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Abstract

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1 Introduction

Rural school districts in the United States face unique challenges relative to their urban and suburban counterparts, such as frequent staffing turnover, high transportation costs, and limited economies of scale (Sipple and Brent, 2015; Showalter et al., 2019). These characteristics may reduce how much rural districts can spend on specialized staff (e.g., social workers and guidance counselors) and curriculum (e.g., AP courses and career and technical education), potentially contributing to the well-documented disparities in educational outcomes between rural and non-rural students. For instance, rural students are four percentage points less likely to attend college and seven percentage points less likely to earn a bachelor's degree than their non-rural peers (Wells et al., 2019).

As a potential remedy to these inequalities, 34 states provide additional funding to rural districts through grants and multipliers that account for their low enrollment, low density of students, and/or isolated location (Education Commission of the States, 2021). While prior literature documents that, on average, increases in school funding improve student outcomes (Jackson, 2020), the heterogeneity in observed effects (Jackson and Mackevicius, 2021) and unique challenges of rural education makes the efficacy of increased funding to rural districts less certain. For example, given rural districts' distinct cost structures, they may allocate additional funds differently than their urban or suburban counterparts, generating different effects on student achievement and educational attainment. Moreover, despite the relevance to policymakers, there are few attempts to estimate the return to additional school funding in rural districts specifically.

In this paper, we evaluate the impact of Wisconsin's Sparsity Aid program, which is one of the largest state-level school finance programs targeting small, rural districts. Currently, the program provides \$28 million in additional, unrestricted funding to over 140 districts in the state. Our empirical approach leverages the introduction of the program in 2008 and subsequent expansion in 2010 in event study and difference-in-differences designs that compare the outcomes of school districts that were eligible and ineligible for sparsity aid, before and after the policy changed. Using comprehensive school finance data from the U.S. Department of Education's Common Core of Data (CCD), we first show that receiving a sparsity aid payment increases annual spending on elementary and secondary education by approximately \$226 per student, or 2% of average spending. With detailed spending data from Wisconsin's Department of Instruction (DPI), we further document that districts use these funds in a variety of ways: we estimate positive, statistically significant increases in total spending on administration, food service, and general operations, as well as non-salary spending on instruction outside of core academic subjects (e.g., extracurricular activities). Notably, we find minimal effects on teacher staffing —including student/teacher ratios, average salaries, and experience levels —but find increases in the staffing of administrative positions. Furthermore, we find substantial heterogeneity in how districts allocate funds, with increases in spending in most categories inversely related to districts' baseline budget shares. This finding suggests that the unrestricted nature of the sparsity aid program allows administrators to put the additional funding towards areas that were relatively underfunded prior to the program's introduction. In a survey of school district administrators, we confirm that districts used these funds flexibly and rarely earmarked them for specific purposes.

We then assess the impact of this increased spending on educational outcomes using student-level data from the Wisconsin DPI. We find little evidence that the increased spending resulting from the program improved student performance on state standardized tests —our point estimate for average scores across grades and subjects is negative, but statistically indistinguishable from zero and we can rule out improvements above 0.75% of a standard deviation. We also see little effect of the program on behavioral outcomes, such as attendance and disciplinary incidence. However, we find modest effects on postsecondary enrollment and completion for students with a low baseline probability of enrolling or completing college. Specifically, we find that the sparsity aid program increases college enrollment within one year of high school graduation by 2.5pp (5.9% of mean) for students with below-median propensity to enroll in college and increases college completion by 1.5pp (6.4% of mean) for students with below-median propensity to complete college. These effects are driven by statistically significant increases in enrollment and completion in the two-year college sector, such as community colleges.

Our findings contribute to several related lines of literature on public investments in education and educational interventions in rural contexts. First, we contribute to a growing set of studies on a variety of education policies targeted at rural schools and students. In recent years, an increasing number of small and rural school districts have adopted four-day school weeks as a cost-saving measure (Thompson et al., 2021). While these adoptions have reduced expenditures (Thompson, 2021a), they have also caused a reduction in student achievement (Thompson, 2021b). Other rural communities have either chosen or been mandated to reign in costs by consolidating school districts (Duncombe and Yinger, 2007). Such consolidations do not necessarily affect districts' economies of scale (Gordon and Knight, 2008), but they may lead to increased student performance (McGee et al., 2022) at the expense of population growth and property values (Smith and Zimmer, 2022). Some states have also begun to allocate funding to rural communities to specifically address teacher shortages, which Tran and Smith (2021) find modestly reduces teacher turnover. Other states have attempted to retain teachers and bolster student success by providing professional development in specific content areas to teachers in rural areas, realizing significant math gains (Barrett et al., 2015). While these studies consider narrowly prescribed interventions, we examine the efficacy of additional, unrestricted funding that allows rural districts to respond to their unique needs.

Second, we add to a broad literature on how school spending affects student outcomes. Much of this literature addresses the endogeneity of school spending by exploiting variation in court-ordered school finance reforms that weakened the correlation between district wealth and per-pupil spending. These studies generally find positive effects of increased school spending on test scores (Papke, 2005; Roy, 2011; Lafortune et al., 2018), high school completion (Candelaria and Shores, 2019), educational attainment (Hyman, 2017), adult earnings (Jackson et al., 2016), and income mobility (Biasi, 2021b). These effects are generated by sustained increases in school spending that are much larger than the increases induced by the Wisconsin sparsity aid program or by other grant programs that target rural school districts. For example, Lafortune et al. (2018) estimate that, on average, a school finance reform increases state funding for low-income districts by \$1,225 per student per year and for high-income districts by \$527 per student per year. The sparsity aid program, in contrast, increases funding by \$300 per student per year, or less, during the timeframe of our analysis. In this way, our findings are more closely aligned with studies of smaller-scale investments in schools, such as funding for new technology purchases (Bass, 2021) or textbooks (Holden, 2016). However, the fact that the sparsity aid program allows for unrestricted spending —along with its focus on rural areas —distinguishes our setting from this prior work.

To our knowledge, there are only two studies that quantify the effect of changes in school spending in rural districts. First, Rauscher (2020) studies how decreases in per-pupil spending, as a result of enrollment increases under a block funding scheme in Kansas, affect student achievement in rural and non-rural school districts. Rauscher finds that a \$900-\$950 decline in per-pupil spending per year led to a 0.13-0.15 standard deviation decline in math achievement and insignificant decline in ELA achievement in rural districts compared to a 0.05-0.07 decline in both subjects in non-rural districts. This larger effect for math scores is not necessarily due to spending having greater importance in rural districts; rather, constant dollar-sized reductions simply make up a greater share of rural districts' budgets. Second, and more closely related to our analysis, Kreisman and Steinberg (2019) exploit two features of Texas' school finance system that provide additional funding to geographically large districts with low enrollment. Using regression discontinuity and regression kink designs, they find that an additional \$1,000 in funding per year over students' schooling years improves reading scores by 0.1 standard deviations, improves math scores by 0.07 standard deviations, decreases high school dropout rates by 1.6 percentage points, and increases college enrollment among students who take college preparatory exams (e.g., SAT, AP exams) by 10 percentage points. However, many students who enroll in a two-year college do not take these exams, so Kreisman and Steinberg are not able to fully measure enrollment changes in the two-year sector.

We build on this prior work by providing new evidence on the impacts of increased education funding to rural communities within a different demographic and policy context. The Wisconsin policy we study serves districts that are, on average, much smaller and less dense than the Texas districts studied by Kreisman and Steinberg (2019). The policy is also structured as a stand-alone grant, rather than being embedded within the state's general aid formula, or a result of enrollment changes under a fixed budget as in Rauscher (2020), which may affect how the program is perceived and used by district administrators. In addition, our empirical approach that leverages the introduction and expansion of the program allows us to estimate effects across all eligible districts, rather than those situated at the enrollment and density cutoffs, which are the largest and least sparse of districts receiving additional funding, or those with large enrollment gains. In doing so, our paper considers how the effects of additional funding to rural school districts may vary across settings and provides new insights into how programs similar to Wisconsin's may affect the outcomes of similarly small and sparse rural districts.

2 Policy Background & Data

2.1 Policy Introduction

Wisconsin is home to 421 unique school districts, each of which is funded by a combination of state aid (46.1%, on average), local property taxes (42.2%), federal funding (7.2%), and other local revenue sources (4.5%) (Kava and Pugh, 2019). The majority (79%) of state aid is allocated via a "general aid" formula that distributes funds based on districts' per pupil value of taxable property to equalize funding across districts with low and high property tax revenues. The remainder (21%) of state aid is allocated via categorical aid programs, which are designed to fund specific costs faced by districts, such as special education, transportation, and limited-English proficiency (LEP) programs. Unlike general aid, categorical aid programs are distributed without regard to the district's local property tax revenues and are not subject to state revenue limits that cap the total amount of general state aid and local property tax revenues a district can receive.[1] As such, categorical aid programs can increase a district's resources even if they are not eligible to receive additional general aid.

In 2007, under Wisconsin Act 20, the state legislature added a new categorical aid program called the Sparsity Aid Program. The goal of the program was to provide additional unrestricted funds to rural school districts experiencing small economies of scale.[2] Initially, districts were eligible to receive sparsity aid funds if (1) they had a pupil membership of no more than 725 in the prior year, (2) they had a density of fewer than 10 members per square mile in the prior year; and (3) at least 20% of the district's students in the prior year were eligible for free or reduced-price

[1] For a full discussion of revenue limits in Wisconsin and the impact on students of raising them, see Baron (2022).
[2] See the Wisconsin Sparsity Aid Program website for a full legislative history.

lunch (FRL). The program first became active in the 2008-2009 school year, during which eligible districts whose FRL percentage fell between 20 and 50 percent were slated to receive \$150 per pupil while those whose FRL percentage exceeded 50 percent received \$300 per pupil. However, the total program appropriation was not large enough to make these payments, so payments were prorated to \$67 per pupil and \$134 per pupil, respectively. Beginning in 2009, the legislature removed the bifurcation and all eligible school districts were eligible to receive \$300 per pupil. Once again, the program was underfunded and, due to proration, eligible districts only received \$69 per pupil.

In 2010, the Wisconsin legislature significantly expanded funding for sparsity aid, from \$3.5 million per year to nearly \$15 million per year. Today, the program allocates almost \$28 million per year to districts and is one of the largest categorical aid programs in the state. From the 2010-2011 school year onward, eligible districts received between \$237 and \$400 per member per year, including any necessary prorations. The only other change in program eligibility occurred in 2015 when the FRL requirement (which was not binding for any otherwise eligible districts) was removed and the enrollment eligibility threshold increased from 725 to 745.

Figure 1 plots the average sparsity aid amount received by districts, both in total and on a per-member basis, that are consistently eligible for sparsity aid from 2008 to 2017.[3] Prior to 2008, districts received no sparsity aid. In 2008 and 2009, districts received an average of about \$32,480 annually, or \$72 per member per year. Since 2010, districts have received an average of \$115,350 annually or \$269 per member per year. For context, this total amount is approximately equal to 2.5 times the average full-time equivalent (FTE) teacher's salary in sparsity-eligible districts. Given that these districts employ an average of 35 teachers, this additional funding —while small in per-member terms relative to previously studied school finance interventions —represents a meaningful increase in districts' available resources.

[3]Of the 106 districts initially eligible for the program in 2008, 104 remain eligible across the entire time series.

2.2 Data Sources

Our primary data source for student outcomes is the Wisconsin DPI student-level records from 2005-2006 through 2017-2018. These records contain demographic information, enrollment history, attendance data, disciplinary infractions, and standardized test scores for every student who attended a Wisconsin public school in the time period. The state further links these records to post-secondary enrollment and completion records from the National Student Clearinghouse (NSC).[4] We supplement the student-level data with several sources of district-level data. We obtain a rich set of district-level demographics, enrollment, and financial information from the National Center for Education Statistics (NCES) Common Core of Data (CCD), along with annual sparsity aid payments and additional school finance outcomes —such as the revenue districts receive from local, state, and federal sources and the amount they spend on instruction, support, and administration —from the DPI. We also obtain district-level information on teacher and administrator staffing, including full-time equivalent (FTE) staffing levels, average salaries, and average years of experience, from the DPI. In addition, we follow Bayer et al. (2021) to aggregate annual census tract-level house price index data from the Federal Housing Finance Agency (FHFA) to the school district level to track district-level house price indices over time.[5]

To validate and expand upon results from these sources of administrative data, we additionally conducted a survey of rural school district leaders throughout Wisconsin. The survey primarily contained questions regarding the usage of sparsity aid funds, but it also measured general awareness of the program and the expected effects of receiving sparsity aid funding. We distributed the survey electronically to all superintendents/district administrators, principals, and financial officers who were employed by a district receiving sparsity aid funding in 2022. The Wisconsin Rural School Alliance (WiRSA) also advertised the survey in an email newsletter. Out of the 409 employees we attempted to contact via email, 39 (9.5%) completed the survey, representing 35 distinct school districts. For our analysis, we drop 3 observations that reported not working in a sparsity aid-eligible district. Appendix B contains the full text of the survey and recruitment email;

[4] Additional information about Wisconsin's use of NSC data is available on the DPI website: `https://dpi.wi.gov/wisedash/districts/about-data/ps-enrollment`.

[5] All indices are measured relative to the first year the FHFA tracts data for a given tract. See Bogin et al. (2018) for more detail on the construction of this dataset.

we reference the results throughout the text.

2.3 Sample Restrictions

Because the goal of our analysis is to compare the outcomes of similar districts that did and did not receive sparsity aid funding, we limit our sample to school districts that offer all grades K-12 and are either always or never eligible for the sparsity aid program.[6] We drop 13 districts that have poor house price index coverage and 5 districts with implausibly large spikes in either revenue or spending that we believe are due to data reporting errors. We then drop 113 districts that were in the top 30% of the enrollment or density distribution prior to the policy's introduction in 2008. This restriction eliminates Wisconsin's largest cities and suburbs, which differ in multiple dimensions from rural areas, and may provide poor estimates for the counterfactual outcomes of rural districts had rural districts not received sparsity aid.[7] Finally, because we will consider specifications with flexible region-specific time trends, we drop 14 non-sparsity districts located in regions where no districts in the region both meet the above criteria and receive sparsity aid payments.[8]

Our final sample consists of 89 districts that receive sparsity aid payments beginning in 2008 and 99 districts that never receive sparsity aid payments. Figure 2 identifies the locations of these districts. Both sparsity-eligible and ineligible districts in our sample are geographically distributed throughout Wisconsin and, often, eligible and ineligible districts are located next to one another. The only area of the state that our sample does not cover is the southeast region, in and around the Milwaukee metropolitan area.

2.4 Summary Statistics

Table 1 provides summary statistics on the school districts in our sample and throughout all of Wisconsin, averaged across the academic years 2003-2007. Panel A provides information on the size and location of sparsity-eligible and ineligible districts. Unsurprisingly given the eligibility

[6]The majority of Wisconsin school districts offer all grades K-12. In 2007, there were 426 local school districts in the state, 369 (86.6%) of which offered grades K-12. 46 districts only offered grades K-8, while 1 only offered grades 6-12 and 10 only offered only grades 9-12.

[7]In Section 4.1 and Appendix Figure A.7, we consider alternative sample restrictions and show that our effects are similar across different enrollment and density criteria.

[8]Throughout the analysis, we define school district regions using Wisconsin's Cooperative Educational Service Agency (CESA) definitions. CESAs are collections of adjacent school districts that facilitate communication and cooperation across districts in the same area of the state. More information is available on the Wisconsin DPI website.

guidelines of the program, sparsity-eligible districts are smaller and less dense than the districts in our comparison group. However, the comparison group itself is relatively small and sparse compared to Wisconsin as a whole: the average Wisconsin school district enrolled 2,035 students and 45.9 students per square mile before 2008, whereas our comparison group enrolled an average of 1,231 students and 9.31 students per square mile. Sparsity districts also tend to have fewer school buildings and fewer students per building than their non-sparsity peer districts in the comparison group, with the comparison districts still being smaller than the overall Wisconsin average.

In Panel A, we also compare school districts' locale classifications from the NCES across our sample and the state. As expected, 97.8% of sparsity-eligible districts are located in rural areas (defined as an area outside of an urban cluster) and 2.2% are located in towns (defined as an area within an urban cluster, but outside of the primary urbanized area). 70.7% of our comparison group are also located in rural areas, with 28.3% in towns and 1% in suburban areas. No districts in our comparison group are located in urban areas. In contrast, 7% of Wisconsin school districts statewide are located in urban areas, with an additional 14.7% located in suburban areas.

Panel B of Table 1 then provides summary statistics on the demographic characteristics of the two groups of school districts. The racial demographics of our treatment and comparison groups are similar, with both enrolling approximately 94% white students. In contrast, the average Wisconsin district was 88.7% white between 2003 and 2007. Students attending sparsity districts are somewhat more disadvantaged than our comparison districts, with an average FRL rate of 34.7% (vs. 23.7%) and an average local child poverty rate of 14.5% (vs. 9.5%). The house price index in sparsity districts is also about 36 percentage points lower than that in the comparison districts, though the comparison district group average is also lower than the Wisconsin average.

Next, Panel C compares district finances in sparsity-eligible districts to the comparison districts in our analysis sample. Sparsity districts both receive and spend more per member than the comparison group —and the state average —prior to the introduction of the sparsity aid program, which is surprising given that sparsity districts tend to be less wealthy than non-sparsity districts. However, in Appendix Figure A.1 we show that there is a striking non-linear relationship between district size and per-member finances: smaller districts receive and spend much more per student than larger districts do, perhaps due to fixed costs and economies of scale that allow districts to reduce per-student costs as enrollment increases. Thus, because sparsity districts are, by definition, smaller than their peer districts that are not eligible for sparsity aid, they tend to have higher revenues and expenditures on a per-student basis. Sparsity districts also tend to rely more heavily on local funding sources than their non-sparsity peers, although both the sparsity and comparison groups are less locally-reliant than the average Wisconsin district.

Panel D then compares teacher and administrator staffing across sparsity-eligible and ineligible districts. Due to their small size, sparsity districts employ fewer teachers and administrators (superintendents, principals, and directors/coordinators). However, sparsity districts have similar —or a bit higher —staffing levels on a per-student basis. In addition, sparsity and non-sparsity districts employ teachers with similar levels of experience. Despite this similarity in experience, and the fact that sparsity districts spend more per-student overall, teachers in sparsity-eligible districts are, on average, paid roughly $2,900 (6.7%) *less* than teachers in the comparison group. This disparity suggests that the higher spending levels in sparsity districts are not due to higher investments in teacher pay, but potentially a result of the higher per-student operating costs sparsity districts face due to their small size and lack of economies of scale.

Finally, Panel E highlights differences in baseline achievement between sparsity districts and the comparison group. While we might expect sparsity districts to outperform their sparsity-ineligible peers because of their higher levels of spending and smaller school sizes (Kuziemko, 2006; Gershenson and Langbein, 2015; Egalite and Kisida, 2016), prior to the sparsity aid reform, they tended to have lower levels of achievement than their non-sparsity peer districts *and* the state average. A smaller share of students grades 3-8 and grade 10 were rated as "proficient" on state math and reading exams, and a smaller share of students enrolled in college within one year of graduating from high school or completed college by the end of our data's timeframe. These disparities lend credence to the idea that sparse, rural school districts face additional challenges in educating their students and motivate our analysis of whether increased state funding via the sparsity aid program can improve outcomes.

3 Empirical Strategy

3.1 Difference-in-Differences Framework

Our empirical strategy leverages the introduction and subsequent expansion of the sparsity aid program, which generated exogenous increases in district funding for eligible districts and should not have affected ineligible districts. To demonstrate that the program indeed increased district revenues, and to assess how districts spent these additional funds, we begin by estimating the effect of the program on district-level outcomes by estimating equations of the following form:

$$Y_{dt} = \beta \text{SparsityAid}_{dt} + \mathbf{Z}_{dt}\mathbf{\Pi} + \theta_d + \delta_t + \varepsilon_{dt} \tag{1}$$

where Y_{dt} is an outcome (e.g., revenues or expenditures per student) for district d in year t and SparsityAid_{dt} indicates whether district d received sparsity funding in year t. This variable "turns on" for all eligible districts in 2008 and remains zero for all ineligible districts throughout the entire timeframe of the sample. $\mathbf{Z}_{dt}$ are time-varying district covariates (e.g., enrollment, student demographic composition) that may also affect a district's outcomes over time. We discuss our choice of control variables in the context of our identification assumptions in Section 3.2. θ_d are district-level fixed effects that capture any time-invariant characteristics of school districts (e.g., location within the state) and δ_t are year fixed effects that capture any state-wide changes in district finances or student outcomes over time. ε_{dt} is the error term. Throughout the analysis, we cluster all standard errors at the school district level.

The coefficient of interest in equation (1) is β, the difference-in-differences (DID) estimate of how a school district's outcomes change when it becomes eligible for the sparsity aid program. In order for β to represent the causal effect of the sparsity aid program on outcomes, it must be the case that sparsity-eligible districts' outcomes would have evolved the same as non-sparsity-eligible districts' outcomes had the sparsity aid program never been implemented.[9] While this counterfactual assumption is inherently untestable, we assess its plausibility by extending our DID

[9] Since the treated districts in our sample all receive treatment at the same time and never lose their treated status, we do not face the econometric problems associated with staggered treatment timing identified in recent DID methodological research. See Roth et al. (2022) for a recent literature review.

equation to the following event study specification:

$$Y_{dt} = \sum_{k=2003, k\neq 2007}^{2017} \beta_k \text{SparsityEligible}_d * 1[t=k] + \mathbf{Z}_{dt}\mathbf{\Pi} + \theta_d + \delta_t + \varepsilon_{dt} \tag{2}$$

where $\text{SparsityEligible}_d$ indicates that a district will be eligible for sparsity aid funding when the policy is implemented and k indexes years. The β_k coefficients, therefore, trace out differences in the trends between sparsity and comparison districts' outcomes before and after the sparsity aid policy was implemented in 2008. If the two groups were trending similarly prior to the policy, we expect that the β_k coefficients will be equal to 0 up until 2006.

We also extend our district-level regression from equation (1) to consider student-level outcomes for students in grades 3-12 by estimating equations of the following form:

$$Y_{igsdt} = \beta \text{SparsityAid}_{dt} + \mathbf{Z}_{dt}\mathbf{\Pi}_g + \mathbf{X}_{it}\mathbf{\Gamma}_g + \lambda_{sg} + \delta_{tg} + u_{igsdt} \tag{3}$$

where Y_{igsdt} is an outcome (e.g., standardized test score or college enrollment) for student i, who is enrolled in grade g in school s in district d in year t. SparsityAid_{dt} is equal to 1 if student i's district d receives sparsity aid funding in year t. $\mathbf{Z}_{dt}$ are the same time-varying district covariates as equation (1) and X_{it} are student characteristics that may or may not vary over time, such as their race, gender, FRL status, and special education status. In specifications that include multiple grade levels, we allow the effects of both sets of covariates to vary by grade level. λ_{sg} are school-by-grade fixed effects that capture any time-invariant characteristics of individual schools at each grade level and δ_{tg} are year-by-grade fixed effects that capture any secular trends by grade level. When we estimate specifications with only one grade level —for example, postsecondary outcomes for graduating seniors —these fixed effects collapse to the school and year levels, as in our district-level regressions. u_{igsdt} is the error term. We continue to cluster standard errors at the district level and also extend equation (3) to an event study equation to test for pre-trends.

3.2 Identification Assumptions

Our DID empirical framework relies on the assumption that school districts ineligible for the sparsity aid program serve as valid counterfactuals for school districts eligible for the sparsity aid program. Functionally, this assumption can be broken down into two parts. The first part is that the outcomes of school districts eligible and ineligible for sparsity aid were trending similarly prior to the introduction and expansion of the program. The β_k coefficients in the event study specifications from equation (2) allow us to test this assumption directly.

The second part of our identification assumption is the untestable assumption that there are no changes in unobserved determinants of our outcome measures that occur concurrently with the introduction of the sparsity aid program *and* which differentially affect sparsity-eligible and ineligible districts. This assumption could be threatened if there are (1) policy changes surrounding the introduction of the sparsity aid program that differentially affect sparsity eligible or ineligible districts and/or (2) if there are underlying demographic and economic trends that differ between sparsity districts and our comparison group. We address both sets of identification challenges in the sections that follow.

3.2.1 Concurrent Policy Changes

While we are unaware of any policy changes that occurred alongside the introduction of the sparsity aid program and specifically targeted sparsity-eligible or ineligible districts, there were several other education policy changes in Wisconsin during the timeframe of our sample that may threaten our empirical approach. One of the largest education-related policy changes in Wisconsin during the past 20 years was the passage of Act 10 in 2011, which discontinued collective bargaining requirements over teachers' salaries. As school districts' existing collective bargaining agreements (CBAs) expired in the years following 2011, they were able to pay teachers outside of standard salary schedules. Biasi (2021a) shows that the end of these CBAs and the subsequent introduction of flexible pay raised salaries of teachers with high value-added (VA) measures, increased cross-district teacher mobility to districts with flexible pay, and improved student achievement. Biasi and Sarsons (2022) further show that the adoption of flexible pay schemes following

Act 10 induced a gender wage gap in teacher salaries.

Because Act 10 occurred at the state level and did not target rural school districts, it is not obvious that its introduction would threaten our identification strategy. However, the policy change could have differentially impacted sparsity-eligible districts if they (1) had collective bargaining agreements that expired earlier or later than those in the comparison districts and/or (2) if they employed higher or lower VA teachers, who faced different incentives to move to flexible play districts after Act 10. While we lack the data to answer these questions precisely, we provide evidence in Section 4.3 that a variety of teacher-related staffing outcomes —including the number of teachers, salary distributions, and experience —trended similarly in sparsity eligible and ineligible districts from 2003 through 2017. Thus, we do not believe that the introduction of Act 10 poses a threat to our identification of the effects of the sparsity aid program.

Besides Act 10, there were two smaller policy changes in Wisconsin during our sample period that may have affected sparsity-eligible and ineligible districts differently. First, beginning in the 2014-2015 academic year, Wisconsin changed its standardized testing regime three times in three years due to a combination of technical troubles in transitioning to online exams and a series of legislative decisions related to the national Common Core curriculum (Mason, 2016). It is reasonable to expect that sparsity-eligible districts —which are smaller, have fewer specialized staff, and may face additional barriers to internet access —were less equipped to deal with these regime changes than our comparison districts. In addition, it is unclear how to compare exam results over time given the changes in content and modality. As such, we limit our analysis of test scores to those through the 2013-2014 academic year.

The second policy change that may have differentially affected sparsity districts was the addition of a "high cost pupil transportation aid" categorical aid program beginning in 2013-2014.[10] As of 2019, the program stipulates that districts receive additional transportation funding if their transportation cost per member is greater than 145% of the state average in the prior year and their density is less than or equal to 50 students per square mile (Wisconsin Legislative Fiscal Bureau, 2019). While sparsity districts may be more likely to meet these criteria, districts in our compari-

[10] Additional information on the high cost pupil transportation aid program is available on the DPI website: `https://dpi.wi.gov/sfs/aid/categorical/high-cost-pupil-transportation-aid`.

son group are also relatively sparse and, thus, may also qualify for the program. Indeed, using data from the DPI on eligibility and payments for the program, we find that 86% of sparsity districts received payments from the program in at least one year from 2013-2017 and 33% of comparison districts did. Given this variation, we present specifications that control for districts' receipt of additional transportation aid. We find that doing so minimally changes our results, indicating that this policy change is not a main driver of our findings.

3.2.2 Demographic & Economic Trends

Rather than policy changes, perhaps the largest threat to our identification strategy is the fact that sparsity-eligible districts experience more pronounced decreases in membership during the timeframe of our analysis than their sparsity-ineligible counterparts. Between 2003 and 2017, sparsity-eligible districts saw an average membership decrease of 76.3 students —or 15.3% of their baseline membership. In contrast, membership in sparsity-ineligible districts declined by an average of 61.7 students which, due to their larger size, represents only a 5% decrease in their baseline membership. Appendix Figure A.2 presents these membership trends, both in raw numbers and in relative decreases from districts' 2003 baselines. Panel A of Appendix Figure A.3 then presents event study estimates of districts' log membership before and after the sparsity aid program's introductions, both with year fixed effects and with year-by-region fixed effects to capture the fact that different regions of the state may be experiencing different migration and fertility trends over time. Both sets of estimates show a consistent decline in membership in sparsity districts that begins before the program began and continues after.

Appendix Figures A.2 and A.3 provide little evidence that the decline in membership in sparsity districts differentially changes when the sparsity aid program is introduced. This consistent downward membership trend, combined with the relatively small amount of funding provided to districts —particularly in the first years of the program —makes it unlikely that households re-sorted between school districts in response to the policy. However, the underlying membership trends in sparsity and non-sparsity districts raise two concerns for our empirical strategy. First, larger membership declines in sparsity districts may reflect changes in districts' demographic characteristics or school quality that are related to academic achievement outcomes. Second, changes in membership will mechanically affect districts' per-member financial outcomes, such as revenues and spending per member. As such, we control for districts' log membership in all of our empirical specifications. In addition, we directly test whether districts' demographic characteristics change differentially in sparsity and non-sparsity districts over our sample period. The remaining panels of Appendix Figure A.3 present event studies for select characteristics. While we see little change in the share of students who qualify for free or reduced-price lunch nor the share identified as eligible for special education services, we see a slight increase in the share of students that are white in sparsity districts, relative to non-sparsity districts. To capture these compositional changes, our preferred empirical specifications control for districts' racial composition (% white, % Black, % Hispanic, and % Asian), along with their % FRL, % special education, and the local child poverty rate.

A related concern to declines in membership is the potential for different effects of the Great Recession on sparsity-eligible and ineligible districts. Given the large role of local property taxes in Wisconsin's school finance system, differential changes in local home values during the housing and financial crisis could result in differential changes in school district resources over the same time period. Appendix Figure A.4 plots changes in districts' house price indices (HPIs) over time. Panel A presents averages of the HPIs, while Panel B standardizes each district's index relative to 2003. While home prices in sparsity-eligible and ineligible districts are generally trending similarly prior to the start of the sparsity aid program, sparsity-eligible districts did not experience as large of a decline in the 2007-2012 period as sparsity-ineligible districts, which had higher prices prior to the start of the Great Recession.

Panel A of Appendix Figure A.5 presents event study estimates of districts' log-HPI, which are somewhat attenuated by the inclusion of year-by-region fixed effects. Panel B shows similar effects for the log of total property values in the district, as reported by the DPI. Panels C and D then provide event study estimates for per-member property values and property taxes, which mechanically capture both the changes in property values and the changes in membership described above. Given the concurrent decline in membership and slight increase in home values, both measures exhibit upward pre-trends which continue after the introduction of the sparsity aid program. While the increase in property taxes per member does not affect eligible districts' receipt of sparsity aid funds, it could affect the total state revenue they receive since Wisconsin's state finance system equalizes resources between districts with low and high property tax revenues. To show that this increasing trend does not contaminate our results, in Appendix Figure A.6 we show that these differential trends largely disappear if we include year-by-region FEs and control for both a district's log membership and log-HPI, which we include in our preferred empirical specifications. Further, in Section 4.2, we show that our estimates of the effects of the sparsity aid program on districts' finances are robust for controlling directly for districts' property value per member or property tax revenue per member.

In summary, to address potential threats to our identification assumption, our preferred specifications include district-level control variables that capture relevant changes in districts' demographic and economic conditions over time that may be related to their financial and student achievement outcomes. Specifically, we control for a district's log membership, log house price index, the number of school buildings, racial composition, % FRL, % special education, and the local child poverty rate, as well as region-by-year FEs. In the results that follow in Section 4.1, we show that our estimated effects of the sparsity aid program on districts' finances are generally similar with and without these controls, further validating our choice of the comparison group and difference-in-differences framework.

4 Effects of Sparsity Aid Program on District Finances

We begin our analysis by estimating how the sparsity aid program affected eligible districts' finances. Because the program provides unrestricted funds to districts —and districts can receive funds even when they have reached the state revenue limit for local revenue and general state aid —we expect to see corresponding increases in districts' revenue and expenditures. We further investigate how districts use sparsity funds by estimating effects across expenditure categories.

4.1 Total Revenues & Spending

Figure 3 presents the event study estimates from equation (2) for districts' financial outcomes. First, in Panel A, we consider the relationship between a district's sparsity aid eligibility and the state revenue they receive from sources other than the general aid formula. From 2001 to 2007, there is no differential trend in non-formula state revenue between sparsity-eligible and ineligible districts. Then, beginning in 2008, we see that sparsity eligibility districts see an increase in non-formula state revenue per member that is approximately the same size as the sparsity payments. This effect persists when the sparsity aid program is expanded in 2010 and becomes somewhat larger than the sparsity payments beginning in 2015. This shift is due to an expansion of high-cost pupil transportation aid in 2015, for which sparsity aid districts were more likely to be eligible. As discussed in Section 3.2, we present specifications that control for districts' receipt of funding from the high-cost pupil transportation program.

In Panels B and C, we present event study estimates of a district's total revenue per member and total current spending on elementary and secondary education per member. For revenues, we see no evidence of pre-trends prior to the sparsity aid policy and clear increases in total revenues per member after the policy that are consistent with the size of sparsity aid payments. For expenditures, we again see evidence of parallel trends before 2008, but we do not see increases in the first two years of the program. This lack of a spending response could be driven by uncertainties over whether the program would be permanent, or it could be the case that other policy changes during the height of the Great Recession muted any effects that the sparsity aid program had on spending. Our survey respondents confirmed that uncertainty surrounding sparsity aid was common in the early years of the program; 20 of our 36 survey respondents said they were unaware of the sparsity aid program when it was introduced, and another 8 believed it was unlikely the program would continue into the future. Nevertheless, beginning with the 2010 program expansion, we see increases in spending per member that align with the size of sparsity payments.

Table 2 summarizes these effects using the difference-in-differences specification from equation (1). Column (1) presents estimates only controlling for log membership, while column (2) adds demographic controls, column (3) interacts the year FEs with twelve different school district region indicators, and column (4) controls for whether a district receives additional transportation funding. The results are similar across specifications, indicating that demographic changes, transportation funding changes, nor regional trends are driving our results. In the most saturated specification in column (4), we find that receiving sparsity aid increases non-formula state revenue by $217 per member (a 26.6% increase), total revenues by $252 per member (a 1.9% increase), and current spending on elementary and secondary education by $226 (a 2% increase). Given that the average sparsity-eligible district enrolled 434 members following the sparsity aid program's implementation, these increases translate to additional funding of approximately $109,000 per year and additional spending of $98,000 per year, more than twice the average teacher salary of sparsity-eligible districts during this time period.

We now conduct several robustness checks of these results. First, in Appendix Table A.1, we conduct placebo tests to further verify that these increases in revenue and spending are not driven by changes to other revenue sources or expenditures. In Panel A, we repeat our difference-in-differences specifications for per-member revenues from all sources *other than* non-formula state aid, which includes local property tax revenues and appropriations from the state general aid formula. Across specifications, we find no evidence that revenues from other sources increased differentially in sparsity districts, relative to non-sparsity districts, indicating that the sparsity aid program alone was responsible for increasing districts' revenues. In Panel B, we further consider district spending in all areas other than elementary & secondary education, including capital outlays, community and adult education programs, payments to other government entities, and debt interest payments. These expenditures are unlikely to be financed with sparsity aid dollars and, again, we do not find that spending in other areas changed in sparsity districts following the implementation of the sparsity aid program, further bolstering our claim that the increases in spending we document above are the result of the sparsity aid program.

Appendix Figure A.8 further verifies this finding by plotting the difference-in-differences coefficients for all 35 revenue sources included in the CCD dataset. While we see some substitution between state general aid revenues and local revenues —which is consistent with declining membership and increased per-member property tax revenue we document in Section 3.2 —the increase in total revenues is primarily driven by state revenue for other programs, which contains sparsity aid payments. In Appendix Figure A.9 we show that our total revenue and current spending event study estimates hardly change if we include additional controls for a district's total property value per member or local property tax revenue per member. Moreover, Appendix Figure A.10 further shows that our difference-in-differences estimates for total revenues and current spending are robust to controlling for districts' per-member revenue from local, state formula, or federal sources. Thus, the increased revenue and spending in sparsity-eligible districts appears to come from the sparsity aid program and not other changes in revenue sources during the timeframe of the data.

Finally, we provide evidence that our estimates are not driven by our sample selection criteria. Recall that our preferred sample removes districts that were in the top 30% of the enrollment or density distribution prior to the start of the sparsity aid program in 2008. In Appendix Figure A.7, we repeat the estimation of β from column (4) in Table 2, varying the percentage of districts dropped from 0% (meaning we do not drop any districts based on their pre-2008 enrollment or density) to 50% (meaning we drop all districts in the top half of the density or enrollment distributions). Across our four outcomes, the estimates vary only slightly across different sample definitions and in no case are statistically different from our preferred specification.

4.2 Allocation of Spending

To understand how districts allocate the additional resources provided by the sparsity aid program, we rely on annual financial reports submitted by Wisconsin districts to the DPI, which summarize all transactions occurring in a district in a fiscal year.[11] Specifically, we consider spending in eight distinct areas that the DPI tracks: (1) general instruction in core curricular areas, (2) instruction in all other curriculum areas (e.g., physical education and co-curricular activities), (3) pupil support (e.g., health, guidance), (4) instructional staff support (e.g., curriculum development, training), (5) administration, (6) transportation, (7) food service operations, and (8) general operations (e.g., maintenance, fiscal services).[12]

[11] These annual reports are available publicly on the DPI website: `https://dpi.wi.gov/sfs/reporting/safr/annual/data-download`.

[12] Consistent with measures of current elementary and secondary spending in the NCES Common Core of Data (CCD), we exclude capital outlays, debt service payments, inter-fund transfers, and the purchase of investment assets from our spending measures. We also exclude payments to other governmental entities, except for payments to other Wisconsin public school districts for special education services. The correlation between

Panel A of Table 3 repeats the specification from column (4) in Table 2 for each of the above spending categories. We find that districts allocate \$56.96 of their additional spending to general instruction, \$56.71 to general operations, \$45.01 to administration, and \$23.46 to food service.[13] The latter three estimates are statistically significant at the 5% level and these categories receive the largest relative increases, with the estimates each representing 4-6% of sparsity-eligible districts' pre-program means. Panels B and C separate the changes in spending in each category into spending on employee salaries and benefits and all non-personnel spending. Our results indicate that districts increase salary and benefit spending related to general instruction, administration, and general operations categories, whereas they increase non-personnel spending related to other other instruction, student transportation, and food service categories.

These results indicate that districts do not use sparsity aid funds for a single use; instead, they allocate the increased funding to a wide variety of areas related to instruction, administration, and operation. Our survey data further confirm this finding. 24 of 39 respondents said the sparsity dollars were rarely or never set aside for a specific purpose, and only 4 respondents said they were always earmarked for a specific purpose. Because districts are unrestricted in how they spend sparsity aid dollars, the average effects presented in Table 3 may mask important differences in how different districts use the funds. To explore heterogeneous effects, we augment our main difference-in-difference specification with an interaction term that allows the effect of receiving sparsity aid to vary based on a district's average budget share in a given spending category in the pre-program period (2003-2007). We define these budget shares as the total spending in a category divided by a district's total revenue, and standardize them to have a mean of 0. We scale our effects such that the coefficients represent the change in the sparsity receipt effect per 1pp increase in pre-period budget share.

Table 4 presents these results and uses the linear interaction terms to estimate how spending effects vary across the budget share distribution of each spending category. For the categories of general instruction, other instruction, pupil support, instructional staff support, and food service,

per-member revenues in the CCD and Wisconsin annual report data is 0.992 and, for expenditures, it is 0.982.

[13] It is possible that this increase in food service spending could be driven in part by changes to the federal nutrition program following the passage of the Healthy, Hunger-Free Kids Act of 2010. However, in Appendix Figure A.11, we show that our estimated effects of the sparsity aid program on spending allocation are robust to controlling for districts' per-member revenues from local, state formula, or federal sources.

we estimate negative and statistically significant interaction terms which indicate that districts' spending allocations differ based on their baseline budget shares. Specifically, districts increase their spending more when they have a low baseline budget share in a given category. For example, districts at the 25th percentile of the general instruction budget share distribution increase spending on undifferentiated curriculum by a statistically significant $109.40, whereas districts at the 75th percentile do not increase their spending in this category. In addition, while there are no effects on spending on instructional or pupil support services on average, districts at the bottom of the budget share distributions for these categories increase their spending by statistically significant and economically meaningful amounts. An opposite pattern emerges for student transportation spending, where the interaction term is positive and statistically significant, indicating that districts with *high* baseline transportation budget shares further increase their transportation spending when they receive sparsity aid funds. As a whole, these heterogeneous effects provide evidence that the unrestricted nature of Wisconsin's sparsity aid program allows districts to allocate funds toward areas that were relatively underfunded prior to the program's introduction.

4.3 District Staffing

In Table 3, we see that receiving sparsity aid increases spending on salary and employee benefits, particularly in the administration category. Specifically, we find that districts increase administrative personnel spending by $41.26 per student, or approximately $18,000 for the average-sized district. To better understand how this spending increase affects the number and type of staff employed by sparsity-eligible districts, we estimate our difference-in-differences and event study specifications for a variety of staffing outcomes. In interpreting these results, it is important to note that sparsity-eligible districts frequently employ less than one full FTE in administrative positions and/or employ a single staff member across multiple positions. For example, prior to 2007, 35.7% of sparsity-eligible districts employed less than 1 FTE prinicipal and 91.2% employed less than 1 FTE in other administrative positions, such as curriculum and special education directors.

Table 5 presents difference-in-difference results for measures of administrative staff. Panel A presents our main results, while Panel B adds an interaction term with districts' standardized baseline administrative budget share, analogous to the specifications in Table 4. In column (1), we find that receiving sparsity aid increases total district administrators —inclusive of superintendents and assistant superintendents, principals and assistant principals, and other directors and coordinators —by 0.021 per 100 students, or 9.1% of an FTE for the average-sized district. This effect is equal to approximately 4.5% of baseline staffing and, in Panel B, we show that it is larger in districts with low baseline administrative spending: districts at the 25th percentile of the baseline administrative budget share distribution increase staffing by 0.036 FTEs per 100 students (15.6% of an FTE for the average-sized district), while districts at the 75th percentile do not meaningfully increase their administrative staffing.

Columns (2) through (6) of Table 5 estimate the effects of sparsity aid receipt on staffing increases across different administrative positions. In column (2) we find that sparsity aid receipt has little effect on the number of superintendents and assistant superintendents per 100 students. This finding is not surprising as, at baseline, over 98% of sparsity-eligible districts report employing a superintendent and only 1.1% of districts report having more than one. Thus, it is unlikely that districts used sparsity aid funds to hire additional superintendent positions.

In columns (4) through (7) we consider sparsity aid effects on principal and other administrator staffing. For each, we consider both the likelihood that a district reports employing non-zero FTEs and the total number of FTEs per 100 students. For principals, we find a statistically insignificant but positive effect on the likelihood that districts employ a principal (which 91.2% report at baseline), as well as on the number of principals per 100 students. Consistent with our spending patterns in Table 3, these effects are larger for districts with low baseline administrative spending. Districts at the 25th percentile of the baseline administrative budget share distribution are 4.8pp more likely to employ non-zero principal FTEs and employ 0.021 more FTEs per 100 students after receiving sparsity aid. The latter estimated effect is statistically significant at the 5% level and represents an increase of 8.9% of the baseline mean.

We find even larger effects for other administrative positions. In column (6), we find that receiving sparsity aid funds increases the likelihood that a district employs non-zero FTEs in other administrative positions by 14.9pp. This effect is highly statistically significant and represents an effect size of over 50% of the baseline mean. This effect is slightly larger for districts with lower baseline administrative spending but remains statistically significant at the 75th percentile of the baseline distribution. In column (7), we find an increase of 0.013 FTEs per 100 students, which is less precise, but similarly large, representing 38% of the baseline mean. While we lack the precision to estimate these effects across more granular positions, the types of administrative staff that sparsity districts are most likely to employ in the post-2008 period include special education directors, business managers, and instruction/curriculum coordinators. Thus, it is reasonable to expect that sparsity aid funds allowed districts to hire staff in these positions.

Taken together, our results indicate that sparsity aid funding allowed districts —particularly those with low baseline administrative spending —to hire more administrative staffing FTEs. In Appendix Figure A.13, we present event study estimates of all outcomes in Table 5, none of which indicate that the results are driven by differential pre-trends between sparsity and non-sparsity districts. Additionally in the appendix, we consider teacher-related staffing outcomes in Table A.2 and Figure A.12. Consistent with our imprecise and relatively small effect on teacher-related salary and benefit spending in Table 3, we find little effect of receiving sparsity aid funds on teacher staffing levels, salaries, benefits, or average experience. These effects further help us rule out that Wisconsin Act 10 differentially affected sparsity and non-sparsity districts and, thus, is unlikely to be driving our results.

5 Effects of Increased Spending on Student Outcomes

To estimate the impact of increased spending from the sparsity aid program on student outcomes, we rely on student-level administrative records from the Wisconsin DPI. We limit our sample to students in grades 3-12 who have non-missing demographic information and who continuously enroll in Wisconsin public schools. These restrictions produce a final sample of 308,630 unique students and 1,484,856 unique student-year observations, of which approximately one-quarter are enrolled in sparsity-eligible districts and three-quarters are not.

5.1 K-12 Outcomes

Table 6 presents estimates of β in equation (3) for student test score outcomes. Because of changes in Wisconsin's testing regime over time, our analysis is limited to math and reading test scores among students in grades 3-8 and grade 10 from 2005 to 2013, along with science, social studies, and writing scores for students in grades 4, 8, and 10 in the same years. We standardize all test scores at the year, grade, and subject level across the universe of test-takers to have a mean of zero and a standard deviation of one. Thus, our β estimates can be interpreted as the percent of a standard deviation change due to the sparsity aid program. We estimate effects for each grade level and test subject, as well as average effects across grade levels and across test subjects.

Panels A through E present our estimated effects for each test subject. For reading, science, social studies, and writing exams, we estimate small and statistically insignificant effects across all grade levels. In addition, our average effects across grade levels are close to zero and not statistically significant. However, for math exams, we estimate consistently negative coefficients and our average effect is statistically different from zero at the 10% level. Panel F then presents average effects across all test subjects taken by a grade level, and across all grades and test subjects. For each grade level, our effects are small and statistically insignificant at conventional levels. The point estimates are particularly small and close to zero for grades 7, 8, and 10. Overall, we do not detect a statistically significant effect on test scores and, with 95% confidence, can rule out that the sparsity aid program increased test scores by more than 0.75% of a standard deviation.

In Appendix Figure A.14 we present analogous event study estimates for each test subject to test whether our effects are driven by existing differential trends in test scores between students in sparsity-eligible and ineligible districts. While the pre-trends are generally flat, the availability of only three pre-treatment periods limits our ability to determine whether the negative effects we estimate —particularly for math test scores —may be due to a longer-run decline in performance in sparsity districts. To further assess the role of pre-trends in our estimates, we obtain a longer panel of school-level test score data for grades 4, 8, and 10 from the Wisconsin DPI. Appendix Figure A.15 plots the average school-level test scores for these grades for schools in sparsity-eligible and ineligible districts from 2002 to 2013 and Appendix Figure A.16 estimates event study specifications over this time frame. While the estimates are quite noisy, we do not see strong evidence of declining math scores prior to treatment years. In addition, we see little evidence of changes in reading test scores for sparsity districts before or after the sparsity aid program began, which aligns with our small, statistically insignificant results in Table 6.

Because of the unrestricted nature of the sparsity aid program and the documented heterogeneous effects across spending categories in Table 4, we also test whether districts with different baseline budget shares —who were induced to allocate funds to different spending areas —experience different test score effects following the introduction of the sparsity aid program. Appendix Table A.4 repeats the specifications from column (8) of Table 6 for reading, math, and average test scores, adding interaction terms between the sparsity receipt dummy variable and a district's standardized baseline budget share in the eight spending categories. Across all specifications, we find no evidence that our test score effects differ based on baseline budget shares. That is, districts that were induced by the sparsity aid program to spend more on different budget areas (e.g., instruction vs. administration) did not experience larger or smaller effects on standardized test scores.

Taken together, these results indicate that, overall, the additional funding from the sparsity aid program had minimal effects on student test scores and we can rule out sizable positive effects. Stated differently, we find additional funding for rural schools in the aftermath of the Great Recession was not able to improve student achievement. However, this finding does not rule out the possibility that the increased funding impacts students' non-cognitive and/or behavioral outcomes. In Appendix Table A.5 we consider how sparsity aid funding affects a variety of non-cognitive and/or behavioral outcomes, including students' annual attendance rate, the likelihood they are involved in a disciplinary incident, the likelihood they repeat a grade level, and, for 10th-12th graders, the likelihood they dual-enroll in a college course while enrolled in a Wisconsin public high school.[14] Across the different outcomes and grade levels, we estimate precise null effects. Appendix Figure A.17 presents event study estimates of these outcomes. The estimates are noisy but do not suggest that the main results are driven by differential pre-trends between sparsity-eligible and ineligible districts. As such, we interpret our findings as indicating that the sparsity aid program also had

[14] We consider outcomes for grades 6-12 in this table, as there is little variation in attendance and disciplinary incidence in elementary grades.

little effect on students' behavior in schools.

5.2 Postsecondary Enrollment & Completion

We now turn to estimating how exposure to the sparsity aid program affects students' longer-run educational attainment by estimating effects on both postsecondary enrollment and completion. Table 7 presents estimates of β from equation (3) on the sample of all seniors in our sample from 2005 to 2017. We estimate effects for the entire sample of students and for students with below-median or above-median likelihoods of enrolling in or completing college. To do so, we first estimate logit models of college enrollment and completion for the 2005-2007 high school cohorts (prior to the introduction of the sparsity aid program), predicting enrollment based on a student's race, gender, FRL status, LEP status, special education status, and the high school in which they were enrolled.[15] We then use these models' coefficients to predict the probability that students in the 2008 cohort and onwards will enroll in and complete college and subset students based on whether their predicted probabilities are below or above the median for enrollment and for completion.

Panel A of Table 7 presents our estimated effects for the full sample of students. Columns (1) to (3) show effects for enrollment in college within 12 months of high school graduation.[16] Overall, we estimate that the sparsity aid program increased college enrollment by 0.3pp, with a larger effect in two-year colleges (0.6pp) than in four-year colleges. However, these effects are not statistically different from zero. Columns (4) to (6) then estimate the effects for college completion at any point in our data's timeframe. We similarly see about a 0.7pp increase in college completion, which is again not statistically different than zero.[17]

Panels B and C then estimate separate effects for students with below- and above-median likelihoods of enrolling in and graduating from college. In Panel B, we see large, positive, and statistically significant effects of the sparsity aid program on college enrollment and completion for

[15] Appendix Table A.6 provides the estimated marginal effects from these logit specifications. White, female, non-FRL, non-LEP, and students who do not qualify for special education services are more likely to enroll in and complete postsecondary education.

[16] Due to changes in how Wisconsin reported high school graduation data during the time period of our analysis, we only observe high school graduation dates for students who enroll in college and are not able to consider graduation as an outcome directly.

[17] Because later cohorts in our data have had fewer years to enroll in and complete college, we also estimate effects only on the 2005-2013 cohorts. Appendix Table A.7 presents these results, which are very similar to our main results.

students with low baseline probabilities of doing so. Specifically, we estimate that the sparsity aid program increased enrollment by 2.5pp (5.9% of the mean), which is driven by a 2.1pp (10% of the mean) increase in enrollment in two-year colleges, such as community colleges.[18] Both of these estimates are statistically significant at the 5% level. We further estimate a 1.5pp increase (6.4% of mean) in college completion for this subsample, which is fully explained by a statistically significant 1.6pp increase in two-year college completion. Given the sizable returns to two-year college completion (Belfield and Bailey, 2017), particularly for students who otherwise would not have attended any postsecondary education (Mountjoy, 2022), these effects are likely to translate into improved future labor market outcomes for students as a result of the sparsity aid program. In contrast, in Panel C, we see that the sparsity aid program had little effect on the college enrollment and completion outcomes for students with above-median likelihoods of enrolling in and completing college.

Figures 4 and 5 further show these heterogeneous effects by presenting event study specifications for enrollment and completion outcomes for each subgroup of students. For students with low propensities to enroll in and/or complete college, we see generally flat pre-treatment trends, with clear increases in enrollment probabilities for sparsity districts after 2008. These effects are most pronounced for two-year college enrollment and completion. However, for students with high postsecondary propensities, we see treatment effects close to zero, with some evidence that the negative coefficients in Table 7 are driven by pre-existing downward trends. As such, we interpret these negative estimates with caution.

Analogously to our heterogeneity analysis for standardized test scores in Section 5.1, we also test whether our postsecondary enrollment and completion effects differ across districts' baseline budget shares. Appendix Table A.8 repeats the specifications from columns (1) and (3) of Table 7 for all students and for students with a below-median propensity to enroll in or complete college. We add interaction effects between the sparsity receipt dummy variable and districts' standardized baseline budget shares across eight budget categories. The interaction effects in columns (1) and (2) suggest that the effects are somewhat larger for districts with *lower* baseline budget shares for

[18] Over 91% of students in our sample who enroll in two-year colleges enroll in public institutions in Wisconsin, i.e., community and technical colleges.

instruction categories, which is consistent with the sparsity aid program inducing these districts to allocate more funds towards instruction. Similarly, we see larger effects for districts at the bottom of the administrative budget share distribution at baseline, who we show in Tables 4 and 5 increase their spending on administration and hire additional administrator FTEs. Taken together, these findings suggest that increased spending across core budget areas may generate improvements in rural students' long-run educational attainment outcomes.

6 Conclusion

Rural schools and school districts in the United States face a myriad of distinct challenges compared to their urban and suburban peers, including high per-pupil costs in areas like transportation and frequent staffing turnover. However, the extent to which such challenges may be overcome with additional unrestricted state funding is not well-established in prior work. We provide new evidence on the returns to school funding in rural districts by exploiting policy variation from Wisconsin's sparsity aid program —one of the largest state-level funding streams geared towards rural school districts. We find that the introduction and subsequent expansion of the program increased school spending by about 2% annually and that districts used the funds in a variety of ways. In particular, we find that districts tend to allocate funds to areas with low baseline budget shares, suggesting that the unrestricted nature of the funding program allowed administrators to target spending to areas that were relatively underfunded prior to the introduction of sparsity aid.

Our results indicate this increased spending did not meaningfully improve student test scores nor change student behavior in schools, but it modestly increased postsecondary enrollment and completion for students with low baseline probabilities of enrolling in or completing college. Specifically, we estimate that exposure to funding from the sparsity aid program increased college enrollment by 2.5 (5.9%) for students with below-median propensity to enroll in college and increased college completion by 1.5pp (6.4%) for students with below-median propensity to complete college. These effects are driven by increased enrollment and completion within the two-year college sector.

These findings have important policy implications for funding programs targeted at rural school districts across the United States. First, our results indicate that rural school districts use additional funding in a variety of ways and are able to supplement areas with low baseline budget shares. Given the heterogeneity in rural districts' size, sparsity, and needs, this flexibility appears important for helping districts meet their unique challenges and suggests that policies that direct districts' use of funds —such as those that provide rural schools with additional funding for transportation or specific capital improvements —may yield different results.

Second, however, we find that this approach to increased funding does not yield increases in student test scores. Thus, if policymakers wish to improve rural achievement, our findings suggest the need for additional interventions to achieve this goal. Finally, our findings indicate that modest increases in school funding —even if they do not improve student test scores —can lead to increases in college-going and educational attainment in rural settings, particularly in two-year colleges and particularly for students who are historically less likely to enter and persist in higher education. Future quantitative and qualitative work that explores the mechanisms behind these positive longer-run effects would be a valuable contribution to the literature and could further inform future rural school finance initiatives.

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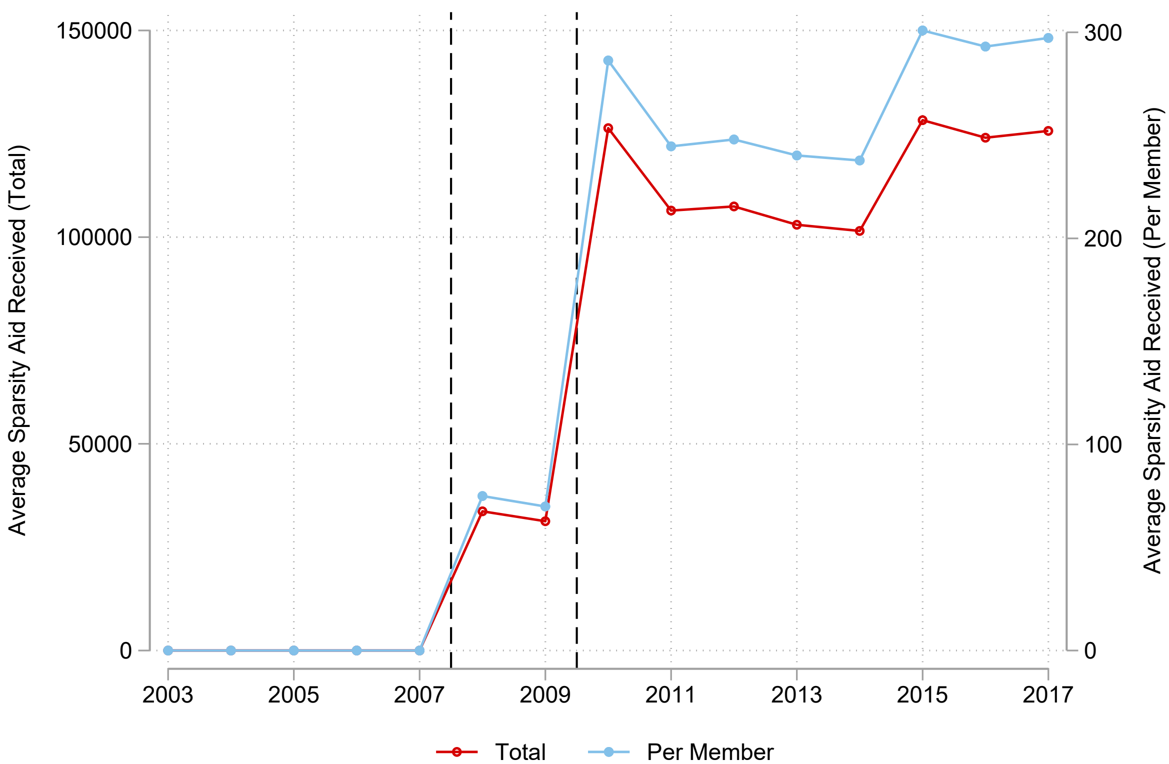
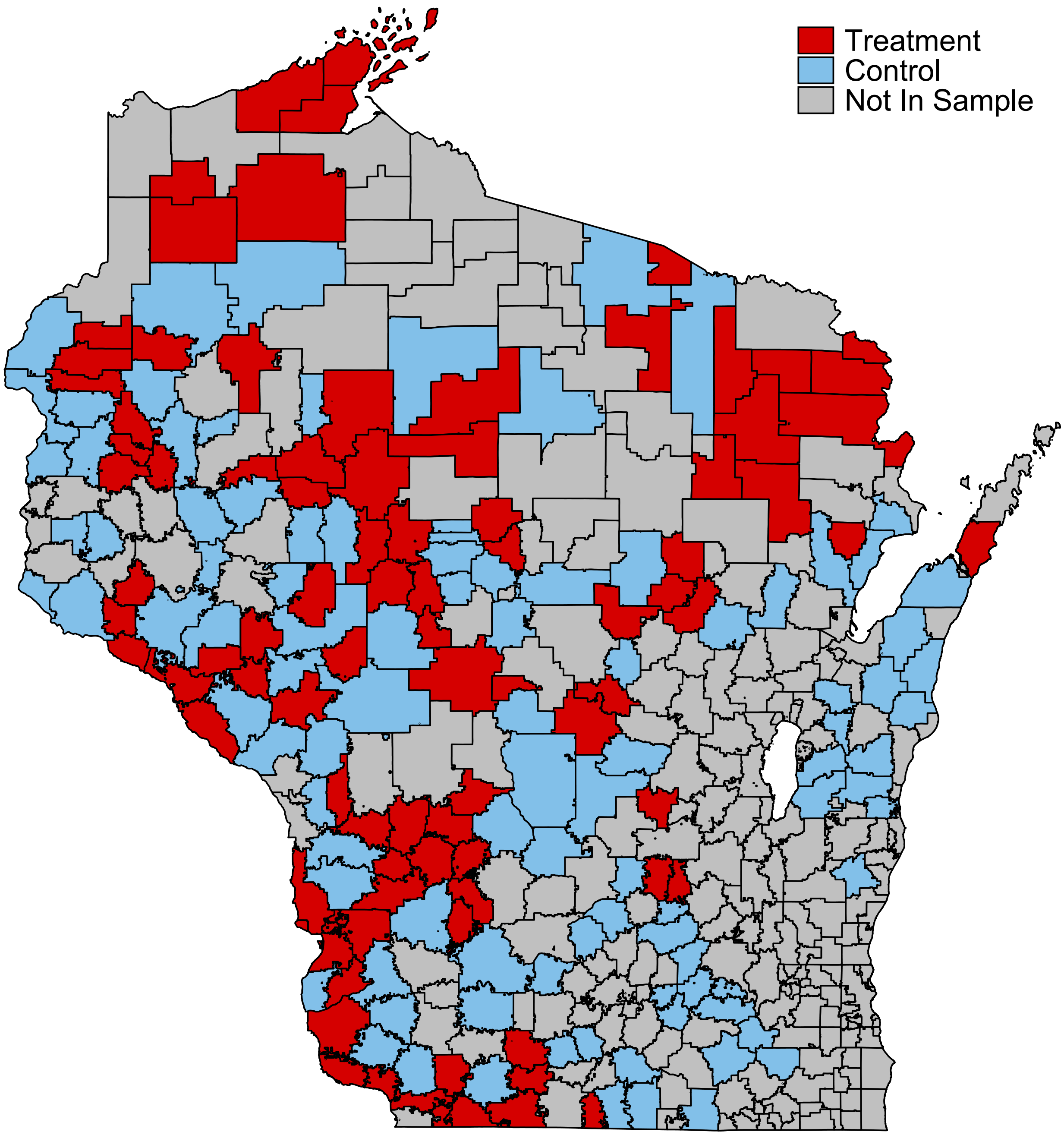
Figure 1: Introduction & Expansion of Sparsity Aid Program



Notes: This figure shows the average sparsity aid funding received each year, both in total and per member, among districts in our sample that are always eligible for sparsity aid funding from 2008 to 2017.

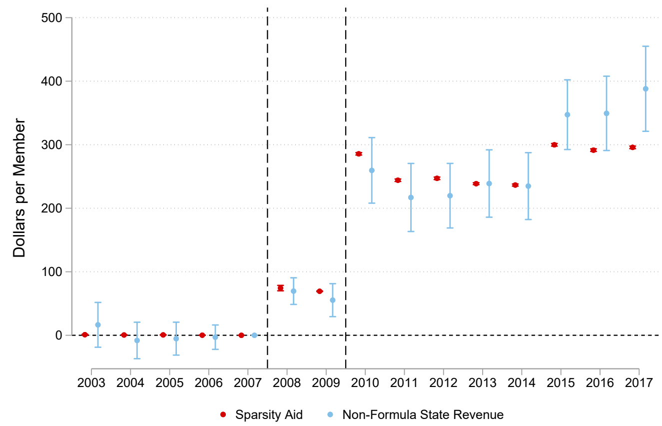
Figure 2: School Districts in Analytic Sample



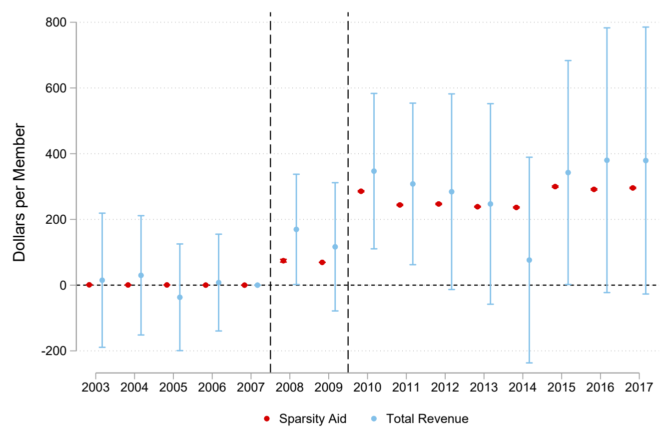
Notes: This figure shows the sparsity-eligible (treatment) and ineligible (comparison) districts in our sample.

Figure 3: Event Study Estimates of Sparsity Aid Program on District Finances

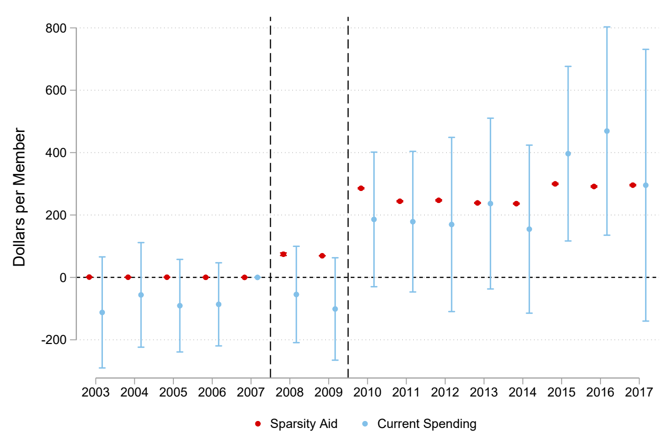
(a) Non-Formula Revenue from State



(b) Total Revenue



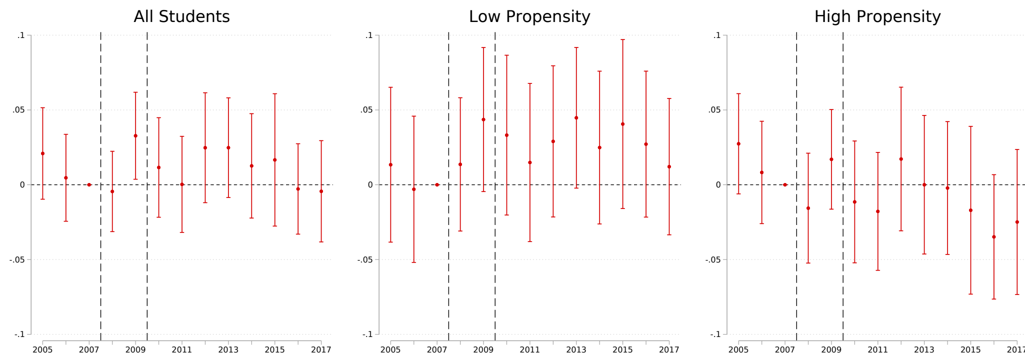
(c) Current Spending on Elementary & Secondary Education



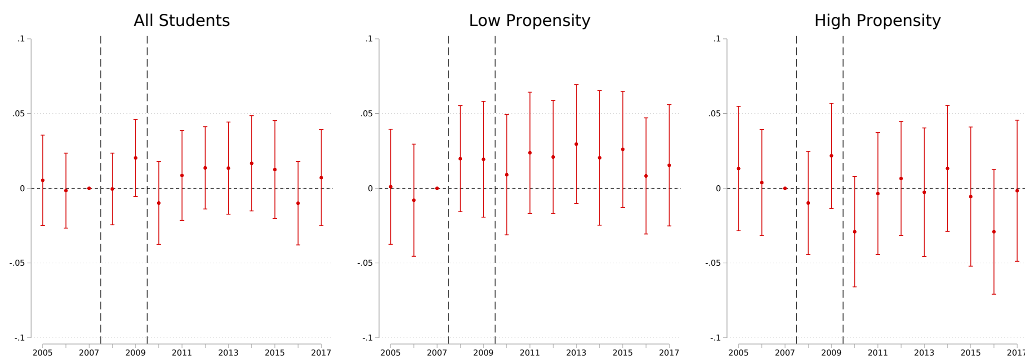
Notes: Each figure presents estimates of the β_k coefficients from equation (2). All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, and year-by-region (CESA) fixed effects. All standard errors are clustered at the school district level.

Figure 4: Event Study Estimates of Sparsity Aid Program on Postsecondary Enrollment

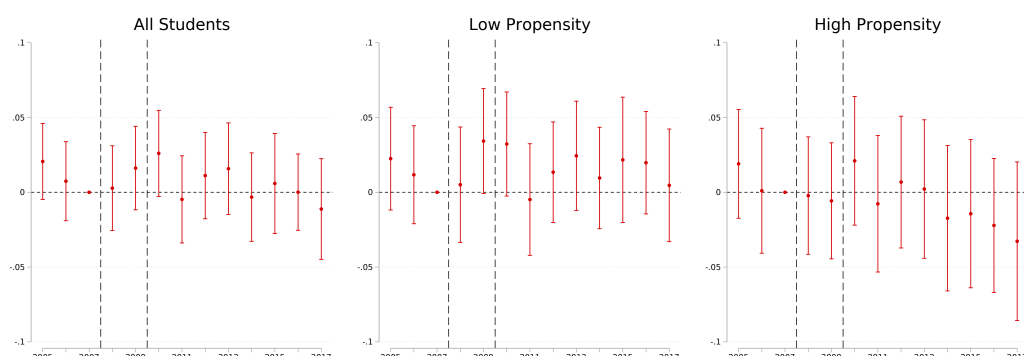
(a) Any College



(b) Two-Year College



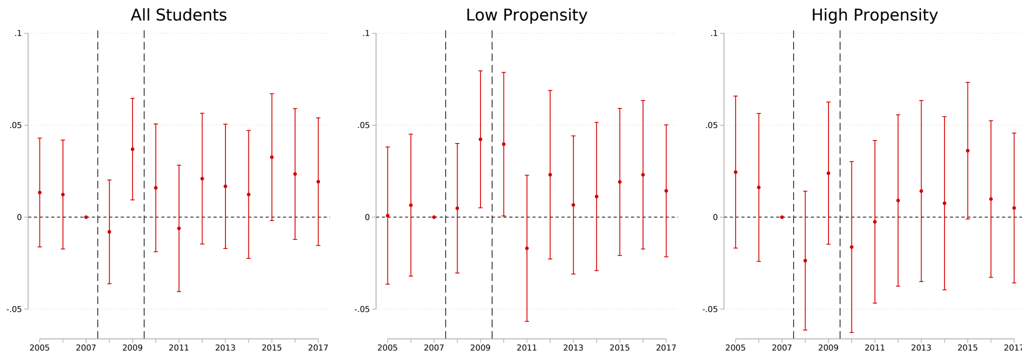
(c) Four-Year College



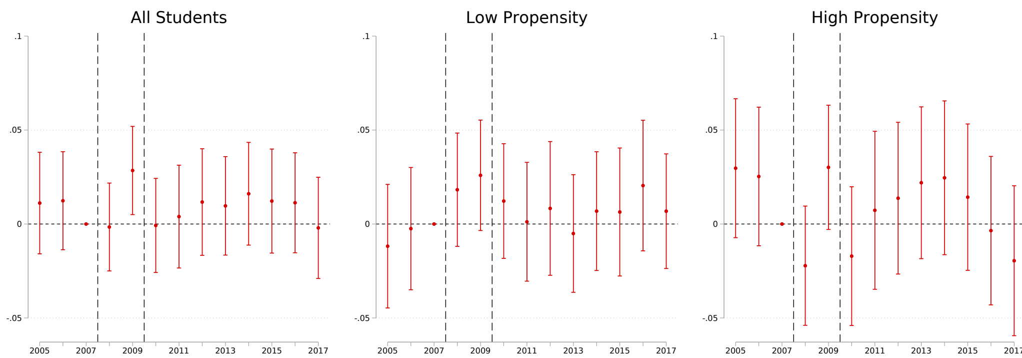
Notes: Each figure presents event study estimates for student-level outcomes. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level.

Figure 5: Event Study Estimates of Sparsity Aid Program on Postsecondary Completion

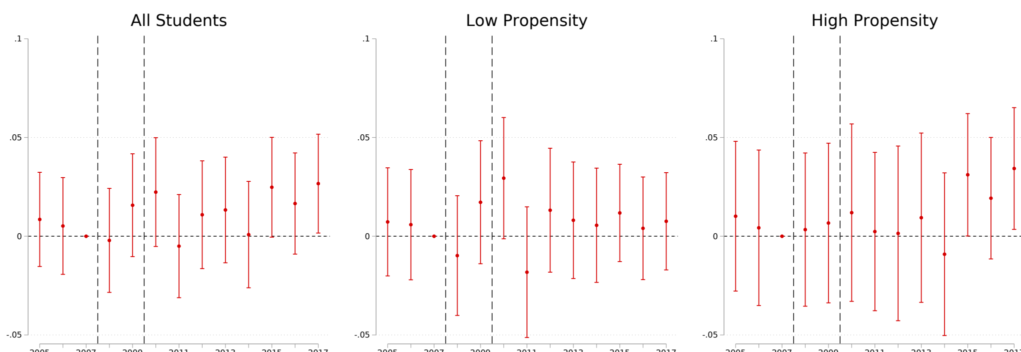
(a) Any College



(b) Two-Year College



(c) Four-Year College



Notes: Each figure presents event study estimates for student-level outcomes. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, and the local child poverty rate, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level.

Table 1: Baseline District Characteristics

	Analysis Sample			Wisconsin
	All	**Sparsity**	**Comparison**	**Average**
	(1)	**(2)**	**(3)**	**(4)**
Panel A. Size & Location				
Membership	875.8	480.1	1,231	2,035
Membership per Square Mile	6.707	3.809	9.312	45.94
Number of Schools	3.252	2.544	3.889	4.899
Avg. Membership per School	270.4	201.1	332.9	355.4
NCES: Rural	0.835	0.978	0.707	0.580
NCES: Town	0.160	0.022	0.283	0.203
NCES: Suburb	0.005	0.000	0.010	0.147
NCES: City	0.000	0.000	0.000	0.070
Panel B. Demographics				
% White	0.940	0.940	0.940	0.887
% Black	0.008	0.006	0.010	0.045
% Hispanic	0.023	0.020	0.026	0.033
% Asian	0.007	0.006	0.008	0.015
% FRL	0.289	0.347	0.237	0.239
% Special Education	0.150	0.157	0.144	0.145
Local Child Poverty Rate	0.119	0.145	0.095	0.098
District House Price Index	181.9	163.0	198.9	207.6
Panel C. Finances				
Revenue per Member	11,776	12,502	11,122	11,789
% Local	0.422	0.436	0.409	0.473
% State Formula	0.477	0.452	0.500	0.433
% State Non-Formula	0.049	0.051	0.047	0.048
% Federal	0.052	0.062	0.044	0.046
Spending per Member	9,856	10,354	9,408	9,966
Panel D. Staffing				
Number of Teachers (FTE)	63.43	37.71	86.54	131.4
Teachers per 100 Members	7.510	8.027	7.045	7.324
Average Teacher Salary	41,928	40,396	43,305	44,087
Average Teacher Experience	15.99	16.03	15.95	15.48
Number of Administrators (FTE)	4.014	2.336	5.523	7.933
Administrators per 100 Members	0.472	0.496	0.450	0.467
Panel E. Educational Outcomes				
Math Proficiency Rate	0.406	0.393	0.418	0.439
Reading Proficiency Rate	0.335	0.324	0.346	0.361
College Enrollment Rate	0.529	0.518	0.538	0.542
College Completion Rate	0.396	0.386	0.405	0.403
Districts	188	89	99	468

Notes: Each column summarizes district-level characteristics over the 2003-2007 academic years. The test score and postsecondary outcomes are averaged over the 2005-2007 academic years. The college enrollment rate is defined as the share of high school seniors who enroll in a postsecondary instutition within one year of graduating from high school and the college completion rate is defined as the share of high school seniors in the 2005-2007 cohorts who completed a postsecondary credential within the timeframe of our data.

Table 2: Effect of Sparsity Aid on District Revenues & Spending

	(1)	(2)	(3)	(4)
Panel A. Sparsity aid dollars				
Received sparsity aid	223.6***	222.2***	221.6***	219.1***
	(1.9)	(2.2)	(2.3)	(2.5)
Observations	2,820	2,820	2,820	2,820
Panel B. Non-formula revenue from state				
Received sparsity aid	253.7***	259.7***	229.9***	217.1***
	(23.4)	(22.1)	(20.6)	(20.4)
	[31.1%]	[31.9%]	[28.2%]	[26.6%]
Observations	2,820	2,820	2,820	2,820
Panel C. Total revenue				
Received sparsity aid	261.6**	263.8**	257.6**	252.4**
	(114.4)	(104.6)	(118.9)	(118.6)
	[1.96%]	[1.97%]	[1.93%]	[1.89%]
Observations	2,820	2,820	2,820	2,820
Panel D. Current spending				
Received sparsity aid	301.5***	291.6***	247.7**	226.3**
	(103.1)	(94.6)	(99.4)	(101.6)
	[2.70%]	[2.61%]	[2.22%]	[2.03%]
Observations	2,820	2,820	2,820	2,820
Membership control	X	X	X	X
Demographic controls		X	X	X
Year-by-CESA FEs			X	X
Transportation funding control				X

Notes: Each coefficient is estimated from a separate regression and represents β in equation (1), the effect of receiving sparsity aid funding. Column (1) controls for a district's log membership, column (2) adds controls for a district's log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, and the local child poverty rate, column (3) adds year-by-region (CESA) fixed effects, and column (4) further controls for whether a district receives funding from the state's high-cost pupil transportation aid program. All standard errors are clustered at the district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Effect of Sparsity Aid on Spending Allocations

	General Instruc. (1)	Other Instruc. (2)	Pupil Supp. (3)	Instruc. Staff Supp. (4)	Admin (5)	Student Transp. (6)	Food Service (7)	General Ops. (8)
Panel A. Effects on Total Spending								
Received sparsity aid	56.96	24.08	-0.354	6.404	45.01**	5.772	23.46***	56.71**
	(50.71)	(35.01)	(14.25)	(18.24)	(18.94)	(9.290)	(8.453)	(26.686)
Observations	2,820	2,820	2,820	2,820	2,820	2,820	2,820	2,820
Baseline Mean	4185.3	1990.4	385.88	416.28	895.57	584.90	417.21	1282.27
Panel B. Effects on Salary & Employee Benefit Spending								
Received sparsity aid	75.12	-5.047	-11.99	2.976	41.26**	-16.42*	9.731	35.01*
	(48.25)	(34.79)	(12.12)	(14.17)	(16.26)	(9.871)	(7.382)	(18.09)
Observations	2,820	2,820	2,820	2,820	2,820	2,820	2,820	2,820
Baseline Mean	3981.4	1810.13	272.87	282.40	778.20	174.83	221.74	662.73
Panel C. Effects on Other Spending								
Received sparsity aid	-18.16	29.13***	11.64	3.428	3.747	22.19*	13.73*	21.70
	(12.27)	(10.75)	(8.914)	(8.052)	(7.012)	(13.27)	(8.126)	(21.85)
Observations	2,820	2,820	2,820	2,820	2,820	2,820	2,820	2,820
Baseline Mean	203.86	180.27	113.00	133.89	117.37	410.07	195.47	619.54

Notes: Each coefficient is estimated from a separate regression and represents β in equation (1), the effect of receiving sparsity aid funding.All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Heterogeneous Effects of Sparsity Aid on Spending Allocations

	General Instruc. (1)	**Other Instruc. (2)**	**Pupil Supp. (3)**	**Instruc. Staff Supp. (4)**	**Admin (5)**	**Student Transp. (6)**	**Food Service (7)**	**General Ops. (8)**
Received sparsity aid	43.605	17.34	-4.052	-7.235	54.56***	3.833	22.41***	60.12**
	(50.087)	(34.819)	(14.879)	(20.088)	(18.759)	(9.081)	(8.341)	(26.399)
Received sparsity aid x budget share	-28.640**	-23.37**	-36.03**	-34.65**	-27.15**	13.66**	-25.99**	19.53
	(11.192)	(10.149)	(16.193)	(15.332)	(13.321)	(6.901)	(12.888)	(13.598)
Baseline Mean Spending $	4185.3	1990.4	385.88	416.28	895.57	584.9	417.21	1282.27
Baseline Mean Share	0.342	0.163	0.032	0.037	0.068	0.044	0.033	0.104
Observations	2,820	2,820	2,820	2,820	2,820	2,820	2,820	2,820
Effect at 10th Percentile	198.3***	121.7**	37.41**	54.21**	91.15***	-14.4	36.96***	21.38
Effect at 25th Percentile	109.4**	64.52	26.48*	37.63*	66.06***	-4.62	31.65***	35.64
Effect at 50th Percentile	54.25	21.90	5.305	6.751	42.72**	5.813	23.01***	55.85**
Effect at 75th Percentile	-6.66	-5.96	-21.9	-21.4	21.60	17.20	14.96*	73.44**
Effect at 90th Percentile	-55.3	-54.7	-37.1	-42.4	4.132	34.12*	3.147	100.7**

Notes: The coefficients in each column are estimated from a separate regression and represent β in equation (1), the effect of receiving sparsity aid funding, with interaction effects based on districts' pre-2008 budget shares. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Effects of Sparsity Aid on Administrator Staffing

	Total Admin	**Superintendent**	**Principals**		**All Other**	
	FTEs per 100 Students	**FTEs per 100 Students**	**Non-Zero FTEs**	**FTEs per 100 Students**	**Non-Zero FTEs**	**FTEs per 100 Students**
	(1)	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Panel A. Main Specification						
Received sparsity aid	0.021*	-0.005	0.030	0.014	0.149***	0.013
	(0.012)	(0.006)	(0.020)	(0.010)	(0.046)	(0.008)
Baseline Mean	0.465	0.193	0.912	0.237	0.283	0.034
Observations	2,820	2,820	2,820	2,820	2,820	2,820
Panel B. Interaction with Baseline Admin Budget Share						
Received sparsity aid	0.028**	-0.003	0.038*	0.017*	0.152***	0.014*
	(0.012)	(0.006)	(0.023)	(0.009)	(0.046)	(0.008)
Received sparsity aid x budget share	-0.019**	-0.006	-0.023	-0.010*	-0.007	-0.003
	(0.008)	(0.004)	(0.017)	(0.006)	(0.023)	(0.004)
Baseline Mean	0.465	0.193	0.912	0.237	0.283	0.034
Observations	2,820	2,820	2,820	2,820	2,820	2,820
Effect at 25th Percentile	0.036***	-0.001	0.048*	0.021**	0.155***	0.015*
Effect at 75th Percentile	0.005	-0.010	0.010	0.005	0.143***	0.011

Notes: The coefficients in each column are estimated from a separate regression and represent β in equation (1), the effect of receiving sparsity aid funding, with interaction effects based on districts' pre-2008 budget shares. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Effect of Sparsity Aid on Standardized Test Scores

	3rd Grade (1)	4th Grade (2)	5th Grade (3)	6th Grade (4)	7th Grade (5)	8th Grade (6)	10th Grade (7)	All Grades (8)
Panel A. Reading								
Received sparsity aid	-0.010	-0.018	-0.020	-0.007	0.021	-0.022	-0.010	-0.009
	(0.024)	(0.021)	(0.021)	(0.020)	(0.019)	(0.020)	(0.020)	(0.012)
Observations	90,631	91,595	92,516	95,059	98,256	100,523	110,142	678,722
Mean	0.047	0.046	0.027	0.057	0.049	0.045	0.039	0.044
Panel B. Math								
Received sparsity aid	-0.015	-0.036	-0.034	-0.062**	-0.026	-0.017	-0.026	-0.030*
	(0.031)	(0.028)	(0.028)	(0.028)	(0.026)	(0.024)	(0.020)	(0.015)
Observations	90,896	91,720	92,612	95,125	98,328	100,574	110,164	679,419
Mean	0.016	0.013	-0.014	-0.006	0.011	0.040	0.046	0.016
Panel C. Science								
Received sparsity aid		0.007				-0.019	-0.008	-0.007
		(0.022)				(0.022)	(0.018)	(0.013)
Observations		91,757				100,556	110,096	302,409
Mean		0.079				0.086	0.085	0.083
Panel D. Social Studies								
Received sparsity aid		-0.014				-0.020	0.003	-0.010
		(0.022)				(0.021)	(0.019)	(0.013)
Observations		91,731				100,407	110,061	302,199
Mean		0.082				0.100	0.079	0.087
Panel E. Writing								
Received sparsity aid		-0.010				0.002	-0.012	-0.007
		(0.021)				(0.022)	(0.018)	(0.014)
Observations		91,607				100,457	109,935	301,999
Mean		0.032				0.025	0.022	0.026
Panel F. Average								
Received sparsity aid	-0.012	-0.015	-0.028	-0.035	-0.002	-0.015	-0.011	-0.016
	(0.025)	(0.020)	(0.022)	(0.022)	(0.020)	(0.019)	(0.016)	(0.012)
Observations	90,619	91,578	92,495	95,036	98,231	100,470	110,025	678,454
Mean	0.032	0.044	0.007	0.026	0.031	0.050	0.045	0.034

Notes: Each coefficient is estimated from a separate regression and represents β in equation (1), the effect of receiving sparsity aid funding on standardized test scores. Average test scores are calculated for students with non-missing math and reading scores. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Effect of Sparsity Aid on Postsecondary Enrollment & Completion

	Enrollment			**Completion**		
	Any	**Two-Year**	**Four-Year**	**Any**	**Two-Year**	**Four-Year**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Panel A. All Students						
Received sparsity aid	0.003	0.006	-0.002	0.007	0.001	0.007
	(0.009)	(0.008)	(0.007)	(0.009)	(0.006)	(0.007)
Observations	165,442	165,442	165,442	165,442	165,442	165,442
Mean	0.558	0.237	0.340	0.345	0.161	0.209
Panel B. Below-Median Propensity Students						
Received sparsity aid	0.025**	0.021**	0.005	0.015	0.016**	0.002
	(0.012)	(0.010)	(0.008)	(0.009)	(0.008)	(0.007)
Observations	82,740	82,740	82,740	82,705	82,705	82,705
Mean	0.422	0.210	0.222	0.236	0.125	0.123
Panel C. Above-Median Propensity Students						
Received sparsity aid	-0.019*	-0.009	-0.012	-0.009	-0.014	0.005
	(0.012)	(0.011)	(0.010)	(0.012)	(0.009)	(0.010)
Observations	82,700	82,700	82,700	82,735	82,735	82,735
Mean	0.695	0.265	0.459	0.455	0.197	0.294

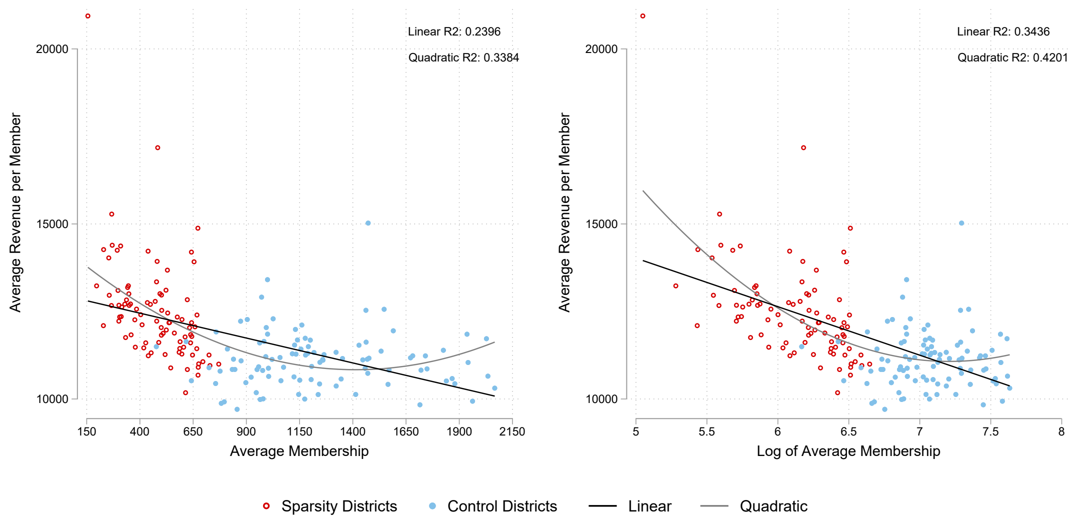
Notes: Each coefficient is estimated from a separate regression and represents β in equation (3), the effect of receiving sparsity aid funding. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Online Appendix: Not for Publication

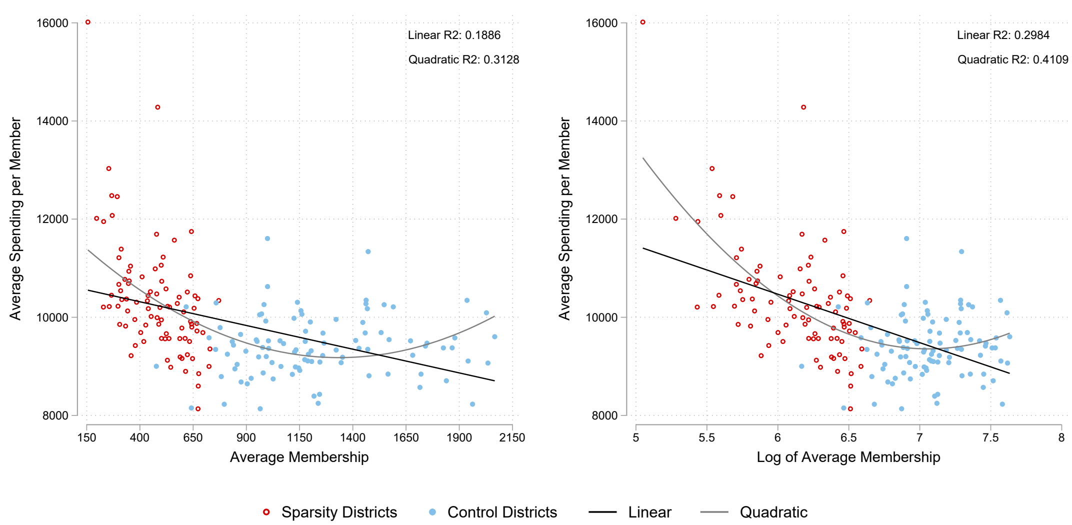
A Additional Figures & Tables

Figure A.1: Relationship Between District Size & Finances, 2003-2007

(a) Total Revenue



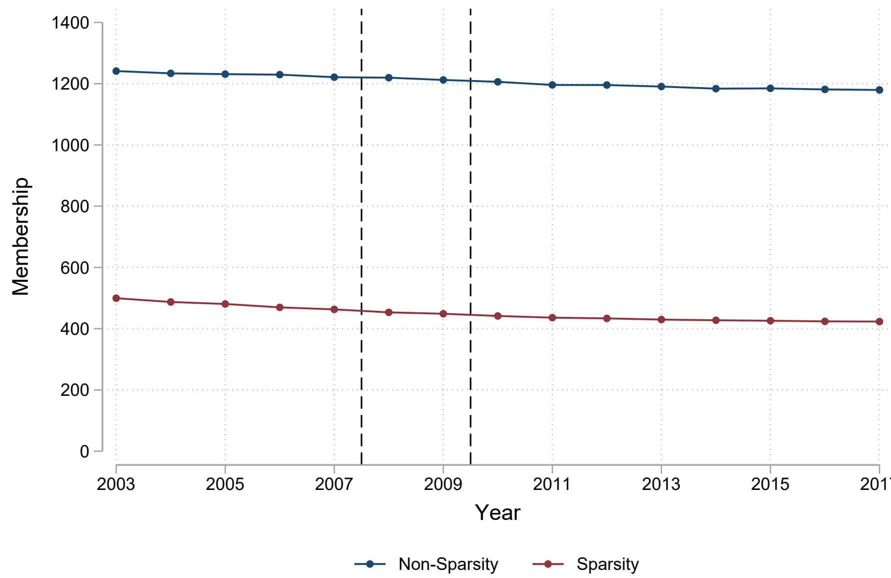
(b) Spending on Elementary & Secondary Education



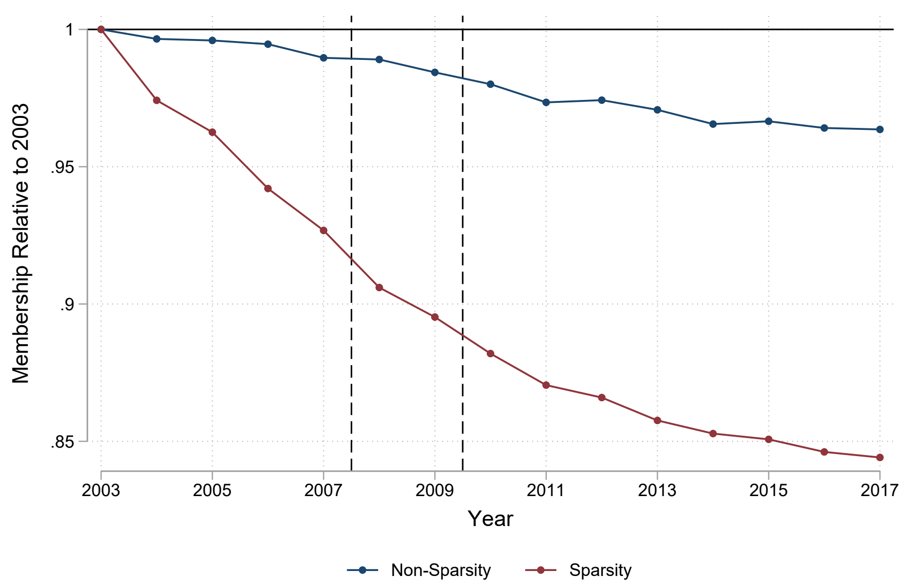
Notes: Each figure presents the relationship between districts' average resources per member and average membership in academic years 2003-2007. Districts that become eligible for the sparsity aid program in 2008 are shaded red, while districts that are never eligible for the sparsity aid program are shaded blue.

Figure A.2: Membership in Sparsity-Eligible & Ineligible Districts, 2003-2017

(a) Membership



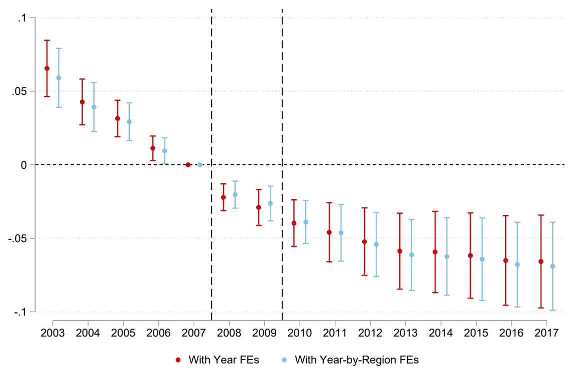
(b) Membership Relative to 2003



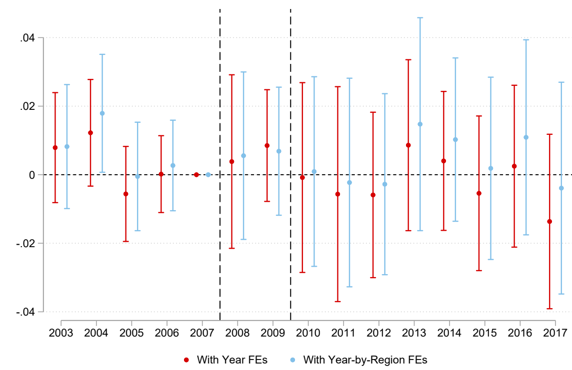
Notes: Panel A plots the average membership in sparsity-eligible and ineligible districts across academic years 2003-2017. Panel B repeats this plot measuring districts' membership relative to 2003.

Figure A.3: Event Study Estimates of Sparsity Aid Program on District Demographics

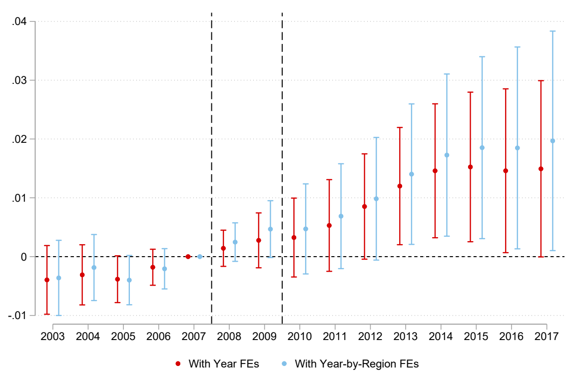
(a) Log Membership



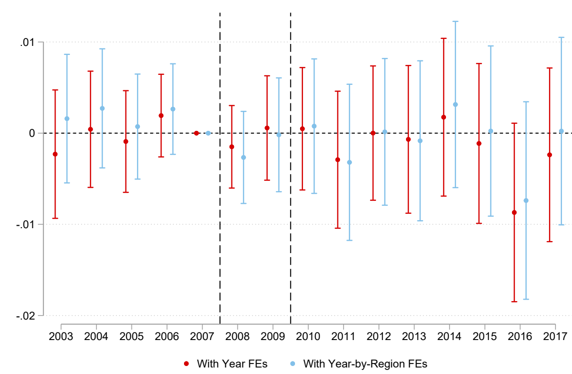
(b) % FRPL



(c) % White



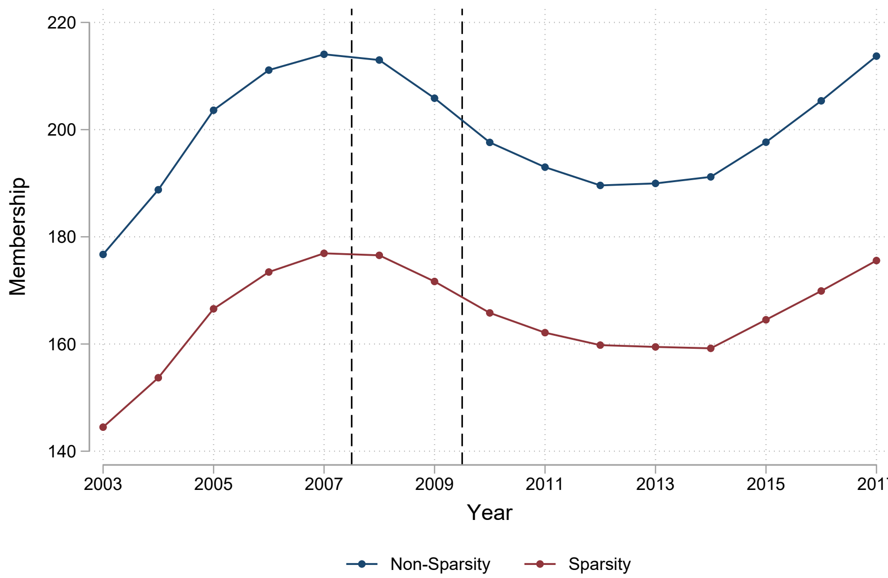
(d) % Special Education



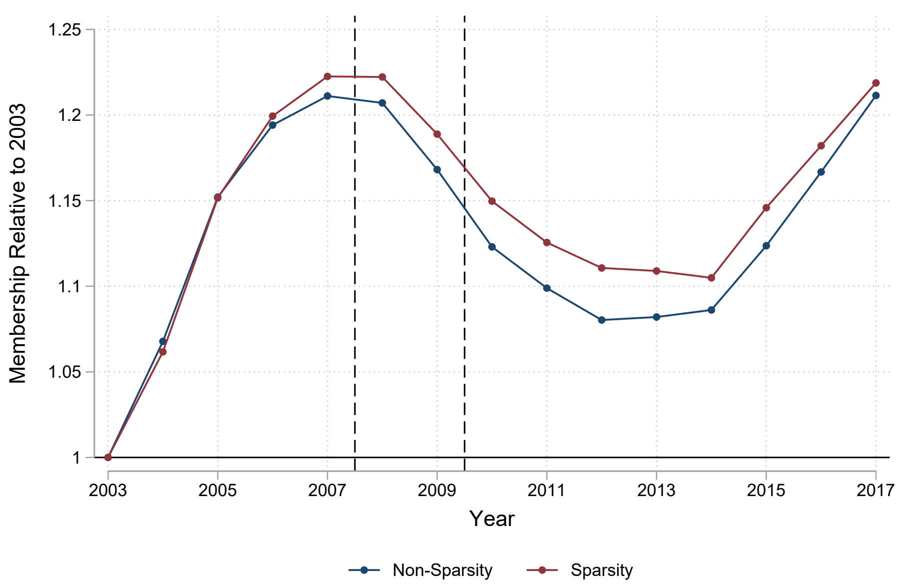
Notes: Each figure presents estimates of the β_k coefficients from equation (2), including either district and year FEs or district and year-by-region FEs. All standard errors are clustered at the school district level.

Figure A.4: House Price Index in sparsity-eligible & Ineligible Districts, 2003-2017

(a) District-Level House Price Index



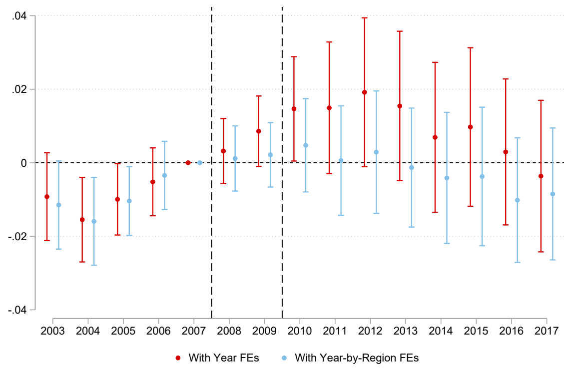
(b) District-Level House Price Index Relative to 2003



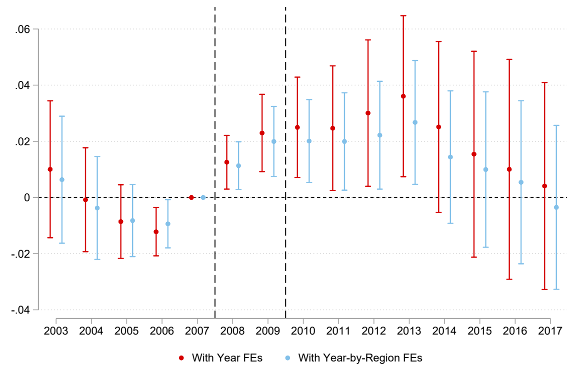
Notes: Panel A plots the average district-level house price index in sparsity-eligible and ineligible districts across academic years 2003-2017. Panel B repeats this plot measuring districts' house price indices relative to 2003.

Figure A.5: Event Study Estimates of Sparsity Aid Program on Local Resources

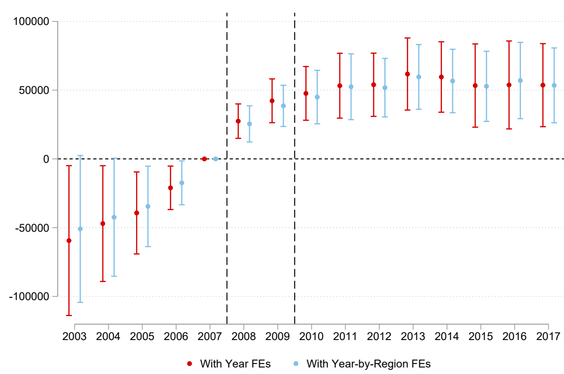
(a) House Price Index



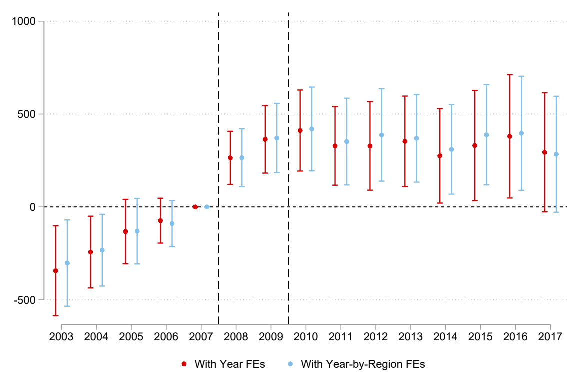
(b) Total Property Value



(c) Property Value per Member



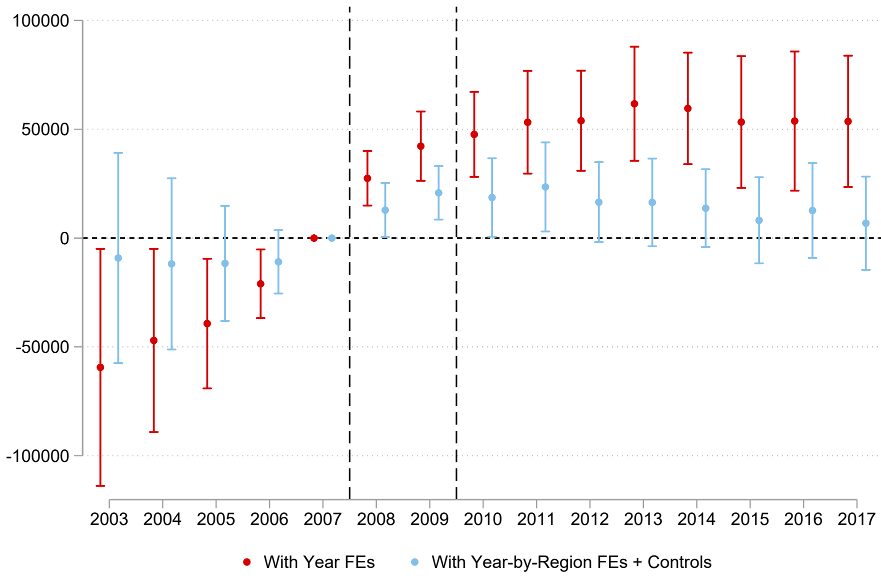
(d) Property Taxes per Member



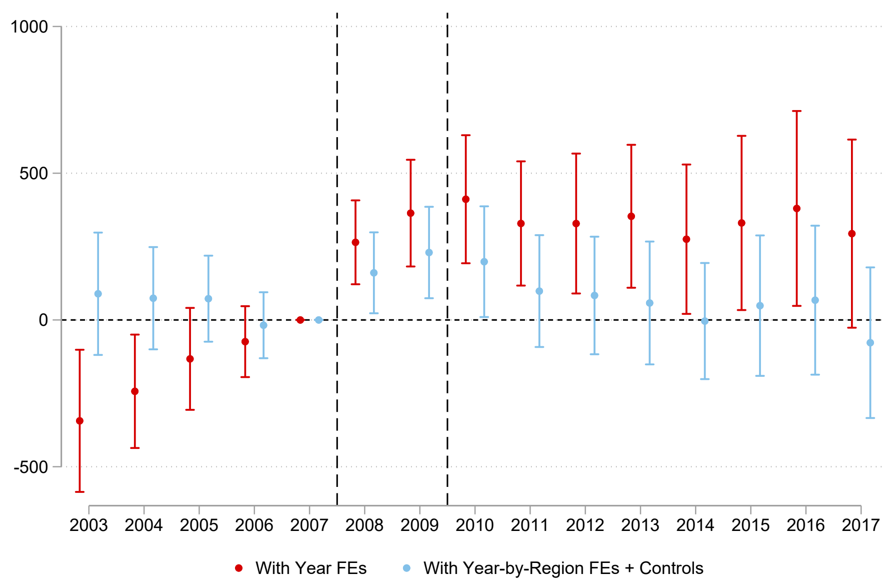
Notes: Each figure presents estimates of the β_k coefficients from equation (2), including either district and year FEs or district and year-by-region FEs. All standard errors are clustered at the school district level.

Figure A.6: Event Study Estimates of Sparsity Aid Program on Local Resources, With Additional Controls

(a) Property Value per Member



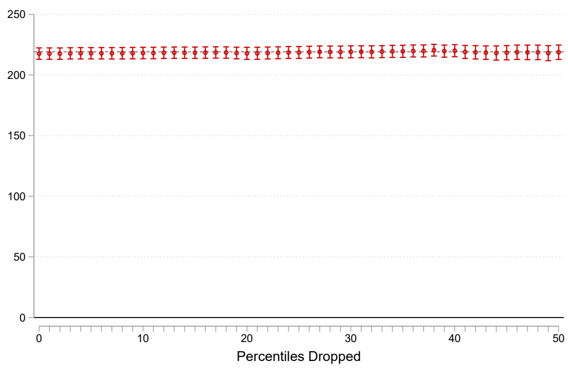
(b) Property Taxes per Member



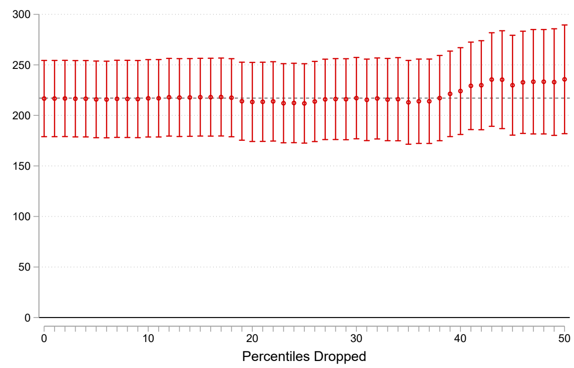
Notes: Each figure presents estimates of the β_k coefficients from equation (2), including either district and year FEs or district and year-by-region FEs, along with districts' log membership and log house price index. All standard errors are clustered at the school district level.

Figure A.7: Sensitivity of School Finance Estimates to Density & Membership Bandwidth Selection

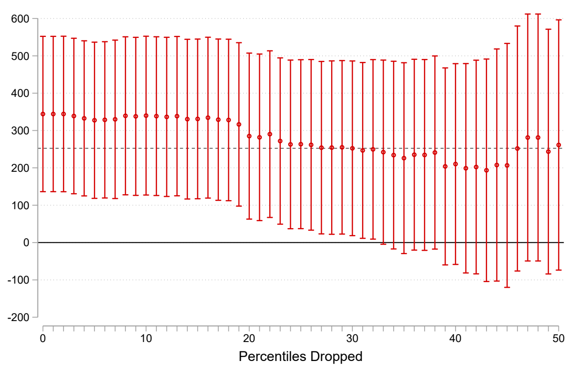
(a) Sparsity Aid



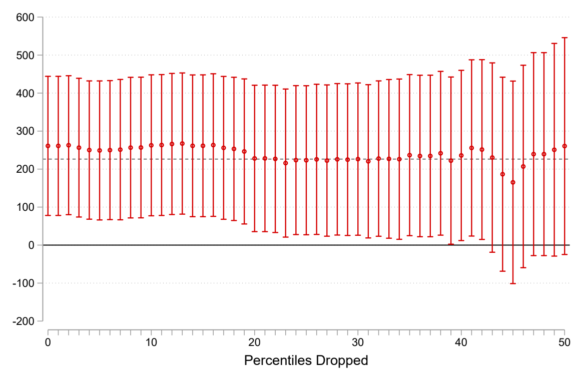
(b) Non-Formula Revenue from State



(c) Total Revenue

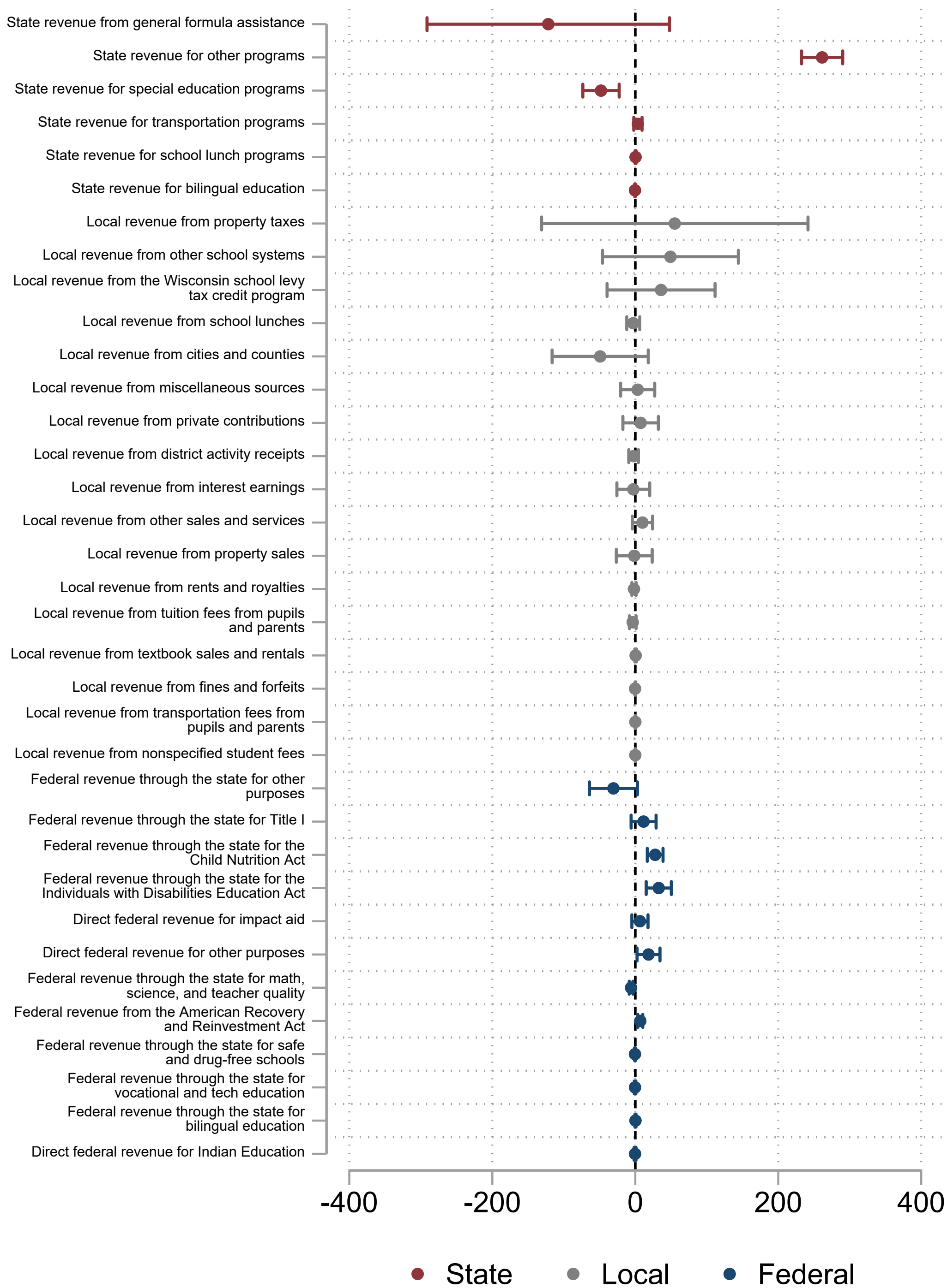


(d) Current Spending



Notes: Each figure presents estimates of β in equation (1), the effect of receiving sparsity aid funding. Each coefficient is estimated from a separate regression, where we restrict the sample by dropping districts at the top of the pre-2008 density and membership distributions. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. All standard errors are clustered at the school district level.

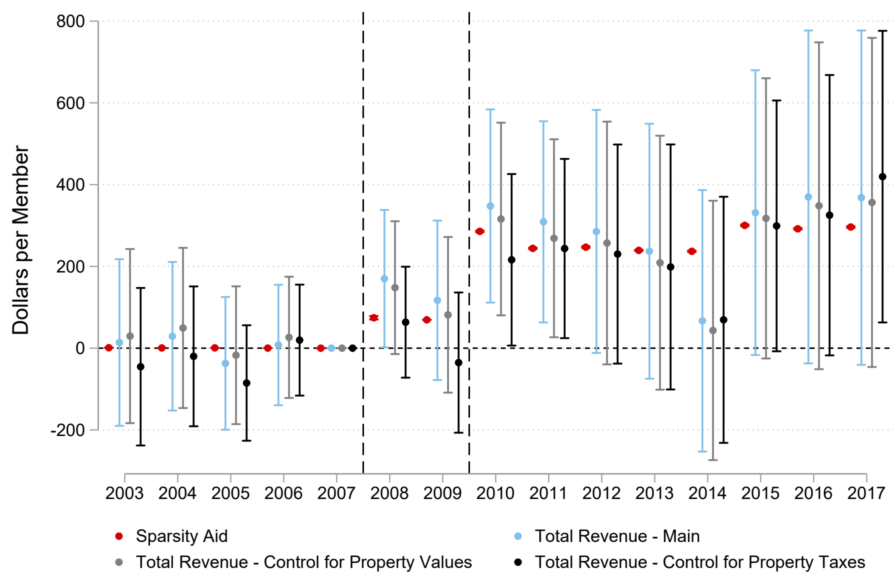
Figure A.8: Effects of Sparsity Aid on District Revenues, by Source



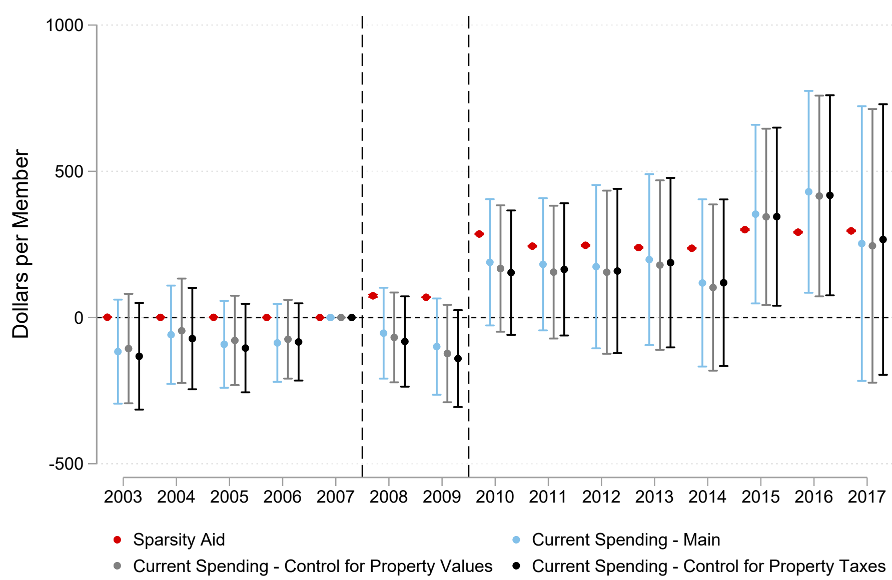
Notes: Each coefficient is estimated from a separate regression and represents β in equation (1), the effect of receiving sparsity aid funding. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. All standard errors are clustered at the school district level.

Figure A.9: Event Study Estimates of Sparsity Aid Program on District Finances, With Additional Controls

(a) Total Revenue



(b) Current Spending on Elementary & Secondary Education

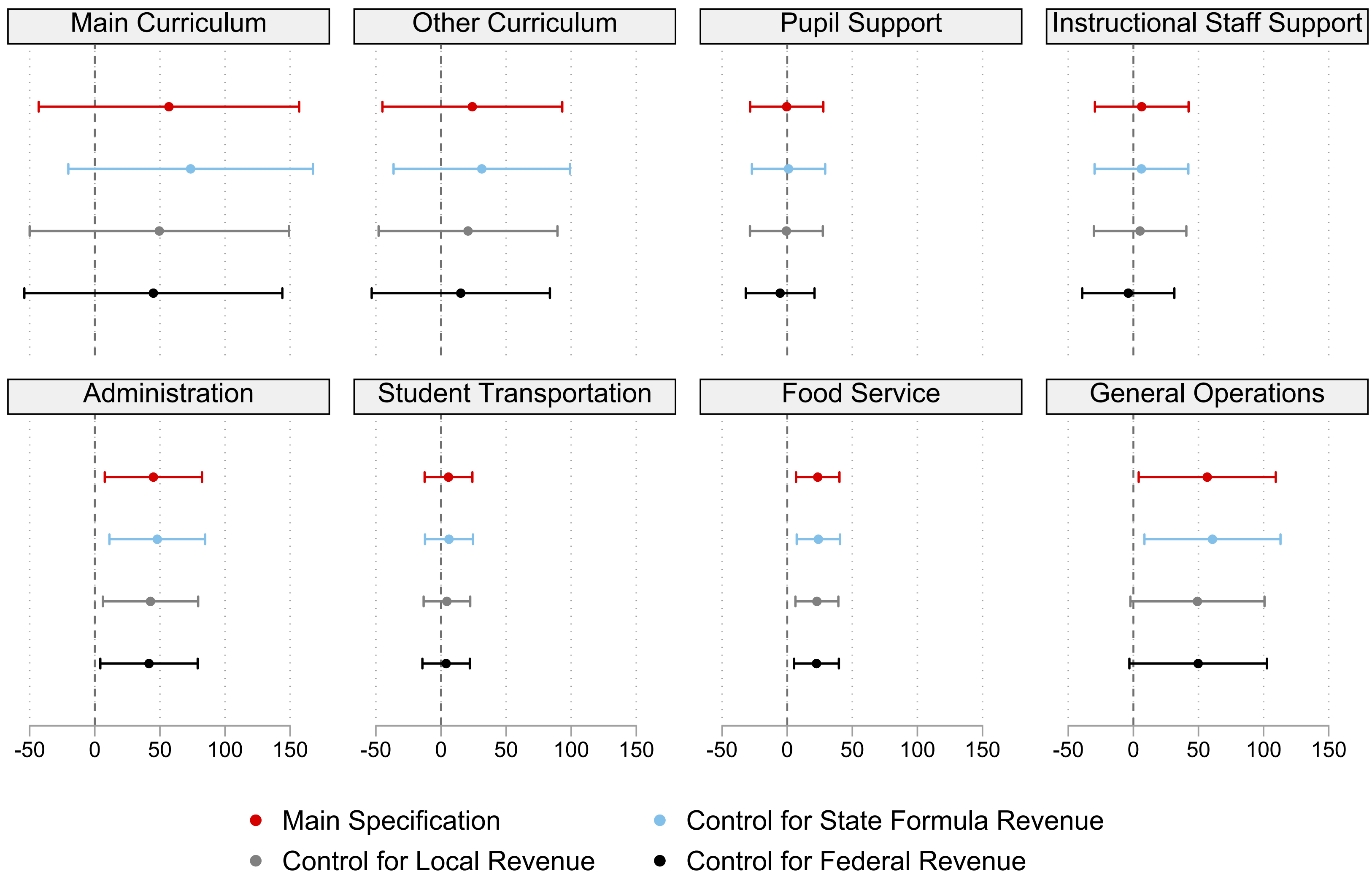


Notes: Each figure presents estimates of the β_k coefficients from equation (2). All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. Additional specifications control for a district's total property value per member or total property tax revenue per member, as indicated. All standard errors are clustered at the school district level.

Figure A.10: Effect of Sparsity Aid Program on District Finances, With Additional Revenue Controls

Notes: Each coefficient is estimated from a separate regression and represents β in equation (1), the effect of receiving sparsity aid funding. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. Additional specifications control for a district's state formula revenue per member, total local revenue per member, or total federal revenue per member, as indicated. All standard errors are clustered at the school district level.

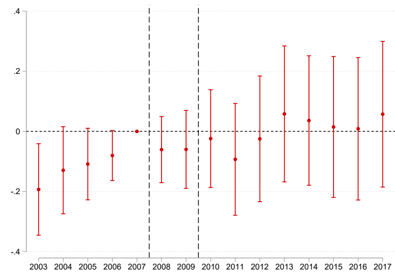
Figure A.11: Effect of Sparsity Aid Program on Spending Allocations, With Additional Revenue Controls



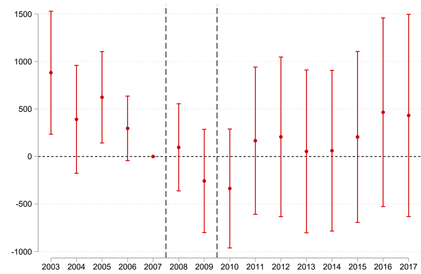
Notes: Each coefficient is estimated from a separate regression and represents β in equation (1), the effect of receiving sparsity aid funding. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. Additional specifications control for a district's state formula revenue per member, total local revenue per member, or total federal revenue per member, as indicated. All standard errors are clustered at the school district level.

Figure A.12: Event Study Estimates of Sparsity Aid Program on Teacher Staffing

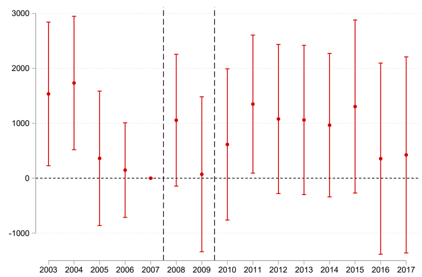
(a) Teacher FTEs per 100 Students



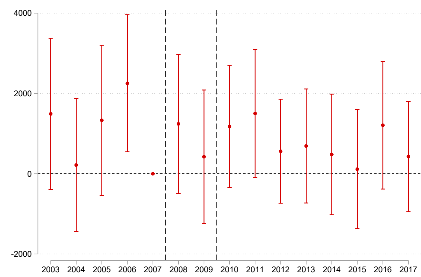
(b) Average Teacher Salary



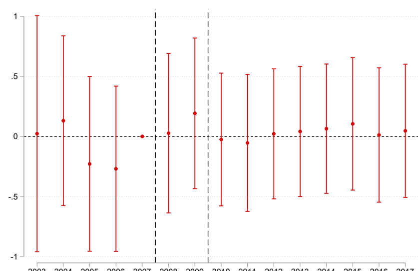
(c) High Teacher Salary



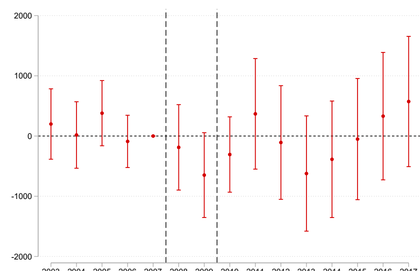
(d) Low Teacher Salary



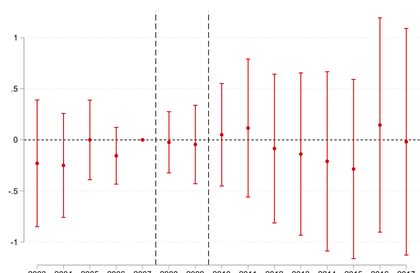
(e) High/Low Teacher Salary Ratio



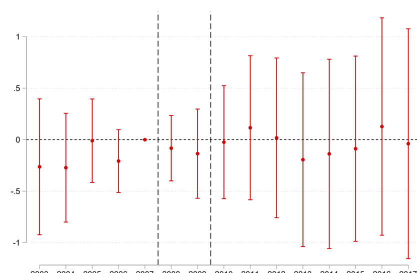
(f) Average Teacher Fringe



(g) Average Teacher Local Experience



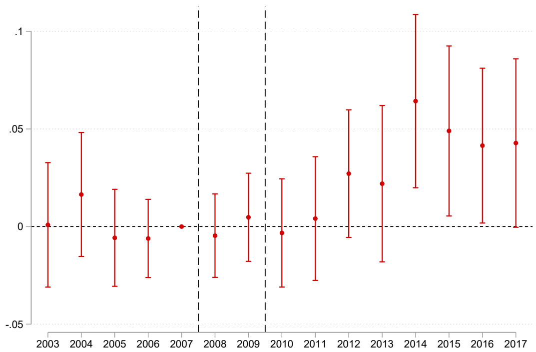
(h) Average Teacher Total Experience



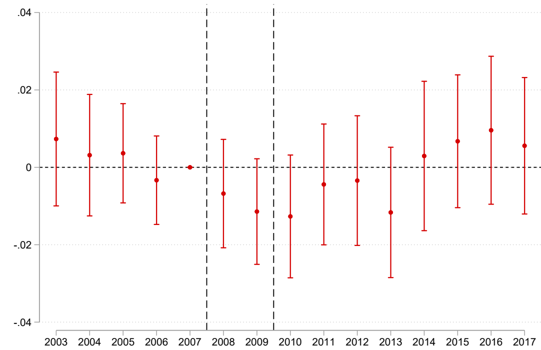
Notes: Each figure presents estimates of the β_k coefficients from equation (2). All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level.

Figure A.13: Event Study Estimates of Sparsity Aid Program on Administrator Staffing

(a) All Administrator FTEs per 100 Students

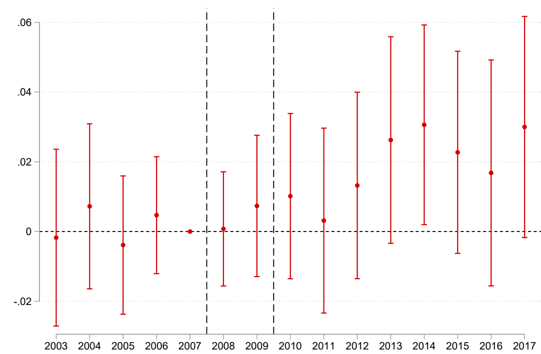


(b) District Leader FTEs per 100 Students

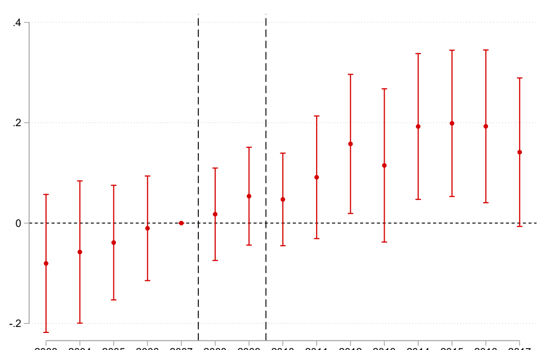


(c) Has Non-Zero Principal FTEs

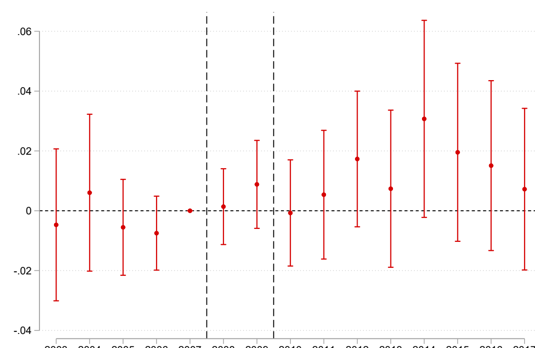
(d) Principal FTEs per 100 Students



(e) Has Non-Zero Other Administrator FTEs

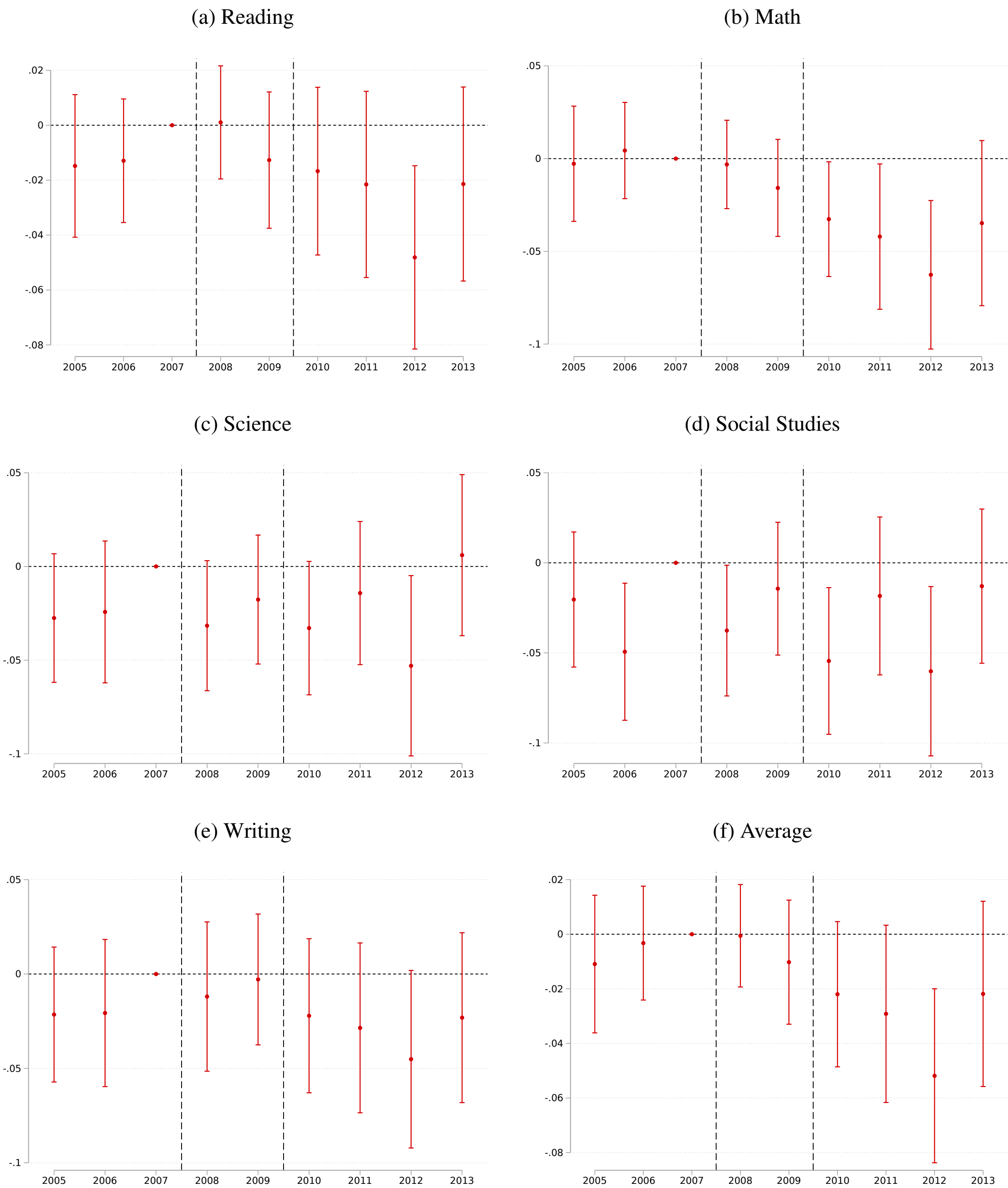


(f) Other Administrator FTEs per 100 Students



Notes: Each figure presents estimates of the β_k coefficients from equation (2). All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level.

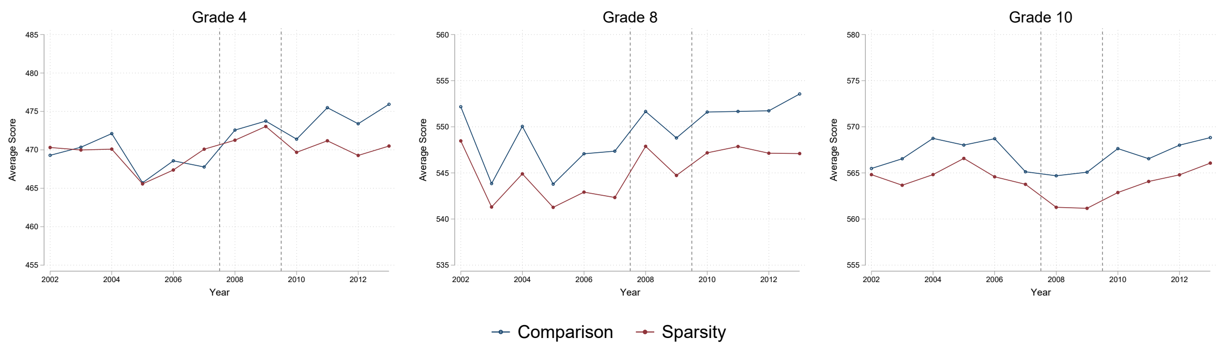
Figure A.14: Event Study Estimates of Sparsity Aid Program on Test Scores



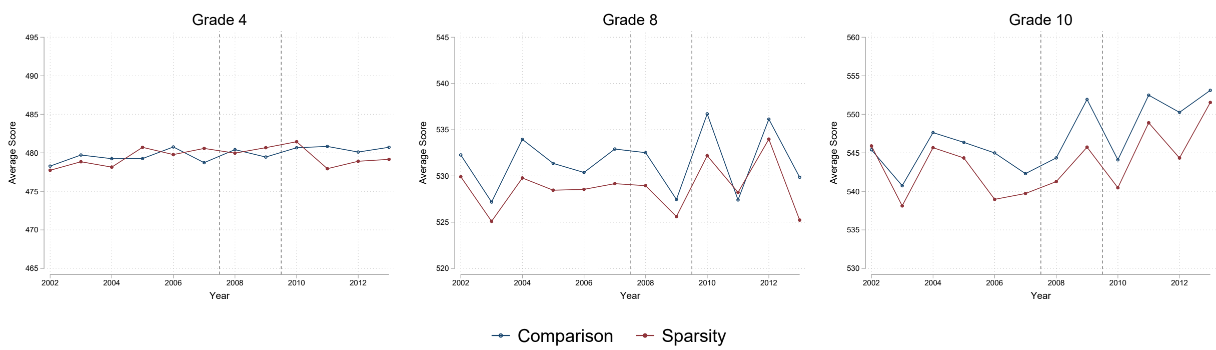
Notes: Each figure presents event study estimates for student-level outcomes. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level.

Figure A.15: School-Level Test Scores, 2002-2013

(a) Math



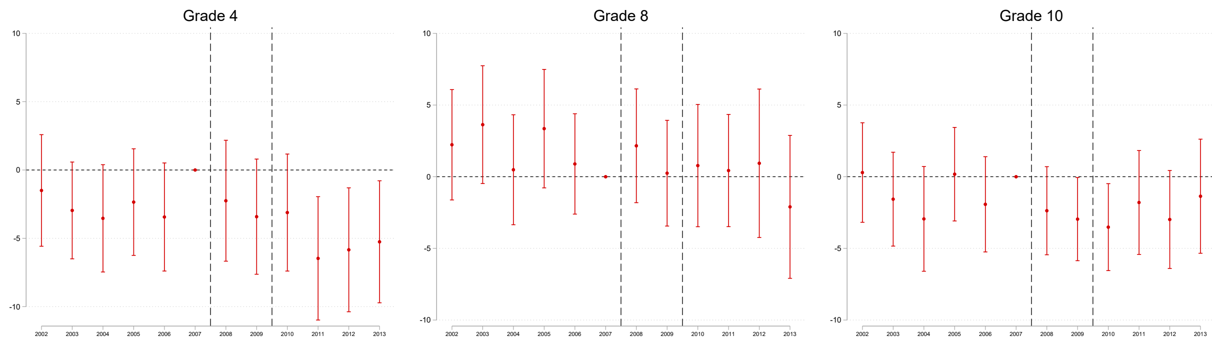
(b) Reading



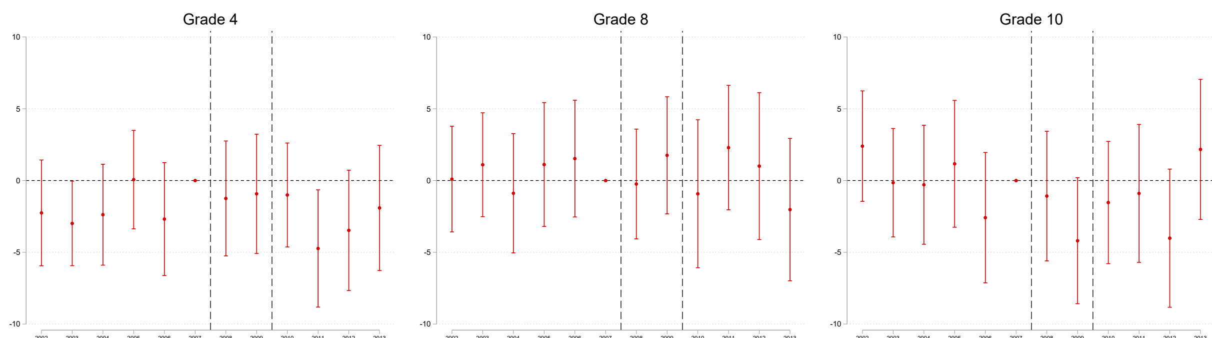
Notes: Panel A plots the average school-level math test scores in sparsity-eligible and ineligible districts across academic years 2002-2013 and grades 4, 8, and 10. Panel B repeats this plot measuring school-level reading test scores.

Figure A.16: Event Study Estimates of Sparsity Aid Program on School-Level Test Scores

(a) Math



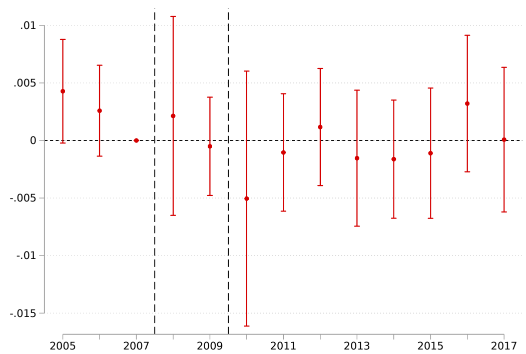
(b) Reading



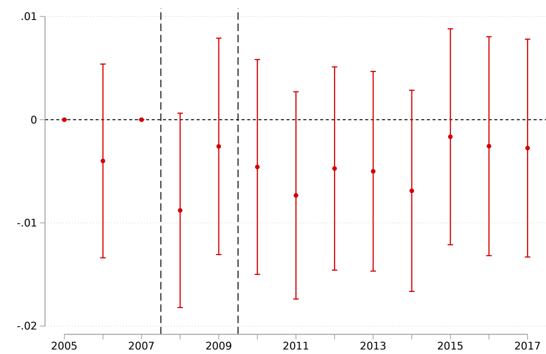
Notes: Each figure presents event study estimates for school-level outcomes. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. All standard errors are clustered at the school district level.

Figure A.17: Event Study Estimates of Sparsity Aid Program on Behavioral Outcomes

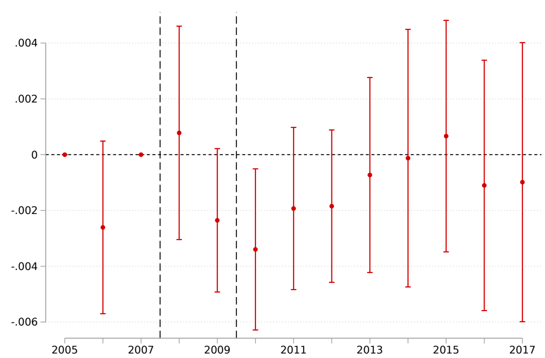
(a) Attendance



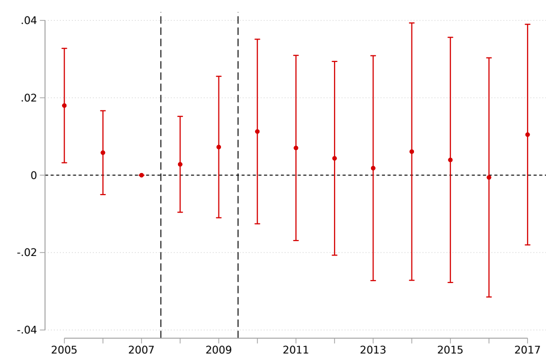
(b) Discipline



(c) Grade Retention



(d) Dual Enrollment



Notes: Each figure presents event study estimates for student-level outcomes. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level.

Table A.1: Effect of Sparsity Aid on Placebo Finance Outcomes

	(1)	(2)	(3)	(4)
Panel A. Revenue other than state non-formula				
Received sparsity aid	7.91	4.13	27.72	35.31
	(108.4)	(101.2)	(117.5)	(117.1)
Observations	2,820	2,820	2,820	2,820
Panel B. Spending other than elementary & secondary education				
Received sparsity aid	58.41	64.51	73.74	77.53
	(161.01)	(163.38)	(183.64)	(188.18)
Observations	2,820	2,820	2,820	2,820
Membership control	X	X	X	X
Demographic controls		X	X	X
Transportation funding control				X
Year-by-CESA FEs			X	X

Notes: Each coefficient is estimated from a separate regression and represents β in equation (1), the effect of receiving sparsity aid funding. Column (1) controls for a district's log membership, column (2) adds controls for a district's log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, and the local child poverty rate, column (3) further controls for whether a district receives additional transportation funding, and column (4) adds year-by-region (CESA) fixed effects. All standard errors are clustered at the district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2: Effects of Sparsity Aid on Teacher Staffing

	FTEs per 100 Students (1)	Avg. Salary (2)	High Salary (3)	Low Salary (4)	Ratio of High/Low (5)	Avg. Fringe (6)	Avg. Local Experience (7)	Avg. Total Experience (8)
Panel A. Main Specification								
Received sparsity aid	0.085	-337.4	98.16	-243.70	0.115	-220.6	0.080	0.103
	(0.082)	(325.9)	(476.4)	(461.4)	(0.158)	(315.3)	(0.313)	(0.328)
Observations	2,820	2,820	2,820	2,820	2,820	2,820	2,820	2,820
Panel B. Interaction with Baseline General Instruction Budget Share								
Received sparsity aid	0.066	-338.2	28.29	-294.0	0.132	-226.9	0.125	0.162
	(0.080)	(328.0)	(488.1)	(462.7)	(0.160)	(312.0)	(0.314)	(0.331)
Received sparsity aid x budget share	-0.041**	-1.906	-149.8*	-107.8	0.037	-13.48	0.095	0.126*
	(0.020)	(68.45)	(77.09)	(91.28)	(0.032)	(67.87)	(0.074)	(0.066)
Observations	2,820	2,820	2,820	2,820	2,820	2,820	2,820	2,820
Effect at 25th Percentile	0.160*	-333.8	373.03	-45.86	0.047	-195.8	-0.094	-0.129
Effect at 75th Percentile	-0.006	-341.5	-234.7	-483.2	0.198	-250.5	0.291	0.384

Notes: The coefficients in each column are estimated from a separate regression and represent β in equation (1), the effect of receiving sparsity aid funding, with interaction effects based on districts' pre-2008 budget shares. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: Effect of Sparsity Aid on Test Proficiency Rates

	3rd Grade (1)	**4th Grade (2)**	**5th Grade (3)**	**6th Grade (4)**	**7th Grade (5)**	**8th Grade (6)**	**10th Grade (7)**	**All Grades (8)**
Panel A. Reading								
Received sparsity aid	0.002	0.001	-0.002	-0.003	0.016*	-0.004	0.001	0.002
	(0.011)	(0.010)	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	(0.005)
Observations	90,631	91,595	92,516	95,059	98,256	100,523	110,142	678,722
Mean	0.343	0.348	0.340	0.348	0.364	0.356	0.369	0.353
Panel B. Math								
Received sparsity aid	-0.011	-0.010	-0.017	-0.037**	-0.006	-0.012	-0.027**	-0.017**
	(0.015)	(0.014)	(0.015)	(0.015)	(0.013)	(0.012)	(0.012)	(0.008)
Observations	90,896	91,720	92,612	95,125	98,328	100,574	110,164	679,419
Mean	0.480	0.473	0.451	0.432	0.441	0.421	0.437	0.447
Panel C. Science								
Received sparsity aid		0.003				0.011	0.003	0.006
		(0.009)				(0.008)	(0.008)	(0.006)
Observations		91,757				100,556	110,096	302,409
Mean		0.816				0.823	0.785	0.807
Panel D. Social Studies								
Received sparsity aid		-0.001				-0.003	-0.005	-0.004
		(0.004)				(0.007)	(0.009)	(0.005)
Observations		91,731				100,407	110,061	302,199
Mean		0.949				0.859	0.808	0.868
Panel E. Writing								
Received sparsity aid		-0.003				0.004	-0.001	-0.000
		(0.009)				(0.011)	(0.008)	(0.006)
Observations		91,607				100,457	109,935	301,999
Mean		0.800				0.652	0.745	0.731
Panel F. Average								
Received sparsity aid	-0.005	-0.002	-0.010	-0.020*	0.005	-0.001	-0.006	-0.005
	(0.012)	(0.007)	(0.010)	(0.011)	(0.010)	(0.007)	(0.007)	(0.005)
Observations	90,619	91,578	92,495	95,036	98,231	100,470	110,025	678,454
Mean	0.412	0.677	0.396	0.390	0.403	0.622	0.629	0.507

Notes: Each coefficient is estimated from a separate regression and represents β in equation (3), the effect of receiving sparsity aid funding. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.4: Heterogeneous Test Score Effects by Baseline Budget Shares

	General Instruc. (1)	**Other Instruc. (2)**	**Pupil Supp. (3)**	**Instruc. Staff Supp. (4)**	**Admin (5)**	**Student Transp. (6)**	**Food Service (7)**	**General Ops. (8)**
Panel A. Reading								
Received sparsity aid	-0.009	-0.008	-0.010	-0.009	-0.010	-0.010	-0.009	-0.010
	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)
Received x budget share	0.001	0.003	-0.006	-0.000	0.002	0.002	-0.004	-0.001
	(0.003)	(0.004)	(0.010)	(0.007)	(0.006)	(0.005)	(0.018)	(0.006)
Observations	678,722	678,722	678,722	678,722	678,722	678,722	678,722	678,722
Effect at 25th Percentile	-0.011	-0.015	-0.005	-0.009	-0.011	-0.011	-0.008	-0.008
Effect at 75th Percentile	-0.008	-0.005	-0.013	-0.010	-0.008	-0.008	-0.010	-0.011
Panel B. Math								
Received sparsity aid	-0.031**	-0.029*	-0.030*	-0.027*	-0.028*	-0.032**	-0.031**	-0.029*
	(0.016)	(0.015)	(0.016)	(0.016)	(0.015)	(0.016)	(0.015)	(0.015)
Received x budget share	-0.002	0.005	0.004	0.008	-0.008	0.006	-0.005	0.005
	(0.003)	(0.004)	(0.013)	(0.009)	(0.009)	(0.008)	(0.025)	(0.007)
Observations	679,419	679,419	679,419	679,419	679,419	679,419	679,419	679,419
Effect at 25th Percentile	-0.026	-0.039**	-0.033*	-0.038**	-0.023	-0.036**	-0.029	-0.035**
Effect at 75th Percentile	-0.035**	-0.023	-0.028	-0.024	-0.038**	-0.026	-0.032*	-0.025
Panel C. Average								
Received sparsity aid	-0.017	-0.015	-0.017	-0.015	-0.015	-0.018	-0.017	-0.016
	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)
Received x budget share	-0.000	0.004	-0.001	0.004	-0.004	0.004	-0.002	0.001
	(0.003)	(0.003)	(0.010)	(0.007)	(0.007)	(0.006)	(0.019)	(0.006)
Observations	678,454	678,454	678,454	678,454	678,454	678,454	678,454	678,454
Effect at 25th Percentile	-0.016	-0.023*	-0.016	-0.02	-0.013	-0.020	-0.016	-0.017
Effect at 75th Percentile	-0.017	-0.011	-0.017	-0.014	-0.020	-0.013	-0.017	-0.016

Notes: The coefficients in each column are estimated from a separate regression and represent β in equation (1), the effect of receiving sparsity aid funding, with interaction effects based on districts' pre-2008 budget shares. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5: Effect of Sparsity Aid on Behavioral Outcomes

	6th Grade (1)	**7th Grade (2)**	**8th Grade (3)**	**9th Grade (4)**	**10th Grade (5)**	**11th Grade (6)**	**12th Grade (7)**	**All Grades (8)**
Panel A. Attendance								
Received sparsity aid	0.001	-0.001	-0.000	-0.003	-0.004	-0.005	-0.005	-0.003
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	140,113	143,606	146,055	156,705	158,689	161,880	165,120	1,072,168
Mean	0.960	0.957	0.954	0.953	0.947	0.940	0.934	0.949
Panel B. Disciplinary Incidence								
Received sparsity aid	-0.005	-0.007	-0.006	-0.002	0.004	-0.003	-0.004	-0.003
	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)	(0.004)
Observations	128,662	131,674	133,521	142,583	144,734	148,048	151,400	980,622
Mean	0.018	0.027	0.033	0.039	0.042	0.039	0.030	0.033
Panel C. Grade Retention								
Received sparsity aid	0.001	-0.004	-0.000	0.001	0.003	-0.001	0.001	0.000
	(0.001)	(0.002)	(0.001)	(0.004)	(0.002)	(0.002)	(0.004)	(0.001)
Observations	125,457	128,167	131,059	134,258	142,107	143,975	147,888	952,911
Mean	0.001	0.003	0.002	0.009	0.004	0.008	0.031	0.009
Panel D. Dual Enrollment								
Received sparsity aid					-0.005	-0.005	0.003	-0.002
					(0.004)	(0.014)	(0.013)	(0.009)
Observations					158,779	162,010	165,442	486,231
Mean					0.008	0.155	0.115	0.042

Notes: Each coefficient is estimated from a separate regression and represents β in equation (3), the effect of receiving sparsity aid funding. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: Predicted Postsecondary Enrollment & Completion, 2005-2007 Sample

	Enrollment		**Completion**	
Variable:	**(1)**	**(2)**	**(3)**	**(4)**
White	0.243***	0.218***	0.237***	0.219***
	(0.017)	(0.013)	(0.017)	(0.015)
Male	-0.091***	-0.090***	-0.106***	-0.107***
	(0.005)	(0.005)	(0.005)	(0.005)
FRL	-0.173***	-0.163***	-0.153***	-0.148***
	(0.011)	(0.010)	(0.010)	(0.010)
LEP	-0.112***	-0.135***	-0.135***	-0.161***
	(0.038)	(0.035)	(0.041)	(0.037)
Special Ed	-0.354***	-0.351***	-0.370***	-0.369***
	(0.009)	(0.009)	(0.009)	(0.009)
Pseudo R2	0.084	0.102	0.074	0.090
N	42,269	42,203	42,269	42,108
School FEs		X		X

Notes: Coefficients in each column are estimated via logit regression. Each coefficient represents the marginal effect of a student attribute on the likelihood that a student will enroll in postsecondary education within one year of high school graduation (columns 1 and 2) or will complete a postsecondary degree within the time frame of the data (columns 3 and 4). All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Effect of Sparsity Aid on Postsecondary Enrollment & Completion, 2005-2013 Sample

	Enrollment			Completion		
	Any	**Two-Year**	**Four-Year**	**Any**	**Two-Year**	**Four-Year**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Panel A. All Students						
Received sparsity aid	0.010	0.008	0.005	0.006	0.001	0.006
	(0.010)	(0.008)	(0.007)	(0.009)	(0.007)	(0.008)
Observations	119,730	119,730	119,730	119,730	119,730	119,730
Mean	0.552	0.237	0.335	0.416	0.174	0.272
Panel B. Below-Median Propensity Students						
Received sparsity aid	0.032**	0.024**	0.012	0.018*	0.017**	0.004
	(0.013)	(0.011)	(0.009)	(0.011)	(0.009)	(0.008)
Observations	58,717	58,717	58,717	58,736	58,736	58,736
Mean	0.413	0.207	0.218	0.283	0.136	0.164
Panel C. Above-Median Propensity Students						
Received sparsity aid	-0.011	-0.007	-0.002	-0.010	-0.013	0.006
	(0.012)	(0.011)	(0.011)	(0.014)	(0.010)	(0.012)
Observations	61,011	61,011	61,011	60,991	60,991	60,991
Mean	0.686	0.267	0.448	0.543	0.211	0.377

Notes: Each coefficient is estimated from a separate regression and represents β in equation (3), the effect of receiving sparsity aid funding. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects, along with student-level race, gender, FRL, special education, and limited English proficiency indicators. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Heterogeneous Postsecondary Effects by Baseline Budget Shares

	General Instruc. (1)	**Other Instruc. (2)**	**Pupil Supp. (3)**	**Instruc. Staff Supp. (4)**	**Admin (5)**	**Student Transp. (6)**	**Food Service (7)**	**General Ops. (8)**
Panel A. Postsecondary Enrollment (All)								
Received sparsity aid	0.002	0.002	0.004	0.005	0.005	0.000	0.003	0.002
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Received x budget share	-0.004*	-0.005**	0.013*	0.005	-0.007	0.011**	0.000	-0.005
	(0.002)	(0.002)	(0.007)	(0.006)	(0.005)	(0.005)	(0.012)	(0.005)
Observations	165,442	165,442	165,442	165,442	165,442	165,442	165,442	165,442
Effect at 25th Percentile	0.012	0.011	-0.006	-0.002	0.008	-0.007	0.003	0.007
Effect at 75th Percentile	-0.005	-0.003	0.011	0.007	-0.003	0.011	0.003	-0.002
Panel B. Postsecondary Enrollment (Low Propensity)								
Received sparsity aid	0.022*	0.023*	0.026**	0.028**	0.029**	0.019	0.024**	0.024*
	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)	(0.013)
Received x budget share	-0.005*	-0.002	0.013	0.007	-0.010	0.014**	-0.021	-0.002
	(0.003)	(0.003)	(0.010)	(0.006)	(0.007)	(0.006)	(0.018)	(0.006)
Observations	82,740	82,740	82,740	82,740	82,740	82,740	82,740	82,740
Effect at 25th Percentile	0.034**	0.027**	0.015	0.018	0.033**	0.010	0.032**	0.027**
Effect at 75th Percentile	0.013	0.022	0.033**	0.031**	0.016	0.032***	0.018	0.022
Panel C. Postsecondary Completion (All)								
Received sparsity aid	0.006	0.005	0.007	0.009	0.011	0.006	0.007	0.004
	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Received x budget share	-0.004*	-0.005**	0.006	0.006	-0.011**	0.003	0.007	-0.008**
	(0.002)	(0.002)	(0.006)	(0.006)	(0.005)	(0.005)	(0.011)	(0.004)
Observations	165,442	165,442	165,442	165,442	165,442	165,442	165,442	165,442
Effect at 25th Percentile	0.015	0.015*	0.002	0.002	0.016	0.004	0.004	0.014
Effect at 75th Percentile	-0.001	0.001	0.011	0.011	-0.003	0.009	0.009	-0.001
Panel D. Postsecondary Completion (Low Propensity)								
Received sparsity aid	0.013	0.015	0.017*	0.017*	0.021**	0.014	0.015	0.012
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)
Received x budget share	-0.003	0.000	0.010	0.005	-0.016***	0.004	0.003	-0.009*
	(0.002)	(0.003)	(0.009)	(0.006)	(0.005)	(0.005)	(0.015)	(0.005)
Observations	82,705	82,705	82,705	82,705	82,705	82,705	82,705	82,705
Effect at 25th Percentile	0.020*	0.015	0.008	0.010	0.028**	0.012	0.014	0.023**
Effect at 75th Percentile	0.008	0.015	0.022*	0.019**	0.002	0.018*	0.016	0.006

Notes: The coefficients in each column are estimated from a separate regression and represent β in equation (1), the effect of receiving sparsity aid funding, with interaction effects based on districts' pre-2008 budget shares. All specifications control for a district's log membership, log house price index, number of school buildings, racial composition (% white, % Black, % Hispanic, and % Asian), % FRL, % special education, the local child poverty rate, a dummy variable indicating whether a district receives funding from the state's high-cost pupil transportation aid program, and year-by-region (CESA) fixed effects. All standard errors are clustered at the school district level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B Survey Text and Recruitment Email

B.1 Email to Wisconsin Association of School District Administrators (WASDA)

Subject: Interview request for research on Wisconsin rural school districts

Dear [Insert Name],

I hope this email finds you well. My name is Dr. Riley Acton and I am an Assistant Professor of Economics at Miami University. I research the economics of education and education policy and am currently working with a team of researchers to study Wisconsin's sparsity aid program. Our work is funded by the Bill & Melinda Gates Foundation and aims to develop a richer understanding of the various challenges that rural school districts face, as well as the ways in which the sparsity aid program helped districts address these challenges. Our ultimate goal is to inform state and district policymakers on a national scale about how state investments in rural schools affect students, districts, and communities.

My research team and I would love to have an opportunity to speak with you or one of your WASDA colleagues about the history of the sparsity aid program and how it has been perceived among school district administrators. Is there a time in the coming weeks when you would be available for a phone or Zoom call? I am cc'ing my collaborator, Salem Rogers, who can coordinate with you or someone from your office to find a time that is most convenient. Thank you for your consideration, and we look forward to hearing back from you!

All the best,

Riley Acton, Ph.D.

B.2 Survey Text

Sparsity Aid Research Survey

Research Consent Information:

You are invited to participate in a research project being conducted by Dr. Riley Acton from Miami University. The purpose of this research is to examine the unique challenges of small, rural school districts and how Wisconsin's sparsity aid program has helped districts address these challenges. Participation in this research is restricted to persons 18 years of age or older.

Completing the survey should take about 10 minutes. Your participation is voluntary, you may skip any questions you do not want to answer, and you may stop at any time. Foreseeable risks and/or discomforts associated with your participation are minimal and you will receive no direct benefit from your participation. However, we hope our study will benefit Wisconsin students, district leaders, and national education policymakers by uncovering how school districts and policymakers can effectively support student success with increased funding, specifically in the rural context. To achieve this goal, we plan on broadly disseminating our findings to the academic community and interested parties in the general public.

Only the research team will have access to individual responses and we will not attribute the name of your school district to any of your answers in any presentations or publications without first receiving your permission, in writing, to do so. Unless you provide this permission, the results of the research will be presented publicly only as aggregate summaries. The research data will be retained until June 30, 2027.

Funding agencies or journal policies may require that individual participant data be made available to other researchers. Sharing data in this way advances the field by allowing the data to be used beyond this study. No personally identifying information will be included in the shared data.

Our research team is not associated with Wisconsin's Department of Public Instruction, and we have no conflicts of interest to disclose. The research project is supported by a non-renewable grant from the Bill & Melinda Gates Foundation (INV-036567).

If you have any questions about this research or you feel you need more information to determine whether you would like to volunteer, you can contact the principal investigator (PI), Dr. Riley Acton, at actonr@miamioh.edu or at (513) 529-2865. If you have questions or concerns about the rights of research subjects, you may contact the Miami University Research Ethics and Integrity Office at (513) 529-3600 or humansubjects@miamioh.edu.

Please keep a copy of this information for future reference.

1. **Do you consent to participate in this study?**
    - I consent.
    - I do not consent.
2. **Have you ever worked for a school district that received funding from Wisconsin's Sparsity Aid program?**
    - Yes
    - No
    - I don't know

    General Background: For these questions, please think about the most recent school district that you worked for that received sparsity aid funding. This can be your current school district.
3. **What school district did you work for?**
4. **What title best described your highest position in this district**
    - District Administrator/Superintendent

- District Treasurer/Business Official
- School Board Member
- Teacher
- Other [open-ended]

5. **During what years did you work for this district? Please select all that apply.**
   - Before 2008
   - 2008
   - 2009
   - ...
   - 2020
   - 2021
   - 2022/Present

Sparsity Aid Funds: For these questions, please continue to think about the most recent school district that you worked for that received sparsity aid funding. This can be your current school district.

6. **When your district received sparsity aid funding, how often were the funds set aside for a specific purpose?**
   - Never
   - Rarely
   - Often
   - Always
   - I don't know

7. **To the best of your knowledge, for what purposes were sparsity aid funds used? Please select all that apply.**
   - Instruction in core academic subjects (e.g., math, reading)
   - Supplemental instruction (e.g., tutoring)
   - Electives (e.g., art, music) or co-curricular activities (e.g., athletics, speech & debate)
   - Pupil Support (e.g., guidance, health, social work)

- Instructional Staff Support (e.g., curriculum development, training)
- Administration (e.g., general district administration, school building administration)
- Operations and/or maintenance (e.g., site and building repairs)
- Pupil Transportation
- Food Service
- I don't know

8. **To the best of your knowledge, were sparsity aid funds spent on specific grade levels? Please select all that apply.**

    - Elementary
    - Middle/junior high
    - High school
    - District-wide
    - I don't know

Perceptions: For these questions, please continue to think about the most recent school district that you worked for that received sparsity aid funding. This can be your current school district.

9. **Thinking back to the early years of the sparsity aid program (2008-2010), how likely did you think it was that the program would continue long-term?**

    - Very unlikely
    - Unlikely
    - Likely
    - Very likely
    - Do not remember
    - Was not aware of the sparsity aid program in 2008-2010

10. **If all other funding sources were held constant, but your district no longer had access to sparsity aid funding, how likely do you think each of the following scenarios would be? [Options: Very Unlikely, Unlikely, Likely, Very likely, No opinion]**

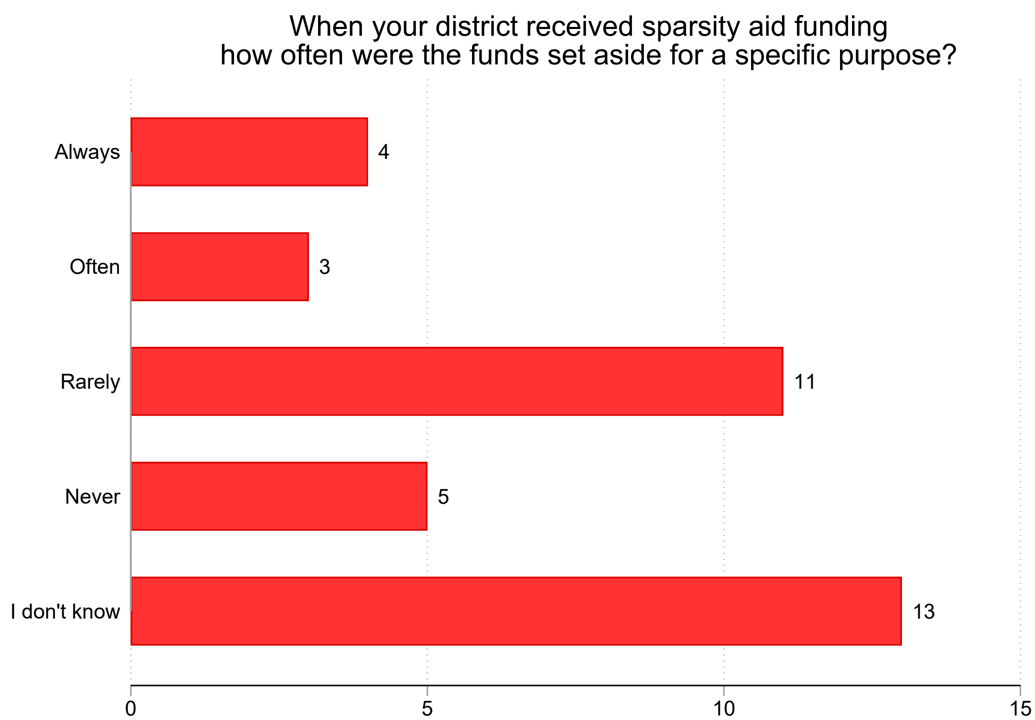
    - My district would employ fewer staff members.

- Staff retention in my district would worsen.
- Student achivement (e.g., test scores) in my district would decline.
- Graduation rates in my district would decline.
- Fewer students in my district would pursue postsecondary education.
- My district would consolidate with a neighboring district.
- My district would implement a four-day school week.

11. **In your opinion, how has the sparsity aid program most affected your district, staff, and students since it began in 2008? Please include as much detail as you are able. [Open-ended response]**

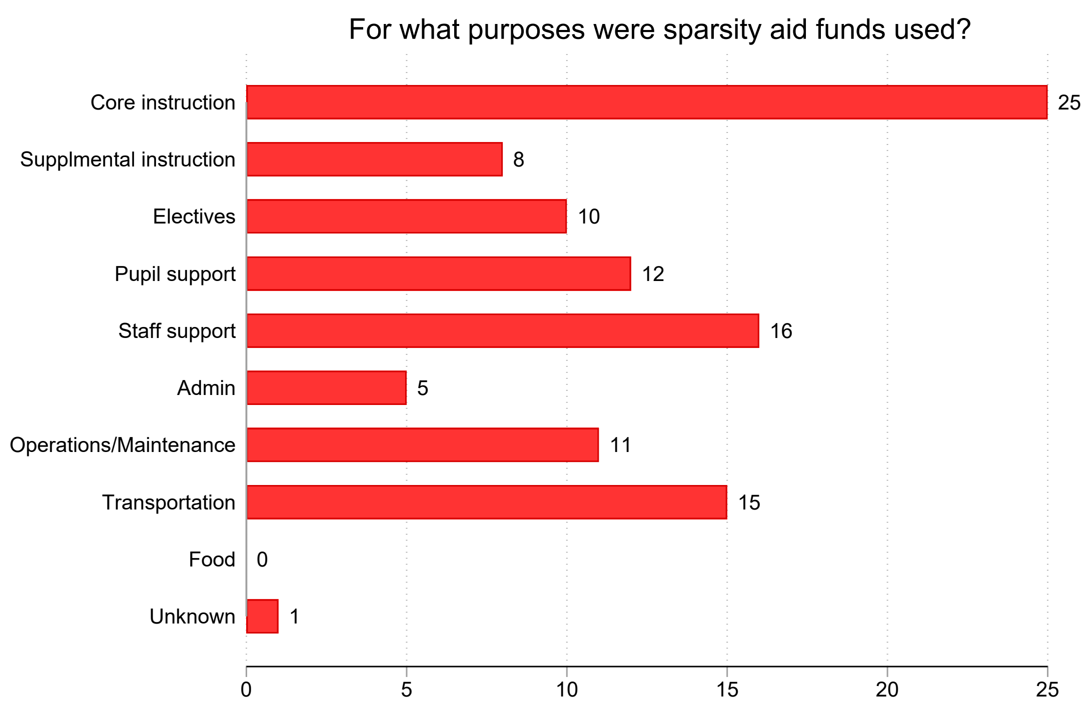
B.3 Survey Results

Figure B.1: Distribution of responses for Q6



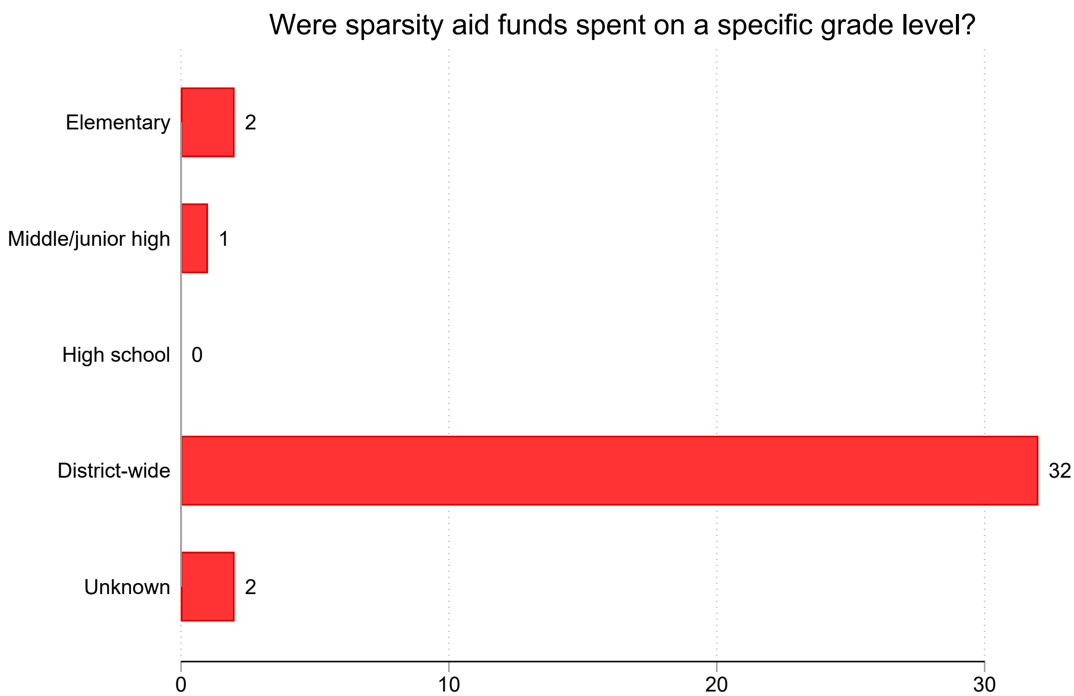
Notes: Each bar represents the number of survey respondents (out of 36) who selected the corresponding answer to, "When your district received sparsity aid funding, how often were the funds set aside for a specific purpose?".

Figure B.2: Distribution of responses for Q7



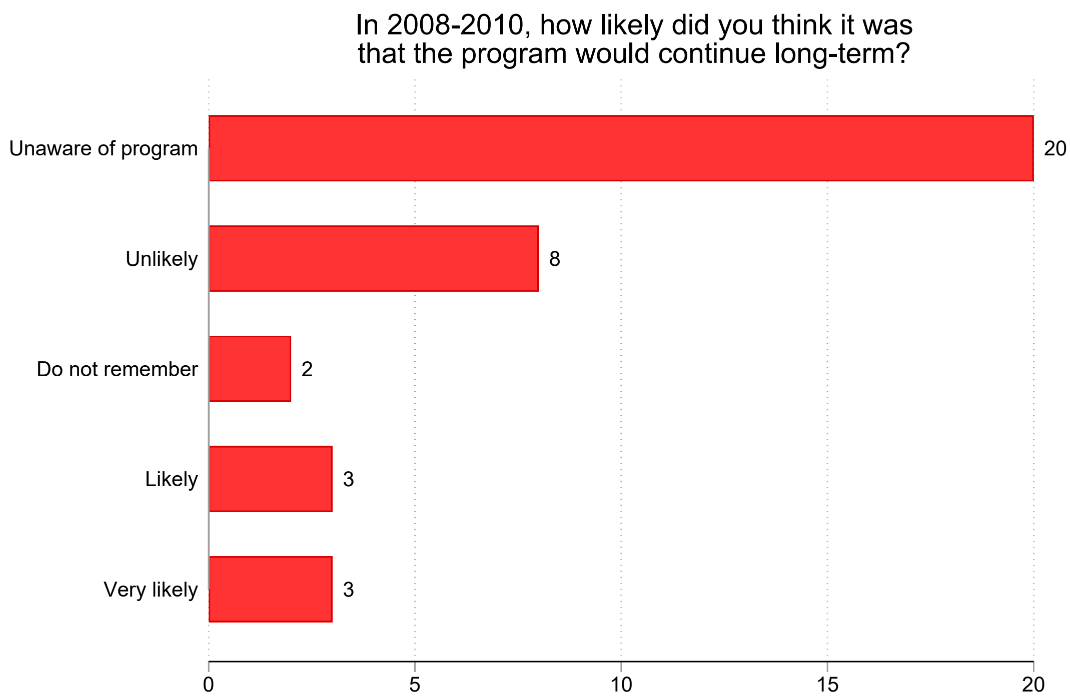
Notes: Each bar represents the number of survey respondents (out of 36) who selected the corresponding answer to, "To the best of your knowledge, for what purposes were sparsity aid funds used? Please select all that apply."

Figure B.3: Distribution of responses for Q8



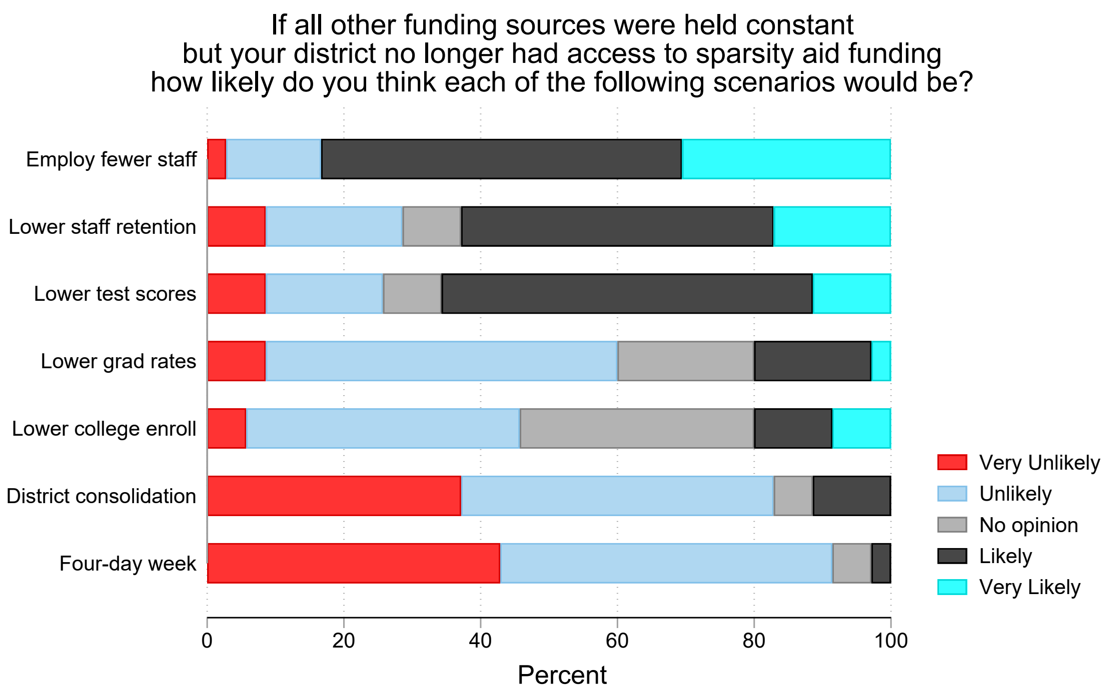
Notes: Each bar represents the number of survey respondents (out of 36) who selected the corresponding answer to, ”To the best of your knowledge, were sparsity aid funds spent on specific grade levels? Please select all that apply.”

Figure B.4: Distribution of responses for Q9



Notes: Each bar represents the number of survey respondents (out of 36) who selected the corresponding answer to, ”Thinking back to the early years of the sparsity aid program (2008-2010), how likely did you think it was that the program would continue long-term?”

Figure B.5: Distribution of responses for Q10



Notes: Each colored bar segment represents the proportion of survey respondents (out of 36) who selected the corresponding answer to, "If all other funding sources were held constant, but your district no longer had access to sparsity aid funding, how likely do you think [scenario] would be?"