



Turning around schools (and neighborhoods?): School improvement grants and gentrification

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ABSTRACT

Most funding intended to close gaps in K-12 education targets schools, rather than students directly. We investigate whether household sorting in response to changes in K-12 school funding inhibits spending from reaching targeted students with a case study in Metro-Nashville Public Schools of the School Improvement Grant (SIG) program, which invested \$7 billion in the nation's lowest-achieving schools between 2009 and 2016. Using a boundary-discontinuity difference-in-differences design and home sales data, we estimate that households were willing to pay more than three times the average per-pupil grant award to live in SIG school zones. Neighborhoods zoned for SIG schools experienced moderate income and racial integration following funding receipt. However, evictions in these neighborhoods increased by 35%, and non-white enrollment at SIG schools declined by 15%. Our findings illustrate a major limitation of place-based public good provision: sorting may displace the initially targeted population.

1. Introduction

K-12 education funding programs often target schools, rather than students directly. The federal government has distributed funding to high-poverty schools since 1965 through the Title I grant program, totaling \$15 billion per year over the past decade. Because schools in the United States are broadly segregated by race and income (McGrew, 2019), programs that aim to increase resources available to disadvantaged students allocate additional funding to schools with large minority or low-income student populations. However, households may respond to shocks to school spending when choosing schools and neighborhoods, potentially reducing the effectiveness of such targeting. In particular, sorting in response to investments in local schools can result in housing price increases that reflect households' valuation of the investments (Bayer et al., 2020, 2007; Cellini et al., 2010; Tiebout, 1956). Household sorting of this nature may result in less effective targeting of education funding if disadvantaged families are priced out of neighborhoods zoned for newly improved schools.

We examine whether household sorting in response to changes in school funding inhibits spending from reaching targeted students with a case study of the effect of federal grants to low-performing schools in Metro-Nashville Public Schools (MNPS). We study the School

Improvement Grant program (SIG), a Title I program that invested over \$7 billion in the nation's lowest-achieving schools between 2009 and 2016. Using real estate assessment and property transaction records linked to MNPS attendance zones, we find that home values in SIG-receiving school zones increased by 10.5% more than that of houses just outside the attendance zone boundary in response to the investment. Under the assumption that no other factors affecting house prices changed across attendance zone boundaries differentially before and after SIG treatment, our results indicate that households were willing to pay \$3 for every dollar in per-pupil grant aid to live in SIG school zones.

This price increase is consistent with funding for education in SIG schools previously being suboptimally low (Oates, 1969) but also represents a potential barrier for low-income families to access newly improved schools. We characterize sorting in response to SIG and explore its consequences using Consumer Financial Protection Bureau (CFPB) mortgage data, eviction counts from the Eviction Lab at Princeton University, and school enrollment demographics. We show that following program roll-out homebuyers moving into previously majority-minority SIG attendance zones reported 9% higher income and were 13% more likely to be white. At the same time, the share of students

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at SIG schools who are white increased by almost 20%. In tandem with evidence of a 35% increase in evictions in neighborhoods zoned for treated schools and a 15% decline in nonwhite enrollment at SIG schools, our results suggest that SIG-induced gentrification displaced disadvantaged residents from their previous neighborhoods and schools.

We identify effects of school improvement grants on housing prices using a difference-in-differences design that appeals to the boundary-discontinuity estimations used in previous literature (Black, 1999; Schwartz et al., 2014). Exploiting variation in access to schools implementing SIG-funded interventions across space and time, we compare changes in prices of homes sold within a half-mile of attendance zone boundaries for SIG-treated schools. Causal interpretation rests on the assumption that home values of properties on either side of attendance zone boundaries for SIG schools would evolve in parallel in the absence of the program, which is supported by common trends in sale prices prior to SIG funding receipt. Our preferred specification restricts the sample to single-family home and duplex sales and includes boundary segment-by-year fixed effects that account for time-varying neighborhood amenities. However, our results are robust to a variety of alternative specifications. We provide evidence that our estimations are not confounded by the Great Recession and housing crash, rising housing prices common to all neighborhoods zoned for low-performing schools, and changes in neighborhood demographics, but note with caution that we cannot rule out bias from other unobservable neighborhood amenities that differentially change just inside of SIG school zones compared to just outside these zones.

Next, we examine how sorting in response to SIG funding affected the demographic composition of neighborhoods and schools. To do so, we match CFPB mortgage data and eviction counts from the Eviction Lab to MNPS attendance zones. Difference-in-differences estimates that compare Census tracts or block groups within SIG attendance zones to those outside these zones before and after SIG receipt indicate that the reported income of mortgage applicants in neighborhoods zoned for SIG schools increased by 9% within two years of grant receipt and that the share of white home buyers rose by 13%. Enrollment data from the National Center for Education Statistics (NCES) reveal similar patterns of racial integration into SIG schools, with the share of students who are white increasing by almost 20% relative to that at non-SIG schools. These results suggest that SIG funding led to moderate integration by income and race in previously majority-minority neighborhoods. However, this influx of higher-income households into SIG neighborhoods also displaced some disadvantaged residents: evictions increased by 35% in neighborhoods zoned for SIG schools relative to non-SIG attendance zones after grant receipt, and nonwhite enrollment at SIG schools declined by 15%.

SIG likely had heterogeneous effects on housing prices across housing markets, and our primary estimates represent local average treatment effects from SIG's implementation in Nashville, a booming housing market. We provide support for the generalizability of our findings by examining the household sorting response to SIG in California, a state where we observe eligibility and funding receipt for all SIG cohorts and which successfully used SIG funding to improve student achievement in low-performing schools (Friday, 2021). Matching data on housing prices, mortgage characteristics, and evictions to school attendance zones in California, we estimate a difference-in-differences specification that compares changes in neighborhoods zoned for schools that received SIG funding to those zoned for SIG-eligible schools that did not receive grants. We find evidence of willingness-to-pay for school funding and neighborhood sorting that is broadly consistent with the results from our case study: for every \$1000 in per-pupil SIG funding, home values in California increased by 3% and the reported income of homebuyers in neighborhoods zoned for SIG schools increased by 3.5%.

Our analysis advances existing research on the relationship between school characteristics and home values by estimating the capitalization of SIG funding (and any associated changes in school quality) into local

housing prices. Prior research finds that housing prices increase by 3%–10% in response to a standard deviation increase in school achievement² and found that households are willing to pay more than \$1 for every dollar increase in K-12 spending (Barrow & Rouse, 2004; Bayer et al., 2020; Cellini et al., 2010). We contribute to this literature by estimating willingness-to-pay for school spending targeting schools at the bottom of the achievement distribution, where additional funding may be especially impactful. Our estimates of over a \$3 increase in house price for a dollar in per-pupil grant aid suggest that education funding among this subgroup falls well below the efficient level of provision (Oates, 1969).

Our results also add to a growing literature examining how changes in the provision of K-12 education influence neighborhood sorting. Notable policies addressing the inequitable provision of K-12 education include desegregation, school choice, and funding increases targeting low-performing schools. Baum-Snow and Lutz (2011) show that court-ordered school desegregation efforts induced white-flight into suburban school districts. Moreover, previous research shows that both “exit options” (Schwartz et al., 2014; Zheng, 2019) and “forced choice” (Wigger, 2020) varieties of school choice weaken the link between neighborhood school characteristics and home values.³ If our results are interpreted as causal, we extend this literature by showing that investments to improve low-performing schools can make neighborhoods served by those schools more desirable and attract wealthier households. Together, these findings indicate the need to consider the potential for neighborhood sorting and displacement when designing K-12 finance and assignment policies aiming to make schooling more equitable.

More broadly, this paper contributes to literatures on the relationship between public good provision and housing markets and the nature of gentrification of low-income neighborhoods. Following SIG-funded interventions, wealthier and whiter residents moved into neighborhoods zoned for SIG schools. Integration into SIG school zones is consistent with the pattern of sorting found in response to other education-related policies (Billings et al., 2017) and broader place-based policies targeting low-income neighborhoods (Diamond & McQuade, 2019). We contribute to these findings by testing whether sorting displaces existing residents of neighborhoods benefiting from increased public good provision. In contrast to previous literature on gentrification, which found little empirical evidence of displacement in gentrifying neighborhoods (Disalvo, 2022; McKinnish et al., 2010), our results suggest that gentrification displaced a non-negligible share of existing residents of SIG neighborhoods via increased evictions. Taken together, our findings illustrate a major limitation of place-based public good provision: sorting, or concurrent gentrification, may displace the initially targeted population.

2. The school improvement grant program

2.1. Program overview

In an effort to boost achievement in the nation's “persistently lowest-achieving schools”, the American Recovery and Reinvestment Act of 2009 allotted \$3 billion to the School Improvement Grants (SIG) program to fund aggressive school turnaround programs. An additional \$4 billion in funding for SIG between 2010 and 2016 followed, and by 2012 the SIG program had invested up to \$6 million per school in

² See, for instance, Bayer et al. (2007), Black (1999), Black and Machin (2011), Collins and Kaplan (2017), Dhar and Ross (2012), Gibbons et al. (2013), Kane et al. (2006), Machin (2011), Schwartz et al. (2014), and Caetano (2019).

³ Wigger (2020) defines “exit options” as choice policies that provide families alternatives to their assigned neighborhood schools and “forced choice” as assignment policies that require all families to submit school choice applications and do not utilize neighborhood school boundaries.

more than 1300 of the country's lowest-achieving schools (Department of Education, 2012).

The federal government first allotted SIG funds to state education agencies (SEAs) via grants based on existing Title I funding formulas. To receive funding, states submitted applications to the Department of Education (Ed) with identified SIG-eligible schools, complying with Ed's three tiers of eligibility. Tier I schools, the highest priority for SIG funds, were comprised of the lowest-achieving five percent of Title I schools in improvement, corrective action, or restructuring in the state. Similarly, Tier II schools consisted of the lowest-achieving five percent of schools eligible for, but not receiving, Title I funds for school improvement. All remaining Title I schools in improvement, corrective action, or restructuring were designated as Tier III schools. SEAs distributed 95% of SIG funds to local education agencies (LEAs; i.e., school districts) to implement school turnaround programs in eligible schools, prioritizing funding toward districts with Tier I and II schools.

Schools awarded SIGs were to use the grant money to implement one of four schoolwide intervention models, each with the dual purpose of disrupting the status quo (i.e., making substantial changes to school operations and staff) and increasing school resources (Zimmer et al., 2017). 95% of SIG schools implemented either the "transformation model" (75%) or the "turnaround model" (20%) (Ginsburg & Smith, 2018).⁴ Both interventions required replacing the principal, implementing a teacher evaluation system accounting for student achievement growth, and increasing learning time; the turnaround model further mandated the replacement of at least 50% of school staff. Increasing learning time intuitively should lead to learning gains, and student achievement also likely benefits from the replacement of low-performing teachers if new teachers are more effective, which becomes more likely with achievement-based evaluations. Empirical evidence supports this notion: in a meta-analysis of school turnaround interventions, Schueler et al. (2020) identify extended learning times and teacher replacement as characteristics of interventions associated with greater effects on student achievement.

Research on school improvement grants generally finds immediate positive effects of the program on student achievement in various local and statewide settings (Carlson & Lavertu, 2018; Friday, 2021; Sun et al., 2020, 2017), and studies with extended time horizons show that test score gains last beyond the three-year intervention (Sun et al., 2020) and may even increase over time (Friday, 2021). Identified underlying mechanisms include SIG treatment reducing unexcused absences, improving retention of effective teachers, and developing greater teacher professional capacity (Sun et al., 2017). A notable exception to the literature's consensus, nationwide analysis in Dragoset et al. (2017) finds no significant effect of SIG-funded interventions on math or reading test scores, high school graduation, or college enrollment. However, the study only analyzes achievement data from 2012 and 2013 and does not estimate longer-term effects.

2.2. SIG and household sorting

Through increasing both perceived and real school quality, SIG-funded interventions may disrupt existing housing market equilibria in settings where school assignment depends on household location. We outline how this may take shape, appealing to the model of public good provision developed by Tiebout (1956).

Canonical theory posits that households choose communities to reside in based in part on the provision of local public goods, "voting with their feet" to reveal their preferences (Tiebout, 1956). Researchers have often applied this framework to study the relationship between

observable measures of school quality and housing prices,⁵ taking advantage of the U.S.'s general reliance on neighborhood schools that admit only (or at least guarantee spots for) students residing in a specific attendance zone. Households that value school quality sort into neighborhoods zoned for high-quality schools, yielding an equilibrium where, all else equal, housing prices across school attendance zones reflect differences in the marginal household's willingness to pay for school quality. A robust literature provides evidence for the capitalization of school quality into housing prices in the U.S., generally finding that a one-standard deviation increase in test scores raises home values by 2%–4% percent (Black, 1999; Black & Machin, 2011; Gibbons et al., 2013; Machin, 2011), although a price gradient as large as 10% for a standard deviation increase in test scores has been found in urban settings (Kane et al., 2006).

Households may observe SIG-funded interventions and their effects on school quality through multiple channels. In our setting of Davidson County, local newspapers reported on the receipt of SIG funding (Barnes, 2015; Gonzales, 2015c), as well as specific aspects of school reform prescribed by SIG interventions such as principal replacement (Gonzales, 2015a, 2015b) and extending learning time (Beecher, 2014). If parents expect SIG-funded interventions will improve low-performing schools, or more generally value school inputs, then housing prices should respond fairly quickly to the announcement of grant receipt and local news coverage. However, parents may instead update beliefs over school quality when they observe changes in observable measures such as standardized test scores (Figlio & Lucas, 2004). Organizations such as GreatSchools publish simple school ratings based on test score levels and growth to inform parents choosing schools. GreatSchools partners with Zillow (and did so at the time of SIG implementation in MNPS), so that households can easily observe school characteristics when searching for homes. Existing literature suggests that the household sorting response to SIG should increase over time. In particular, Sun et al. (2017) find an increase in the popularity of SIG schools on school choice applications in San Francisco that grows over time and mirrors the pattern of achievement effects found by the authors. This supports the notion that SIG interventions are salient enough to elicit behavioral responses from households when they improve student outcomes.

Sorting in response to SIG may inhibit funding from reaching targeted students and induce broad change of neighborhood demographics. In particular, relatively wealthy households with substantial willingness to pay for access to SIG-funded schools may price out low-income households of neighborhoods zoned for these newly improved schools. Billings et al. (2017) find sorting of relatively high-income households into neighborhoods zoned for previously low-performing schools in Charlotte, NC following school closures under No Child Left Behind that gave school choice priority to students residing within attendance zones of newly closed schools. On a larger scale, Bayer et al. (2020) exploit exogenous variation in school spending from court-mandated school finance reforms across the country and estimate that school poverty rates decreased by 0.21% in response to a 1% increase in school spending. SIG may elicit a similar response by drastically increasing school spending over three years to implement a turnaround reform.

2.3. SIG and Metro-Nashville Public Schools

This paper examines the neighborhood sorting response to SIG funding receipt in Nashville, Tennessee. Serving Nashville and the surrounding area that constitutes Davidson County, Metro Nashville Public Schools (MNPS) consisted of 67 elementary schools, 28 middle schools, and 12 high schools as of the 2009–2010 school year,

⁴ The other 5% of SIG schools implemented either the "restart model", which handed over schools to a charter management organization, or the "closure model", which shut down low-performing schools and allowed previously assigned students to enroll in higher-achieving schools within the district.

⁵ E.g., Bayer et al. (2007), Black (1999), Black and Machin (2011), Caetano (2019), Cellini et al. (2010), Collins and Kaplan (2017), Dhar and Ross (2012), Gibbons et al. (2013), Kane et al. (2006), Machin (2011), Schwartz et al. (2014), and Bayer et al. (2020).

when SIG was expanded by the ARRA. Students are zoned to attend neighborhood schools based on their residence and follow “pathways” across school levels in MNPS, such that all students from a given elementary school feed into the same middle school, and all students from said middle school move onto the same high school. MNPS offers multiple forms of “exit option” school choice, including magnet schools and charter schools, which potentially allow for families dissatisfied with their assigned neighborhood school to move to a different school. However, at the beginning of the twenty-first century the predominant form of choice was to leave the district altogether: between 2000 and 2012, MNPS lost nearly 10 percent of its student population annually to private schools and other public school districts. Simultaneously, the percent of the MNPS student population qualifying for free or reduced-price meals rose from 45 percent to over 70 percent.

When the SIG program was expanded under the Obama Administration, MNPS presented a clear need to address its lowest-performing schools. Achievement across the district was poor: the percentage of students in grades 3–8 meeting proficiency in Math was just 28%. Furthermore, the district’s low-performers fell even further below district goals. Proficiency rates on standardized tests were below 20% in reading and 10% in math at seven middle schools.

MNPS received school improvement grants in the 2012–2013 and 2015–2016 school years to implement intervention models in two cohorts of low-performing schools. The 2012 cohort consisted of three middle schools (Brick Church, Gra-Mar, and John Early Paidea Magnet) and three elementary schools (Buena Vista, Napier, and Robert Churchwell Museum Magnet).⁶ Two middle schools and one elementary school implemented the turnaround model, while the remaining two middle schools and elementary school carried out the transformation model. The 2015 cohort consisted of two elementary schools expanding early learning programs (Inglewood and John Whitsitt), two middle schools implementing the turnaround model (Jere Baxter and Madison), a high school implementing the transformation model (Pearl Cohn), and an elementary school undergoing a restart as a charter school (KIPP at Kirkpatrick).⁷ Fig. 1 depicts geographic variation in SIG treatment across the district, and Figure A1 portrays the location of traditional and charter schools in MNPS.

Because the Tennessee Department of Education (TDOE) switched testing regimes in 2015, rendering comparisons of student achievement using publicly available school-level testing data before and after the change uninformative, we are limited in our ability to estimate the effects of SIG-funded interventions on test scores in MNPS.⁸ We instead rely on the previously mentioned literature as evidence of the “first-stage” of our analysis. We also speak to one potential mechanism through which SIG improved outcomes. By year two of interventions, the 2012 cohort of schools in MNPS had increased their school year length by an average of 47.8 h, equivalent to an additional week of instruction and a 3.9% increase relative to the 2011–2012 mean.

⁶ Notably, two treated schools are magnet schools. Since these do not map to attendance zones, we do not examine them in our housing price analysis. The presence of desirable magnet schools that do not require residence in specific neighborhoods for enrollment would attenuate estimates of SIG on housing prices by offering households zoned for untreated schools a degree of access to SIG schools. Still, magnet school admission in MNPS is not guaranteed, in contrast to zoned neighborhood schools.

⁷ Although not initially one of the four prescribed turnaround programs, SIG grants in later cohorts could be used to add or expand pre-k and kindergarten programs at elementary schools through the “Early Learning” module.

⁸ Appendix Table A1 presents difference-in-differences estimates of the test score effects of SIG for the 2012 cohort in MNPS, using school-by-grade achievement data from TDOE. Although noisy, estimates suggest that SIG interventions improved the lower tail of achievement at treated schools, as evident by reductions the share of students scoring “Below Basic” in reading and math.

3. Data and descriptive statistics

3.1. Davidson county property sales

We create a panel of Davidson County parcels and sales by year using property assessment and sales data obtained from the Metro Nashville Davidson County Division of Assessments. We observe the sales price, transaction date, and parcel location for all parcel transfers from 2000 until 2019. Through linking sales records to the most recent prior county assessment, we observe parcel and housing characteristics at the time of sale (e.g. the number of stories and bedrooms). We drop all sales with missing sales prices or those of less than \$1000, limiting our sample to “arms-length” transactions. Furthermore, we restrict our analysis to single-family homes and duplexes, the most likely forms of housing utilized by households with school-age children. Our final panel includes 86,651 sales involving 41,116 unique properties.

In order to designate SIG treatment to homes, we first match parcels with a geocoded map of school assignment zones for Metro Nashville Public Schools. We use zones from a year prior to the initial cohort of SIG in MNPS as our baseline, avoiding bias from any potentially endogenous changes in zoning following SIG. After geocoding our sales data through the Census Bureau’s geocoding service, we match each parcel to their assigned elementary, middle, and high school. Furthermore, we calculate the distance between parcels and assignment zone boundaries, allowing us to limit our sample to homes close to boundaries of SIG-treated schools during analysis.

Homes are assigned treatment based on whether their zoned elementary, middle, or high school implement a SIG-funded intervention. Table 1 presents summary statistics and balance tests for homes sold from 2000 to 2011 (all sample years prior to the implementation of SIG interventions for the earliest cohort) zoned for schools that later received SIG treatment compared to those zoned for untreated schools. Columns 1, 2, and 3 correspond to sales across our entire sample in 2010, while Columns 4, 5, and 6 limit sales to those of parcels within a half-mile of SIG school zone boundaries. Homes zoned for SIG schools sold for over \$55,000 less (in 2010 dollars) than those zoned for non-SIG schools prior to treatment. This difference can partially be explained by the smaller size of homes in SIG school zones, as measured by the number of rooms, baths, finished square footage, and property acreage. A joint F-test confirms what is clear from the differences across individual factors: homes sold in the pre-period significantly differed across observables by their treatment status.

3.2. Neighborhood composition

We further characterize the household sorting response to SIG using data on neighborhood composition from multiple sources. Baseline neighborhood demographics are observed in the 2010 Decennial Census, which we link to treatment data by matching geocoded maps of Census block groups and MNPS school zones. Table 2 depicts average household characteristics for block groups zoned for untreated and SIG schools. SIG and non-SIG neighborhoods consisted of starkly contrasting compositions of races: non-SIG school zones were 76.3% white, while SIG zones were 56.1% Black. Households in SIG neighborhoods typically housed more occupants than those in non-SIG zones, and untreated and SIG neighborhoods also differed in the type of housing utilized: households in SIG block groups were 13.4 percentage points more likely to be renter-occupied. Median household income in non-SIG school zones nearly doubled that of SIG zones, consistent with other stark socioeconomic differences. Finally, the last two rows of Table 2 present summary statistics of data from the Eviction Lab at Princeton University, which has made aggregations of individual records of eviction cases from courts across the country publicly available. SIG block

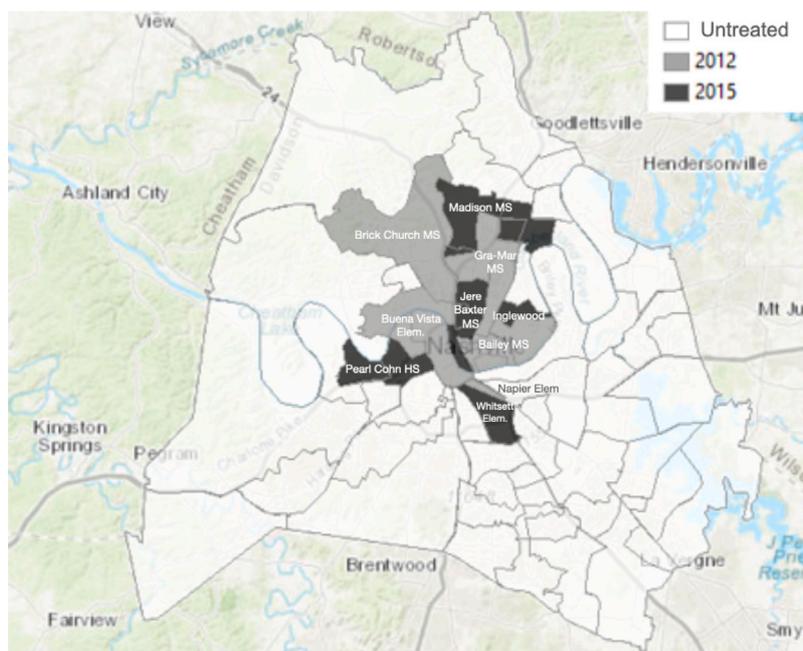


Fig. 1. MNPS school zones: SIG intervention year. Notes: This figure maps MNPS school zones, denoting attendance zones for schools receiving school improvement grants (labeled by recipient school) in the 2012–2013 and 2015–2016 school years. Each attendance zone corresponds to a pathway of assigned elementary, middle, and high schools.

Table 1
Davidson county sales data.

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Close to boundary		
	Untreated	SIG	Diff	Untreated	SIG	Diff
Sale price	180,987 (139,282)	125,216 (101,423)	-55,771*** (1084)	138,963 (85,441)	115,400 (73,624)	-23,562*** (1358)
Ln (Sale price)	11.886 (0.659)	11.489 (0.727)	-0.397*** (0.006)	11.691 (0.563)	11.473 (0.642)	-0.219*** (0.010)
Bedrooms	3.034 (0.794)	3.005 (0.841)	-0.029*** (0.007)	2.969 (0.768)	2.928 (0.809)	-0.041*** (0.013)
2+ Stories	0.112 (0.315)	0.106 (0.307)	-0.006** (0.003)	0.071 (0.257)	0.085 (0.279)	0.014*** (0.005)
Rooms	6.454 (1.747)	6.163 (1.620)	-0.290*** (0.015)	6.135 (1.509)	5.960 (1.488)	-0.175*** (0.025)
Baths	1.778 (0.815)	1.724 (0.711)	-0.055*** (0.007)	1.658 (0.694)	1.672 (0.692)	0.014 (0.012)
Half baths	0.272 (0.467)	0.199 (0.420)	-0.072*** (0.004)	0.187 (0.408)	0.161 (0.384)	-0.026*** (0.007)
Finished Sq. Feet	1775 (854)	1569 (653)	-206*** (7)	1562 (638)	1501 (643)	-61*** (11)
Parcel acreage	0.442 (0.931)	0.301 (0.511)	-0.140*** (0.007)	0.395 (1.044)	0.335 (0.585)	-0.061*** (0.014)
Building age	41.123 (26.528)	45.072 (31.229)	3.950*** (0.258)	41.286 (25.959)	41.371 (28.181)	0.085 (0.460)
Observations	25,793	25,827	51,620	7262	6808	14,070

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents summary statistics and balance tests for homes sold from 2000–2011 zoned for schools that later received SIG treatment compared to those zoned for untreated schools. Columns 1 through 3 correspond to the entire sample of 2010 sales in Davidson County, while Columns 4 through 6 limit sales to parcels within a half-mile of attendance zone boundaries for SIG schools. P-values for Columns 3 and 6 represent standard t-tests of mean equality. Sales prices are in 2010 dollars, and the underlying data were obtained from the Metro Nashville Davidson County Division of Assessments.

groups had approximately triple the number of court-ordered evictions and eviction filings per 1000 residents as non-SIG block groups.⁹ Notably, differences in observable neighborhood characteristics substantially shrink when we limit the sample to block groups contiguous

⁹ An eviction filing may result in a court ruling for or against eviction or a settlement between the landlord and tenant.

to SIG attendance zone boundaries. For example, the difference in median household income falls from \$27,812 to \$4456.

To examine sorting in response to SIG, we use loan-level Consumer Financial Protection Bureau mortgage data, available from 2007 to 2017. Although the exact address of homes are suppressed to maintain privacy, individual mortgages are identified by Census tracts, which we match to school zones. The data include both characteristics of the applicant (race, gender, income) and the loan (amount, agency, property type, approval status). By observing applicant income and

Table 2
Davidson County 2010 occupied housing demographics.

	(1)	(2)	(3)	(4)	(5)	(6)
	All block groups			Contiguous block groups		
	Untreated	SIG	Diff	Untreated	SIG	Diff
% White	0.763 (0.190)	0.367 (0.249)	-0.395*** (0.010)	0.694 (0.203)	0.452 (0.242)	-0.242*** (0.019)
% African American	0.142 (0.159)	0.561 (0.274)	0.419*** (0.009)	0.183 (0.188)	0.484 (0.265)	0.301*** (0.019)
% Households with 3+ occupants	0.323 (0.139)	0.357 (0.109)	0.033*** (0.007)	0.279 (0.149)	0.320 (0.087)	0.041*** (0.013)
% Renter occupied	39.076 (26.889)	52.515 (20.827)	13.440*** (0.920)	53.396 (25.803)	49.802 (22.899)	-3.595** (1.685)
Median household income	59,168 (38,113)	31,356 (11,636)	-27,812*** (12,23)	37,391 (17,037)	32,935 (12,525)	-4456*** (1066)
Median gross rent	817.852 (363.861)	676.294 (230.955)	-141.557*** (12.289)	724.408 (222.556)	654.334 (255.341)	-70.073*** (16.298)
Eviction rate	6.062 (9.770)	17.102 (15.626)	11.039*** (0.634)	8.899 (10.188)	18.845 (20.590)	9.946*** (1.571)
Eviction filings rate	11.768 (20.114)	34.325 (36.094)	22.557*** (1.375)	20.059 (27.452)	38.006 (46.797)	17.947*** (3.783)
Observations	4149	1207	5356	718	416	1134

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents summary statistics and balance tests for demographics from Davidson County blocks in the 2010 Decennial Census. Columns (1) through (3) reflect the entire county, while columns (4) through (6) limits the sample to “contiguous” block groups, defined as those that are at least partially contained within a half-mile buffer around SIG attendance zone boundaries. Block groups that do not completely fall on one side or the other of the boundary (i.e., those that are partially treated) are tossed out. P-values for Columns 3 and 6 represent standard t-tests of mean equality.

Table 3
Davidson County mortgage characteristics, 2007–2011.

	(1)	(2)	(3)
	Untreated	SIG	Diff
Loan amount	180,816 (36,715)	121,510 (30,624)	-59,306*** (6685)
Applicant income	75,234 (16,752)	51,344 (17,078)	-23,890*** (3630)
White share of borrowers	0.721 (0.163)	0.545 (0.298)	-0.176*** (0.061)
Black share of borrowers	0.125 (0.167)	0.311 (0.315)	0.186*** (0.064)
Observations	25	186	532

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents summary statistics and balance tests for pre-SIG mortgage characteristics using data from the Consumer Finance Protection Bureau. Individual mortgages are identifiable at the census tract-level, and census tracts are coded as treated (SIG) if they are fully contained within school attendance zones for SIG schools; similarly, groups are coded as untreated if they are fully contained within attendance zones of non-SIG schools. P-values for Column 3 represent standard t-tests of mean equality.

race, we can characterize households moving into SIG school zones following grant receipt and test if the sorting response is consistent with gentrification. Table 3 presents summary statistics and balance tests for mortgages of homes sold in SIG and non-SIG school zones from 2007 to 2011, demonstrating that homebuyers in SIG neighborhoods received smaller loans, reported lower income, and were more likely to be Black prior to treatment.

3.3. School characteristics

We utilize school-level data on student achievement and demographics from the Tennessee Department of Education and National Center for Education Statistics Common Core of Data to present baseline differences in characteristics of SIG and non-SIG schools. Along with enrollment demographics, we observe the share of students in grades 3–8 that score in each of four achievement categories of the state’s standardized exams in math and reading, Below Basic, Basic, Proficient, and Advanced, since the 2009–2010 school year. As shown

in Table 4, student demographics starkly differed between SIG and untreated schools in 2010. SIG elementary schools served an almost exclusively Black population, while 44.9% of students at non-SIG elementary schools were Black—a slim plurality. The student populations of SIG and untreated middle schools similarly differed along demographics (79.4% and 37.6% Black, respectively). Student achievement across MNPS was quite low; still, SIG schools stood out as the lowest performers. Two-thirds of students at SIG middle schools scored Below Basic in math, compared to 47% of students in non-SIG middle schools.

4. Methods

We exploit variation in access to SIG-funded schools across space and time via a boundary-discontinuity difference-in-differences design to identify the capitalization of school improvement grants into housing prices. To motivate this design, consider a canonical hedonic model of housing markets:

$$\ln(P_{iz}) = \beta Q_z + \gamma X_{iz} + \epsilon_{iz} \tag{1}$$

where the price of house i in attendance zone z P_{iz} is a function of school quality Q_z and observable house characteristics X_{iz} . If unobserved house or neighborhood characteristics are correlated with school quality, then estimates of β will be biased. Researchers have addressed this endogeneity issue by extending the regression discontinuity design to analysis of housing markets, noting that school quality discontinuously changes at school attendance zone boundaries (Black, 1999; Gibbons et al., 2013; Schwartz et al., 2014). The “boundary discontinuity design” is often specified through boundary segment fixed effects ω_k , along with restricting the sample to homes close (e.g., within a half-mile) to shared attendance zone boundaries.

To isolate the effects of school improvement grants from preexisting differences in school quality, we follow Schwartz et al. (2014) in extending the boundary-discontinuity approach to a dynamic setting. One way to do so would be to estimate the following two-way fixed effects (TWFE) specification:

$$\ln(P_{izkt}) = \alpha_0 + \gamma X_{izkt} + \alpha_z + \alpha_t + \omega_k + \beta^{TWFE} SIG_{izt} + \epsilon_{izkt} \tag{2}$$

where α_z accounts for time-invariant differences in home prices across school zones, α_t captures time-varying shocks to prices that affect

Table 4
MNPS 2010 demographics and achievement.

	(1)	(2)	(3)	(4)	(5)	(6)
	Elementary schools			Middle schools		
	Untreated	SIG	Diff	Untreated	SIG	Diff
Total enrollment	183 (80)	164 (42)	-19 (57)	568 (144)	430 (91)	-138 (105)
% White	0.350 (0.224)	0.016 (0.009)	-0.333** (0.160)	0.393 (0.145)	0.129 (0.035)	-0.264** (0.105)
% African American	0.449 (0.267)	0.962 (0.010)	0.513*** (0.190)	0.376 (0.120)	0.794 (0.094)	0.417*** (0.088)
% Free or reduced lunch	0.746 (0.219)	0.948 (0.042)	0.201 (0.156)	0.712 (0.168)	0.923 (0.005)	0.210 (0.122)
% Below basic RLA	0.180 (0.081)	0.477 (0.149)	0.297*** (0.060)	0.221 (0.075)	0.349 (0.038)	0.128** (0.055)
% Below basic math	0.174 (0.084)	0.422 (0.036)	0.249*** (0.060)	0.470 (0.132)	0.664 (0.071)	0.193* (0.096)
% Basic RLA	0.473 (0.093)	0.427 (0.066)	-0.045 (0.066)	0.443 (0.064)	0.487 (0.028)	0.044 (0.046)
% Basic math	0.465 (0.089)	0.462 (0.010)	-0.003 (0.063)	0.339 (0.048)	0.275 (0.068)	-0.064* (0.036)
% Proficient RLA	0.266 (0.087)	0.087 (0.081)	-0.179*** (0.062)	0.287 (0.082)	0.144 (0.001)	-0.143** (0.060)
% Proficient math	0.259 (0.077)	0.101 (0.017)	-0.157*** (0.055)	0.135 (0.058)	0.046 (0.004)	-0.089** (0.042)
% Advanced RLA	0.082 (0.077)	0.009 (0.002)	-0.073 (0.055)	0.049 (0.048)	0.020 (0.012)	-0.029 (0.034)
% Advanced math	0.103 (0.076)	0.014 (0.009)	-0.089 (0.054)	0.055 (0.045)	0.015 (0.006)	-0.041 (0.032)
Observations	61	2	63	19	4	23

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents summary statistics and balance tests for demographics and standardized test scores at schools receiving SIG grants in 2012 and non-SIG schools in MNPS. Columns 1 through 3 correspond to elementary schools, while Columns 4 through 6 correspond to middle schools. There were no treated high schools in the initial SIG cohort at MNPS. P-values for Columns 3 and 6 represent standard t-tests of mean equality. The underlying data were obtained from the National Center for Education Statistics Common Core of Data and the Tennessee Department of Education.

the entire sample—such as the 2007 housing crash, and SIG_{it} is an indicator variable that equals one for sales of homes zoned for SIG schools occurring after grant receipt.

Goodman-Bacon (2021) shows that when treatment timing varies, estimates of β^{TWFE} will be biased if treatment effects are dynamic.¹⁰ There are multiple reasons to believe SIG-funded interventions will generate dynamic treatment effects in housing markets. Grant funding through the program is received over three years to implement turnaround interventions. If families value school inputs, they may desire access to treated schools immediately upon funding receipt. On the other hand, parents may not base beliefs in school quality off of school inputs, but rather school output, in which case demand for housing in SIG-zoned schools would only increase if and when observable achievement metrics improve. Previous literature suggests that improvements in achievement from SIG interventions increase over time (Friday, 2021; Sun et al., 2017), and as the observable improvements in achievement become more salient, households may increasingly desire access to SIG schools. If this holds true, comparisons of homes zoned for the 2015 cohort of SIG schools to those of the 2012 cohort would bias estimates of β^{TWFE} .

To bypass the bias identified by Goodman-Bacon (2021), we follow Deshpande and Li (2019) and Flynn and Smith (2021) in estimating a stacked difference-in-differences specification to identify effects of SIG-funded interventions on housing prices, an estimation strategy that

¹⁰ In particular, Goodman-Bacon (2021) shows that when treatment timing varies, estimates of β^{TWFE} partially reflect comparisons of newly treated units to previously treated units. If treatment effects are dynamic, this use of previously treated units as a control group typically biases estimates of β^{TWFE} away from the sign of the true treatment effect. Intuitively, units still responding to treatment are not a valid counterfactual to represent potential outcomes in the absence of treatment.

will make no comparisons based off of variation in treatment timing. To implement this, we first re-organize our data into two “stacks”, representing our two cohorts of treated schools. The first stack is comprised of homes sold in attendance zones for schools receiving SIG grants in 2012, labeled as treated homes, and homes sold in attendance zones for schools that never receive a SIG grant, labeled as untreated homes. Similarly, the second stack is comprised of homes sold in attendance zones for schools receiving SIG grants in 2015, labeled as treated homes, and homes sold in attendance zones for schools that never receive a SIG grant, labeled as untreated homes. We further restrict the 2012 stack to consist of only homes close to attendance zone boundaries for schools treated in 2012 and similarly restrict the 2015 stack to consist of only homes close to attendance zone boundaries for schools treated in 2015, following the boundary discontinuity literature (Black, 1999; Gibbons et al., 2013; Schwartz et al., 2014).¹¹

Fig. 2 visually portrays the identifying variation, mapping MNPS school zone boundaries with half-mile buffers surrounding attendance zone boundaries that separate zones for schools receiving school improvement grants in the 2012–2013 and 2015–2016 school years from untreated schools. The first stack makes comparisons of homes within a half-mile of boundaries for schools treated in 2012 (dark boundary segments, light buffer), and the second stack makes comparisons of homes within a half-mile of boundaries for schools treated in 2015 (light boundary segment, dark buffer).¹² Our working data set for estimations consists of these two stacks appended together.

The stacked difference-in-differences estimation separately estimates a standard difference-in-differences for each cohort of SIG

¹¹ We also omit boundaries along rivers or highways, following Black (1999) in excluding borders that physically divide neighborhoods.

¹² We refer to school years by their fall calendar year (i.e., calling the 2012–2013 school year “2012”).

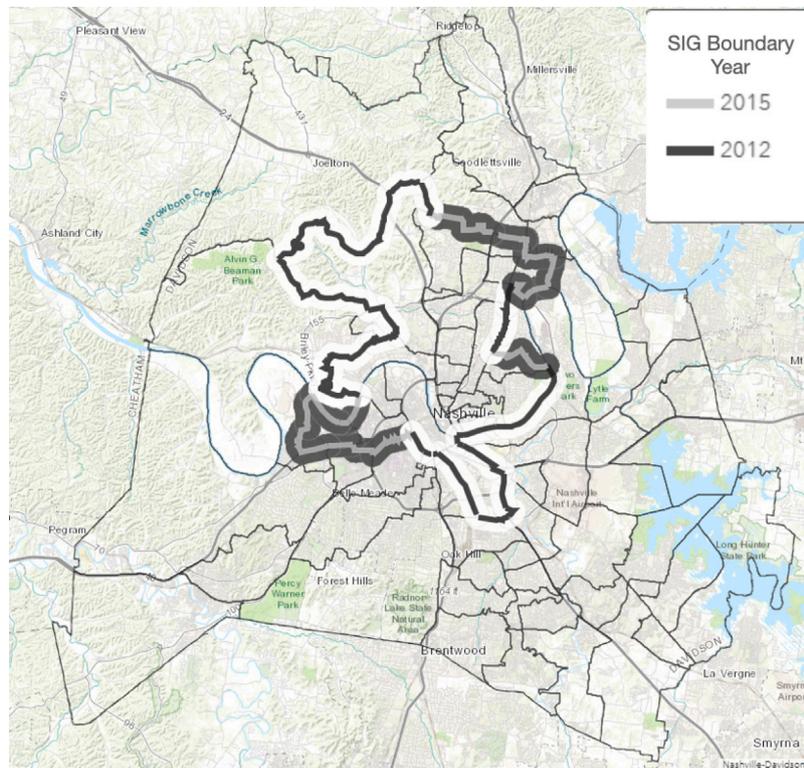


Fig. 2. Stacked DD geographic variation. Notes: This figure maps MNPS school zone boundaries, tracing out half-mile buffers surrounding attendance zone boundaries that separate schools receiving school improvement grants in the 2012–2013 and 2015–2016 school years from untreated schools. Our stacked difference-in-differences estimation compares changes in sale prices of homes located just inside attendance zone boundaries for schools receiving SIG grants in to untreated schools located just outside these boundaries. The first stack makes comparisons of homes within a half-mile of boundaries for schools treated in 2012 (dark boundary segments, light buffer) and the makes comparisons of homes within a half-mile of boundaries for schools treated in 2015 (light boundary segment, dark buffer).

schools, which compares changes in the sale price of homes located just inside SIG attendance zones to untreated homes located just outside SIG attendance zones, and averages the two simple difference-in-differences coefficients from the 2012 and 2015 stack. Formally, we estimate the following specification:

$$\ln(P_{iczk}) = \alpha_0 + \gamma X_{izkt} + \alpha_{cz} + \alpha_{ct} + \omega_{ck} + \beta^{stacked} SIG_{iczt} + \epsilon_{iczk} \quad (3)$$

where $i, c, z, k,$ and t index homes, stacks, school attendance zones, boundary segments, and years, respectively. Because sale price P_{iczt} is determined in part by the physical qualities of a unit, we control for X_{izkt} , a vector of home characteristics observed in the most recent assessment that includes the number of bedrooms and bathrooms, the age of the home, and the size of both the lot and home itself. All fixed effects, α_{cz}, α_{ct} , and ω_{ck} are indexed by both stack and another characteristic (school zone, year, and border segment, respectively). Segmenting fixed effects by stack ensures that $\beta^{stacked}$ is only identified off of within-stack (i.e., within cohort) variation. Specifically, this prevents $\beta^{stacked}$ from reflecting any comparison that uses already-treated homes as comparisons.¹³ With this in mind, α_{cz} and α_{ct} serve the same purpose as their TWFE counterparts, school zone and year fixed effects: to capture time-invariant differences in home prices across school zones and time-varying shocks to prices that affect all

¹³ Although Goodman-Bacon (2021) identifies comparisons of newly to previously treated units as a potential source of bias, yet-to-be treated units can serve a valid counterfactual for newly treated units (under the assumption of exogenous treatment timing). In our setting, however, exploiting this type of comparison would come at a cost. Since the later-treated cohort began interventions only three years after the earlier cohort, we would have to restrict our post period to three years to maintain a balanced panel. If housing prices are sticky, this may not be a long enough post period to detect price responses to SIG-funded interventions.

of Nashville, respectively. Moreover, stack-by-boundary-segment fixed effects ω_{ck} account for time-invariant neighborhood amenities that plausibly benefit houses on either side of an attendance zone boundary, and augmenting the specification to instead include stack-by-boundary-segment-by-year fixed effects ω_{ckt} will account for changes over time in neighborhood amenities common across an attendance zone boundary. Applying the intuition of Black (1999), we specify boundary fixed effects as dummy variables indicating to which segment of the attendance zone boundaries of stack c 's treated schools a particular home is located closest. Finally, SIG_{iczt} is an indicator variable that equals one for sales of homes zoned for treated schools in stack c occurring after the intervention period.

Interpreting β as the causal effect of a SIG-funded intervention on home values requires the identifying assumption that in the absence of the program, house prices would evolve in parallel on either side of attendance zone boundaries for would-be treated schools. Although this assumption is fundamentally untestable, an implication is that prices evolve similarly leading up to the start of SIG funding receipt. To test this, and to flexibly estimate the full dynamics of SIG treatment, we specify the following stacked event study:

$$\ln(P_{iczk}) = \alpha_0 + \gamma X_{izkt} + \alpha_{cz} + \alpha_{ct} + \omega_{ck} + \sum_{y=-11}^7 \pi_y 1\{t-t_c^* = y\} * treat_{icz} + \epsilon_{iczk} \quad (4)$$

where t_c^* is the initial year of SIG funding receipt in stack c (2012 for $c = 1$ and 2015 for $c = 2$) and $treat_{icz}$ is a dummy variable indicating if a parcel is zoned for a school treated in its stack. Estimates of π_y represent the difference in prices of homes on either side of attendance zone boundaries for treated schools, relative to this difference one year prior to grant receipt. Examination of trends in home prices along

Table 5
Housing characteristics along SIG boundaries.

	(1) Acreage	(2) Finished SF	(3) Building age	(4) Two+ Stories	(5) Rooms	(6) Bedrooms	(7) Bathrooms	(8) Ln (HPI)
.5 mi bandwidth								
SIG	-0.0915*** (0.0317)	-41.77 (37.88)	-8.771* (4.303)	0.105* (0.0573)	-0.252* (0.142)	-0.00851 (0.0607)	0.0362 (0.0514)	0.00794 (0.0327)
N	14 368	14 368	14 368	14 368	14 368	14 368	14 368	14 368

Standard errors clustered by boundary segment (23 clusters) are in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table depicts if and how characteristics of homes on either side of SIG attendance boundaries differed. Estimates reflect coefficients from separately regressing observable characteristics (acreage, finished square feet, age of the building, a dummy for if the house had at least two stories, the number of rooms, the number of bedrooms, the number of bathrooms, and a hedonic housing price index that summarizes overall housing quality in a single measure.) on an indicator variable for whether or not a home was zoned for a SIG-treated school, year fixed effects, and attendance zone boundary segment fixed effects. We limit the sample to single-family homes and duplexes sold between 2012 and 2019 (after the first cohort of MNPS was implemented) within a half-mile of a SIG attendance zone border.

SIG boundaries in the pre-period may be particularly important in our setting given our treatment timing relative to the housing crash and onset of the Great Recession.¹⁴

The appeal of using boundary discontinuity designs to identify the capitalization of school characteristics into housing prices lies in the notion that homes located within small distances of school attendance boundaries (on either side) likely benefit from similar unobservable neighborhood amenities. Although we cannot test if unobservables vary across attendance zone boundaries for treated schools, we can provide support for the notion that untreated homes located just outside of SIG school zones are valid counterfactuals for those located just inside treated zones by testing if observable characteristics discontinuously change at SIG boundaries. Table 5 presents coefficients from separately regressing observable characteristics of homes sold between 2012 and 2019 (after the first cohort of MNPS was implemented) on an indicator variable for whether or not a home was zoned for a SIG-treated school, year fixed effects, and attendance zone boundary segment fixed effects. Although some coefficients are significant at conventional levels, their magnitudes are small relative to sample means. Furthermore, column (8) shows that a hedonic housing price index that summarizes overall housing quality in a single measure does not economically or statistically significantly differ across SIG attendance boundaries.¹⁵

To help contextualize magnitudes of our main house price estimations, we estimate the capitalization of school quality prior to SIG implementation in MNPS (2000 to 2011). Table 6 presents coefficients

¹⁴ Mean reversion from the housing crash could result in substantial increases in housing prices in our pre-period or, if sufficiently delayed, in our post-period; this would be of particular concern if the bite of the crash was stronger in SIG school neighborhoods than in control areas. Although the variation in treatment timing in our research design should guard against mean reversion from the 2007 housing crash and recovery from overpowering housing price estimations, as a falsification exercise we formally test whether home values on either side of SIG boundaries were more affected by the crash with an event study that estimates the evolution of the difference in housing prices just inside to just outside SIG boundaries relative to this difference at the start of the housing crash. Figure A2 provides evidence that housing prices evolved similarly on both sides of SIG boundaries through the crash and Great Recession. Another potential concern is that the Great Recession may have more harshly affected school and household resources in SIG schools and neighborhoods, resulting in test score declines that pushed schools into SIG eligibility. We show similar trends in district-level expenditures in MNPS, Shelby County Schools (Memphis), and the rest of the state in Figure A3 through the Great Recession. Although we cannot directly rule out the recession affecting household resources differentially in SIG neighborhoods, we note that such a dynamic would primarily be of concern for our displacement analysis. We find no differential changes in eviction rates in SIG and non-SIG school zones through the Great Recession in Figure A4, suggesting that the recession does not explain our displacement results in Section 5.2.

¹⁵ The housing price index is constructed from estimating a linear regression of sale price on the other characteristics presented in Table 5 with pre-period data for all sales in Davidson County.

Table 6
Pre-treatment school quality capitalization.

	(1) Pre-SIG	(2) + Controls	(3) Test scores	(4) + Controls
SIG	-0.197** (0.0717)	-0.166** (0.0657)		
Proficiency rates			0.139** (0.0562)	0.0993** (0.0373)
N	14 070	14 070	13 622	13 622

Standard errors clustered by boundary segment (23 clusters) are in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table depicts how sale prices of single-family homes and duplexes differed along SIG school zone borders prior to treatment (2000–2011). Estimates reflect coefficients from separately regressing observable characteristics the natural log of sale price on an indicator for SIG treatment (Columns 1 and 2) and on a standardized measure of elementary school test scores (Columns 3 and 4). All estimations control for year and boundary fixed effects, and columns 2 and 4 control for observable home characteristics. Coefficients for Columns 3 and 4 correspond to the change in housing price from a one standard deviation increase in school-level elementary test scores, and the sample size is smaller for test score estimations because of missing data for two schools. Estimations in Columns 3 and 4 also include high school fixed effects, so that differences in secondary school quality do not bias estimates of capitalization of elementary school quality.

from boundary discontinuity specifications with the natural log of sale price as the dependent variable. Columns (1) and (2) show the pre-treatment difference in housing prices along SIG attendance zone borders, indicating that homes just inside attendance zones that later implemented a SIG-funded intervention sold for 16.6 log points less than homes just on the other side of attendance zone boundaries. Columns (3) and (4) instead follow previous literature in using elementary school test scores as a proxy for school quality. We follow Black (1999) in constructing a standardized index based off the sum of school-level achievement (proficiency rates in our setting) on math and reading standardized tests. Estimates shows that a one standard deviation increase in elementary school achievement was associated with a 10.4% higher sale price, a nearly identical magnitude to that found by Kane et al. (2006) in the comparable setting of Charlotte, North Carolina.

5. Results

5.1. Capitalization of school improvement grants

We begin by presenting event study coefficients from estimating Eq. (4) in Fig. 3, limiting our sample to single family homes and duplexes within a half-mile of attendance zone boundaries separating SIG and non-SIG school zones. Home values evolve in parallel prior to the start of SIG interventions, suggesting that they would continue to do so in the absence of the program. Following treatment, prices of homes zoned for SIG schools immediately increase relative to their neighbors across the boundary. Furthermore, this increase grows over

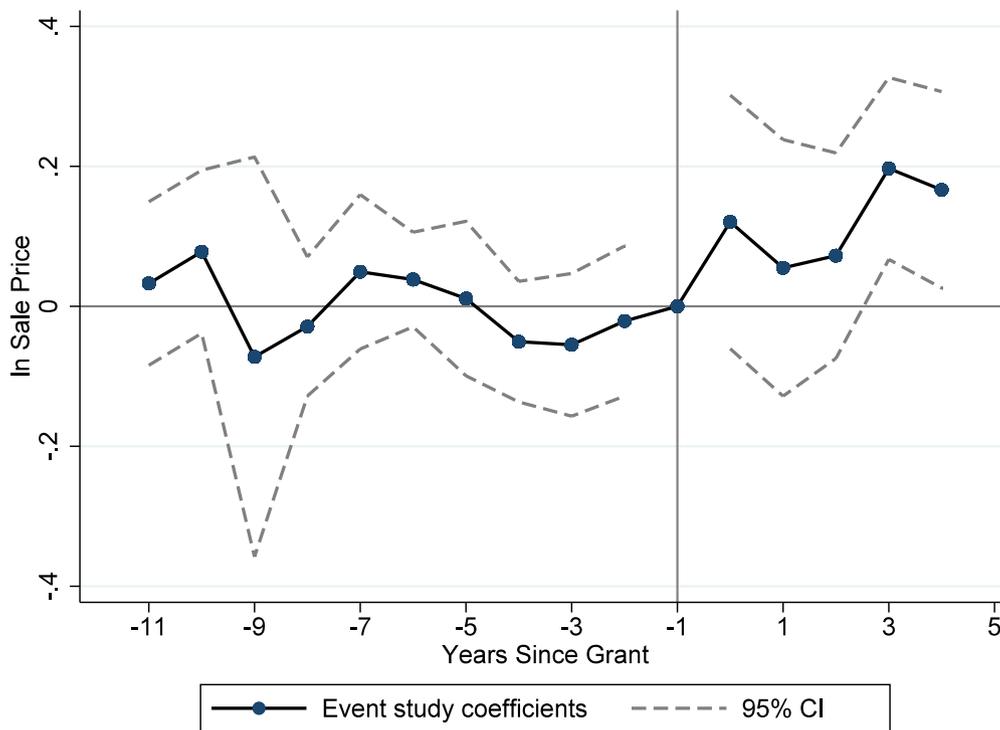


Fig. 3. Housing price stacked event study. Notes: This figure plots π_y coefficients from Eq. (4), a stacked event study specification that compares changes in the natural log of sale price (in 2010 \$) of homes located on either side of attendance zones of schools implementing SIG-funded interventions. The sample is limited to single family homes and duplexes located within a half-mile of attendance zone boundaries separating treated and untreated school zones. Dashed lines represent the 95% confidence interval of estimates based off of robust standard errors clustered by boundary segment-by-post.

time, matching the dynamics of the achievement effects found by previous research on SIG (Friday, 2021; Sun et al., 2017) and the dynamics of housing price effects found in literature analyzing other K-12 policies (Bayer et al., 2020; Wigger, 2020). These treatment effect dynamics confirm that two-way fixed estimates would be biased and support the use of a stacked design. Moreover, an immediate response that grows over time suggests that parents respond both to the receipt of SIG funding (changes in school inputs) and presumptive improvements in test scores (changes in school outputs).¹⁶

We summarize the dynamic effects seen in Fig. 3 by estimating Eq. (3), a stacked difference-in-differences (DD) specification. Table 7 contains estimates of $\beta^{stacked}$ from our preferred specification and a variety of robustness checks. Inference is based off of robust standard errors clustered at the boundary segment-by-post level, appealing to the notion that our identifying variation comes from the discontinuous access to SIG treatment across attendance zone boundaries in the post-period but not the pre-period. Panel A reflects estimates on a sample of single-family homes and duplexes, while Panel B restricts the sample only to single-family homes. Our base model estimates a stacked DD on a sample of sales of homes located within a half-mile of SIG attendance boundaries, comparing changes in sale prices of homes zoned for SIG-treated schools to those just on the other side of the attendance zone boundary. Columns (1) and (2) estimate this specification with and without controlling for observable home characteristics, yielding coefficients in Panel A that indicate SIG-funded interventions increased housing prices by 10.2 (p-value .144) and 10.0 (p-value of .112) log

¹⁶ Although due to data limitations we are unable to confirm that SIG-funded interventions improved test scores in Nashville, existing literature finds positive effects of SIG on student achievement in various local and statewide settings (Carlson & Lavertu, 2018; Friday, 2021; Sun et al., 2020, 2017) and positive effects of other school turnaround programs on student achievement in Tennessee, including in MNPS (Pham et al., 2020).

points, respectively. To account for time-varying neighborhood amenities common to both sides of attendance zone boundaries, we include boundary segment-by-year fixed effects in Columns (3) onward. Doing so greatly improves the precision of our estimates, and our estimate of 10.0 log points in Column 3 is statistically different from zero at the 1% level of significance. That our estimate changes little when controlling for observable home characteristics suggests that homes sold on either side of SIG attendance borders had similar characteristics. Still, differential changes in the composition of unobservable characteristics of homes sold across SIG attendance boundaries could bias estimates. To address this concern, we include parcel fixed effects in Column (4), identifying the estimate of β from repeat sales of the same homes and finding a similar coefficient of 10.2 log points (p-value of .011). Because we expect SIG treatment to increase neighborhood desirability by improving the quality of targeted schools, increases in home values should be driven by households with school-age children. As a proxy for the presence of children, we limit our sample to only homes with at least three bedrooms in Column (5). Finally, Columns (6) and (7) narrow our bandwidth to limit the sample to homes sold within .35 and .25 miles of SIG school zone boundaries, respectively.

To further characterize the price effects shown in Table 7, we explore whether SIG treatment of elementary, middle, or high schools had heterogeneous effects on housing prices. Table 8 presents coefficients from separately estimating our stacked difference-in-differences on samples limited to homes within a half-mile of attendance zone boundaries for SIG-treated elementary (Columns 1 and 2), middle (Columns 3 and 4), and high schools (Columns 5 and 6). Consistent with prior literature that finds stronger capitalization of elementary and high school quality (Caetano, 2019), our estimates suggest that SIG-funded interventions in elementary or high schools resulted in larger housing price increases than those in middle schools. We interpret the high school result with caution, however, because our sample only features one treated high school.

Our setting allows for a natural application of Oates's (1969) empirical test for whether a local public good is efficiently provided.

Table 7
Capitalization stacked DD: SIG-funded interventions raise house prices.

	(1) .5 mi	(2) + Controls	(3) + Boundary X Year FEs	(4) + Parcel FEs	(5) 3+ BR	(6) .35 Mile	(7) .25 Mile
A. SF & Duplexes							
SIG	0.102 (0.0687)	0.0999 (0.0618)	0.100*** (0.0305)	0.102** (0.0381)	0.0822** (0.0352)	0.0723*** (0.0232)	0.0413* (0.0219)
N	22134	22134	22083	17505	16285	15109	10255
B. Single-Family Only							
SIG	0.112 (0.0690)	0.106* (0.0622)	0.108*** (0.0311)	0.118*** (0.0360)	0.0848** (0.0354)	0.0814*** (0.0241)	0.0586*** (0.0210)
N	20648	20648	20599	16400	14956	14163	9544

Robust standard errors clustered by boundary segment-by-post (46 clusters) are in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of β from Eq. (3), a stacked difference-in-difference specification that compares changes in the logged sale price of homes sold just on either side of SIG attendance zone boundaries. All estimations include school zone-by-stack and stack-by-year fixed effects. Panel A includes both single-family homes and duplexes, and Panel B shows robustness to restricting only to single-family homes. Column (1) limits the sample to homes within a half-mile of a SIG attendance zone boundary and includes boundary segment fixed effects. Column (2) adds controls for observable home characteristics. Columns (3) onward use boundary segment-by-year fixed effects to account for time-varying neighborhood amenities common across both sides of a school zone boundary. Column (4) includes parcel fixed effects, so that the estimate of β is identified by repeat sales. Column (5) limits the sample to homes with at least three bedrooms. Finally, Columns (6) and (7) restrict the sampling bandwidth to .35 and .25 miles from SIG attendance zone boundaries, respectively.

Table 8
Capitalization stacked DD: Heterogeneity by school level.

	(1) Elem.	(2) + Controls	(3) MS	(4) + Controls	(5) HS	(6) + Controls
SIG	0.195*** (0.0504)	0.181*** (0.0440)	0.0719 (0.0453)	0.0722 (0.0454)	0.228** (0.0784)	0.236** (0.0768)
N	5183	5183	13872	13872	5374	5374

Robust standard errors clustered by boundary segment-by-post are in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of β from estimations of Eq. (3), a stacked difference-in-difference specification that compares changes in sale price of homes sold just on either side of SIG attendance zone boundaries, that separately limit the sample to boundary segments for SIG-treated elementary, middle, and high school attendance zones, respectively. All estimations include school zone-by-stack and stack-by-boundary-segment-by-year fixed effects. All coefficients reflect a sample of single-family homes and duplexes sold within .5 miles of a SIG attendance zone boundary. Columns (2), (4), and (6) include controls of observable home characteristics.

Appealing to the Tiebout (1956) model of local public good provision based on households ‘voting with their feet’, Oates hypothesized that an increase in home values in response to a marginal increase in locally financed public goods would indicate that households value the marginal \$1 in expenditure more than the marginal \$1 lost via taxation and that the local public good had been underprovided. Because SIG was financed completely by the federal government and required no increase in local taxation, in our setting the Oates test translates to testing whether home values increase by more or less than \$1 for every \$1 in increased expenditure.¹⁷ Based on the mean pre-treatment sale price of a SIG-zoned home of \$125,216, estimates from our preferred specification (Column 3) imply an increase in home value of \$13,148 in response to a SIG intervention. Compared to the average per-pupil grant of over \$3818, the greater than \$3 willingness-to-pay for every additional \$1 in per-pupil funding is consistent with funding for SIG-receiving schools previously being suboptimally low.¹⁸ We note with caution that this interpretation requires the assumption that no other local amenities are changing differentially along SIG school zone boundaries.

The above interpretation requires the assumption that no other local amenities are changing differentially along SIG school zone boundaries at the same time as treatment. Although this assumption is fundamentally untestable, we examine testable implications of violations due to potential confounders—namely, concurrent gentrification. We

¹⁷ We implicitly assume that SIG funding leads to a dollar-for-dollar increase in school spending (i.e., no crowd-out); Friday (2021) finds evidence of this at the district level.

¹⁸ The willingness-to-pay estimates associated with each specification from Table 7 all yield this same conclusion from an Oates (1969) efficiency test.

first conduct three sets of falsification exercises to support a causal claim of our estimates. Because Nashville sustained a booming housing market over the past decade, one may be concerned that all low-income neighborhoods experienced large price increases, so that estimates from Table 7 do not reflect the capitalization of improved school quality from SIG interventions.¹⁹ We note two sets of attendance zone boundaries that should not experience a changing price gradient if our observed housing price increases are caused by SIG: attendance zone boundaries separating untreated elementary schools *within the attendance zone of a SIG-treated middle or high school* and boundaries for schools that were *eligible for SIG but did not receive grants*. Both of these sets of attendance zone boundaries represent actual changes in school assignment, along with potentially other neighborhood characteristics, but homes on both sides experience the same SIG treatment status. Appendix Figure A5 imposes attendance zone boundary segments used for these two falsification exercises over a map of MNPS attendance zones. For a third set of falsification exercises, we construct placebo SIG attendance zone boundaries by shifting the real boundaries 1, 3, and 5 miles in each cardinal direction. These placebo boundaries do not represent changes in school assignment or other neighborhood characteristics except for those that arise by chance.

Table 9 presents estimates from the first two falsification exercises. Coefficients in panel A reflect estimates from a stacked difference-in-differences specification that compares changes in sale price of homes sold just on either side of attendance zone boundaries for elementary schools within the same SIG-treated middle school or high school.

¹⁹ In particular, Guerrieri et al. (2013) predict that during to a housing boom, high-income households expand their housing consumption by migrating to poorer areas adjacent to high-income neighborhoods.

Table 9
Falsification exercises: House prices do not change where they should not.

	(1)	(2)	(3)	(4)	(5)	(6)
	.5 mi	+ Controls	+ Boundary X Year FEs	3+ BR	.35 Mile	.25 Mile
A. Within-Zone Boundaries						
Placebo SIG	0.00311 (0.00610)	0.00326 (0.00625)	0.00403 (0.00659)	0.00352 (0.00767)	0.00345 (0.00735)	0.00267 (0.00647)
N	15 537	15 537	15 523	11 786	11 378	7892
B. SIG Eligibility						
Placebo SIG	-0.0259 (0.0791)	-0.0259 (0.0791)	-0.0443 (0.0575)	-0.0699 (0.0595)	-0.0630 (0.0633)	-0.162 (0.103)
N	16 815	16 815	16 777	11 555	11 686	7959

Robust standard errors clustered by boundary segment-by-post are in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of two sets of falsification exercises. Coefficients in Panel A reflect estimates of β from a stacked difference-in-difference specification that compares changes in sale price of homes sold just on either side of attendance zone boundaries for elementary schools within the same SIG-treated middle school or high school zone. Placebo “treatment” is assigned to the elementary school within each treated upper school with the lowest 2010 standardized test scores, mimicking the true SIG treatment assignment mechanism. Coefficients in Panel B reflect estimates of a difference-in-differences specification with placebo “treatment” defined as an indicator equaling unity for sales occurring in 2012 or after for homes zoned for schools that were eligible for SIG but did not receive grants to implement turnaround programs. All estimations include school zone-by-stack and stack-by-year fixed effects. Column (1) limits the sample to homes within a half-mile of a SIG attendance zone boundary. Column (2) adds controls for observable home characteristics. Columns (3) onward include boundary segment-by-year fixed effects that account for time-varying neighborhood amenities common across attendance zone boundaries. Column (4) limits the sample to homes with at least three bedrooms. Finally, Columns (5) and (6) restrict the sampling bandwidth to .35 and .25 miles from SIG attendance zone boundaries, respectively.

We assign placebo “treatment” to the elementary school within each treated upper school with the lowest 2010 standardized test scores, mimicking the true SIG treatment assignment mechanism. Coefficients in Panel B reflect estimates of a difference-in-differences specification with placebo “treatment” defined as an indicator equaling unity for sales occurring in 2012 or after for homes zoned for elementary schools that were eligible for SIG but did not receive grants to implement turnaround programs. We find null results across all variants of these two exercises. Table A3 presents estimates from the final set of falsification estimations. Only one out of twelve difference-in-differences estimates statistically differs from zero, consistent with the frequency we would expect to find a statistically significant result purely from chance. Also consistent with randomness, the estimates vary in sign and magnitude both within and across directions and distances.

The falsification exercises suggest that no confounding shocks systematically improved housing prices in low-income neighborhoods zoned for low-performing schools throughout Nashville, but they do not eliminate the possibility of confounding amenity changes specifically in SIG neighborhoods. Because we find evidence in Section 5.2 that increased demand to live in SIG school zones is accompanied by changing neighborhood demographics—particularly, that white and wealthy homebuyers disproportionately move in, the estimates in Table 7 may reflect a combination of valuation of improved school quality and of increasingly desirable neighbors. We present estimates in Table A4 from stacked difference-in-differences estimations that additionally control for two time-varying neighborhood-level controls from Consumer Financial Protection Bureau mortgage data: the natural log of the average reported income of homebuyers and the share of homebuyers that are white. Although these endogenous characteristics represent “bad controls”, in that they are outcomes that might also be affected by SIG, we are reassured by finding that their inclusion does not affect our capitalization estimates (Angrist & Pischke, 2009). We note with caution that other changes to unobservable neighborhood amenities that differentially change just inside of SIG school zones compared to just outside these zones may still be reflected in changing housing prices.

Finally, we discuss the plausibility of the magnitudes of our housing price estimates within the context of other literature. Our preferred specification indicates that following SIG funding receipt, home values just inside SIG school zone boundaries rose by \$13,148 relative to those just across the boundary. Although this magnitude may seem large, back-of-the-envelope calculations based on existing estimates of

test-score capitalization and direct estimates of SIG’s effect on test scores from the literature support its plausibility under the assumption that SIG had a similarly sized effect on test scores in MNPS as found in other settings. Specifically, our point estimate falls between the seminal test-score capitalization estimates of Black (1999) scaled by SIG’s causal effect on test scores found by Sun et al. (2020) (\$9814) and those found by Friday (2021) (\$19,886).²⁰ The salience of SIG-funded comprehensive turnaround interventions, which often required schools to replace principals and 50% of school staff and were covered by local media (Barnes, 2015; Beecher, 2014; Gonzales, 2015a, 2015b, 2015c), likely explains the immediacy of the housing price response. The estimated effect of a 10.5% increase in housing prices also resembles the magnitude of effects of salient interventions affecting school quality in comparable settings of Memphis, TN (Collins & Kaplan, 2017), Denver, CO (Wigger, 2020), and Charlotte, NC (Billings et al., 2017),²¹ and SIG joins capital bonds (Cellini et al., 2010) and school finance reforms (Bayer et al., 2020) as sources of K-12 spending increases that yielded greater willingness-to-pay estimates of more than a dollar for a dollar increase in spending.

5.2. Effects on neighborhood and school composition

In addition to bidding up prices of homes zoned for treated schools, sorting in response to SIG interventions may have altered the composition of SIG-zoned neighborhoods and schools. We examine how characteristics of households moving into treated neighborhoods changed

²⁰ Black (1999) finds a willingness-to-pay of \$9039 (2010\$) for a school-level deviation (approximately 5%) increase in test scores. Sun et al. (2020) finds that SIG led to a 0.228 student-level standard deviation increase in math scores by the intervention’s third year. We translate this effect to 1.09 school-level standard deviations using Kane et al.’s (2006) student-level to school-level standard deviation conversion ratio of 0.21:1. Friday (2021) finds that SIG led to an 11% improvement in school proficiency rates. We scale the \$9039 willingness-to-pay by 1.09 and 2.2 (11%/5%) to reach \$9814 and \$19,886, respectively.

²¹ Collins and Kaplan (2017) finds a 7.8% increase in sale price of homes subject to redistricting in Memphis, TN, Wigger (2020) finds that housing values for homes previously assigned to higher rated neighborhood schools fall by up to 9.3% when reassigned to larger “forced choice” zones in Denver, CO, and Billings et al. (2017) finds that housing prices in the “highest-quality” neighborhoods that qualify for priority in enrollment lotteries for oversubscribed schools increase by 8.8%.

Table 10
Mortgage characteristic DDs: Higher-income whites move to SIG neighborhoods.

	(1)	(2)	(3)	(4)
	Ln loan amount	Ln applicant income	% White buyers	% Black buyers
SIG	0.0657* (0.0355)	0.0859* (0.0509)	0.0710*** (0.0231)	-0.0545*** (0.0182)
N	1661	1661	1661	1661

Standard errors clustered by census tract (119 clusters) are in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of SIG-funded interventions on neighborhood composition, as measured by Consumer Finance Protection Bureau mortgage data identifiable at the census tract. Estimates reflect coefficients from a stacked difference-in-difference regression that compares changes in characteristics of tracts zoned for schools treated as part of the 2012 or 2015 SIG cohorts to those of untreated schools. The specification controls for tract-by-stack and year-by-stack fixed effects.

in response to SIG-funded interventions using mortgage data from the Consumer Financial Protection Bureau. Although we cannot observe property addresses, mortgages are identifiable at the Census-tract level, which we match to MNPS school zones. We adapt our stacked difference-in-differences design to fit tract-level mortgage data, estimating the following event study and DD specifications:

$$y_{cat} = \alpha_0 + \alpha_{ca} + \alpha_{ct} + \sum_{\substack{y=-4 \\ y \neq -1}}^2 \pi_y 1\{t - t_c^* = y\} * treat_{ca} + \epsilon_{cat} \quad (5)$$

$$y_{cat} = \alpha_0 + \alpha_{ca} + \alpha_{ct} + \beta^{stacked} SIG_{cat} + \epsilon_{cat} \quad (6)$$

where c , a , and t index stacks (i.e. cohort of SIG), Census tracts, and years, respectively.²² Interpreting β as causal requires the identifying assumption that in the absence of SIG treatment, outcomes would evolve in parallel for Census tracts zoned for SIG non-SIG schools.

Fig. 4 presents event study coefficients for mortgage characteristics of interest: the natural logs of loan amount (a) and applicant income (b) and the shares of white (c) and Black (d) homebuyers. Each panel displays flat pre-trends, suggesting that outcomes in SIG and non-SIG Census tracts would continue to evolve in parallel in the absence of treatment. Panel (a) confirms the primary result of our capitalization analysis. Following the start of SIG-funded interventions, loan amounts increase in Census tracts zoned for SIG schools relative to those zoned for non-SIG schools—a response that grows over time. This phenomenon corresponded with an increase in reported applicant income, and appears to be driven by the differential migration of white households into SIG neighborhoods, as evident by panels (b), (c), and (d). DD estimates of these effects, presented in Table 10, show moderately sized effects of SIG on homebuyer characteristics, all of which are statistically significant at least the 10% level.²³

The results in Table 10 are consistent with gentrification of SIG neighborhoods following treatment. We explore whether the influx of white, high-income households moving into SIG neighborhoods displaced existing residents using block group counts of court-ordered evictions and eviction filings from the Eviction Lab at Princeton University. Block groups are granular enough that we can mimic the sample restrictions of our capitalization analysis by limiting the sample to block groups that intersect with a half-mile buffer surrounding attendance zone boundaries separating SIG and non-SIG schools (excluding block groups that do not fully lie on the SIG or non-SIG side of the boundary). Figure A6 depicts this sample. Because we only observe

²² Because we only observe mortgage data from 2007 to 2017, to maintain a balanced panel our event time is limited to run from -4 to 2.

²³ The estimated effect of SIG on loan amount as measured by mortgage data may seem inconsistent with that from our full capitalization analysis (it is noticeably smaller). This is due to only observing mortgage data through 2017—recall that our main estimate of house price effects grow over time. When limiting the post period in our capitalization estimations to also be only two years, we obtain a DD estimate of housing price effects that is statistically indistinguishable from the loan amount estimate in Table 10.

these data from 2000 to 2016, we do not analyze the 2015 cohort of SIG schools.

Fig. 5 presents two-way fixed effects event studies for evictions per 1000 residents and eviction filings per 1000 residents in panels (a) and (b), respectively, restricting the sample to block groups contiguous to SIG attendance zones. After evolving similarly prior to treatment, block groups just inside SIG attendance zones experience more eviction filings and evictions per 1000 residents than those just outside SIG attendance zones. Difference-in-differences estimates, presented in Table 11, indicate that evictions and eviction filings increased by 6.54 and 23.27 per 1000 residents in SIG neighborhoods. Relative to the pre-treatment means, these magnitudes represent 34.7% and 61.2% increases, respectively.²⁴

To examine how the above sorting affected the overall composition of neighborhoods zoned for SIG schools, we estimate a stacked difference-in-differences with block group-level data from the 2010 Decennial Census and ACS (2013–2019). Because this data only gives us one year of observations prior to treatment for the initial cohort of SIG schools in MNPS, we view this set of results as suggestive evidence and caution against causal interpretation. Appendix Table A2 presents coefficients from stacked difference-in-differences estimations. Although noisy, the coefficients are consistent with our mortgage analysis, suggesting that neighborhoods zoned for SIG schools became whiter and wealthier following treatment.

Finally, we examine to what degree changes in neighborhood composition corresponded with changes in classroom composition. Using grade-level enrollment data from NCES, we estimate stacked difference-in-differences and event study specifications that compare changes in enrollment demographics in SIG schools to non-SIG schools in MNPS. Fig. 6(a) shows that after evolving in parallel prior to SIG implementation, white share in SIG schools increased by 2 percentage-points following treatment. Consistent with our narrative of displacement, 6(b) shows that three years after the start of SIG-funded interventions, 15 fewer nonwhite students were enrolled in a given grade. Table 12 presents DD coefficients and confirms the statistical significance of these demographic changes. Relative to the 2010 means, the estimate in Column (1) corresponds to a 19.9% increase in the share of students who are white in SIG schools and that of Column (4) corresponds to a 15% decline in the number of nonwhite students in SIG schools following funding receipt.²⁵ Columns (2), (3), (5), and (6) show that these changes occurred in both treated elementary and middle schools.

²⁴ Neither our eviction data nor our home sales panel allow us to observe whether homes where landlords evicted tenants were then sold for owner-occupancy or rented continually. We examine whether the share of owner-occupied units changes in SIG school zones using block group-level data from the 2010 Decennial Census and ACS (2013–2019) in Appendix Figure A7 and find no effect of SIG on owner-occupancy rates, although we cannot rule out sizable changes in either direction.

²⁵ Ideally, we would examine changes in the economic status of student demographics in SIG schools, too. However, measures of student poverty for MNPS schools are not reliable: the average share of students eligible for free or reduced-price lunch in SIG schools in 2010 was a non-sensical 157%.

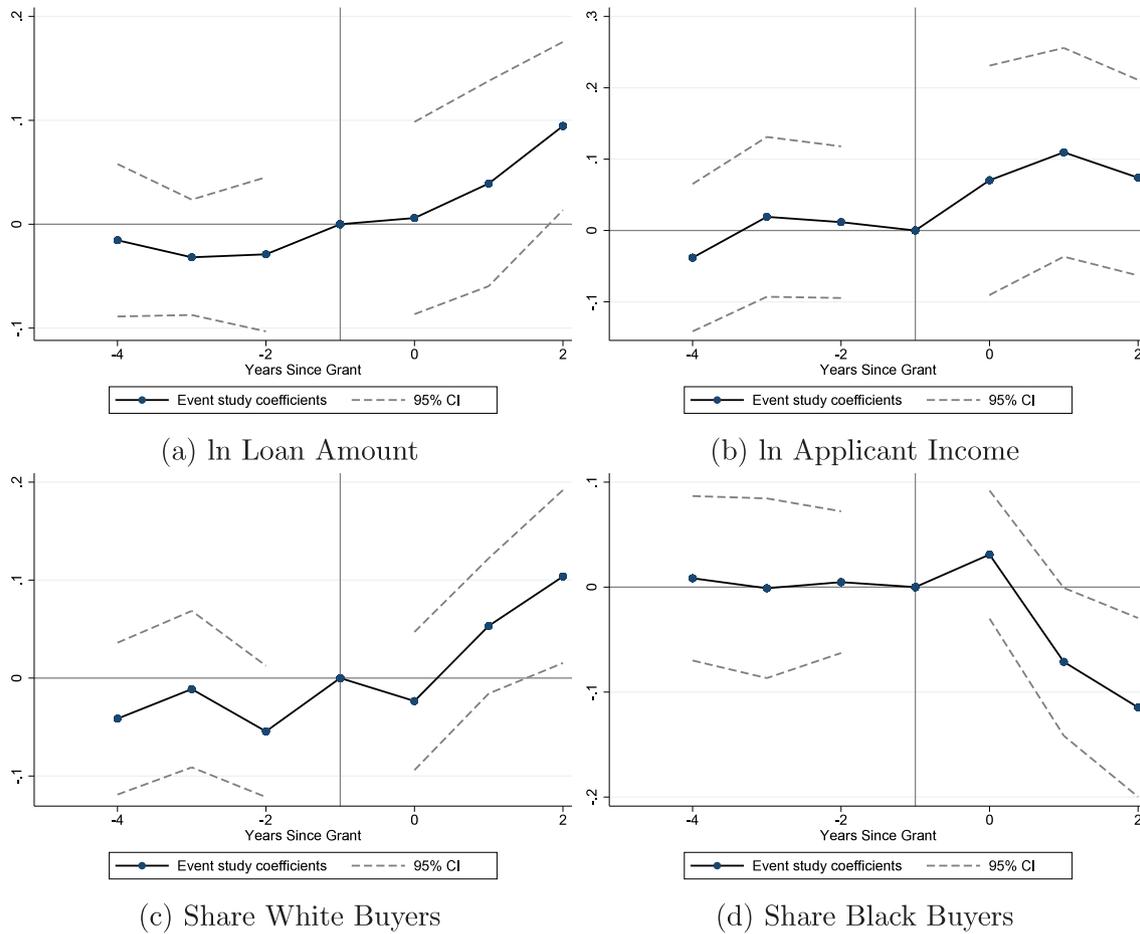


Fig. 4. Stacked event studies: Homebuyer characteristics. Notes: These figures presents estimates of the effect of SIG-funded interventions on neighborhood composition, as measured by Consumer Financial Protection Bureau mortgage data identifiable at the Census tract. Estimates reflect coefficients from a stacked event study regression that compares changes in characteristics of tracts zoned for schools treated as part of the 2012 or 2015 SIG cohorts to those of untreated schools. The specification controls for tract-by-stack and year-by-stack fixed effects.

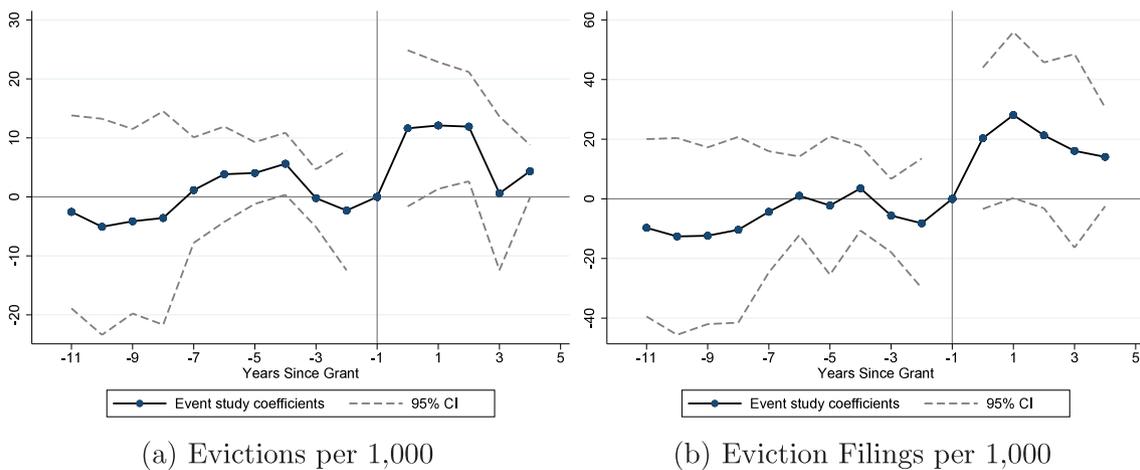


Fig. 5. TWFE event studies: Evictions. Notes: These figures presents event study estimates of the effect of SIG-funded interventions on evictions using data from the Eviction Lab at Princeton University. We limit the sample to block groups that are at least partially contained within a half-mile buffer around SIG attendance zone boundaries. Estimates reflect coefficients from a two-way fixed effects event study regression that compares changes in characteristics of block groups just inside attendance zones for schools treated as part of the 2012 SIG cohort to those of block groups located just outside of treated attendance zones. The specification controls for block group and year fixed effects. Panels (a) and (b) present estimates of the effect of SIG treatment on the number of court-ordered evictions per 1000 residents and eviction filings (which may result in a ruling for or against eviction, or a settlement between the landlord and tenant) per 1000 residents, respectively.

Overall, estimated effects on mortgage outcomes show that neighborhoods zoned for SIG schools became moderately wealthier and whiter following interventions, estimated effects on evictions indicate

that sorting displaced a share of existing residents, and estimated effects on enrollment suggest that school student populations experienced similar demographic changes and displacement. We interpret these results

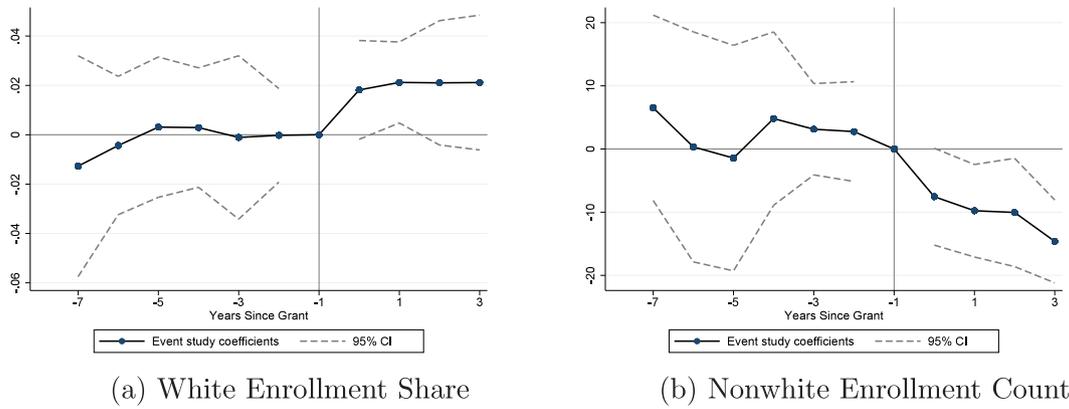


Fig. 6. Stacked event studies: Enrollment. This figure presents coefficients from a stacked event study specification that regresses school-by-grade enrollment demographics on stack-by-school-grade and stack-by-year fixed effects, using school-by-grade enrollment data from NCES. The outcome in panel (a) is white enrollment share and in (b) is the nonwhite enrollment count.

Table 11
SIG-induced gentrification increases evictions.

	(1) Evictions per 1000	(2) Eviction filings per 1000
SIG	6.540* (3.782)	23.27* (12.12)
N	608	608

Robust standard errors clustered by block group (38 clusters) in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates of the effect of SIG-funded interventions on evictions from a two-way fixed effects DD. We use data on evictions and eviction filings from the Princeton University Eviction Lab (2000–2016) and limit the sample to block groups that are at least partially contained within a half-mile buffer around SIG attendance zone boundaries. Because we only observe the data through 2016, we only analyze the 2012 cohort of SIG. Columns (1) and (2) present estimates of the effect of SIG treatment on the number of court-ordered evictions per 1000 residents and eviction filings (which may result in a ruling for or against eviction, or a settlement between the landlord and tenant) per 1000 residents, respectively.

Table 12
School demographics reflect changing neighborhood demographics.

	(1) (2) (3)			(4) (5) (6)		
	Percent of students who are white			Number of nonwhite students		
	All schools	Elementary	MS	All schools	Elementary	MS
SIG	0.0221** (0.00948)	0.0249** (0.0104)	0.0269 (0.0180)	-12.69** (6.132)	-7.345** (3.483)	-18.72 (11.84)
N	12 477	9716	2761	12 477	9716	2761

Robust standard errors clustered by school in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This figure presents coefficients from a stacked difference-in-difference specification that regresses school-by-grade enrollment demographics on stack-by-school-grade and stack-by-year fixed effects, using school-by-grade enrollment data from NCES. Column (1) estimates the change in the white enrollment share for all SIG schools relative to non-SIG schools, and Columns (2) and (3) limit the sample to elementary and middle schools respectively. Columns (4), (5), and (6) follow this same pattern for nonwhite enrollment counts.

as evidence of SIG funding inducing gentrification of targeted schools and neighborhoods.²⁶ If instead, the observed gentrification specifically

in SIG neighborhoods still would have occurred in the absence of SIG-funded interventions, then these results may be interpreted as evidence that concurrent gentrification can inhibit place-based public goods from reaching their intended recipients.

²⁶ An alternative explanation for our results is that gentrifiers themselves advocated to policymakers to apply for SIG funding for schools in neighborhoods with potential for redevelopment. Selection into treatment based on neighborhood development potential would bias our estimates toward finding results consistent with gentrification that do not actually reflect causal effects of increased school funding. However, the nature of SIG’s grant allocation process gave little room for this type of manipulation. Eligibility for SIG was primarily determined by a school’s standardized test scores falling in the bottom five percent of its state. Moreover, nearly all of MNPS schools that were eligible for SIG by this rigid criteria were awarded grants, leaving almost no leeway for selection into grant receipt in our setting. Anecdotally, we find no narrative of parents or commercial groups lobbying to policymakers for specific schools in Davidson County to receive SIG funding in media.

6. Generalizability

To examine the generalizability of our Nashville-based results, we supplement our primary analyses by estimating the effects of SIG-funded interventions on housing prices and neighborhood composition in California. We choose California as our supplementary setting because we are able to observe grant eligibility and receipt for all SIG cohorts in the state and because previous work provides evidence that SIG-funded interventions successfully improved student achievement at low-performing schools in California (Friday, 2021).

6.1. Data and methods

Replicating our Nashville-based analyses in California requires measures of housing prices, neighborhood and school demographics, and displacement linked to school attendance zones. To measure housing prices in neighborhoods across the entire state, we follow previous literature (Bayer et al., 2020) in utilizing tract-level house price indices (HPIs) constructed by the Federal Housing Finance Agency (FHFA). Similarly to Case–Shiller indices (Case & Shiller, 1989), FHFA HPIs are constructed based on changes in the value of houses that have been sold or refinanced multiple times. We again use loan-level mortgage data from the Consumer Financial Protection Bureau, school demographic data (from the California Department of Education), and eviction counts from the Eviction Lab to characterize the sorting response to SIG-funded improvements in school quality.

We observe attendance zone boundaries for neighborhood schools in all districts in California from the School Attendance Boundaries Survey, a mapping of attendance zones for 70,000 schools across 12,000 districts across the country undertaken by NCES and the Census Bureau. School assignment zones in much of California differ from those in Nashville in that they do not perfectly overlap in a ‘school pathways’ structure (i.e., not all students in a given elementary school will advance to the same middle school). Thus, often a single neighborhood straddles multiple assignment zone boundaries for different school levels. We assign treatment to neighborhoods (Census tracts) defined as the average per-pupil SIG grant amount received by a neighborhood’s zoned schools, weighted by the share of the neighborhood’s population zoned for each SIG school.

2687 schools in California were eligible for SIG, 162 of which received grants averaging \$4077 per-pupil to implement school turnaround interventions across four cohorts (2010–2011, 2011–2012, 2014–2015, and 2016–2017). Table 13 presents pre-treatment descriptive statistics and balance tests for SIG-recipient and eligible non-recipient neighborhoods and schools. Much like in Nashville, baseline characteristics of SIG schools and neighborhoods in California differed from those of their eligible non-SIG counterparts. Home values were lower in SIG neighborhoods, and homebuyers in SIG neighborhoods reported lower income and were approximately half as likely to be white and twenty percentage-points more likely to be Hispanic than those in neighborhoods zoned for SIG-eligible schools that did not receive grants. The student populations of treated and SIG-eligible neighborhoods differed less substantially than their home-owning populations, however: student populations at SIG schools had a 9.7 percentage-point smaller share of white students, with this difference evenly accounted for by larger proportions of Black and Hispanic students. Both SIG-recipients and SIG-eligible schools consisted of majority Hispanic populations.

To identify the effects of SIG-funded interventions on housing prices and neighborhood composition in California, we estimate a stacked difference-in-differences specification that compares changes in outcomes of neighborhoods zoned for SIG-recipient schools to those zoned for schools that were eligible for, but did not receive, SIG grants. Causal interpretation of these estimates requires the assumption that conditional on grant eligibility, grant receipt was exogenous to trends in neighborhood outcomes. To assess the plausibility of this assumption – specifically, the implication that outcomes in treated and eligible tracts would evolve in parallel prior to treatment – and to trace out potential dynamic treatment effects, we estimate flexible event study specifications.

6.2. Results

We first present event study coefficients in Fig. 7 that represent the changes in housing prices in neighborhoods zoned for SIG-recipient schools associated with a \$1000 increase in per-pupil SIG funding relative to those zoned for eligible non-recipient schools. Panel (a) shows that prior to treatment, housing prices in treated neighborhoods

declined relative to those in control neighborhoods. Following treatment, this trend reversed: housing prices in neighborhoods zoned for SIG schools increased relative to prices in neighborhoods zoned for eligible schools. Although the break from trend suggests that SIG-funded interventions had a positive effect on housing prices, the differential pre-trends cast doubt on the identifying assumption of parallel trends between treated and control neighborhoods in the absence of treatment and would bias difference-in-differences estimates downward.

We account for bias from group-specific linear pre-trends by using a procedure proposed by Goodman-Bacon (2021), estimating a pre-trend in housing prices for treated and control groups and removing this trend from the full panel.²⁷ The resulting event study and difference-in-differences estimations on the pre-trend-adjusted outcome variable are robust to linear trends and time-varying treatment effects. To interpret the adjusted estimations as causal effects of SIG-funded interventions, we must assume that housing prices would continue to decline linearly in treated neighborhoods relative to control neighborhoods in the absence of treatment. Because this is stronger than the identifying assumption of the standard difference-in-differences estimator, we cautiously interpret these results as suggestive.

Panel (b) of Fig. 7(b) presents the housing price index event study adjusted for group-specific linear pre-trends. Following grant receipt, home prices steadily increase in neighborhoods zoned for schools implementing SIG-funded interventions. Treatment effect growth over time mirrors the response found in Nashville. The stacked difference-in-differences specification, presented in Table 14, summarizes this dynamic response as a 2.6 unit increase in HPI per \$1000 in SIG funding. This coefficient corresponds to a 3.6% housing price increase in the average SIG-zoned neighborhood upon receipt of the average grant of \$4077. Specifications using mortgage data from the CFPB can be used to construct a specific dollar amount increase in price, allowing for an Oates (1969) efficiency test. As shown in Table 14, the average mortgage increased by 3.0 log points (3.0%) for every \$1000 in SIG funding. Based on the average pre-treatment loan size in SIG-zoned neighborhoods of \$277,273, this represents over an \$8000 increase in housing prices. The 8:1 return on SIG investments is even larger than the corresponding return estimated in Nashville.

Just as in Nashville, the capitalization of SIG-funded interventions into housing prices in California may represent substantial neighborhood change and distort who can access newly improved schools. We characterize the sorting response to SIG in California by estimating pre-trend-adjusted stacked difference-in-differences specifications for the effect of SIG-funded interventions on homebuyer characteristics, displacement, and school demographics and present coefficients in Table 14.²⁸ Our results suggest income-based sorting of even greater magnitude than in Nashville: reported income of homebuyers in SIG neighborhoods increased by 15.3% in response to the average grant. Moreover, although less pronounced than in Nashville, we still find evidence of racial sorting and displacement from neighborhoods zoned for newly improved schools. Overall, Table 14 supports the notion that the effects of SIG-funded interventions on housing prices and neighborhood composition were not unique to Nashville.

7. Discussion & conclusion

This paper examines whether household sorting in response to School Improvement Grant funding inhibits spending from reaching targeted students. Estimating a boundary-discontinuity difference-in-differences model on property sales data from 2000 to 2019, we find

²⁷ To account for additional variance generated by the first-step estimation, we construct standard errors utilizing a two-step bootstrap procedure clustered at the county or school-district level.

²⁸ The corresponding event studies are presented in Appendix Figures A8 through A10.

Table 13
California pre-treatment descriptive statistics.

	(1) SIG-eligible non-recipients	(2) SIG-recipients	(3) Diff
Panel A. Housing Prices			
Tract HPI	348 (186)	299 (154)	-49*** (3)
Observations	25,587	3402	28,989
Panel B. Mortgage Characteristics			
Loan amount	305,906 (163,130)	277,273 (101,105)	-28,632*** (4382)
Applicant income	111,519 (76,842)	91,110 (38,770)	-20,410*** (2043)
Share white buyers	0.450 (0.244)	0.231 (0.219)	-0.219*** (0.007)
Share Hispanic or Latino Buyers	0.276 (0.241)	0.464 (0.286)	0.189*** (0.007)
Share black buyers	0.031 (0.054)	0.098 (0.149)	0.067*** (0.002)
Observations	8694	1476	10,170
Panel C. Evictions			
Evictions per 1000	4.237 (37.441)	5.076 (6.289)	0.839* (0.468)
Eviction filings per 1000	5.042 (46.840)	5.950 (7.248)	0.908 (0.585)
Observations	40,590	6800	47,390
Panel D. School Demographics			
Non-Hispanic white enrollment share	0.171 (0.190)	0.074 (0.088)	-0.097*** (0.006)
Non-Hispanic black enrollment share	0.088 (0.123)	0.146 (0.165)	0.058*** (0.004)
Hispanic enrollment share	0.646 (0.246)	0.703 (0.208)	0.057*** (0.008)
Observations	21,560	1010	22,570

Notes: This table presents summary statistics and balance tests for pre-SIG neighborhood and school characteristics using data from the Federal Housing Finance Agency (Panel A), the Consumer Financial Protection Bureau (Panel B), the Eviction Lab at Princeton University (Panel C), and the California Department of Education (Panel D). Census tracts in Panels A through C are considered treated if they at least partially fall in an assignment zone for a school that later implemented a SIG-funded turnaround; similarly, control tracts are those that at least partially fall in an assignment zone for a school that was eligible for SIG but never received a grant. P-values for Column 3 represent standard t-tests of mean equality.

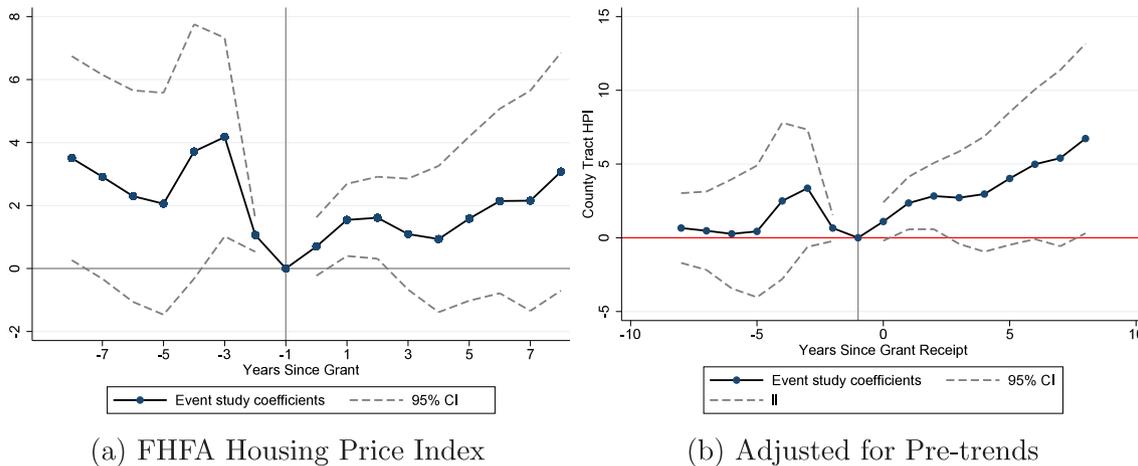


Fig. 7. Stacked event studies: California housing prices. Notes: These figures present event study coefficients from stacked event study specifications that compare changes in tract-level housing price indices from the Federal Housing Finance Agency for neighborhoods zoned for schools receiving SIG grants to those zoned for SIG-eligible non-recipients. Treatment in both panels is defined as the average per-pupil SIG grant amount received by a tract’s zoned schools, weighted by the share of the tract’s population zoned for each SIG school. Coefficients represent the increase in housing prices associated with a \$1000 increase in per-pupil SIG funding. Panel (a) is a standard event study, and standard errors are clustered by county. Panel (b) adjusts for pre-trends following a process outlined by Goodman-Bacon (2021), first estimating a pre-trend in housing prices for treated and control groups and removing this trend from the full panel prior to estimating the event study. To account for variation from the first-step detrending estimation, we construct two-step bootstrap standard errors clustered at the county level.

that home values in SIG school zones increased by 10.5% relative to houses just across attendance zone boundaries in response to SIG-funded interventions. This increase in price coincided with wealthier, whiter households moving into SIG attendance zones and schools and

the displacement of disadvantaged residents and students, as evident by a 35% increase in evictions and a 15% decline in the number of nonwhite students in SIG schools. Supplementary analyses in California suggest that this phenomenon was not unique to Nashville.

Table 14
Effect of SIG on housing prices and neighborhood demographics in California.

	(1)	(2)	(3)	(4)	5)	(6)	(7)	(8)
	HPI	Ln (Loan amount)	Ln (App. income)	Share white buyers	Evictions per 1000	Filings per 1000	White Enr. share	Hispanic Enr. share
SIG	2.634** (1.098)	0.030** (0.014)	0.035*** (0.009)	0.007*** (0.003)	0.280 (0.171)	0.277 (0.203)	0.0005 (0.0009)	-0.0027*** (0.0010)
N	112,119	65,952	65,952	65,952	123,744	123,744	83,847	83,847

Two-step bootstrap standard errors clustered at the county level for Columns (1) through (6) and at the school district level for Columns (7) and (8) are in parenthesis

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Notes: This table presents estimates from stacked pre-trend-adjusted difference-in-differences specifications that compare changes in outcomes of schools and neighborhoods receiving SIG grants to SIG-eligible non-recipients. Treatment across Columns (1) through (6) (tract-level outcomes) is defined as the average per-pupil SIG grant amount received by a tract's zoned schools, weighted by the share of the tract's population zoned for each SIG school; treatment in Column (7) (a school-level outcome) is defined as the per-pupil SIG grant received by a school. Coefficients across all specifications represent the change in outcomes associated with a \$1000 increase in per-pupil funding. All specifications adjust for group-specific linear pre-trends following a process outlined by Goodman-Bacon (2021), first estimating a pre-trend in outcomes for treated and control groups and removing this trend from the full panel prior to estimating the difference-in-differences. To account for variation from the first-step detrending estimation, we construct two-step bootstrap standard errors. Column (1) uses tract-level housing price indexes from the Federal Housing Finance Agency. Columns (2) through (4) use tract-level mortgage data from the consumer Financial Protection Bureau. Columns (5) and (6) use tract-level eviction counts from the Eviction Lab at Princeton University. Finally, Columns (7) and (8) use school enrollment data from the California Department of Education.

Although previous research has found generally positive effects of school turnaround programs on student achievement (Carlson & Lavertu, 2018; Friday, 2021; Sun et al., 2020, 2017), we provide direct evidence that households observe and value improvements in school quality from SIG-funded interventions. Estimates from our preferred specification imply that households were willing to pay \$13,148 to move into a SIG school zone, more than a 3:1 return on the average per-pupil total grant award of \$3817.57 to MNPS schools.²⁹ ³⁰ In the framework of Oates (1969), a dollar of outside funding (i.e., that which does not need to be paid by local taxes) toward a public good should increase housing prices by more than \$1 if the public good is under provided. Our estimated willingness to pay of over \$3 for a dollar in SIG funding is consistent with the hypothesis program was an efficient use of resources in Nashville, and the replication of this result in California is consistent with suboptimal funding of education in the nation's lowest-performing schools more generally. We contribute to literature on the capitalization of school quality by examining willingness-to-pay for funding targeting schools specifically at the bottom of the achievement distribution, where additional spending may be especially impactful.

We characterize who gained access to newly improved schools by examining characteristics of households moving into neighborhoods zoned for schools implementing SIG-funded interventions. Following grant receipt, higher-income, white households differentially moved into previously majority-minority SIG school zones, and the share of students who are white at SIG schools increased by nearly 20%. Billings et al. (2017) similarly find gentrification of neighborhoods that suddenly receive access to higher quality schools in Charlotte, North Carolina,³¹ and Bayer et al. (2020) find that school poverty rates decline by 0.21% in response to a 1% increase in school spending. These results and this paper's findings support the notion that households may sort across neighborhoods to access newly improved schools, a phenomenon

²⁹ \$13,148 = 10.5% (i.e., 10.0 log points) \times \$125,216, the mean pre-treatment house price in SIG school zones

³⁰ Although our estimated price effects fall on the larger end of estimates from the capitalization literature, they are qualitatively similar to the magnitudes of estimates of longstanding capitalization of school quality (Kane et al., 2006) and sorting (Billings et al., 2017) found in Charlotte, NC, a comparable setting to Nashville.

³¹ The authors find that average home price and household income in the "highest-quality" neighborhoods that qualify for priority in enrollment lotteries for oversubscribed schools increase by 8.4 and 13.1 log points, respectively. These results are qualitatively similar to the housing price and income effects we find (10.0 and 8.6 log point respective increases), and come from a comparable setting to ours, in regards to both the similarity of Nashville and Charlotte's housing markets and that both our paper and Billings et al. (2017) examine policies that affect attendance zones for low-performing schools.

that requires consideration when designing targeted K-12 funding and assignment policies.

The extent to which displaced households and students are "losers" of SIG-funded interventions depends on the quality of neighborhoods and schools to which they relocate. Because SIG targeted the country's lowest-performing schools in Nashville, with only *student* displacement, displaced students would mechanically move into schools with test scores at least as high as their original schools prior to SIG funding receipt. However, because SIG funding was largely used as 'hazard pay' to attract higher-quality teachers to targeted schools, the realized quality of local non-SIG schools populated by displaced students could be negatively affected by *teacher* poaching (Kho et al., 2020). Future work with a general equilibrium analysis of SIG's effect on teacher labor markets and the quality of schooling experienced by displaced students would fill this gap in policy evaluations of SIG and other K-12 funding interventions.

More broadly, our sorting results relate to the literature on public good provision and racial and income-based segregation. Moderate integration following SIG funding receipt is consistent the results of Diamond and McQuade (2019), whom find that place-based investments via the form of Low Income Housing Tax Credit-funded developments in low-income, high-minority neighborhoods raise house prices and increase neighborhood socioeconomic diversity. We cannot speak to whether the observed moderate integration into neighborhoods experiencing improved school quality will persist, and previous literature lends support to either lasting integration or a reversal. Card et al. (2011) find no evidence of minority flight when minority-majority neighborhoods experience small influxes of white neighbors; however, Banzhaf and Walsh (2013) find that race-based sorting dominates income-based sorting based on public goods when differences in public good quality across neighborhoods diminish.

Finally, our results contribute to the literature on the nature of gentrification of low-income neighborhoods, a central question of which is whether gentrification helps or harms (predominantly minority) existing neighborhood residents. McKinnish et al. (2010) find no evidence of displacement of low-education or minority residents of gentrifying neighborhoods during the 1990s, despite large-scale in-migration of college-educated whites that increased housing prices and neighborhood income. We provide evidence that gentrification following SIG funding receipt displaced a non-negligible share of existing residents through increased evictions. This phenomenon demonstrates a limitation of place-based public good provision: sorting may displace the initially targeted population. Normative evaluations of policies that directly or indirectly affect housing markets will benefit from further research on this dynamic.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary results

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.econedurev.2023.102382>.

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