The Value of Local Public Goods: Evidence from Massachusetts' Property Tax Limits*

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ABSTRACT This paper quantifies households' willingness-to-pay for local public goods using Massachusetts' Proposition 2½, which caps annual property tax increases but permits voter-approved overrides to finance specific projects. Using a novel dataset of all proposed tax overrides from 1996–2023, we implement a dynamic regression discontinuity design that leverages close-margin elections. We find that passing an override causes a sustained 2.8% increase in housing prices over ten years. This estimate rises to 4% in a boundary discontinuity design comparing adjacent properties across municipal borders, controlling for unobserved neighborhood characteristics. We show this capitalization is driven by an influx of higher-income households attracted by enhanced public services. Consistent with this mechanism, overrides lead to a cumulative 25% increase in per-pupil district and teacher expenditures, while the enrollment share of low-income students simultaneously declines. Back of the envelope calculations reveal that homeowners are willing to pay approximately \$2 for each \$1 of override-funding spending.

Keywords: Proposition $2^{1/2}$; Willingness-to-pay; Housing markets; Public Goods; Inequality

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1 Introduction

Public goods are central to many societal outcomes, from fostering intergenerational mobility (Mayer and Lopoo 2008; Weide et al. 2024) and reducing inequality (Currie and Gruber 1996; Jackson, Johnson, and Persico 2015; Gethin 2025a; 2025b) to improving health and overall well-being (Cha, Han, and Lee 2025). In many states, many of these services are provided by local government. In the U.S., for instance, municipalities are responsible for K–12 education, public safety, and infrastructure, together accounting for roughly 10% of the national GDP. Given their scale and redistributive impact, the provision of local public goods requires careful assessment of households' willingness-to-pay (WTP) to ensure that these amenities do not reinforce, but rather mitigate, socioeconomic inequality. Without such knowledge, policymakers risk underproviding services that are most valued, or overinvesting in areas with limited social returns, thereby undermining redistribution and intergenerational mobility goals (Chetty, Hendren, and Katz 2016; Castanheira, Mariani, and Tricaud 2025).

Estimating the WTP for public goods is a foundational question in public economics (Samuelson 1954; Tiebout 1956). A large empirical literature has examined how local public goods capitalize into housing markets, providing a revealed-preference measure of households' WTP. However, the literature has yet to reach a consensus. Cellini, Ferreira, and Rothstein (2010) find that each dollar of school facility spending raises housing values by at least $1.50, while Greenstone and Gallagher (2008) document much smaller and often statistically insignificant effects of hazardous waste cleanups. More recent work, such as Biasi, Lafortune, and Schönholzer (2025), shows that impacts vary sharply across project types and districts. Studies of safety and environmental quality likewise find nontrivial but heterogeneous capitalization effects (e.g., Linden and Rockoff 2008; Chay and Greenstone 2005).

This paper estimates households' willingness to pay (WTP) for sustained improvements in local public goods by exploiting quasi-experimental variation generated by Massachusetts' Proposition 2½ overrides. These voter-approved referenda provide localized and exogenous shocks to municipal service funding, allowing us to identify how residents value changes in public-good provision. Using detailed data on override elections, municipal finances, and housing transactions, we estimate causal effects through a Dynamic Regression Discontinuity (DRD) framework that compares municipalities with narrowly decided override outcomes over time.

Proposition 2½ is a Tax and Expenditure Limitation (TEL) policy that caps the annual growth of local property tax revenues at 2.5%, but allows communities to permanently exceed this cap through direct voter approval. Each override ballot specifies both the dollar amount of the increase and the project it will fund—such as school operations, infrastructure, or public safety—creating a transparent and permanent change in the municipal levy limit. Because these referenda are decided by simple majority vote, they generate sharp, localized, and voter-determined shocks to public-good spending. Our empirical strategy leverages this institutional design to isolate the capitalization of sustained service improvements into housing prices, while accounting for the presence of multiple overrides within municipalities over time. This setting is ideally suited to addressing the sources of heterogeneity highlighted in prior work. Unlike studies focusing on one-off capital investments—such as school construction or environmental cleanups—overrides capture households' valuation of sustained improvements in public services. Moreover, the institutional design provides large, exogenous, and varied shifts in local spending across comparable jurisdictions while mitigating concerns about sorting and neighborhood preferences correlated with service quality (Bayer, Ferreira, and McMillan 2007).

To estimate WTP for public services, we first gather an unbalanced panel dataset with detailed information on Proposition $2^{1/2}$ referenda (e.g., overrides), from 1992 to 2025. This information includes approval outcomes, proposed tax amounts, and ballot language, which allows us to identify both the timing and scope of public goods expansions. We then construct a novel dataset by spatially joining override vote records with county assessor and real estate transaction data from the Massachusetts Department of Revenue (MDOR). We focus on single-family houses, for which we observe sale price, property characteristics (e.g. square feet, number of rooms, years of construction), as well as latutude and longitude, which we use to precisely geolocate the transactions. To assess the robustness of these data, we also combine our dataset with the FHFA house price index constructed by Larson and Contat (2022). We combine the previous data with information on education expenditures at the district level and with student enrollment at the school level from the Massachusetts Department of Elementary and Secondary Education (MDESE), which we leverage to investigate whether an override is linked with increased provision of public services. Finally, to further control for demographic characteristics, across and within municipalities, we merge our dataset with census tract or block-level (depending on availability) data from the American Community Survey (ACS).

We estimate the causal effect of tax overrides and recover households' WTP for public goods using the DRD framework developed by Cellini, Ferreira, and Rothstein (2010). This design is particularly well suited to our setting, because overrides may occur multiple times and the probability of approval is correlated with their history. We complement this approach with a boundary discontinuity design (BDD), which leverages variation from housing prices for properties located in close proximity but on opposite sides of municipal borders where one side narrowly passed an override while the other narrowly failed. This methodology addresses potential concerns linked to household sorting at very fine geographic levels, which may bias our WTP estimates if unobserved neighborhood characteristics are correlated with demand for public goods (Bayer, Ferreira, and McMillan 2007; Schonholzer 2018; Schönholzer 2024). This "across-the-street" comparison allows us to separate the value of public goods from the value of living near higher-income neighbors or other unobserved covariates

We find that overrides increase housing prices by 2.8% over 10 years. This translates into an increase for the average single-family house transacted of around $11,500. This effect is robust to using alternative housing price estimates and to the BDD identification approach, which suggest even larger capitalization effects (nearing 4-8% over a 10-year period). The observed rise in housing prices suggests that households are willing to bear this increase even at the cost of higher property taxes, suggesting that the local government was inefficiently providing local public goods in those jurisdiction. We use the coefficients from the DRD and BDD specification to perform a back-of-the-envelope calculation to retrieve household WTP for local public goods. We find that homeowners are willing to pay roughly $2 in present value for each $1 of override-funded spending, a result in line with the idea that Massachusetts's tax cap constrained efficient local investment. This value is larger than the one found by Cellini, Ferreira, and Rothstein (2010), which is around $1.50. This difference can be explained by the fact that we look at a broader set of public goods, not just education, and that our shock is permanent, while Cellini, Ferreira, and Rothstein (2010) focus on one-off capital investments.

The increase in housing prices following override approvals motivates us to examine whether these changes reflect improvements in the provision of public goods. Using per-pupil in-district and teacher expenditures, we find that overrides are associated with a persistent rise in spending, with per-pupil expenditures increasing by about 25% over ten years. This pattern suggests that higher housing values are driven by enhanced public service quality. The rise in housing prices also indicates that wealthier households may be selectively moving into these municipalities. To assess whether this sorting translates into socioeconomic segregation, we use MDESE enrollment data and document a decline of up to 1.5% in the share of low-income students in municipalities that pass an override, which suggests that overrides effectively make municipalities more desirable, by allowing them to provide higher value of local public goods. While higher spending and service quality benefit incumbent residents, the sorting mechanism implies that future gains accrue disproportionately to wealthier households, with disadvantaged families increasingly excluded from the very public goods that their taxes also help finance.

2 Literature

This paper primarily speaks to the literature investigating how local fiscal institutions shape the provision of public goods, residential sorting, and housing markets.

Tax Limitation Rules and Local Public Goods: Our first contribution is to the literature examining the effects of property tax and expenditure limitations (TELs). This literature flourished decades ago, finding anyway different effect of TELs on local revenus and local public goods quality (Cutler, Elmendorf, and Zeckhauser 1999a, and Figlio and Rueben (2001)). Over the past two decades, several empirical studies have revisited these questions using more robust identification strategies. Ferreira and Gyourko (2011) document how tax and expenditure limitations, by reducing public money collect through property taxes, hamper local revenues and alter the distribution of school spending. Clemens and Miran (2012) leverage state-level variation to show that fiscal rules significantly constrain municipalities during recessions. We contribute to this literature by showing that TELs, through their override mechanisms, can be used to estimate household WTP by focusing on their effect on the housing market. At teh same time, they can have unexpected segregation effects.

Moreover, we also contribute to the literature studying the equity considerations of the U.S. property tax system. Our findings offer a potential mechanism to explain why several scholars have documented the possibility that public investments may lead to regressive outcomes (Kennedy-Moulton et al. 2022; Agrawal and Bütikofer 2022; Brinkman and Lin 2022). By allowing voters to approve additional funding for public services, overrides can generate disparities in service provision between communities that can pass overrides and those that cannot. This mechanism may contribute to the observed inequities in public service delivery, particularly in education, where wealthier communities are better positioned to fund high-quality schools through overrides.

Housing Prices and Capitalization of Public Goods: Our paper also relates to the extensive literature on the capitalization of public goods into housing prices (Oates, 1969; Rosen, 1974; Brueckner, 1979; Berglas, 1984; Brueckner and Lee, 1989). Much of this literature focuses on the effect of school investment on real estate markets, generally finding positive results (see Cellini, Ferreira, and Rothstein (2010); Neilson and Zimmerman (2014); Goncalves, 2015; Conlin and Thompson, 2017). Black (1999) pioneered a Boundary Discontinuity Design (BDD) to account for differences in education services across school districts, finding a positive effect on housing prices. Black (1999) ‘s findings are qualitatively similar, though smaller in magnitude, to those of Bayer, Ferreira, and McMillan (2007), who examine the impact of local public goods and unobserved neighborhood characteristics in the San Francisco Bay Area.

We contribute to this literature by analyzing public good investments that extend beyond education, including public safety and infrastructure. This broader perspective enables us to capture a wider spectrum of public goods that may influence both housing prices and residential segregation. Moreover, we build on the BDD approaches used by Black (1999) and Bayer, Ferreira, and McMillan (2007) by comparing houses located on either side of a municipal border and focusing on those within a narrow buffer zone (Schönholzer 2024). Additionally, our findings are connected to more recent works (Cellini, Ferreira, and Rothstein 2010; Imberman, Kugler, and Sacerdote 2015) that focus on the effects of specific capital investments and policy reforms.

Sorting, Segregation, and preferences for public goods: Finally, we contribute to the literature on how local fiscal policies affect residential segregation. In a seminal contribution, Tiebout (1956) provided the theoretical foundation for how consumers with heterogeneous preferences over public goods sort into jurisdictions that offer their preferred bundles of local public services and property tax levels. More recent work has sought to disentangle how sorting decisions are influenced by public goods valuation and preferences over other unobserved neighborhood amenities that may be correlated with those valuations (see Monarrez and Schönholzer (2023), for a review). Bayer, Ferreira, and McMillan (2007) demonstrate that accounting for unobserved neighborhood quality reduces the estimated positive effect of public goods on housing prices. Schönholzer (2024) confirms this pattern while still documenting a positive and significant effect of preferences for public goods on housing market outcomes.

We leverage override votes that exogenously increase the level of public goods in a given jurisdiction by comparing housing prices at the borders of municipalities that did and did not pass overrides. This design allows us to isolate the effect of local public goods valuation. We find a positive and significant effect of local amenities on housing prices. Our findings confirm that public amenities play a significant role in household sorting decisions and may thus contribute to residential segregation. In this regard, our results are broadly consistent with Chetty, Hendren, and Katz (2016), who highlight the welfare implications of neighborhood amenities.

3 Institutional Background

Proposition $2^{1/2}$ is a TEL policy enacted in 1980 in response to the economic pressures that were hitting Massachusetts as in other U.S. states (Cutler, Elmendorf, and Zeckhauser 1999b). Among the causes of these economic pressures was a high per capita property tax burden, nearly double the national average by 1977, and a structurally high reliance on property taxation as a revenue source. This fiscal strain coincided with a prolonged stagnation in real household income, fostering a perception of government inefficiency (Poterba 1994) similar to the environment that led to California's Proposition 13 in 1978. The property tax's "visibility" and its disconnect from current income exacerbated voter discontent during economic hardship (Wallin and Zabel 2011).

Proposition $2^{1/2}$ operates through two primary constraints: a levy ceiling limiting the total property tax levy to 2.5% of total taxable property value, and a levy limit restricting annual levy growth to 2.5%. It is important to note that this 2.5% increase applies solely to the levy limit, not directly to individual property tax bills. While the levy limit is typically more binding, a "new growth" provision enables revenue increases tied to new development, thereby incentivizing pro-growth local policy. Rigidity is tempered by voter-approved "overrides," which permanently increase the levy limit. This mechanism transforms the limit into a system of direct democracy over marginal spending.

The economic impact of Proposition $2^{1/2}$ is clarified by comparison to California's Proposition 13. While Proposition 13 limits tax rates and assessed value growth, creating a "lock-in" effect that distorts residential mobility decisions, Proposition $2^{1/2}$ constrains the municipal tax levy's growth, making it a more ideal setting for identifying the effects of revenue constraints on public service provision (Lang and Jian 2004). Moreover, Proposition 13 centralized local finances, whereas Proposition $2^{1/2}$, via its override mechanism, preserves a degree of local democratic fiscal control, offering insights into direct democracy in local public finance (Cutler, Elmendorf, and Zeckhauser 1999b).

Proposition $2^{1/2}$ overrides provide communities with a mechanism to exceed their levy limit through direct voter approval. When the override is approved, there is a permanent property tax increase intended to generate sustained revenue streams to support ongoing municipal service provision. Therefore, a successful override permanently elevates the levy limit, and the additional revenue amount becomes embedded in the levy base for calculating future annual 2.5% increases. The ballot questions presented to voters for an override must specify a precise dollar amount and a defined purpose, which can range from general operating expenses to highly specific services such as education or snowplowing. Approval requires a majority vote from the electorate.

The design of Proposition $2^{1/2}$, particularly the override mechanism, introduces localized fiscal autonomy that can generate inter-municipal disparities in public service provision and, consequently, potentially increase segregation. While the law imposes strict limits on property tax revenue growth, the override provision allows communities to permanently exceed this limit with voter approval for ongoing operations. This means that municipalities face heterogeneous constraints under the 2.5% limit. Instead, their actual fiscal capacity for public services becomes contingent on local voter preferences and their collective ability to pay. Communities with a higher collective WTP for public services, such as better schools, which are often correlated with higher income levels, are more likely to successfully pass overrides. This capacity to raise additional, permanent revenue enables these communities to maintain or enhance the quality of their public services, particularly education. This unequal capacity to finance public services, within a uniform state-imposed constraint, creates a strong mechanism for stratification in the quality of local public goods. This divergence, in turn, can attract different socioeconomic groups, thereby reinforcing residential sorting and contributing to socioeconomic segregation, even if the state policy's primary intent was fiscal control. The override, while intended as a flexible tool, may emerge as a key institutional driver of inter-municipal inequality.

4 Data

This study utilizes a comprehensive dataset, compiled from various publicly available sources, to analyze the impact of Proposition $2^{1/2}$ overrides on socioeconomic segregation and housing prices in Massachusetts. The data span from the year 1996 onwards.[2], allowing for a robust panel analysis.

Proposition $2^{1/2}$ Overrides Data Data on Proposition $2^{1/2}$ override votes, including their approval status, dollar amounts, and the text of the ballot specifying its purpose, are collected

[2] While override data are available from 1992, wwe start our analysis based on the first year we observe our outcome variables. For the case of pupil enrollment, it is 1996; for housing prices, it is 2001; while for education expenditures, it is 2009.

from the Massachusetts Department of Revenue (MDOR) website. This dataset provides detailed information on municipal fiscal decisions to exceed the standard levy limits, i.e., overrides. The latter represent voter-approved permanent increases in the tax levy, intended to fund ongoing municipal service provision. The unit of observation is at the town-year level. A municipality can pass multiple overrides in a given year, when this happens, we keep the largest override in terms of monetary amount.[3].

The final dataset includes override attempts dating back to 1992 for 303 out of 351 municipalities in Massachusetts (approximately 86 percent). Around 72 percent of them successfully passed at least one override. The dataset includes 1801 overrides attempts, with 63 percent of them being successful overrides. The average amount of a successful override is 731.77 thousands of dollars.

Figure 1 plots, on the left, the spatial distribution of the first override proposals across Massachusetts municipalities. We begin in 1996, the first year for which our outcome data are available. Most municipalities proposed their first override in the late 1990s and early 2000s, with a smaller wave in the late 2010s; 98 municipalities never proposed an override after 1996. The right panel reports the year in which each municipality first successfully passed an override.

Appendix Figure A.1 presents analogous maps including proposals and approvals prior to 1996. Figure A.3 instead shows the total number of overrides proposed and approved and by each municipality. The median municipality in our sample proposed at least four overrides and successfully passed two.

[3]This approach is consistent with that used by Biasi, Lafortune, and Schönholzer (2025) as they focus on bond referenda. This ensures that our analysis captures the most significant fiscal event for each municipality-year, allowing us to isolate the largest relevant treatment while minimizing confounding from smaller, concurrent overrides.

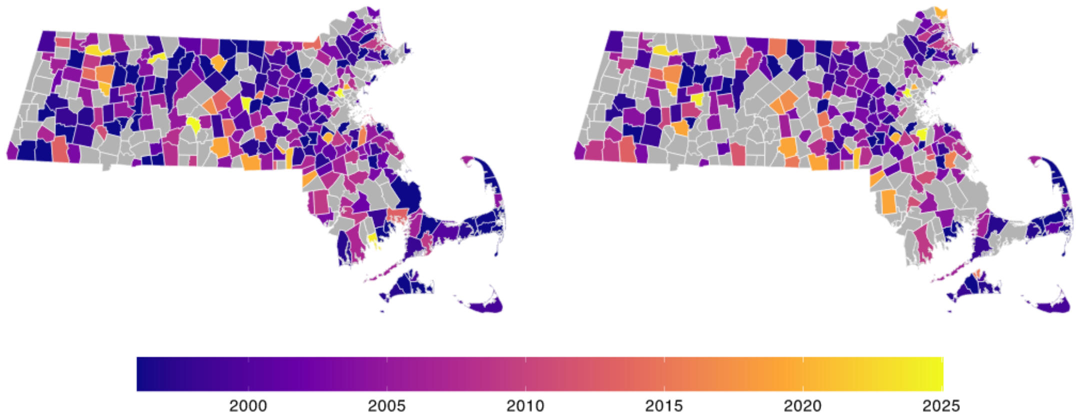

Figure 1: First Override Proposals and Approvals in Massachusetts Municipalities (1996-2025). The figure plots the year of the first proposed (left) and passed (right) override in each municipality in Massachusetts, for the period 1996-2025. Municipalities that never proposed or passed an override are colored in grey.

Finally in Table 1 we report a summary of the override data by fiscal year.

Housing Data: To assess the impact on housing prices, we rely on two primary sources:

1. Assessor and Real Estate Transaction Data from the Massachusetts Department of Revenue (MDOR): we gather data on the universe of real estate transactions starting in 2001 from MDOR public data.[4] This information records each transacted property in Massachusetts, including details such as sale price, date of sale, buyer and seller names, and a property identifier. From the raw data, we keep only residential single-family homes, which are the most common type of housing in Massachusetts. We merge this information with county assessor's data, which provides detailed property characteristics such as residential area, lot size, number of rooms, year of construction, and exact location of the properties. This allows for a more accurate estimation of the housing price effects by controlling for property characteristics that may influence transaction prices. Moreover, observing the location of the properties allows us to spatially merge transacted properties with the municipality at the census tract/block administrative level in which the property is located. This is crucial to, first, combining real estate data with demographic characteristics at the census tract/block level and, second, to implement a boundary discontinuity design (BDD), which requires precise property-level location data to

[4]Data can be accessed at https://dls-gw.dor.state.ma.us/gateway/dlspublic/parcelsearch.

identify properties close to a municipal border. The final real estate dataset includes 824,967 single-family home transactions, spanning from 2001 to 2020.

Table 1: Override Summary by Fiscal Year

Fiscal Year	Succ. Overrides	Avg. Amount (1000$)	Share Approved	Vote Share Mean	Vote Share SD
1996	55	116.06	0.33	0.56	0.04
1997	29	184.48	0.31	0.62	0.10
1998	41	164.55	0.46	0.64	0.12
1999	35	237.44	0.70	0.61	0.09
2000	27	249.63	0.48	0.60	0.07
2001	52	346.63	0.64	0.63	0.08
2002	55	544.38	0.73	0.59	0.07
2003	55	887.07	0.60	0.59	0.08
2004	80	492.96	0.50	0.62	0.09
2005	83	312.77	0.51	0.63	0.09
2006	93	520.11	0.54	0.61	0.10
2007	52	653.31	0.37	0.60	0.08
2008	48	746.42	0.47	0.59	0.07
2009	60	618.28	0.43	0.59	0.08
2010	33	496.41	0.54	0.63	0.08
2011	27	475.85	0.47	0.61	0.08
2012	21	727.68	0.42	0.64	0.11
2013	20	500.05	0.47	0.64	0.09
2014	14	1119.61	0.52	0.62	0.08
2015	22	741.26	0.63	0.61	0.10
2016	21	1107.57	0.52	0.64	0.11
2017	14	281.02	0.56	0.64	0.08
2018	14	516.26	0.64	0.61	0.09
2019	36	872.00	0.82	0.67	0.13
2020	23	1394.60	0.77	0.62	0.10
2021	12	895.18	0.57	0.63	0.02
2022	20	954.60	0.87	0.78	0.12
2023	18	376.46	0.82	0.68	0.07
2024	47	1097.05	0.75	0.64	0.10
2025	23	2094.04	0.45	0.64	0.08

The table reports summary statistics on Proposition 2^1/2^ overrides by fiscal year. Succ. Overrides is the number of successful overrides; Avg. Amount (1000$) is the average amount of successful overrides in thousands of dollars; Share Approved is the share of successful overrides; Mean and SD are the mean and standard deviation of the vote share in successful overrides.

2. FHFA House Price Index (FHFA HPI): This comprehensive index, provided by the Federal Housing Finance Agency (Contat and Larson, 2022), measures changes in single-family home values across all 50 states and over 400 American cities, with data extending back to the mid-1970s. The FHFA HPI incorporates tens of millions of home sales and is constructed using a weighted, repeat-sales statistical technique, analyzing price changes from repeat sales or refinancings on the same properties. This provides a broad and reliable indicator of house price trends at various geographic levels. We use information provided at the census tract level. We then assign the centroid of each census tract to a municipality and compute municipality-year-level weighted averages of the HPI in Massachusetts.

In Figure A.4 and Figure A.5 in the appendix, I plot the distribution of (log) sale prices for the whole sample and in municipalities that passed and never passed an override. The histograms for the 3 samples reassuringly report a similar pattern. In municipalities that passed at least one override, the distribution of sale prices is shifted to the right.

Student Enrollment Data and Segregation Measure: student enrollment data for each public school in Massachusetts, starting from the year 2000, come from the Massachusetts Department of Elementary and Secondary Education (MDESE). MDESE reports the enrollment rate of low-income pupils for each school in the state. This metric is widely used in both economic and sociological research as a proxy for socioeconomic segregation in schools (Reardon and Owens 2014; Jackson, Johnson, and Persico 2015; Lafortune, Rothstein, and Schanzenbach 2018). Schools with higher concentrations of low-income students often face fewer resources, which can contribute to disparities in educational outcomes. The use of this measure allows us to capture the extent to which students from different socioeconomic backgrounds are sorted into distinct educational environments.

To proxy for public goods quality, we collect district and teacher expenditures per pupil for the period 2009–2025. These variables help contextualize variation in public education quality and resource allocation in Massachusetts.

Table Table 2 reports mean values for a set of key variables across different samples of our data. Column (1) presents the full sample, while Columns (2) and (3) restrict to districts that never passed an override and those that passed more than one, respectively. More relevant to our empirical strategy are Columns (4) and (5), which report median values in the year prior to passing or failing an override. These two groups, which mimic our treatment and control samples, are very similar in terms of average housing prices and the share of low-income students.

5 Methodology

To estimate the causal effect of Proposition 2½ overrides on housing prices and socioeconomic segregation, we employ a dynamic regression discontinuity (DRD) design, extending the framework of Cellini, Ferreira, and Rothstein (2010). This design is particularly well-suited for our context, where municipalities can propose and pass multiple override measures over time, and where the effects of these overrides may unfold dynamically.

We interpret override approval as a positive and permanent shock to the fiscal capacity of a municipality, favouring higher levels of public service provision. If households value these enhanced public local amenities, such as education, we should observe higher housing prices in jurisdictions that succeed in passing overrides. However, we cannot simply compare municipalities that passed overrides to those that did not, as the two groups may differ in systematic and unobserved ways that also affect housing prices. Thus, this comparison would likely yield biased estimates of the effect of overrides.

To this extent, we leverage the quasi-random assignment of treatment by comparing municipalities with override proposals that passed or failed by a narrow margin. As shown by an extensive literature (Grembi, Nannicini, and Troiano 2016), in municipalities in close elections, the assignment of treatment is as-if random, as voters and other unobserved characteristics on either side of the margin are likely to be continuous. This approach helps to mitigate concerns about selection bias and confounding factors that could otherwise violate the parallel trends assumptions and thus distort our estimates. We do not rely on a sharp, discontinuous change in treatment assignment at a certain margin threshold. We follow the approach of Biasi, Lafortune, and Schönholzer (2025) and Cellini, Ferreira, and Rothstein (2010) employing a local polynomial regression approach. Specifically, we model the relationship between the outcome variable and the vote share margin using a third-degree polynomial function on either side of the cutoff. This flexible functional form allows us to capture potentially non-linear relationships between the vote margin and the outcomes, ensuring that the estimated discontinuity at the cutoff is not driven by misspecification of the underlying trend. Moreover, in this way we retain the full sample, which is important for identifying dynamic treatment effects over time.

Table 2: **Summary Statistics by Override Passage Status**

	(1) Full Sample	(2) Never Pass Override	(3) Pass at least 1 Override	(4) Pass Override in t-1	(5) Fail Override in t-1	(6) Diff. (4) - (5)
Education data						
% Low Income Pupils	0.30	0.29	0.14	0.11	0.15	-0.04^{***}
% White Pupils	0.70	0.76	0.87	0.89	0.90	-0.01
% Black Pupils	0.08	0.05	0.03	0.03	0.02	0.01
District exp. (log)	9.63	9.64	9.63	9.75	9.52	0.23^{***}
Teacher exp. (log)	8.68	8.68	8.67	8.79	8.56	0.23^{***}
Sale price (log)	12.96	12.81	13.16	13.18	12.96	0.22^{***}
Transactions data						
Square Footage	1.95	1.81	2.10	2.05	2.01	0.04^{***}
Lot Size	3.25	2.43	4.30	6.38	8.43	-2.05^{***}
Sale Volume	826.13	478.65	331.01	33.70	14.46	19.24
Median Income (log)	11.31	11.22	11.47	11.30	11.23	0.07^{***}
Census data						
% White People	0.92	0.91	0.93	0.95	0.96	-0.01^{***}
% Black People	0.04	0.05	0.02	0.02	0.01	0.01^{***}

The table reports summary statistics for key variables across different samples of our data. Column (1) presents the full sample, while Columns (2) and (3) restrict to districts that never passed an override and those that passed more than one, respectively. Columns (4) and (5) report median values in the year prior to passing or failing an override.

Another threat to identification is the dynamic and recurrent nature of overrides. In our smaple, around 60% of municipalities approved at least one override and the median jurisdiction proposed at least 4 overrides. As a result, in any given period some treated units may already have experienced prior overrides or may face future ones. Estimating a standard DRD model in this setting would bias the estimated effect of the current override due to spillovers from past and anticipated referenda (Cellini, Ferreira, and Rothstein 2010, for the mathematical derivation of this estimator).

To address this issue, we follow Cellini, Ferreira, and Rothstein (2010) and estimate the one-step Treatment-on-the-Treated (TOT) effect. This approach allows us to recover the causal impact of approving an override as if it were computed relative to a control group of municipalities that do not pass overrides in the future. As they emphasize, this estimator provides a way to recover households' WTP for an incremental improvement in public goods provision. The TOT measures the impact of exogenously authorizing an override in a given year while fully conditioning on a district's prior and future override history, thereby constructing a de facto counterfactual in which never-treated units are authorized to pass overrides in the future. In our context, these estimates are particularly valuable because they capture the local WTP for marginal public spending, that is, the value households place on the additional fiscal capacity created by a specific override authorization.

This specification translates into the following model:

$$Y_{i(h/s)t} = \alpha_i + \gamma_{it} + \sum_{k \neq 0} [\beta_k D_{it-k} + P^g(V_{it-k}, \delta_k^g) + \phi M_{it-k}] + X_{i(h/s)t}\theta + u_{it} \qquad (1)$$

Where Y_{it} is the outcome of interest for municipality i at time t, α_i represents municipality fixed effects, γ_{it} are year fixed effects, D_{it-k} is a dummy variable indicating whether municipality i passed an override in year $t-k$, V_{it-k} is the vote share margin for municipality i in year $t-k$, and M_{it-k} is equal to one if municipality i proposed an override k years before t. The term $P^g(V_{it-k}, \delta_k^g)$ represents a polynomial function of the vote share margin, allowing for flexible functional forms. The parameter g represents the order of the polynomial and δ_k^g are its coefficients. The coefficients β_k capture the dynamic effects of overrides over time and are estimated by leveraging the quasi-random variation generated by close elections. Finally, u_{it} is the error term. Finally, $X_{it}\theta$ is a vector of controls that we include in some of our specifications. Depending on the specification, Y_{it} and $X_{it}\theta$ are defined at either the property level ($h \in i$) or the school level ($s \in i$), but are always indexed to municipality i and year t. These controls include time-varying municipality (or census tract/block) characteristics such as total population, median household income, and ethnic composition. Including these controls helps to account for other factors that may influence the outcome variable and ensures that our estimates of the override effects are not confounded by concurrent changes in municipal characteristics.

Importantly, we set k to range from minus five to plus ten, meaning that we estimate the effect of passing an override from 5 years before to ten years after the vote. This allows us to capture both anticipation effects (if any) and the dynamic evolution of the treatment effect over time. Extending the horizon much further introduces two complications: first, treatment effects become confounded by subsequent referenda, policy changes, and local shocks that weaken the as-if-random assignment generated by close elections; second, as pointed out by Cellini, Ferreira, and Rothstein (2010), estimation precision deteriorates as the number of units at extreme leads and lags becomes sparse, inflating standard errors and weakening power.[5] Moreover, most fiscal and educational responses to school finance overrides, such as construction, staffing adjustments, and enrollment shifts, materialize within a few years, making the −5 and +10 year window sufficient for capturing the primary effects of interest. By limiting the horizon in this way, the estimates remain both interpretable and statistically reliable.

A final challenge to our identification strategy is disentangling households' WTP for public services from preferences for correlated, unobserved neighborhood characteristics. As discussed in the literature (Black 1999; Bayer, Ferreira, and McMillan 2007; Lafortune and Schönholzer 2022), sorting on such unobservables can confound estimates of the valuation of public services. Consequently, the estimated effect of a tax override on housing prices may reflect not only enhancements in public service but also endogenous household sorting. For instance, if wealthier households, who may have a higher marginal WTP for public education, systematically sort into municipalities that pass these overrides, our estimates would be confounded by preferences for higher-income neighbors.

To address this potential endogeneity, we augment our empirical strategy by incorporating a boundary discontinuity design. The identifying assumption of the BD approach is that while unobserved locational amenities are continuous at the municipal border, the override induces a sharp discontinuity in public service provision and local tax burdens. We therefore restrict the sample to properties transacted within a narrow bandwidth (500 meters) around borders separating municipalities that narrowly passed overrides from adjacent ones that narrowly failed.

[5] In Cellini, Ferreira, and Rothstein (2010)'s main TOT result, it is possible to observe how at larger leads confidence intervals become much wider.

By comparing properties on opposite sides of these specific borders, the BD design allows for identification of the causal effect of override approval on housing prices, while holding constant unobserved neighborhood characteristics.

5.1 Validation of the Research Design

We perform several tests to validate the credibility of our design. First, we assess whether our override data satisfy the key assumptions of an RDD by performing the McCrary (2008) test. Figure 2 shows the results and confirms the absence of manipulation of the electoral margin variable around the cutoff.

Second, Table 2 reports average outcomes and covariates for municipalities that narrowly passed versus narrowly failed an override in the year prior to the vote. The two groups are closely aligned in terms of average housing prices and the share of low-income students, providing reassurance that treatment and control units are comparable at baseline.

As a robustness check, we follow the concern noted by Biasi, Lafortune, and Schönholzer (2025) that including future treatment variables may introduce “bad controls” if they are themselves endogenous to the focal decision. In our setting, this corresponds to the possibility that future override (bond) elections are affected by the outcome of the current election. To address this, we re-estimate our stacked DRD specification excluding the terms for future bond history (M_{it-k} for $k < 0$). The results (Section C.2.1) are nearly identical to our baseline estimates, providing reassurance that our findings are not driven by this specification choice.

Finally, in Section 6, we show that housing prices exhibit no pre-trends or anticipation effects of future overrides. This strengthens confidence that, absent an override, treated and control municipalities would follow parallel trajectories in housing prices.

Figure 2: McCrary (2008) Density Test. We perform the McCrary (2008) density test around the 0% cutoff (using a uniform kernel and a cubic polynomial). The histogram shows the distribution of municipalities' vote margins, with separate density estimates for those just below ("Unsuccessful") and just above ("Successful") the 0% threshold. The dashed vertical line marks the cutoff at 0. A smooth density is fitted on each side to assess whether there is a discontinuity at the threshold, which would suggest potential manipulation in the running variable. The hypotehsis of manipulation is rejected (p. value = 0.39).

6 Results

To retrieve the WTP for public goods, we first estimate equation (1) retaining the full sample of transacted houses. We then refine our estimates by implementing a BD design, restricting the sample to properties located within a 500-meter bandwidth of municipal borders, still comparing jurisdictions that narrowly passed overrides to those that narrowly failed. This approach allows us to isolate the effect of override approval on housing prices while controlling for unobserved neighborhood characteristics that may influence housing market outcomes.

6.1 Effect on Housing Prices and Willingness to Pay for Public Goods

Successful overrides, by improving public goods and attracting higher-income residents, are expected to be capitalized into housing prices through the residential property market. Our primary analysis of housing prices uses a rich dataset of real estate transactions for single-family homes, including detailed property characteristics and local demographics, as detailed in Section 4.

Our outcome variable is the log value of sale price for each single-family house transacted in the period 2001-2023. We use latitude and longitude information to geolocate our real estate transaction data. This allows us to control for median income and ethnic composition at the census block or tract level depending on data availability. To adjust for inflation, we deflate all prices to 2016 dollars using the Consumer Price Index (CPI) from the Bureau of Labor Statistics. We also control for a set of property characteristics, including lot size, residential area, number of rooms, and the year of construction. Finally, we include municipality and year fixed effects and cluster standard errors at the municipality level.

As mentioned in Section 5, treated municipalities are the ones that uccessfully passed an override, while controls are the ones that fail to do so. In doing so, we control for the polynomials of the vote margin and for the past and future referenda history that, as noted by Cellini, Ferreira, and Rothstein (2010), allow us to causally identify the effect of the override by focusing on municipalities in close elections. The results from this specification are shown in Figure 3 .

The estimates indicate a positive response of housing prices to override approval. In the first years following an override, housing prices increase modestly, with an average effect of about 1 percent that is not statistically significant. Starting in year four, however, the impact grows, and by year five housing prices are significantly higher, with an effect of roughly 2.5 percent.

This pattern yields three insights. First, as expected, households positively value the increase in public goods provision associated with override approval. Second, the capitalization effect exceeds the additional tax burden households must bear, implying that voters are willing to pay a premium for the enhanced services (Cellini, Ferreira, and Rothstein 2010; Biasi, Lafortune, and Schönholzer 2025). Third, the delayed timing of the effect suggests that it takes several years for the improvement in local services to be fully reflected in housing markets. These findings are consistent with the hypothesis that households place a positive value on residing in municipalities able to sustain higher-quality public services. The magnitude of the effect is in line with prior estimates on the capitalization of local public goods into housing values (Lafortune and Schönholzer 2022).

It is worth stressing that the coefficients β_{t+k} report an effect that is independent from future overrides. This is obtained by including leads and lags of the override proposal margins M_{it-k}, which controls for the potential anticipation effects of future overrides. Finally, the coefficients β_{t-k} show no evidence of pre-existing trends; coefficients are tightly centered around zero and are statistically insignificant, reinforcing the plausibility of a causal interpretation. These findings indicate that override passage leads to significant increases in housing prices, reflecting households' willingness to pay for enhanced local public goods.

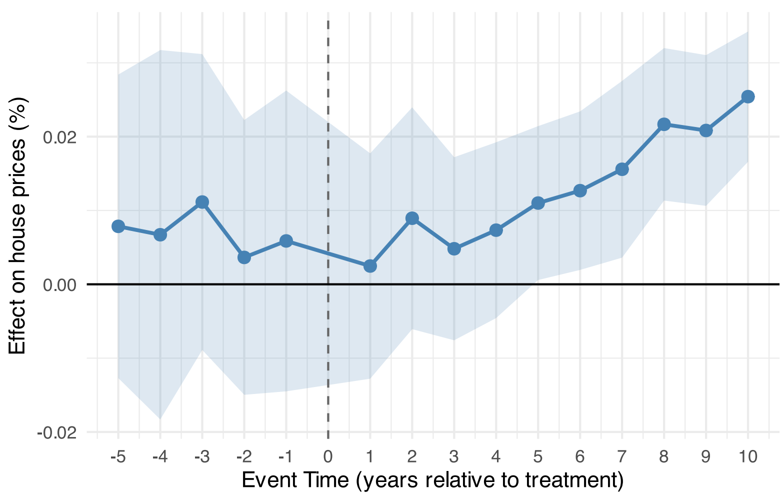

Figure 3: Effect of Overrides Approval on Housing Prices. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the log of sale prices, and the unit of observation is the property–year. We control for housing characteristics, including residential area, lot size, number of rooms, and year of construction. Demographic controls include census tract or block-level shares of White and Black residents, and median income. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

We assess the robustness of our main estimates in several ways. Figure A.13 and Figure A.14 present results when excluding demographic and property characteristic controls, and when excluding only property controls, respectively. In both cases, the estimates closely track those from the baseline specification. The positive effect of overrides becomes statistically significant only in the last three years, but the magnitude converges to a level comparable with the main results shown in Figure 3 .

In Figure Figure A.17 , we use the FHFA House Price Index (HPI), aggregated to the municipality-year level, as the outcome variable. Here too the effect of overrides is positive, with coefficients becoming statistically significant after year 8 and growing thereafter. By year 10, the estimates imply that override approval raises the HPI by roughly 8 percent. These results are consistent with Biasi, Lafortune, and Schönholzer (2025), who find similarly persistent effects of school bond referenda on house prices proxied by the HPI.

Our favorite estimates are the ones in Figure 3 . Even with municipality and year fixed effects, including both property and neighborhood demographic sharpens identification. First, property attributes guard against compositional bias in the transactions sample: if the mix of homes sold shifts around treatment (e.g., larger/newer units), estimates without these controls would confound composition with treatment. Second, tract/block demographics (median income and ethnic composition) capture within-municipality, time-varying demand shifts and neighborhood composition that fixed effects can't absorb; they help prevent omitted-variable bias if those shifts correlate with treatment timing. For these reasons, we place more weight on specifications that include both sets of controls, and treat them as our preferred estimates.

6.2 Disentangling Preferences for Public Goods from Sorting

The evidence we show in the Section 6.1 suggests that households are willing to pay a premium to live in municipalities that provide higher-quality public services, particularly education. A central concern, however, is endogenous sorting on unobservables: higher-income households may both demand better schools and select into municipalities that pass overrides, and they may also prefer neighborhoods with favorable but unobserved amenities (low crime, better access, peer composition). If so, event-study estimates such as the ones presented in Section 6.1 that rely only on municipality and year fixed effects may conflate the capitalization of public goods with changes in who buys and which homes transact.

To address this issue, we implement a boundary discontinuity design (BDD) in the spirit of Bayer, Ferreira, and McMillan (2007), and Lafortune and Schönholzer (2022). We compare transactions located in close proximity but on opposite sides of a border of two municipalities in close elections and include fixed effects for narrow boundary segments. This design leverages the fact that taxes and public services change discretely at jurisdictional lines due to the override, whereas other amenities and unobserved characteristics vary smoothly over short distances.

Under a local continuity assumption that, absent the override, prices and unobservables would not jump at the boundary, this augmented BDD specification differences out time-invariant, segment-specific amenities and sharply limits bias from sorting and neighborhood composition. Concretely, our main specification restricts the sample to transactions within 500 meters (0.31

miles) of a boundary separating a municipality that narrowly passed an override from one that narrowly failed, reducing the sample from roughly 800,000 to 250,000 transactions.

First, this "near-boundary" comparison sharply reduces unobserved amenity differences (views, noise, access to transit/retail, soil/topography) that vary across space but are essentially smooth at very short distances. Any time-invariant, boundary-segment-specific amenities are absorbed by the boundary fixed effects, so post-override price gaps can be interpreted as capitalization of jurisdiction-specific overrides. The design also mitigates sorting and compositional bias. Even if higher-income households gradually select into treated municipalities, comparing transactions within narrow boundary bands holds constant local peer environments and housing stock, making it less likely that observed price differences reflect who buys or which types of homes happen to transact. In event-time, the absence of pre-trends near the boundary further supports the idea that post-passage divergence reflects the valuation of improved public goods rather than differential trends in neighborhood desirability.

Figure 4 reports the resulting event-study coefficients. Price effects are positive throughout the ten years following passage, become statistically significant beginning around year five, peak at roughly 4% between years six and eight, and stabilize at around 3% by year ten. The absence of differential pre-trends near the boundary strengthens the causal interpretation. Relative to the municipality–year FE specification, these Boundary FE estimates more cleanly isolate the valuation of overrides from neighborhood amenities and peer effects, providing a more credible measure of households' willingness to pay for improved local services.

We assess the robustness of these findings in the appendix. Figures Figure A.15 and Figure A.16 present estimates excluding demographic controls and excluding both demographic and property controls, respectively. In both cases, the post-passage effects remain positive and grow over time, though the magnitude and statistical significance vary somewhat. Our preferred specification, which includes both demographic and property controls (Figure 4), offers the most credible isolation of the willingness-to-pay for public goods from potential confounders.

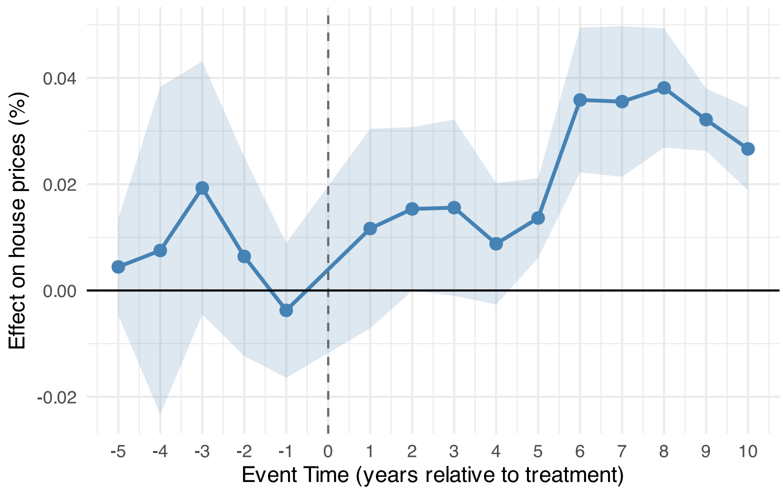

Figure 4: Effect of Overrides Approval on Housing Prices (BDD). Estimates and confidence intervals for the coefficients (β_k) in (1) including boundary-year fixed effects on top a municipality fixed effect. The boundaries are defined as neighborhoods spanning two municipalities. We focus on houses transacted within 500 m of the border. The dependent variable is the log of sale prices, and the unit of observation is the property–year. We control for housing characteristics, including residential area, lot size, number of rooms, and year of construction. Demographic controls include census tract or block-level shares of White and Black residents, as well as median income. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

6.3 Effect on Public Goods Quality

The evidence that housing prices rise following an override suggests that households value the improved public services these measures enable. For this to hold, however, overrides must actually lead to enhancements in local public goods. Overrides increase municipal revenues by raising property taxes, but whether these additional funds translate into better services depends on local government decisions. Mayer and Lopoo (2008) showed that municipalities that pass overrides tend to increase public spending, independently from the category of the override. Moreover, the large majority of overrides in Massachusetts are dedicated to funding public education, making it a key channel through which overrides can enhance local public goods.

To further validate this mechanism, we examine whether overrides lead to tangible improvements in local public goods, focusing on education as a key channel. We collect data on district per pupil spending on teacher salary and general district expenditures from the Massachusetts Department of Elementary and Secondary Education (MDESE) for the period 2009-2025. These metrics are critical inputs into educational production and directly affect the quality of schooling provided to students.

Figure 5 shows the event-study estimates for the effect of overrides on total district expenditures per pupil. The results indicate a substantial and immediate increase in spending following override passage. In the first year after an override, district expenditures per pupil rise by approximately 1.5%, with the effect growing to around 3.5% by year seven, then decreasing but remaining approximately 1.8% higher. This pattern suggests that overrides provide districts with additional resources that are quickly deployed to enhance educational spending.

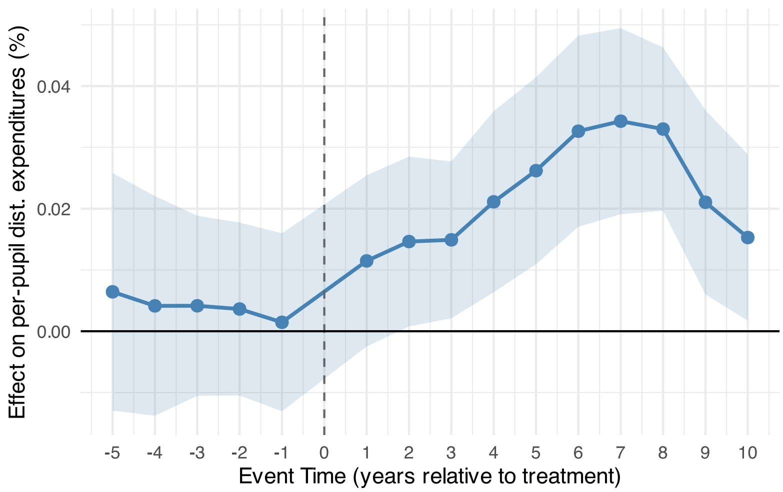

Figure 5: Effect of Overrides Approval on Per-Pupil District Expenditures. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the log of per-pupil district expenditures, and the unit of observation is the municipality–year. Controls include demographic and socioeconomic characteristics (share of White and Black pupils, share of low-income students, and share of non-English mother tongue). We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

Figure 6 displays the dynamic impact of a successful override on per pul teacher expenditures. The estimates indicate a sharp and sustained increase in salaries following override approval. In the first five years after the override passage, per pupil expenditures in treated municipalities increase up to 3.5% even if in year one and four the effect is not statistically significant. The effect then goes down to 2% in year ten but remains consistently significant in the last five years.[6]

Crucially, the coefficients in the pre-treatment period are small and statistically indistinguishable from zero, providing empirical support for the parallel trends assumption underlying our event-study framework. These findings indicate that override passage provides school districts

[6]An anonymized teacher's account describing the impact of receiving an override on their salary appears consistent with our findings: https://www.facebook.com/groups/732617870648037/posts/1809977959578684/

with resources to significantly increase teacher compensation and, consequently, enhance the provision of local public goods, particularly education.

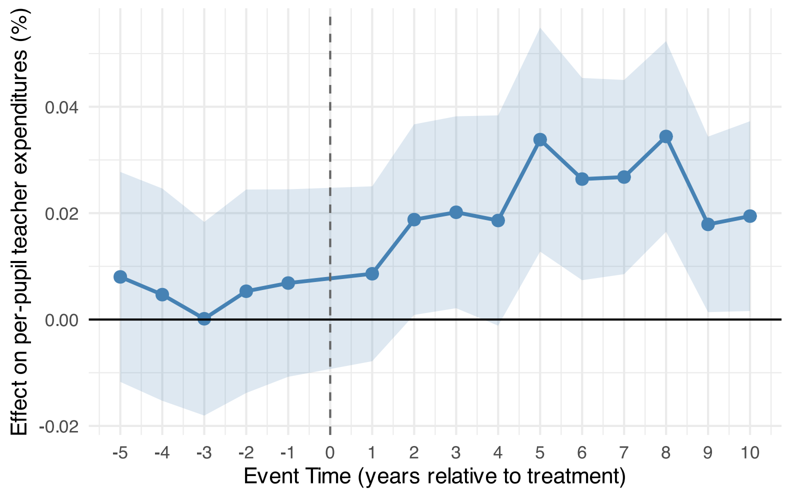

Figure 6: Effect of Overrides Approval on Per-Pupil Teacher Expenditures. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the log of per-pupil teacher expenditures, and the unit of observation is the municipality–year. Controls include demographic and socioeconomic characteristics (share of White and Black pupils, share of low-income students, and share of non-English mother tongue). We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

Figure 7 shows the cumulative effect of overrides on per-pupil expenditures for districts (left) and teachers (right). The two figures show a very similar pattern. The cumulative effect grows steadily over time, reaching about 25% by year 10, and is always significant from, respectively, year three and five onward. This pattern suggests that the additional resources provided by overrides are sustained and accumulate over time, enabling districts to make longer-term investments in educational quality. For teacher expenditures, the cumulative effect exhibits a very similar pattern. This indicates that overrides lead to sustained increases in teacher compensation, which can enhance teacher quality and retention, ultimately benefiting student outcomes.

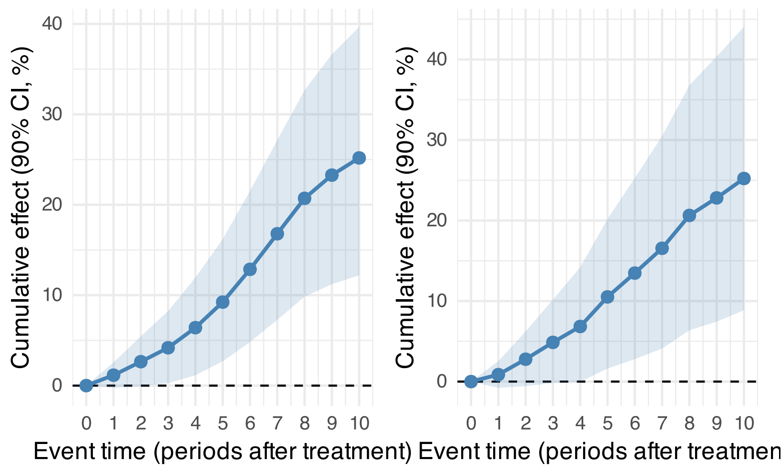

Figure 7: Cumulative Effect of Overrides Approval on Per-Pupil Teacher Expenditures. Cumulative estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the log of per-pupil district expenditures, and the unit of observation is the municipality–year. Controls include demographic and socioeconomic characteristics (share of White and Black pupils, share of low-income students, and share of non-English mother tongue). We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

6.4 Effect on Sorting and Segregation

We have shown that overrides, by expanding the provision of local public services, raise property values. Because rising housing prices tend to favor higher-income households, these gains are unlikely to be evenly distributed. To probe this mechanism, we examine the effect of override passage on the socioeconomic composition of local public schools. Specifically, we use the annual share of low-income students enrolled in public schools as a proxy for socioeconomic segregation, since this measure captures the extent to which disadvantaged households are concentrated in, or excluded from, municipalities that successfully pass overrides.

The event study estimates in Figure Figure 8 show a clear downward trajectory in the share of low-income pupils following override approval. Within six years of passage, the share falls by about one percentage point, and the effect persists over time. Taken together with the property price results (Figure 3 and Figure 4), this evidence suggests that overrides not only increase the value of housing stock but also alter the demographic composition of communities. In particular, the sorting of wealthier households into override municipalities means that the benefits of higher public spending, such as improved school quality and local amenities, are disproportionately captured by more advantaged families.

This dynamic highlights a potential unintended consequence of fiscal limits and their override provisions: while they generate efficiency gains by aligning spending with household willingness-to-pay, they may simultaneously exacerbate socioeconomic segregation across municipalities. Such stratification risks reinforcing existing inequalities in access to public goods and may, over the long run, dampen intergenerational mobility.

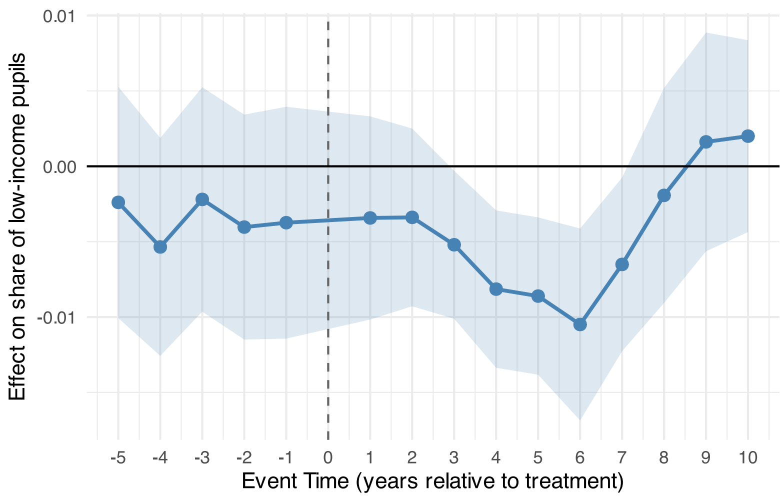

Figure 8: Effect of Overrides Approval on Share of Low-Income Pupils Enrollment Share. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the share of low-income pupil enrolling in school s at time t. Controls include demographic and socioeconomic characteristics (share of White and Black pupils, and share of non-English mother tongue). We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

6.5 Quantification of the WTP for public goods

To benchmark the capitalization of local fiscal decisions in the housing market, we compute an implied willingness-to-pay (WTP) per dollar of override-funded spending, closely following the methodology of Cellini, Ferreira, and Rothstein (2010). The intuition is straightforward: if housing prices capitalize the benefits of additional local public goods, then the observed price increase following an override vote reflects the discounted value of residents' willingness to pay for the enhanced services. At the same time, homeowners bear higher property taxes to service the override. Combining the two components provides a measure of net WTP. Normalizing by the per capita spending increment yields a statistic that can be directly compared across contexts.

Our calculation proceeds in three steps. First, we measure the housing price capitalization effect. The regression discontinuity estimates imply that passage of an override raises home values by approximately 4 percent. With a representative median house valued at \$390,000, this corresponds to a capitalization effect of about \$15,600.

Second, we compute the present discounted value of additional property tax liabilities. From the assessor data we know that the median municipality has a tax base of \$13.06 billion. For a representative override of \$1 million, the implied tax rate increment is $\tau = 0.001/13.06 \approx 0.0000766$. Applied to the median home, this yields an additional annual tax burden of about \$29.9. Under a perpetuity assumption with a 3 percent discount rate, the present discounted value of tax costs is $\$29.9/0.03 \approx \$997$ per home. Adding this capitalization effect to the tax cost gives a total WTP of approximately \$16,597.

Third, we normalize by the present value of the spending stream funded by the override. The average annual per capita increment is \$98.38, calculated as the median override amount divided by the median municipal population in Massachusetts. Treating this as a perpetuity at a 3% discount rate yields a present value of \$3,279 per person. With an average of 2.5 residents per housing unit, the denominator per home is thus \$8,198. Dividing the total WTP (\$16,597) by this denominator yields an implied WTP of roughly \$2.00 per dollar of override spending per capita.

This ratio exceeds the corresponding estimates in Cellini, Ferreira, and Rothstein (2010), who report a WTP of roughly \$1.44 per dollar of bond-financed school spending. The divergence reflects several differences. First, our overrides fund not only education but also other local public goods, potentially generating broader capitalization. Second, our denominator measures per person spending, whereas Cellini, Ferreira, and Rothstein (2010) normalize by per pupil outlays, which are mechanically larger. Finally, the infinite-horizon assumption enlarges the denominator considerably, compressing the ratio. Nevertheless, the basic pattern is clear: homeowners appear willing to pay multiple dollars in property value for each dollar of locally financed public spending, as revealed through housing market capitalization.

7 Conclusions and Discussion

This paper provides new causal evidence on how households value improvements in local public goods. Exploiting quasi-experimental variation generated by Proposition $2^{1/2}$ overrides in Massachusetts, I show that voter-approved increases in municipal revenues lead to sustained rises in housing prices, consistent with higher willingness to pay (WTP) for enhanced local services. The capitalization effect, which grows gradually and stabilizes around 3–4 percent within a decade after passage, exceeds the additional tax burden implied by the override. This pattern suggests that the marginal dollar of spending financed through overrides yields positive net benefits for residents. Moreover, the timing of the effect—emerging several years after passage—indicates that households internalize not only the immediate service improvements but also the credibility and persistence of fiscal capacity signaled by successful overrides.

Beyond documenting this positive valuation, the analysis highlights how institutional constraints on local taxation shape both efficiency and equity in public service provision. The findings imply that binding tax and expenditure limitations (TELs) can lead to underprovision of welfare-enhancing local goods, particularly in municipalities unable to secure voter approval for overrides. Because fiscal capacity and political participation correlate strongly with income and education, these mechanisms risk reinforcing spatial inequality. Wealthier communities, better equipped to pass overrides, can sustain higher-quality public services, while poorer jurisdictions remain fiscally constrained. This dynamic contributes to persistent disparities in local amenities, school quality, and, ultimately, residential sorting patterns.

These results have broader implications for fiscal design and regional development policy. They suggest that local public finance and place-based policy are deeply intertwined: fiscal autonomy without compensatory mechanisms can amplify territorial disparities in service quality and opportunity. Well-designed place-based interventions, such as targeted capital grants, conditional transfers, or matching schemes favoring fiscally constrained municipalities, could complement decentralized tax systems by offsetting differences in local capacity to invest. Rather than substituting for local initiative, such instruments can enhance allocative efficiency by enabling high-return investments that poorer jurisdictions would otherwise forgo. In this sense, integrating local fiscal rules with spatially aware redistribution mechanisms may be essential for achieving both efficiency and cohesion objectives.

Ultimately, the evidence underscores that the institutional design of local public finance is not merely a question of efficiency, but a determinant of who benefits from public spending and where opportunity is created. Policies that recognize the geographic dimension of fiscal inequality —combining local accountability with place-sensitive redistribution—can help ensure that the benefits of public investment are more evenly shared across communities. Understanding how fiscal rules, voter behavior, and local capacity interact is thus crucial for designing equitable and growth-enhancing systems of multilevel governance.

Appendix

A Data Cleaning

We build our dataset by merging multiple data sources.

Overrides Data: Data on Proposition $2^{1/2}$ overrides are obtained from the division of Local Services of the Massachussets Department of Revenue.[7] As mentioned in Section 4, we retain data from 1996, 2001, or 2009 depending on the specification. The treatment year is always equal to the fiscal year of the passage. We keep one override per municipality-year. In case of multiple overrides, we keep information on the winning one. If a municipality passes multiple overrides, we keep the one with the largest amount. If a municipality held multiple overrides without passing them, we keep the largest failed override.

Real Estate Transaction and Assessor Data Property transaction data for the period 2001 - 2023 come from the Department of Revenus.[8] For each transacted property, the data report the price of the transaction, the building use code (whether it is a residential or commercial unit, etc.), the property type (e.g., single-family, condo, etc.) the owner and seller full names, the address of the property, and latitude/longitude. We first drop all non-residential units and keep only single-family houses. Then we use latitude and longitude to geolocalize properties at both the municipality level and at the census-block/tract level.

The real estate transaction data do not report important property characteristics that affect sale price (e.g., the residential sqft, the number of rooms, the lot size and the year of construction, etc.). To obtain this information, we use Assessor data collected yearly by county Assessors. The Department of Revenus harmonizes such data and make them publicly available here.[9] These data, collected per property tax reasons, report many structural characteristics on the property. We therefore merge the assessor data with the real estate transaction data. To merge the maximum number of properties we adopt a "fuzzy" approach.

1. We first keep properties for which a full match using the property ID variable is found.

[7]Data can be accessed here: https://www.mass.gov/orgs/division-of-local-services.
[8]Data can be accessed here: https://dls-gw.dor.state.ma.us/gateway/dlspublic/parcelsearch.
[9]Data can be accessed here: https://www.mass.gov/info-details/massgis-data-property-tax-parcels.

2. We remove unusual characters from the property ID and we merge them by this "clean" ID, city, street name and street number.
3. We further remove all blank spaces in the "clean" ID (as we notice there were different number of spaces for the same property, some times) and we merge again by the "clean" ID, city, street name and street number.
4. We merge by city, street name, street number and residential area (sqft).
5. We merge by city, street name, street number and latitude and longitude.
6. We merge by city, street name and the "clean" ID.
7. We merge city, street number and latitude and longitude.
8. Finally, we merge by city and "clean" ID.

At the end of this procedure we drop all duplicates after each step and we drop all non-merged transacted properties. We are able to merge around 85% of the original transaction data.

To build the dataset for our BDD approach we first draw a buffer of 500 meters from each border of any municipalities i. We then keep the properties that fall within this buffer inside municipality i. Then we repeat the same procedure for all municipalities. While doing so, we keep track of the names of the closest municipality for each property. In this way we can create the boundary variable that identifies the boundary segment between two municipalities.

Geographic and Census Data Municipal shapefiles are obtained from the MASSGIS repository[10], which reports boundaries for Massachusetts municipalities in year 2024. Census block and tract shapefiles are from the U.S. Census Bureau (2020). We merge these with socioeconomic characteristics from the decennial census and the American Community Survey (ACS). For years prior to 2005 we use the 2000 Census; for later years we use ACS five-year estimates. Block-level information is preferred, but tract-level data are used when block-level data are unavailable in order to maximize sample size.

School enrollment data Enrollment data come from the Massachusetts Department of Elementary and Secondary Education (MDESE)[11] and are reported at the school level. This information cover the period 1996-2025. Municipalities that are also school districts are matched

[10]Data can be accessed here: https://www.mass.gov/info-details/massgis-data-municipalities.

[11]Data can be accessed here: https://educationtocareer.data.mass.gov/Students-and-Teachers/Enrollment-Grade-Race-Ethnicity-Gender-and-Selecte/t8td-gens/about_data.

directly. For others, we link schools to overrides by assuming that a school is affected if any municipality served by its district passes an override.

School expenditure data Expenditure data come from the Massachusetts Department of Elementary and Secondary Education (MDESE)[12] and are reported at the school-district level. These data cover the priod 2009-2025. We again assign overrides at the district level by assuming that a district is treated if at least one municipality it serves passes an override.

B Supplemental Figures

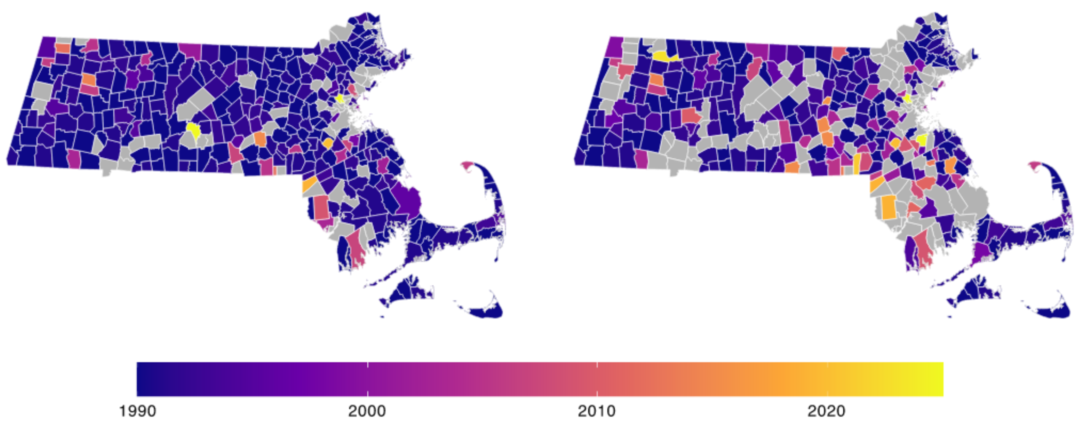

Figure A.1: First Override Proposals and Approvals in Massachusetts Municipalities (1990-2025). The figure plots the year of the first proposed (left) and passed (right) override in each municipality in Massachusetts, for the period 1990-2025. Municipalities that never proposed or passed an override are colored in grey.

[12]Data can be accessed here: https://educationtocareer.data.mass.gov/Finance-and-Budget/District-Expenditures-by-Spending-Category/er3w-dyti/about_data.

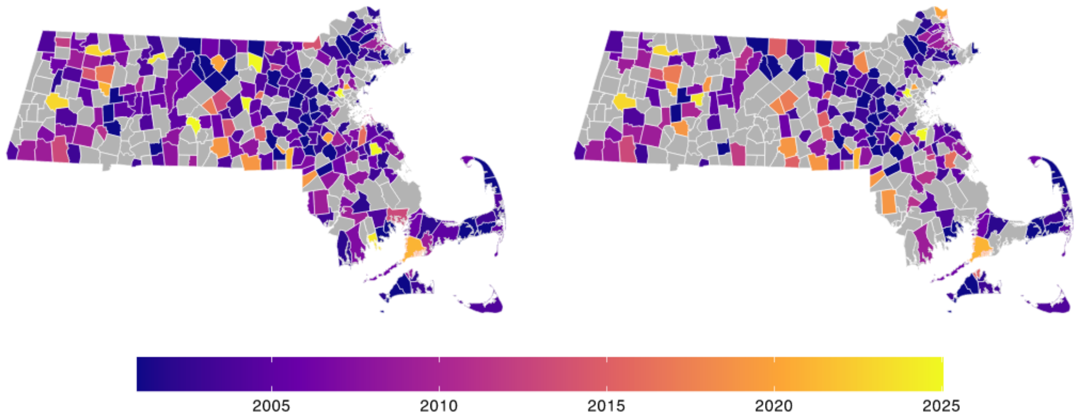

Figure A.2: First Override Proposals and Approvals in Massachusetts Municipalities (2001-2025). The figure plots the year of the first proposed (left) and passed (right) override in each municipality in Massachusetts, for the period 2001-2025. Municipalities that never proposed or passed an override are colored in grey.

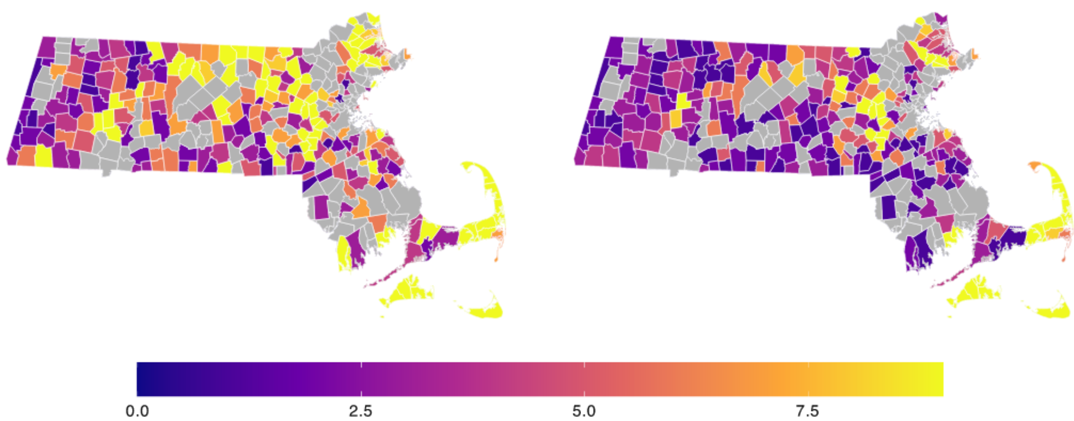

Figure A.3: Total Override Proposals and Approvals in Massachusetts Municipalities (1990-2025). The figure plots the total number of proposed (left) and passed (right) overrides in each municipality in Massachusetts, for the period 1990-2025. Municipalities that never proposed or passed an override are colored in grey.

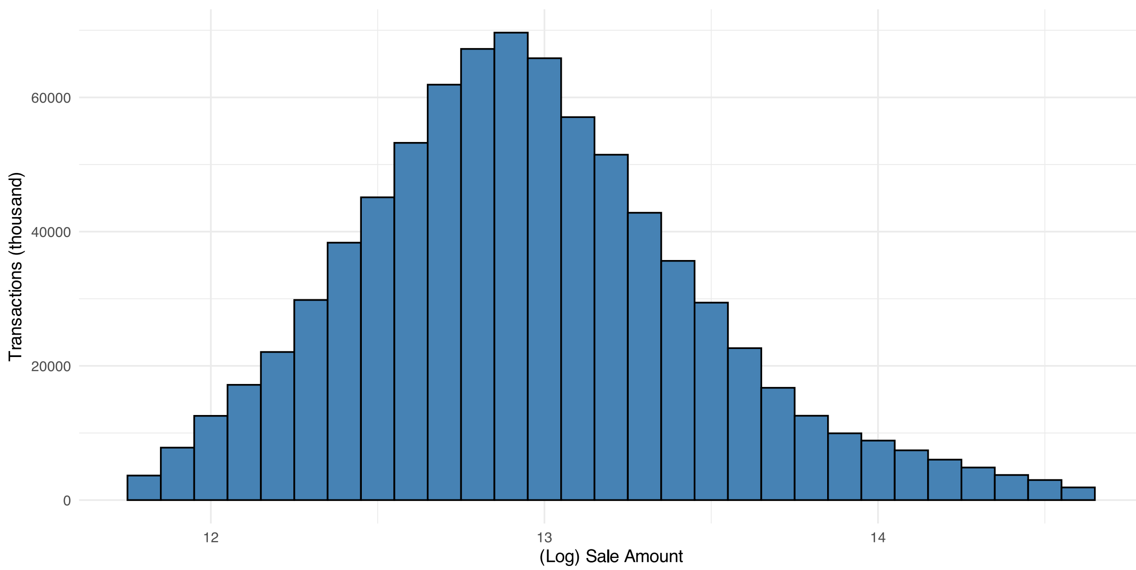

Figure A.4: **Histogram of Log Sale Amount (Full Sample)**. The x-axis shows the log of sale amount, and the y-axis shows the frequency.

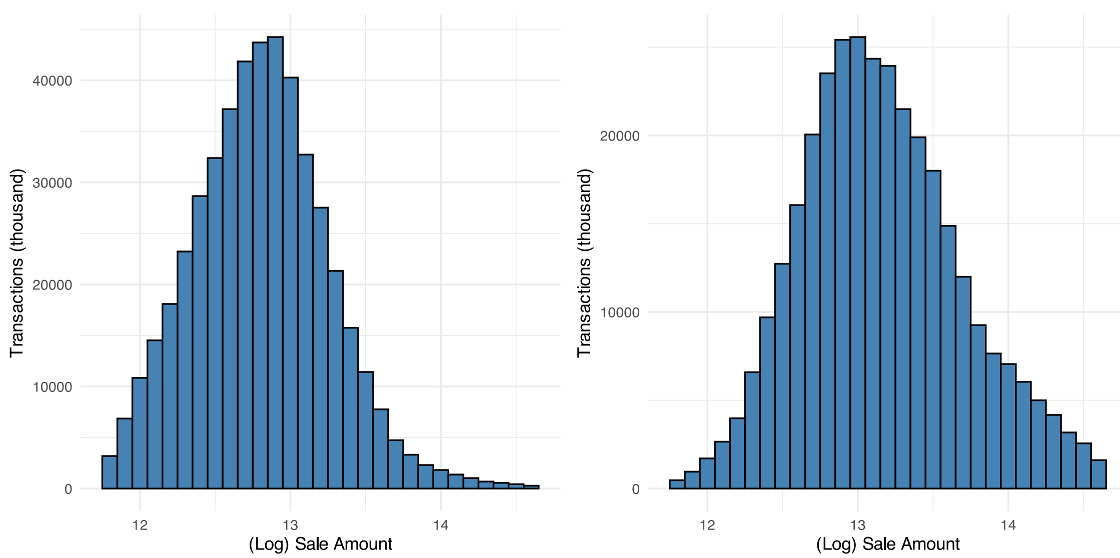

Figure A.5: **Histogram of Log Sale Amount**. Two panels show the histogram of log sale amount separately for municipalities with no override wins (left) and with at least one override win (right). The x-axis shows the log of sale amount, and the y-axis shows the frequency.

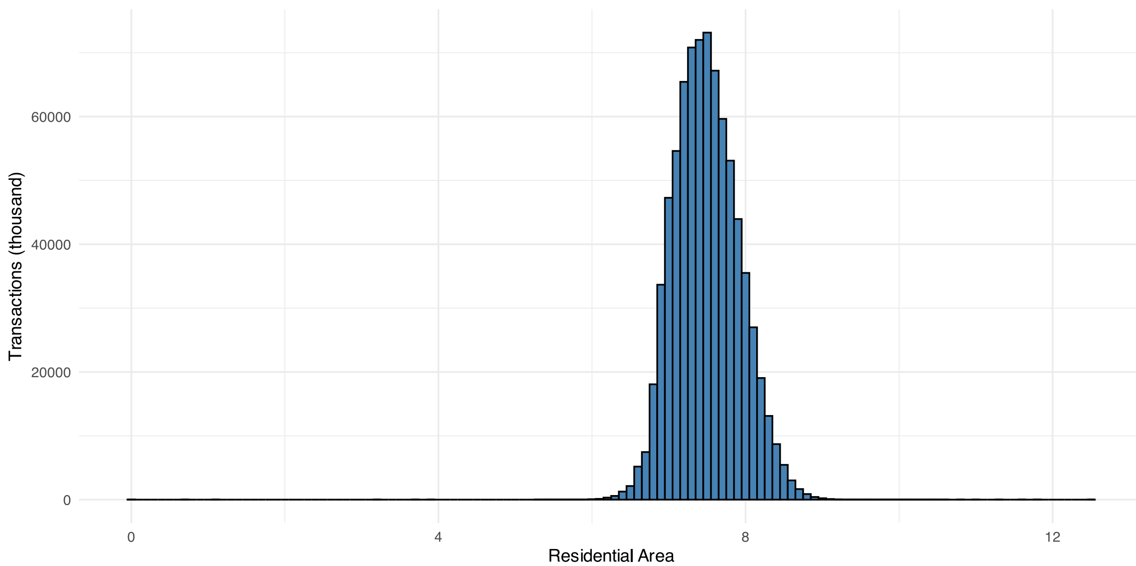

Figure A.6: **Histogram of Residential Area (Full Sample)**. The x-axis shows the log of residential area, and the y-axis shows the frequency.

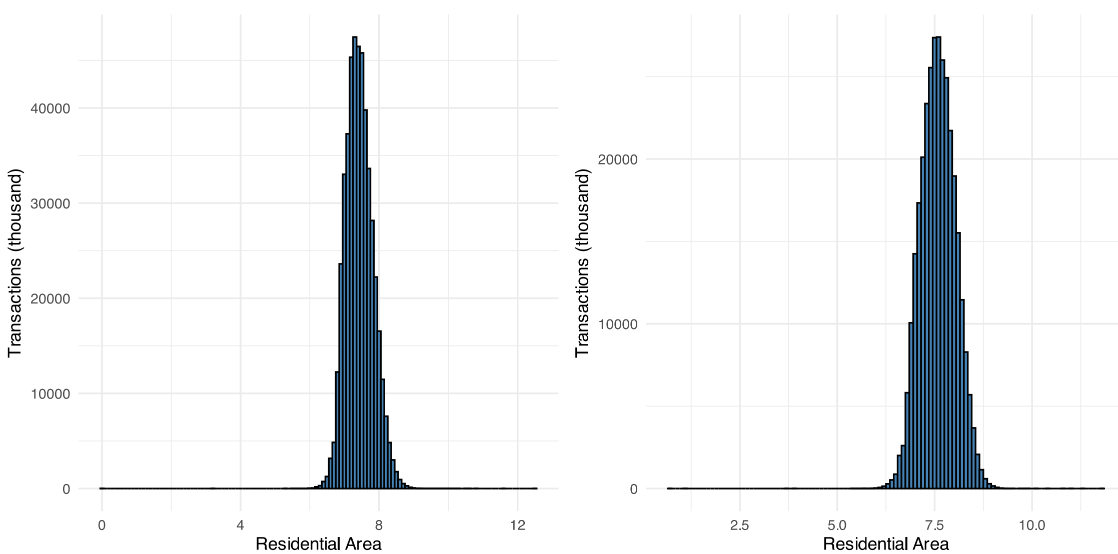

Figure A.7: **Histogram of Residential Area**. Two panels show the histogram of log residential area separately for municipalities with no override wins (left) and with at least one override win (right). The x-axis shows the log of residential area, and the y-axis shows the frequency.

C Supplemental Results

C.1 Event Study Tables

Table A.1: Effect of Overrides Approval on Housing Prices (Figure 3)

Avg effect over:	(1)	(2)	(3)
years 1–3	−0.009	−0.004	0.005
	(0.007)	(0.006)	(0.008)
years 4–6	−0.001	0.003	0.010 *
	(0.005)	(0.005)	(0.006)
years 7–10	0.011 **	0.013 ***	0.021 ***
	(0.005)	(0.005)	(0.006)
Dem. Controls			X
Prop. Controls		X	X
Mun + Year FE	X	X	X
Adj. R^2	0.609	0.768	0.788
Observations	808,452	808,452	627,857

Note: Estimates and standard errors of linear combinations of the parameters $b\eta_k$ in (1) . The dependent variable is the log of the sale price. Column (1) shows the baseline specification without any controls. Column (2) includes only property controls, such as residential area, lot size, number of rooms, and year of construction. Column (3) adds both property and demographic controls at the census-block or tract depending on data availability. Standard errors are computed using the delta method and clustered at the municipality level. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage. * = 0.1; ** = 0.05; *** = 0.01.

Table A.2: Effect of Overrides Approval on Housing Prices - BDD Apprach (**?@fig-estudy_housing_bfe_3**)

Avg effect over:	(1)	(2)	(3)
years 1−3	−0.004	0.005	0.014 *
	(0.008)	(0.006)	(0.009)
years 4−6	0.000	0.010 ***	0.019 ***
	(0.007)	(0.004)	(0.005)
years 7−10	0.019 ***	0.021 ***	0.033 ***
	(0.006)	(0.003)	(0.005)
Dem. Controls			X
Prop. Controls		X	X
Boundary-Year FE	X	X	X
Mun FE	X	X	X
Adj. R^2	0.609	0.788	0.798
Observations	236,205	236,205	184,971

Note: Estimates and standard errors of linear combinations of the parameters $b\eta_k$ in (1) augmented via a BDD approach. The dependent variable is the log of the sale price. Column (1) shows the baseline specification without any controls. Column (2) includes only property controls, such as residential area, lot size, number of rooms, and year of construction. Column (3) adds both property and demographic controls at the census-block or tract depending on data availability. Standard errors are computed using the delta method and clustered at the municipality level. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage. * = 0.1; ** = 0.05; *** = 0.01.

Table A.3: Effect of Overrides Approval on Per-Pupil District and Teacher Expenditures (Figure 5 and Figure 6)

Avg effect over:	(1)	(2)	(3)	(4)	(5)	(6)
years 1−3	0.010	0.012	0.014 *	0.016	0.015	0.016
	(0.009)	(0.008)	(0.008)	(0.011)	(0.010)	(0.010)
years 4−6	0.024 ***	0.024 ***	0.027 ***	0.027 **	0.025 **	0.026 **
	(0.009)	(0.009)	(0.009)	(0.012)	(0.011)	(0.011)
years 7−10	0.026 ***	0.024 ***	0.026 ***	0.026 ***	0.024 ***	0.025 ***
	(0.008)	(0.008)	(0.007)	(0.009)	(0.009)	(0.009)
Dem. Controls		X	X		X	X
Econ. Controls			X			X
Mun + Year FE	X	X	X	X	X	X
Adj. R^2	0.932	0.934	0.936	0.862	0.869	0.872
Observations	5,141	5,141	5,141	5,141	5,141	5,141

Note: Estimates and standard errors of linear combinations of the parameters $b\eta_k$ in (1) . The dependent variable is the log of the per-pupil district expenditures (Columns (1), (2), (3)) and log of the per-pupil teacher expenditures (Columns (4), (5), (6)). Columns (1) and (4) show the baseline specification without any controls. Columns (2) and (5) include only ethnic composition controls, measured by the shares of White and Black pupils enrolled in year t. Columns (3) and (6) add both ethnic composition and socioeconomic controls, including the shares of low-income students and students with a non-English mother tongue. Standard errors are computed using the delta method and clustered at the municipality level. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage. * = 0.1; ** = 0.05; *** = 0.01.

Table A.4: Effect of Overrides Approval on Low-Income Pupil Enrollment Share (Figure 8)

Avg effect over:	(1)	(2)	(3)
years 1−3	−0.001	−0.004	−0.004
	(0.004)	(0.003)	(0.003)
years 4−6	−0.010 **	−0.010 ***	−0.009 ***
	(0.004)	(0.003)	(0.003)
years 7−10	−0.003	−0.002	−0.001
	(0.003)	(0.003)	(0.004)
Dem. Controls		X	X
Econ. Controls			X
Mun + Year FE	X	X	X
Adj. R^2	0.844	0.889	0.896
Observations	58,259	58,102	55,169

Note: Estimates and standard errors of linear combinations of the parameters $b\eta_k$ in (1) . The dependent variable is the log of low-income pupils (identified as those requesting FRPL). Column (1) shows the baseline specification without any controls. Column (2) includes only ethnic composition controls, measured by the shares of White and Black pupils enrolled in year t. Column (3) add both ethnic composition and socioeconomic controls, including the shares of low-income students and students with a non-English mother tongue. Standard errors are computed using the delta method and clustered at the municipality level. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage. * = 0.1; ** = 0.05; *** = 0.01.

C.2 Robustness Checks

C.2.1 Excluding Future Overrides History

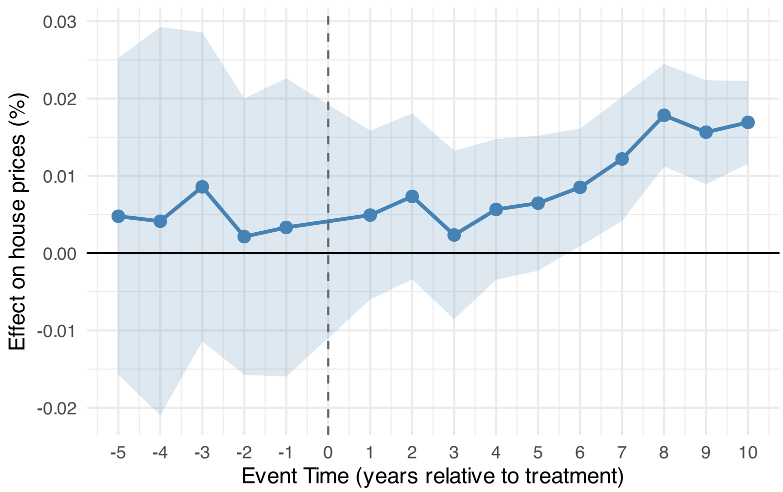

Figure A.8: Effect of Overrides Approval on Housing Prices. Estimates and confidence intervals for the coefficients (β_k) in (1) without controlling for the future history of overrides. The dependent variable is the log of sale prices, and the unit of observation is the property–year. We control for housing characteristics, including residential area, lot size, number of rooms, and year of construction. Demographic controls include census tract or block-level shares of White and Black residents, and median income. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

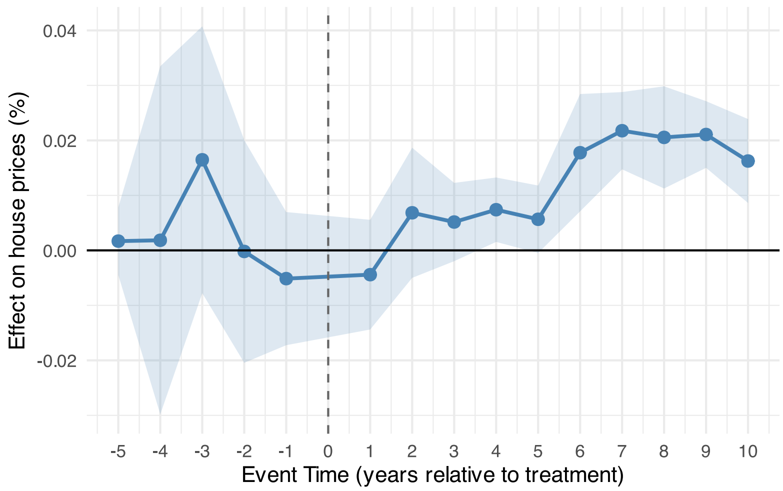

Figure A.9: Effect of Overrides Approval on Housing Prices (BDD). Estimates and confidence intervals for the coefficients (β_k) in (1) without controlling for the future history of overrides but including boundary-year fixed effects on top a municipality fixed effect. The boundaries are defined as neighborhoods spanning two municipalities. We focus on houses transacted within 500 m of the border. The dependent variable is the log of sale prices, and the unit of observation is the property–year. We control for housing characteristics, including residential area, lot size, number of rooms, and year of construction. Demographic controls include census tract or block-level shares of White and Black residents, as well as median income. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

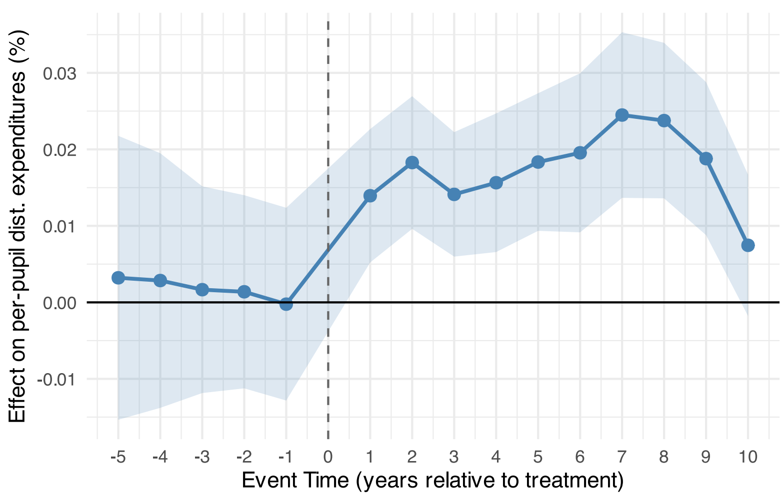

Figure A.10: Effect of Overrides Approval on Per-Pupil District Expenditures. Estimates and confidence intervals for the coefficients (β_k) in (1) without controlling for the future history. The dependent variable is the log of per-pupil district expenditures, and the unit of observation is the municipality–year. Controls include demographic and socioeconomic characteristics (share of White and Black pupils, share of low-income students, and share of non-English mother tongue). We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

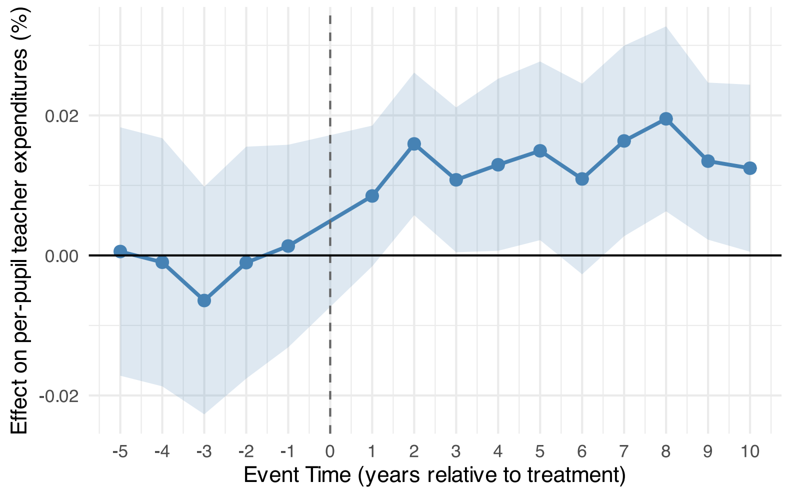

Figure A.11: Effect of Overrides Approval on Per-Pupil Teacher Expenditures. Estimates and confidence intervals for the coefficients (β_k) in (1) without controlling for the future history. The dependent variable is the log of per-pupil teacher expenditures, and the unit of observation is the municipality–year. Controls include demographic and socioeconomic characteristics (share of White and Black pupils, share of low-income students, and share of non-English mother tongue). We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

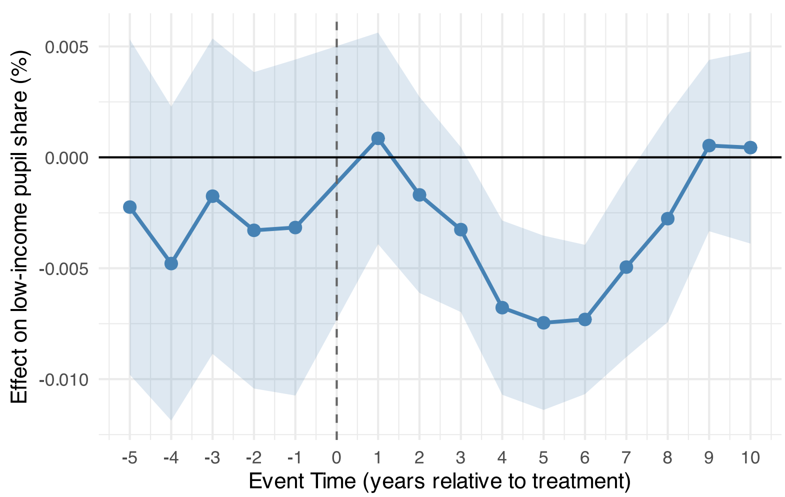

Figure A.12: Effect of Overrides Approval on Share of Low-Income Pupils Enrollment Share. Estimates and confidence intervals for the coefficients (β_k) in (1) without controlling for the future history.. The dependent variable is the share of low-income pupil enrolling in school s at time t. Controls include demographic and socioeconomic characteristics (share of White and Black pupils, and share of non-English mother tongue). We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

C.2.2 Excluding Covariates and HPI Index

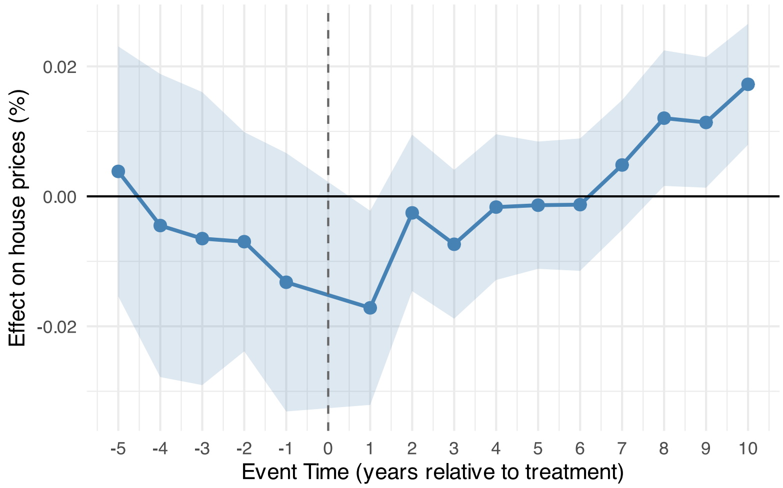

Figure A.13: Effect of Overrides Approval on Housing Prices. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the log of sale prices, and the unit of observation is the property–year. We do not include any controls in this specification. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

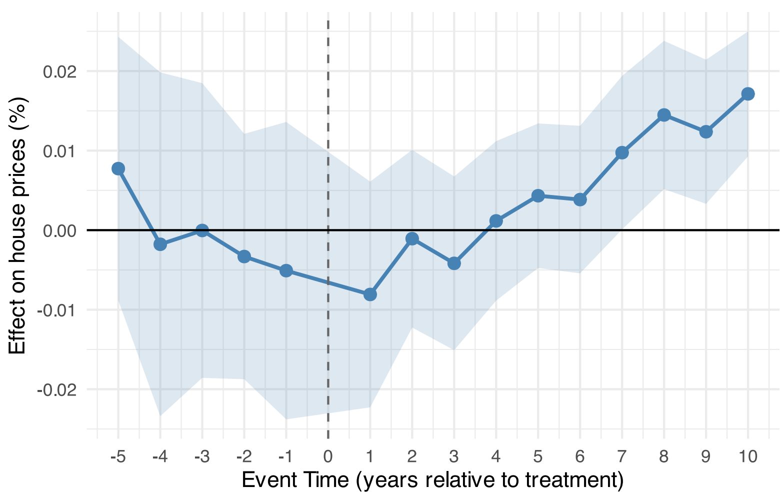

Figure A.14: Effect of Overrides Approval on Housing Prices. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the log of sale prices, and the unit of observation is the property–year. We include only property controls: residential area, lot area, number of rooms and year of construction. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

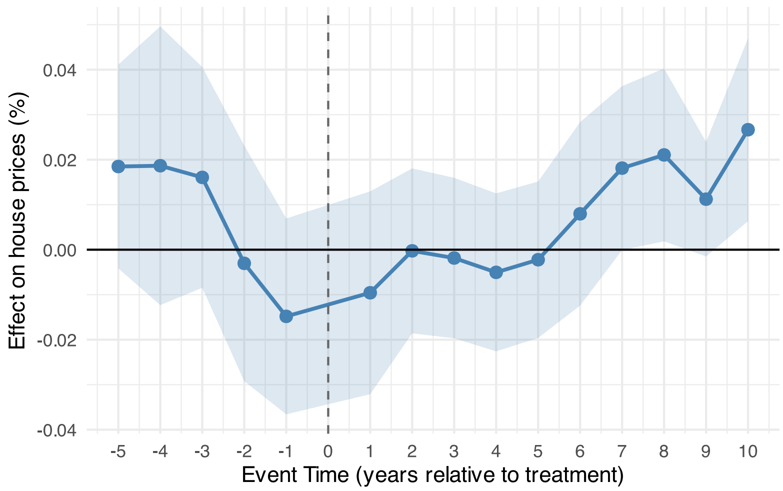

Figure A.15: Effect of Overrides Approval on Housing Prices (BDD). Estimates and confidence intervals for the coefficients (β_k) in (1) including boundary-year fixed effects on top a municipality fixed effect. The boundaries are defined as neighborhoods spanning two municipalities. We focus on houses transacted within 500 m of the border. The dependent variable is the log of sale prices, and the unit of observation is the property–year. We do not include any controls in this specification. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

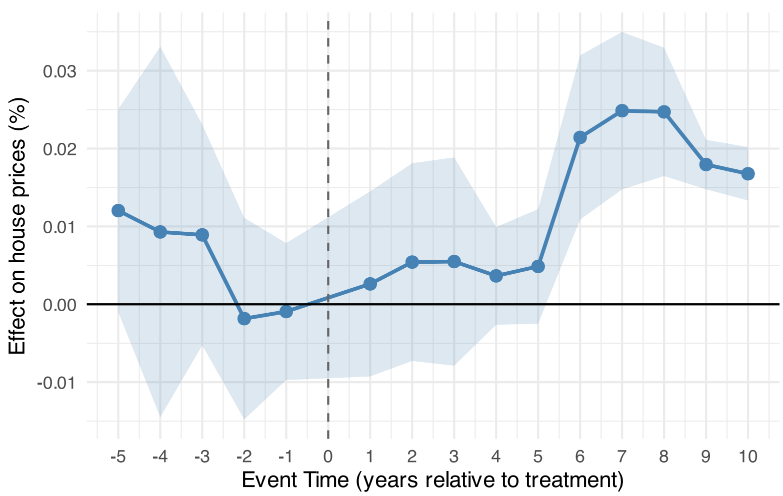

Figure A.16: Effect of Overrides Approval on Housing Prices (BDD). Estimates and confidence intervals for the coefficients (β_k) in (1) including boundary-year fixed effects on top a municipality fixed effect. The boundaries are defined as neighborhoods spanning two municipalities. We focus on houses transacted within 500 m of the border. The dependent variable is the log of sale prices, and the unit of observation is the property–year. We control for housing characteristics, such as residential area, lot size, number of rooms, and year of construction. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

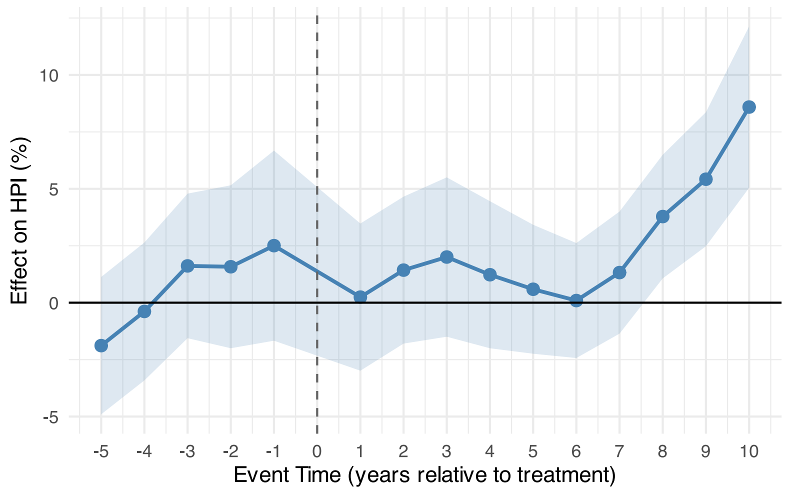

Figure A.17: Effect of Overrides Approval on Housing Price Index (FHFA). Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the FHFA index aggregate at the municipality-year. The specification does not include any controls. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

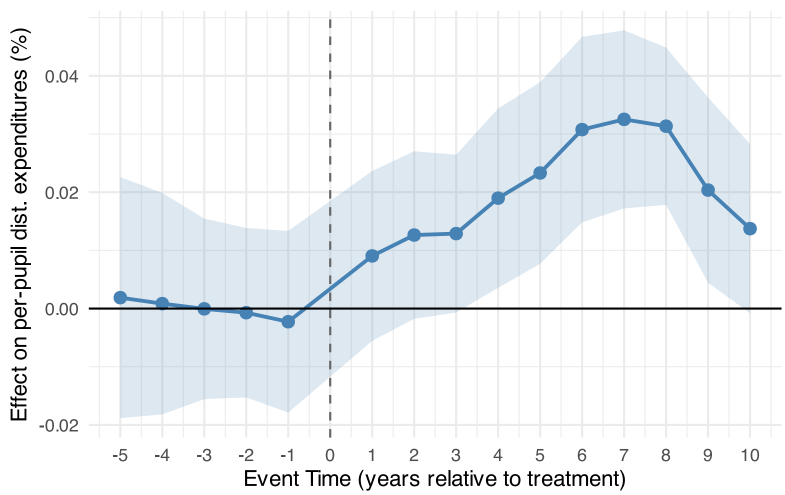

Figure A.18: Effect of Overrides Approval on Per-Pupil District Expenditures. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the log of per-pupil district expenditures, and the unit of observation is the municipality–year. We do not include any controls in this specification. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

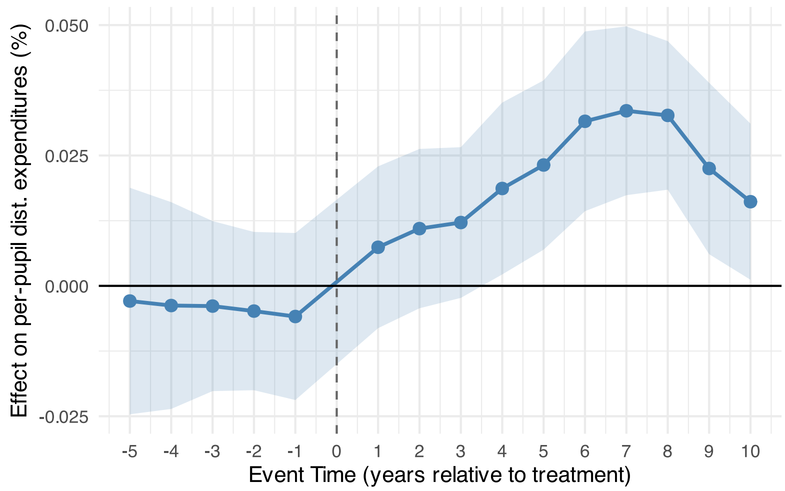

Figure A.19: Effect of Overrides Approval on Per-Pupil District Expenditures. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the log of per-pupil district expenditures, and the unit of observation is the municipality–year. We include only socio-economic covariates, such as the share of low-income students and the share of non-English mother tongue. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

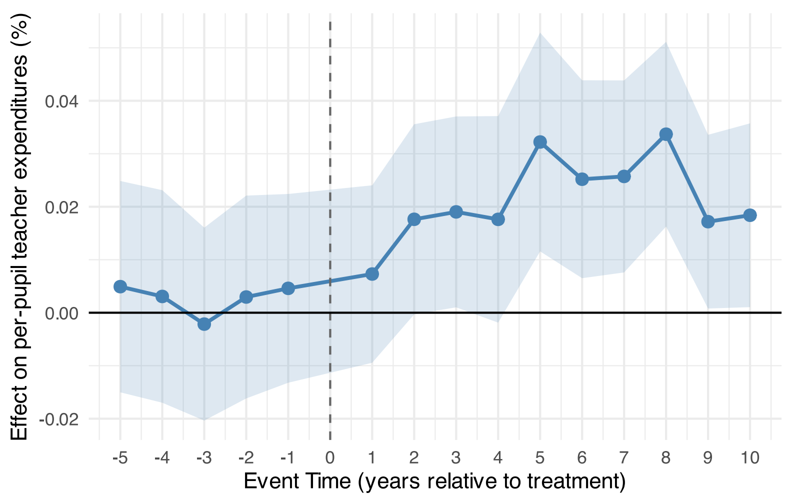

Figure A.20: Effect of Overrides Approval on Per-Pupil Teacher Expenditures. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the log of per-pupil teacher expenditures, and the unit of observation is the municipality–year. We do not include any controls in this specification. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

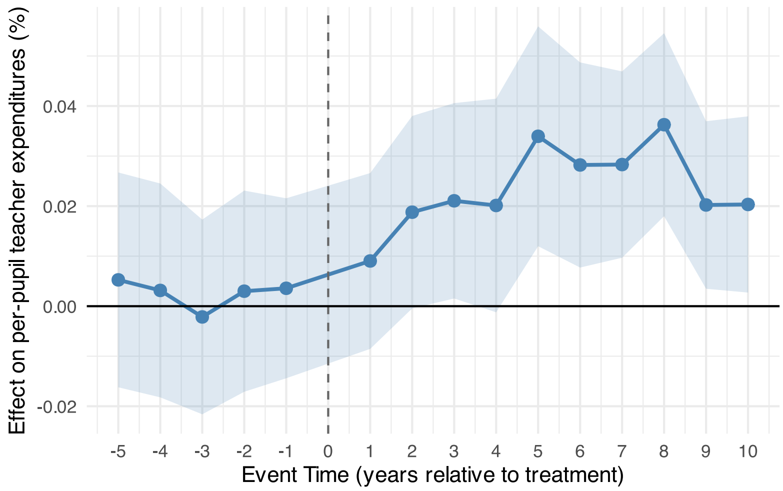

Figure A.21: Effect of Overrides Approval on Per-Pupil Teacher Expenditures. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the log of per-pupil teacher expenditures, and the unit of observation is the municipality–year. We include only socio-economic covariates, such as the share of low-income students and the share of non-English mother tongue. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, with 0 corresponding to the year of passage.

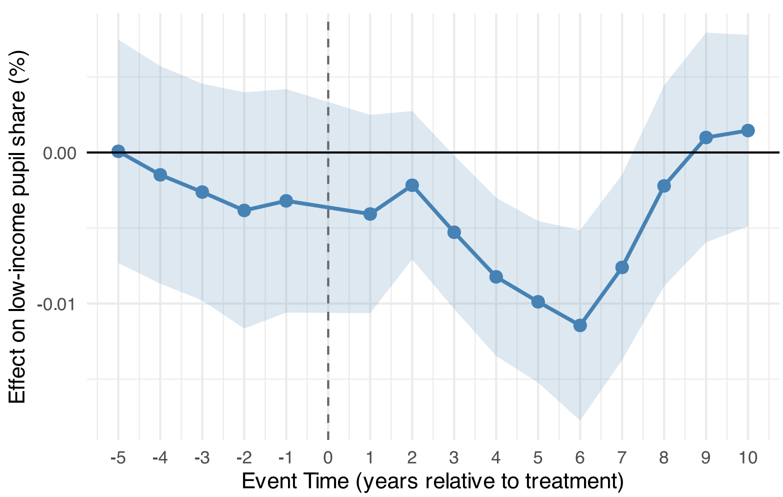

Figure A.22: Effect of Overrides Approval on Share of Low-Income Pupils Enrollment Share. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the share of low-income pupil enrolling in school s at time t. We do not include any controls in this specification. We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

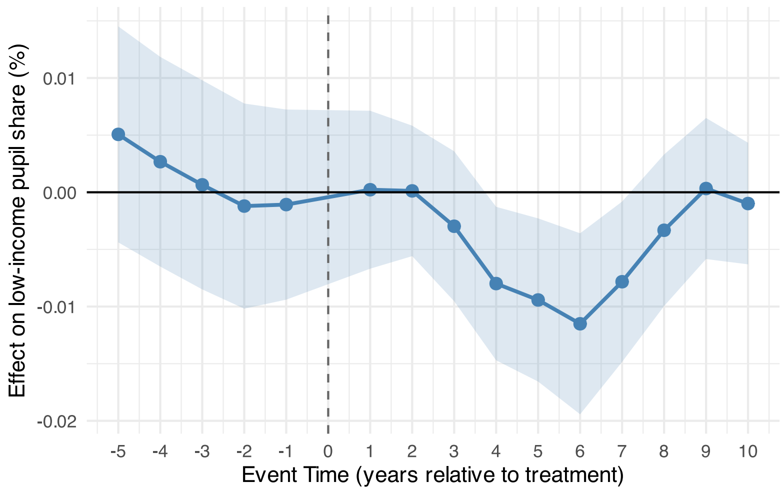

Figure A.23: Effect of Overrides Approval on Share of Low-Income Pupils Enrollment Share. Estimates and confidence intervals for the coefficients (β_k) in (1) . The dependent variable is the share of low-income pupil enrolling in school s at time t. Controls include pupils ethnic composition (share of White and Black pupils). We include municipality and year fixed effects. Standard errors are clustered at the municipality level. Coefficients (β_k) from equation (1) are plotted with 95% confidence intervals. The event time variable denotes years relative to override passage, where 0 corresponds to the year of passage.

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