rural voters and rural education policy, areal methods hamper neighboring-district contrasts, which may be important, as voters in rural districts have been shown to have different preferences for education policies (Arnzen and Houston, 2026). Broadly, studies using areal weights may be sensitive to the specific decision of how to characterize voters living near boundary lines and whether there is systematic bias in those attributions.

Figure 1, Panel B illustrates the error associated with areal methods in some parts of the nation.

¹Reported later in Table 4

²Some districts fall between Milwaukee and neighboring counties. This is the count of 17 districts (unified and elementary) whose interior point falls in Milwaukee County.

A single precinct on the boundary between the Farmington and Aztec Municipal School Districts in New Mexico holds 586 two-party votes. Areal weighting assigns 76 percent of them to Farmington because most of the precinct's land lies on that side of the line. Weighting by developed land instead assigns 9 percent to Farmington and 91 percent to Aztec. Panel C shows why: Farmington's portion of the precinct is dominated by arid desert, while nearly all of the precinct's settled land sits in the thin river valley along its southeastern boundary, on Aztec's side. To be clear, in parts of the nation where developed land correlates highly with population, areal interpolation will be as precise as dasymetric measures, but we suspect that rural areas like those in northwest New Mexico are most susceptible to error without dasymetric weighting.

A third approach to measuring school district partisanship uses surveys and public opinion methods to align individual voters to districts. Arnzen and Jacobsen 2025, for example, conduct exit polls of Michigan and Rhode Island voters while Anglum and Manion 2026 survey Missouri residents to capture partisan identity. While exit polling data may generate measures for individual districts, it is hard to scale up. And while public opinion surveys are more generalizable to the broader public, they are hard to design to be representative of school districts specifically. These methods have high human labor costs and rely on public cooperation.

Shepardson et al. (2026) implement a "Query, Overlay, Recover" (QOR) pipeline which geocodes voter registration addresses through the U.S. Census Geocoder and overlays the resulting coordinates on district boundaries. The authors demonstrate that this method is 95 percent accurate in North Carolina and Washington. Scaling the approach nationally, however, poses two challenges: 1) access to voter files and 2) biases introduced by geocoding. Voter files can be acquired state by state, meaning widely-varying costs, formatting, permitted use, and who may request them. Several states restrict use to election-related purposes or bar redistribution entirely. Alternatively, purchasing voter files from commercial vendors can come with high costs. Regardless, assembling a national file is a procurement problem as it must be repeated as registration records age. Because voter files are restricted and frequently licensed, analyses built upon them are difficult for other

researchers to reproduce. Once voting records are obtained, manually assigning non-matches remains costly in terms of human labor and can introduce additional error from cognitive load.

The Census Geocoder is an application which allows for addresses or coordinates to be submitted to find a location or Census geography (Census, 2026). When addresses are submitted, the tool converts the address to an approximate coordinate position. The Census Geocoder does not observe where a residence sits, but rather it converts an address to “an approximate coordinate” by interpolating along address ranges in the Master Address File (Census, 2026). The problem is not confined to addresses the geocoder rejects. Berg et al. 2026 show that successfully geocoded addresses are placed in the wrong census block at a substantial rate, and that the misplacement is systematic rather than random, a problem they term overlay uncertainty. School district boundaries are coarser than blocks, so most such displacements are absorbed within the correct district, but the exception is where a district boundary follows along street boundaries. An address interpolated onto the wrong side of a street could systematically allocate votes to the wrong boundary. The resulting error is concentrated at boundaries rather than distributed across a district, which is to say it is concentrated exactly where the classification is decided. For analysis of districts using streets as boundaries (see Fischel 2009).

3 Constructing a Dasymetric Measure

Below we detail the construction of a dasymetric measure of voter partisanship in school district shapes, which can scale across elections and all school districts within states. This measure, available to all scholars interested in education politics, local politics, or education policy, leverages the strengths of the county/areal approaches via publicly available data while increasing the precision of estimates which is the goal of costly survey and address allocation approaches.

3.1 Data

We utilize precinct returns from the Voting and Election Science Team (VEST) for the 2016 and 2020 national elections, and we use the 2024 *New York Times* (NYT) precinct reports (Amos et al., 2024; New York Times, 2024). These sources report precincts as shapefiles in addition to vote counts. This combination of VEST and NYT sources yields the highest precinct-school district coverage for those elections. Table 1 reports the number of precincts by state and year. For each precinct, we retain Republican and Democratic presidential votes and, where reported, a combined third-party count.

School district boundaries come from the Census Bureau’s TIGER/Line school district files, collected for the National Center for Education Statistics through an annual survey of state education officials and distributed through NCES’s Education Demographic and Geographic Estimates (EDGE) program (Geverdt and Maselli, 2024). Boundaries reflect TIGER updates through May of the school year, and we match each election to the school year that contains it, so that SY2016–17 boundaries pair with the 2016 election. TIGER distinguishes three kinds of school district: unified districts that serve all grades, and elementary and secondary districts that divide the grades between them. We retain only districts that operate schools, dropping the fictitious entries TIGER carries to fill its geographic hierarchy where no district operates, which removes 110 of 13,368 records in 2024, 86 of which are secondary districts. Elementary and secondary districts are not mutually exclusive geographies. In 14 states an area can lie inside both a K–8 district and a 9–12 district serving the same residents: Arizona, California, Connecticut, Illinois, Maine, Massachusetts, Montana, New Hampshire, New Jersey, New York, Oregon, Rhode Island, Vermont, and Wisconsin. Under the 2024 boundaries, these states hold 1,726 elementary and 392 secondary districts, which between them form 1,625 overlapping pairs, counting only pairs that share at least a hectare of ground rather than a boundary line. The arrangement is most common in Illinois with 477 pairs, California with 433, and Montana with 289, and each secondary district overlaps 4.2 elementary districts on average. The pattern is stable across the panel, with 14 states in 2016 and 2024 and 13 in 2020,

when Vermont’s elementary and secondary districts do not overlap.

Our third dataset is the USGS National Land Cover Database (NLCD), a series of 30-meter maps of surface characteristics across the conterminous United States, constructed from satellite imagery and classified into thematic categories such as forest, wetland, cropland, and developed land. Developed land is represented by classes 21 through 24, in ascending order of built intensity. NLCD does not extend to Alaska or Hawaii, so precincts in those states fall back to the areal rule described further below (though Hawaii is a single school district).

3.2 Weighting

Our method improves on previous measures by weighting votes from precincts to where voters are more likely to live. We classify all land pixels as a single binary indicator: developed land or not. Recent work argues for greater precision beyond binary weighting through graduation (Fekrazad, 2025), and we do not dispute that such weights might improve accuracy where they can be estimated. The obstacle is that estimating requires regional calibration with the use of ancillary datasets not yet scaled for a national universe. Improvements in the future proposed by geographers include combining one or more approaches of parcel records, address points, building footprints, and thermal maps, but these are not available nationwide on consistent basis.

The cost of the simpler binary rule appears to be modest, as dasymetric refinement studies find that a binary or three-class treatment captures most of the improvement over areal weighting, with diminishing returns thereafter (Mennis, 2003; Eicher and Brewer, 2001; Langford, 2006). Baynes et al. (2022) find that the residential densities in all four developed classes are orders of magnitude above all other classes, so the developed and undeveloped distinction carries most of the available signal.

3.3 Interpolation

Dasymetric interpolation assigns voters from precincts (the source geography) to school districts (the target geography) using ancillary land coverage weights. Let p index precincts and d index school districts in election year t . Let $A(p,d)$ be the geographic intersection of precinct p and district d (the sliver of land they share); let $n(p,d)$ be the number of 30-meter NLCD pixels within that sliver whose centers fall in a developed class, and let $a(p,d)$ be its area. Precinct p reports $R(p)$ Republican votes, $D(p)$ Democratic votes, and $T(p)$ total votes cast for president.

The share of precinct p 's votes assigned to district d is the developed land in their shared sliver as a fraction of all the developed land in that precinct:

$$\lambda_{pd} = \frac{n_{pd}}{\sum_{d'} n_{pd'}} \quad (1)$$

The denominator sums over slivers rather than over the whole precinct, so the weights a precinct distributes sum to one by construction. Interpolation therefore conserves votes exactly: district totals sum to the precinct totals they came from, with no leakage and no duplication. This is the property Tobler (1979) termed pycnophylactic, and it separates the estimator from centroid assignment, which sends each precinct whole to the district containing its center and conserves votes only when every centroid lands inside some district (a condition that fails along coastlines and wherever districts do not tile).

A precinct split between districts with no developed land anywhere in its slivers carries no dasymetric information, and is undefined. For those precincts we weight by area instead:

$$\lambda_{pd} = \frac{a_{pd}}{\sum_{d'} a_{pd'}} \quad (2)$$

Areal weighting is the foundation dasymetric interpolation improves on, but it remains the next-best option when the ancillary layer is unobtainable. Two conditions make universal dasymetric vote allocation impossible. The first condition is that precincts whose developed-land footprint is

empty have no dasymetric weight to apply. Outside Alaska and Hawaii this is rare, affecting 295 precincts in 2016 (0.17 percent), 751 in 2020 (0.44 percent), and 20 in 2024 (0.01 percent), which together hold well under one hundredth of one percent of the two-party vote. The second obstacle is that the Annual NLCD product covers the conterminous United States only, so no developed-land weights exist for Alaska or Hawaii and every precinct in those states is allocated by area. Those two states account for 705 of the 1,000 fallback precincts in 2016 and 662 of 1,413 in 2020, and for nearly all of the vote allocated by area in any year.

Other than areas with no usable precincts, each district’s Republican votes are the weighted sum over every precinct that touches it:

$$R_d = \sum_p \lambda_{pd} R_p \quad (3)$$

and its Democratic votes take the same form. Total votes are accumulated identically. Because elementary and secondary districts overlay one another, the sums in equations 2 through 4 run over a partition of districts rather than across all districts at once. We interpolate twice, once across unified and elementary districts, once across unified and secondary districts, and report each district from the pass in which its own grade span tiles the state.

Our measure accounts for the two-party Republican share:

$$\pi_{dt} = 100 \cdot \frac{R_d}{R_d + D_d} \quad (4)$$

Third-party votes are counted in the district total but excluded from equation 4, so the measure stays comparable across elections.

3.4 Coverage

While our election data covers the overwhelming majority of the country for the three elections, not every county has usable precinct returns in the 2024 election (see New York Times 2024).

Alaska, for example, reports absentee, early and provisional ballots district-wide; Oklahoma reports absentee ballots countywide in some counties; some South Dakota counties' returns are not yet reported; Mississippi is complete except Issaquena. Connecticut, Maine, New Hampshire, Rhode Island and Vermont report by township, a coarser unit that still tiles the state. Other precinct inconsistencies are tracked in Amos and McDonald (2024).

Where part of a district falls outside every reporting precinct, its totals are assembled from the precincts that did report and published as such rather than suppressed. Alaska contributes no usable 2024 returns, so its districts carry no 2024 estimate. Estimates for the remaining eight states should be read with corresponding caution in cross-year comparisons.

4 The Partisan Landscape of American School Districts

4.1 Concentration and the scarcity of competition

Using our measure, we find that most children attend school in a school district with a dominant political leaning, either Democrat or Republican. In 2016, 75.6 percent of districts (10,008) recorded a two-party margin wider than 60–40 with 74.0 percent of districts in 2020 and 74.5 percent in 2024. In all three elections roughly three districts in four sat outside a twenty-point margin, and that percentage moved less than two points across eight years and two changes of national election outcome. Likewise, politically competitive school districts are relatively scarce. Counting a district as competitive when its two-party share falls between 45 and 55 (less than a 10-percentage point gap), there were 1,544 in 2016 (11.7 percent), 1,671 in 2020 (12.7), and 1,696 in 2024 (12.9). Politically competitive districts, however, enroll 20.3 percent of students, and we find these districts are concentrated in populous suburbs.

4.2 Variation Within States

Figure 2 maps district partisanship in 2016 (Panel A) and 2024 (Panel B). The district unit illustrates the heterogeneity county maps do not, particularly in the narrow Democratic regions along the Northeast corridor, the Great Lakes cities and the Pacific coast, plus three persistent non-metropolitan Democratic regions: the Black Belt and Mississippi Delta, the reservation districts of the plains and Southwest, and the lower Rio Grande valley. Comparing the two panels, the clearest movement is in south Texas and south Florida, where heavily Hispanic districts shift toward the Republican candidate.

Figure 3 shows the within-state distribution of district partisanship in 2024, unweighted in Panel A and weighted by enrollment in Panel B, with states held in the same order across both panels so they can be read row by row. Two patterns stand out in Panel A. The states with the highest Democratic medians have the most compressed distributions: Vermont and Massachusetts have narrow interquartile ranges and few districts far from the state median. California and Texas, both often portrayed as hallmark geographies for each political party, each show wide variation when voters are represented by school districts. Panel B describes the distribution a student faces rather than the distribution of districts, and weighting reverses the picture in several states: Maryland's median district leans Republican, while the median Maryland student attends a solidly Democratic school district. Illinois, Virginia, Colorado and Georgia move similarly because a few very large districts in each state are responsible for large portions of total student enrollment.

4.3 County Variation and the Role of Fragmentation

Within-state partisan variation is largely what a county-level measure flattens. Decomposing the variance in district partisanship into between- and within-county components (Table 2), 25.2 percent of the national variance lay within counties in 2016, 22.9 percent in 2020 and 22.6 percent in 2024. We argue this is further evidence not to use county-ascribed partisanship as doing so

negates the consistent within-county variance nationwide.

Among the 1,969 counties that contain more than one district, within-county disagreement is the rule rather than the exception. We see only 18.6 percent of counties with five points of variation or less, and 39.8 percent with ten points or fewer. The median multi-district county spans 12.7 points between its most and least Republican district, and nearly a third span more than twenty. These shares are essentially fixed across the period: the five-point share moves between 18.2 percent and 19.4 percent and the ten-point share between 39.6 percent and 40.7 percent from 2016 to 2024. A county value is therefore not a noisy version of the district value so much as a different quantity. It describes the county's aggregate electorate, which in most places no single district shares.

We find that fragmentation is related to variation. Nationally there are 4.2 districts per county and the median state has 3.1. The two series track closely in Table 2. The states at the top (New Hampshire at 81 percent within-county, Connecticut at 72, Rhode Island at 71) are among the most fragmented, while the county-based systems of the South and mid-Atlantic sit at the bottom, with Florida, Maryland, Nevada and West Virginia at zero by construction. Counties holding two districts differ internally by a median of 5.8 points, counties with three or four by 10.2, counties with five to nine by 17.8, and counties holding ten or more districts by 34.8. The correlation between the number of districts in a county and its internal spread is 0.57, and 0.65 against log district count. Fragmentation also tracks the county's own partisanship: the median two-district county is 76.0 percent Republican, while the median county holding ten or more districts is 52.0 percent.

The consequence is a measurement problem that concentrates where county-assignment would most flatten results. The 273 counties with ten or more districts are 14 percent of multi-district counties but contain 4,866 districts, 42 percent of all districts in multi-district counties. A county proxy is least accurate in precisely the counties that hold the largest share of the nation's districts and enrollment, and because fragmentation is itself correlated with partisanship, its errors are systematic with respect to the variable it is meant to measure rather than random.

Table 3 and Figure 6 further illustrate how county proxies of partisanship misattribute votes.

In 1,698 districts (13.0 percent of the panel, enrolling 6,317,541 students) the county's two-party share and the district's own fall on opposite sides of fifty, so using county returns elicits the wrong partisan majority for districts.

4.4 Locale

Locale organizes the measure more tightly than any other district characteristic we examined, meaning locale more closely relates to partisan variation than previous categorizations. In 2024, the median large-city district recorded 38.6 percent Republican and the median remote-rural district 78.7, a gradient of forty points (Table 4 and Figure 4). The ordering is nearly monotonic, and the single inversion is informative: rural fringe districts, at the edge of urban clusters, are less Republican at 65.9 than either distant or remote town districts at 66.4 and 67.9. Proximity to a metropolitan area predicts partisanship better than the rural label does.

Districts at either end of the urban-rural spectrum are politically lopsided: 61.4 percent of large-city and 90.5 percent of remote-rural districts fall outside 40–60, while competitive districts concentrate in large and midsize suburbs, where 29.9 and 25.4 percent fall within 45–55.

4.5 Change from 2016-2024

Across districts, very little change in partisanship occurs for three presidential elections. Of the 12,930 districts with an estimate in both 2016 and 2024 elections, 61.9 percent moved less than three points in either direction between 2016 and 2024, and 82.7 percent moved less than five points. The median district shifted only half a point toward the Republican candidate. Districts that did move were more likely to move Republican: at the three-point threshold, 24.0 percent became more Republican and 14.1 percent more Democratic; at five points, the movement is 12.3 and 5.0 percent respectively (Figure 5).

By locale designation, changes in partisanship are confined largely to suburban and rural school

districts. Between 2016 and 2024, Figure 7 shows urban districts moving 0.1 points Republican, suburban moving 0.8 points Democratic, towns moving 0.3 points Republican and rural districts moving 1.0 points Republican.

4.6 Limitations of Dasymetric Measurement

Dasymetric interpolation outperforms county and areal measures, but does not perfectly allocate votes to districts. Because votes are counted at the precinct and not the household, we force an assumed distribution of partisanship on the cleaving of votes between districts for each overlapping precinct. This means that if 100 percent of Republicans live to the east of a precinct, 100 percent of Democrats live to the west, we may falsely mix the two groups within their spatial-weighted allocations.

It is possible for developed land to contain no voters, which some dasymetric sources have been able to differentiate. Industrial parks, shopping malls, and office buildings are developed land area that contain no voters, and rasters such as the Environmental Protection Agency’s EnviroAtlas Intelligent Dasymetric Mapping toolbox (Baynes et al., 2022) undoubtedly would reduce error between precinct-district overlaps. At the time of this paper, intelligent dasymetric maps do not exist at scale for the election years in our panel. Future iterations of this work can incorporate more sensitive rasters such as building footprints, thermal maps, etc.

Our binary classification treats every developed pixel as equally populated, so a precinct containing both a commercial corridor and a residential neighborhood assigns weight to the corridor that its residents do not warrant. And within residential land, density varies by an order of magnitude between single-family and multi-family development, so a precinct split between a subdivision and an apartment district will misallocate in the direction of the subdivision. To be clear, multi-residential homes only become misattributed in dense populations fragmented between school district zones, but the reality is that many U.S. cities operate a single school district (Shoked, 2016). We adopt the binary rule deliberately, because the alternative requires density coefficients that are

themselves estimated and that vary regionally, but the cost is a known bias toward low-density land within split precincts.

5 Validation and Replication

We validate our measures using two methods. First, we examine county aggregations of precinct votes and compare that to school districts which fully compose exactly one county, as is the case in a few states. In the 763 districts where the two geographies perfectly coincide, the mean absolute error of the dasymetric estimate is 0.005 percentage points and no districts nationwide are assigned the wrong partisan majority using dasymetric interpolation (Table 5). The error appears randomly assigned, as correlation with the number of precincts is -0.013 and correlation with the number of districts within a state is -0.014. Three-quarters of the county districts return exact estimates. The largest single source of district error by this standard is 1.2 points.

We then validate our measure against the school district partisanship measure used by Hartney and Finger (2022), who argue that “...that mass partisanship and vested interests best explain the degree to which schools reopened.” They find that “Republican (Democratic) districts were far more (less) likely to reopen in person...” We replicate and extend their main analyses instead using our measure of district partisanship. Hartney and Finger (2022) use “the two-party share of the vote won by President Trump in each district’s parent county in 2016.” Comparing their parent county measure with our dasymetric measure, like the county-coterminous results, we find that the distributions of the two partisanship measures are remarkably similar. Our measure, shows counties as slightly more Republican than their estimated, showing a one-point higher Republican vote share in the average Ohio district relative to their Ohio district measure. The median district is 0.8 points more Republican, the 25th percentile district is 1.2 points more Republican, and the 75th percentile is 1.4 points more Republican. The dispersion scores are similar, with their measure reflecting a 14.29 standard deviation versus the dasymetric measure at 14.76, and their interquartile

range spanning 17.5 points versus the dasymetric 17.8 points.

To demonstrate the impact of increasing granularity for school district partisanship measures, we replicate Hartney and Finger's (2022) analysis of district reopening during the COVID-19 pandemic, drawing on their public replication archive (Hartney and Finger, 2022). Their analysis is also the largest application of the county-level proxy we are aware of, covering 9,338 districts in 47 states, which makes it the most demanding available test of whether the flattening documented in Table 3 has consequences. Hartney and Finger find that a one standard deviation increase in county Trump support is associated with an 18-point decrease in the probability of fully remote schooling (they explore the outcome along an ordinal range of 1: fully remote, 2: hybrid, and 3: fully in-person). They estimate a series of ordered probit models clustered by state, with fixed effects for state and for NCES locale, and controls for enrollment, per-pupil spending, median family income, percentage of White students, August COVID case rates, and the per capita supply of Catholic and secular private schools. Illustrated in their Table 1, Hartney and Finger use incremental models to compare districts within progressively smaller areas: in their Model 1 compares districts within a state, Model 2 narrows that to districts within a state and locale type, and Model 3 to districts within a commuting zone. Models 4 and 5 test alternative measures of teacher union strength, on a national subsample and in Washington. Only Model 6 introduces county fixed effects, and it does so for a single state. The authors state, "Ohio's 600+ school districts where we were able to assemble data on Trump vote share at the school district level", because "the advantage of having detailed district-level data on mass partisanship for an entire state is that we can introduce county fixed effects into our analysis" (Hartney and Finger, 2022, 466). A county proxy takes a single value within each county, so county fixed effects absorb it entirely.

We find no systematic reason that a few districts have large differences between methods. Mean absolute differences range only from 1.56 to 1.78 percentage points across the four locale categories and from 1.42 to 1.80 across quartiles of district vote total, and the absolute difference correlates at -0.10 with district size and -0.03 with the district's own partisanship. The signed difference does

vary modestly by locale, with the dasymetric estimate reading about 1.4 points more Republican than theirs in rural districts and 1.4 points less Republican in city districts, though only 17 city districts are available to support that comparison. For practical purposes the discrepancy behaves as idiosyncratic error rather than as bias correlated with the covariates district-level research typically conditions on, which means substituting one measure for the other should not systematically shift coefficients on locale, enrollment, or partisanship itself. This assumes their Ohio district measure carries no systematic error of its own.

We then substitute our national school district measure into their specifications and re-estimate them, completing for the whole country, building on the estimates that Hartney and Finger (2022) make in just Ohio. In re-estimating, we use the same controls, the same fixed effects, and the same clustering—only the partisanship variable differs. Table 6 reports the main effects for partisanship across their various models. For the most part, their parent county and our dasymetric measures agree. In Model 1, with state and locale fixed effects, their estimate is 2.951 and ours is 2.873; in their Model 3, which adds commuting zone fixed effects, theirs is 2.743 and ours is 2.709. Both differences are small against standard errors of roughly 0.2, so we read them as agreement, but what changes is the precision. At commuting zone fixed effects the district measure reduces the standard error from 0.371 to 0.216 and raises the z statistic from 7.39 to 12.54, because the within-county variation a county proxy discards does more work as the geographic controls tighten.

The pattern changes when comparing their Model 6, which only estimates Ohio using their measure of district partisanship. In Ohio, on the same 606 districts and the same specification, substituting our estimate for the district-level measure the authors assembled raises the coefficient from 2.046 to 2.648 while leaving the standard error at 1.216. While their estimate is only significant at the $p < .10$ threshold; our measures' additional precision clears $p < .05$ using the same data. Here the measure moves the estimate rather than its precision. Given that our measure is national, we also extend the same model beyond Ohio. Estimating it nationally with dasymetric partisanship gives a coefficient of 2.580 with a standard error of 0.329 on 8,157 districts in 1,672 counties, significant

at $p \leq .001$ (Table 6). At 80 percent power and conventional thresholds, the Ohio sample could reliably detect a coefficient no smaller than 3.41, larger than the effect the national data recover, leaving it roughly even odds of detecting what it was testing for; the national specification detects effects down to 0.92. Ultimately, what was suggestive in a single state can now be established nationally: districts facing identical county case counts and identical county public health guidance made systematically different reopening decisions according to the partisanship of their own voters.

6 Discussion & Conclusion

The dataset we have created addresses an access problem in education politics research. As Collins and Reckhow (2024) note in their review of education politics research developments, local school district data presents a common issue for education politics research. Studies that want to condition on district partisanship have had to either restrict themselves to county-coterminous states, undertake the costly voter-file geocoding exemplified by Shepardson et al. (2026), or accept the flattening that comes with a county-level proxy. A public, nationwide dasymetric measure of two-party vote share at the district level lets researchers treat district partisanship as a covariate, an outcome, or a blocking variable without resolving the boundary-mismatch problem (differently) each time. Short of a national database of voter registration addresses, our approach shows relatively full national coverage and straightforward replication.

The 13 percent of school districts, enrolling 6.3 million students, where a county proxy yields the wrong partisan majority (Table 3) underscores the value of our measure. Concentrations of misattributed votes in California, New York, New Jersey, and Illinois show the utility of our measure for highly fragmented school district states. Both county-matching and areal interpolation measures systematically misrepresent partisanship, skewing partisan estimates for highly fragmented states and rural districts, respectively. Both problems raise familiar concerns, similar to aggregation bias in ecological inference (King, 2004). The correction is the one that geographers have suggested for

a long time: using finer units when they exist. Our dasymetric approach accomplishes just that.

Because the dasymetric weights depend only on precinct geography and land cover rather than on the presidential contest itself, the same crosswalk extends to any office reported at the precinct level: state legislative and gubernatorial returns, for example, would let researchers test whether the partisanship documented here reflects durable district compositions or a presidency-specific artifact of nationalized politics. Where school board races themselves are held on a common date with precinct-level reporting, the same method could produce a genuinely independent measure of local electoral behavior.

The stability in district partisanship shown here, a median shift of half a point across two partisan changes in the presidential administrations between 2016 and 2024, fits a growing body of work suggesting that local political geography, once formed, is durable against short-term swings. School district lines, drawn independently of other jurisdictions, appear to lock the rural-urban partisan gap into place. The movement that does occur, often happens at the margins. Scholars pointing out that political conflict in schools was concentrated in the suburbs (Rogers et al., 2026; White et al., 2023; Holman et al., 2026) appear to be correct. Our findings also generally corroborate that rural areas are consolidating behind Republicans (Mettler and Brown, 2025; Brown et al., 2021).

In this paper we have demonstrated a technique for applying precinct-level votes to school districts using dasymetric interpolation. We validate our method via county-coterminous and published district-level counts for the 2020 election. We implemented our method to validate and improve upon a study that partisanship as a critical covariate, finding increased effects and smaller error. We demonstrate the common patterns of interest associated with national partisanship measures for school districts. Ultimately, our method does not replace the need for difficult, time-intensive address matching studies, but can be a viable substitute when national and statewide measures are needed.

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Figures

Figure 1: Challenges in Existing Measures of District Partisanship

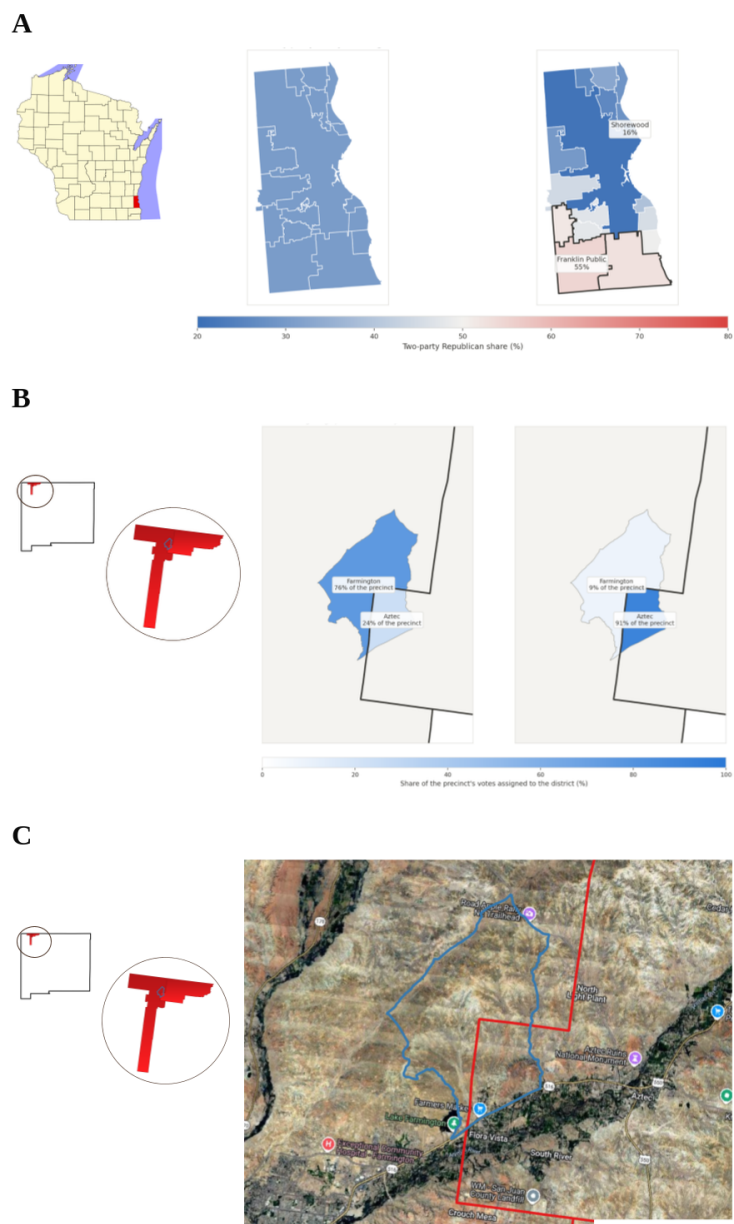


Figure 2: Partisanship of U.S. School Districts, 2016 & 2024

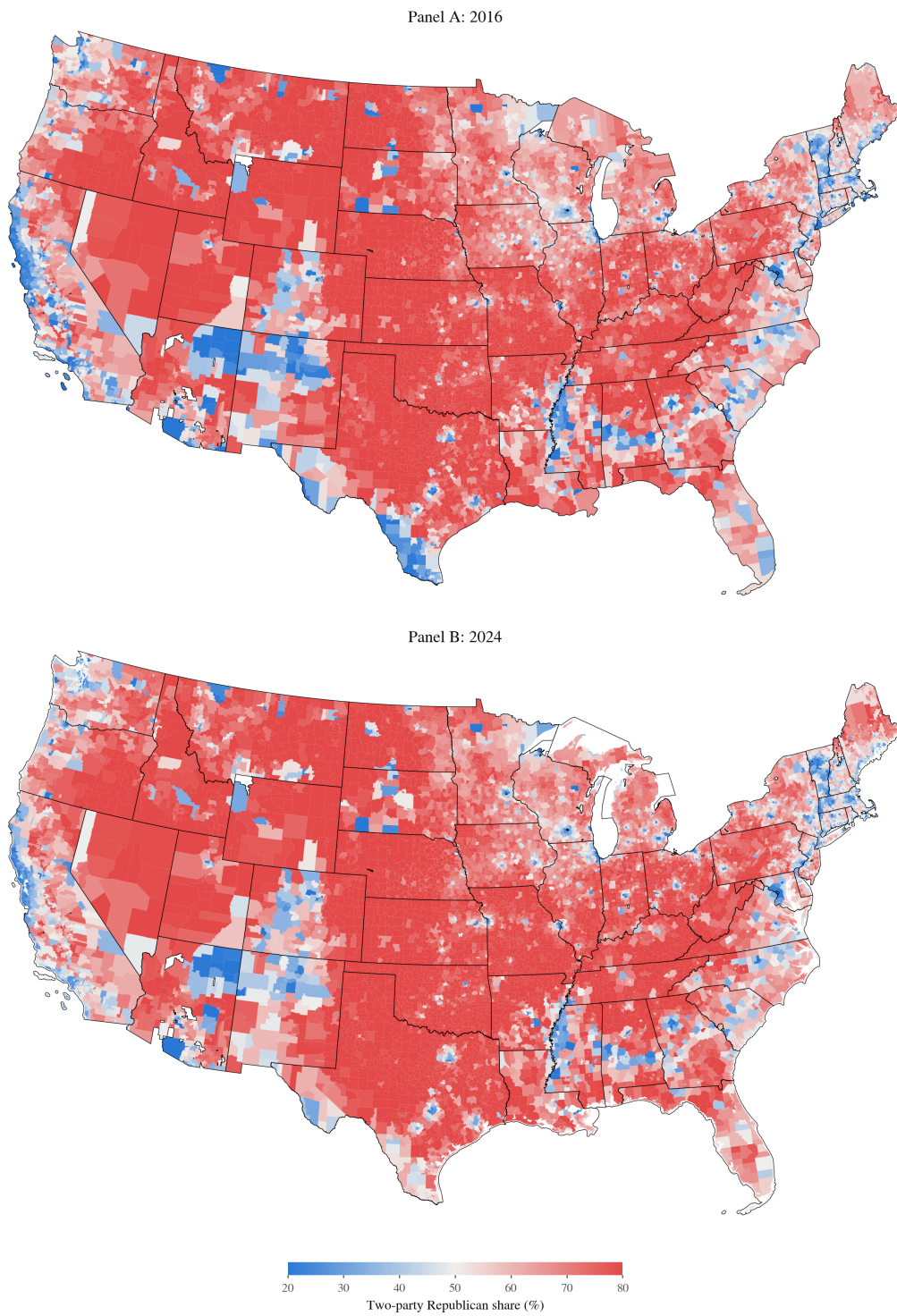
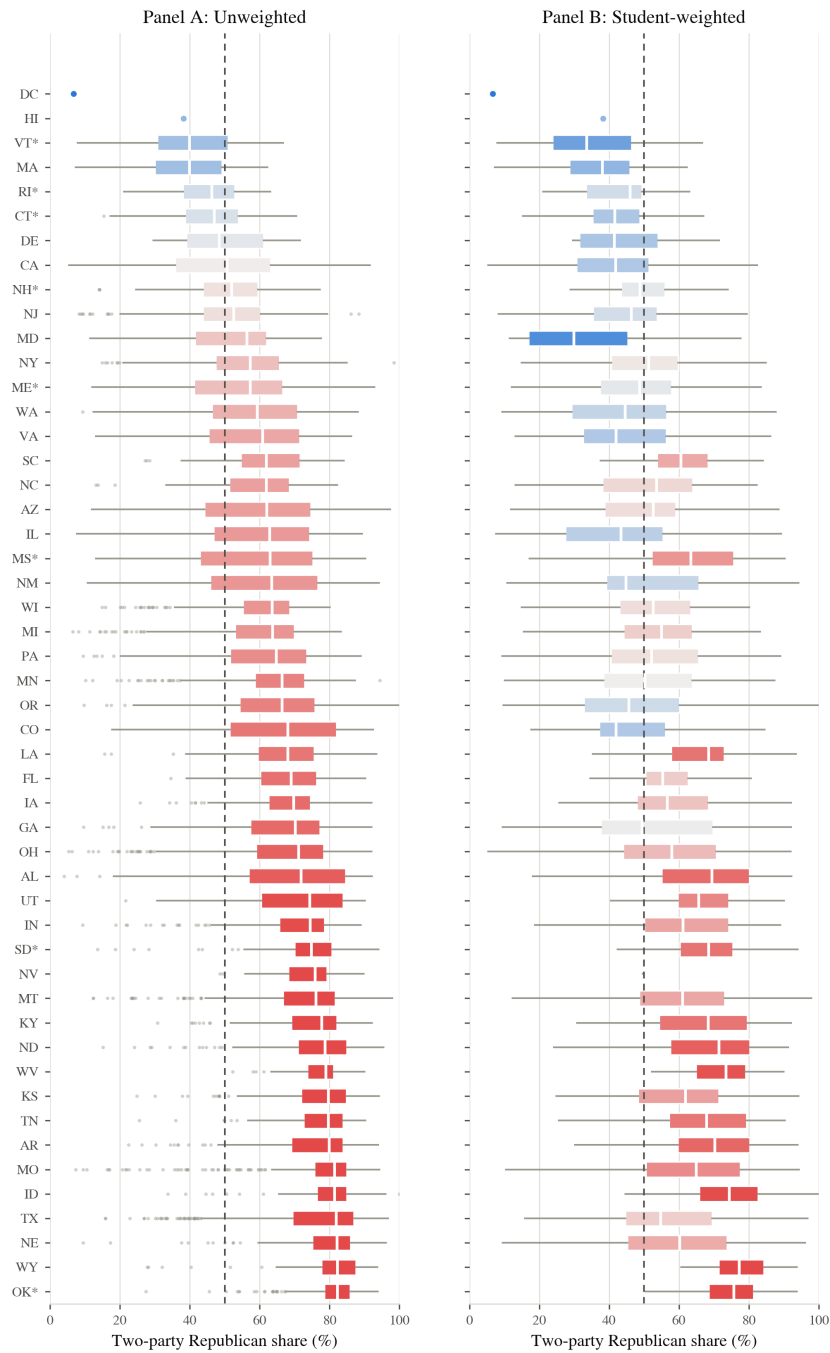


Figure 3: Within-State Variation in School District Partisanship



One box per state in the 2024 election, ordered by the unweighted median and held in that order in both panels so the two can be read row by row. Boxes span the interquartile range; whiskers reach 1.5 times that range. States with fewer than five districts are drawn as points. Where quantiles are weighted by district enrollment, each box describes the distribution a student in that state faces rather than the distribution of its districts; outlying districts are not marked there, since a district is not a student. An asterisk marks states whose 2024 returns carry a collection caveat in the source data and require careful interpretation: VT, RI, CT, NH, ME, MS, SD, OK.

Figure 4: Variation in District Partisanship in 2024, by NCES Locale

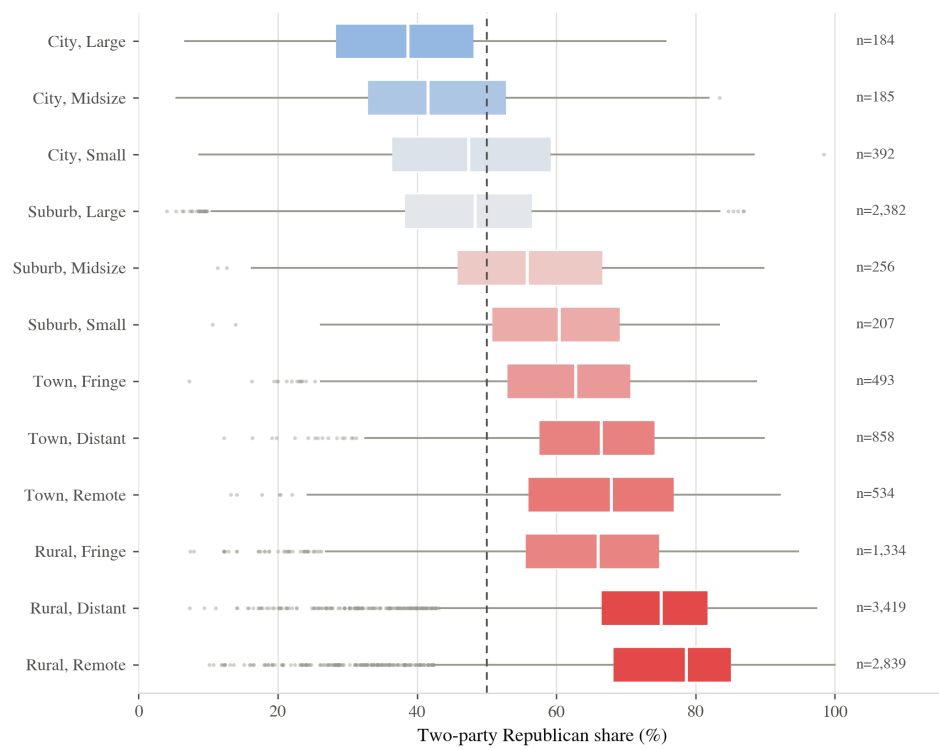
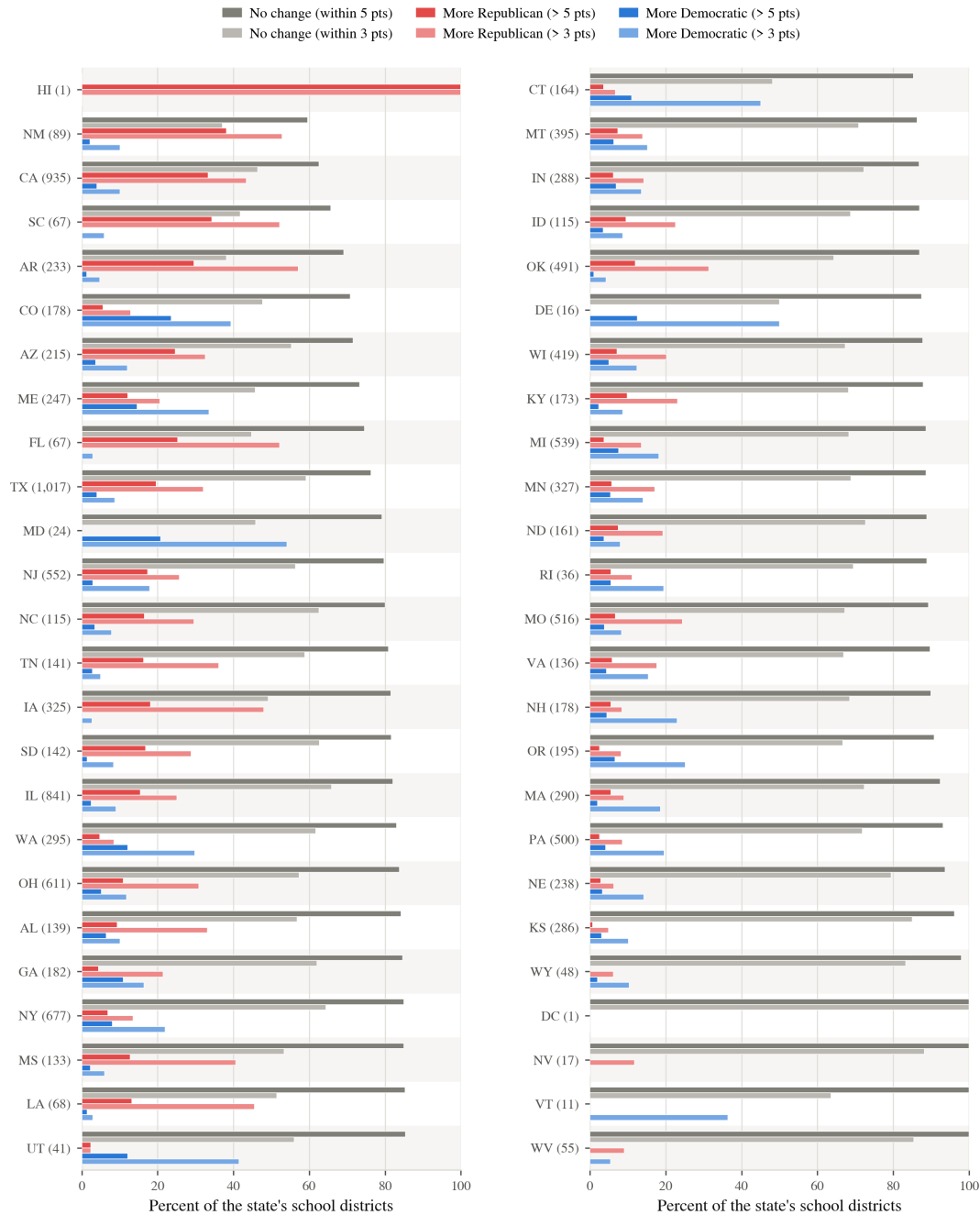


Figure 5: Partisan Change In U.S. School Districts from 2016 to 2024



Each state's districts split by the change in their two-party Republican share, at a 5 point threshold and again at a 3 point threshold. The darker bar in each pair is the 5 point figure and is a subset of the lighter bar beside it, so the pair should be read as a range rather than as two quantities to add. District counts in parentheses. Of the 12,930 districts estimated in both elections, 24.0 percent became more Republican by more than 3 points and 12.3 percent by more than 5; 14.1 percent became more Democratic by more than 3 points and 5.0 percent by more than 5. States are ordered by the share that did not move at the 5 point threshold, continuing down the second column.

Tables

Table 1: Precincts by State and Election Year

State	2016	2020	2024
Alaska	441	441	0
Alabama	2,000	1,972	1,945
Arkansas	2,525	2,591	2,681
Arizona	1,469	1,489	1,694
California	21,758	20,799	18,539
Colorado	3,010	3,215	3,162
Connecticut	743	741	169‡
District of Columbia	143	144	144
Delaware	430	434	526
Florida	6,115	6,160	5,583
Georgia	2,700	2,682	2,680
Hawaii	264	262	235
Iowa	1,680	1,661	1,653
Idaho	926	935	960
Illinois	10,090	10,083	8,527
Indiana	5,378	5,166	5,070
Kansas	4,021	4,062	3,673
Kentucky	3,733	1,225†	3,207
Louisiana	3,704	3,753	3,646
Massachusetts	2,174	2,173	2,374
Maryland	2,039	2,042	1,954
Maine	600	567	493‡
Michigan	4,812	4,756	3,997
Minnesota	4,120	4,110	4,049
Missouri	3,324	3,733	3,061
Mississippi	1,797	1,764	1,730
Montana	689	666	726
North Carolina	2,704	2,662	2,655
North Dakota	432	422	380
Nebraska	1,375	1,386	1,323
New Hampshire	321	321	238‡
New Jersey	6,339	726†	6,384
New Mexico	1,492	1,917	2,165
Nevada	2,067	2,094	1,237
New York	14,876	15,376	12,984
Ohio	8,895	8,941	8,876
Oklahoma	1,956	1,948	1,439
Oregon	1,348	1,331	1,303
Pennsylvania	9,166	9,157	9,144
Rhode Island	422	423	39‡
South Carolina	2,234	2,263	2,308
South Dakota	747	737	626
Tennessee	1,985	1,961	1,858
Texas	8,832	9,016	9,718
Utah	2,296	2,423	2,678
Virginia	2,456	2,477	2,536
Vermont	278	278	237‡
Washington	7,197	7,464	7,747
Wisconsin	6,881	7,093	3,503‡
West Virginia	1,721	1,671	1,638
Wyoming	483	481	431
Total	177,188	170,194	163,925

Notes: Reporting units with presidential returns, after geometry validation. Returns for 2016 and 2020 come from the Voting and Election Science Team; returns for 2024 come from the New York Times precinct collection. A state absent in a year contributed no usable returns; see Table A1 for the districts affected in 2024. A dagger marks a state whose 2020 returns are reported on units coarser than a precinct, so the count is not comparable to the other years in that row: Kentucky, New Jersey. A double dagger marks the same for 2024, where the New York Times collection reports New England by municipality and Wisconsin by combined ward reporting units: Connecticut, Maine, New Hampshire, Rhode Island, Vermont, Wisconsin. These counts are municipal rather than approximate; Connecticut's 169 units are its 169 towns and Rhode Island's 39 are its 39 cities and towns. Because a coarse unit spreads a single partisan average across every district it touches, within-state and within-county spread is understated for these states in that year.

Table 2: Within-County Variance in School District Partisanship, 2016 to 2024

State	Districts	Counties	Per county	2016	2020	2024
New Hampshire	178	10	17.8	81.3	81.4	81.4
Connecticut	164	9	18.2	75.8	72.2	71.9
Rhode Island	36	5	7.2	86.9	78.0	70.9
Arizona	213	15	14.2	74.2	70.3	69.0
Massachusetts	291	14	20.8	65.2	63.5	62.9
New Jersey	553	21	26.3	56.7	57.4†	60.2
Oklahoma	495	77	6.4	46.8	45.1	54.8
Montana	397	56	7.1	55.9	53.9	52.2
New York	680	58	11.7	48.6	47.8	51.1
Michigan	539	83	6.5	51.8	47.8	49.1
Vermont	116	15	7.7	47.1	41.5	48.9
Maine	254	16	15.9	53.3	46.6	48.1
Wyoming	48	23	2.1	48.2	46.7	45.1
Washington	295	39	7.6	44.2	43.8	43.0
Idaho	115	44	2.6	38.9	39.7	40.9
Iowa	325	99	3.3	36.6	37.6	35.8
Pennsylvania	500	67	7.5	39.2	35.0	34.2
Tennessee	140	94	1.5	42.9	35.8	33.9
Arkansas	234	75	3.1	34.0	34.5	33.4
North Dakota	168	54	3.1	46.6	45.1	33.3
Indiana	290	92	3.2	33.0	32.5	32.8
Oregon	196	36	5.4	29.9	31.0	31.5
Ohio	611	88	6.9	36.4	33.0	31.4
California	936	58	16.1	43.4	39.6	31.3
Wisconsin	421	72	5.8	30.7	31.5	30.9
Illinois	849	102	8.3	28.3	27.7	29.7
Minnesota	329	87	3.8	32.9	31.3	29.1
Delaware	16	3	5.3	22.0	24.0	27.8
Kansas	286	105	2.7	25.4	26.7	26.7
Nebraska	243	93	2.6	28.0	26.9	26.2
Colorado	178	63	2.8	30.1	27.3	25.5
Mississippi	137	81	1.7	26.6	25.5	25.2
Alabama	139	67	2.1	27.0	26.0	24.9
South Dakota	143	62	2.3	20.0	21.4	23.3
Texas	1,017	253	4.0	20.5	22.2	23.1
South Carolina	71	45	1.6	22.1	21.7	21.2
Missouri	516	115	4.5	24.8	22.1	21.1
Utah	41	29	1.4	22.4	20.5	18.6
Louisiana	69	64	1.1	10.4	9.7	14.6
Kentucky	171	119	1.4	19.3	2.9†	14.4
New Mexico	89	33	2.7	12.6	12.4	13.3
North Carolina	118	100	1.2	11.5	11.0	10.6
Georgia	180	159	1.1	5.2	5.1	3.9
Virginia	131	126	1.0	2.9	2.8	2.3
Florida	67	67	1.0	0.0	0.0	0.0
Maryland	24	24	1.0	0.0	0.0	0.0
Nevada	17	17	1.0	0.0	0.0	0.0
West Virginia	55	55	1.0	0.0	0.0	0.0
District of Columbia	1	1	1.0			
Hawaii	1	1	1.0			
National	13,083	3,089	4.2	25.2	22.9	22.6

Notes: Each district is assigned the county of its administrative office, as reported in the NCES ELSI district file. A district lying in one county is unaffected; a district spanning a county line is placed by the location of its central office rather than by the share of its land, which is the assignment a researcher using a district-county crosswalk would also make. Total variance in the two-party Republican share decomposes into a between-county and a within-county component; the year columns give the within-county share in percent, which is the variation any measure assigning county returns to districts sets to zero by construction. Per county is districts divided by counties, the fragmentation measure used in the text; the median state has 3.1 districts per county and the national figure is 4.2. States are ordered by their 2024 within-county share. A dagger marks a state whose 2020 returns are reported on units coarser than a precinct, which mechanically lowers the within-county share for that year only: New Jersey, Kentucky.

Table 3: School Districts Whose County Proxy Assigns the Wrong Partisan Majority

State	Districts	Wrong majority	%	Students	Mean gap (pp)
California	936	165	17.6	1,036,270	14.3
New York	680	160	23.5	413,045	13.8
New Jersey	553	143	25.9	253,673	12.2
Illinois	849	141	16.6	327,146	17.7
Michigan	539	105	19.5	303,205	15.1
Pennsylvania	500	79	15.8	266,092	13.9
New Hampshire	178	78	43.8	62,069	11.4
Ohio	611	66	10.8	272,432	15.5
Washington	295	65	22.0	189,817	19.1
Texas	1,017	64	6.3	1,004,358	13.8
Arizona	213	62	29.1	407,971	22.1
Massachusetts	291	60	20.6	109,944	10.9
Montana	397	54	13.6	24,307	21.7
Maine	254	46	18.1	46,554	15.2
Wisconsin	421	44	10.5	138,769	11.2
Connecticut	164	41	25.0	80,774	11.3
Oregon	196	39	19.9	103,218	17.3
Minnesota	329	38	11.6	101,159	14.4
Iowa	325	28	8.6	75,277	14.3
Indiana	290	25	8.6	177,795	13.2
Colorado	178	24	13.5	113,355	18.0
Vermont	116	19	16.4	10,756	10.7
Mississippi	137	18	13.1	32,901	20.2
Missouri	516	16	3.1	129,697	20.4
Kansas	286	13	4.5	92,879	14.8
Arkansas	234	12	5.1	39,544	13.1
Rhode Island	36	12	33.3	27,063	12.6
Alabama	139	12	8.6	110,695	17.5
Nebraska	243	11	4.5	47,804	21.5
Tennessee	140	10	7.1	44,295	30.1
North Dakota	168	9	5.4	13,410	28.5
Kentucky	171	7	4.1	14,753	17.9
New Mexico	89	6	6.7	23,760	11.0
Utah	41	4	9.8	71,721	19.4
Georgia	180	3	1.7	10,999	18.1
Wyoming	48	3	6.2	1,432	37.0
Idaho	115	2	1.7	24,429	13.7
Delaware	16	2	12.5	10,381	13.0
North Carolina	118	2	1.7	5,697	24.4
Louisiana	69	2	2.9	12,613	28.2
South Dakota	143	2	1.4	495	22.8
South Carolina	71	2	2.8	24,308	20.9
Oklahoma	495	2	0.4	49,665	27.8
Virginia	131	2	1.5	11,014	19.6
Total	13,083	1,698	13.0	6,317,541	15.3

Notes: A district is counted when its two-party Republican share and that of its assigned county fall on opposite sides of 50, so a study using county returns records the wrong partisan majority for the district. County shares are aggregated from the same district estimates, so proxy and measure rest on identical returns and the disagreement is geographic rather than a difference of source. Mean gap is the average absolute difference in percentage points among misclassified districts. Each district is assigned the county of its administrative office, as reported in the NCES ELSI district file. A district lying in one county is unaffected; a district spanning a county line is placed by the location of its central office rather than by the share of its land, which is the assignment a researcher using a district-county crosswalk would also make. Enrollment is the NCES Common Core of Data student count.

Table 4: School District Partisanship by NCES Locale, 2016 to 2024

Locale	N	Students	2016	2020	2024	Change	Close	Landslide
City, Large	191	6,651,420	34.1	34.2	38.6	+1.8	19.6	61.4
City, Midsize	185	3,481,207	40.9	39.1	41.6	+0.6	21.1	56.8
City, Small	397	3,328,979	47.7	45.2	47.4	-0.6	23.0	54.6
Suburb, Large	2,385	16,673,608	48.4	45.3	48.3	-0.7	29.9	45.6
Suburb, Midsize	259	1,566,559	57.3	53.8	55.8	-1.5	25.4	53.5
Suburb, Small	209	726,577	60.2	58.8	60.3	-0.6	18.4	62.3
Town, Fringe	494	1,455,134	63.2	61.2	62.8	-0.7	14.6	67.1
Town, Distant	868	2,208,767	65.8	64.7	66.4	+0.9	12.4	73.9
Town, Remote	544	1,230,098	67.2	65.8	67.9	-0.0	11.6	77.9
Rural, Fringe	1,348	3,108,569	65.8	64.0	65.9	-0.4	12.8	73.8
Rural, Distant	3,436	3,269,523	74.0	73.6	75.0	+1.2	5.2	88.7
Rural, Remote	2,902	1,430,428	77.2	77.3	78.7	+1.3	4.2	90.5
Total	13,218	45,130,869	66.5	65.2	66.9	+0.5	12.9	74.5

Notes: N is the number of districts. Locale is the NCES urban-centric classification assigned to each district in the Common Core of Data. The year columns give the median two-party Republican share among districts in that locale, in percentage points, and Change is the median district-level change between 2016 and 2024 rather than the difference of the two medians. Close is the percentage of the locale's districts whose 2024 share falls between 45 and 55; Landslide is the percentage outside 40 to 60, in either direction. Districts without a published estimate in a given year are excluded from that year. Enrollment is the CCD student count for the school year containing the 2024 election.

Table 5: Validation Against County Returns in Districts Coterminous With Counties

State	Districts	Mean —error— (pp)	Max —error— (pp)	Misclassified
Georgia	129	0.003	0.134	0
Virginia	100	0.001	0.053	0
Kentucky	82	0.000	0.000	0
Tennessee	65	0.028	1.249	0
North Carolina	62	0.001	0.018	0
West Virginia	55	0.000	0.000	0
Louisiana	42	0.000	0.000	0
Mississippi	39	0.000	0.000	0
Florida	30	0.007	0.100	0
Alabama	24	0.001	0.029	0
South Carolina	20	0.006	0.044	0
Utah	20	0.002	0.017	0
Texas	19	0.000	0.001	0
Nevada	17	0.000	0.000	0
Indiana	14	0.002	0.026	0
Colorado	7	0.000	0.000	0
Maryland	7	0.001	0.003	0
Wyoming	7	0.000	0.000	0
Idaho	6	0.000	0.000	0
South Dakota	5	0.000	0.000	0
New Mexico	3	0.000	0.000	0
Pennsylvania	3	0.000	0.000	0
Alaska	2	0.294	0.585	0
Kansas	2	0.000	0.000	0
Arkansas	1	0.001	0.001	0
Ohio	1	0.042	0.042	0
Missouri	1	0.000	0.000	0
Total	763	0.005	1.249	0

Notes: Districts whose boundary is a county's in the 2020 election, so the county's reported returns are the district's own and the dasymetric estimate can be checked against an observed quantity rather than an estimated one. A district qualifies on three conditions: every precinct in the county belongs to it and it holds no precinct outside the county; the two polygons differ by less than 1% of the county's area; and the state does not place an elementary and a secondary district over the same ground, since a county served by both at once has returns belonging to no single district. Of 1,083 districts meeting the membership condition, 1,038 remain after the overlay exclusion, 942 after containment and 763 after the shape tolerance. Because every precinct then nests inside the district, this test bounds error in the allocation machinery rather than in the land cover weighting, which cannot matter where a precinct lies wholly inside one district. Mean and maximum absolute error are in percentage points. Misclassified counts districts whose estimated and observed shares fall on opposite sides of 50. Source: `coterminous2020_by_state.csv`.

Table 6: Reproducing Hartney and Finger (2022): Partisanship Coefficients and In-Person Reopening

Specification	Hartney & Finger (2022) measure	z	Our Dasymetric Measure	z	Districts
Model 1. State and locale FE	2.951 (0.263)	11.22	2.873 (0.240)	12.00	9,338
Model 2. State $\times$ locale FE	2.916 (0.272)	10.72	2.811 (0.244)	11.52	9,338
Model 3. Commuting zone FE	2.743 (0.371)	7.39	2.709 (0.216)	12.55	9,338
Model 6. County FE, Ohio	2.046 (1.217)	1.68	2.648 (1.216)	2.18	606
County FE, national	n/a	n/a	2.580 (0.329)	7.84	8,157

Notes: Cell entries are ordered probit coefficients on partisanship with standard errors in parentheses. Standard errors are clustered by state in the first three rows and by county in the last two, following Hartney and Finger (2022). Their measure is the county proxy in the first three rows and the district-level Ohio measure they assembled in the fourth. Ours is the dasymetric district estimate substituted for that variable on the same districts, so a difference is attributable to the measure rather than to sample attrition. The final row has no published counterpart: a county proxy takes one value within each county, so county fixed effects absorb it and their measure cannot enter the model in any state. A district that is the only one in its county is fit exactly by its own dummy and contributes nothing to the likelihood under any measure; 1,181 of the 9,338 districts are in that position, leaving 8,157 districts in 1,672 counties. Published estimates are as reported in their Table 1. Re-estimating their published specifications recovers their coefficients to within 0.03 and their standard errors to three decimals in columns 1, 2 and 6. In column 3 the coefficient reproduces (2.745 against their 2.743) but the standard error is 0.330 against their 0.371, which we attribute to the commuting zone crosswalk: ours resolves 562 zones.

Online Appendix - Supplementary Materials

Figure 6: School District Partisanship vs. County Partisanship

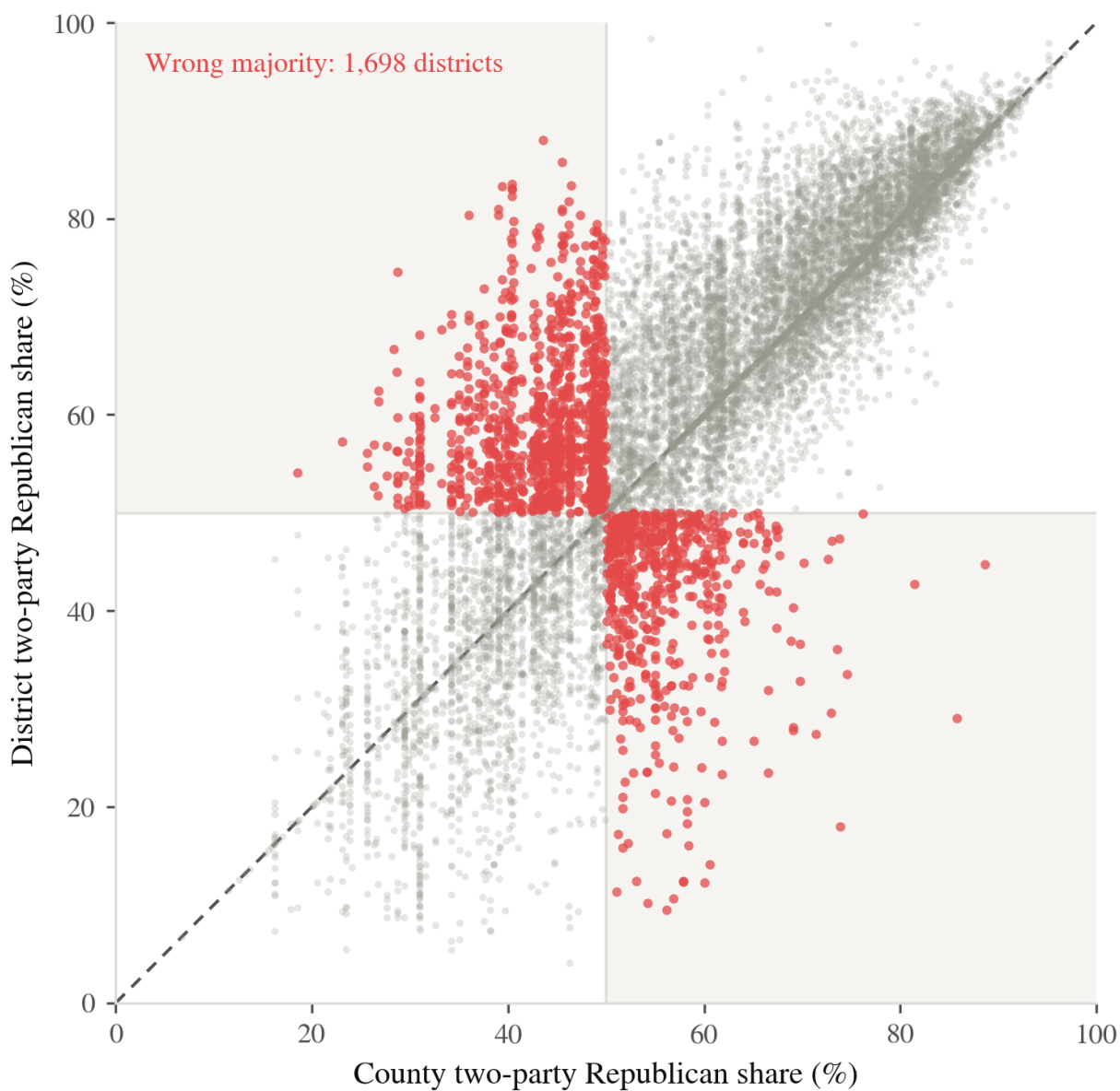
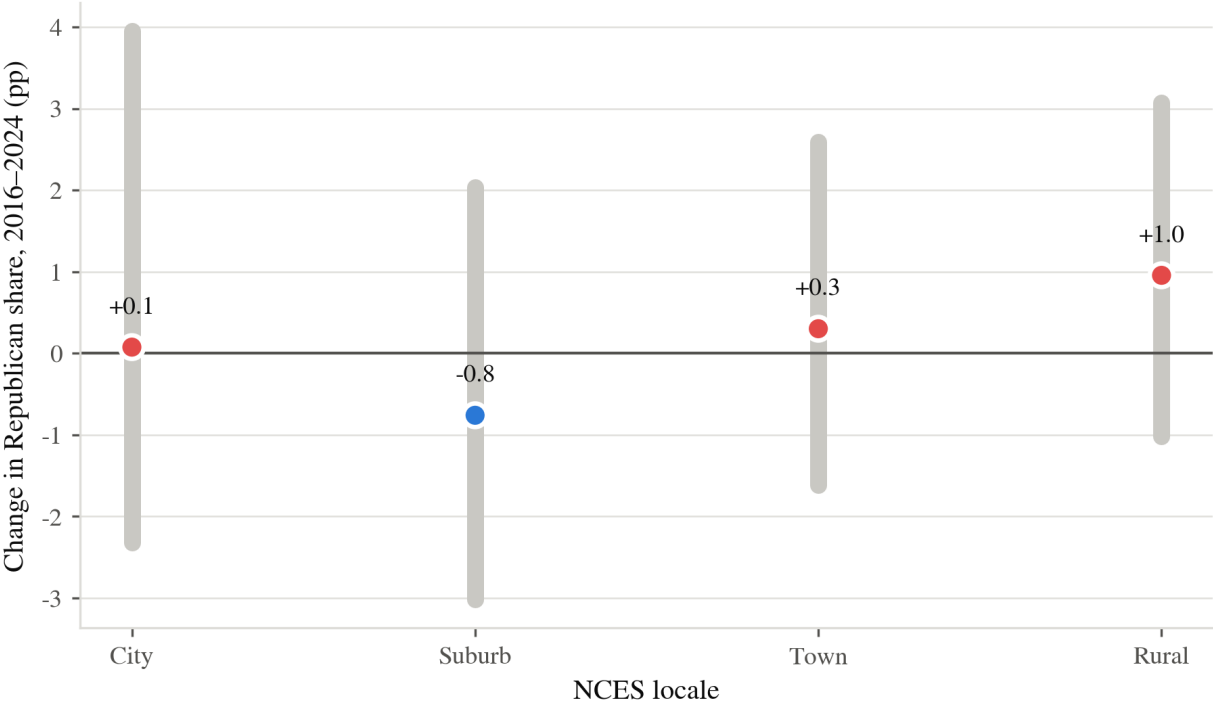


Figure 7: Shifts in Partisanship by Locale



Points are medians; bars span the interquartile range.