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The Effect of Foreclosure on Boston Public School Student Academic Performance

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Abstract

Although the recent wave of mortgage foreclosures has clearly been accompanied by economic hardship, relatively little research has examined how foreclosures affect the academic performance of students. This paper investigates the relationship between mortgage foreclosures and the academic performance of students using a unique dataset that matches information on the standardized test scores and attendance of individual Boston Public School students with real estate records indicating whether the student lived at an address involved in foreclosure and whether that student's parent or guardian was the owner or a tenant in the property. Econometric analysis of this relationship suggests that foreclosures are associated with slightly lower test scores and attendance, controlling for the previous-year's test score and attendance as well as other student characteristics and environmental factors. The results suggest that both the foreclosure event and the diminished student outcomes stem from underlying economic stress within the family. School changes during the school year, which are sometimes induced by foreclosure-related residential moves but also occur independently of foreclosure, may be associated with more substantial negative effects on academic performance than foreclosures, although this causal relationship is not certain. This latter finding suggests that policies that decouple residential moves from school changes during the school year may help to mitigate this indirect effect of foreclosure on student performance.

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I. Introduction

The housing bust and subsequent Great Recession have triggered many adverse consequences, but the wave of mortgage foreclosures is arguably the defining characteristic of the crisis. Mortgage delinquencies and foreclosures increased much more sharply than is typically the case during a recession and early stages of economic expansion, and these were accompanied by a great deal of economic hardship. As others have documented, foreclosures impose heavy costs on property owners and neighborhoods.[1] It is difficult to distinguish the adverse effects of the foreclosure crisis itself from the costs of the general economic hardship of the Great Recession. Moreover, it is not clear whether the recent spate of foreclosures has had adverse effects on foreclosed households beyond the acute disruption surrounding the foreclosure event itself. This paper investigates one important channel through which foreclosures may have had adverse consequences: by affecting the academic performance of students whose families experienced a foreclosure, whether as homeowners or as tenants in foreclosed properties.

Mortgage delinquencies and foreclosures are typically precipitated by a combination of negative house equity and a negative shock to the owner's finances (for example, job loss, uninsured medical expenses, or divorce).[2] In considering the social welfare consequences of the foreclosure crisis and developing policies to address them, it may be useful to distinguish between the effects of the events narrowly associated with the foreclosure itself, such as losing one's residence, and the effects of the economic stress that precipitated the mortgage delinquency and foreclosure.

We contribute to the understanding of how economic stress and foreclosures affect school-age children, using a unique dataset that matches information on academic performance of Boston Public School students with real estate records indicating whether the student lived at

[1] See, for example, Kingsley et al. (2009).

[2] Foote, Fuhrer, Mauskopf, and Willen (2009) summarize the "double trigger" model of mortgage default as follows: "Foreclosures may occur for a variety of reasons, but our research indicates that they most often occur when two things happen at the same time. First, the homeowner must have negative equity, that is, she owes more on the house than the property is worth. Second, the borrower must have suffered some adverse life event (for example, job loss, illness, or divorce) that makes it difficult for her to keep up with her mortgage payments" (p. 2).

an address involved in foreclosure and whether that student's parent or guardian was the owner or a tenant in the property. We estimate the effect of foreclosures on measures of academic performance, including both standardized test scores and attendance.

We find that foreclosures are associated with slightly lower test scores and attendance, controlling for the previous-year's test score or attendance as well as other student characteristics and environmental factors. The results suggest that, rather than having direct causal impacts on student outcomes, foreclosure events stem from economic stress in the family, stress that also leads to lower test scores and spottier attendance for the children of such families. In addition, foreclosures may lead indirectly to more substantial negative impacts on student outcomes if the affected student changes schools during the school year as a result of the foreclosure, since we find that such school changes—independently of foreclosures—have sizable negative effects on our measures of student performance. The degree to which these relationships are causal is uncertain, however.

Although a completed foreclosure almost certainly precipitates a change of residence for the student, under Boston's school assignment policies a residential move by a student does not necessarily mean that the student must change schools. Therefore, we can control separately for residential moves and school changes in our analysis. Interestingly, residential moves alone exert negligible effects on student outcomes, and therefore the policy that (in some cases) allows students to stay in the same school following a residential move may reduce the cost of such moves relative to policies that more strongly link school assignment to residential location.

The focus of this paper is on students who were directly affected by foreclosure; in future work, we also hope to examine the indirect spillover effects of the foreclosure crisis on the academic performance of students in schools and neighborhoods with high rates of foreclosures and transience.

The next section of the paper briefly reviews the literature on the effect of foreclosures on school children. We then describe the student performance and residential foreclosure data that we use in our empirical analysis and briefly discuss three important institutional factors: the composition of Boston's housing stock, the foreclosure process, and Boston's school assignment policy. This is followed by a section presenting econometric estimates of the

relationship between academic performance and foreclosures. Linear regression estimates are presented first, to explore the partial correlations between student performance, foreclosure activity, residential moves, school changes, and other factors. Specifications that better control for student heterogeneity, and that arguably come closer to identifying causal relationships, are then presented. These are followed by a section examining the sensitivity of our econometric results to different treatments of residential property ownership and the stages of the foreclosure process. The paper concludes with a discussion of the implications of the econometric results and directions for future research.

II. Related Literature

Several groups of researchers are studying the impact of foreclosures on school children. Dastrup and Betts (2012) report results for the San Diego Unified School District, examining the incidence of foreclosure and the relationship between mortgage delinquencies among parents who are owner-occupants of single-family houses in San Diego and the test scores of their children. They find that "financial stress at home, manifest in a mortgage default, decreases elementary student math scores while increasing absenteeism." (p. 1). The "three-city study" includes researchers at the Urban Institute, NYU, and the University of Baltimore looking at foreclosures and public school students in Washington, D.C, New York City, and Baltimore, respectively.[3] These researchers have documented the prevalence of foreclosures across neighborhoods and schools as well as students' residential moves and changes among schools associated with foreclosures. They have not yet published results relating individual student test scores to foreclosures, aside from an "exploratory" regression using the Baltimore data.[4]

Beyond the limited research directly relating foreclosures and school performance, a more extensive literature relates school performance to family economic disruption, residential moves, and school changes. Within this literature, some papers have found that parental job loss and family income instability are adversely associated with school performance measures

[3] The following url provides links to the reports of the three-city study:
http://www.neighborhoodindicators.org/activities/projects/effects-foreclosure-children-and-schools

[4] See Pettit and Comey, 2012, Table A6.

such as suspensions, grade repetition, or grade point average—especially for children from low-income or low-wealth families, who are typically less able to insulate their incomes from shocks.[5] Other research documents a negative link between behavior problems or grade repetition and multiple residential moves during childhood, controlling for a variety of socio-demographic risk factors.[6] In addition, several papers provide evidence suggesting that children who move frequently from one school to another may experience subsequent drops in performance.[7] While the latter findings may merely reflect (fixed or transitory) differences between students who change schools frequently and relatively stable students, Schwartz and Steifel (2012) have recently enriched this literature by identifying plausibly causal effects of school mobility on student performance. They find that the impact of a current-year school change on standardized test scores depends on whether the change is structural—the student reaches the terminal grade of his/her school (negative impact)—or nonstructural (other school switching, which may have a positive impact) and also depends on the number of previous moves.

Family economic distress, residential moves, and school changes are all potential pathways through which a home foreclosure may affect a student's academic performance. Literature suggests that each of these situations may negatively affect test scores and other school outcomes. In the current analysis, we are unable to examine economic distress and foreclosures separately because foreclosures are almost our only indicator of economic distress; we can, however, directly observe residential moves and school changes separately from foreclosures and hence quantify their impacts somewhat independently.

III. Data and Sample

The Boston Public Schools (BPS) provided data for school years 2003/2004 through

[5] See Kalil and Ziol-Guest 2008, Kalil and Wightman 2010, Stevens and Schaller 2011, Hill et al. 2010, Rege, Telle, and Votruba 2011.

[6] Wood et al. 1993.

[7] Lovell and Isaacs (2008) say that the National Assessment of Educational Progress "has found that students with two or more school changes in the previous year are half as likely to be proficient in reading as their stable peers. Math performance can also suffer, as a government study found mobile third grade students to be nearly twice as likely to perform below grade level in math, as compared with those who had not changed schools" (p. 1).

2009/2010 relating to both individual students and schools. In the current analysis we make use of the student information only and use school fixed effects to control for school characteristics. The student data include Massachusetts Comprehensive Assessment System (MCAS) test scores, characteristics such as age, race, and gender, programmatic information such as grade, school, special needs and limited-English-proficient status, and performance measures other than test scores such as attendance and suspensions. In addition, the data report students' home addresses and indicate whenever parents report a change of address to BPS; they also report changes in the school each child attends and the reasons for any school change, whether over the summer or during the school year.

Using students' home addresses, we merged the BPS data with City of Boston Assessor data and Warren Group data, pertaining, respectively, to ownership status and foreclosure-related events, both at the level of the individual property. The Assessor data indicate whether a property was owner-or renter-occupied as of January 1 each year, based on whether the owner claimed the residential exemption, which provides a property-tax discount for owner-occupants in the city. The Warren Group compiles information on foreclosures in Massachusetts and other states from public records. They record all property deeds in the City of Boston as well as a set of foreclosure-related events, including foreclosure petition, foreclosure auction, and foreclosure deed.[8] These records include the names of the people involved in the various transactions, such as the buyer and seller when a property changes hands.

Using BPS information on the student's address and the last name(s) of her parent(s) or guardian, together with the Assessor data and Warren Group data, we define home-ownership status for the student based on two criteria: (1) whether the address was designated as owner-occupied in the Assessor data for the appropriate time period and (2) whether the student's or parent/guardian's name matched (with reasonably accuracy, as described in the appendix) the name(s) attached to the property in both the Assessor data and in the Warren Group data. Addresses designated by the Assessors' office as owner-occupied that do not also satisfy the

[8] Many properties have multiple auctions scheduled; that is, an initial foreclosure auction may be postponed several times. To avoid double-counting, we use data on actual completed foreclosure auctions compiled directly from public records by Lauren Lambie-Hanson and Timothy Lambie-Hanson rather than the information on scheduled auctions provided in the Warren Group data; see Lambie-Hanson and Lambie-Hanson (2012).

name-match requirements are defined as rentals in our sample.[9] Table 1 reports the owner and structure type mix of housing units occupied by BPS students as of October of each school year 2004/2005 through 2009/2010.[10] The table indicates that the predominant structure type in our sample is a 2–3 family house, and just under 20 percent of our BPS families are identified as owners of their respective residences, with the remainder classified as tenants.[11]

A. Foreclosures in Boston

We provide a brief description of the mortgage process before describing our foreclosure data. A necessary precondition for the foreclosure process to begin is that the owner of a property misses mortgage payments and becomes delinquent on the loan. The lender may then file a petition in the Land Court, seeking authority to foreclose on the property; typically, this is done only after three missed payments.[12] At that point, the borrower can avoid further foreclosure action by either becoming current on the mortgage, selling the property and paying the mortgage balance, negotiating with the lender to sell the property for less than the outstanding mortgage principal (a "short sale"), or renegotiating the terms of the mortgage. If none of these events occurs, then typically after six to nine months an auction is held and the property is sold.

After matching the Warren Group data with BPS student addresses, we observe 6,096 student-year observations with a foreclosure petition during the school years 2004/2005 through

[9] The name-match method is conservative in attributing ownership; see the data appendix for further discussion. In section IV.D. below, we compare name-match results with those using the Assessor definition of owner-occupancy.

[10] The student address-change data are incomplete for the 2003/2004 school year; the first date for which we observe addresses for all students is June 30, 2004.

[11] Data from the U.S. Census Bureau's American Community Survey for 2010 indicate that the fraction of owner-occupied housing units in Boston, at almost one-third, exceeds the fraction observed among the BPS families. The discrepancy likely implies that non-BPS families—that is, families with no school-age children or families whose children attend private schools—are more likely to own their home than are BPS families, although it also may reflect the conservative name-matching procedure that we employ in defining home-ownership status. BPS families are also more concentrated in 2–3 family structures and less likely to live in either apartment buildings or single-family units than the average Boston household, based on the overall composition of Boston's housing stock.

[12] A 90-day right-to-cure law was enacted in Massachusetts in May 2008, requiring servicers to notify borrowers of their delinquency (after 3 or 4 missed payments) and allowing an additional 90 days for the borrower to become current before the servicer could file a foreclosure petition with the Land Court.

2009/2010 and 1,984 foreclosure auctions.[13] Figure 1 displays the time pattern of foreclosure petitions and auctions in our BPS sample, and also breaks out foreclosure petitions by ownership status of the property as defined above. Almost one-fifth (19 percent) of the foreclosure petitions observed for BPS students occur in properties that we classify as owned by the student's parent or guardian.[14] Almost three-quarters (72 percent) of the petitions occur in two-or three-family properties.

The filing of a foreclosure petition is generally preceded by some event that triggers missed mortgage payments. Job loss, uninsured medical expenses, divorce, or some other shock to the borrower's financial stability are common reasons for mortgage default. We do not directly observe owners' financial shocks in our data. But after a shock and with a lag, depending on their other resources, owners may begin to miss payments on their mortgage and eventually receive a foreclosure petition from their lender. Thus, foreclosure petitions are both the first signal of financial distress and the first indicator of foreclosure activity that we observe in our data. A foreclosure petition is eventually followed by an auction if the borrower is unable to take any of the steps needed to avoid the auction. An auction indicates that the financial shock was sufficiently deep and persistent to prevent the borrower from becoming current on the mortgage and/or that the mortgage was so far underwater that a short-sale or refinancing was impossible.

For tenants in properties undergoing foreclosure, the timing of impacts would seem to be different: In the mid-to-late 2000s, most tenants were evicted at the time of foreclosure, and such evictions, while admittedly disruptive, would perhaps be the only direct consequence of foreclosure. Changes in both U.S. and Massachusetts law created protections after 2009 that allowed tenants to remain in their units until their lease expired or, in the absence of a lease, with 90 days' notice, delaying and possibly mitigating the shock caused by an eviction.

In Boston and surrounding cities, however, these hypothesized differences in the timing

[13] Each school year is defined as the period between July 1 of a given year and June 30 of the following year. As noted above, we do not have addresses for all students during the 2003/2004 school year and hence cannot associate foreclosure events with them that year.

[14] Using the owner designations in the Assessors' data, almost three-eighths (37 percent) of the foreclosures observed for BPS students occur in *properties* that are owner-occupied.

of foreclosure's impact on owners and renters may be blurred by the predominance of small, multi-family structures, many of which are owner-occupied.[15] As Table 1 indicates, 58 percent of students in our sample reside in one-to-three-family structures. Of the single-family structures, over half (56 percent) are owner-occupied (based on name-match). Among the two- and three-family structures where our students reside, the Assessor data indicate that the property owner lives in the structure in over half (53 percent) of the cases. Thus, the students in two-to-three-family structures are either owner-occupants themselves (21 percent of cases), share the structure (as tenants) with the owner (32 percent), or live with other tenants in a non-owner-occupied two-to-three-family structure (47 percent).[16]

Conventional wisdom suggests that many tenants in owner-occupied double- or triple-deckers are relatives of the owners and therefore might also be affected by an economic shock to the owner. Even without a family relationship, the loan default may be precipitated by a shock to a tenant family's finances that results in missed rent payments that the landlord experiences as an income shock. In this case, the "economic shock" runs from the tenant to the owner, as the failure to collect rent causes the owner to miss mortgage payments (possibly with a lag, depending on the owner's other resources) and eventually face a foreclosure petition. In one-, two-, or three-family properties, losing one tenant's rent could represent a substantial income shock (or shock to the rental revenue stream dedicated to paying the mortgage) for the owner.

These alternative hypotheses relating to Boston's idiosyncratic housing stock do not eliminate the expectation that foreclosure activity would have different effects on owner-occupiers than it would on tenants. However, they weaken the supposition that children of owner-occupants would experience the effects of foreclosure both earlier and more severely than would children of tenants. We investigate these differences in our econometric estimation.

[15] Note that the "multifamily structures" we refer to here are not condominiums with one owner per unit, but rather have a single owner for all the units. Therefore, if a 3-family structure is "owner-occupied," it means that the owner resides in one of the units and rents out the other units to tenants. As Table 1 indicates, condominiums are considered a distinct "structure type."

[16] The Assessor data tell us whether the *property* is owner-occupied; our name match data indicate whether the student's parent or guardian is the owner, that is, whether the student's own housing *unit* is owner-occupied.

B. School Assignment in Boston

For school assignment purposes in kindergarten through eighth grade, the city of Boston is divided into three broad zones (West, East, and North). When entering BPS for the first time (in kindergarten or after a move into the city or from a private school), parents express their preferences among schools within the zone where their residence is located. BPS assigns students via a computer algorithm based on those preferences, sibling and walk-radius priority, and available space (with ties broken by a random number assigned by the computer). The West zone includes 26 elementary and middle schools, the North zone has 32, and the East zone 35 elementary and middle schools. The 35 BPS high schools are citywide, rather than zone-specific, schools; that is, residential location does not restrict the high schools a student may attend (although sibling and walk-radius priority still enter the assignment process). One K-8 school and one middle school are also citywide.

In our analysis, we distinguish between "structural" and "nonstructural" school changes. A structural school change consists of a move from one ("departing") school to another ("receiving") school over the summer, following completion of the highest grade level offered at the departing school or acceptance into an exam school in grade 7. Most Boston public schools are either elementary schools offering grades K–5, middle schools (grades 6–8), or high schools (grades 9–12). A small but growing number of schools offer grades K–8, a few are Early Learning Centers (ELCs) offering grades K–1, a couple offer grades 6–12, and there are three exam schools offering grades 7–12. (Exam schools admit students beginning in either grade 7 or grade 9.) Therefore, the majority of structural school changes reflect transitions between grades 5 and 6 (elementary to middle school) or transitions between grades 8 and 9 (middle or K–8 to high school), although some reflect exit from an ELC after grade 1 or entrance into an exam school in grade 7.

A nonstructural school change is defined as any other change from one school to another, either over the summer or during the school year. Nonstructural school changes may reflect parental requests for a change, recommendations by school personnel that a student would be better served at another BPS school, or a residential move from one of Boston's three

school zones to another.[17] A residential move across zone lines requires a student to change to a school in the zone of the new home address unless the parents assume responsibility for transportation to the old school. (In addition, a small number of school changes occur when schools shut down or merge during our sample period; we control for these school-change "discontinuities" separately from structural and nonstructural school changes.)

About 12 percent of BPS students changed residence each school year; see the right-hand column of Table 2. Another 10 percent moved out of the Boston Public Schools and we cannot track them. Three-fifths of students' within-BPS residential moves were "in-zone," that is, the family lived within the same broad school zone before and after the move. As the assignment policy implies, out-of-zone moves were more likely to be followed by or associated with a nonstructural school change than were in-zone residential moves, especially for elementary and middle school students (compare the middle and lower panels of Table 2).

Overall, because of Boston's fairly loose link between residential location and school assignment, only 23 percent of students whose families moved experienced a nonstructural school change in the same school year, or 34 percent if we also include nonstructural school changes in the school year following the residential move.[18] That is, two-thirds to three-quarters of movers do not experience a potentially disruptive, nonstructural school change under Boston's assignment policy.

C. **Matched Student-Address Sample**

After matching the student-year observations provided by the BPS to property address records from the Warren Group and the City of Boston Assessing Department, we have the dataset we use for the econometric estimation presented in the next section. A detailed

[17] In addition, nonstructural school changes may reflect a residential move into Boston from another school district, or a transition from a private school into a Boston public school. However, our preferred specification below, which includes a lagged dependent variable, counts very few such moves into BPS from outside Boston or from a private school as a nonstructural change, because observations are limited to students for whom we have a BPS test score or attendance record for the school year before the school change occurs. Therefore, such school changes would be observed only for students returning to BPS after a brief absence to attend a private school or another school district.

[18] Furthermore, 7 percent of nonmovers experienced a nonstructural school change in the school year in which they did not move, and 15 percent experienced a nonstructural school change in that or the following school year. Thus, moving elevates the incidence of nonstructural school changes by only 15 to 20 percentage points.

description of the data sources and the matching process is provided in the appendix. We start with 350,329 student-year observations with BPS test-score and/or attendance information, after dropping observations not matched to school change information, not matched to property address information, or not satisfying other criteria described in the appendix. This dataset includes students in all grades and forms the basis of the sample for the attendance regressions. Students in Massachusetts public schools take math and English language arts (ELA) standardized MCAS exams every spring in grades 3 through 8 and also in grade 10;[19] we have 172,828 student-year observations on test scores. The actual number of observations in the regressions is substantially smaller because we use leads and lags and because of missing values in the data for for the regression variables.

Table 3 presents sample means of the variables used in regressions that include lags of the dependent variables and lags and leads on foreclosure petitions (our preferred specifications, as discussed in Section IV). We use MCAS test scores that have been normed to the state distribution by grade, test, and year. The mean math score value of -0.47 shown in Table 3 indicates that students in our sample on average scored 0.47 state-wide standard deviations lower than the state-wide mean; similarly, the -0.58 mean ELA value indicates that students in our sample had a mean score that was 0.58 state-wide standard deviations lower than the state-wide mean.

IV. School Performance and Foreclosures

Based on standard models of student performance and expecting that foreclosures disrupt students' lives similarly to other family economic shocks documented in the literature, we estimate regressions with student performance measures (test scores or attendance) as the dependent variable and, at a minimum, foreclosure measures and student characteristics as explanatory variables. In order to incorporate and examine some of the details regarding school assignment and the housing mix discussed in the preceding section that are idiosyncratic to Boston, we also (sequentially) control for students' housing and tenure type, school

[19] Annual ELA testing did not occur in grades 5, 6, and 8 until the 2005-06 school year; similarly, math tests were not administered in grades 5 and 7 until 2005/2006.

characteristics, and neighborhood characteristics, and we add as explanatory variables measures of residential moves and structural and nonstructural school changes.

For example, a child's family could experience an economic shock that might directly impair the child's school performance and also cause the parents to miss mortgage payments (if they are owners) or miss rent payments (if they are tenants); the latter income shock for the landlord would raise the odds of missed mortgage payments as well. Missed mortgage payments can eventually lead to a foreclosure petition. Because we cannot observe economic shocks affecting BPS students' families directly, but can see the ones accompanied by foreclosure petitions at the student's home address, the estimated coefficients on foreclosure petition are likely to reflect at least some effects of economic shocks on student performance. If a foreclosure (or economic shock) also leads to a residential move, the move may further affect school performance. In addition, if the student is not in high school and the move is across zone boundaries, the move may precipitate a nonstructural school change, with potential further effects on performance. Another child's family may be subject to similar economic stresses that do not lead to a foreclosure at home; those stresses may also lead to a residential move and perhaps a nonstructural school change. Our estimating strategy attempts to investigate and quantify each of these potential influences on students'school performance even though we can observe economic shocks only insofar as they are proxied by foreclosure.

A. Partial Correlations

Our first step in investigating these hypothesized influences on students' school performance and summarizing the patterns in our data is to estimate simple regressions of students' test scores on indicators of foreclosure and selected other variables. Although the regression coefficients are partial correlations and do not necessarily indicate the presence of a causal relationship, they show the extent to which there is an empirical association between test scores and foreclosure.

The indicator of foreclosure that we use in this initial analysis is whether a mortgage servicer filed a petition for foreclosure during the academic year (July 1 – June 30) on a property address where the student resided. As described in Section III.A. above, a foreclosure petition

represents the first step in the foreclosure process, and not all petitions lead to a foreclosure auction as the final outcome. Using the petition event as the indicator of foreclosure results in a larger set of affected students than would obtain if we used the auction event. Further, the petition event constitutes an arguably strong signal of economic distress in the student's family, which may or may not result in a foreclosure auction, an eventual residential move, and/or school change.[20] To gain further insight into the mechanisms by which foreclosures affect student outcomes, we run alternative versions of our models in which we add indicators of foreclosure auctions; these results are discussed in Section IV.E. below.

Column 1 in Tables 4 and 5 reports the coefficient on the foreclosure petition variable during the current school year (July 1-June 30) when the dependent variable is either the math (Table 4) or ELA (Table 5) standardized test score in March-April or May of the same school year, controlling only for school year and test grade. Column 1 of Table 6 does the same, using the dependent variable "percent attended," an alternative measure of school performance indicating the fraction of the full school year that the student attended. Taken together, these regressions indicate that school performance is significantly lower for student-year observations in which a petition for foreclosure is filed on the student's home address. Because the dependent variables (in Tables 4 and 5) are standardized test scores, the coefficients on the foreclosure petition indicator variables may be interpreted in terms of standard deviations of the state-wide distribution of scores. For example, the coefficient value of -0.21 on the foreclosure petition for the math score regression indicates that BPS students who lived at an address on which a foreclosure petition was filed scored, on average, roughly a fifth of a state-wide standard deviation lower on the math MCAS exam than did those BPS students whose homes were not subject to a foreclosure petition. In the attendance regressions, the estimated coefficient of -1.4 indicates that a foreclosure petition is associated with 2½ fewer days of attendance, based on the standard 180-day school year in Massachusetts.

The ensuing columns of these tables add a lag and a lead on the foreclosure petition

[20] Dastrup and Betts (2012) use a measure of mortgage default that indicates the month in which an owner first missed a mortgage payment, a variable they observe only for those who eventually enter foreclosure and not for all who miss a payment.

variable and then add groups of control variables such as student characteristics (some of which are time-varying), characteristics of the properties at students' home addresses, and, eventually, fixed effects for individual schools and for Census tracts of students' home addresses (Table 7 lists the specific variables included in each group of control variables). The right-most column of Tables 4–6 also includes an indicator of a residential move (occurring at any point in the academic year) and multiple indicators of school changes (two for nonstructural changes, corresponding to changes occurring over the summer or between October 1 and June 30, one for structural school changes, and one for "discontinuous" school changes, including mergers and closings.

Three key patterns emerge across the columns of Tables 4–6: (1) The estimated coefficients on foreclosure variables are generally consistently negative and significantly different from zero, even though (2) their size and significance weakens as we add control variables across the columns, and (3) residential moves and nonstructural school changes are strongly negatively related to school performance.

The fact that the estimated coefficients on a lead as well as on a lag on foreclosure are consistently negative raises issues related to timing and dynamics. One interpretation of this coefficient pattern—and particularly the large size of the coefficient on next year's foreclosure in the test-score equations—is that a foreclosure petition is a symptom or result of economic stress affecting a student's family, and that such stress predates the foreclosure petition. Indeed, economic difficulties undoubtedly precede the first delinquent payment. A bank's decision to begin the foreclosure process (petition) typically lags the first delinquent payment by many months, and further lags occur between the foreclosure petition and foreclosure auction and/or deed.[21] Also telling is the general drop in size (and, in some cases, significance) of estimated coefficients on the foreclosure petition variables, as control variables, especially student characteristics, are added. These two types of results suggest that the negative correlations observed in column (1) reflect in part the fact that students who experience foreclosure are

[21] During most of our 2004–2010 study period and especially as the prevalence of foreclosures exceeded 1.5 percent in 2008 and thereafter, more Boston mortgage loans were 90 days or more delinquent and not in foreclosure than were in foreclosure. In January 2010, the fraction of loans 90 days or more delinquent and not in foreclosure peaked at 4 percent, while the fraction in foreclosure was 2.5 percent . Source: LPS data tabulated by Lauren Lambie-Hansen.

students who have lower test scores for any number of reasons, including educational disadvantages, family economic disadvantages, housing type and neighborhood location, or school attended. Some of the same factors that lead to lower test scores appear to be also associated with increased risk of mortgage default. That is, the negative relationships reflect cross-sectional differences among students as much or more than changes in performance for any one student from one year to the next as foreclosures occur.

School fixed effects are added as controls in column 5 of the tables. In these models, the regressions are controlling for the tendency of some schools persistently to have students with characteristics—including high foreclosure rates and high mobility rates—that might create negative spillovers onto other students' scores. Therefore, some of the indirect effects of foreclosures and school changes are controlled for by the school effects. The school fixed effects may also reflect students' own unobserved characteristics to the extent that students tend to select into schools in ways that create differences across schools in the distribution of student characteristics that may affect academic performance. The inclusion of the school effects results in a substantial decrease in the magnitude of the foreclosure petition coefficients.

Census tract fixed effects, which control for potential spillover effects of persistent neighborhood characteristics such as residential transience and social capital, are added in column 6. The census tract effects may also control for unobserved student characteristics to the extent that families tend to be sorted across neighborhoods in ways that create differences in the distribution of attributes such as income, wealth, and vulnerability to unemployment and other financial shocks. It is surprising that the inclusion of the census tract fixed effects results in little change in the estimated foreclosure petition coefficients, but it suggests that sorting on "unobservables" at the tract level is not strong.

In the final column of the three tables, dummy variables indicating a residential move and various types of school changes are also included. These results indicate that residential moves and nonstructural school changes are negatively related to test scores and attendance. Not surprisingly, nonstructural school changes over the summer appear to be less disruptive than those during the school year in terms of both test scores and attendance. This highlights a potential channel through which economic shocks may affect student performance. Shocks may

decrease student performance by precipitating a residential move and school change during the academic year. In the sections below, we explore the extent to which specifications that control for heterogeneity in unobserved student characteristics suggest that these empirical relationships may represent a causal connection, and we also discuss how Boston's school assignment policies may attenuate the connection between school changes and residential moves induced by foreclosures or economic shocks.

It is interesting to compare the estimated coefficients shown in column 7 of Tables 4–6 with the coefficients on the other control variables included in these regressions. Table 8 reports these estimated coefficients; the three columns in this table correspond to column 7 of Tables 4, 5, and 6. Economic disadvantage—as indicated by eligibility for free or reduced-price lunch ("low-income"), not being an owner-occupant, or living in subsidized housing ("exempt")—is negatively associated with test scores. A student's own educational disadvantages, such as having limited-English-proficiency (LEP), especially in the first year of LEP[22] or requiring special education services, are also strongly negatively associated with test scores. The patterns are similar for attendance, except that the characteristics low-income and LEP are positively related to attendance. Note also that the magnitude of the estimated coefficients on foreclosure events is generally much smaller than the magnitude of the coefficients associated with a student's requiring LEP or special education services, and also smaller than the effects associated with changing schools during the school year.

B. Lagged dependent variable

As discussed above, the regression results in Tables 4–6 do not offer rigorous proof that foreclosures lead to reduced test scores. Although the specifications with the most complete set of covariates control for many characteristics that might be correlated with academic performance, there are undoubtedly other characteristics that are correlated with test scores that we do not observe in our data and therefore cannot control for in our analysis. If these variables

[22] The "no scaled score" variable refers to students who take the test and have a recorded raw score (and hence are included in our data) but for whom no scaled score is computed because they are not "counted" in the school and district test results. This is done in the first year a student is labeled LEP and also when illness causes the student to miss some of the testing days or something else goes wrong in the testing process.

are also correlated with the likelihood of foreclosure, then the estimated effects of foreclosure petitions on outcomes will be subject to omitted variables bias. While the bias could run in either direction, we are particularly concerned about factors that would predict both lower test scores (or lower attendance) and a higher risk of foreclosure, since omission of such factors would result in an overestimation (in absolute magnitudes) of any negative effects of foreclosure on outcomes.

To address this concern, and also to examine the dynamics of students' test performance, estimates from test score and attendance regressions that include lagged values of the dependent variable—either the lagged test score or lagged attendance—are reported in Tables 9–11. The sample size is smaller with this specification than in Tables 4–6 because creating the lagged dependent variable requires that we have at least two adjacent years of observations for each included student. Because math and ELA are tested in grades 3–8 and 10, we have no one-year-lagged score for 10th graders; hence we substitute the two-years-ago 8th-grade test score as the lagged dependent variable in these cases.[23]

In the model that includes the lagged test score, for example, the lagged score controls for the student characteristics, educational inputs, and any other factors that "determined" the previous score and, to a great degree, purges the estimates of their cross-sectional aspect.[24] The other explanatory variables, which are the same as those in Tables 4-6, are therefore intended to capture influences on the current score that are not reflected in the lagged score.[25] The earlier models, by excluding the lagged score, effectively assumed that the test score in any given period reflects the impact of current inputs only, where student fixed effects (when included) captured the contribution of fixed student inputs. Analogous statements apply to models of

[23] We anticipated that the two-year lag for 10th graders would obtain a smaller coefficient than the one-year lag (for all other test grades), but instead the two coefficients were very similar in magnitude.

[24] The estimated coefficients on the control variables in the LDV formulation are generally smaller than those shown in Table 8 for the partial correlation specification, implying that cross-sectional differences play a substantial role in the Table 4–6 estimates. Nonetheless, while fewer than in the partial correlation version, a majority of the coefficients on these "levels" control variables are significantly different from zero in the LDV specification.

[25] Note that this model is not equivalent to a differences formulation, which would have the change in test score as the dependent variable and changes in characteristics as explanatory factors. As explicated in Todd and Wolpin (2003), this model reflects the notion that the achievement level at a given time is cumulative, reflecting both the history of past education inputs (captured by the lagged score) and current inputs. A differences model assumes that the coefficient on the lagged dependent variable equals one, while this model estimates a value for this coefficient.

attendance that include lagged attendance as an explanatory factor.

In these equations for math and attendance but not for ELA, the estimated coefficient on foreclosure is negative and significantly different from zero when controlling only for school year, grade, and the appropriate lagged dependent variable—either lagged test score or lagged attendance (column 1). Lags and leads on foreclosure are added in column 2. The lag, lead, and contemporaneous foreclosure coefficients are all negative and significantly different from zero in the math score regression, but in the ELA score regression only the coefficients on the lag and lead (and not on contemporaneous foreclosure) are significantly different from zero. As control variables are added to the test-score regressions (in columns 3–7), the lead foreclosure coefficient continues to be significant in the math score regression, but the contemporaneous and lag coefficients eventually become insignificant and have point estimates substantially smaller in magnitude. In contrast, in the ELA score regression only the lagged foreclosure variable remains significantly negative as control variables are added. The difference in the timing of the respective foreclosure effects on ELA and math test performance is intriguing. It may be that the math test score depends to a greater extent on recent classroom learning than does the ELA test score, and so is affected by the disruption of economic shocks and foreclosure early on. The ELA test score may depend more on cumulative progress in spelling, vocabulary, and reading comprehension, and so appears to be affected with a lag. While the timing of effects differs between math and ELA test scores, the magnitude of the estimated significant coefficient is quite similar after all control variables are included in column (7), at -0.05. This magnitude is also quite similar to the estimated coefficient on the control variable "entering low-income status"—another flag of economic distress—also associated with a 0.05 drop in math or ELA test score.[26]

In the attendance equations, contemporaneous foreclosure retains its negative and significant coefficient as other variables are added; the lag also enters significantly, while the lead does not, although its estimated coefficient is negative. This makes intuitive sense. Although economic stress may affect learning well before a foreclosure petition is filed, one

[26] In the math score equation with lagged dependent variable, the estimated coefficient on "entered low-income status" is -0.049, with a standard error of 0.012; in the ELA equation, the corresponding figures are -0.051 and 0.013.

would expect the impact of foreclosure on attendance to be concentrated during the foreclosure process, which can extend after the petition. It is interesting to contrast these results with those in the partial correlations attendance regression (Table 6), where all three foreclosure coefficients continue to be sizable, negative, and significant even as controls are added. This again indicates that the estimated coefficients on foreclosures in the partial correlations specifications actually reflect the contributions of fixed cross-sectional differences and/or variation in other prior educational inputs, contributions that are captured by the lagged dependent variable in the current model.

Even controlling for the previous year's test score or attendance, nonstructural school changes that occur between October 1 and June 30 have negative and significant effects on test scores and attendance, although the coefficients are smaller here than in Tables 4–6. These coefficients are considerably larger than the estimated coefficients on any of the foreclosure variables; thus one way that foreclosures may reduce test scores in this context is by precipitating a nonstructural school change. Been et al. (2011) discuss the potential link between foreclosures and school changes. Almost 5 percent of students with a foreclosure petition in a given year also experience a nonstructural school change between October and June of the same school year; this fraction is only slightly higher than the fraction of non-foreclosure-affected students experiencing a nonstructural school change in any given school year (4 percent); see Table 12.

The main path by which a foreclosure results in a nonstructural school change is via a residential move associated with the foreclosure. Each year, about 12 percent of all BPS students change addresses, but that figure jumps to 17 percent for those receiving a foreclosure petition (memo line of Table 12). Eleven percent of students experiencing both a foreclosure petition and a residential move in a given school year also experience a nonstructural school change between October and June of the same school year, while only 3 percent of students experiencing a foreclosure but not a move (and 4 percent of all students) exhibit an October-to-June nonstructural school change in the same year (Table 12). Among students not experiencing foreclosure, the fraction of residential movers who experience a nonstructural school change between October and June of the same school year is a slightly higher 14 percent. "Petition-and-

move" students are also more likely than other students to experience a nonstructural school change in the school year following the petition; see the lower panel of Table 12.[27]

While these data indicate a higher incidence of nonstructural school changes for students experiencing a foreclosure petition than not, this relationship is likely less strong than in many other cities because of Boston's public school assignment policies. As noted earlier, all Boston high schools are citywide, and non-high-school students are not required to change schools following a residential move unless the new address falls within a different schooling zone (among West, East, and North) than the previous address. This policy likely reduces the effect of foreclosures on student performance relative to alternative policies that might require school changes in a greater fraction of residential moves.

Structural school changes also have significant negative effects on test scores (but not attendance). Structural changes occur only in grades 2 (from Early Learning Centers), 6 (from K–5 elementary schools, 7 (into exam schools), and 9 (into high schools); of these, only grades 6 and 7 are tested in math and ELA. Because we control for school fixed effects, test-score differentials for students entering exam schools in grade 7 should be picked up by the school fixed effect, as will test-score differentials for students moving from elementary to middle schools in grade 6. However, a small fraction of students who finish K–5 elementary schools move over to K–8 schools in grade 6 instead of moving into middle school. Students who attend first an elementary school and then a middle school have lower test scores, on average, than students who attend K–8 schools; thus this negative estimated coefficient appears to be picking up the fact that the few students who transition into K–8 schools from K−5 elementary schools attain lower test scores than the existing 6th graders at the K–8 schools, other things being equal.[28] Residential moves, by contrast, have no independent effect on test scores in the year in which they occur, but are associated with lower attendance, perhaps in part because the

[27] Taken together, 15 percent of students with a foreclosure petition and a residential move in a school year experience a nonstructural school change between October and June of the same or following school year, while only 7 percent of students with a petition and no move (and 7 percent of all students) do so; see the lower panel of Table 12.

[28] The idiosyncratic nature of the few structural school changes that "identify" this coefficient probably imply that our estimates are not comparable to those of Schwartz and Stiefel (2012), although they are similar in magnitude. Schwarz and Stiefel look only at ELA test scores and find OLS effects around -0.05 test-score standard deviations associated with a structural school change this year.

disruption of the move itself may directly cause some absences.

C. Student Fixed Effects

Although conditioning on a lag of the dependent variable controls to some extent for unobserved differences among students, a more direct way to control for such differences is to include student fixed effects. In models that include such effects, the estimated coefficients are driven solely by within-student (across-time) covariances, and the effect of student characteristics that do not vary over time are absorbed by the constant term. Tables 13–15 present regression estimates including student fixed effects. For these regressions, we restrict the sample to students for whom we have at least three observations; in addition, we eliminate race and sex from the control variables because they do not vary over time. In column (1), the estimated coefficient on foreclosure petition in the ELA equation is positive and significantly different from zero when controlling for nothing else; in the math and attendance equations, foreclosures appear to have no significant relationship with contemporaneous performance. These patterns continue across the columns as leads, lags, and additional controls are added. That is, foreclosures, whether in the current year, the prior year, or the following year, appear to have no relationship with math test scores or attendance. Even in these student-fixed-effect regressions, nonstructural school changes between October and June appear to have significant negative effects on math scores, ELA scores, and attendance, but the estimated coefficients are smaller than in the corresponding models involving lagged dependent variables.

Because the fixed effects estimator is driven by the within-student (across-time) covariances of the measured variables, correct measurement of the timing of changes in observed variables is of critical importance for consistency of the estimator. Unfortunately, the nature of the foreclosure process (and the administrative reporting thereof) makes it difficult to pinpoint the timing of relevant underlying events—such as a parent's job loss—that we expect to influence student outcomes. Foreclosure may evolve gradually from an initial economic shock to a first missed mortgage payment, a later foreclosure petition, and, if it goes that far, an eventual foreclosure auction. Our inclusion of the lead, lag, and contemporaneous indicators of foreclosure petitions serves as a means of capturing the potentially protracted and time-varying

effects of the foreclosure process on student outcomes, although the strategy is imperfect. The foreclosure process itself, and its impact on the student, might stretch across several school years, beginning even before we first observe a student or continuing beyond our last observation. Hence, the student fixed effect, which is intended to absorb only the non-time-varying influences, may be contaminated by our use of foreclosure indicators that are flawed in capturing the precise timing of the true latent effects of the foreclosure process. As a result, we return to the lagged dependent variable versions as our preferred specification as we explore several additional topics in the next subsection of the paper.

D. Owners versus Tenants

As discussed in section II.A. above, we expect that foreclosures have different effects on students whose parents are the owner-occupants of foreclosed properties than on students whose families rent in properties that undergo a foreclosure. For owner-occupants, a severe income shock may precipitate missed mortgage payments and eventually lead to a foreclosure petition. But a majority of BPS students live in one-to-three-family properties in which tenants' family relationships with the owner or a tenant income shock causing missed rent payments could also lead fairly directly to owner default. We have not yet found evidence to support or disprove such possible connections.

To see whether our data shed light on differential effects of foreclosures based on housing tenure, we generate a dummy variable indicating whether a student's parents are the owner-occupants in a one-to-three-family structure or of a condo.[29] We interact that dummy with the lag, lead, and contemporaneous foreclosure petition variable for that property. Using name-matched owner-occupants, we found that the parents were owner-occupants in about one-fifth (19 percent) of foreclosure petitions affecting BPS students' homes between 2004 and 2010. We re-estimate the lagged dependent variable regressions shown in column 7 of Tables 9–11 and include those interactions along with the original lag, lead, and contemporaneous

[29] As noted earlier, we have two methods of identifying owner-occupants: one that relies on a name match between the owner listed in the Assessor's records and the last name of the student's parent or guardian, and another that assumes the student's parents are the owner if the Assessor's records say the property was owner-occupied when the student resided there.

foreclosure petition variables. The estimated coefficients on the non-foreclosure variables in the equations are virtually unchanged, the non-interacted foreclosure variables' coefficients change very little, and none of the owner-interacted foreclosure variables obtains an estimated coefficient significantly different from zero. See columns 2, 5, and 8 in Table 16, where columns 1, 4, and 7 repeat the earlier estimates with no owner interactions.[30]

In the next set of regressions, we expand the definition of owners to include BPS students living in any property labeled by the Assessor's office as an owner-occupied one-to-three-family structure or an owner-occupied condo; this definition includes—in addition to those tallied above—tenant units in owner-occupied 2–3 family structures as well as any other occupants in properties the Assessor says are owner-occupied but where the owner's name does not match the parent or guardian's name. Under the Assessor definition, 37 percent of all foreclosure petitions at BPS student addresses are owner-occupied. The test-score regression estimates, as with the name-match owner interactions, are very little changed and none of the estimated coefficients on owner-petition interactions are significantly different from zero (columns 3, 6, and 9 in Table 16). Consequently, we find no support for the notion that the effects of foreclosure are different in either their timing or their strength for the children of owners as opposed to the children of renters. These results contrast markedly with those of Dastrup and Betts (2012), who find no effects of mortgage default on test scores of children whose parents are renters and who thereafter restrict their analysis to single-family owner occupants. This difference may be due partly to the fact that Dastrup and Betts's (2012) default variable (first missed payment) is dated earlier in the foreclosure process and partly to the very different mix of property and tenure types in the Boston and San Diego school districts.[31] As noted earlier, the financial stability of owners of Boston's many two-to-three-family structures may be particularly vulnerable to missed rent payments resulting from their tenants' economic difficulties.

[30] Only the estimated coefficients on the foreclosure-related variables are shown in Table 16.

[31] According to Dastrup and Betts's (2012) Table 1, one-half of the housing units occupied by SDUSD students are in single-family structures and over 40 percent of student homes are owner-occupied. Contrast these figures with the 15 percent single-family share and the 18 percent owner-occupancy rate among BPS students (both shown in our Table 1 above).

E. Foreclosure Auctions

The foreclosure variable included in the regressions is a dummy variable equal to one for a student in any school year during which his/her home address experienced a foreclosure petition. To investigate whether the next and final stage in the foreclosure process—the foreclosure auction—has an additional or differential relationship with student performance, we re-estimate the lagged dependent variable regressions, adding, alternatively, two indicators of foreclosure auction. Many properties cured in some way after receiving a foreclosure petition and did not continue in the process all the way to a foreclosure auction. Our first approach is to create a dummy variable indicating properties that were served by petitions and that eventually experienced a foreclosure auction; this "petition-to-auction" variable is turned on in the school year in which the *petition* occurred. This variable indicates that at least one-third of foreclosure-petitioned properties did not subsequently cure—they went to auction. We include the lead, lag, and contemporaneous petition-to-auction variable.

The estimated coefficients on the non-foreclosure-related variables are not changed by inclusion of this additional foreclosure indicator. The estimated coefficients on the foreclosure petition variables are affected in some cases, but the petition-to-auction variables never obtain coefficients significantly different from zero. See columns 2, 5, and 8 of Table 17 (columns 1, 4, and 7 repeat the earlier estimates with petitions only). In the math test score equation, when controlling for petition-to-auction, the estimated coefficient on the lead of petition becomes slightly weaker, while the negative coefficient on contemporaneous petition is slightly larger (the coefficient on the contemporaneous petition variable is not significant when the petition-to-auction variables are not included). These estimates suggest that, in addition to the year before, foreclosure petitions that do not subsequently go to auction have a modest negative relationship with math scores in the year of the petition, while those that do go to auction are unrelated to scores in the year of the petition.[32] For ELA test scores, there are no marked effects of including the petition-to-auction variables, although their inclusion reduces the significance of the lag on petition. In the attendance equation, including the petition-to-auction variables reduces the

[32] Note that the full effect of a petition that goes to auction is the sum of the coefficients on the two variables. That is, the petitions that go to auction are included in the petitions variable.

significance of all the petition variables below 90 percent.

The second approach to investigating the effects of a foreclosure auction is to include a dummy variable for foreclosure auction, which is turned on in the school year in which the auction occurs, provided the student was living at the relevant address around the time of the auction.[33] There are three reasons this second auction dummy variable would not be turned on for students for whom the prior "petition-to-auction" variable *was* turned on: (1) Within our sample it typically took six to nine months for a property to go from petition to auction; therefore, some students had moved out of a foreclosure-petition property well before it went to auction and so we did not turn on the auction variable for them. A justification for this choice is that these students were not expected to be further affected by the property's changing hands well after they had already moved out. (2) If a student in our sample experienced a foreclosure petition and subsequently left the BPS dataset prior to the school year in which the auction occurred or (3) if the auction occurred after the 2009/2010 school year but was observable in the foreclosure data through 2011, the auction variable would not be turned on in the regression observations.[34]

Columns 3, 6, and 9 of Table 17 report results for the foreclosure auction dummy variable. As in columns 2, 5, and 8, the estimated coefficients on the lagged dependent variable, residential moves, and structural and nonstructural school changes are not affected by inclusion of the auction dummy (results other than foreclosure variables are not shown). Compared with the petitions-alone specification, the only marked change in the test-score estimates with inclusion of the auction dummy is that in the ELA equation (column 6), the coefficient on the lag on petitions becomes smaller and not significantly different from zero and the coefficient on the lead of petitions becomes larger and marginally significant, while the estimated coefficient on the lag on foreclosure auction is negative and significantly different from zero. Given that the auction occurs after the petition, it is surprising that the negative relationship between

[33] One-half of the foreclosure auctions in our sample occurred in the same school year as the associated petition. As the appendix notes, we use a 90-day window to judge whether a student was affected by an auction. That is, we assume students were affected by an auction only if they were living at the property address at the time of the auction or if they moved out no more than 90 days before the auction.

[34] On the other hand, some students may have experienced auctions in the early years of the panel, but the petition occurred before we observe the student.

foreclosures and ELA test scores is strongest in the year after an auction. However, if there is reason to believe that ELA test scores represent longer-term cumulative (that is, lagged) effects of any shock, as we conjectured above, these results are not inconsistent with our previous findings; furthermore, the negative coefficient on next year's petition is similar to (although smaller than) the math-score results. For attendance, inclusion of the auction variables eliminates even the modest significance of the petition variables. At the same time, a foreclosure auction seemingly has a large and significant negative relationship with attendance in the year of the auction, a result consistent with disruption associated with the move such an auction is likely to force. Taken together, these supplemental analyses including foreclosure auctions provide only weak evidence in support of any additional relationship between a foreclosure auction and performance beyond that of a petition.

F. Are School Changes Exogenous?

In considering the implications of our results for school assignment policy, a crucial consideration is whether nonstructural school changes are exogenous. If nonstructural school changes are not exogenous, then the estimated coefficients will not be reliable guides to the causal effect of nonstructural school changes on test scores. Endogeneity might arise if the underlying factors leading to a student changing schools are only imperfectly controlled for by the variables we condition on in the regressions. If the unobserved factors leading to school changes also affect test performance, then the estimated school change coefficients may reflect the effect of these factors as well as the direct effects of the school changes. For example, if student behavioral problems (unobserved in our data) contribute to both a higher probability of nonstructural school changes and negatively affect test performance, then the estimated nonstructural school change coefficients will tend to be larger in magnitude than the true causal effect of nonstructural school changes.

The potential endogeneity of school changes is explored in Tables 18 and 19, which add one-year lags and leads of the school change variables to the lagged dependent variable and student fixed effects specifications reported in column 7 of Tables 9-11 and 13-15. In the lagged dependent variable specification (shown in Table 18), inclusion of the lag and lead of

nonstructural school changes that occur between October 1 and June 30 does not greatly change the magnitude of the contemporaneous coefficients, but the lead coefficients are negative and highly significant for both of the test score regressions and also for the attendance regression. A similar pattern emerges in the student fixed effects specification (reported in Table 19), although in this case the lead of the academic year nonstructural school change variable is not statistically significant in the ELA test score regression. The significance of the coefficients on the lead of the October 1-to-June 30 nonstructural school change variable may indicate that transient unobserved factors are both adversely affecting students' current test scores and contributing to students changing schools in the following year. In the test score regressions, the contemporaneous coefficients are substantially larger than the lead coefficients. This pattern is consistent with October 1-to-June 30 nonstructural school changes having a negative causal effect on test scores, but it is also possible that the contemporaneous coefficient estimates are driven largely by unobserved factors. Policy makers should be cautious in drawing conclusions regarding the causal effect of academic year nonstructural school changes on students' test scores based on these results. There may be a substantial negative causal effect, but that is not certain.

V. Discussion and Conclusion

The regression results in this paper suggest that foreclosures at home have a small negative association with individual Boston Public School students' test scores and attendance, controlling for the student's previous-year test score or attendance. The timing of this association is mixed, with math scores mostly lower in the year before a foreclosure petition, ELA scores weaker in the year after a foreclosure petition, and attendance lower in the year of or year following the petition.[35]

The results also show a potentially important indirect effect of foreclosures on performance via the residential moves and associated nonstructural school changes they may

[35] Over a student's successive testing years, the full relationship between a foreclosure petition and test score would reflect the combination of the lead, contemporaneous, and lag coefficients, carried from one year to the next via the lagged dependent variable.

induce. While a foreclosure petition at a student's home is associated with a drop of about 0.05 standard deviation in the student's math or ELA test score (with the year in which that drop occurs differing between math and ELA), a nonstructural school change between October and June typically reduces a test score by about one-fifth of a standard deviation.[36] Thus, a student who experiences a foreclosure followed by an October-to-June nonstructural school change would see an average drop of about one-quarter of a standard deviation in his/her test score. However, this larger effect applies only to the 5 percent of students receiving petitions who also experience a nonstructural school change in that school year. As noted earlier, this fraction is only slightly higher than the corresponding fraction of non-foreclosure-affected students who experience an October-to-June nonstructural school change, and the smallness of the difference is probably largely attributable to Boston's school assignment policy. That policy implies that most students in Boston undergoing residential moves, whether associated with a foreclosure or not, do not experience a nonstructural school change. Possibly because we cannot separately observe residential moves motivated by economic duress from other residential moves, the estimated effect of residential moves on test scores is insignificant. As a result, our estimates indicate that moves add to the eventual impact of foreclosures only insofar as they lead to nonstructural school changes. As we discuss above, however, the apparent negative impact of nonstructural school changes on test scores may not be causal.

The estimated importance of the lead on foreclosure petition, the small size of the estimated foreclosure coefficients, and the mixed timing across equations all suggest that the "foreclosure impacts" we quantify above may more accurately be construed as the impacts of an economic stress such as parental job loss that occurs before or along with the foreclosure (and may be its cause). Using only BPS data, we have almost no independent indicators of such economic stress affecting individual students and their families that would allow us to distinguish foreclosures from other stresses. While it is not unreasonable to assume that different types of serious economic stress (job loss, illness, divorce, foreclosure) have similar

[36] The estimated coefficient on nonstructural school change October 1-June 30 is -0.21 in the LDV math score equation and -0.18 in the ELA test-score regression.

effects on test scores,[37] the magnitude of our estimate based on observed foreclosure petitions is likely a downward biased estimator of the true economic stress effect, because we are not "observing" (and hence cannot control for) any of the other serious stresses that are likely to reduce the test scores of BPS students who experience them.

Additional research is needed to sort out the channels through which foreclosures (and other economic shocks) may affect student school performance. Some of the reported results merit further investigation, particularly the school and neighborhood fixed effects. For example, Ananat, Gassman-Pines, and Gibson-Davis (2011) have documented that job losses in a community are associated with poorer academic outcomes and that the effects are stronger for low-income and low-achieving students. Using tract-level data from the American Community Survey, we plan to investigate the relationships between neighborhood characteristics such as high joblessness and the neighborhood fixed effects used in the current paper's results, also considering neighborhood characteristics that might ameliorate the pass-through from stresses to scores. Related to neighborhood effects is the fact that other researchers have documented negative effects of student mobility or churn on test scores of all students in an affected school, not just those who move.[38] We plan to examine this question for nonstructural school changes in general and also for those associated specifically with foreclosures; similarly, we intend to investigate the spillover effects of concentrations of students experiencing foreclosure petitions (even if they do not change schools) on test scores of other students in the same school.

[37] Indeed, our LDV estimates of the relationship between test scores and entering low-income status—the only other "flag" of economic distress—are similar in magnitude to our estimates of foreclosure's relationship with test scores.

[38] See, for example, Raudenbush, Jean, and Art (2011).

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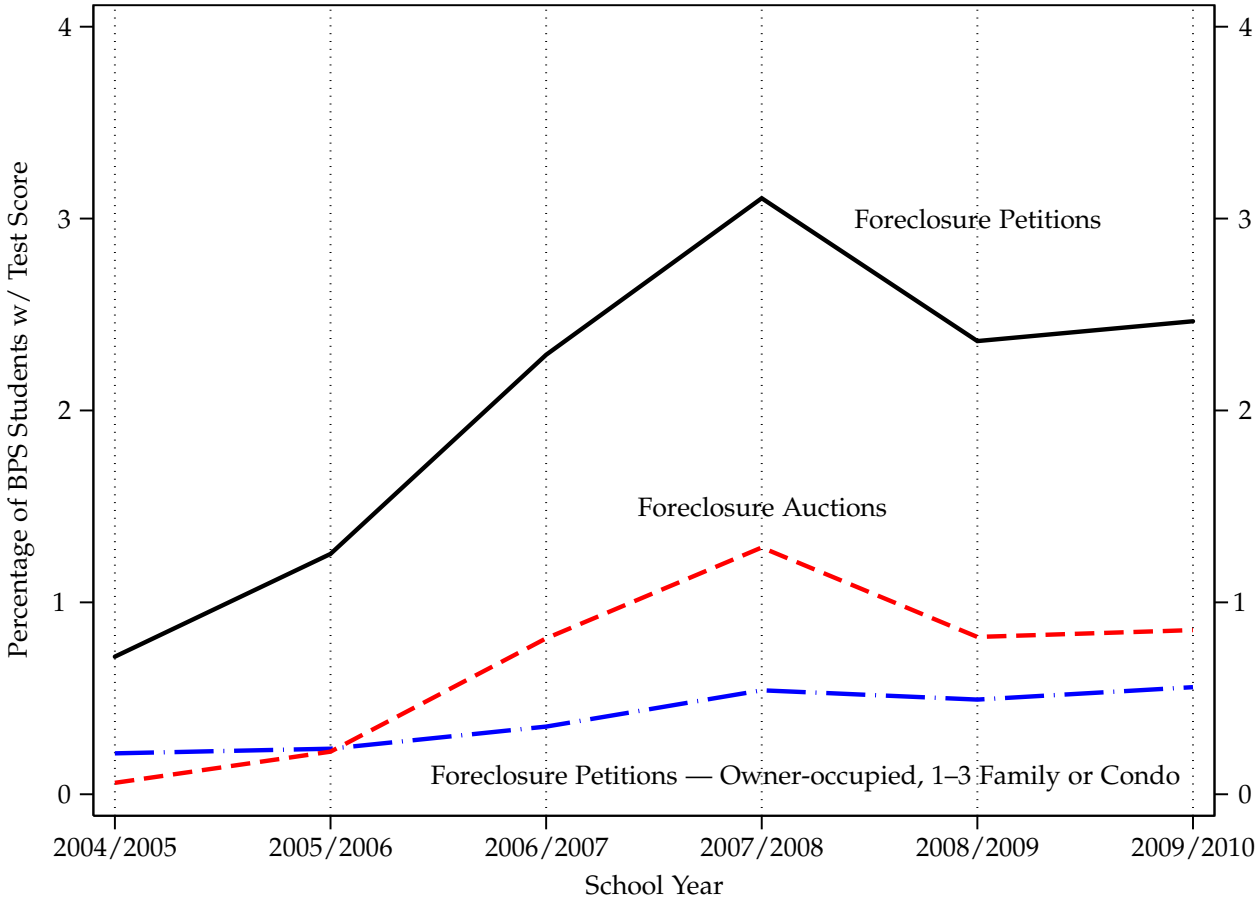
Table 1: Structure Type and Owner Status of BPS Student Addresses (October)

Structure Type	Owner: No		Owner: Yes		Total	
	No.	%	No.	%	No.	%
Single Family	19,183	6.5	24,088	8.1	43,271	14.6
2–3 Family	102,047	34.4	26,366	8.9	128,413	43.3
4+ Family	48,081	16.2	530	0.2	48,611	16.4
Condo	3,490	1.2	1,608	0.5	5,098	1.7
Exempt	59,274	20.0	0	0.0	59,274	20.0
Other	11,455	3.9	162	0.1	11,617	3.9
Total	243,530	82.2	52,754	17.8	296,284	100.0

Notes: "Owner" here and elsewhere represents our name-match definition of ownership, which is described in the data appendix. *Source*: Authors' calculations based on data from the Boston Public Schools and the City of Boston Assessing Department.

Figure 1: Time Trends of Foreclosure Events Among BPS Students



Notes: This figure presents the percentage of BPS students in each school year with a math or ELA test score who experience a foreclosure event. *Source:* Authors' calculations based on data from the Boston Public Schools, the City of Boston Assessing Department, the Warren Group (foreclosure petitions), and Lauren and Timothy Lambie-Hansen (foreclosure auctions).

Table 2: Residential Moves and School Changes in the Same School Year

Residential Moves	Percentage of Students (Row %)			Number of Students	Percentage of Students (Column %)
	No School Change	Nonstructural School Change	Structural School Change Only		
All BPS Students	79.6	9.1	11.3	296,646	100.0
No Residential Move	81.6	7.2	11.2	261,841	88.3
Any Residential Move	65.3	23.2	11.5	34,805	11.7
Move out of zone	55.8	33.1	11.1	13,906	4.7
Move within zone only	71.6	16.6	11.8	20,899	7.0
High School Students	77.2	6.9	15.9	120,848	100.0
No Residential Move	78.5	5.8	15.6	109,720	90.8
Any Residential Move	64.1	17.3	18.6	11,128	9.2
Move out of zone	62.6	19.2	18.2	4,354	3.6
Move within zone only	65.0	16.1	18.8	6,774	5.6
Non-high-school Students	81.3	10.6	8.1	175,798	100.0
No Residential Move	83.7	8.2	8.0	152,121	86.5
Any Residential Move	65.9	25.9	8.2	23,677	13.5
Move out of zone	52.7	39.4	7.9	9,552	5.4
Move within zone only	74.8	16.8	8.4	14,125	8.0

Notes: "Move out of zone" includes students with two or more residential moves, of which at least one was out of zone. "Nonstructural School Change" includes students with two or more school changes, of which at least one was nonstructural. *Source*: Authors' calculations based on data from the Boston Public Schools.

Table 3: Means of Select Variables by Sample Definition

	Math	ELA	% Attended
Normed Math Score	-0.472		
Normed ELA Score		-0.584	
% Attended			91.829
Grade 1			0.009
Grade 2			0.099
Grade 3	0.007	0.009	0.098
Grade 4	0.151	0.207	0.092
Grade 5	0.185	0.192	0.087
Grade 6	0.141	0.143	0.089
Grade 8	0.157	0.215	0.100
Grade 9			0.119
Grade 10	0.165	0.084	0.107
Grade 11			0.096
Grade 12			0.013
2006/2007	0.289	0.254	0.251
2007/2008	0.284	0.293	0.248
2008/2009	0.282	0.289	0.244
Male	0.512	0.514	0.516
White	0.135	0.134	0.134
Asian	0.090	0.088	0.082
Hispanic/Latino	0.334	0.334	0.339
Native American	0.004	0.004	0.004
Multi-race	0.005	0.005	0.006
LEP	0.148	0.152	0.169
Special Education	0.215	0.221	0.218
Low-income	0.778	0.787	0.757
Entered Low-income Status	0.066	0.064	0.072
Exited Low-income Status	0.083	0.079	0.097
No Scaled Score (ELA)	0.004	0.004	
No Scaled Score (Math)	0.003	0.002	
Owner	0.184	0.181	0.185
Condo	0.016	0.016	0.016
1 Family	0.141	0.140	0.144
4+ family	0.165	0.167	0.165
Exempt	0.219	0.222	0.213
Other	0.038	0.037	0.039
Residential Move	0.127	0.129	0.124
Nonstructural School Change (Jul 1 to Oct 1)	0.058	0.062	0.066
Nonstructural School Change (Oct 1 to Jun 30)	0.025	0.025	0.032
Structural School Change	0.135	0.132	0.157
School-Change Discontinuity	0.012	0.007	0.013
L.Foreclosure Petition	0.018	0.019	0.017
Foreclosure Petition	0.023	0.024	0.022
F.Foreclosure Petition	0.026	0.026	0.025
L.Foreclosure Petition $\rightarrow$ Auction	0.007	0.007	0.006
Foreclosure Petition $\rightarrow$ Auction	0.008	0.008	0.008
F.Foreclosure Petition $\rightarrow$ Auction	0.009	0.008	0.009
L.Foreclosure Auction	0.006	0.007	0.005
Foreclosure Auction	0.008	0.009	0.008
F.Foreclosure Auction	0.010	0.009	0.009
Observations	57517	55315	129538

Notes: The sample means are computed using our preferred regression samples; see Tables 9–11. L. and F. represent lags and leads, respectively. The grade variables represent test grade for the test score columns and student grade for the % attended column. *Source:* See Figure 1.

Table 4: Partial Correlations — Math

	Normed Math Score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.Foreclosure Petition		-0.216***	-0.099***	-0.114***	-0.068**	-0.063**	-0.060**
		(0.028)	(0.024)	(0.025)	(0.023)	(0.023)	(0.023)
Foreclosure Petition	-0.213***	-0.202***	-0.080***	-0.096***	-0.066***	-0.062**	-0.059**
	(0.023)	(0.024)	(0.022)	(0.022)	(0.020)	(0.020)	(0.020)
F.Foreclosure Petition		-0.205***	-0.071***	-0.089***	-0.076***	-0.074***	-0.071***
		(0.024)	(0.021)	(0.021)	(0.019)	(0.019)	(0.019)
Residential Move							-0.024**
							(0.009)
Nonstructural School Change (Jul 1 to Oct 1)							-0.072***
							(0.013)
Nonstructural School Change (Oct 1 to Jun 30)							-0.314***
							(0.020)
Structural School Change							0.111***
							(0.014)
Constant	-0.524***	-0.545***	-0.239***	-0.326***	0.523***	0.488***	0.452***
	(0.010)	(0.011)	(0.016)	(0.018)	(0.020)	(0.034)	(0.035)
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Demographics	No	No	Yes	Yes	Yes	Yes	Yes
Student Characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	No	No	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	No	No	Yes	Yes	Yes
Census Tract Fixed Effects	No	No	No	No	No	Yes	Yes
School-Change Discontinuity	No	No	No	No	No	No	Yes
R-squared	0.012	0.015	0.295	0.302	0.402	0.407	0.410
Observations	115517	82051	81865	81829	81829	81829	81829

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. *Source:* See Figure 1.

Table 5: Partial Correlations — ELA

	Normed ELA Score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.Foreclosure Petition		-0.143***	-0.072**	-0.092***	-0.057*	-0.050*	-0.046+
		(0.030)	(0.026)	(0.026)	(0.025)	(0.025)	(0.024)
Foreclosure Petition	-0.127***	-0.130***	-0.042+	-0.063**	-0.041+	-0.037+	-0.035
	(0.025)	(0.026)	(0.022)	(0.022)	(0.021)	(0.021)	(0.021)
F.Foreclosure Petition		-0.151***	-0.058**	-0.079***	-0.073***	-0.071***	-0.067***
		(0.025)	(0.021)	(0.021)	(0.020)	(0.020)	(0.020)
Residential Move							-0.039***
							(0.010)
Nonstructural School Change (Jul 1 to Oct 1)							-0.072***
							(0.014)
Nonstructural School Change (Oct 1 to Jun 30)							-0.284***
							(0.022)
Structural School Change							0.090***
							(0.015)
Constant	-0.616***	-0.666***	-0.059***	-0.141***	0.554***	0.521***	0.496***
	(0.010)	(0.012)	(0.016)	(0.018)	(0.019)	(0.034)	(0.035)
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Demographics	No	No	Yes	Yes	Yes	Yes	Yes
Student Characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	No	No	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	No	No	Yes	Yes	Yes
Census Tract Fixed Effects	No	No	No	No	No	Yes	Yes
School-Change Discontinuity	No	No	No	No	No	No	Yes
R-squared	0.005	0.006	0.343	0.349	0.422	0.426	0.429
Observations	115909	82198	82019	81983	81983	81983	81983

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. *Source:* See Figure 1.

Table 6: Partial Correlations — % Attended

	% Attended						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.Foreclosure Petition		-1.092***	-0.793***	-0.875***	-0.769***	-0.722**	-0.644**
		(0.236)	(0.233)	(0.233)	(0.224)	(0.224)	(0.221)
Foreclosure Petition	-1.385***	-1.122***	-0.822***	-0.940***	-0.803***	-0.782***	-0.702***
	(0.185)	(0.201)	(0.200)	(0.200)	(0.193)	(0.193)	(0.192)
F.Foreclosure Petition		-0.937***	-0.662***	-0.797***	-0.669***	-0.651***	-0.577***
		(0.184)	(0.183)	(0.183)	(0.175)	(0.175)	(0.173)
Residential Move							-0.893***
							(0.091)
Nonstructural School Change (Jul 1 to Oct 1)							-1.375***
							(0.124)
Nonstructural School Change (Oct 1 to Jun 30)							-5.644***
							(0.275)
Structural School Change							3.384***
							(0.167)
Constant	92.815***	92.300***	94.542***	93.320***	96.480***	95.645***	94.580***
	(0.078)	(0.097)	(0.149)	(0.160)	(0.185)	(0.325)	(0.334)
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Demographics	No	No	Yes	Yes	Yes	Yes	Yes
Student Characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	No	No	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	No	No	Yes	Yes	Yes
Census Tract Fixed Effects	No	No	No	No	No	Yes	Yes
School-Change Discontinuity	No	No	No	No	No	No	Yes
R-squared	0.091	0.093	0.129	0.140	0.216	0.221	0.236
Observations	228178	129664	129664	129598	129598	129598	129598

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. *Source:* See Figure 1.

Table 7: Defining Groups of Control Variables

Variable Group	Variables	Time Observed
Student Demographics	race, sex	June
Student Characteristics	LEP, special education, low-income, changes in low-income status between year t and year $t+1$, no scaled score	October
Owner and Property Type	owner, property type (single family, 2–3 family, 4+ family, exempt, other)	Previous June 30
Student Grade	Student grade dummies	October
Test Grade	MCAS test grade dummies	Time of test
School Year	school year dummies	
School Fixed Effects	BPS school ID dummies	March
Census Tract Fixed Effects	Census tract ID dummies	Previous June 30

Notes: "no scaled score" included in test score regressions only.

Table 8: Partial Correlations — Displaying (Almost) All Coefficients

	(1) Math	(2) ELA	(3) % Attd.
Male	0.041***	-0.195***	-0.146*
	(0.008)	(0.009)	(0.070)
White	0.313***	0.224***	-1.473***
	(0.016)	(0.016)	(0.138)
Asian	0.739***	0.372***	1.268***
	(0.018)	(0.019)	(0.150)
Hispanic/Latino	0.126***	0.063***	-1.568***
	(0.011)	(0.011)	(0.100)
Native American	0.055	0.060	-1.086+
	(0.063)	(0.066)	(0.570)
Multi-race	0.106*	0.040	-1.342***
	(0.051)	(0.050)	(0.388)
LEP	-0.362***	-0.754***	1.845***
	(0.012)	(0.013)	(0.096)
Special Education	-0.744***	-0.927***	-1.686***
	(0.011)	(0.012)	(0.098)
Low-income	-0.123***	-0.171***	0.275*
	(0.012)	(0.012)	(0.112)
Entered Low-income Status	-0.147***	-0.153***	-0.122
	(0.015)	(0.015)	(0.142)
Exited Low-income Status	0.025*	0.043***	-1.213***
	(0.011)	(0.012)	(0.119)
No Scaled Score (ELA)	-0.329***	-1.763***	
	(0.049)	(0.053)	
No Scaled Score (Math)	-1.069***	-0.159*	
	(0.046)	(0.072)	
Test Grade 3	-0.105***	-0.109***	
	(0.021)	(0.021)	
Test Grade 4	0.009	-0.056**	
	(0.020)	(0.020)	
Test Grade 5	-0.008	-0.077***	
	(0.019)	(0.020)	
Test Grade 6	-0.032*	0.026+	
	(0.013)	(0.015)	
Test Grade 8	0.012	-0.006	
	(0.008)	(0.009)	
Test Grade 10	0.232***	0.132***	
	(0.015)	(0.016)	
Student Grade 1			-1.728***
			(0.246)
Student Grade 2			-0.036
			(0.147)
Student Grade 3			0.323*
			(0.145)
Student Grade 4			0.530***
			(0.144)
Student Grade 5			0.543***
			(0.143)
Student Grade 6			-1.450***
			(0.155)
Student Grade 8			0.162
			(0.108)

Student Grade 9			-2.296***
			(0.178)
Student Grade 10			0.626**
			(0.219)
Student Grade 11			0.397+
			(0.224)
Student Grade 12			-5.330***
			(0.592)
2006/2007	-0.022***	0.010	0.285***
	(0.006)	(0.007)	(0.062)
2007/2008	-0.008	0.059***	0.560***
	(0.007)	(0.007)	(0.069)
2008/2009	-0.063***	0.024**	-0.186*
	(0.008)	(0.008)	(0.076)
Owner	0.093***	0.085***	1.547***
	(0.012)	(0.012)	(0.094)
Condo	-0.018	0.035	-0.004
	(0.032)	(0.033)	(0.259)
1 Family	0.022	0.008	0.624***
	(0.014)	(0.013)	(0.106)
4+ family	-0.048***	-0.033*	-0.080
	(0.013)	(0.014)	(0.116)
Exempt	-0.055***	-0.052**	-0.541***
	(0.016)	(0.016)	(0.138)
Other	-0.036	-0.016	-0.455*
	(0.022)	(0.022)	(0.197)
Residential Move	-0.024**	-0.039***	-0.893***
	(0.009)	(0.010)	(0.091)
Nonstructural School Change (Jul 1 to Oct 1)	-0.072***	-0.072***	-1.375***
	(0.013)	(0.014)	(0.124)
Nonstructural School Change (Oct 1 to Jun 30)	-0.314***	-0.284***	-5.644***
	(0.020)	(0.022)	(0.275)
Structural School Change	0.111***	0.090***	3.384***
	(0.014)	(0.015)	(0.167)
School-Change Discontinuity	-0.086**	-0.078*	-0.828*
	(0.032)	(0.033)	(0.383)
L.Foreclosure Petition	-0.060**	-0.046+	-0.644**
	(0.023)	(0.024)	(0.221)
Foreclosure Petition	-0.059**	-0.035	-0.702***
	(0.020)	(0.021)	(0.192)
F.Foreclosure Petition	-0.071***	-0.067***	-0.577***
	(0.019)	(0.020)	(0.173)
Constant	0.452***	0.496***	94.580***
	(0.035)	(0.035)	(0.334)
School Fixed Effects	Yes	Yes	Yes
Census Tract Fixed Effects	Yes	Yes	Yes
R-squared	0.410	0.429	0.236
Observations	81829	81983	129598

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. *Source:* See Figure 1.

Table 9: Lagged Dependent Variable — Math

	Normed Math Score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.Foreclosure Petition		-0.059**	-0.035+	-0.037+	-0.026	-0.025	-0.025
		(0.020)	(0.020)	(0.020)	(0.019)	(0.019)	(0.019)
Foreclosure Petition	-0.064***	-0.055**	-0.027	-0.030+	-0.021	-0.021	-0.021
	(0.015)	(0.018)	(0.018)	(0.018)	(0.017)	(0.017)	(0.017)
F.Foreclosure Petition		-0.083***	-0.052**	-0.056***	-0.053**	-0.055***	-0.053**
		(0.017)	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)
Residential Move							0.006
							(0.008)
Nonstructural School Change (Jul 1 to Oct 1)							-0.009
							(0.012)
Nonstructural School Change (Oct 1 to Jun 30)							-0.208***
							(0.018)
Structural School Change							-0.050***
							(0.012)
L.Normed Math (L2 for 10th graders)	0.808***	0.805***	0.723***	0.721***	0.675***	0.672***	0.672***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Constant	-0.028***	-0.028***	0.003	-0.023*	0.143***	0.136***	0.167***
	(0.007)	(0.008)	(0.010)	(0.011)	(0.013)	(0.022)	(0.023)
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Demographics	No	No	Yes	Yes	Yes	Yes	Yes
Student Characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	No	No	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	No	No	Yes	Yes	Yes
Census Tract Fixed Effects	No	No	No	No	No	Yes	Yes
School-Change Discontinuity	No	No	No	No	No	No	Yes
R-squared	0.672	0.669	0.688	0.689	0.708	0.710	0.710
Observations	73023	57580	57535	57517	57517	57517	57517

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. *Source:* See Figure 1.

Table 10: Lagged Dependent Variable — ELA

	Normed ELA Score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.Foreclosure Petition		-0.082***	-0.061**	-0.067**	-0.053*	-0.049*	-0.050*
		(0.023)	(0.022)	(0.022)	(0.021)	(0.021)	(0.021)
Foreclosure Petition	-0.015	-0.019	0.008	0.002	0.004	0.005	0.005
	(0.018)	(0.021)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
F.Foreclosure Petition		-0.045*	-0.020	-0.026	-0.026	-0.025	-0.025
		(0.019)	(0.019)	(0.019)	(0.018)	(0.018)	(0.018)
Residential Move							0.001
							(0.009)
Nonstructural School Change (Jul 1 to Oct 1)							0.001
							(0.013)
Nonstructural School Change (Oct 1 to Jun 30)							-0.179***
							(0.022)
Structural School Change							-0.065***
							(0.014)
L.Normed ELA (L2 for 10th graders)	0.763***	0.766***	0.656***	0.653***	0.615***	0.613***	0.612***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Constant	-0.142***	-0.118***	0.009	-0.014	0.180***	0.191***	0.222***
	(0.011)	(0.012)	(0.014)	(0.014)	(0.016)	(0.025)	(0.026)
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Demographics	No	No	Yes	Yes	Yes	Yes	Yes
Student Characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	No	No	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	No	No	Yes	Yes	Yes
Census Tract Fixed Effects	No	No	No	No	No	Yes	Yes
School-Change Discontinuity	No	No	No	No	No	No	Yes
R-squared	0.612	0.617	0.650	0.651	0.666	0.667	0.668
Observations	73711	55419	55340	55315	55315	55315	55315

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. *Source:* See Figure 1.

Table 11: Lagged Dependent Variable — % Attended

	% Attended						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.Foreclosure Petition		-0.467*	-0.365+	-0.373+	-0.368+	-0.344+	-0.320+
		(0.197)	(0.196)	(0.197)	(0.193)	(0.194)	(0.192)
Foreclosure Petition	-0.520***	-0.426*	-0.327+	-0.357*	-0.346*	-0.338*	-0.309+
	(0.154)	(0.174)	(0.174)	(0.174)	(0.172)	(0.172)	(0.172)
F.Foreclosure Petition		-0.181	-0.087	-0.126	-0.119	-0.114	-0.096
		(0.154)	(0.153)	(0.154)	(0.152)	(0.152)	(0.152)
Residential Move							-0.274***
							(0.079)
Nonstructural School Change (Jul 1 to Oct 1)							0.026
							(0.116)
Nonstructural School Change (Oct 1 to Jun 30)							-3.496***
							(0.262)
Structural School Change							0.261+
							(0.146)
L.% Attended	0.746***	0.744***	0.725***	0.718***	0.675***	0.672***	0.664***
	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Constant	22.738***	22.758***	25.549***	25.639***	30.936***	30.991***	31.754***
	(0.534)	(0.713)	(0.748)	(0.749)	(0.783)	(0.804)	(0.807)
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Demographics	No	No	Yes	Yes	Yes	Yes	Yes
Student Characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	No	No	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	No	No	Yes	Yes	Yes
Census Tract Fixed Effects	No	No	No	No	No	Yes	Yes
School-Change Discontinuity	No	No	No	No	No	No	Yes
R-squared	0.413	0.416	0.422	0.423	0.445	0.446	0.449
Observations	207593	129604	129604	129538	129538	129538	129538

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. *Source:* See Figure 1.

Table 12: Foreclosure Petitions and Residential Moves vs. Nonstructural School Changes

	Nonstructural School Change (This Year)				All Students
	Jul–Oct or Oct–Jun		Oct–Jun		
	No.	% of All	No.	% of All	No.
No Foreclosure Petition	26,337	9.1	11,510	4.0	290,550
No Move and No Petition	18,481	7.2	6,869	2.7	256,770
Move and No Petition	7,856	23.3	4,641	13.7	33,780
Foreclosure Petition	660	10.8	285	4.7	6,096
No Move and Petition	447	8.8	174	3.4	5,071
Move and Petition	213	20.8	111	10.8	1,025
Total	26,997	9.1	11,795	4.0	296,646

Memo:	All	No Petition	With Petition
% with Residential Move	11.7	11.6	16.8

	Nonstructural School Change (This Year or Next)				All Students
	Jul–Oct or Oct–Jun		Oct–Jun		
	No.	% of All	No.	% of All	No.
No Foreclosure Petition	33,864	17.2	13,722	7.0	197,433
No Move and No Petition	25,995	14.9	9,748	5.6	174,125
Move and No Petition	7,869	33.8	3,974	17.0	23,308
Foreclosure Petition	749	19.5	309	8.1	3,836
No Move and Petition	537	17.1	207	6.6	3,142
Move and Petition	212	30.5	102	14.7	694
Total	34,613	17.2	14,031	7.0	201,269

Source: See Figure 1.

Table 13: Student Fixed Effects — Math

	Normed Math Score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.Foreclosure Petition		-0.028	-0.026	-0.026	-0.016	-0.017	-0.018
		(0.025)	(0.025)	(0.025)	(0.024)	(0.024)	(0.024)
Foreclosure Petition	-0.008	-0.022	-0.025	-0.026	-0.020	-0.022	-0.022
	(0.017)	(0.023)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
F.Foreclosure Petition		-0.023	-0.026	-0.027	-0.020	-0.018	-0.018
		(0.020)	(0.020)	(0.020)	(0.019)	(0.019)	(0.019)
Residential Move							0.008
							(0.008)
Nonstructural School Change (Jul 1 to Oct 1)							-0.007
							(0.013)
Nonstructural School Change (Oct 1 to Jun 30)							-0.116***
							(0.019)
Structural School Change							0.045***
							(0.011)
Constant	-1.462***	-1.584***	-1.599***	-1.606***	-1.634***	-1.602***	-1.651***
	(0.047)	(0.057)	(0.060)	(0.061)	(0.061)	(0.074)	(0.075)
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	No	No	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	No	No	Yes	Yes	Yes
Census Tract Fixed Effects	No	No	No	No	No	Yes	Yes
School-Change Discontinuity	No	No	No	No	No	No	Yes
R-squared	0.028	0.026	0.035	0.035	0.083	0.087	0.089
Observations	61292	47742	47731	47727	47727	47727	47727

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The sample is restricted in all seven columns to students for whom we have at least three observations. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. *Source:* See Figure 1.

Table 14: Student Fixed Effects — ELA

	Normed ELA Score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.Foreclosure Petition		-0.012	-0.009	-0.009	-0.001	-0.001	-0.002
		(0.027)	(0.027)	(0.027)	(0.026)	(0.026)	(0.027)
Foreclosure Petition	0.049*	0.043+	0.043+	0.042+	0.047*	0.050*	0.050*
	(0.019)	(0.025)	(0.024)	(0.024)	(0.023)	(0.023)	(0.023)
F.Foreclosure Petition		0.005	0.005	0.006	0.013	0.014	0.014
		(0.022)	(0.022)	(0.022)	(0.021)	(0.021)	(0.021)
Residential Move							-0.003
							(0.009)
Nonstructural School Change (Jul 1 to Oct 1)							-0.015
							(0.014)
Nonstructural School Change (Oct 1 to Jun 30)							-0.062**
							(0.021)
Structural School Change							0.078***
							(0.013)
Constant	-1.698***	-1.662***	-1.735***	-1.723***	-1.984***	-1.949***	-2.038***
	(0.059)	(0.066)	(0.064)	(0.066)	(0.067)	(0.080)	(0.082)
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	No	No	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	No	No	Yes	Yes	Yes
Census Tract Fixed Effects	No	No	No	No	No	Yes	Yes
School-Change Discontinuity	No	No	No	No	No	No	Yes
R-squared	0.033	0.026	0.052	0.052	0.089	0.094	0.095
Observations	61889	47849	47838	47834	47834	47834	47834

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The sample is restricted in all seven columns to students for whom we have at least three observations. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. *Source:* See Figure 1.

Table 15: Student Fixed Effects — % Attended

	% Attended						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.Foreclosure Petition		-0.153	-0.152	-0.139	-0.032	-0.055	-0.040
		(0.245)	(0.245)	(0.245)	(0.240)	(0.241)	(0.240)
Foreclosure Petition	-0.120	-0.136	-0.139	-0.136	-0.085	-0.100	-0.076
	(0.185)	(0.243)	(0.243)	(0.243)	(0.238)	(0.238)	(0.237)
F.Foreclosure Petition		0.012	0.005	0.002	0.046	0.034	0.040
		(0.210)	(0.210)	(0.211)	(0.208)	(0.209)	(0.209)
Residential Move							-0.095
							(0.085)
Nonstructural School Change (Jul 1 to Oct 1)							0.054
							(0.129)
Nonstructural School Change (Oct 1 to Jun 30)							-1.952***
							(0.288)
Structural School Change							1.016***
							(0.138)
Constant	99.306***	96.374***	95.708***	95.385***	96.747***	96.863***	95.851***
	(0.363)	(0.427)	(0.441)	(0.451)	(0.478)	(0.619)	(0.633)
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics	No	No	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	No	No	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	No	No	Yes	Yes	Yes
Census Tract Fixed Effects	No	No	No	No	No	Yes	Yes
School-Change Discontinuity	No	No	No	No	No	No	Yes
R-squared	0.113	0.080	0.080	0.081	0.116	0.119	0.122
Observations	141416	92489	92489	92481	92481	92481	92481

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The sample is restricted in all seven columns to students for whom we have at least three observations. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. *Source:* See Figure 1.

Table 16: LDV Model with Owner-Foreclosure Interactions — Math, ELA, % Attended

	Normed Math Score			Normed ELA Score			% Attended		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
L.Foreclosure Petition	-0.025	-0.019	-0.019	-0.050*	-0.051*	-0.058*	-0.316+	-0.383+	-0.512*
	(0.019)	(0.020)	(0.022)	(0.021)	(0.023)	(0.026)	(0.192)	(0.210)	(0.236)
Foreclosure Petition	-0.021	-0.014	-0.003	0.004	0.004	0.003	-0.315+	-0.355+	-0.220
	(0.017)	(0.019)	(0.021)	(0.020)	(0.022)	(0.024)	(0.172)	(0.198)	(0.221)
F.Foreclosure Petition	-0.053**	-0.058**	-0.040*	-0.024	-0.024	-0.011	-0.073	-0.079	-0.203
	(0.016)	(0.018)	(0.020)	(0.018)	(0.020)	(0.023)	(0.153)	(0.176)	(0.202)
L.Foreclosure Petition*1–3 Family or Condo*Owner		-0.041			0.006			0.369	
		(0.053)			(0.057)			(0.510)	
Foreclosure Petition*1–3 Family or Condo*Owner		-0.045			0.002			0.214	
		(0.044)			(0.050)			(0.373)	
F.Foreclosure Petition*1–3 Family or Condo*Owner		0.031			0.000			0.016	
		(0.042)			(0.045)			(0.326)	
L.Foreclosure Petition*1–3 Family or Condo*Owner (Ass.)			-0.009			0.037			0.717+
			(0.041)			(0.044)			(0.399)
Foreclosure Petition*1–3 Family or Condo*Owner (Ass.)			-0.047			0.015			-0.192
			(0.037)			(0.041)			(0.346)
F.Foreclosure Petition*1–3 Family or Condo*Owner (Ass.)			-0.029			-0.031			0.419
			(0.034)			(0.037)			(0.299)
School Year									
Test Grade	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Student Grade	No	No	No	No	No	No	Yes	Yes	Yes
Student Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Owner (Ass.) and Property Type	No	No	Yes	No	No	Yes	No	No	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School-Change Discontinuity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Regressors (6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.710	0.710	0.710	0.668	0.668	0.668	0.449	0.449	0.449
Observations	57517	57517	57517	55315	55315	55315	129538	129538	129538

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. The six "Other Regressors" are as follows: the residential-move indicator, three school-change indicators, lagged dependent variable, and a constant term. *Source:* See Figure 1.

Table 17: LDV Model with Foreclosure Petitions and Foreclosure Auctions — Math, ELA, % Attended

	Normed Math Score			Normed ELA Score			% Attended		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
L.Foreclosure Petition	-0.025	-0.026	-0.019	-0.050*	-0.046+	-0.035	-0.320+	-0.281	-0.087
	(0.019)	(0.024)	(0.021)	(0.021)	(0.027)	(0.024)	(0.192)	(0.237)	(0.217)
Foreclosure Petition	-0.021	-0.040+	-0.023	0.005	-0.004	-0.006	-0.309+	-0.305	-0.159
	(0.017)	(0.021)	(0.019)	(0.020)	(0.024)	(0.021)	(0.172)	(0.209)	(0.182)
F.Foreclosure Petition	-0.053**	-0.047*	-0.051**	-0.025	-0.017	-0.031+	-0.096	-0.082	-0.109
	(0.016)	(0.020)	(0.017)	(0.018)	(0.021)	(0.019)	(0.152)	(0.175)	(0.157)
L.Foreclosure Petition → Auction		0.001			-0.010			-0.104	
		(0.038)			(0.043)			(0.384)	
Foreclosure Petition → Auction		0.053			0.024			-0.019	
		(0.034)			(0.038)			(0.354)	
F.Foreclosure Petition → Auction		-0.013			-0.023			-0.049	
		(0.033)			(0.038)			(0.325)	
L.Foreclosure Auction			-0.049			-0.087*			-0.395
			(0.034)			(0.038)			(0.377)
Foreclosure Auction			0.024			0.017			-0.901**
			(0.029)			(0.032)			(0.343)
F.Foreclosure Auction			-0.014			0.040			-0.096
			(0.028)			(0.031)			(0.279)
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Student Grade	No	No	No	No	No	No	Yes	Yes	Yes
Student Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School-Change Discontinuity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Regressors (6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.710	0.710	0.710	0.668	0.668	0.668	0.449	0.449	0.449
Observations	57517	57517	57517	55315	55315	55315	129538	129538	129538

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. L. and F. represent lags and leads, respectively. See Table 7 for information on each of the suppressed variable categories. The six "Other Regressors" are as follows: the residential-move indicator, three school-change indicators, lagged dependent variable, and a constant term. *Source:* See Figure 1.

Table 18: Robustness of School-Change Effects — Lagged Dependent Variable Model

	Normed Math Score		Normed ELA Score		% Attended	
	(1)	(2)	(3)	(4)	(5)	(6)
L.Foreclosure Petition	-0.025	-0.027	-0.050*	-0.051*	-0.320+	-0.331+
	(0.019)	(0.019)	(0.021)	(0.021)	(0.192)	(0.190)
Foreclosure Petition	-0.021	-0.021	0.005	0.005	-0.309+	-0.317+
	(0.017)	(0.017)	(0.020)	(0.020)	(0.172)	(0.170)
F.Foreclosure Petition	-0.053**	-0.049**	-0.025	-0.021	-0.096	-0.027
	(0.016)	(0.016)	(0.018)	(0.018)	(0.152)	(0.151)
L.Nonstructural School Change (Jul 1 to Oct 1)		0.010		0.003		-0.436***
		(0.012)		(0.012)		(0.102)
Nonstructural School Change (Jul 1 to Oct 1)	-0.009	-0.005	0.001	0.005	0.026	0.177
	(0.012)	(0.012)	(0.013)	(0.013)	(0.116)	(0.115)
F.Nonstructural School Change (Jul 1 to Oct 1)		-0.073***		-0.084***		-2.795***
		(0.015)		(0.016)		(0.145)
L.Nonstructural School Change (Oct 1 to Jun 30)		-0.028+		-0.047*		-1.508***
		(0.015)		(0.018)		(0.187)
Nonstructural School Change (Oct 1 to Jun 30)	-0.208***	-0.199***	-0.179***	-0.170***	-3.496***	-3.231***
	(0.018)	(0.018)	(0.022)	(0.022)	(0.262)	(0.261)
F.Nonstructural School Change (Oct 1 to Jun 30)		-0.141***		-0.109***		-4.415***
		(0.016)		(0.019)		(0.215)
L.Structural School Change		0.023*		0.019		-0.013
		(0.009)		(0.012)		(0.134)
Structural School Change	-0.050***	-0.093***	-0.065***	-0.095***	0.261+	0.181
	(0.012)	(0.013)	(0.014)	(0.016)	(0.146)	(0.153)
F.Structural School Change		0.167***		0.109***		0.365***
		(0.011)		(0.012)		(0.106)
School Year	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	No	No
Student Grade	No	No	No	No	Yes	Yes
Student Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
School-Change Discontinuity	Yes	Yes	Yes	Yes	Yes	Yes
Other Regressors (3)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.710	0.712	0.668	0.669	0.449	0.459
Observations	57517	57517	55315	55315	129538	129538

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. L. and F. represent lags and leads, respectively. The three "Other Regressors" are as follows: the residential-move indicator, lagged dependent variable, and a constant term. *Source:* See Figure 1.

Table 19: Robustness of School-Change Effects — Student Fixed Effects Model

	Normed Math Score		Normed ELA Score		% Attended	
	(1)	(2)	(3)	(4)	(5)	(6)
L.Foreclosure Petition	-0.018	-0.018	-0.002	-0.002	-0.040	-0.039
	(0.024)	(0.024)	(0.027)	(0.027)	(0.240)	(0.239)
Foreclosure Petition	-0.022	-0.021	0.050*	0.051*	-0.076	-0.100
	(0.022)	(0.022)	(0.023)	(0.023)	(0.237)	(0.236)
F.Foreclosure Petition	-0.018	-0.016	0.014	0.016	0.040	0.078
	(0.019)	(0.019)	(0.021)	(0.021)	(0.209)	(0.208)
L.Nonstructural School Change (Jul 1 to Oct 1)		0.016		0.005		-0.385**
		(0.012)		(0.013)		(0.124)
Nonstructural School Change (Jul 1 to Oct 1)	-0.007	-0.008	-0.015	-0.019	0.054	-0.508***
	(0.013)	(0.014)	(0.014)	(0.015)	(0.129)	(0.153)
F.Nonstructural School Change (Jul 1 to Oct 1)		-0.032*		-0.039*		-2.107***
		(0.015)		(0.017)		(0.172)
L.Nonstructural School Change (Oct 1 to Jun 30)		-0.025		-0.045*		-1.567***
		(0.019)		(0.022)		(0.236)
Nonstructural School Change (Oct 1 to Jun 30)	-0.116***	-0.143***	-0.062**	-0.082***	-1.952***	-3.101***
	(0.019)	(0.021)	(0.021)	(0.023)	(0.288)	(0.318)
F.Nonstructural School Change (Oct 1 to Jun 30)		-0.085***		-0.035		-2.867***
		(0.019)		(0.022)		(0.265)
L.Structural School Change		0.050***		0.067***		0.072
		(0.013)		(0.015)		(0.163)
Structural School Change	0.045***	0.075***	0.078***	0.127***	1.016***	1.015***
	(0.011)	(0.017)	(0.013)	(0.019)	(0.138)	(0.179)
F.Structural School Change		0.030**		0.016		0.101
		(0.010)		(0.011)		(0.123)
School Year	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	No	No
Student Grade	No	No	No	No	Yes	Yes
Student Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Owner and Property Type	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Census Tract Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
School-Change Discontinuity	Yes	Yes	Yes	Yes	Yes	Yes
Other Regressors (2)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.089	0.091	0.095	0.096	0.122	0.131
Observations	47727	47727	47834	47834	92481	92481

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The sample is restricted in all seven columns to students for whom we have at least three observations. L. and F. represent lags and leads, respectively. The two "Other Regressors" are the residential-move indicator and a constant term. *Source:* See Figure 1.

Data Appendix[1]

This appendix discusses the procedures used to clean and match the BPS, City of Boston Property Parcel, and Warren Group datasets.

1 Boston Public Schools Data

The BPS provided the following data for school years 2003/2004 through 2009/2010:

- MCAS Test Score Data: These data provide students' raw and scaled MCAS scores in math, ELA, and science and technology. We keep only math and ELA exams, discard 38,246 retests, and standardize the remaining raw scores within school year and grade using statewide statistics obtained from the student-level research files posted by the Massachusetts Department of Elementary and Secondary Education.

- Student Characteristics Data: These data provide programmatic variables as of October and June of each school year, such as grade, special needs and limited-English proficiency status, and performance measures other than test scores, such as attendance and suspensions. We drop students outside grades 1–12 (that is, students in pre-kindergarten, kindergarten, and special education beyond grade 12).

- Student Demographics Data: These data provide demographic variables as well as student and parent/guardian names as of the end of each school year.

- School Change Data: These data indicate the school each student was attending at any given time and the reason he or she enrolled. We use these data along with information on student grade to identify structural and nonstructural school changes.

- Address Change Data: Address records come from two sources. The student demographics data lists the address on file for each student as of the end of each school year, while another dataset chronicles all student address changes registered with the BPS between

[1]Prepared by James Fogel and Ryan Kessler, research assistants at the Federal Reserve Bank of Boston.

June 2004 and June 2010. We combine these data, creating a student address panel that allows us to identify each student's address at any given time, albeit with some measurement error.[2]

Merging the first four datasets on student number and school year results in 384,044 student-year observations with test score and/or attendance information.

2 City of Boston Property Parcel Data

The City of Boston Assessing Department maintains detailed property tax records and parcel ownership data based upon recorded property transactions at the Registry of Deeds. We use the department's parcel shapefiles to link addresses to unique property identification numbers and thereby to the department's standardized property parcel dataset, which provides variables as of January 1 each year indicating the property owner's name, property type (apartment, condominium, single-family, etc.), and whether the property received the residential tax exemption for owner-occupants.

To qualify for the residential exemption, owners must occupy the property as their principal residence on January 1 preceding the fiscal year for which they are applying.[3] This indicator is certainly an imperfect proxy for owner-occupancy, as not all owner-occupants claim (or are eligible to claim) the exemption and not all units in multi-family structures receiving the exemption are, in fact, owner-occupied. To rid the indicator of the latter form of error, we compare student and parent last names with owner last names listed in the Assessor data and buyer and seller last names tied to relevant deed transfers in the Warren Group data. We declare a name match when the last names are identical or the number of single-character edits required to make them identical is one and both last names are at least five characters long.[4] Students for whom we identify a name match are designated owner-occupants, while others are assumed to be tenants.

[2]This error stems from the fact that the date on which the move was recorded with the BPS may lag the date on which the move actually occurred, and that there is no guarantee that all student address changes are actually registered with the BPS.

[3]The value of the residential exemption is 30 percent of the average value of all residential property in the city. Source: http://www.cityofboston.gov/assessing/exemptions/resexempt.asp

[4]For example, the last names "Anderson" and "Andersen" would be declared a match, whereas the last names "Lee" and "Lew" would not be declared a match.

This name-match methodology is conservative in attributing ownership. For example, in a property the Assessor labels as single-family, owner-occupied, one would expect the occupant to be the owner. Our name-match process, however, attributes ownership to only 69 percent of BPS parents/guardians living in single-family properties designated by the Assessing Department as owner-occupied. The equivalent figures for condos, 2-family structures, and 3-family structures are 74, 46, and 32 percent, respectively.

3 Warren Group Foreclosure Data

The Warren Group compiles information on all residential home sales and foreclosures in Massachusetts and other states from public records. These data include information on virtually all housing transactions registered over the last 20 years, including mortgage originations, foreclosure petitions, and deed transfers, for both foreclosure and non-foreclosure sales. Of particular interest to us are the Warren Group data on foreclosures — namely, foreclosure petitions filed with the Land Court and foreclosure deeds filed with the Registry of Deeds.

Much of our analysis focuses on the Warren Group data on foreclosure petitions, since they are the first indication in the data that a property is in distress. We also consider foreclosure sales, which signify the eviction of a household or homeowner from a property. But to this end, the Warren Group data are deficient in that the date on which a foreclosure deed is filed with the Registry does not tell us exactly when the foreclosure sale occurred. Fortunately, we are able to supplement the Warren Group data on foreclosure deeds with foreclosure auction dates compiled directly from the Suffolk County Registry by Lauren and Timothy Lambie-Hansen.[5] These data include for each foreclosure deed filed in Boston between 2000 and 2011 the date on which the property was sold at auction as well as the book and page number of the document in the Registry of Deeds. Of the 3,133 foreclosure deeds in the Warren Group data filed in Boston within our sample period, 3,083 are matched on book and page number to a foreclosure auction date; for those that are not, we impute the auction date by subtracting 60 days — the median duration from foreclosure auction to foreclosure deed observed in the data — from the date on

[5]See Lambie-Hanson and Lambie-Hanson (2012).

which the foreclosure deed was filed.

4 Matching

We begin by matching student addresses with the City of Boston property parcel data. Using ArcGIS software, we overlay student addresses with the Assessing Department's parcel shapefiles, spatially joining nearly all student addresses to a parcel and associated property identification number. The validity of each spatial join is determined as follows:

(1) All student addresses that match the address of the parcel to which they are spatially joined are deemed valid matches.

(2) Large apartment complexes may have multiple street addresses but only one address listed as the official parcel address.[6] All student addresses spatially joined to apartment complexes that do not match any other parcel address are deemed valid matches. This assumes that student addresses in apartment complexes not matched to a parcel address were joined correctly. An audit of these matches using Google Maps confirms that this assumption is reasonable.

(3) Student addresses that are not spatially joined to a parcel but match a parcel address are deemed valid matches.

(4) Non-apartment addresses that meet the other criteria in (2) and are spatially joined with an especially high degree of precision are deemed valid matches. An audit of these matches using Google Maps supports this designation.

Taken together, we are able to match 91 percent of student addresses to a property identification number. Discarding the remaining 9 percent, we merge student addresses on property identification number with the Assessing Department's standardized property parcel data.

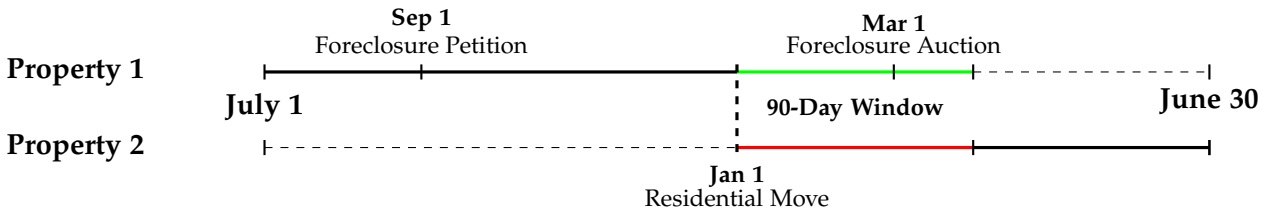
We link each student address to its full Warren Group transaction history — deed transfers, foreclosure petitions, foreclosure auctions, and the dates on which these events occurred

[6]For example, the street addresses 2, 4, and 6 Main St. may all be part of the same parcel, which is listed for tax purposes as 2 Main St. in the Assessor data.

— using the same property identification number. Unfortunately, this variable is missing for approximately 9 percent of properties in the Warren Group data, meaning that there are likely to be some students who experience a foreclosure event that we do not observe.

Lastly, we assign foreclosure petitions and foreclosure auctions to student-school year observations, taking account of residential moves. For foreclosure petitions, this amounts to checking whether a student was living at the property on the date the foreclosure petition was filed. For foreclosure auctions, we define a 90-day window following residential moves during which all records are attributed to the student who moved away from the property; see Figure App.1 for an example.

Figure App.1: Assigning Foreclosure Events to Student-Year Observations — An Example



Notes: Here we consider a student who moved from property 1 to property 2 on January 1. Both of the foreclosure events associated with property 1 — the foreclosure petition on September 1 and the foreclosure auction on March 1 — would be tied to this student in this school year, since the foreclosure petition was filed during the student's tenure at the property and the foreclosure auction took place less than 90 days after the registered move date. To avoid double counting, foreclosure events filed within the 90-day window at property 2 would not be tied to this student.

Merging this subset of the student address data with the 384,044 student-year observations gleaned in the first section leaves 350,329 student-year observations (172,828 on test scores) in our panel for use in our analysis.