UNINTENDED CONSEQUENCES: THE IMPACT OF PROPOSITION $2\frac{1}{2}$ OVERRIDES ON SCHOOL SEGREGATION IN MASSACHUSETTS

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Abstract
I investigate a possible unintended consequence of Proposition $2\frac{1}{2}$ override behavior—that it led to increased segregation in school districts in Massachusetts. This can occur because richer, low-minority towns tend to have more successful override votes that attract similar households with relatively high demands for public services who can afford to pay for them. To evaluate this hypothesis, I collect panel data on override behavior from 1982 to 2012 and merge this with data on school district enrollments and other district- and town-level characteristics. I find evidence that passing overrides earmarked for schools results in a significant decrease in the percent of nonwhite students enrolled in Massachusetts school districts. This happens in districts with below-average nonwhite school enrollments, and hence increases segregation.

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1. INTRODUCTION

A recent study by Northeastern University researchers found that public schools in Springfield (second place) and Boston (fourth place), Massachusetts, are some of the most segregated in the United States (McArdle, Osypuk, and Acevedo-García 2010). Although this study measured within-district segregation, there is evidence that most segregation is between school districts (Clotfelter 2004). As Rivkin (1994) points out, school segregation is driven by residential segregation because students are usually assigned to local schools. Furthermore, the relationship between school segregation and residential segregation increased between 2000 and 2010 (Frankenberg 2013).

Since Tiebout (1956), we understand that households "vote with their feet." That is, households will choose where to live based on the local public goods provided by jurisdictions. This results in local public goods provision being capitalized into house prices as households are willing to pay for these services by bidding up house prices in these towns. One of the most important local public goods is school quality. Because school quality is a normal good, households with higher incomes and those with school-aged children will likely reside in towns with better schools. Hence, it is not too surprising that minority households, which tend to have lower incomes, attend high-minority, high-poverty, and low-quality schools (McArdle, Osypuk, and Acevedo-García 2010).

For more than 50 years an important driver of the changing demographic makeup of towns in Massachusetts (as is the case for most major metropolitan areas) is the relocation of white households from central cities to the suburbs (Boustan and Margo 2013), so-called "white flight." Based on data for Massachusetts from the 2010 Decennial Census, Schworm and Caroll (2011) conclude that this has resulted in public school districts with a high percentage of minority students, particularly in low-income communities, "plagued by violent crime and struggling schools."

To provide some evidence about trends across segregation in school districts in Massachusetts, I plot the percent nonwhite enrolled in school districts in 1985 and 2013 (the first and last years for which I have district enrollments). Districts are aggregated into 10-point bins based on percentage nonwhite enrollment, and those in the extreme bins are ones with high levels of segregation.[1] First, note that the percent nonwhite enrolled in Massachusetts school districts has increased over this period—the median was 2.3 percent in 1985 and rose to 15.2 percent in 2013. The bins are therefore measured relative to the median. Second, because the percent nonwhite enrollment in Massachusetts is low, many districts will have low nonwhite enrollments. Hence, I focus on districts with percent nonwhite enrollments above the median.

1. See Clotfelter (2013) for an example of this technique.

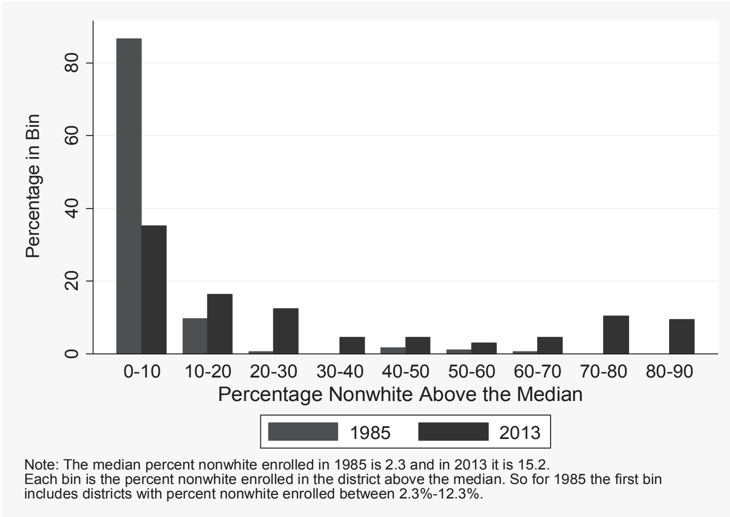

Figure 1. Percentage Nonwhite Enrolled Above the Median: 1985 and 2013 MA School Districts with Percentage Nonwhite above the Median.

The results are given in figure 1. In 1985, 86.7 percent of the districts with above-median percent nonwhite enrollments had values that were within 10 percentage points of the median and 96.3 percent were within 20 percentage points of the median, whereas in 2013, only 35.2 percent and 51.5 percent were within 10 and 20 percentage points of the median, respectively. In 2013, therefore, there were many more districts with percent nonwhite enrolled that was substantially above the median than was the case in 1985. Furthermore, in an absolute sense, in 2013 more than 10 percent of all the districts had nonwhite enrollment of 80 percent or more, whereas none of the districts had nonwhite enrollments at this level in 1985. This indicates that the segregation of nonwhites into districts with a high percentage of nonwhite enrollments has increased over this period.

Additional support for Schworm and Caroll's (2011) conclusion is the fact that for the majority of towns in Massachusetts that are also school districts the correlation between the percent nonwhite enrolled in town schools and the median household income has increased in magnitude from −0.15 in 1985 to −0.30 in 2009. That is, not only has there been an increase in districts with a high percentage of nonwhite enrollment, but they tend to be increasingly in low-income areas.

Another sign of increasing segregation in schools is the decreasing percentage of nonwhites in districts with high enrollments of white students. Given the low percentage of nonwhites in school districts in Massachusetts, particularly in 1985, the above approach is not appropriate for showing changes in segregation that occur with a decrease in nonwhite enrollments. In this paper, I provide an alternative means for providing evidence of increased "whitening" of already majority white school districts. I show this is an unintended consequence of a major property tax limitation law, Proposition $2\frac{1}{2}$, which was implemented in the early 1980s in Massachusetts. It limited local property taxes to 2.5 percent of assessed value (levy ceiling) and restricted growth in the levy limit to 2.5 percent a year (with an allowance for new growth). The measure did allow residents to vote to override the 2.5 percent increase in the levy limit as long as it did not result in taxes that exceeded the levy ceiling. An override results in a permanent increase in the city or town's levy limit (increasing the base for each successive year's allowable 2.5 percent increase). In a recent paper (Wallin and Zabel 2011), we find that richer towns tend to have more successful override votes and tend to be in better fiscal condition than poorer towns. That is, richer towns attract households with a relatively high demand for public services who can afford to pay for them.

In this paper, I investigate whether successful overrides earmarked for school expenditures led to increased segregation in school districts in Massachusetts; in particular, that it decreased the percent of nonwhites enrolled in districts with already high percentages of white enrollments. The logic is that these successful overrides led to greater spending on schools which, in turn, led to a greater concentration of residents with high demands for school quality. These households tend to have higher incomes and, because of the negative correlation with race, are more likely to be white. Furthermore, successful overrides tend to occur in high-income towns with relatively low minority enrollment. Given that increased school spending leads to increased school quality and increased school quality is capitalized into house prices, there is a multiplier effect as higher house prices attract higher income and white households that demand greater school quality.[2]

I collect panel data on overrides from 1982 to 2012 and merge this with data on school district enrollment starting in 1985, along with numerous town-level characteristics. I rely on the panel nature of the data to use a difference-in-difference estimator. I find evidence that passing overrides earmarked for schools results in a significant decrease in the percent of nonwhites enrolled in Massachusetts town schools. This happens in towns with below-average nonwhite school attendance and hence increases segregation.

These results will be useful for state and local policy makers as they provide information about the unintended consequences of tax and expenditure

2. Bayer, Ferreira, and McMillan (2004) find the indirect effect of school quality on residential sorting is significantly larger than the direct effect.

limitations (TELs) on school segregation. Depending on the goals of the TEL, the possibility of increased segregation might make it less desirable.

The paper is organized as follows. Section 2 provides background information about Proposition $2\frac{1}{2}$. The literature on the impact of TELs on schools is reviewed in section 3. Section 4 gives details about the data. Section 5 discusses the identification strategy used in this study. Estimation results are given in section 6, and section 7 concludes.

2. BACKGROUND

In the 1970s, the characterization of Massachusetts as "Taxachusetts" became popular. This label was justified as Massachusetts residents saw their overall state and local tax effort, as determined by the U.S. Advisory Commission on Intergovernmental Relations, rise from an already high 129 (29 percent above average) in 1975, to 133 in 1977, and then to 144 in 1979—second highest in the nation to New York. In 1977, Massachusetts' per capita property taxes were almost twice that of the average state (Cutler, Elmendorf, and Zeckhauser 1999). In 1980, 49.5 percent of the general revenue of local governments in Massachusetts came from property taxes, compared with 28.2 percent in the average state (Bradbury and Ladd 1982). This was due, in part, to the fact that local revenue options were limited and the percentage of local government revenue in Massachusetts that came from state aid, 27.8 percent, was below the national average of 35 percent.

Given this backdrop, the passage in 1978 of California's property tax limitation law (Proposition 13) motivated Massachusetts citizens to follow a similar path. On November 4, 1980, Massachusetts residents approved Proposition $2\frac{1}{2}$ by a 59 percent to 41 percent vote. The measure limited local property taxes to 2.5 percent of assessed value (levy ceiling), and restricted growth in the levy limit to 2.5 percent a year plus an allowance for new growth. Many towns were above the levy ceiling and, starting in fiscal year (FY) 1982, had to reduce their property tax collections by no more than 15 percent per year until they reached the levy ceiling. Some towns required three years to do so.

Once towns reached their levy ceiling after these cuts, the levy limit was increased by 2.5 percent on an annual basis. Given that assessed property values increased by more than 2.5 percent, on average, the difference between the levy limit and the levy ceiling continued to grow over time such that exceeding the latter was of little concern. Proposition $2\frac{1}{2}$ does allow residents to increase taxes above the levy limit by passing overrides that result in a permanent increase in the town's levy limit (increasing the base for each successive year's allowable 2.5 percent increase) but it cannot exceed the levy ceiling. First, a majority of the town's selectmen or town or city council members must vote to place the override on the ballot, in some cases with the consent of the mayor. Then the override is approved if it receives 50 percent or more of the town vote (Massachusetts Department of Revenue 2007).

Besides overrides, towns have two other means for increasing revenues beyond the levy limit. Temporary increases in the levy limit (even if this surpasses the levy ceiling) are allowed via a capital or debt exclusion. These exclusions are typically used to fund specific capital projects. In the case of a capital exclusion, taxes are increased by the cost of the capital project (so that taxes are only increased for one year). A debt exclusion raises taxes by the amount of the debt service for the life of the debt as projects are funded by issuing bonds. Successful debt exclusions do not become part of the base upon which the levy limit is calculated for future years. In both cases, a two thirds vote by the local government is required to put the capital or debt exclusion on the ballot. A capital or debt exclusion is approved if it receives 50 percent or more of the town's vote (Massachusetts Department of Revenue 2007).

Bradbury (1991) finds that towns attempting at least one override vote had higher incomes per capita, lower new growth as a percent of the previous year's property tax levy limit, and lower levels of excess capacity (the difference between the levy limit and actual property tax revenues) relative to their levy limit. These towns also tended to be smaller and have lower property tax rates. Bradbury concludes that voters in many towns in Massachusetts do appear to get what they want from the Proposition $2\frac{1}{2}$ override process. But she notes that one problem with the override process is that towns in most need of additional public services (those with relatively low incomes) are less likely to pass an override.

Bradbury, Mayer, and Case (2001) analyze the impact of Proposition $2\frac{1}{2}$ on the fiscal behavior of towns in Massachusetts during 1990–94 when (in the beginning of this period) the Massachusetts economy was in a recession and real state aid was cut by 30 percent. As a result, there was a great deal of override activity to make up for the decline in revenues. Further, there was pressure to increase school spending due to a demographically driven rise in enrollments (see figure 3).

Data are limited to 208 of the 351 towns in Massachusetts because Case-Schiller repeat sales indices are only available for 214 towns with enough sales to generate reliable indices. Six towns are excluded due to other data limitations. The results indicate towns most constrained by Proposition $2\frac{1}{2}$ prior to 1990 had the slowest growth in school spending during the 1990–94 period. House price regressions indicate changes in school spending were correlated with growth in house prices between 1990 and 1994. But this relationship was limited to towns constrained by Proposition $2\frac{1}{2}$—those at their levy limit and that had not passed an override prior to 1990.

Lang and Jian (2004) examine how Proposition 2½ affected the relationship between local revenues (property taxes and fees) and house prices in Massachusetts for the period 1984–88. They argue that towns constrained by Proposition 2½ provide local public goods at suboptimal levels and, therefore, house prices will be negatively affected. Increasing property taxes in these towns is expected to increase house values. In this case, towns will set fees to maximize house values. Lang and Jian state that some towns are unable to optimize the fee levels and so increasing fees in these towns will also increase house values.

The preferred sample is limited to 178 of the 351 towns in Massachusetts. Towns with fewer than 40 housing transactions in 1984 (76) and those where the housing stock increased by more than 20 percent between 1980 and 1990 (84) were excluded. The Two-Stage Least Squares results that control for the endogeneity of state aid indicate that all three revenue sources (property taxes, state aid, and fees) have a positive and significant impact on the percent change in the real equalized assessed property value. This is particularly true for towns constrained by Proposition 2½.

Similar results are obtained when using the real per capita change in the assessed value of residential property. Thus the evidence supports the hypothesis that, given the constraints imposed by Proposition 2½, increases in property tax revenues lead to greater growth in house values. The positive impact of fees on house values is not as robust across different specifications. In some cases the impact is not significantly different from zero, which is the expected outcome if there was no constraint on imposing fees. As Lang and Jian (2004) point out, their results are similar to those in Bradbury, Mayer, and Case (2001), even though that study focuses on the impact of expenditures on house values and Lang and Jian focus on revenues.

Wallin and Zabel (2011) look at the impact on local fiscal conditions of Proposition 2½. We find richer towns tend to have more successful override votes and tend to be in better fiscal condition than poorer towns. That is, richer towns attract households with a relatively high demand for public services who can afford to pay for them.

We estimate a dynamic override equation that includes lags of binary indicators of an override vote, OVERRIDE, and whether the override passed, WIN. We find the coefficient estimate for the lag of OVERRIDE is positive and significant at the 1 percent level, whereas the coefficient estimate for the lag of WIN is negative but only significant at the 10 percent level. The estimated semi-elasticities for the lags of OVERRIDE and WIN are 40.0 percent and −20.1 percent, respectively. This implies the experience of putting an override on the ballot (win or lose) increases by 40 percent the likelihood of another override vote the following year. Although a successful override decreases the likelihood of another vote the next year, the full impact of a winning override vote on the likelihood of an override vote the following year is the sum of the coefficients on the lags of OVERRIDE and WIN. The estimate of this sum is positive with a *p*-value of 0.015 and a semi-elasticity of 20 percent—so even having a winning override vote in the previous year makes it more likely there will be a vote in the current year.

In the conclusion of the paper, I speculate that Proposition $2\frac{1}{2}$ led to increased sorting across towns in Massachusetts as the passage of overrides led to greater levels of public goods which, in turn, led to a greater concentration of residents with high demands for these goods. The ability of these towns to provide more services is enhanced as the "median voter" is now more likely to vote for more services. Given that these additional services are capitalized into house prices, this can further entice high-income households to move in and further raise house prices, resulting in a multiplier effect. The result is an even greater distinction between the high- and low-spending towns than otherwise would be the case without the overrides that are a part of Proposition $2\frac{1}{2}$.

3. LITERATURE REVIEW

There is a rapidly growing literature that documents the important role schools play in residential sorting. This literature is surveyed in Brunner (2013). I focus on two strands of literature that are closely related to this study. First, I review the literature on the impact of TELs on schools. Second, I review the literature on local bond referenda for school expenditures because this is similar to override votes in Massachusetts.

The Impact of TELS on Schools

There is a large number of papers that investigate the impact of TELs on town-level outcomes such as local public goods provision. Not surprisingly, many focus on schools because this is one of the most important local public goods that jurisdictions provide. Downes and Figlio (1999) review the literature that evaluates the impact of TELs on student performance. They note that such analyses are complicated by the fact that many states implemented school reforms close to the time when tax limitations became effective. It follows that panel data on school districts of which a subset are subject to tax limits and/or school reforms are needed to best estimate the impact of tax limitations on student performance. Their conclusion is that the literature generally supports the outcome that TELs have a long-run negative impact on student performance, particularly for math.

Other school characteristics have also been affected by TELs. In a nationwide study, Figlio (1997) finds that TELs are associated with larger student-teacher ratios and lower cost-of-living-adjusted starting teacher salaries, but not with administrative costs. Figlio (1998) also finds that Measure 5, a TEL imposed in Oregon in 1990, led to significantly higher student-teacher ratios and that the ratio of administration to educational spending was unaffected or possibly even increased after Measure 5 was in effect. Figlio and Rueben (2001) find that TELs led to a reduction in the quality of new public school teachers. Shadbegian (2003) finds that state-level TELs have little effect on local public education. Results also indicate that local TELs have no effect on spending because TEL-related cuts were offset by increases in state aid. Local TELs do have a small positive impact on student-teacher ratios but not on teacher salaries.

Two papers focus specifically on the impact of Proposition $2\frac{1}{2}$ on Massachusetts schools. Shadbegian and Jones (2005) look at the impact of Proposition $2\frac{1}{2}$ on per capita own-source and intergovernmental (state and federal) revenues, expenditures per pupil, and eighth-grade reading and math Massachusetts Educational Assessment Program test scores. Data on school district finances come from the Census of Government for the years 1972, 1977, 1982, 1987, and 1992; and economic, demographic, and school enrollment data come from the 1970, 1980, and 1990 Decennial Censuses. The sample is limited to 1982, 1987, and 1992 for 139 towns in Massachusetts that have their own K–12 public schools or their own K–8 public schools and then send their students to a regional high school. The Proposition $2\frac{1}{2}$ variables include two indicators of whether the town was required to cut property taxes for at least one year and two or three years to initially reach the levy ceiling and an indicator of a successful override earmarked for the general operating or school budget.

The results using a school district fixed effects estimator show that the lowering of property taxes to the levy ceiling did reduce own-source revenues but these reductions were offset by increases in state and federal aid, thus the net effect on spending per pupil was zero. Yet, the authors find some evidence that Proposition $2\frac{1}{2}$ had a negative effect on student performance. Compared with towns that did not have to reduce their property taxes, test scores in those towns that had to make property tax cuts for at least one year were 1.4 percent lower. But this result is based on ordinary least squares (OLS) estimates so it could just reflect unobserved differences in the two groups of towns.

Bradbury, Case, and Mayer (1998) document the changes in school enrollment in Massachusetts in the 1980s and 1990s. Public school enrollments declined throughout the 1980s as baby boomers graduated from high school, but enrollments rose in the 1990s as birth cohorts started to expand in the mid 1980s. In fact, this trend continued until 2001 and has steadily declined since then (see figure 3). Of course, there is a large amount of heterogeneity across towns around this state trend. The authors' goal was to determine factors that were correlated with this variation in town enrollments. They ran regressions of the change in net enrollments (the percentage point difference between actual and predicted enrollments) between 1980 and 1985 and 1990 and 1995 on a school quality index, percent developable land, demographic characteristics, median house prices, and median rents.

Bradbury, Case, and Mayer (1998) find that school quality was significantly positively correlated with enrollments in the 1990s but not in the 1980s. One explanation is that as the returns to education have increased over time, households have placed a greater importance on their children's education. Bradbury, Case, and Mayer also investigate the role that Proposition $2\frac{1}{2}$ has played in town enrollments. They include indicators of whether Proposition $2\frac{1}{2}$ was binding—whether the town had to make one cut or two or three cuts in property tax revenues to initially meet the levy ceiling, and for the 1990s regression whether the town was at the levy limit in 1989. For the 1980s regression, they find the indicator of two or three revenue cuts has a negative and significant impact on enrollments and the impact is economically significant—compared with towns with no revenue cuts, those with two or three revenue cuts experienced, on average, a 0.37 standard deviation decline in net enrollments. For the 1990s regression, enrollments in towns at the levy limit in 1989 were 0.23 standard deviations lower than those towns not at the levy limit. These results led Bradbury, Case, and Mayer to conclude that Proposition $2\frac{1}{2}$ resulted in a movement of households from towns that were constrained by this law to those that were not.

The results from these two papers indicate the initial reduction in property taxes brought on by the implementation of Proposition $2\frac{1}{2}$ resulted in a drop in enrollments, had no impact on per-pupil spending, and possibly had a negative impact on student performance. In this paper, I look at the subsequent impact of Proposition $2\frac{1}{2}$ on school enrollments. That is, I am looking at the impact of tax increases on school enrollments in the context of a TEL (Proposition $2\frac{1}{2}$).

As discussed by Downes and Figlio (1999), it is important to account for any school reforms that occurred around the time a TEL was promulgated. Massachusetts instituted comprehensive school reform in 1993—the Massachusetts Education Reform Act (MERA). The main reform involved a new system of school financing (Chapter 70) that, initially, redistributed state aid to provide relatively greater support for low-spending school districts. MERA also initiated a higher level of accountability and a new statewide standardized test—the Massachusetts Comprehensive Assessment System (MCAS)—that was designed to evaluate districts' efforts to meet these new learning expectations. One way that MERA might affect the impact of successful overrides is through its impact on state aid. But, for the most part, Chapter 70 involves a mechanical procedure for calculating state aid that is unlikely to be affected by override behavior. Furthermore, as I discuss subsequently, I find no significant evidence of a reduction in state aid when an override is passed. Hence, the impact of MERA on how successful overrides affect segregation in school districts is likely to be minimal at best.

Local Bond Referenda and the Financing of School Capital

Cellini, Ferreira, and Rothstein (2010) analyze the impact of bond financing for school capital on house prices, test scores, and local demographics. They compare house prices in school districts that just passed votes on bonds versus ones that just failed to pass bonds. They include jurisdiction-year observations in an eight-year window around the bond votes. House price data are collected at the census tract level in California for 1988–2005. Bond data are from the California Educational Data partnership for 1987–2006. Racial composition and average family income are from information collected as a result of the Home Mortgage Disclosure Act. Test data correspond to third and fourth grade math and reading scores.

The results show that successful bonds significantly increase capital outlays but not current instructional expenditures two to four years after passage. Furthermore, house prices steadily increase from around 3 percent in the year of passage to around 7–10 percent six years later (though prices are higher in districts with successful votes and it is not clear that these changes are adjusted for these higher prices prior to passage). The impact of bond passage on test scores is generally not significant so it appears that house price increases reflect willingness to pay for academic school outputs that are not captured by test scores. Interestingly, the authors find no effect of bond passage on average income, enrollments, racial composition, or average parental education.

Balsdon, Brunner, and Rueben (2003) develop a framework for estimating the demand for local school infrastructure based on outcomes of bond referenda. An important insight into this model is that the referenda that come to a vote are not randomly selected. Because they are proposed by school boards (or in the case of overrides by local officials), these referenda likely represent the preferences of the school board members as well the preferences of their constituencies. Hence the model of the demand for school infrastructure investment that the authors propose includes a referenda selection equation that will account for the nonrandom selection of referenda that are put to a vote.

The data used by Balsdon, Brunner, and Rueben (2003) are similar to those used by Cellini, Ferreira, and Rothstein (2010); district-level data from California for 1996–2000. They estimate and test two models of school board decision making—the competitive and the budget-maximizing agenda-setting models. The results are consistent with the budget-maximizing model with risk aversion. The estimate of the price elasticity is −0.59 and the estimate of the income elasticity is 0.74, so spending on school infrastructure is reasonably responsive to both price and income. Controlling for sample selection does affect the results, although the impact is modest as the OLS estimates of the price and income elasticities are −0.52 and 0.60, respectively. At least as important, though, is the addition of sociodemographic variables to the model, which decreases the estimate of the income elasticity to 0.54.

A closely related paper is by Neilson and Zimmerman (2011), who evaluate the effect of a large school facility investment program in New Haven, Connecticut, on student achievement, residential sorting, and home prices. The city of New Haven underwent a fifteen-year, $1.4 billion school construction program. The first school project was completed in 1998 and the last is scheduled to end in 2014. The mean expenditure per project was $34 million (in $2005) or about $78,000 per capita in the affected area. A key to the identification of the causal impacts of the school construction program is the fact that the process for the choosing of and the timing of schools for renovation was not based on factors related to the schools or the local community. The authors then use a difference-in-difference framework based on the variation in the timing of the school construction projects.

Neilson and Zimmerman (2011) find that six years after construction began, a $10,000 increase in per capita expenditures on construction raised reading scores by 0.027 standard deviations but had no significant effect on math scores. At the mean construction expenditure this is an increase of 0.21 standard deviations. House prices rise by about 1 percent and enrollments increase by up to 4.4 percent after project completion for every $10,000 in per capita construction expenditures.

4. DATA

The data used in this study are from four main sources: (1) Massachusetts Department of Revenue (MDOR), (2) Massachusetts Department of Elementary and Secondary Education (MDOESE), (3) National Center for Education Statistics (NCES) and, (4) the Census Bureau.

Information on Proposition $2\frac{1}{2}$ comes from the MDOR. An override vote is an attempt to permanently increase the levy limit to a level that is no higher than the levy ceiling. The ballot must state the purpose and dollar amount of the override. A successful override vote results in the amount of the override being included in the levy limit for that year. The median amount of an override vote is $1.00 million (in $2012). The median amount of successful overrides is $1.24 million (in $2012).

The override data cover activity from FY1982, the first fiscal year in which overrides were allowed, until FY2012. There were a total of 4,662 overrides

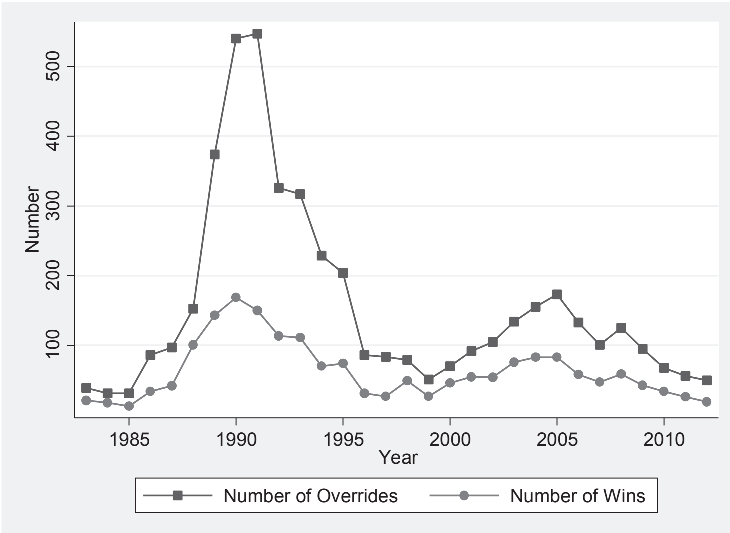

Figure 2. Number of Override Votes and Wins FY1983–FY2012.

reported in the data. Not all overrides are included because it is up to town officials to report override votes. Of the 351 towns, 305 have reported at least one override vote, 50 percent have reported nine or fewer votes, 5 percent have reported 50 or more, and two have reported 100 or more.

The annual number of overrides has varied considerably over the thirty-one years of data. Figure 2 shows the annual number of override attempts on local ballots. This has ranged from a low of 31 in 1984 and 1985 (after the infusion of state aid) to 538 and 548 in 1990 and 1991, respectively (in response to state aid reductions). The number of overrides showed a steady increase at the beginning of the 2000s but never reached the number attained in the early 1990s. In fact, the number has steadily decreased since 2005, with only 56 override attempts in FY2011 and only 51 in FY2012. The percentage of wins was 45.9 percent in the 1980s, 33.4 percent in the 1990s, and 51.1 percent in the 2000s. Hence the difference in the number of recent wins compared with those in the early 1990s is not as great as the difference in overrides.

I will not only look at the impact of override wins on school segregation but also at successful debt and capital exclusions to see if these also have an impact. Furthermore, without controlling for successful debt and capital exclusions, the impact of successful overrides might be biased. The data on capital exclusions cover FY1988 to FY2012. There were a total of 1,442 capital exclusion votes during this period, of which 59 percent passed. The median was $5.18 million (in $2012) for all capital exclusion attempts and $5.43 million (in $2012) for successful votes. There were 7,704 debt exclusions for the period

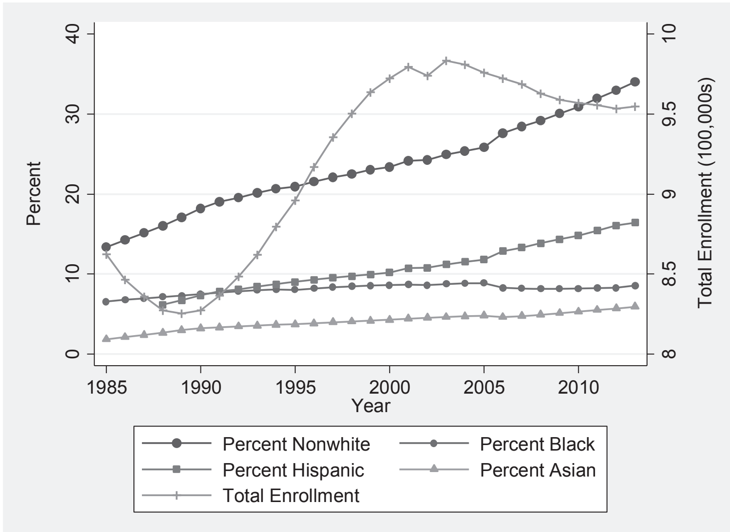

Figure 3. Percent Minorities Enrolled and Total Enrollment Massachusetts Public Schools, 1985–2013.

FY1982 to FY2012, over 50 percent more than the number of overrides. Nearly two thirds of the debt exclusion votes passed. It is hard to know the amount of the debt exclusions because only the net excludable debt for winning votes in each fiscal year is given.

The school-related data come mostly from the MDOESE and are augmented for certain series using the Common Core data from NCES. I have school district-level enrollment data for Massachusetts from 1985 to 2013 that include the percent of nonwhite, black, Hispanic, and Asian students in the district.

Figure 3 shows the annual total enrollment of students in public schools in Massachusetts. During this time period, total enrollments were in decline until 1989. There was then a steady increase until the early 2000s (19 percent total increase) and then a constant, though slight, decline in the last ten years (3 percent total decline). With this setting in mind, the percent of black, Hispanic, Asian, and nonwhite students enrolled in public schools in Massachusetts between 1985 and 2013 are also plotted in figure 3. There has been a fairly steady annual increase of about two thirds of a percentage point in the percent nonwhites enrolled, rising from 13.4 percent in 1985 to 34.0 percent in 2013. There has been only a slight increase in the percent black enrolled (6.6 percent in 1985 and 8.6 percent in 2013), but a 10.2 percentage point increase in Hispanics between 1988 and 2013 (from 6.2 percent to 16.4 percent).

Data from the 1980, 1990, and 2000 Decennial Censuses and from the 2007–2011 American Community Survey (ACS) 5-Year Estimates are obtained from the Census Bureau. The middle year is used as the year corresponding to the ACS 5-Year data sets. The data from the 1980 and 1990 Decennial Censuses are from the extra databases put together by Terry Long and available from the Interuniversity Consortium for Political and Social Research and from the National Historical Geographic Information System.[3] I collect town-level data on family income, race, age, education, population, the number of housing units, persons per unit, median house price, and percent renter. I use family income because it is consistently available in all the data that I have collected. The income data are reported as the number of families with income within specific ranges. I use the midpoint of the ranges to assign income value and then calculate the 10th, 25th, 50th, 75th, and 90th percentiles of the family income distribution.

The unit of observation for this analysis is the town-year. The period of observation varies depending on the variables used, though this generally covers 1985–2012.

The variable OVERRIDE_{it} is an indicator of whether or not there was at least one override vote in town i in fiscal year t. WIN is an indicator of whether or not there was at least one successful override vote. AMOUNT_{it} is the (real) per capita total dollar amount of all override wins in town i and year t. Similar variables are also generated for capital and debt exclusion votes. Variable definitions and summary statistics are given in table 1.[4]

Table 2 gives variable means for the 305 towns that ever put an override on the ballot and the 46 towns that never did so. The p-value of the test of equal population means is given in column 3. It is clear that these two groups of towns are very different. Towns that never put an override on the ballot are larger, have lower family incomes, and lower house prices (in 1980) though, surprisingly, they have a higher percentage of residents with a bachelor's degree. The middle panel provides the same information for the 252 towns that ever had a winning override and the 53 towns that had only unsuccessful overrides. These two groups are also quite different in similar ways as the previous comparison between towns that ever/never voted on an override.

5. IDENTIFICATION STRATEGY

My strategy for identifying the causal impact of override wins on the racial composition of enrollments relies on the difference-in-difference approach. The identification comes from comparing the change in the racial composition

3. See the National Historical Geographic Information System Web site at www.nhgis.org.
4. I exclude two towns (Gosnold and Monroe) from the summary statistics for school data from the MDOESE and NCES and from the regressions because their annual enrollment is never greater than twenty and this skews some of the values of variables based on the percentage enrolled. For example, in Gosnold there were only three students enrolled in 2001 and all are recorded as special education students.

Table 1. Summary Statistics

Name	Number	Mean	SD	Min	Max
	Override Data from MADOR				
Override	10,881	0.16	0.37	0	1
Win (Override = 1)	1,790	0.64	0.48	0	1
Real Amount Per Capita (Override = 1)	1,790	38.27	113.86	0.00	2, 093.15
Debt Exclusion	10,881	0.24	0.23	0	1
Win (Debt Exclusion = 1)	2,617	0.87	0.33	0	1
Capital Exclusion	8,775	0.06	0.24	0	1
Win (Capital Exclusion = 1)	540	0.75	0.43	0	1
	School Data from MDOESE and NCES				
Percent Nonwhites Enrolled in Town Schools	7,361	11.19	14.99	0.00	94.30
Percent Blacks Enrolled in Town Schools	7,361	3.15	5.64	0.00	54.00
Percent Hispanics Enrolled in Town Schools	6,369	4.97	10.34	0.00	90.40
Percent Asians Enrolled in Town Schools	7,361	2.70	3.81	0.00	34.20
	1980 Decennial Census Data				
Population (1,000s)	351	16.34	35.82	0.06	562.99
Percent Nonwhite	351	2.08	3.56	0.00	36.04
Percent Black	351	0.93	2.00	0.00	22.46
Percent Aged 65 or Older	351	11.73	4.30	1.91	30.15
Percent Aged 17 or Less	351	27.91	4.53	3.45	36.98
Percent with Bachelor's Degree (25 and older)	351	21.23	11.57	0.00	61.72
Percent with No High School Degree (25 and older)	351	24.34	10.87	4.14	64.73
Median Family Income ($1,000s)	351	20.76	4.93	6.25	45.00
Median House Price ($1,000s)	351	47.94	16.00	22.92	131.55
Number of Housing Units (1,000s)	351	6.29	14.92	0.08	241.44
Percent 4 or more Persons Per Housing Unit	351	32.07	7.47	10.34	50.65
	1990 Decennial Census Data				
Population (1,000s)	351	17.14	36.54	0.10	574.28
Percent Nonwhite	351	3.58	5.45	0.00	49.00
Percent Black	351	1.28	2.44	0.00	25.54
Percent Aged 65 or Older	351	13.04	4.33	2.67	34.06
Percent Aged 17 or Less	351	22.85	3.68	6.12	32.65
Percent with Bachelor's Degree (25 and older)	351	28.10	13.25	6.94	68.50
Percent with No High School Degree (25 and older)	351	16.31	8.04	2.22	53.30

Table 1. Continued.

Name	Number	Mean	SD	Min	Max
Median Family Income (1,000s)	351	45.37	10.84	26.25	87.50
Median House Price (1,000s)	351	154.58	55.85	58.13	447.92
Number of Housing Units (1,000s)	351	7.04	15.66	0.08	250.86
Percent 4 or more Persons Per Housing Unit	351	28.20	6.29	6.25	44.04
	2000 Decennial Census Data				
Population (1,000s)	351	18.09	37.33	0.06	589.14
Percent Nonwhite	351	6.20	7.49	0.00	51.25
Percent Black	351	1.50	2.76	0.00	24.94
Percent Aged 65 or Older	351	13.39	4.45	3.45	36.10
Percent Aged 17 or Less	351	24.70	3.95	8.10	33.72
Percent with Bachelor's Degree (25 and older)	351	34.51	15.57	9.84	83.41
Percent with No High School Degree (25 and older)	351	11.32	6.73	0.64	43.41
Median Family Income (1,000s)	351	61.46	17.89	17.50	175.00
Median House Price (1,000s)	351	184.98	88.38	70.00	727.27
Number of Housing Units (1,000s)	351	7.47	15.79	0.07	251.94
Percent 4 or more Persons Per Housing Unit	351	26.47	6.33	5.97	41.04
	2007–2011 5-Year American Community Survey				
Population (1,000s)	351	18.55	38.45	0.12	609.94
Percent Nonwhite	351	8.37	9.48	0.00	67.90
Percent Black	351	2.21	4.10	0.00	43.20
Percent Aged 65 or Older	351	14.97	4.95	6.60	42.92
Percent Aged 17 or Less	351	22.00	4.68	2.73	33.91
Percent with Bachelor's Degree (25 and older)	351	39.77	16.08	11.70	83.35
Percent with No High School Degree (25 and older)	351	7.77	5.38	0.22	35.35
Median Family Income (1,000s)	351	78.75	26.09	30.00	175.00
Median House Price (1,000s)	351	324.07	131.73	137.50	875.00
Number of Housing Units (1,000s)	351	7.98	16.97	0.08	272.01

in town schools in Massachusetts before and after successful override votes. Town-years without override votes are used to control for trends in the racial composition of schools over the time period. The key is that the identification comes from within-town changes in the racial composition of schools as a result of override wins.

To determine the impact of override wins on the enrollment of nonwhites, I regress the percent nonwhite enrolled in town school districts,

Table 2. Comparison of Means for 1980 Demographics

Variable	Never Override	Ever Override	*p*-value[b]
Percent Nonwhite Enrolled[a]	7.14	4.59	0.07
Percent Black Enrolled[a]	2.90	1.85	0.14
Percent Nonwhite Residents	2.65	1.99	0.24
Percent Black Residents	1.37	0.86	0.11
Population	45.43	11.98	0.00
Percent Aged 65 or Older	26.40	28.14	0.01
Percent Aged 17 or Less	12.76	11.57	0.08
Percent with Bachelor's Degree (25 and older)	32.40	23.13	0.00
Percent with No High School Degree (25 and older)	15.01	22.16	0.00
Family Income - 10th pct	6.14	7.60	0.00
Family Income - 25th pct	11.96	13.51	0.01
Family Income - 50th pct	19.67	20.92	0.11
Family Income - 75th pct	27.47	29.34	0.07
Family Income - 90th pct	35.79	39.19	0.02
House Prices - 10th pct	25.55	27.86	0.15
House Prices - 25th pct	33.69	37.30	0.07
House Prices - 50th pct	42.68	48.74	0.02
House Prices - 75th pct	53.67	62.66	0.01
House Prices - 90th pct	66.75	80.04	0.00
Number	47	305	
Variable	**Never Win**	**Ever Win**	***p*-value[b]**
Percent Nonwhite Enrolled[a]	5.43	4.38	0.51
Percent Black Enrolled[a]	1.46	1.95	0.27
Percent Nonwhite Residents	1.86	2.02	0.69
Percent Black Residents	0.68	0.90	0.20
Population	20.00	10.27	0.00
Percent Aged 65 or Older	28.71	28.02	0.22
Percent Aged 17 or Less	11.48	11.59	0.84
Percent with Bachelor's Degree (25 and older)	30.44	21.59	0.00
Percent with No High School Degree (25 and older)	14.22	23.84	0.00
Family Income - 10th pct	6.91	7.75	0.01
Family Income - 25th pct	12.43	13.74	0.00
Family Income - 50th pct	19.69	21.18	0.01
Family Income - 75th pct	27.17	29.80	0.00
Family Income - 90th pct	36.18	39.83	0.00
House Prices - 10th pct	24.37	28.59	0.00
House Prices - 25th pct	31.94	38.42	0.00
House Prices - 50th pct	41.10	50.34	0.00
House Prices - 75th pct	52.16	64.87	0.00
House Prices - 90th pct	64.31	83.34	0.00
Number	252	53	

Notes: [a]Data are from 1985.
[b]*p*-value for test of equal population means across groups.

PCTNW_ENROLL, on whether or not the town passed at least one override vote in a given year, WIN. I include time dummies in the model because the number of successful override votes is not evenly distributed over time and hence the impact might be confounded by the positive trend in PCTNW_ENROLL (see figure 3). The distribution of PCTNW_ENROLL is right-skewed so I take the log, which has a distribution that looks much more like a normal distribution. The difference-in-difference model is thus

$$\begin{aligned}\ln(\mathrm{PCTNW\_ENROLL}_{it}) = \beta_{0t} &+ \sum_{j=0}^{K} \beta_{1j}\mathrm{WIN}_{i,t-j} + \sum_{j=1}^{M} \beta_{2j}\mathrm{WIN}_{i,t+j} \\ &+ \sum_{j=0}^{K} \beta_{3j}\mathrm{LOSE}_{i,t-j} + \sum_{j=1}^{M} \beta_{4j}\mathrm{LOSE}_{i,t+j} \\ &+ \beta_{5t}X_{i,1980} + u_i + v_{it} \qquad (1)\end{aligned}$$

where $X_{i,1980}$ is a vector of demographic factors from the 1980 Decennial Census. I use data from 1980 because it predates observed values of PCTNW_ENROLL and hence can be considered to be exogenous. As in Lutz (2011), the coefficient (vector) on $X_{i,1980}$ is allowed to vary over time to control for trends in PCTNW_ENROLL that are related to the demographic variables in $X_{i,1980}$. I include the percent of residents aged 25 and over with a bachelor's degree and without a high school degree; the percent of residents aged 17 or less and aged 65 or older; the logs of median house value, median family income, the number of housing units, and population; and the percent nonwhite in the town and the percent of housing units with four or more people.

It is likely that PCTNW_ENROLL and WIN are affected by factors such as population, state aid, income, house prices, and local economic conditions such as the unemployment rate. Therefore, despite the fact that I have allowed the impact of $X_{i,1980}$ to vary over time, there are still concerns with omitted variables bias if the contemporaneous values of these factors are excluded.[5] The problem with including these variables in Equation 1 is they are likely to be endogenous due to unobserved factors that affect residential sorting. Furthermore, these variables are, themselves, likely to be affected by WIN so that controlling for them will not allow for an estimate of the full effect of successful overrides on the racial makeup of schools (i.e., the direct effect plus the indirect effects on PCTNW_ENROLL that arise from the changes in population and the other variables due to successful overrides). It is likely these factors affect PCTNW_ENROLL with a lag but even only including lags will not fully solve the endogeneity problems. In any case, I will include

5. I thank the referee for raising this point.

contemporaneous and lagged values of population, state aid, income, house prices, and the unemployment rate to see if they affect the estimates of the impact of successful overrides on PCTNW_ENROLL.

Passing an override might not have an immediate impact on the percent nonwhite enrolled in schools, as sorting across towns takes time to fully play out. I therefore include four lags of WIN in the model. This will also allow me to see if any change in PCTNW_ENROLL is maintained over time. I also include two leads of WIN. If towns that passed overrides tended to do so when the racial composition of schools is changing, then any changes in PCTNW that are caused by WIN should be measured relative to any changes prior to the successful override.

I also include the variable, LOSE (plus two leads and four lags), that is an indicator that all override ballots in a given town-year were unsuccessful. The question to answer by including LOSE is "does losing all override votes in a year affect the percent nonwhite enrolled in the town's schools?" This could be viewed as a negative signal that might deter households from moving to the town (or vice versa).

The key to the difference-in-difference framework is the inclusion of the town fixed effects, u_t in equation 1. This implies the identification of β_{1j} and β_{2j} comes from within-town changes in PCTNW_ENROLL before and after an override win. Note that by including LOSE in equation 1, the "control" group is town-years with no override vote.

6. RESULTS

The Impact of Overrides on Town Demographics

To get some idea of the impact of successful overrides on town residential composition, I look at changes in percent nonwhite, percent black, percent of residents aged 25 and older with a bachelor's degree, percent renters, population, the percent of residents aged 17 or less and aged 65 or older, real family income, real median house values, the number of housing units, and the percent of housing units with four or more people across decennial censuses for 1980, 1990, and 2000.[6] I also use the five-year ACS for 2007–2011 to generate values for 2009, though these are estimates and not actual values as is the case for the decennial censuses. I regress the decadal change in these variables including the 10th, 25th, 50th, 75th, and 90th percentiles, the interquartile range, and the 10th to 90th percentile range of the town's real family income, on the number of years over the decade in which there was a least one successful override, DSUM, and whether there was at least one successful override in the

6. Note that the model developed in the previous section (equation 1) does not apply because these data are only available in the Decennial Censuses. This is the reason I consider a model based on decadal changes in these variables.

decade, DWIN (separate regressions for each successful override variable).[7] I run the regressions for all three decadal changes combined. Hence, there are a total of 1,053 observations. I include base-year dummies and run regressions with and without base-year values of these variables.

There are significant negative effects of DWIN and/or DSUM on the percent nonwhite, percent black, and percent of residents aged 65 or older, and significant positive effects on the percent of residents aged 17 or less, the percent of housing units with four or more people, population, median real house price, and number of housing units, but no significant effects on the percent with a bachelor's degree or the percent renters. These results suggest households with school-aged children are moving into towns in response to successful overrides. These households are likely both replacing older households without children in existing units and moving into new units. Furthermore, there are significant decreases not only in the percent nonwhite and percent black residents as a result of successful overrides but in the number of nonwhites and blacks that reside in towns that pass overrides. Hence, the evidence supports the result that successful overrides lead to a decline in nonwhites and blacks in an absolute sense and not just in a relative sense.

In the case of the family income distribution, successful overrides over the decade are significantly positively correlated with increases in the 10th, 25th, and 50th percentiles but not with changes in the 75th or 90th percentiles. It appears that successful overrides raise the lower half but do not affect the upper portion of the town income distribution. There is also some mild evidence that override wins lead to more homogeneity in town incomes, as declines in the interquartile and 10–90 percentile ranges are associated with override wins. Overall, these results support the hypothesis that successful overrides have affected sorting across towns in Massachusetts.[8] Given that sorting across towns and schools are related, this supports the next step, which is to look at the impact of successful overrides on school segregation.

The Impact of Overrides on School District Enrollments by Race/Ethnicity

I now turn to the impact of override behavior on the racial composition of schools. Data on school enrollments at the town level are required because this is the level at which overrides occur. One advantage of focusing on Massachusetts is that school districts tend to coincide with town boundaries. Still, only 172 of the 351 towns in Massachusetts have their own K–12 schools. The remaining 179 towns send their children to schools in another town or

7. Of course, the change from 2000 to 2009 is only for nine years.
8. Results available on request.

to a regional school for at least one grade (usually at least high school).[9] Of these 179, anywhere from 65 to 118 (depending on the year) report information on the characteristics of the students enrolled in the town schools. Towns that do not have their own schools for any grades obviously do not report this information and hence are excluded for most of the analysis.[10]

Data on school district enrollments by race are available starting in 1985. I estimate equation 1 using town fixed effects with standard errors clustered at the town level. This is the difference-in-difference model. Again, the identification comes from within-town changes in PCTNW_ENROLL before and after a successful override vote. None of the lags of WIN are significant at even the 10 percent level. The coefficients for LOSE and its lead and lags are also not significant.[11,12]

Overrides must explicitly state the use of the funds proposed in the vote. This allows me to determine which overrides were intended for school improvement. These make up 25.7 percent of all the override votes. There are 1,790 town-years where there was at least one override vote and 47.2 percent of these include at least one override vote where the funds were intended for schools. Of the 1,140 town-years with at least one override win, 40 percent have at least one successful vote that was dedicated to schools. Furthermore, I allow for the impact to be different for town-years where all successful overrides are dedicated to schools and town-years where at least one, but not all, successful overrides are earmarked for schools. These impacts might be different if the information about the successful overrides for schools is not as salient when there are other override wins as they are for town-years where all the successful overrides are going to schools or if the total dollar amount of the overrides that go to schools is greater in the latter versus the former case. On the other hand, one might be concerned that such differentiation is nonrandom and hence any difference in the results is due to unobservable differences in these towns rather than actual differences in the impacts

9. One might think that when regional schools need to increase spending there would be coordination across member towns in proposing overrides, but this does not appear to happen (Christine M. Lynch, Director, School Governance, MDOESE, personal communication). Furthermore, towns with excess capacity might be able to increase property taxes without resorting to an override vote.
10. Note that enrollments in regional schools are broken down by town but not by race in each town.
11. A comparable model is based on first differences versus fixed effects. The first-difference estimator is more efficient if the error term is serial correlated. I tested for serial correlation using the xtserial command in Stata. The *p*-value of the test is 0.014; hence the absence of serial correlation is not rejected at the 1 percent level. Still, I ran the model using the first-difference estimator and the results are similar to those using the fixed effects estimator.
12. Baseline town-level demographic and economic characteristics whose impacts vary by year are included to control for trends in PCTNW_ENROLL that are related to these variables. An alternative approach to controlling for general trends in PCTNW_ENROLL is town-specific linear trends. When I include these linear trends in the model, the results are very similar to the results without the linear trends.

themselves. This concern should be assuaged by the inclusion of the town fixed effects and the town-level demographic variables.[13]

To recap, town-years with successful overrides are divided into three groups: (1) those where all wins were dedicated to schools, (2) those with multiple wins where some (but not all) were earmarked for schools, and (3) those where none of the wins were intended for schools. I also differentiate between override losses that include ones that were earmarked for schools and ones that were not. Failed override votes that are dedicated for schools could be viewed as a negative signal that might deter households who are interested in good schools from moving to the town.

Results are given in table 3. It does appear that the impact on percent nonwhite is significant when successful overrides are only earmarked for schools. The coefficient estimates for WIN and its four lags are all negative and jointly significant at the 1 percent level, and lags 3 and 4 are individually significant at the five percent level or better. The (gross) semi-elasticities are given in column (2); they range from −1.4 percent for the first lag of WIN to −8.5 percent for the fourth lag. The coefficient estimates for the two leads of WIN are neither individually nor jointly significant at even the 10 percent level. Although not significant, the first lead of WIN is negative with a semi-elasticity of −1.9 percent. This is a weak indication that, on average, successful override votes occur in town-years where the percent nonwhite enrolled is lower than in town-years that did not have an override vote. Hence, I also calculate the net semi-elasticity—the percent change in the percent nonwhite relative to the change in the year prior to the override win. These are also listed in column 2 of table 3. The net semi-elasticity for the third and fourth lags of WIN is −5.3 percent and −6.7 percent, respectively.

There is not a statistically significant impact on PCTNW_ENROLL when there is a mix of override wins where at least one is earmarked for schools. Again, this indicates override wins dedicated to schools may not carry as strong a signal when there are successful overrides earmarked for other purposes. There is also no impact when all successful overrides are to be used for non-school purposes. Furthermore, override losses have no impact on the percent of nonwhites enrolled in schools regardless of whether any were earmarked for schools (results not included in table 3). Therefore, it does not appear that losing an override vote has an effect on the percent nonwhite enrolled in the town's schools.

Given that successful overrides earmarked for schools result in a decrease in nonwhite enrollments, does this lead to an increase in segregation across

13. A comparison of means of observable characteristics across town-years where all successful overrides are dedicated to schools, and town-years where at least one but not all successful overrides are earmarked for schools, produces very few significant differences.

Table 3. Regression Results Where ln(PCTNW_ENROLL) Is Dependent Variable

	Override Win Type					
	Wins for Schools Only		**Mixed Wins**		**No Wins for Schools**	
VARIABLES	**Coeff Est (1)**	**Semi Elast Gross/Net (2)**	**Coeff Est (3)**	**Semi Elast Gross/Net (4)**	**Coeff Est (5)**	**Semi Elast Gross/Net (6)**
win	−0.020	−2.0/−0.1	−0.031	−3.1/−5.2	(0.017)	−0.4/0.3
	(0.024)		(0.026)		−0.003	
win_{t-1}	−0.014	−1.4/0.6	0.012	1.3/−1.0	(0.016)	−0.3/0.4
	(0.021)		(0.025)		−0.014	
win_{t-2}	−0.027	−2.6/−0.7	0.004	0.4/−1.8	(0.017)	−1.4/−0.7
	(0.026)		(0.026)		−0.004	
win_{t-3}	−0.074**	−7.1/−5.3	0.015	1.5/−0.7	(0.015)	−0.4/0.3
	(0.030)		(0.023)		−0.005	
win_{t-4}	−0.089***	−8.5/−6.7	0.011	1.2/−1.1	(0.019)	−0.5/0.2
	(0.025)		(0.029)		−0.007	
WIN_{T+1}	−0.019	−1.9	0.022	2.2	(0.020)	−0.7
	(0.023)		(0.020)		0.000	
WIN_{T+2}	0.006	0.6	−0.008	−0.8	(0.020)	0.0
	(0.022)		(0.029)		(0.017)	
p-value: WIN + lags = 0	0.009		0.641		0.967	
p-value: WIN leads = 0	0.219		0.363		0.831	
Observations	6,643					
R^2	0.667					
Number of Towns	288					

Notes: Also included as explanatory variables: LOSE and 4 lags and 2 leads for overrides, time dummies, Percent nonwhite, Percent Aged 65 or Older, Percent Aged 17 or Less, Percent with Bachelor's Degree, Percent No HS Degree, Percent of housing units with four or more people, and the natural logs of median family income, median house price, and the number of housing units all from the 1980 Decennial census.
Standard errors in parentheses.
** Statistically significant at the 5% level; *** statistically significant at the 1% level.

towns? This would be true if these overrides occurred in towns where the percent nonwhite enrolled in schools was below average (or median). Of the 290 town-school districts where I have enrollment data by race, 106 had at least one year with override wins only devoted to schools, 36 had at least one year with a mix of wins devoted to schools and other purposes, 57 had override wins only devoted to nonschool purposes, and 91 never had an override win. I then compare the percent of nonwhite enrollments in the 106 towns that had at least one year with override wins only devoted to schools with those in the 91 towns that never had an override win. Evidence supporting the hypothesis that successful overrides earmarked for schools lead to increased segregation would show lower nonwhite enrollments in the former set of towns as compared to the latter.

Table 4 gives the means of PCTNW_ENROLL for these two groups of towns for 1985, 1990, 1995, 2000, 2005, and 2010. The mean (median) of PCTNW_ENROLL in towns where there was at least one year with override wins devoted only to schools is always less than the mean (median) for towns that never had an override win—this difference grows over time. Similar results hold for blacks and Hispanics but not for Asians (see table 4).

Figure 4 provides a visual representation of this result. It displays the cumulative distribution function (CDF) for PCTNW_ENROLL in towns where there was at least one year with override wins devoted only to schools and in towns that never had an override win for five-year intervals. Given that PCTNW_ENROLL is lower in the former group of towns, then its CDF for PCTNW_ENROLL should lie above the CDF for the latter group of towns. In fact, this is the case, and furthermore, the difference between the CDFs grows over time. Of course, the increase in these differentials could be due to factors other than override wins, but what is clear is that those towns passing overrides earmarked for schools have fewer minorities enrolled than towns that never pass overrides.

What about reverse causality? Could an override be in response to increasing enrollments (particularly those dedicated to schools)? I run a simple regression of overrides dedicated to schools on the log of total enrollments and its four lags and a separate regression on town enrollments and its four lags, time dummies, and town fixed effects. There is no significant evidence that enrollments in the current or previous four years have an effect on putting an override on the ballot. Still, to account for any differences in the outcome variable (whether it is enrollments or otherwise) I calculate net semi-elasticities as the relevant measures of the impact of successful overrides as they are measured with respect to the impact of the first lead on the outcome variable.

What about the role of state aid? Maybe there is a decline in state aid in towns that pass overrides that might counteract any impact from the successful override. I run a regression with the log of real net state aid per capita as the dependent variable and WIN and its four leads and four lags, LOSE and its four leads and four lags, time dummies, and town fixed effects as the independent variables. There is no significant evidence of a reduction in state aid when an override is passed or when it fails.

As I discussed in section 5, it is likely that PCTNW_ENROLL and WIN are affected by factors such as population, state aid, income, house prices, and local economic conditions such as the unemployment rate. Excluding them

Table 4. Mean/Median for Percent Enrolled for Towns That Had at Least a Year with Override Wins Earmarked Only for Schools and Towns that Never Had Override Wins

	Never Win		At Least One Win for Schools		
Year	Mean (1)	Median (2)	Mean (3)	Median (4)	*p*-value[a] (5)
			Percent Nonwhite Enrolled		
1985	6.22	59	4.02	48	0.08
1990	10.13	61	5.23	45	0.01
1995	13.12	64	5.93	41	0.00
2000	15.45	66	6.73	40	0.00
2005	18.67	66	8.69	44	0.00
2010	24.05	66	12.73	45	0.00
			Percent Black Enrolled		
1985	2.13	55	1.83	50	0.62
1990	2.98	59	1.99	41	0.16
1995	3.77	57	2.18	39	0.04
2000	4.30	59	2.20	37	0.02
2005	5.01	59	2.28	41	0.00
2010	4.98	63	2.17	43	0.00
			Percent Hispanic Enrolled		
1985	0.75	64	0.25	48	0.00
1990	1.38	59	0.40	44	0.00
1995	6.23	68	1.51	40	0.00
2000	7.35	66	1.85	35	0.00
2005	9.30	67	2.47	35	0.00
2010	12.29	71	3.63	35	0.00
			Percent Asian Enrolled		
1985	1.23	50	1.32	57	0.67
1990	2.48	60	1.80	50	0.13
1995	3.00	61	2.08	47	0.09
2000	3.55	58	2.51	50	0.11
2005	4.04	54	3.60	54	0.54
2010	4.33	55	4.19	53	0.87

Notes: [a]*p*-value for the comparison of means for towns that never had a winning override (column 1) versus towns that had at least one year with wins earmarked only for schools (column 3).

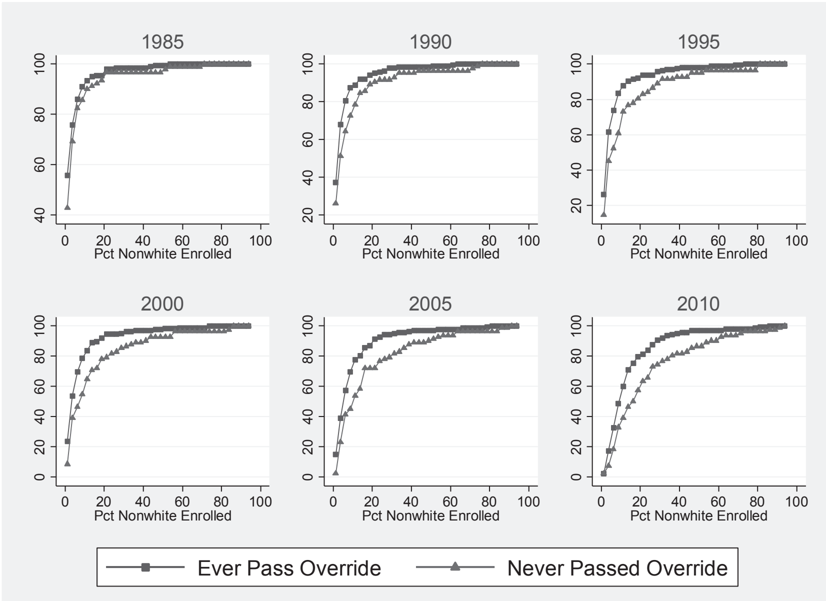

Figure 4. CDFs for Percent Nonwhite Enrolled: Towns That Ever and Never Pass Overrides.

has the potential of generating omitted variables bias. Because these variables are likely to be endogenous, I first include only their lags and then add in the contemporaneous values. In both cases none of the variables in levels or lags are individually significant at the 1 percent level and there is little impact on the coefficient estimates for the successful override variables. It appears that allowing the impact of the 1980 Decennial Census variables to vary over time has done a good job in accounting for the impact of factors such as population, state aid, income, house prices, and the unemployment rate on PCTNW_ENROLL and WIN.

The impact of successful overrides could be tied to the size of the override. I replace WIN (an indicator of a successful override) with the (real) per capita amount of the successful override, AMOUNT_PC. I distinguish between amounts dedicated to schools when all wins are only for schools, AMOUNT_PC_SCHOOLS_ALL, when only some of the wins are for schools, AMOUNT_PC_SCHOOLS_SOME, and when none of the wins are for schools, AMOUNT_PC_OTHER. The results are comparable to those when indicators of override wins are included; the coefficient estimates for AMOUNT_PC_SCHOOLS_ALL and its four lags are all negative and jointly significant, and the third and fourth lags are individually significant. Also, evaluating the impacts at the median override amount for schools results in similar impacts as those obtained using WIN (see table 3). The difference in the impact on PCTNW_ENROLL for the 5th percentile override amount and the 95th percentile override amount for an override that was passed four years earlier is 2.3 percent. This compares with a net semi-elasticity of 6.7 percent (table 3). So there is some heterogeneity in the impact on PCTNW_ENROLL from an override win based on the amount of the override.

The median real per capita amount of the override when override wins are only for schools is \$26.20 and the median real per capita amount of the override when only some of the wins are for schools is \$19.40. It could be, therefore, that the reason override wins solely devoted to schools affect enrollments and wins that are only partially earmarked for schools do not is that the amount of money for schools is larger in the former case. But this reasoning is only partially correct because the coefficient estimates for AMOUNT_PC_SCHOOLS_SOME and its four lags are insignificant. This is also the case for AMOUNT_PC_OTHER.

I next look at the impact of successful debt, WIN_DEBT, and capital exclusions, WIN_CAP, on percent nonwhite enrolled. Typically, expenditures earmarked for schools from debt and capital exclusions go toward physical plant upgrades, whereas overrides designated for schools go toward the school operating budget, of which a large portion is teacher salaries. Thus, the impact of a successful debt or capital exclusion on percent nonwhite enrolled could differ from the impact of a successful override.

I estimate equation 1 replacing WIN with WIN_DEBT and WIN_CAP, respectively. In both cases, I differentiate override wins for schools only, a mix of schools and other purposes, and no wins earmarked for schools. Neither WIN_DEBT and its fours lags and two leads nor WIN_CAP and its fours lags and two leads are individually or jointly significant. Next, I include override, debt exclusion, and capital exclusion wins in the same regression. First, the results for successful overrides are essentially unchanged. Second, debt and capital exclusion wins still do not significantly affect percent nonwhite enrolled whether they are dedicated to schools or otherwise.

The impact of override wins might differ by race/ethnicity. I run the model with differential effects by type of override using the natural log of the percent black, PCTBLACK_ENROLL, Hispanic, PCTHISPANIC_ENROLL, and Asian, PCTASIAN_ENROLL, enrollment as the dependent variable. The results are given in table 5. The impacts of override wins exclusively dedicated to schools on the percent enrollments for blacks are all insignificant—the largest impact (in magnitude) is −3.0 percent. Only the impact of the fourth lag of override wins exclusively dedicated to schools on the percent enrollments for Hispanics is significant. The estimate for the first lead is actually positive so the net semi-elasticities are all negative, and the values for the third and fourth lags are −4.6 percent and −8.8 percent, respectively. The impacts of override wins

Table 5. Regression Results for Black, Hispanic, and Asian Enrollments

	Dependent Variable (in natural logs)					
	Percent Black		Percent Hispanic		Percent Asian	
VARIABLES	Coeff Est (1)	Semi Elast Gross/Net (2)	Coeff Est (3)	Semi Elast Gross/Net (4)	Coeff Est (5)	Semi Elast Gross/Net (6)
			Wins Only for Schools			
win	−0.030 (0.020)	−3.0/−2.7	0.013 (0.025)	1.3/−1.0	−0.022 (0.024)	−2.2/1.9
win_{t-1}	−0.016 (0.022)	−1.6/−1.3	−0.008 (0.024)	−0.8/−3.0	−0.004 (0.023)	−0.4/3.8
win_{t-2}	0.009 (0.027)	0.9/1.2	0.006 (0.027)	0.7/−1.6	−0.027 (0.023)	−2.7/1.4
win_{t-3}	−0.031 (0.022)	−3.0/−2.7	−0.025 (0.035)	−2.5/−4.6	−0.027 (0.028)	−2.7/1.3
win_{t-4}	−0.023 (0.022)	−2.2/−1.9	−0.070** (0.030)	−6.8/−8.8	−0.027 (0.029)	−2.7/1.4
WIN_{T+1}	−0.003 (0.021)	−0.3	0.022 (0.023)	2.3	−0.041* (0.021)	−4.0
WIN_{T+2}	0.002 (0.019)	0.2	0.023 (0.030)	2.4	−0.020 (0.018)	−1.9
p-value: WIN + lags = 0	0.287		0.032		0.462	
p-value: WIN leads = 0	0.965		0.616		0.136	
			Mixed Wins			
win	−0.026 (0.023)	−2.5/−4.0	−0.036 (0.031)	−3.5/0.3	0.004 (0.027)	0.4/−2.9
win_{t-1}	0.026 (0.025)	2.6/1.1	−0.042 (0.032)	−4.1/−0.4	0.008 (0.029)	0.8/−2.6
win_{t-2}	0.005 (0.024)	0.5/−1.1	0.020 (0.032)	2.0/6.0	0.011 (0.023)	1.1/−2.3
win_{t-3}	0.008 (0.022)	0.8/−0.8	0.027 (0.034)	2.7/6.8	−0.027 (0.022)	−2.6/−5.9
win_{t-4}	0.015 (0.025)	1.5/−0.1	0.045 (0.035)	4.6/8.7	−0.025 (0.028)	−2.4/−5.7
WIN_{T+1}	0.016 (0.022)	1.6	−0.039 (0.031)	−3.8	0.034 (0.022)	3.5
WIN_{T+2}	−0.003 (0.024)	−0.3	−0.014 (0.039)	−1.4	−0.009 (0.024)	−0.9
p-value: WIN + lags = 0	0.309		0.028		0.413	
p-value: WIN leads = 0	0.646		0.355		0.068	

Table 5. Continued.

	Dependent Variable (in natural logs)					
	Percent Black		Percent Hispanic		Percent Asian	
VARIABLES	**Coeff Est (1)**	**Semi Elast Gross/Net (2)**	**Coeff Est (3)**	**Semi Elast Gross/Net (4)**	**Coeff Est (5)**	**Semi Elast Gross/Net (6)**
	No Wins for Schools					
win	0.025 (0.016)	2.6/1.8	−0.001 (0.025)	−0.1/−1.7	−0.024 (0.015)	−2.3/0.2
win_{t-1}	0.014 (0.016)	1.4/0.7	−0.011 (0.024)	−1.1/−2.7	−0.006 (0.015)	−0.6/2.0
win_{t-2}	0.000 (0.016)	0.0/−0.7	0.004 (0.025)	0.4/−1.2	−0.001 (0.016)	−0.1/2.4
win_{t-3}	0.001 (0.015)	0.1/−0.6	−0.007 (0.023)	−0.7/−2.3	−0.023 (0.015)	−2.3/0.3
win_{t-4}	0.019 (0.017)	1.9/1.2	0.020 (0.024)	2.0/0.4	−0.017 (0.018)	−1.7/0.9
WIN_{T+1}	0.007 (0.017)	0.7	0.016 (0.026)	1.6	−0.026* (0.015)	−2.5
WIN_{T+2}	0.005 (0.018)	0.5	0.025 (0.025)	2.5	−0.017 (0.019)	−1.7
p-value: WIN + lags = 0	0.419		0.571		0.190	
p-value: WIN leads = 0	0.918		0.592		0.222	
Observations	6,643		5,651		6,643	
R^2	0.303		0.579		0.406	
Number of Towns	288		269		288	

Notes: Also included as explanatory variables: LOSE and 4 lags and 2 leads, time dummies, Percent nonwhite, Percent Aged 65 or Older, Percent Aged 17 or Less, Percent with BA, Percent No HS Degree, Percent of housing units with four or more people, and the natural logs of median family income, median house price, and the number of housing units all from the 1980 Decennial census. Standard errors in parentheses.
*Statistically significant at the 10% level; **statistically significant at the 5% level.

exclusively dedicated to schools on the percent enrollments for Asians are all negative but none are significant. The coefficient estimate for the first lead is negative and significant so the net elasticities are all positive; override wins exclusively dedicated to schools appear to occur in towns where the enrollment rates for Asians are lower than in towns with no override votes.

Note that the R^2 for the regression with percent black enrolled as the dependent variable (0.303) is much lower than the regression using percent nonwhite (0.667). This is likely due to the small percent of blacks (and its variation) enrolled in Massachusetts schools, which leads to a much worse fit for this regression and less precision in the coefficient estimates. This may explain the imprecision of the coefficient estimates for WIN and its lags (though the magnitudes are also generally smaller than those for the PCTNW_ENROLL regression).

As previously mentioned, 172 towns have K–12 schools and hence students attend schools only in their town. One might believe the impacts of overrides would be even stronger in these towns. I run regressions for (the natural log of) PCTNW_ENROLL, PCTBLACK_ENROLL, PCTHISP_ENROLL, and PCTASIAN_ENROLL that are limited to towns with K–12 schools. If anything, the results are somewhat weaker for PCTNW_ENROLL and PCTHISP_ENROLL, but they do show stronger results for PCTBLACK_ENROLL. It appears that the negative impact of successful overrides dedicated to schools is felt most by Hispanics and blacks but not by Asians.

7. CONCLUSION

This study investigated an unintended consequence of the property tax limitation law in Massachusetts—the impact of successful overrides on segregation in Massachusetts town school districts. I find evidence that successful overrides earmarked for schools do reduce nonwhite enrollments in a town's schools. The impact is largest four years after the win; nonwhite enrollments fall by 6.7 percent four years after the override is passed. The results show towns that do pass overrides intended for schools have lower minority enrollments than those towns that do not, and therefore the reduction in nonwhite enrollments does result in more segregation in town-school districts. Given that the mean number of years for a town with at least one override win is three, this can lead to a substantial reduction in nonwhite enrollments over the long run.

The amount of the override does appear to matter. A successful override at the 95th percentile of the override amount distribution will lead to a decline in the percent of nonwhite enrollments of 7.9 percent, whereas a successful override at the 5th percentile will lead to a decline in the percent of nonwhite enrollments of 5.5 percent.

There does not appear to be a significant impact of successful overrides on enrollments when other successful overrides in the same year are earmarked for nonschool purposes. There are two reasons why this might be the case. First, for these overrides, the median real per capita amount of successful overrides in a given year is \$19.40, whereas the median real per capita amount of the override when wins are only for schools is \$26.20. So the average impact on enrollments of a successful override when there are other successful overrides not dedicated to schools should be smaller than for override wins that are only for schools. Second, it could be that the information about the successful overrides for schools is not as salient when there are other override wins as compared to when all the successful overrides are going to schools.

The results indicate that successful capital and debt exclusions earmarked for schools do not impact school enrollments. Why might this be the case? The main difference between overrides and capital and debt exclusions that are earmarked for schools is that the latter are generally used for school-related capital projects whereas overrides are used for noncapital school expenditures. Note that my results are consistent with Cellini, Ferreira, and Rothstein (2010), who find that the passage of bonds for school infrastructure spending results in no impact on test scores, enrollments, or racial makeup in California school districts. This supports the conclusion that additional capital school expenditures that are the result of successful overrides/bond referenda do not affect school characteristics, whereas successful votes for noncapital school expenditures do have an impact. It appears that, in terms of school enrollments, households react more to increases in spending on noncapital budget items (such as more teachers) than they do to expenditures on new buildings.[14]

What does this say about policy? States that are thinking about implementing property tax limitations should be aware that there may well be unintended consequences of such laws. Depending on the goals of the law, the possibility of increased segregation might make it less desirable.

One interesting issue for future research is whether there are spillover effects of successful overrides on nearby towns. In particular, are there any spillover effects when one town in a regional school district passes an override vote and another does not?

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