Migration Responses to Targeted K–12 Funding:

Evidence from California's Local Control Funding Formula*

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This version: October 2026

Abstract

California's 2013 Local Control Funding Formula (LCFF) increased state aid to K–12 school districts with large shares of low-income students without raising local taxes. Using an event-study difference-in-differences design, I find that the reform made the benefiting districts more attractive to households: home values increased substantially and net in-migration slightly. By 2019, revenue per pupil had risen by $1,000 to $1,600 and home values by 8% to 14% in more exposed districts relative to less exposed districts. Origin-to-destination migration flows show that the neighborhoods migrants left and entered changed little, and student composition remained largely unchanged. The results suggest that households do respond to school funding when it comes without higher taxes. This complements prior work, which finds small or null responses to reforms that combine funding with tax increases.

1 Introduction

School finance reforms can affect both the education that districts provide and where households choose to live. Additional school spending has been linked to higher test scores, more years of schooling, and better adult outcomes (Jackson et al., 2016; Biasi, 2023). Higher spending may also make disadvantaged districts more attractive to families, including higher-income families who would otherwise live elsewhere (Tiebout, 1956; Figlio and Lucas, 2004). Such moves could reduce residential stratification across districts. However, increased demand may be capitalized into housing prices, limiting changes in the number and composition of residents (Oates, 1969). The balance between price and sorting responses depends on housing supply and how households value additional school spending (Epple and Ferreyra, 2008).[1]

In this paper, I examine these responses in the context of California's 2013 Local Control Funding Formula (LCFF). The reform directed additional state aid to school districts with larger

*Preliminary

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[1]Epple and Ferreyra (2008) show that equilibrium sorting responses to school finance reforms depend on several factors, including the strength of peer effects and how districts use additional funds. Under sufficient capitalization, changes in residential composition can be limited, though this is not a uniform prediction of the model.

shares of low-income students, English learners, and foster youth. A key feature of LCFF is that it increased school funding without requiring higher local taxes, which lets me study how households respond to additional school spending alone. Existing work finds that LCFF raised test scores and graduation rates in more exposed districts (Lafortune and Mehlotra, 2021; Johnson, 2023).[2] I study whether it also made these districts more attractive to households, as reflected in housing prices and residential sorting.

The closest study is Ainsworth and Clark (2025), who measure residential-sorting responses to parcel taxes in California school districts. Parcel taxes combine two changes: higher school operational spending, which households have reason to value, and higher local taxes, which households must pay. Because the two changes can affect residential demand in opposite directions, the effect of parcel-tax passage on sorting is unclear a priori. They find that parcel taxes increase spending but generate little residential sorting. It is therefore informative to examine a policy that changes only the spending side, such as LCFF.

A challenge in studying residential sorting is measuring changes in where households live. Studies of school finance reforms often examine housing values and resident or student composition (Dee, 2000; Cellini et al., 2010; Chakrabarti and Roy, 2015; Biasi et al., 2025; Aaronson, 1999; Ferreyra, 2009). These measures capture sorting only indirectly. Resident characteristics can change as incomes change, populations age, and cohorts turn over, even without migration. Student composition can also change through reclassification or moves between public and private schools (Ainsworth and Clark, 2025). I use origin-to-destination migration flows between census block groups (Agostini et al., 2025), aggregated to school districts. These flows allow me to measure migration into and out of districts and characterize the neighborhoods that migrants leave and enter.

I compare districts by their baseline exposure to LCFF in an event-study difference-in-differences design and present three main sets of findings.

First, LCFF increased funding in disadvantaged districts. By 2019, revenue per pupil was about $1,000 higher in districts where 55 to 80% of students were eligible for additional funding and about $1,600 higher in districts where the eligible share exceeded 80%.

Second, home values increased in more exposed districts, with larger effects where exposure was greater. By 2019, home values were about 8 to 9% higher in districts with eligible shares of 55 to 80% and about 13 to 14% higher in districts with shares above 80%. The increases were slightly larger in high school districts than in elementary districts and had not narrowed by the end of the sample.

Third, net in-migration into the most exposed districts turned positive, peaking at about 0.5 percentage points of population in 2016 and 2017, but residential sorting was limited. The characteristics of the neighborhoods that migrants left and entered changed little, and student composition remained largely unchanged.

Overall, LCFF made the benefiting districts more attractive to households. The added demand appeared mainly in home values, while net in-migration rose only slightly and the composition of

[2] See also Johnson and Tanner (2018) and Lafortune et al. (2023).

residents changed little. Relatively inelastic housing supply in California may help explain the limited sorting response (Saiz, 2010). These results help clarify when households respond residentially to school funding. Ainsworth and Clark (2025) find little sorting after parcel-tax increases, which raise spending and local taxes together. LCFF raised spending alone, and households responded, mostly through home values. The findings also differ from those of Chakrabarti and Roy (2015), who find both capitalization and sorting following Michigan's Proposal A.

1.1 The Local Control Funding Formula

In 2013, California replaced its K–12 school finance system with the Local Control Funding Formula (LCFF). Under the previous system, funding came from district-specific revenue limits and more than fifty restricted categorical programs (California Department of Education, 2025). As a result, districts with similar student needs could receive different amounts of funding. LCFF replaced this system with a formula consisting of a base grant, a supplemental grant for each unduplicated pupil, and a concentration grant for unduplicated pupils above a district-wide 55% threshold.[3]

An unduplicated pupil is a student who is eligible for free or reduced-price meals, is an English learner, or is in foster care. Students who fall into more than one category are counted only once. In 2013–14, the average unduplicated pupil percentage (UPP) across school districts was about 60% (California Department of Education, 2024, 2015).

LCFF also gave districts greater discretion over how to spend their funding. Supplemental and concentration funds must be used to improve services for unduplicated pupils, but districts are not required to adopt any particular program. Districts document their spending plans in Local Control and Accountability Plans (California Department of Education, 2025). Because funding under LCFF depends in part on the share of unduplicated pupils, districts with different baseline shares faced different increases in funding. I use this variation in exposure to study the effects of the reform on housing prices and residential sorting.

2 Data and Descriptive Statistics

I combine administrative, housing, and migration data to study how LCFF affected district finances, student composition, housing markets, and residential mobility. I organize the data at the school district by year level when annual information is available and express all dollar amounts in constant 2020 dollars. I focus on the 2011 to 2019 period because the migration data do not extend beyond 2019. This sample period also keeps the analysis before the pandemic, when housing and mobility patterns differed from earlier years.

District finances. I construct district revenue and expenditure measures from the California Department of Education's Standardized Account Code Structure (SACS) files. I focus on the

[3]For school districts and charter schools, concentration grants apply only to the share of unduplicated pupils above the 55% threshold. The concentration grant factor was 50% when LCFF was introduced and increased to 65% beginning in 2021–22, outside my sample period.

general fund, the main operating fund through which LCFF revenue is allocated.[4] My revenue measure is total general fund revenue. For expenditures, I use two measures, following Agostini et al. (2025) and Bruno (2018). The first is total general fund spending. The second is K–12 student spending, which excludes pre-K and adult education, debt service, transfers, and other categories not directly tied to K–12 instructional activity. I divide both measures by average daily attendance (ADA) to obtain per-pupil values.

Student administrative records. I use California Department of Education administrative files to measure student disadvantage, poverty, and enrollment. Unduplicated pupil counts, available beginning in 2013, capture the district-level disadvantage share used in the LCFF formula. I use districts' 2013–14 unduplicated pupil shares to define treatment and control groups throughout the analysis. I measure student poverty using the free and reduced-price meal eligibility files, which cover 2011 to 2019. Public school enrollment comes from the California Longitudinal Pupil Achievement Data System (CALPADS) and is measured on Census Day. Private school enrollment comes from the California Department of Education's annual Private School Affidavit files. I aggregate both to the school district level and use them to construct total enrollment and the public school share.

Housing. I measure home values with Zillow's Home Value Index (ZHVI), which reports monthly estimates of typical single-family home values at the ZIP code level. To match the housing data to the school-year panel, I average monthly values from May through April. For example, the value for the 2013 school year is based on monthly values from May 2012 through April 2013.

I aggregate the ZIP code-level data to school districts using Census relationship files and a population-weighted crosswalk based on the NCES School District Geographic Relationship Files. Because ZIP codes and school district boundaries do not always match in California, district-level home values are population-weighted averages of the ZIP code values assigned to each district. I construct the measure separately for K–8 and 9–12 districts because elementary and high school district boundaries can differ. Unified districts cover both sets of grades, so I include them in both samples.

ZHVI is unavailable for some ZIP codes in some months, mostly in areas with limited housing transactions. This slightly reduces the housing sample, but districts with ZHVI coverage account for more than 95% of total enrollment.

Migration. I use annual origin-to-destination migration data from the MIGRATE project (Agostini et al., 2025), which estimates residential flows between census block groups. I link these flows to block-group characteristics from the 2009–2013 American Community Survey to measure the income and demographic characteristics of the neighborhoods migrants leave and enter. I then use a block-group-to-school-district crosswalk to aggregate the data to the district-year level. The resulting measures capture district inflows and outflows, along with proxy measures of the characteristics of origin and destination neighborhoods.

[4]The SACS data also report all-funds measures, which include capital projects and other special-purpose accounts. I focus on the general fund because LCFF revenue flows through this fund.

2.1 Sample and Descriptive Statistics

Following Bruno (2018) and Lafortune and Mehlotra (2021), I restrict the sample to districts with ADA of at least 250 in every year and drop observations with per-pupil K–12 spending below 20% or above 500% of the California annual mean. These restrictions drop about 26% of district-year observations but only 1.1% of total enrollment. The final sample remains broadly representative of California students by enrollment.

Table 1 reports descriptive statistics for the district-year sample. Panel A covers K-8 districts, and Panel B covers 9-12 districts. The first three columns summarize all district-years. The remaining columns report means for the three exposure groups used in the analysis: districts below 55%, between 55 and 80%, and above 80% in baseline unduplicated pupil share.

The table includes several constructed measures. Because the migration measures are less standard, I describe them here. Net migration rate is net in-migration divided by district population. The income and minority gaps compare the flow-weighted neighborhood characteristics of migration into and out of each district-year. These measures describe the neighborhoods linked by migration flows, not the individual income or race of migrants.

Districts with higher LCFF exposure were more disadvantaged at baseline, with higher FRPM shares and lower home values. Appendix Figure 4 shows that the highest-exposure districts also had relatively high revenue and spending before LCFF. After 2013, revenue and spending rise fastest in these districts.

3 Empirical Framework

I begin with an event-study difference-in-differences design that uses cross-district variation in LCFF exposure. I estimate the model separately for K-8 and 9-12 districts to match the way the housing measures are constructed.:

$$Y_{dt} = \sum_{j=1}^{2} \sum_{k \in \mathcal{K}} \beta_{j,k} \left(\mathbb{1}\{t - t_0 = k\} \times \text{Tier}_{jd} \right) + \gamma_d + \lambda_t + \varepsilon_{dt}. \tag{1}$$

Here, Y_{dt} is the outcome of interest for district d in year t. Throughout the paper, years refer to school-year end. Thus, 2013 denotes the 2012–13 school year, and 2014 denotes 2013–14, the first year of LCFF implementation. The set $\mathcal{K} = \{-3, -2, 0, 1, \ldots, 5\}$ indexes event time relative to 2013–14, with $k = -1$ denoting 2012–13, the omitted reference year.

Tier_{jd} is an indicator equal to 1 if district d belongs to tier j. Tier 1 includes districts with an unduplicated pupil share between 55 and 80 %, and Tier 2 includes districts with a share above 80 %. Districts below 55% form the omitted group. Because LCFF was implemented statewide, the design compares districts that were more exposed to the new funding formula with districts that were less exposed. I split treated districts into two tiers to allow treatment effects to vary with baseline exposure.

Table 1: Descriptive statistics on school districts

Variable	Full sample, 2011-2019			Subgroup means by tier		
	Obs.	Mean	SD	Control (<55%)	Tier 1 (55-80%)	Tier 2 (80%+)
Panel A. K-8 elementary districts						
Treatment and district size:						
Unduplicated pupil share	5,620	0.594	0.254	0.336	0.668	0.887
Average daily attendance	5,620	7,861	22,733	6,322	7,787	10,181
District finances:						
Revenue per pupil ($)	5,620	11,675	3,038	11,406	11,748	11,984
Spending per pupil ($)	5,620	11,436	2,906	11,205	11,509	11,692
K-12 spending per pupil ($)	5,620	11,133	2,768	10,954	11,168	11,354
Enrollment and composition:						
Enrollment	5,618	9,485	31,154	7,533	9,505	12,305
Public school share	5,618	0.944	0.078	0.918	0.948	0.978
FRPM share	5,618	0.521	0.253	0.273	0.582	0.812
Residential migration:						
Net migration rate (pp)	5,616	0.187	2.748	1.169	0.325	−1.395
Income gap, in minus out ($)	5,616	−389	719	−358	−300	−535
Minority gap, in minus out (pp)	5,616	0.290	0.293	0.292	0.297	0.279
Housing market:						
Home value index ($)	5,595	463,006	409,565	684,157	367,667	249,976
Distinct districts	628			256	196	176
Panel B. 9-12 high school districts						
Treatment and district size:						
Unduplicated pupil share	3,463	0.583	0.230	0.351	0.670	0.875
Average daily attendance	3,463	11,245	28,451	8,990	10,539	16,866
District finances:						
Revenue per pupil ($)	3,463	12,277	3,301	11,987	12,392	12,629
Spending per pupil ($)	3,463	12,042	3,105	11,772	12,169	12,331
K-12 spending per pupil ($)	3,463	11,666	2,861	11,474	11,733	11,915
Enrollment and composition:						
Enrollment	3,463	13,722	39,115	10,880	12,848	20,775
Public school share	3,463	0.947	0.071	0.925	0.955	0.978
FRPM share	3,463	0.509	0.228	0.288	0.587	0.795
Residential migration:						
Net migration rate (pp)	3,459	0.225	2.283	0.876	0.245	−1.069
Income gap, in minus out ($)	3,459	−330	618	−311	−291	−439
Minority gap, in minus out (pp)	3,459	0.282	0.266	0.280	0.286	0.280
Housing market:						
Home value index ($)	3,459	439,183	341,758	632,418	347,684	231,665
Distinct districts	386			157	147	82

Notes: Descriptive statistics on California school districts in the 2011–2019 DiD sample. Panel A covers K–8 elementary and unified districts. Panel B covers 9–12 high school and unified districts, so unified districts appear in both panels. Control, Tier 1, and Tier 2 are defined by the 2013–14 unduplicated pupil share: below 55%, 55–80%, and 80% and above. Per-pupil finance variables divide general fund totals by average daily attendance. Enrollment is total public and private enrollment. Public school share is public enrollment divided by total enrollment. FRPM share is measured among public school students. Dollar values are in constant 2020 dollars. Net migration rate and minority gap are reported in percentage points. See Section 2 for data sources and variable definitions.

The coefficients of interest, $\beta_{j,k}$, capture how outcomes in tier-j districts changed relative to the omitted group in event year k, using 2012–13 as the reference year. District fixed effects γ_d absorb time-invariant district characteristics, and year fixed effects λ_t absorb common shocks across all districts. Standard errors are clustered at the district level. This strategy relies on a parallel trends assumption. Without LCFF, outcomes in districts with different baseline exposure would have evolved similarly over time.

The event-study results show how spending and sorting outcomes vary with baseline LCFF exposure. They also naturally lead to a more direct question: did districts with larger spending increases also see larger changes in housing values? To study that relationship, I collapse the panel to a single cross-section and compute the change in each variable between 2013, the last pre-LCFF year, and 2019, the last year in the migration sample. I then estimate

$$\Delta Y_d = \beta \, \Delta S_d + \mu_c + \varepsilon_d, \tag{2}$$

where ΔY_d is the percent change in the Zillow Home Value Index for district d. ΔS_d is the percent change in per-pupil K-12 spending. County fixed effects μ_c absorb common local trends in housing markets. The coefficient β summarizes the relationship within counties between spending growth and changes in home values. All regressions are weighted by 2013 average daily attendance, and standard errors are clustered at the county level. I restrict observations with percent changes below the 1st or above the 99th percentile to limit the influence of extreme values driven by small denominators.

This specification is descriptive. It does not isolate the effect of spending growth, since districts with larger funding increases may also differ in other ways that affect housing demand. Still, it helps show whether housing values changed more in districts with larger spending increases. I report these estimates alongside scatter plots of the county-residualized data to show that relationship.

4 Results

4.1 Main Results

I begin with district finances results. LCFF's effects on other outcomes depend on whether the reform actually shifted resources. In both K–8 and 9–12 districts, revenue and spending rise more in districts with higher baseline shares of unduplicated pupils, and the increases are largest in the highest-exposure tier. Tables 2 and 3 show that these differences are economically meaningful. By 2019, five years after LCFF began, revenue per pupil is about $1,000 higher in the 55 to 80% tier and roughly $1,600 in the above-80% tier. Spending follows a similar pattern. Pre-period coefficients are small and mostly flat for the 55 to 80% tier. For the above-80% tier, revenue and spending show some positive movement before LCFF, so the magnitudes should be read with this caveat in mind.

Home values rise more after LCFF in districts with greater exposure to the reform. The effect

is still clear at the end of the sample. By 2019, the log home value index is about 0.073 higher in the 55 to 80% tier and about 0.126 higher in the above-80% tier for K-8 districts. These estimates imply roughly 8% and 13% higher home values. The corresponding estimates for 9-12 districts are similar. Prior work shows that LCFF raised test scores in high-UPP districts (Lafortune and Mehlotra, 2021). One interpretation is that households valued the school improvements associated with the reform. The estimates are slightly larger for 9-12 districts, perhaps because home buyers pay more attention to district-level school quality when students are older. [5]

The next question is whether the housing response was accompanied by residential mobility or compositional sorting. Net in-migration into more exposed districts is positive after the reform. The estimates are largest around 2016 and 2017, reaching about 0.5 percentage points in both K-8 and 9-12 districts. They remain positive by 2019, but the magnitudes are smaller.[6] The 55 to 80% tier also shows positive estimates, but they are smaller and less consistently significant. These results suggest some mobility response, but not a large one.

Small net flows could still matter if the inflows and outflows come from different types of neighborhoods. To examine this, I compare the block-group characteristics associated with migration into and out of each district-year. These measures are neighborhood-level proxies, not individual characteristics of migrants. If LCFF changed the composition of migration flows, the income or minority gaps between incoming and outgoing flows should widen after the reform. The estimates show little movement in these gaps.[7] The reform appears to have changed the volume of migration more than the composition of migration flows.

I also find little evidence of a sorting response in student composition measures. The change in the public school share remains close to zero, and the share of students eligible for free or reduced-price meals changes little after the reform. This does not rule out household responses to school characteristics that are harder to observe, but those responses do not appear in the observed composition measures. Total enrollment declines more in the exposed districts, especially in the highest-exposure tier. I do not interpret those declines as clear LCFF effects. Treated districts were already shrinking before the reform, and the migration data cover all residents, not just families with school-age children.

Given that the clearest response is in housing prices, I next ask how directly the spending increase maps onto home value growth. Districts with larger spending increases also saw larger gains in home values, as Figure 3 illustrates. This relationship is descriptive because spending growth is endogenous, as discussed in Section 3. Still, it lines up with the event-study result that home values increase most in the districts most exposed to LCFF.

[5] See Crone (2006), who finds stronger boundary-based evidence for high-school test scores than for elementary-school scores.

[6] For a district with 50,000 residents, a 0.5 percentage point increase implies about 250 additional net in-migrants per year.

[7] The 2019 income-gap estimates for the most exposed districts are about −$120 to −$135 and statistically significant, but small: roughly 0.2 standard deviations of the income gap, and this group already shows a positive gap before the reform. Minority-gap intervals include zero, with upper bounds of 0.05 to 0.07 percentage points.

4.2 Robustness and Heterogeneity

My results are robust in several alternative specifications. Restricting the sample to unified districts and adding county-by-year fixed effects do not change the main conclusion. The county-by-year specification compares districts within the same county and year, so it absorbs broad local housing-market shocks. The top-tier home value estimates become larger in this specification. This makes it less likely that the housing result is driven by cross-county differences. Tables 4 to 6 report these estimates.

Appendix Tables A2 to A3 also report estimates without ADA weights. These estimates give the same weight to small and large districts, rather than weighting districts by the number of students they serve. The funding pattern remains clear, but the estimates for the 55 to 80% tier become smaller. This is consistent with the descriptive statistics in Table 1, which show that larger districts are more common in the higher exposure groups. The unweighted estimates are therefore a useful check, but I do not treat them as the main specification.

This difference also helps motivate the size analysis in the main text. The size split shows that the housing response is clearer in larger districts. Migration estimates are also somewhat more positive there, while student composition barely moves in both small and large districts. The appendix reports a related split by urban-rural status. Those estimates show a similar pattern: the housing and migration responses are more visible in urban districts, while student composition remains stable. See Appendix Tables A4 to A6.

5 Conclusion

This paper studies how households responded to California's Local Control Funding Formula, a reform that raised school funding in disadvantaged districts through state aid rather than local taxes. Because residents of more exposed districts did not face a higher tax burden, the reform isolates household responses to additional school spending. Revenue per pupil rose substantially in the most exposed districts, and home values rose with exposure. The evidence for residential sorting is more limited. Net in-migration into more exposed districts turned positive after the reform but stayed small, the neighborhoods that migrants left and entered changed little, and enrollment in exposed districts continued to decline along pre-reform trends.

These results complement Ainsworth and Clark (2025), who find little residential sorting in response to parcel taxes, which combine higher spending with higher local taxes. Under LCFF, where spending rose without higher local taxes, sorting also remained small.

In contrast, Chakrabarti and Roy (2015) find both capitalization and sorting after Michigan's Proposal A. One explanation is housing supply. Housing supply is more constrained in much of California (Saiz, 2010), so a funding increase of this size may be absorbed by prices rather than by new residents, consistent with the inelastic case in the model of Chakrabarti and Roy (2015). I also cannot rule out that my data miss the relevant margin. The migration measures cover all households, not just families with school-age children, so sorting among the households most likely

to respond could go undetected.

Future work could use local variation in housing supply elasticity or microdata on household composition to examine when school funding increases lead to capitalization, sorting, or both.

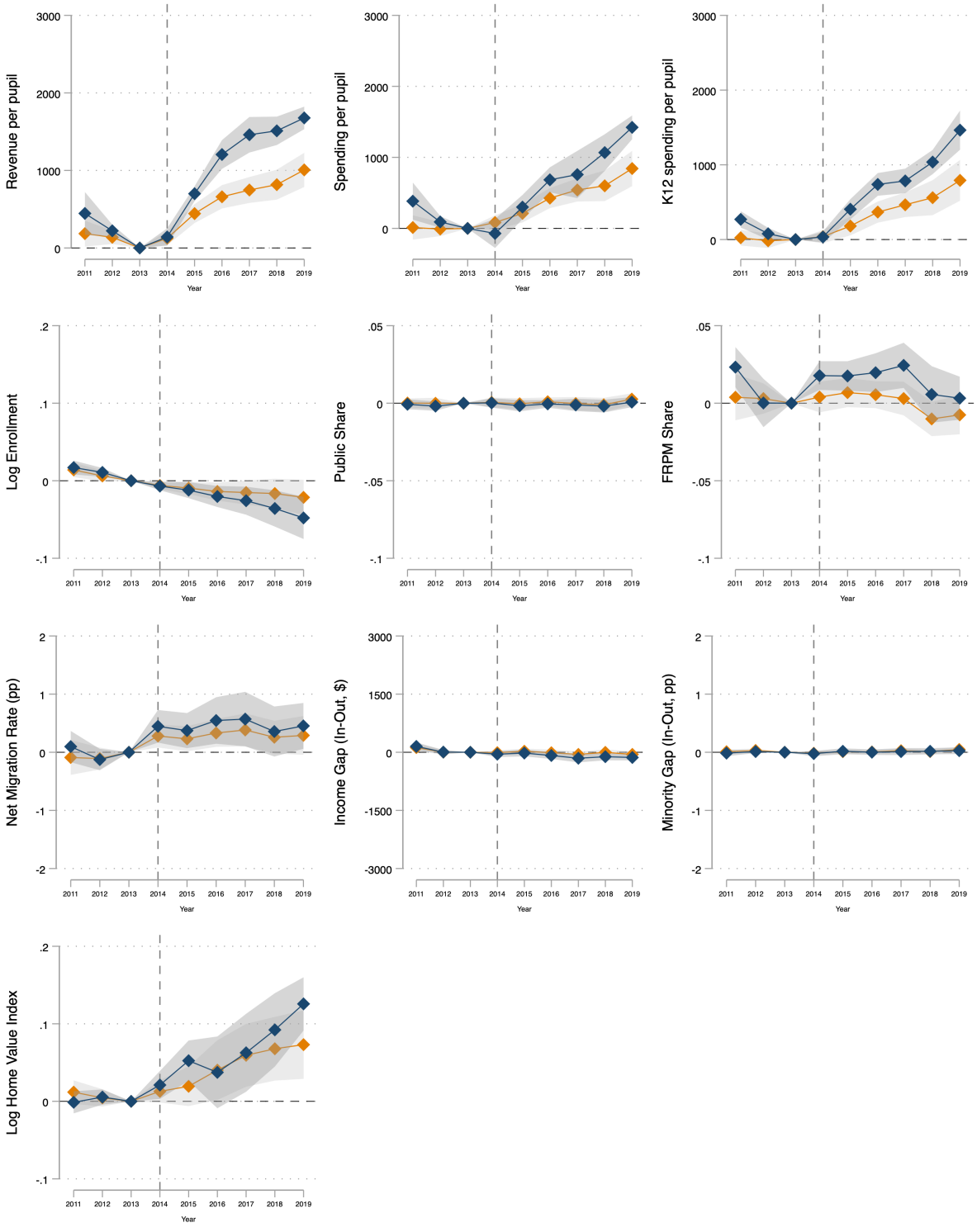


Figure 1: Event Study Estimates: K–8 Districts

Notes: Each panel plots the DiD coefficients and 95% confidence intervals by school-year end, with 2013 (school year 2012–13) as the omitted base year. The dashed vertical line indicates the start of LCFF implementation. Orange represents the treated 55–80% UPP group, and blue represents the treated 80%+ UPP group. Columns are grouped by data source.

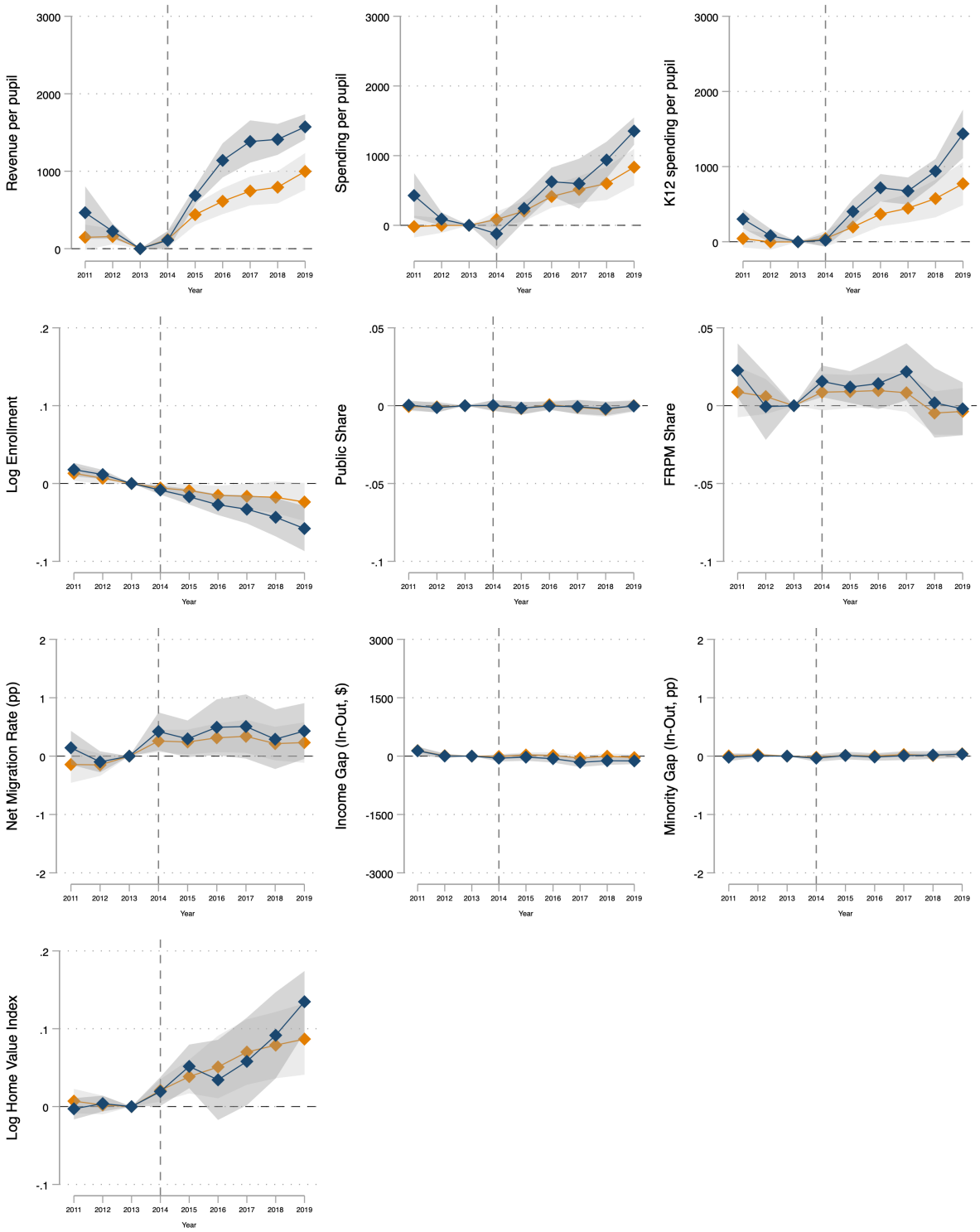


Figure 2: Event Study Estimates: 9–12 Districts

Notes: See notes to Figure 1.

Table 2: DiD Event Study Estimates — K–8 Elementary Districts

	Funding ($)			Enrollment & Composition			Housing & Migration			
	Revenue Per Pupil (1)	Spending Per Pupil (2)	K-12 Spending Per Pupil (3)	Log Enrollment (4)	Public Share (5)	FRPM Share (6)	Net Migration Rate (pp) (7)	Income Gap (In–Out, $) (8)	Minority Gap (In–Out, pp) (9)	Log Home Value Index (10)
Panel A: Treated (55–80%) × Year										
2011	185**	12	24	0.014***	0.000	0.004	−0.090	121***	0.010	0.012
	(85)	(87)	(53)	(0.005)	(0.001)	(0.008)	(0.152)	(43)	(0.020)	(0.008)
2012	137***	−10	−17	0.006*	−0.000	0.003	−0.108	3	0.032**	0.004
	(44)	(50)	(49)	(0.004)	(0.002)	(0.005)	(0.095)	(30)	(0.016)	(0.006)
2014	125***	83	39	−0.006**	0.001	0.004	0.279***	−10	−0.017	0.013*
	(46)	(51)	(45)	(0.002)	(0.002)	(0.005)	(0.100)	(29)	(0.015)	(0.007)
2015	443***	212***	181***	−0.009**	−0.000	0.007	0.231**	27	0.011	0.019
	(62)	(65)	(64)	(0.004)	(0.002)	(0.005)	(0.110)	(34)	(0.016)	(0.013)
2016	660***	426***	368***	−0.014**	0.001	0.005	0.332***	−11	0.005	0.040**
	(76)	(72)	(72)	(0.006)	(0.001)	(0.004)	(0.122)	(40)	(0.016)	(0.020)
2017	748***	541***	465***	−0.015*	−0.000	0.003	0.384***	−54	0.027	0.059***
	(85)	(83)	(84)	(0.008)	(0.002)	(0.006)	(0.144)	(37)	(0.019)	(0.021)
2018	817***	600***	560***	−0.016*	−0.001	−0.010*	0.258*	−2	0.012	0.068***
	(98)	(110)	(119)	(0.010)	(0.002)	(0.006)	(0.144)	(37)	(0.019)	(0.021)
2019	1,007***	843***	792***	−0.021*	0.003	−0.008	0.290*	−55	0.049**	0.073***
	(112)	(126)	(139)	(0.012)	(0.002)	(0.006)	(0.169)	(39)	(0.021)	(0.022)
Panel B: Treated (80%+) × Year										
2011	446***	383***	270***	0.017***	−0.001	0.023***	0.098	152***	−0.015	−0.001
	(140)	(133)	(55)	(0.005)	(0.002)	(0.007)	(0.137)	(52)	(0.027)	(0.007)
2012	222***	91**	78*	0.011***	−0.002	0.000	−0.126	4	0.012	0.006
	(40)	(42)	(40)	(0.003)	(0.002)	(0.008)	(0.093)	(27)	(0.013)	(0.005)
2014	142**	−72	34	−0.007**	0.000	0.018***	0.446***	−50	−0.023	0.021**
	(57)	(104)	(38)	(0.003)	(0.002)	(0.005)	(0.143)	(39)	(0.024)	(0.009)
2015	700***	300***	406***	−0.012**	−0.002	0.018***	0.373**	−20	0.017	0.052***
	(48)	(87)	(69)	(0.005)	(0.002)	(0.005)	(0.154)	(44)	(0.026)	(0.013)
2016	1,206***	684***	738***	−0.020***	−0.000	0.020***	0.547***	−84*	0.001	0.037
	(94)	(90)	(77)	(0.007)	(0.001)	(0.006)	(0.203)	(46)	(0.024)	(0.024)
2017	1,460***	758***	785***	−0.026***	−0.001	0.024***	0.571**	−154***	0.011	0.063**
	(117)	(169)	(83)	(0.009)	(0.002)	(0.007)	(0.239)	(55)	(0.030)	(0.026)
2018	1,511***	1,068***	1,037***	−0.036***	−0.002	0.006	0.357	−114**	0.017	0.092***
	(93)	(131)	(82)	(0.012)	(0.002)	(0.009)	(0.219)	(48)	(0.027)	(0.024)
2019	1,677***	1,423***	1,464***	−0.048***	0.001	0.003	0.454**	−135***	0.027	0.126***
	(74)	(85)	(133)	(0.014)	(0.002)	(0.007)	(0.201)	(38)	(0.029)	(0.017)
Observations	5,620	5,620	5,620	5,618	5,618	5,618	5,616	5,616	5,616	5,591
Clusters	628	628	628	628	628	628	628	628	628	628

Notes: Each column reports coefficients from a separate regression of the outcome on treatment tier × year interactions, with 2013 as the omitted base year. All regressions include district and year fixed effects and are weighted by ADA. Standard errors clustered at the district level are in parentheses. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value Index is the natural log of the Zillow Home Value Index (2020$). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: DiD Event Study Estimates — 9–12 High School Districts

	Funding ($)			Enrollment & Composition			Housing & Migration			
	Revenue Per Pupil (1)	Spending Per Pupil (2)	K-12 Spending Per Pupil (3)	Log Enrollment (4)	Public Share (5)	FRPM Share (6)	Net Migration Rate (pp) (7)	Income Gap (In–Out, $) (8)	Minority Gap (In–Out, pp) (9)	Log Home Value Index (10)
Panel A: Treated (55–80%) × Year										
2011	147*	−18	43	0.013***	−0.001	0.009	−0.144	142***	0.004	0.007
	(82)	(79)	(60)	(0.005)	(0.001)	(0.008)	(0.159)	(46)	(0.020)	(0.008)
2012	157***	−2	−3	0.007*	−0.001	0.006	−0.150	12	0.024	0.002
	(51)	(54)	(53)	(0.004)	(0.002)	(0.006)	(0.096)	(31)	(0.016)	(0.006)
2014	119**	84	43	−0.006**	−0.000	0.009	0.259***	−8	−0.020	0.021***
	(53)	(58)	(53)	(0.003)	(0.002)	(0.006)	(0.097)	(28)	(0.016)	(0.007)
2015	440***	204***	197***	−0.009**	−0.002	0.009*	0.242**	30	0.013	0.039***
	(69)	(73)	(72)	(0.004)	(0.002)	(0.005)	(0.107)	(34)	(0.017)	(0.011)
2016	612***	414***	368***	−0.015**	0.001	0.010*	0.314**	17	0.002	0.051**
	(85)	(82)	(83)	(0.006)	(0.001)	(0.006)	(0.125)	(42)	(0.017)	(0.020)
2017	745***	514***	447***	−0.016**	−0.001	0.008	0.339**	−50	0.027	0.070***
	(94)	(97)	(96)	(0.008)	(0.002)	(0.006)	(0.143)	(37)	(0.019)	(0.021)
2018	793***	601***	575***	−0.018*	−0.002	−0.005	0.214	−2	0.009	0.079***
	(106)	(120)	(127)	(0.010)	(0.002)	(0.007)	(0.146)	(37)	(0.020)	(0.022)
2019	997***	835***	772***	−0.024*	0.000	−0.004	0.232	−37	0.040*	0.087***
	(121)	(133)	(145)	(0.012)	(0.002)	(0.008)	(0.176)	(40)	(0.022)	(0.023)
Panel B: Treated (80%+) × Year										
2011	465***	428***	302***	0.018***	0.000	0.023**	0.143	137**	−0.019	−0.003
	(173)	(163)	(64)	(0.004)	(0.002)	(0.009)	(0.147)	(56)	(0.033)	(0.007)
2012	226***	90*	82*	0.012***	−0.001	−0.001	−0.098	2	0.005	0.004
	(48)	(48)	(45)	(0.003)	(0.002)	(0.011)	(0.092)	(30)	(0.015)	(0.005)
2014	109*	−122	21	−0.008***	0.000	0.016***	0.422**	−52	−0.035	0.019**
	(61)	(117)	(44)	(0.003)	(0.002)	(0.005)	(0.166)	(44)	(0.031)	(0.009)
2015	686***	244**	402***	−0.017***	−0.002	0.012**	0.296*	−26	0.011	0.052***
	(58)	(97)	(85)	(0.005)	(0.002)	(0.005)	(0.160)	(47)	(0.033)	(0.014)
2016	1,140***	625***	718***	−0.027***	−0.000	0.014*	0.494**	−65	−0.015	0.034
	(112)	(103)	(93)	(0.007)	(0.001)	(0.008)	(0.243)	(51)	(0.032)	(0.026)
2017	1,383***	598***	675***	−0.033***	−0.001	0.022**	0.507*	−158**	0.008	0.058**
	(139)	(182)	(91)	(0.009)	(0.002)	(0.009)	(0.279)	(62)	(0.039)	(0.029)
2018	1,411***	937***	939***	−0.043***	−0.002	0.002	0.290	−118**	0.019	0.092***
	(102)	(133)	(83)	(0.012)	(0.002)	(0.011)	(0.259)	(54)	(0.033)	(0.028)
2019	1,573***	1,353***	1,437***	−0.058***	−0.000	−0.002	0.430*	−123***	0.035	0.135***
	(83)	(99)	(164)	(0.015)	(0.002)	(0.009)	(0.243)	(43)	(0.034)	(0.020)
Observations	3,463	3,463	3,463	3,463	3,463	3,463	3,459	3,459	3,459	3,459
Clusters	386	386	386	386	386	386	386	386	386	386

Notes: Each column reports coefficients from a separate regression of the outcome on treatment tier × year interactions, with 2013 as the omitted base year. All regressions include district and year fixed effects and are weighted by ADA. Standard errors clustered at the district level are in parentheses. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value Index is the natural log of the Zillow Home Value Index (2020$). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Robustness: Unified Districts Only

	Funding ($)			Enrollment & Composition			Housing & Migration			
	Revenue Per Pupil (1)	Spending Per Pupil (2)	K-12 Spending Per Pupil (3)	Log Enrollment (4)	Public Share (5)	FRPM Share (6)	Net Migration Rate (pp) (7)	Income Gap (In–Out, $) (8)	Minority Gap (In–Out, pp) (9)	Log Home Value Index (10)
Panel A: Treated (55–80%) × Year										
2011	147*	−67	−2	0.014**	−0.000	0.007	−0.190	137***	0.004	0.009
	(84)	(86)	(65)	(0.006)	(0.002)	(0.009)	(0.184)	(51)	(0.024)	(0.008)
2012	165***	−29	−34	0.007	−0.001	0.005	−0.164	1	0.029	0.001
	(53)	(61)	(60)	(0.004)	(0.002)	(0.006)	(0.112)	(35)	(0.019)	(0.006)
2014	153***	110*	60	−0.006**	−0.000	0.004	0.276**	3	−0.034*	0.018**
	(56)	(60)	(53)	(0.003)	(0.002)	(0.006)	(0.111)	(33)	(0.018)	(0.009)
2015	492***	215***	196**	−0.010**	−0.002	0.007	0.212*	49	0.002	0.036***
	(72)	(78)	(77)	(0.005)	(0.002)	(0.006)	(0.123)	(38)	(0.019)	(0.013)
2016	675***	444***	386***	−0.017**	0.001	0.005	0.310**	41	−0.009	0.065***
	(92)	(86)	(87)	(0.007)	(0.002)	(0.005)	(0.143)	(49)	(0.019)	(0.022)
2017	788***	549***	470***	−0.018*	−0.001	0.002	0.392**	−44	0.024	0.085***
	(102)	(99)	(100)	(0.009)	(0.003)	(0.006)	(0.163)	(42)	(0.023)	(0.024)
2018	883***	642***	611***	−0.021*	−0.002	−0.012*	0.234	16	0.009	0.094***
	(117)	(132)	(143)	(0.012)	(0.003)	(0.006)	(0.164)	(43)	(0.023)	(0.024)
2019	1,106***	918***	880***	−0.027**	0.002	−0.009	0.240	−27	0.046*	0.103***
	(133)	(151)	(167)	(0.014)	(0.002)	(0.007)	(0.199)	(46)	(0.026)	(0.026)
Panel B: Treated (80%+) × Year										
2011	472***	405**	267***	0.019***	0.000	0.023***	0.129	154**	−0.023	−0.007
	(174)	(167)	(69)	(0.005)	(0.002)	(0.009)	(0.164)	(60)	(0.034)	(0.007)
2012	219***	72	62	0.013***	−0.001	−0.002	−0.120	7	0.005	0.001
	(52)	(53)	(50)	(0.003)	(0.002)	(0.011)	(0.101)	(33)	(0.016)	(0.005)
2014	122**	−108	36	−0.009***	0.000	0.018***	0.435**	−54	−0.038	0.019*
	(62)	(116)	(44)	(0.003)	(0.002)	(0.005)	(0.172)	(46)	(0.033)	(0.010)
2015	694***	261***	417***	−0.018***	−0.002	0.016***	0.272	−14	0.010	0.052***
	(61)	(98)	(90)	(0.005)	(0.002)	(0.005)	(0.171)	(51)	(0.034)	(0.015)
2016	1,159***	656***	744***	−0.028***	−0.000	0.018**	0.485*	−58	−0.020	0.048*
	(113)	(106)	(98)	(0.007)	(0.001)	(0.008)	(0.250)	(55)	(0.034)	(0.026)
2017	1,422***	628***	712***	−0.034***	−0.001	0.025***	0.563*	−158**	0.008	0.071**
	(141)	(184)	(97)	(0.010)	(0.003)	(0.009)	(0.295)	(66)	(0.040)	(0.029)
2018	1,454***	963***	970***	−0.045***	−0.002	0.007	0.331	−108*	0.018	0.104***
	(104)	(135)	(87)	(0.013)	(0.003)	(0.011)	(0.270)	(58)	(0.035)	(0.028)
2019	1,664***	1,439***	1,524***	−0.059***	0.000	0.004	0.453*	−122***	0.035	0.148***
	(83)	(101)	(167)	(0.015)	(0.002)	(0.009)	(0.254)	(47)	(0.036)	(0.020)
Observations	2,806	2,806	2,806	2,806	2,806	2,806	2,802	2,802	2,802	2,802
Clusters	313	313	313	313	313	313	313	313	313	313

Notes: The sample is restricted to unified school districts. Each column reports coefficients from a separate regression of the outcome on treatment tier × year interactions, with 2013 as the omitted base year. All regressions include district and year fixed effects and are weighted by ADA. Standard errors clustered at the district level are in parentheses. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value Index is the natural log of the Zillow Home Value Index (2020$). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Robustness: County × Year Fixed Effects — K–8 Districts

	Funding ($)			Enrollment & Composition			Housing & Migration			
	Revenue Per Pupil (1)	Spending Per Pupil (2)	K-12 Spending Per Pupil (3)	Log Enrollment (4)	Public Share (5)	FRPM Share (6)	Net Migration Rate (pp) (7)	Income Gap (In–Out, $) (8)	Minority Gap (In–Out, pp) (9)	Log Home Value Index (10)
Panel A: Treated (55–80%) × Year										
2011	153	18	13	0.015***	−0.000	0.001	−0.094	84*	0.016	0.013**
	(111)	(119)	(58)	(0.004)	(0.002)	(0.007)	(0.165)	(46)	(0.017)	(0.005)
2012	100**	−33	−37	0.007**	−0.000	0.001	−0.174	1	0.035**	0.001
	(43)	(48)	(46)	(0.003)	(0.002)	(0.005)	(0.112)	(33)	(0.016)	(0.002)
2014	118**	71	27	−0.007***	−0.001	0.003	0.239**	−25	−0.025	0.013***
	(49)	(56)	(49)	(0.003)	(0.002)	(0.005)	(0.109)	(33)	(0.016)	(0.005)
2015	430***	217***	188***	−0.012***	−0.002	0.004	0.145	27	0.001	0.019***
	(59)	(74)	(68)	(0.004)	(0.002)	(0.005)	(0.130)	(37)	(0.018)	(0.007)
2016	703***	502***	458***	−0.019***	0.000	0.003	0.202	−16	−0.008	0.038***
	(71)	(73)	(69)	(0.006)	(0.001)	(0.005)	(0.139)	(42)	(0.018)	(0.012)
2017	834***	627***	571***	−0.025***	−0.001	0.000	0.229	−79*	0.027	0.054***
	(99)	(95)	(89)	(0.008)	(0.003)	(0.006)	(0.164)	(41)	(0.019)	(0.013)
2018	926***	717***	653***	−0.027***	−0.003	−0.015**	0.085	−36	0.010	0.066***
	(110)	(109)	(105)	(0.009)	(0.003)	(0.006)	(0.170)	(37)	(0.019)	(0.014)
2019	1,018***	876***	792***	−0.037***	0.002	−0.012*	0.090	−92**	0.048**	0.070***
	(117)	(124)	(120)	(0.010)	(0.002)	(0.006)	(0.188)	(44)	(0.020)	(0.014)
Panel B: Treated (80%+) × Year										
2011	395**	367**	227***	0.012***	−0.001	0.028***	0.037	136***	−0.026	−0.016***
	(160)	(165)	(58)	(0.004)	(0.002)	(0.007)	(0.170)	(46)	(0.018)	(0.004)
2012	196***	51	35	0.008***	−0.001	0.000	−0.255**	7	−0.002	−0.012***
	(45)	(44)	(45)	(0.003)	(0.001)	(0.007)	(0.109)	(34)	(0.017)	(0.002)
2014	133***	−60	23	−0.005	0.001	0.019***	0.537***	−51	−0.030	0.039***
	(51)	(77)	(46)	(0.003)	(0.001)	(0.006)	(0.121)	(36)	(0.019)	(0.004)
2015	687***	319***	395***	−0.008*	−0.001	0.019***	0.382***	29	−0.007	0.072***
	(54)	(80)	(69)	(0.005)	(0.002)	(0.006)	(0.142)	(39)	(0.022)	(0.006)
2016	1,194***	713***	760***	−0.016**	0.001	0.016**	0.502***	−39	−0.021	0.098***
	(81)	(80)	(69)	(0.006)	(0.001)	(0.007)	(0.157)	(40)	(0.020)	(0.009)
2017	1,505***	879***	891***	−0.024***	0.000	0.017**	0.364**	−114**	−0.001	0.126***
	(90)	(116)	(85)	(0.008)	(0.002)	(0.007)	(0.177)	(45)	(0.024)	(0.010)
2018	1,528***	1,204***	1,167***	−0.033***	−0.000	−0.007	0.112	−79*	0.009	0.151***
	(89)	(104)	(85)	(0.010)	(0.002)	(0.008)	(0.181)	(41)	(0.021)	(0.011)
2019	1,774***	1,607***	1,617***	−0.047***	0.001	−0.012	0.116	−117***	0.005	0.178***
	(85)	(96)	(112)	(0.011)	(0.002)	(0.007)	(0.173)	(41)	(0.020)	(0.011)
Observations	5,557	5,557	5,557	5,555	5,555	5,555	5,551	5,551	5,551	5,526
Clusters	621	621	621	621	621	621	621	621	621	621

Notes: Each column reports coefficients from a separate regression of the outcome on treatment tier × year interactions, with 2013 as the omitted base year. All regressions include district and county-by-year fixed effects and are weighted by ADA. Standard errors clustered at the district level are in parentheses. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value Index is the natural log of the Zillow Home Value Index (2020$). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Robustness: County × Year Fixed Effects — 9–12 Districts

	Funding ($)			Enrollment & Composition			Housing & Migration			
	Revenue Per Pupil (1)	Spending Per Pupil (2)	K-12 Spending Per Pupil (3)	Log Enrollment (4)	Public Share (5)	FRPM Share (6)	Net Migration Rate (pp) (7)	Income Gap (In–Out, $) (8)	Minority Gap (In–Out, pp) (9)	Log Home Value Index (10)
Panel A: Treated (55–80%) × Year										
2011	115	4	36	0.016***	−0.001	0.008	−0.173	123**	0.004	0.007
	(87)	(99)	(68)	(0.005)	(0.001)	(0.009)	(0.180)	(50)	(0.019)	(0.006)
2012	111**	−12	−20	0.008**	−0.002	0.006	−0.223*	20	0.022	−0.001
	(49)	(56)	(54)	(0.003)	(0.002)	(0.006)	(0.117)	(34)	(0.017)	(0.003)
2014	133**	109*	69	−0.007**	−0.001	0.010	0.220**	−23	−0.033*	0.022***
	(53)	(62)	(54)	(0.003)	(0.002)	(0.006)	(0.109)	(32)	(0.017)	(0.005)
2015	442***	239***	231***	−0.011**	−0.004*	0.010	0.193	38	−0.002	0.034***
	(67)	(82)	(76)	(0.005)	(0.002)	(0.006)	(0.129)	(35)	(0.019)	(0.007)
2016	671***	521***	492***	−0.021***	−0.000	0.009	0.211	20	−0.015	0.063***
	(80)	(79)	(76)	(0.006)	(0.001)	(0.007)	(0.142)	(43)	(0.019)	(0.012)
2017	867***	621***	568***	−0.027***	−0.002	0.007	0.181	−79*	0.019	0.081***
	(105)	(101)	(95)	(0.008)	(0.003)	(0.007)	(0.160)	(44)	(0.021)	(0.013)
2018	920***	722***	675***	−0.028***	−0.004	−0.007	0.021	−34	0.001	0.092***
	(119)	(126)	(120)	(0.010)	(0.003)	(0.008)	(0.177)	(38)	(0.021)	(0.014)
2019	1,042***	881***	806***	−0.037***	−0.001	−0.008	0.006	−60	0.035	0.097***
	(127)	(134)	(128)	(0.011)	(0.002)	(0.008)	(0.199)	(45)	(0.021)	(0.015)
Panel B: Treated (80%+) × Year										
2011	398**	420**	292***	0.014***	0.000	0.025***	0.060	158***	−0.033	−0.015***
	(199)	(204)	(73)	(0.005)	(0.002)	(0.009)	(0.196)	(55)	(0.021)	(0.006)
2012	204***	74	63	0.009**	−0.001	−0.002	−0.243**	19	−0.013	−0.011***
	(58)	(54)	(55)	(0.003)	(0.002)	(0.010)	(0.114)	(39)	(0.020)	(0.004)
2014	120**	−79	35	−0.006*	0.001	0.018***	0.521***	−59	−0.041*	0.038***
	(59)	(94)	(58)	(0.004)	(0.002)	(0.006)	(0.131)	(41)	(0.024)	(0.005)
2015	671***	253***	375***	−0.012**	−0.002	0.014**	0.319**	23	−0.003	0.073***
	(70)	(97)	(87)	(0.005)	(0.002)	(0.006)	(0.144)	(40)	(0.026)	(0.007)
2016	1,125***	665***	737***	−0.022***	0.000	0.009	0.448**	−13	−0.032	0.105***
	(101)	(93)	(85)	(0.007)	(0.001)	(0.010)	(0.178)	(47)	(0.026)	(0.010)
2017	1,444***	712***	762***	−0.028***	−0.000	0.013	0.312*	−107*	−0.000	0.134***
	(110)	(122)	(94)	(0.009)	(0.002)	(0.010)	(0.188)	(55)	(0.031)	(0.012)
2018	1,414***	1,014***	1,005***	−0.034***	−0.001	−0.010	0.060	−69	0.016	0.161***
	(110)	(111)	(96)	(0.011)	(0.002)	(0.012)	(0.209)	(49)	(0.026)	(0.013)
2019	1,648***	1,480***	1,531***	−0.047***	−0.001	−0.017*	0.100	−88*	0.011	0.191***
	(104)	(115)	(139)	(0.013)	(0.002)	(0.010)	(0.197)	(47)	(0.024)	(0.013)
Observations	3,382	3,382	3,382	3,382	3,382	3,382	3,376	3,376	3,376	3,376
Clusters	377	377	377	377	377	377	377	377	377	377

Notes: Each column reports coefficients from a separate regression of the outcome on treatment tier × year interactions, with 2013 as the omitted base year. All regressions include district and county-by-year fixed effects and are weighted by ADA. Standard errors clustered at the district level are in parentheses. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value Index is the natural log of the Zillow Home Value Index (2020$). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: DiD Heterogeneity by District Size — K–8 Districts

Panel A: Finance, Enrollment, and Composition

	Small Districts (Below Median)						Large Districts (Above Median)					
	(1) Revenue Per Pupil	(2) Spending Per Pupil	(3) K-12 Spending Per Pupil	(4) Log Enrollment	(5) Public Share	(6) FRPM Share	(7) Revenue Per Pupil	(8) Spending Per Pupil	(9) K-12 Spending Per Pupil	(10) Log Enrollment	(11) Public Share	(12) FRPM Share
Treated (55–80%) × Year												
2011	697	846	239***	0.026**	−0.004	0.003	132*	−70	3	0.013**	0.001	0.005
	(567)	(539)	(83)	(0.012)	(0.003)	(0.009)	(78)	(81)	(58)	(0.005)	(0.002)	(0.008)
2012	68	172**	127*	0.015**	−0.004	0.001	141***	−27	−30	0.005	0.000	0.004
	(158)	(77)	(70)	(0.007)	(0.004)	(0.006)	(47)	(55)	(54)	(0.004)	(0.002)	(0.005)
2014	157*	210**	197**	−0.015**	0.000	0.006	120**	72	25	−0.005*	0.001	0.004
	(88)	(98)	(83)	(0.007)	(0.002)	(0.007)	(50)	(55)	(48)	(0.003)	(0.002)	(0.005)
2015	173	351**	297**	−0.017	0.004	0.013*	468***	200***	173**	−0.008*	−0.001	0.006
	(136)	(142)	(124)	(0.012)	(0.003)	(0.007)	(66)	(71)	(69)	(0.004)	(0.002)	(0.005)
2016	458***	446***	340***	−0.008	−0.001	0.001	679***	425***	372***	−0.014**	0.001	0.006
	(131)	(152)	(130)	(0.018)	(0.004)	(0.008)	(84)	(78)	(79)	(0.006)	(0.001)	(0.005)
2017	427***	490***	347**	−0.002	0.001	−0.002	777***	544***	477***	−0.016*	−0.000	0.004
	(155)	(156)	(145)	(0.021)	(0.004)	(0.010)	(93)	(90)	(91)	(0.008)	(0.002)	(0.006)
2018	354**	439**	316**	0.001	−0.002	−0.001	862***	615***	585***	−0.018*	−0.001	−0.011*
	(160)	(170)	(160)	(0.026)	(0.004)	(0.008)	(107)	(120)	(131)	(0.010)	(0.002)	(0.006)
2019	384**	517***	348**	−0.005	0.001	−0.003	1,074***	879***	841***	−0.023*	0.003	−0.008
	(194)	(190)	(176)	(0.029)	(0.005)	(0.009)	(122)	(138)	(152)	(0.012)	(0.002)	(0.007)
Treated (80%+) × Year												
2011	469***	432***	371***	0.010	−0.004	0.020*	435***	372**	261***	0.017***	−0.000	0.025***
	(97)	(109)	(92)	(0.011)	(0.003)	(0.010)	(150)	(144)	(61)	(0.005)	(0.002)	(0.007)
2012	350***	116	132	0.016***	−0.008***	−0.003	207***	88*	74*	0.010***	−0.001	0.001
	(86)	(96)	(81)	(0.006)	(0.003)	(0.009)	(44)	(46)	(44)	(0.003)	(0.002)	(0.009)
2014	300***	113	69	−0.004	−0.000	−0.003	127**	−85	33	−0.007**	0.000	0.020***
	(74)	(95)	(83)	(0.006)	(0.002)	(0.010)	(59)	(108)	(40)	(0.003)	(0.002)	(0.005)
2015	670***	291**	197	−0.005	0.002	0.003	703***	302***	426***	−0.012**	−0.002	0.019***
	(101)	(141)	(124)	(0.012)	(0.003)	(0.009)	(53)	(93)	(73)	(0.005)	(0.002)	(0.005)
2016	1,219***	588***	474***	−0.002	−0.003	−0.003	1,204***	693***	762***	−0.021***	−0.000	0.022***
	(133)	(157)	(141)	(0.017)	(0.004)	(0.011)	(102)	(98)	(81)	(0.007)	(0.001)	(0.007)
2017	1,480***	806***	617***	−0.005	−0.004	−0.004	1,457***	751***	799***	−0.027***	−0.001	0.027***
	(141)	(174)	(168)	(0.021)	(0.004)	(0.010)	(127)	(184)	(92)	(0.009)	(0.002)	(0.008)
2018	1,430***	876***	655***	0.001	−0.006	−0.022**	1,520***	1,084***	1,071***	−0.038***	−0.002	0.008
	(160)	(183)	(188)	(0.025)	(0.004)	(0.011)	(101)	(146)	(94)	(0.012)	(0.002)	(0.010)
2019	1,393***	1,037***	792***	−0.002	−0.004	−0.018*	1,712***	1,462***	1,530***	−0.051***	0.001	0.005
	(187)	(199)	(201)	(0.028)	(0.005)	(0.011)	(79)	(93)	(135)	(0.014)	(0.002)	(0.007)
Observations	2,858	2,858	2,858	2,856	2,856	2,856	2,762	2,762	2,762	2,762	2,762	2,762
Clusters	320	320	320	320	320	320	308	308	308	308	308	308

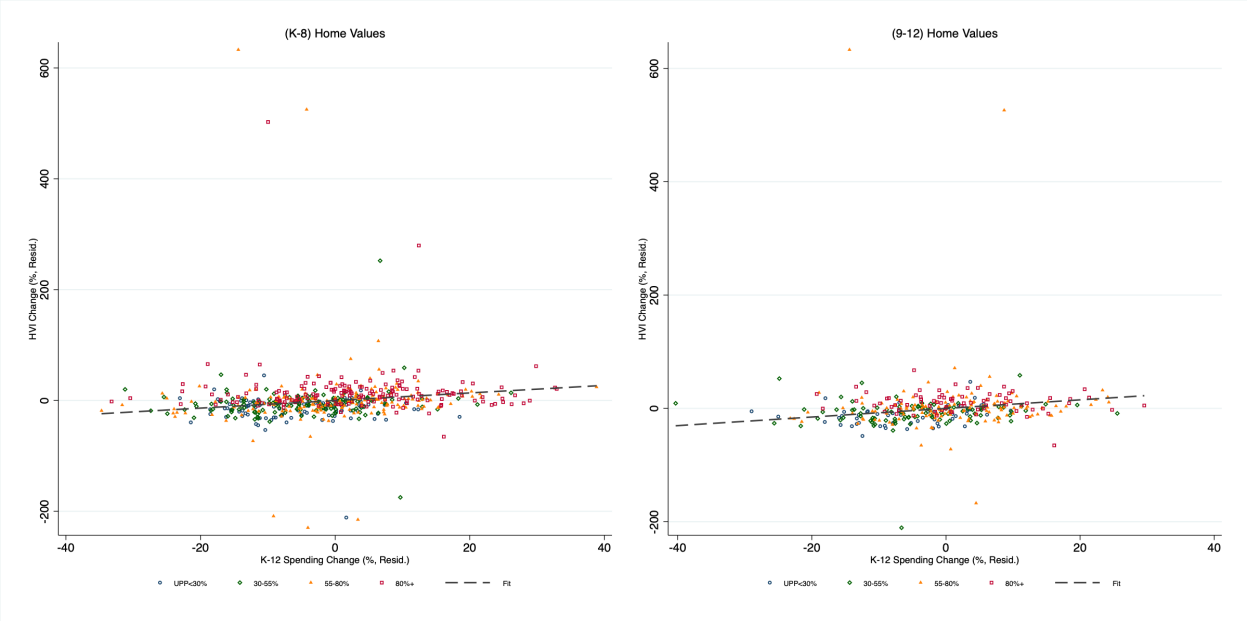
Notes: This table reports difference-in-differences event study estimates with 2013 as the omitted base year. All regressions include district and year fixed effects and are weighted by ADA. Standard errors clustered at the district level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: DiD Heterogeneity by District Size — K–8 Districts (Continued)

Panel B: Migration and Housing

	Small Districts (Below Median)				Large Districts (Above Median)			
	(1) Net Mig. Rate (pp)	(2) Income Gap ($)	(3) Minority Gap (pp)	(4) Log Home Value Index	(5) Net Mig. Rate (pp)	(6) Income Gap ($)	(7) Minority Gap (pp)	(8) Log Home Value Index
Treated (55–80%) × Year								
2011	−0.094	214**	−0.016	0.022	−0.100	113**	0.010	0.011
	(0.343)	(101)	(0.042)	(0.028)	(0.165)	(47)	(0.022)	(0.008)
2012	−0.415	39	0.055*	0.033***	−0.084	1	0.028*	0.002
	(0.290)	(88)	(0.031)	(0.009)	(0.102)	(32)	(0.017)	(0.006)
2014	0.099	−138*	0.016	−0.009	0.301***	5	−0.020	0.015*
	(0.316)	(81)	(0.034)	(0.022)	(0.106)	(31)	(0.017)	(0.008)
2015	0.377	14	0.057	−0.085	0.208*	31	0.006	0.029**
	(0.359)	(96)	(0.050)	(0.088)	(0.116)	(36)	(0.017)	(0.012)
2016	0.709**	−68	0.038	−0.023	0.293**	−2	0.002	0.050**
	(0.319)	(92)	(0.049)	(0.074)	(0.131)	(44)	(0.017)	(0.020)
2017	0.609	−63	0.051	−0.001	0.366**	−50	0.024	0.069***
	(0.446)	(105)	(0.040)	(0.073)	(0.153)	(39)	(0.021)	(0.021)
2018	0.437	139	0.002	0.006	0.253*	−15	0.014	0.077***
	(0.418)	(101)	(0.042)	(0.072)	(0.151)	(40)	(0.021)	(0.021)
2019	0.611	32	0.024	0.004	0.272	−63	0.051**	0.083***
	(0.443)	(95)	(0.037)	(0.071)	(0.181)	(43)	(0.023)	(0.023)
Treated (80%+) × Year								
2011	0.457	255**	0.004	−0.022	0.054	145***	−0.019	−0.000
	(0.327)	(101)	(0.040)	(0.044)	(0.148)	(55)	(0.029)	(0.007)
2012	−0.320	−98	0.030	0.015*	−0.116	14	0.009	0.004
	(0.298)	(86)	(0.038)	(0.008)	(0.097)	(29)	(0.014)	(0.005)
2014	0.440	−87	−0.004	0.033	0.453***	−42	−0.025	0.020**
	(0.292)	(91)	(0.038)	(0.025)	(0.151)	(40)	(0.026)	(0.009)
2015	1.053**	−7	0.064	0.075***	0.303*	−18	0.013	0.051***
	(0.410)	(108)	(0.055)	(0.028)	(0.158)	(46)	(0.028)	(0.014)
2016	0.902***	−125	0.045	−0.009	0.513**	−75	−0.004	0.046*
	(0.310)	(104)	(0.051)	(0.059)	(0.216)	(49)	(0.026)	(0.023)
2017	0.358	−342*	0.021	0.027	0.593**	−134**	0.010	0.070***
	(0.441)	(187)	(0.056)	(0.059)	(0.259)	(59)	(0.033)	(0.025)
2018	0.407	−130	0.050	0.044	0.369	−113**	0.016	0.101***
	(0.467)	(213)	(0.072)	(0.058)	(0.227)	(49)	(0.030)	(0.024)
2019	0.686*	−81	−0.040	0.059	0.452**	−139***	0.033	0.136***
	(0.406)	(81)	(0.050)	(0.059)	(0.206)	(41)	(0.031)	(0.018)
Observations	2,854	2,854	2,854	2,829	2,762	2,762	2,762	2,762
Clusters	320	320	320	320	308	308	308	308

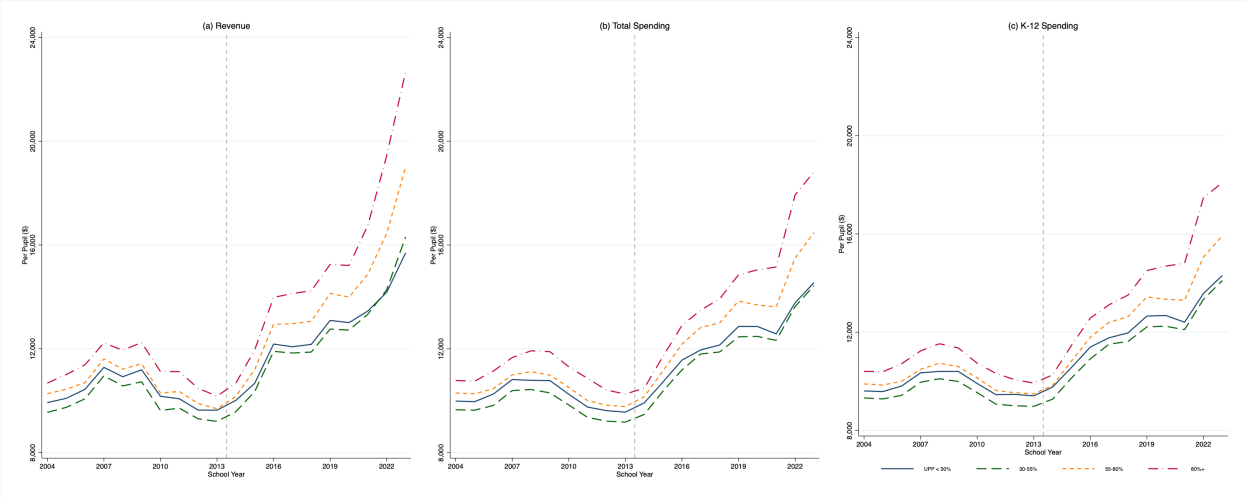
Notes: See Panel A. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value is the natural log of the Zillow Home Value Index (2020$).



Notes: Each panel plots county-residualized changes in K-12 per-pupil spending against county-residualized changes in home values between 2013 and 2019. The left column covers K-8 districts and the right column covers 9-12 districts. The dashed line is the ADA-weighted fitted line corresponding to Equation 2. See Section 3 for details.

Figure 3: K-12 Spending Change and Home Value Growth Across Districts

6 Appendix



Notes: Each panel plots ADA-weighted mean per-pupil values by unduplicated pupil percentage (UPP) group from 2004 to 2023. Groups are defined by districts' 2013 UPP: below 30%, 30 to 55%, 55 to 80%, and above 80%. Panel (a) shows total general fund revenue per pupil. Panel (b) shows total general fund spending per pupil. Panel (c) shows general fund K-12 spending per pupil, which excludes pre-K and adult education, debt service, transfers, and other non-K-12 categories. All values are in constant 2020 dollars. The dashed vertical line marks the first year of LCFF implementation (2013–14). See Section 2 for data sources and variable definitions.

Figure 4: District Finance Trends by Baseline Exposure

Difference-in-Kinks Analysis

I also estimate a difference-in-kinks specification around the 55% concentration-grant threshold, following Bockerman et al. (2025). Unlike the event-study DiD, which captures broader differences in LCFF exposure across districts, this design focuses on the change in the slope of outcomes at the 55% cutoff after LCFF.

$$Y_{dt} = \beta_1(H_d - 0.55) \cdot Post_t + \beta_2(H_d - 0.55) \cdot \mathbf{1}\{H_d \geq 0.55\} \cdot Post_t + \gamma_d + \lambda_t + \varepsilon_{dt} \tag{3}$$

Here, Y_{dt} is the outcome of interest for district d in year t, and H_d is district d's 2013–14 unduplicated pupil share, centered at the 55% threshold. $Post_t$ equals 1 in the post-LCFF period and 0 otherwise. District fixed effects absorb all time-invariant terms in H_d , including the baseline slope and kink. Year fixed effects absorb the $Post_t$ main effect. What remains is how the slope changes after LCFF: β_1 captures the change in the overall linear slope, and β_2, the coefficient of interest, captures the change in the kink at the threshold.

In the baseline specification, I restrict the sample to districts within a bandwidth of 0.15 around the threshold. All specifications include district and year fixed effects, and standard errors are clustered at the district level. The key assumption is that, without LCFF, the slope of the outcome with respect to H_d would have changed smoothly through the threshold over time.

Since H_d is measured as a share from 0 to 1, β_2 is scaled to a one-unit change in unduplicated pupil share. A 10 percentage point increase above the threshold corresponds to $0.1 \times \beta_2$. Given the 0.15 bandwidth, a comparison from 55% to 70% corresponds to $0.15 \times \beta_2$.

Table A1 presents the estimates. For K–8 districts, the revenue coefficient is 2,878. This implies about $288 per pupil for a 10 percentage point increase above the threshold, or about $432 for moving from 55% to 70%. For 9–12 districts, the revenue coefficient is 7,456. This implies about $746 per pupil for a 10 percentage point increase, or about $1,118 for moving from 55% to 70%. The spending coefficients are also positive, but they are smaller and imprecisely estimated. The funding estimates line up with the concentration grant increasing funding more sharply above the cutoff.

Outside funding, the estimates are noisy and do not show a clear pattern. For example, the home value coefficient is negative for K–8 districts and positive for 9–12 districts, and the migration coefficients are negative in both panels. Given this imprecision and lack of a stable pattern, I do not draw strong conclusions for the non-funding outcomes.

Table A1: Difference-in-Kinks Estimates of LCFF Effects

	Funding ($)			Enrollment & Composition			Housing & Migration			
	Revenue Per Pupil (1)	Spending Per Pupil (2)	K-12 Spending Per Pupil (3)	Log Enrollment (4)	Public Share (5)	FRPM Share (6)	Net Migration Rate (pp) (7)	Income Gap (In–Out, $) (8)	Minority Gap (In–Out, pp) (9)	Log Home Value Index (10)
Panel A: K–8 Districts										
Kink × Post	2,878	1,049	1,512	0.067	0.059	−0.028	−1.861	−1,246	−0.138	−1.736
	(2,961)	(2,516)	(2,264)	(0.578)	(0.050)	(0.114)	(4.068)	(919)	(0.370)	(1.403)
Observations	2,117	2,117	2,117	2,117	2,117	2,117	2,115	2,115	2,115	2,090
Clusters	237	237	237	237	237	237	237	237	237	237
Panel B: 9–12 Districts										
Kink × Post	7,456	1,653	1,379	0.871	0.031	−0.308**	−1.397	−590	0.023	0.333
	(6,076)	(3,541)	(2,917)	(0.891)	(0.054)	(0.142)	(3.531)	(919)	(0.396)	(0.734)
Observations	1,454	1,454	1,454	1,454	1,454	1,454	1,452	1,452	1,452	1,452
Clusters	162	162	162	162	162	162	162	162	162	162

Notes: This table reports Difference-in-Kinks (DiK) estimates. Each cell shows the coefficient on Kink × Post, which captures the change in the kink effect at the 55% UPP threshold after LCFF implementation (2014–2019) relative to the pre-period (2011–2013). All specifications include UPP Slope, Change in Slope (Kink), UPP Slope × Post, and year fixed effects, and are weighted by ADA. Standard errors clustered at the district level are in parentheses. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value Index is the natural log of the Zillow Home Value Index (2020$). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Additional Robustness

Table A2: Robustness: Unweighted Estimates — K–8 Districts

	Funding ($)			Enrollment & Composition			Housing & Migration			
	Revenue Per Pupil (1)	Spending Per Pupil (2)	K-12 Spending Per Pupil (3)	Log Enrollment (4)	Public Share (5)	FRPM Share (6)	Net Migration Rate (pp) (7)	Income Gap (In–Out, $) (8)	Minority Gap (In–Out, pp) (9)	Log Home Value Index (10)
Panel A: Treated (55–80%) × Year										
2011	292*	268*	135**	0.012	−0.000	0.004	0.048	155***	−0.001	0.020
	(168)	(158)	(57)	(0.009)	(0.002)	(0.006)	(0.168)	(53)	(0.020)	(0.023)
2012	146**	112**	77	0.005	−0.001	0.004	−0.249	34	0.033*	0.017*
	(71)	(56)	(50)	(0.007)	(0.002)	(0.005)	(0.170)	(44)	(0.018)	(0.009)
2014	67	152***	138***	−0.010	0.002	0.014***	0.139	−93*	−0.007	−0.001
	(54)	(56)	(49)	(0.006)	(0.002)	(0.005)	(0.183)	(48)	(0.021)	(0.012)
2015	257***	249***	221***	−0.014	0.002	0.013**	0.074	33	0.005	−0.103
	(69)	(77)	(73)	(0.010)	(0.002)	(0.005)	(0.215)	(57)	(0.026)	(0.110)
2016	505***	378***	345***	−0.009	0.002	0.009	0.538**	−60	0.011	−0.020
	(85)	(101)	(95)	(0.013)	(0.002)	(0.006)	(0.227)	(55)	(0.023)	(0.083)
2017	615***	489***	414***	−0.010	0.002	0.010	0.439*	−36	0.020	−0.001
	(91)	(98)	(94)	(0.014)	(0.002)	(0.006)	(0.248)	(56)	(0.023)	(0.083)
2018	567***	503***	440***	−0.010	0.000	0.000	0.152	80	−0.000	0.009
	(110)	(111)	(104)	(0.017)	(0.003)	(0.006)	(0.251)	(63)	(0.022)	(0.083)
2019	671***	645***	553***	−0.017	0.004	0.002	0.413	−56	0.027	0.014
	(137)	(128)	(124)	(0.019)	(0.003)	(0.006)	(0.310)	(65)	(0.022)	(0.083)
Panel B: Treated (80%+) × Year										
2011	383***	404***	344***	0.010*	−0.002	0.020***	0.290	193***	−0.023	−0.038
	(100)	(106)	(68)	(0.006)	(0.002)	(0.006)	(0.192)	(53)	(0.028)	(0.036)
2012	281***	154**	132**	0.012***	−0.005***	−0.010	−0.149	−28	0.019	0.005
	(62)	(64)	(58)	(0.004)	(0.002)	(0.006)	(0.165)	(43)	(0.023)	(0.005)
2014	199***	50	42	−0.007	0.000	0.007	0.302*	−65	−0.030	0.029*
	(48)	(53)	(46)	(0.005)	(0.001)	(0.006)	(0.176)	(49)	(0.022)	(0.015)
2015	624***	298***	252***	−0.007	0.001	0.010	0.461**	7	−0.009	0.071***
	(61)	(75)	(71)	(0.009)	(0.002)	(0.006)	(0.231)	(64)	(0.033)	(0.020)
2016	1,240***	663***	555***	−0.010	0.000	0.007	0.610***	−170***	−0.016	−0.016
	(86)	(95)	(90)	(0.011)	(0.002)	(0.006)	(0.220)	(58)	(0.030)	(0.077)
2017	1,533***	975***	806***	−0.017	−0.001	0.003	0.388	−206***	−0.016	0.011
	(89)	(100)	(95)	(0.013)	(0.002)	(0.007)	(0.236)	(68)	(0.028)	(0.076)
2018	1,518***	1,100***	944***	−0.019	−0.002	−0.023***	0.183	−77	−0.029	0.028
	(104)	(114)	(111)	(0.017)	(0.002)	(0.008)	(0.240)	(73)	(0.035)	(0.076)
2019	1,569***	1,228***	1,057***	−0.029	−0.000	−0.016**	0.316	−165***	−0.033	0.041
	(123)	(120)	(120)	(0.019)	(0.002)	(0.007)	(0.256)	(54)	(0.030)	(0.076)
Observations	5,620	5,620	5,620	5,618	5,618	5,618	5,616	5,616	5,616	5,591
Clusters	628	628	628	628	628	628	628	628	628	628

Notes: Each column reports coefficients from a separate regression of the outcome on treatment tier × year interactions, with 2013 as the omitted base year. All regressions include district and year fixed effects. Standard errors clustered at the district level are in parentheses. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value Index is the natural log of the Zillow Home Value Index (2020$). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Robustness: Unweighted Estimates — 9–12 Districts

	Funding ($)			Enrollment & Composition			Housing & Migration			
	Revenue Per Pupil (1)	Spending Per Pupil (2)	K-12 Spending Per Pupil (3)	Log Enrollment (4)	Public Share (5)	FRPM Share (6)	Net Migration Rate (pp) (7)	Income Gap (In–Out, $) (8)	Minority Gap (In–Out, pp) (9)	Log Home Value Index (10)
Panel A: Treated (55–80%) × Year										
2011	−169	−16	60	0.012	−0.001	0.005	−0.120	157***	0.009	−0.006
	(203)	(92)	(67)	(0.010)	(0.001)	(0.008)	(0.162)	(50)	(0.021)	(0.023)
2012	−66	54	57	0.006	−0.001	0.003	−0.279*	59	0.028	0.014
	(202)	(62)	(53)	(0.008)	(0.002)	(0.006)	(0.162)	(42)	(0.018)	(0.011)
2014	−214	98	78	−0.011***	−0.001	0.013***	−0.143	−70*	−0.026	0.017***
	(217)	(67)	(58)	(0.004)	(0.001)	(0.005)	(0.148)	(40)	(0.020)	(0.007)
2015	90	168	219***	−0.020**	−0.003*	0.009*	0.130	25	0.026	0.031***
	(125)	(105)	(84)	(0.008)	(0.002)	(0.006)	(0.187)	(43)	(0.023)	(0.009)
2016	203	285**	304***	−0.025**	−0.000	0.005	0.334*	−1	0.010	−0.080
	(239)	(115)	(104)	(0.012)	(0.002)	(0.007)	(0.184)	(50)	(0.025)	(0.155)
2017	357	362***	348***	−0.027*	0.000	0.014**	0.178	−104**	0.035	−0.058
	(242)	(127)	(106)	(0.015)	(0.002)	(0.007)	(0.205)	(47)	(0.023)	(0.155)
2018	367	475***	452***	−0.031	−0.001	−0.001	0.015	−40	0.012	−0.053
	(259)	(140)	(117)	(0.019)	(0.002)	(0.006)	(0.190)	(45)	(0.023)	(0.155)
2019	520*	624***	523***	−0.038*	−0.001	0.003	−0.136	−105*	−0.001	−0.049
	(282)	(139)	(138)	(0.021)	(0.002)	(0.007)	(0.279)	(56)	(0.026)	(0.154)
Panel B: Treated (80%+) × Year										
2011	309*	450**	383***	0.006	−0.001	0.008	0.177	197***	−0.016	−0.064
	(177)	(184)	(102)	(0.006)	(0.001)	(0.008)	(0.209)	(62)	(0.035)	(0.051)
2012	147**	55	77	0.011***	−0.004**	−0.012*	−0.069	18	0.036	0.005
	(71)	(74)	(65)	(0.004)	(0.002)	(0.006)	(0.190)	(54)	(0.024)	(0.006)
2014	125*	67	42	−0.003	−0.001	0.004	0.316*	25	−0.069***	0.022***
	(76)	(71)	(62)	(0.004)	(0.001)	(0.006)	(0.181)	(62)	(0.026)	(0.007)
2015	551***	269***	224**	−0.003	−0.002	−0.001	0.451**	75	−0.029	0.053***
	(93)	(100)	(91)	(0.011)	(0.001)	(0.008)	(0.220)	(73)	(0.034)	(0.011)
2016	1,074***	568***	425***	−0.007	−0.001	−0.012	0.548**	−40	−0.055	−0.102
	(113)	(112)	(104)	(0.014)	(0.001)	(0.008)	(0.223)	(74)	(0.036)	(0.152)
2017	1,311***	831***	648***	−0.009	−0.002	−0.005	0.325	−212**	0.004	−0.078
	(122)	(122)	(117)	(0.017)	(0.002)	(0.009)	(0.297)	(106)	(0.039)	(0.152)
2018	1,274***	981***	810***	−0.006	−0.003	−0.025***	0.274	−121	−0.003	−0.060
	(115)	(122)	(126)	(0.027)	(0.002)	(0.008)	(0.304)	(119)	(0.052)	(0.152)
2019	1,354***	1,182***	966***	−0.016	−0.003	−0.019**	0.476*	−101*	−0.030	−0.049
	(147)	(140)	(140)	(0.029)	(0.002)	(0.008)	(0.283)	(61)	(0.041)	(0.152)
Observations	3,463	3,463	3,463	3,463	3,463	3,463	3,459	3,459	3,459	3,459
Clusters	386	386	386	386	386	386	386	386	386	386

Notes: Each column reports coefficients from a separate regression of the outcome on treatment tier × year interactions, with 2013 as the omitted base year. All regressions include district and year fixed effects. Standard errors clustered at the district level are in parentheses. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value Index is the natural log of the Zillow Home Value Index (2020$). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Additional Heterogeneity Results

K–8 Elementary Districts

Table A4: DiD Heterogeneity by Urban/Rural — K–8 Districts

Panel A: Finance, Enrollment, and Composition

	Non-Urban						Urban					
	(1) Revenue Per Pupil	(2) Spending Per Pupil	(3) K-12 Spending Per Pupil	(4) Log Enrollment	(5) Public Share	(6) FRPM Share	(7) Revenue Per Pupil	(8) Spending Per Pupil	(9) K-12 Spending Per Pupil	(10) Log Enrollment	(11) Public Share	(12) FRPM Share
Treated (55–80%) × Year												
2011	201*	109	85	−0.019	−0.001	−0.015	190**	5	15	0.015***	0.000	0.005
	(116)	(113)	(108)	(0.016)	(0.003)	(0.015)	(93)	(95)	(58)	(0.005)	(0.002)	(0.008)
2012	99	−45	−24	0.002	−0.005**	−0.024***	141***	−13	−22	0.006	0.000	0.005
	(98)	(89)	(87)	(0.007)	(0.002)	(0.007)	(47)	(54)	(53)	(0.004)	(0.002)	(0.005)
2014	54	145	178*	−0.009	0.002	−0.001	130***	72	20	−0.006**	0.000	0.004
	(99)	(99)	(96)	(0.011)	(0.002)	(0.007)	(50)	(55)	(48)	(0.003)	(0.002)	(0.006)
2015	250*	255	311**	−0.021	0.006	0.008	459***	194***	158**	−0.009**	−0.001	0.006
	(133)	(166)	(153)	(0.021)	(0.004)	(0.007)	(67)	(70)	(69)	(0.004)	(0.002)	(0.005)
2016	383**	273	231	−0.011	0.002	−0.017	686***	437***	381***	−0.016**	0.001	0.007
	(175)	(168)	(157)	(0.031)	(0.003)	(0.011)	(84)	(78)	(78)	(0.006)	(0.001)	(0.005)
2017	698***	519***	443**	−0.030	0.004	−0.001	758***	550***	479***	−0.016*	−0.000	0.003
	(177)	(192)	(177)	(0.037)	(0.003)	(0.011)	(93)	(89)	(91)	(0.008)	(0.002)	(0.006)
2018	582***	462**	432**	−0.041	0.004	−0.015	850***	624***	591***	−0.017*	−0.001	−0.011*
	(193)	(202)	(182)	(0.044)	(0.003)	(0.012)	(109)	(122)	(133)	(0.010)	(0.002)	(0.006)
2019	721***	742***	604***	−0.058	0.007*	−0.002	1,042***	864***	831***	−0.022*	0.002	−0.009
	(209)	(209)	(206)	(0.050)	(0.004)	(0.012)	(125)	(141)	(156)	(0.012)	(0.002)	(0.007)
Treated (80%+) × Year												
2011	466***	284*	262**	−0.039**	−0.003	0.030*	446***	394***	271***	0.021***	−0.001	0.022***
	(114)	(146)	(126)	(0.015)	(0.003)	(0.018)	(151)	(143)	(59)	(0.005)	(0.002)	(0.007)
2012	347***	15	60	−0.007	−0.007***	−0.006	210***	95**	78*	0.012***	−0.002	0.000
	(103)	(140)	(130)	(0.006)	(0.002)	(0.011)	(43)	(43)	(42)	(0.003)	(0.002)	(0.009)
2014	279**	67	55	0.002	−0.001	0.005	128**	−85	32	−0.008***	0.000	0.019***
	(109)	(95)	(93)	(0.010)	(0.002)	(0.012)	(57)	(109)	(40)	(0.003)	(0.002)	(0.005)
2015	571***	104	72	−0.009	0.002	0.028	708***	312***	433***	−0.013**	−0.002	0.016***
	(125)	(180)	(154)	(0.021)	(0.004)	(0.018)	(51)	(93)	(69)	(0.005)	(0.002)	(0.005)
2016	1,100***	366**	291*	−0.006	−0.003	0.003	1,210***	709***	777***	−0.022***	−0.000	0.020***
	(160)	(174)	(154)	(0.031)	(0.003)	(0.018)	(101)	(100)	(76)	(0.007)	(0.001)	(0.007)
2017	1,588***	976***	868***	−0.023	−0.002	0.020	1,450***	740***	782***	−0.027***	−0.001	0.025***
	(170)	(236)	(249)	(0.036)	(0.003)	(0.018)	(125)	(180)	(88)	(0.009)	(0.002)	(0.008)
2018	1,568***	973***	916***	−0.035	−0.002	−0.008	1,507***	1,079***	1,054***	−0.038***	−0.002	0.006
	(189)	(209)	(217)	(0.044)	(0.003)	(0.013)	(100)	(146)	(92)	(0.012)	(0.002)	(0.010)
2019	1,657***	1,262***	1,089***	−0.053	−0.002	0.003	1,678***	1,442***	1,505***	−0.050***	0.001	0.003
	(211)	(227)	(229)	(0.049)	(0.004)	(0.016)	(78)	(91)	(132)	(0.014)	(0.002)	(0.008)
Observations	2,165	2,165	2,165	2,163	2,163	2,163	3,455	3,455	3,455	3,455	3,455	3,455
Clusters	242	242	242	242	242	242	386	386	386	386	386	386

Notes: This table reports difference-in-differences event study estimates with 2013 as the omitted base year. All regressions include district and year fixed effects and are weighted by ADA. Standard errors clustered at the district level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: DiD Heterogeneity by Urban/Rural — K–8 Districts (Continued)

Panel B: Migration and Housing

	Non-Urban				Urban			
	(1) Net Mig. Rate (pp)	(2) Income Gap ($)	(3) Minority Gap (pp)	(4) Log Home Value Index	(5) Net Mig. Rate (pp)	(6) Income Gap ($)	(7) Minority Gap (pp)	(8) Log Home Value Index
Treated (55–80%) × Year								
2011	0.714**	107	0.048	−0.003	−0.142	115**	0.012	0.012
	(0.338)	(82)	(0.046)	(0.027)	(0.163)	(46)	(0.021)	(0.008)
2012	0.461	42	0.091**	0.008	−0.153	−4	0.028	0.002
	(0.391)	(91)	(0.042)	(0.009)	(0.098)	(32)	(0.017)	(0.006)
2014	−0.174	−138	−0.035	−0.035	0.299***	2	−0.018	0.018**
	(0.333)	(98)	(0.048)	(0.043)	(0.105)	(30)	(0.016)	(0.008)
2015	−0.128	252*	0.007	−0.093	0.228**	13	0.007	0.033***
	(0.526)	(150)	(0.088)	(0.078)	(0.112)	(35)	(0.017)	(0.012)
2016	0.472	9	−0.068	−0.088	0.285**	−18	0.007	0.049**
	(0.414)	(100)	(0.052)	(0.092)	(0.125)	(42)	(0.017)	(0.021)
2017	0.446	−64	−0.022	−0.077	0.301**	−61	0.029	0.068***
	(0.540)	(118)	(0.053)	(0.092)	(0.147)	(38)	(0.020)	(0.022)
2018	−0.057	10	−0.104*	−0.069	0.199	−11	0.016	0.076***
	(0.525)	(105)	(0.060)	(0.092)	(0.146)	(39)	(0.020)	(0.022)
2019	−0.188	−73	−0.020	−0.064	0.277*	−53	0.052**	0.082***
	(0.682)	(126)	(0.051)	(0.092)	(0.167)	(41)	(0.023)	(0.024)
Treated (80%+) × Year								
2011	0.861**	209**	0.001	−0.049	0.052	144***	−0.014	0.002
	(0.375)	(84)	(0.057)	(0.044)	(0.143)	(53)	(0.027)	(0.007)
2012	0.369	17	0.012	−0.015*	−0.157	2	0.014	0.007
	(0.387)	(109)	(0.065)	(0.009)	(0.097)	(28)	(0.013)	(0.005)
2014	−0.073	−32	−0.105*	−0.019	0.476***	−53	−0.018	0.024**
	(0.318)	(105)	(0.054)	(0.042)	(0.152)	(39)	(0.025)	(0.010)
2015	−0.074	298*	−0.084	0.018	0.391**	−42	0.024	0.055***
	(0.566)	(170)	(0.108)	(0.045)	(0.157)	(44)	(0.026)	(0.014)
2016	0.318	−46	−0.181**	−0.044	0.555**	−89*	0.014	0.041*
	(0.390)	(107)	(0.072)	(0.091)	(0.216)	(47)	(0.024)	(0.024)
2017	−0.222	−233	−0.119	−0.025	0.611**	−150***	0.021	0.066**
	(0.569)	(202)	(0.088)	(0.091)	(0.252)	(56)	(0.031)	(0.026)
2018	−0.717	−234	−0.097	−0.022	0.411*	−106**	0.022	0.098***
	(0.623)	(211)	(0.090)	(0.090)	(0.227)	(48)	(0.029)	(0.025)
2019	−0.205	−47	−0.126	−0.014	0.479**	−143***	0.039	0.134***
	(0.539)	(117)	(0.082)	(0.091)	(0.206)	(39)	(0.029)	(0.018)
Observations	2,161	2,161	2,161	2,136	3,455	3,455	3,455	3,455
Clusters	242	242	242	242	386	386	386	386

Notes: See Panel A. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value is the natural log of the Zillow Home Value Index (2020$).

9–12 Districts

Table A5: DiD Heterogeneity by District Size — 9–12 Districts

Panel A: Finance, Enrollment, and Composition

	Small Districts (Below Median)						Large Districts (Above Median)					
	(1) Revenue Per Pupil	(2) Spending Per Pupil	(3) K-12 Spending Per Pupil	(4) Log Enrollment	(5) Public Share	(6) FRPM Share	(7) Revenue Per Pupil	(8) Spending Per Pupil	(9) K-12 Spending Per Pupil	(10) Log Enrollment	(11) Public Share	(12) FRPM Share
Treated (55–80%) × Year												
2011	−306	144	122	0.014	−0.003	0.008	172**	−29	38	0.013***	−0.000	0.009
	(341)	(133)	(127)	(0.018)	(0.003)	(0.014)	(85)	(84)	(63)	(0.005)	(0.002)	(0.009)
2012	−221	73	85	0.014	−0.003	−0.002	178***	−6	−8	0.006	−0.001	0.006
	(352)	(107)	(97)	(0.010)	(0.006)	(0.010)	(51)	(57)	(56)	(0.004)	(0.002)	(0.006)
2014	−365	120	93	−0.015	−0.003	0.005	146***	82	39	−0.005*	0.000	0.009
	(377)	(149)	(118)	(0.010)	(0.003)	(0.008)	(53)	(61)	(55)	(0.003)	(0.002)	(0.006)
2015	−73	220	295	−0.032	−0.002	0.010	469***	204***	191**	−0.008*	−0.002	0.009
	(230)	(234)	(183)	(0.021)	(0.004)	(0.010)	(71)	(76)	(75)	(0.004)	(0.002)	(0.006)
2016	−199	188	208	−0.042	−0.001	−0.019	658***	427***	378***	−0.014**	0.001	0.011*
	(426)	(232)	(185)	(0.031)	(0.004)	(0.012)	(87)	(85)	(87)	(0.006)	(0.001)	(0.006)
2017	−87	245	214	−0.046	0.002	−0.003	792***	530***	461***	−0.015*	−0.001	0.009
	(448)	(259)	(216)	(0.038)	(0.005)	(0.014)	(98)	(102)	(101)	(0.008)	(0.002)	(0.007)
2018	−106	298	261	−0.055	−0.001	−0.014	844***	618***	593***	−0.015	−0.003	−0.004
	(457)	(259)	(222)	(0.046)	(0.005)	(0.011)	(109)	(127)	(134)	(0.011)	(0.003)	(0.007)
2019	−55	483*	337	−0.061	−0.004	−0.015	1,058***	855***	798***	−0.021*	0.000	−0.003
	(495)	(250)	(241)	(0.051)	(0.005)	(0.013)	(125)	(141)	(154)	(0.013)	(0.002)	(0.008)
Treated (80%+) × Year												
2011	410***	554***	512***	−0.027	−0.003	0.009	467***	418**	294***	0.020***	0.001	0.024**
	(146)	(196)	(159)	(0.017)	(0.002)	(0.015)	(180)	(170)	(68)	(0.005)	(0.002)	(0.009)
2012	139	−110	8	0.009	−0.010**	−0.006	230***	98**	85*	0.012***	−0.001	−0.000
	(137)	(149)	(117)	(0.009)	(0.005)	(0.010)	(51)	(49)	(47)	(0.003)	(0.002)	(0.011)
2014	211*	257	186	0.003	−0.000	−0.009	105*	−139	13	−0.009***	0.000	0.017***
	(126)	(165)	(140)	(0.010)	(0.002)	(0.014)	(63)	(117)	(45)	(0.003)	(0.002)	(0.006)
2015	620***	326	254	−0.006	0.001	−0.008	691***	242**	410***	−0.017***	−0.002	0.013**
	(164)	(263)	(226)	(0.020)	(0.003)	(0.013)	(61)	(99)	(88)	(0.005)	(0.002)	(0.005)
2016	871***	294	218	−0.009	0.000	−0.028*	1,155***	643***	743***	−0.028***	−0.000	0.016*
	(207)	(280)	(244)	(0.030)	(0.003)	(0.014)	(118)	(110)	(93)	(0.007)	(0.001)	(0.008)
2017	1,143***	675**	480	−0.007	0.001	−0.017	1,398***	596***	686***	−0.034***	−0.001	0.024**
	(248)	(298)	(293)	(0.038)	(0.004)	(0.013)	(145)	(187)	(96)	(0.009)	(0.002)	(0.009)
2018	1,115***	799***	519*	−0.004	−0.002	−0.038***	1,427***	945***	960***	−0.045***	−0.002	0.004
	(237)	(285)	(305)	(0.047)	(0.005)	(0.014)	(107)	(140)	(88)	(0.012)	(0.002)	(0.012)
2019	1,207***	1,099***	765**	−0.009	−0.005	−0.033**	1,594***	1,366***	1,470***	−0.059***	0.000	−0.000
	(274)	(300)	(298)	(0.052)	(0.005)	(0.014)	(86)	(104)	(164)	(0.014)	(0.002)	(0.009)
Observations	1,262	1,262	1,262	1,262	1,262	1,262	2,201	2,201	2,201	2,201	2,201	2,201
Clusters	141	141	141	141	141	141	245	245	245	245	245	245

Notes: This table reports difference-in-differences event study estimates with 2013 as the omitted base year. All regressions include district and year fixed effects and are weighted by ADA. Standard errors clustered at the district level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: DiD Heterogeneity by District Size — 9–12 Districts (Continued)

Panel B: Migration and Housing

	Small Districts (Below Median)				Large Districts (Above Median)			
	(1) Net Mig. Rate (pp)	(2) Income Gap ($)	(3) Minority Gap (pp)	(4) Log Home Value Index	(5) Net Mig. Rate (pp)	(6) Income Gap ($)	(7) Minority Gap (pp)	(8) Log Home Value Index
Treated (55–80%) × Year								
2011	−0.252	99	0.023	−0.004	−0.139	145***	0.002	0.008
	(0.393)	(131)	(0.063)	(0.028)	(0.167)	(48)	(0.021)	(0.008)
2012	−0.527	37	0.077*	0.021*	−0.128	11	0.021	0.001
	(0.396)	(125)	(0.044)	(0.012)	(0.099)	(32)	(0.017)	(0.006)
2014	−0.485	−125	−0.055	0.020	0.304***	−1	−0.018	0.021***
	(0.354)	(94)	(0.047)	(0.013)	(0.100)	(30)	(0.017)	(0.008)
2015	0.142	−5	0.124*	0.033*	0.247**	32	0.007	0.039***
	(0.502)	(124)	(0.068)	(0.018)	(0.110)	(35)	(0.017)	(0.012)
2016	0.471	19	0.023	−0.020	0.306**	17	0.001	0.056***
	(0.378)	(145)	(0.073)	(0.149)	(0.130)	(44)	(0.017)	(0.020)
2017	0.221	−280**	0.074	0.002	0.348**	−36	0.024	0.075***
	(0.458)	(131)	(0.055)	(0.141)	(0.149)	(38)	(0.020)	(0.021)
2018	0.175	−55	0.005	0.001	0.220	1	0.010	0.084***
	(0.499)	(124)	(0.069)	(0.141)	(0.151)	(39)	(0.020)	(0.021)
2019	−0.429	−161	−0.023	0.007	0.276	−30	0.044*	0.092***
	(0.503)	(103)	(0.052)	(0.135)	(0.184)	(42)	(0.023)	(0.023)
Treated (80%+) × Year								
2011	0.390	263	−0.007	−0.017	0.123	133**	−0.022	−0.002
	(0.451)	(159)	(0.065)	(0.038)	(0.155)	(57)	(0.034)	(0.007)
2012	−0.051	21	0.063	0.004	−0.099	2	0.001	0.004
	(0.472)	(153)	(0.053)	(0.013)	(0.094)	(31)	(0.016)	(0.005)
2014	0.185	87	−0.062	0.027**	0.441***	−57	−0.033	0.018*
	(0.374)	(164)	(0.051)	(0.011)	(0.170)	(44)	(0.033)	(0.010)
2015	0.825*	148	0.074	0.063***	0.271*	−34	0.008	0.051***
	(0.497)	(167)	(0.074)	(0.018)	(0.162)	(47)	(0.035)	(0.015)
2016	0.576	68	−0.023	−0.059	0.493*	−71	−0.015	0.041
	(0.429)	(193)	(0.075)	(0.132)	(0.252)	(51)	(0.033)	(0.026)
2017	−0.141	−569	0.058	−0.024	0.544*	−136**	0.006	0.064**
	(0.844)	(401)	(0.105)	(0.125)	(0.294)	(66)	(0.041)	(0.028)
2018	−0.105	−401	0.054	−0.010	0.317	−103*	0.018	0.099***
	(0.844)	(460)	(0.134)	(0.127)	(0.267)	(55)	(0.035)	(0.028)
2019	0.173	−72	−0.088	0.007	0.459*	−124***	0.042	0.143***
	(0.588)	(117)	(0.062)	(0.121)	(0.244)	(44)	(0.035)	(0.020)
Observations	1,258	1,258	1,258	1,258	2,201	2,201	2,201	2,201
Clusters	141	141	141	141	245	245	245	245

Notes: See Panel A. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value is the natural log of the Zillow Home Value Index (2020$).

Table A6: DiD Heterogeneity by Urban/Rural — 9–12 Districts

Panel A: Finance, Enrollment, and Composition

	Non-Urban						Urban					
	(1) Revenue Per Pupil	(2) Spending Per Pupil	(3) K-12 Spending Per Pupil	(4) Log Enrollment	(5) Public Share	(6) FRPM Share	(7) Revenue Per Pupil	(8) Spending Per Pupil	(9) K-12 Spending Per Pupil	(10) Log Enrollment	(11) Public Share	(12) FRPM Share
Treated (55–80%) × Year												
2011	−145	38	46	−0.021	−0.003	−0.010	181**	−21	40	0.014***	−0.000	0.010
	(200)	(135)	(129)	(0.019)	(0.003)	(0.018)	(86)	(85)	(64)	(0.005)	(0.002)	(0.009)
2012	−152	−69	−64	0.003	−0.003	−0.025***	187***	−1	−1	0.007	−0.001	0.008
	(198)	(96)	(86)	(0.007)	(0.003)	(0.008)	(52)	(58)	(57)	(0.004)	(0.002)	(0.006)
2014	−259	98	154	−0.005	0.001	−0.006	153***	79	30	−0.006**	−0.000	0.010
	(215)	(122)	(113)	(0.011)	(0.001)	(0.007)	(55)	(62)	(56)	(0.003)	(0.002)	(0.007)
2015	−38	111	246	−0.010	0.003	0.007	476***	200**	183**	−0.009**	−0.002	0.009
	(173)	(197)	(172)	(0.023)	(0.002)	(0.009)	(73)	(78)	(77)	(0.004)	(0.002)	(0.006)
2016	−21	107	126	0.001	0.001	−0.021	664***	433***	386***	−0.017***	0.000	0.012*
	(277)	(199)	(184)	(0.036)	(0.003)	(0.013)	(90)	(87)	(89)	(0.006)	(0.001)	(0.006)
2017	306	427*	397*	−0.020	0.005*	0.002	778***	519***	455***	−0.017**	−0.002	0.008
	(296)	(243)	(217)	(0.043)	(0.003)	(0.012)	(100)	(104)	(104)	(0.009)	(0.002)	(0.007)
2018	207	373	371	−0.031	0.006	−0.017	847***	622***	602***	−0.018*	−0.003	−0.005
	(306)	(254)	(227)	(0.052)	(0.004)	(0.013)	(114)	(131)	(140)	(0.011)	(0.003)	(0.008)
2019	329	581**	486*	−0.042	0.005	−0.011	1,056***	858***	807***	−0.025*	−0.001	−0.004
	(330)	(250)	(252)	(0.059)	(0.004)	(0.015)	(130)	(146)	(160)	(0.013)	(0.002)	(0.008)
Treated (80%+) × Year												
2011	371***	259	261	−0.049***	−0.005*	0.022	472**	443**	305***	0.022***	0.001	0.022**
	(130)	(180)	(160)	(0.018)	(0.003)	(0.023)	(186)	(175)	(68)	(0.005)	(0.002)	(0.010)
2012	245**	−104	−38	−0.010	−0.005**	−0.005	221***	103**	90*	0.013***	−0.001	−0.001
	(114)	(157)	(146)	(0.007)	(0.003)	(0.011)	(52)	(49)	(47)	(0.003)	(0.002)	(0.012)
2014	200	113	118	0.009	−0.001	−0.006	95	−144	13	−0.010***	0.000	0.017***
	(127)	(119)	(110)	(0.011)	(0.002)	(0.015)	(59)	(118)	(45)	(0.003)	(0.002)	(0.006)
2015	430***	75	73	0.005	−0.000	0.023	698***	250**	427***	−0.019***	−0.002	0.011**
	(150)	(226)	(188)	(0.023)	(0.003)	(0.023)	(61)	(102)	(86)	(0.005)	(0.002)	(0.005)
2016	891***	203	147	0.011	−0.003	−0.014	1,150***	654***	763***	−0.031***	0.000	0.016*
	(180)	(206)	(180)	(0.035)	(0.003)	(0.023)	(119)	(115)	(90)	(0.006)	(0.001)	(0.009)
2017	1,321***	940***	863***	−0.002	−0.000	0.010	1,379***	563***	659***	−0.037***	−0.001	0.022**
	(210)	(282)	(301)	(0.043)	(0.004)	(0.022)	(145)	(183)	(94)	(0.009)	(0.002)	(0.010)
2018	1,276***	892***	853***	−0.015	−0.001	−0.018	1,415***	938***	949***	−0.047***	−0.002	0.003
	(231)	(249)	(264)	(0.052)	(0.004)	(0.015)	(107)	(143)	(89)	(0.012)	(0.002)	(0.012)
2019	1,384***	1,095***	949***	−0.029	−0.002	−0.007	1,579***	1,374***	1,483***	−0.062***	−0.000	−0.002
	(269)	(283)	(282)	(0.059)	(0.004)	(0.021)	(86)	(105)	(163)	(0.014)	(0.002)	(0.010)
Observations	1,329	1,329	1,329	1,329	1,329	1,329	2,134	2,134	2,134	2,134	2,134	2,134
Clusters	148	148	148	148	148	148	238	238	238	238	238	238

Notes: This table reports difference-in-differences event study estimates with 2013 as the omitted base year. All regressions include district and year fixed effects and are weighted by ADA. Standard errors clustered at the district level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: DiD Heterogeneity by Urban/Rural — 9–12 Districts (Continued)

Panel B: Migration and Housing

	Non-Urban				Urban			
	(1) Net Mig. Rate (pp)	(2) Income Gap ($)	(3) Minority Gap (pp)	(4) Log Home Value Index	(5) Net Mig. Rate (pp)	(6) Income Gap ($)	(7) Minority Gap (pp)	(8) Log Home Value Index
Treated (55–80%) × Year								
2011	0.567	128*	0.039	−0.012	−0.187	139***	0.005	0.007
	(0.365)	(76)	(0.049)	(0.021)	(0.169)	(49)	(0.022)	(0.008)
2012	0.355	61	0.091**	0.007	−0.188*	6	0.020	0.000
	(0.423)	(92)	(0.044)	(0.011)	(0.098)	(32)	(0.017)	(0.006)
2014	−0.547	−73	−0.064	0.010	0.300***	−2	−0.020	0.023***
	(0.340)	(96)	(0.050)	(0.014)	(0.101)	(30)	(0.017)	(0.008)
2015	0.080	176	0.039	0.023	0.225**	21	0.009	0.041***
	(0.552)	(127)	(0.083)	(0.023)	(0.108)	(35)	(0.017)	(0.012)
2016	0.318	53	−0.073	−0.144	0.280**	10	0.004	0.063***
	(0.447)	(100)	(0.057)	(0.154)	(0.127)	(44)	(0.017)	(0.021)
2017	0.242	−100	−0.046	−0.127	0.285**	−54	0.030	0.082***
	(0.590)	(111)	(0.057)	(0.146)	(0.144)	(39)	(0.020)	(0.022)
2018	0.094	−69	−0.104	−0.120	0.160	−4	0.012	0.090***
	(0.598)	(98)	(0.067)	(0.147)	(0.147)	(39)	(0.021)	(0.022)
2019	−0.775	−71	−0.084	−0.103	0.261	−31	0.047**	0.097***
	(0.760)	(123)	(0.053)	(0.140)	(0.173)	(42)	(0.024)	(0.024)
Treated (80%+) × Year								
2011	0.760**	236***	−0.027	−0.021	0.110	126**	−0.016	−0.002
	(0.369)	(85)	(0.055)	(0.024)	(0.155)	(56)	(0.033)	(0.007)
2012	0.343	78	−0.009	−0.013	−0.126	−4	0.008	0.005
	(0.436)	(125)	(0.077)	(0.011)	(0.094)	(31)	(0.014)	(0.005)
2014	−0.333	55	−0.164***	0.003	0.460***	−62	−0.026	0.021*
	(0.325)	(113)	(0.056)	(0.012)	(0.176)	(44)	(0.032)	(0.011)
2015	0.026	223	−0.106	0.032	0.300*	−44	0.021	0.054***
	(0.615)	(164)	(0.109)	(0.019)	(0.160)	(46)	(0.033)	(0.016)
2016	−0.020	−10	−0.256***	−0.126	0.521**	−72	0.003	0.043
	(0.429)	(114)	(0.075)	(0.154)	(0.261)	(52)	(0.030)	(0.027)
2017	−0.396	−304	−0.178*	−0.103	0.551*	−150**	0.021	0.066**
	(0.665)	(248)	(0.104)	(0.145)	(0.296)	(64)	(0.038)	(0.029)
2018	−0.654	−312	−0.146	−0.104	0.335	−105*	0.028	0.102***
	(0.748)	(263)	(0.104)	(0.147)	(0.272)	(54)	(0.035)	(0.030)
2019	−0.525	3	−0.214**	−0.090	0.463*	−133***	0.053	0.148***
	(0.607)	(122)	(0.094)	(0.140)	(0.247)	(44)	(0.033)	(0.021)
Observations	1,325	1,325	1,325	1,325	2,134	2,134	2,134	2,134
Clusters	148	148	148	148	238	238	238	238

Notes: See Panel A. Net Migration Rate and Minority Gap are in percentage points. Income Gap is in dollars. Log Home Value is the natural log of the Zillow Home Value Index (2020$).

Table A7: Cross-Sectional Association Between Spending Growth and Home Value Growth

	HVI Change (%), K-8		HVI Change (%), 9-12	
	(1) OLS	(2) OLS+Cty	(3) OLS	(4) OLS+Cty
K-12 Spending Change (%)	0.7300***	0.6844***	0.7655***	0.7593***
	(0.1281)	(0.0941)	(0.1597)	(0.1213)
Observations	592	592	372	372
R^2	0.052	0.422	0.058	0.446

Notes: Each column reports the coefficient from a regression of the outcome on the percent change in K-12 per-pupil spending between 2013 and 2019. Odd columns include no additional controls. Even columns include county fixed effects with standard errors clustered at the county level. All regressions are weighted by 2013 average daily attendance. Standard errors in parentheses. Observations with percent changes in spending or home values below the 1st or above the 99th percentile are excluded. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See Section 3 for details.

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