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Within-School Spillover Effects of Foreclosures and Student Mobility on Student Academic Performance

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Abstract

Aside from effects on nearby property values, research is sparse on how foreclosures may generate negative externalities. Employing a unique dataset that matches individual student records from Boston Public Schools—including test scores, demographics, home address moves, and school changes—with real estate records indicating whether the student lived at an address involved in foreclosure, we investigate the degree to which the test scores of students attending high-foreclosure schools suffer, even among students not directly experiencing foreclosure. We also explore the impact on individual test scores of school-level (by grade and year) student mobility—that is, inflows of new students to a school during the school year—including mobility induced by residential moves (in some cases caused by foreclosures) and mobility arising for other reasons. We find fairly robust evidence that higher student mobility at a school, induced by residential moves, imposes significant negative effects on test scores of students at the receiving school. Beyond this channel, school-level foreclosure prevalence does not appear to generate externalities. Since we also find that residential-move-induced school changes appear to harm the outcomes of the school-changers themselves, policies that seek to limit such changes within the academic year may uniformly raise test scores, at least in the short run.

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The wave of foreclosures that followed the recent housing bust and subsequent Great Recession is one of the defining characteristics of the economic crisis. Foreclosures have obvious adverse consequences for families living in or owning properties undergoing foreclosure, and may also generate negative spillover effects on neighboring residents due to the increased prevalence of vacant, and often derelict, housing units—conditions that may invite crime (Cui 2010) and/or depress surrounding property values (Gerardi et al. 2012). In addition, the adverse effects of foreclosure may extend beyond these relatively immediate costs. Families' lives are disrupted by the stress, economic loss, and dislocation that accompany foreclosure, and these factors have the potential to affect many aspects of families' wellbeing and behavior. For example, Currie and Tekin (2011) find a positive and arguably causal association at the zip-code level between increased foreclosures and increases in medical visits for mental health concerns and stress-related medical conditions. More recently, Arcaya et al. (2014) find that nearby neighbors of (real estate-owned) foreclosed homes have experienced statistically significant increases in systolic blood pressure.

In previous research, we asked whether foreclosures negatively affected the academic performance, including standardized test scores and attendance rates, of students living in properties undergoing foreclosure. Using data on students in Boston's public schools, together with data on housing characteristics and foreclosures that were matched to students' addresses, we found that foreclosures may have harmed student outcomes indirectly, insofar as they resulted in a student's having to change schools during the academic year. We found no negative effects of the foreclosure per se once the school change and numerous other factors, including the student's prior-year performance, were accounted for, although we did find evidence for the hypothesis that (unobserved) economic shocks within a family—shocks that often precede and contribute to foreclosures—exert negative effects on student outcomes.

There is cause for concern that students otherwise unaffected by foreclosures may have been subject to negative spillover effects from attending a school where many students experienced foreclosures. Foreclosures among student households may have resulted in an increase in the flow of students into or out of a given school, creating disruptions that may have affected the entire student body. A number of papers have shown that students in schools with highly mobile student populations tend to perform less well than students in schools with more stable student bodies (see, for example, Ingersoll, Scamman, and Eckerling 1989). Taking the analysis a step further, Hanushek, Kain, and Rivkin (2004) find that greater mobility in a school's population imposes significant costs on achievement growth for all students in the school, regardless of individual mobility status, with these costs greater among lower-income and minority students.[1]

In this paper, we estimate the degree to which the test scores of students attending high-foreclosure schools are lower, even among students not directly experiencing foreclosure. In addition, we examine the impact on individual test scores of school-level (by grade and year) student mobility, including mobility induced indirectly by foreclosures and mobility arising for other reasons, where mobility is defined in terms of incoming (rather than outgoing) students during the course of the school year, not including summer. In this context, the analysis investigates a potential mechanism—namely, the influx foreclosure-displaced students—whereby foreclosures experienced by some students may impose negative externalities on other students. Furthermore, we employ a unique dataset that helps us to separate any disruptive effects of school-level mobility from endogenous confounders such as unobserved differences in school quality.

Our dataset matches individual student records from Boston Public Schools—records that include standardized test scores, socio-demographic characteristics, address-change history and school-change history—with real estate records indicating whether the student lived at an

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[1] Other papers finding a negative association between average student performance and school mobility include Rumberger (2003). In related research, Imberman, Kugler, and Sacerdote (2012) do not test for mobility effects per se, but identify that newly entering, disruptive students (evacuees from Hurricanes Katrina and Rita) imposed negative externalities on other students' test scores. A number of studies have estimated the impact of a student's mobility status on her own outcomes, including Reynolds, Chen, and Herbers (2009), Hanushek, Kain, and Rivkin (2004), and Schwartz and Stiefel (2012). Hanushek, Kain, and Rivkin (2004) observe negative adjustment costs of school changes for students who change schools within a region or district. At the same time, both Hanushek, Kain, and Rivkin (2004) and Schwartz and Steifel (2012) find that some school-switchers experience improvements in school quality, results attributed to the voluntary or "Tiebout" nature of some school changes.

address involved in foreclosure and whether that student's parent or guardian was the owner of the property or instead a tenant. The student school-change histories distinguish between school changes occasioned by a residential move, such as might follow a foreclosure, and school changes arising from requests by parents or transfers instigated by school officials, among other observed reasons. The ability to distinguish among reasons for school changes helps to overcome an identification problem created by the fact that students may change schools in search of a better educational outcome, and therefore school-level mobility rates may be correlated with school quality, a portion of which is unobserved by the econometrician.[2] Controlling for fixed differences in school quality is not sufficient, because turnover may vary within a school over time in response to changes in unobserved school quality. However, if we observe school changes that are prompted by events or factors unrelated to school quality, the effects of the prevalence of such changes on other students at the receiving school will not be confounded with effects of unobserved changes in school quality.[3]

In addition to distinguishing among types of school changes, the data enable us to control for numerous measures of time-varying school characteristics. The reasons for school changes that we observe include (but are not limited to) foreclosure of the student's home (followed by a school change in the same year), a home-address change not associated with a foreclosure, a desire on the part of parents (or the student) to obtain a different academic program (or to place siblings in the same school), and a number of other reasons that include more suitable special education and bilingual placements and student behavior and disciplinary issues.

We find evidence suggesting that higher student mobility at a school (by grade and year), induced by residential address changes, imposes statistically and economically significant negative effects on the math test scores of students at the receiving school, controlling for a student's own mobility status and numerous other fixed and time-varying individual and

[2] School-level mobility may be correlated with school quality even if school changes are not motivated by school quality, for example if characteristics of the population the school draws upon, such as socioeconomic status, are associated with both lower test scores and higher residential mobility. Our identification strategy addresses this concern as well.

[3] Hanushek, Kain, and Rivkin (2004) adopt a similar approach using proxies for the underlying motivations for school changes, lacking direct information on such motivations.

school-level factors. For ELA test scores, the relationship is somewhat smaller in magnitude and includes a lag—students whose *current* school-grade-mates experienced more residential-move-induced school changes in the *previous* year show significantly lower test scores than do students whose current peers were less mobile in the previous year. Although we cannot rule out the possibility that these effects reflect unobserved differences—for example, in the financial stability of the student population—that render some schools-by-grade or schools-by-year more susceptible to both higher rates of address-change-induced turnover and lower achievement (among nonmovers as well as movers), the results are strongly suggestive that higher turnover imposes negative achievement externalities on students, especially in light of their robustness to inclusion of school fixed effects and numerous controls for observed, time-varying school-grade-year level and student-level factors relating to socioeconomic status, ability, and other-reasons mobility. These findings agree with earlier evidence from Texas (Hanushek, Kain, and Rivkin 2004) that student turnover at a school imposes negative externalities, suggesting that such externalities are not unique to the current setting. Since we also find that residential-move school changes appear to harm the outcomes of the school-changers themselves, policies that seek to limit such changes within the academic year may uniformly raise test scores, at least in the short run.

We also find evidence, in some models, that a concentration of foreclosures in the student body is negatively associated with test scores. However, these effects become insignificant once we control for other student-body characteristics, and so we conclude that foreclosure prevalence at the school-by-grade-by-year level has no special effects on test scores. Rather, to the extent that foreclosures among students result in residential moves and concomitant school changes for these students, those foreclosures may have negative effects on the other students at the schools that receive the displaced students. Therefore, in calculating the total effects of foreclosures on student outcomes, policymakers should take into account the system-wide contribution of foreclosures to student mobility across schools.

The remainder of the paper is organized as follows: The next section describes the student panel data, the additional datasets with which we merge them, and how we compile the regression datasets. Section II below presents our analytical approach, which we illustrate with a simple theoretical model of academic achievement. Section III describes the empirical results and interprets them in the context of the theoretical model. Section IV follows with analysis of several alternative specifications that test the robustness of our findings. Section V concludes the paper with a discussion of our results.

I. Data and Sample

The Boston Public Schools (BPS) provided data for school years 2003/2004 through 2009/2010 relating to both individual students and schools. 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-proficiency status, and performance measures other than test scores, such as attendance and suspensions. In addition to every student's address as of June 30 each year, a supplementary address-change dataset from BPS records the date, the previous address, and the new address each time a parent reports a change in home address to BPS.

Using students' home addresses over the course of the school year, we merge the BPS data with City of Boston Assessor data and Warren Group data, both at the level of the individual property. The Assessor data indicate the structure type of the property and whether the property was owner- or renter-occupied as of January 1 each year, based on whether the owner claimed a residential tax exemption, which provides a property-tax discount for owner-occupants. The Warren Group compiles information 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 petitions. These records include the names of the people involved in the various transactions, such as the buyer and seller when a property changes hands.

During the period of our study, Boston had a complex, zone-based school assignment system. The entire city is divided into only three broad school attendance zones for elementary and middle school attendance—each of the zones includes 25 to 35 schools—and all 35 of the city's high schools are open to residents citywide. Within the three zones and for high school students, some priority is given to students within a one-mile (or 1.5-mile for middle schools) walking radius, but sibling preference also enters and ties are broken via random number generation. Thus, there is a much looser link in Boston between the neighborhood where a student lives and the school she attends than in a city with local neighborhood schools.

The BPS data also report the school each student attends at fixed intervals. In addition, for every school-change event for any individual student, the data indicate the date of the change, the identities of the sending and receiving schools, and the reason for the change. For each student and for each academic year (July 1 to June 30), we use this information to construct a set of binary indicators for various types of school changes, distinguished along various dimensions, including whether a change took place over the summer of the given academic year (defined as July 1 to October 1) or during the course of the school year (defined as October 1 to June 30). Specifically, we construct a dummy variable for the event of a structural school change, which by definition (and of necessity) occurs over the summer following the completion of the highest grade at a given school; and we construct dummy variables for two types of nonstructural school change (a school change for any other reason)—one for a nonstructural change that occurs over the summer and one for a nonstructural change that occurs during the academic year. We distinguish between summer and mid-academic-year changes because earlier research suggests that changes during the academic year are associated with greater negative effects on performance than are moves that occur over the summer, both for individual movers and at the school level. .[4] In addition, we divide the mid-year nonstructural school changes reported in the BPS administrative data into three types, based on the reason for the change. The three (aggregated) reasons are as follows::

- Discretionary school changes: Students changed schools for programmatic reasons or because they were on a waitlist for a school they preferred and/or because sibling preference allowed them to move in when a space became available. Programmatic changes are usually

[4] In addition, a student experiences a "school change discontinuity" when his or her school shuts down or merges during our sample period.

requested by parents or students, often to a school with programs not available at the sending school.

- Residential-move school changes: Students moved into the school (a) because their family moved residences *within* Boston and changed to a new school or (b) as a result of their family moving into the city of Boston and entering the Boston Public schools (whether from non-Boston public, parochial, or private schools in Massachusetts or elsewhere).[5] Note that these residential-move school changes (unlike changes in the discretionary or "other" categories) are not likely to reflect efforts by parents seeking a better school for their children, since all three broad Boston zones include both higher- and lower-quality schools, and residential location does not guarantee any specific school within a zone.
- Other school changes: Students moved into the school (a) to obtain more appropriate bilingual or special needs services—moves generally initiated by BPS staff; (b) for reasons related to medical issues, behavioral issues, or issues of safety, including readmissions; or (c) for any other reason, including Boston residents who moved from a charter, parochial, private, or home school.[6]

In addition to student-level observations, we compile the school change data at the school-by-grade-by-year level, aggregating student nonstructural school changes *into* each school between October and June by reason for change, and expressing them as a fraction of March enrollment at the school. We also compile the student characteristics at the school-by-grade-by-year level, indicating the fraction of students who have limited English language proficiency, are receiving

[5] The latter category—students moving into Boston and entering BPS—represent only one-quarter of residential-move school changes among the students in our regression sample because most students moving into Boston do not have lagged test scores and prior-year home-address information. Most of those entering BPS from elsewhere during the school year who appear in our regression sample must be re-entrants to BPS—their prior-year information would date from before they moved away.

[6] The BPS staff-initiated school changes to more appropriate bilingual and special needs services represent over half of other school changes, medical-behavioral-safety (type-b) school changes account for about 30 percent of "other" reason school changes, and other-other (type-c) changes represent about one-eighth of other school changes. We separate these latter Boston-resident moves into BPS from those in the residential-move school-change category because a move from charter, private, or parochial school to BPS may be made in response to being admitted to a specific preferred public school via the BPS assignment system; alternatively, a move out of private or parochial school might reflect a worsening of the family's economic situation, making school tuition too great a burden. We are unable to distinguish these possible motivations, and the former explanation suggests that the estimated coefficient on such moves could combine a positive school-quality effect with a negative disruption effect. Like those entering BPS from elsewhere during the school year (previous footnote), these residents coming into BPS would not appear in our regressions unless they were re-entrants; otherwise BPS would lack their prior-year test score.

free or reduced-price lunch, etc. In the case of most of these characteristics, we calculate these fractions (both numerator and denominator) based on the school's October enrollment for the given grade level and year, and then attach them to individual students using the school attended in March. We use the March school because the MCAS tests are administered in late March or early April in the case of English language arts (ELA) and in May in the case of math.

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 350,329 student-year observations in grades 1–12. Students in Massachusetts public schools take math and ELA standardized MCAS exams every spring in grades 3 through 8 and also in grade 10;[7] we have 172,828 student-year observations on test scores. We norm the raw MCAS test scores to the *state* distribution by grade, test, and year.[8] 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 the regression variables. We also drop students with nonstandard test-grade patterns,[9] or when they are attending any of 10 special schools for which October-to-June mobility exceeds 50 percent,[10] or when they are attending a school-grade-year with fewer than 20 students.[11]

We have no 3rd grade observations in our regressions because third graders do not have prior-year test scores. We lose the 2003/04 school year because we have no student address information until June 2004; we lose 2004/05 and 2009/10 because we include a lead and a lag on foreclosure (and eventually on other variables). This leaves us with four school years (2005/06–2008/09) and six grades (4th through 8th and 10th) of data,[12] amounting to about 50,000 student-by-year observations in the math and ELA regressions. Table 1 reports the means of all the

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[7] 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 2005/06.

[8] We use only ELA and math scores and discard retests.

[9] A nonstandard test-grade pattern occurs when this year's test grade is not equal to the previous test grade plus one (or plus two when this year is 10th grade because the previous test is 8th grade).

[10] These special schools teach troubled students on a temporary basis (months at a time) and then send the students back to a standard Boston public school.

[11] By conditioning on enrollment size, we lose 93 school-by-grade-by-year combinations (referring to 17 schools) in the math regression sample and 97 school-grade-years (referring to the same 17 schools) in the ELA regressions.

[12] However, we do not have 24 grade-by-year pairs because the testing gaps prior to 2005/06 cited in a previous footnote lead to a lack of lagged test scores for some grades in 2005/06 and even 2006/07 (10th grade ELA).

student-level variables in the two regression samples.

II. Analytical Approach

Our earlier analysis of the effects of foreclosure at home on individual students' academic performance found that students whose home address was subject to foreclosure—regardless of whether their parents were owners or renters in the home—had lower test scores than unaffected students, controlling for observed individual characteristics. However, the effects became almost indistinguishable from zero when we also controlled for the student's prior-year test score. Alternatively, including student fixed effects also eliminated negative foreclosure effects. These results suggest that some common underlying factor, such as persistent poverty or an economic shock to the family, caused both the foreclosure and the poor academic performance.

However, insofar as a foreclosure led to a residential-move school change for the student, the foreclosure may have had an indirect negative effect on a student's outcome, because changing schools between October and June following a residential move was associated with lower test scores in both math and ELA as well as with lower attendance.[13] Interestingly, in Boston, the fraction of residential moves that translate into academic-year school changes is restrained by a school assignment policy that involves a fairly weak link between home location and school attended. For example, the earlier paper noted that only 14 percent of students who move residences experience a nonstructural school change between October and June of the same school year.

In sum, we concluded that students directly affected by home foreclosure may suffer academically as a result of events precipitating the foreclosure, such as a parent's job loss, and also, possibly, as a result of a school change induced by the foreclosure. In addition to a range of individual covariates and the lagged dependent variable, our previous analysis controlled for school and neighborhood factors using fixed effects. In the current analysis, we unpack the

[13] This significant negative relationship between an October-to-June school change and a student's test score held up even with student fixed effects.

school-level variation in test scores, focusing in particular on the contributions to individual test scores, if any, of foreclosure prevalence and student mobility (by reason) at the level of the school by grade level and year. Prior research (Hanushek, Kain, and Rivkin 2004, henceforth HKR) suggests that high student mobility creates negative externalities for all students at a school, stemming from disruptions that come with assimilating new students, whether at the start of the year or during the course of the year. This research suggests that increases in student turnover caused by foreclosures could hurt academic outcomes of all students at schools that experience such increases in turnover, regardless of an individual student's own foreclosure status or mobility status. In addition, high foreclosure prevalence at a school, independently of any impact of foreclosure prevalence on student turnover, might place added demands on school resources and negatively affect outcomes for all students.

To clarify the identification issues associated with testing for such effects, we present a simple, value-added model of academic achievement and then break out the components related to student mobility and foreclosure prevalence, each measured at the school level (by grade and year). This model draws on the theoretical framework for academic achievement developed by Todd and Wolpin (2003). In addition, we borrow components related to mobility externalities from the model of HKR.

$$A_{isgt} = \kappa A_{is'g\text{-}1,t\text{-}1} + SQ_{sgt} + \boldsymbol{\alpha_0}'\mathbf{M}_{it} + \boldsymbol{\alpha_1}'\mathbf{M}_{i,t\text{-}1} + \boldsymbol{\beta}'\mathbf{X}_{it} + \varepsilon_{it} + \lambda\varepsilon_{i,t\text{-}1} \qquad (1)$$

In the above, A_{isgt} represents the achievement level of student *i*, at school *s*, in grade *g*, and academic year *t*, which we measure using a standardized score (from the MCAS test) for either math or ELA. For reasons discussed in our earlier paper, we include the student's once-lagged test score on the right-hand side, denoted $A_{is',g\text{-}1,t\text{-}1,}$ for student *i* in school *s′* in grade *g*-1 at time *t*-1, where *s′* may or may not be identical to *s*.[14] This lagged score controls for the influence on the current achievement level of lagged schooling inputs. The lagged test score also controls,

[14] We eliminate grade repeaters, so that, in most cases, the grade level in year *t*-1 will equal *g*-1. However, in observations of 10th-grade scores, the lagged score is actually the 8th-grade score because MCAS tests are not administered in 9th grade. Therefore, in these cases, the lagged score refers to period *t*-2 and grade *g*-2.

imperfectly, for the fixed student and family contribution to the achievement level.[15] The term SQ_{sgt}, for "school quality," captures the contribution to the current score made by all observed and unobserved factors at the level of the school by grade and academic year. This term will be decomposed in various ways in different empirical specifications, described below. $\mathbf{M}_{it}$ refers to a vector of individual-level "mobility" or "transition" indicators, including the dummies described above for the various types of school changes, a dummy for whether a student changed home residence between October and June of the school year, and an indicator for whether the student's home was subject to a foreclosure petition at any time during the given academic year (July 1–June 30). In the cases of foreclosure petitions and the mid-year school changes, we also include indicators for the once-lagged events, denoted by the vector $\mathbf{M}_{i,t-1}$. This step allows the effects of such transitions to persist (or to be delayed) into the year following their occurrence.

Also in equation (1), $\mathbf{X}_{it}$ refers to a vector of other observed, time-varying individual/family determinants of academic achievement, such as whether the student is classified as limited-English-proficient (LEP), classified as a special education student, and whether the student comes from a low-income family, based on qualifying to receive free or reduced-price lunch, among other measures commonly associated with academic achievement.

The term ε_{it} represents an idiosyncratic shock to the student's achievement level at time t, reflecting unobserved changes in relevant student or family circumstances such as parental job loss or a pay cut, divorce, birth of a new sibling, onset of student or parental illness, et cetera. While these shocks are assumed to be independent and identically distributed over time, we allow for such shocks to exert some persistence in their effect on student outcomes, such that the previous period's shock affects the current outcome, adjusted by the factor λ, which is assumed to lie between 0 and 1.[16] As discussed at length in Bradbury, Burke, and Triest (2013),

[15] We assume that there is a fixed student/family contribution to the achievement level rather than to the achievement gain in a given year. By allowing for decay of the lagged score, the estimation leaves out a portion of this fixed contribution to the level. However, we find it is not empirically feasible to include both a lagged test score and a fixed effect. For discussion of empirical evidence favoring fixed effects in gains vs. fixed effects in levels, see Burke and Sass (2013).

[16] We could, alternatively, model the shocks themselves as exhibiting serial correlation over time, but this introduces

such shocks challenge the identification of the direct effects of the own mobility and foreclosure events, whether current or lagged. In particular, identification of the coefficient vectors α_0 and α_1 may be confounded by the fact that the unobserved shocks, such as parental job loss, may exert direct effects on test scores and may themselves precipitate a foreclosure, a residential move, and/or a school change. While identification of the individual mobility effects is not the primary focus of the current paper, we revisit these issues in the context of the empirical analysis below.

As discussed in HKR, there is another important issue that affects the interpretation of estimated effects of individual school changes. An individual school change may causally affect a student's outcome via two basic pathways: first, a student may experience a change in school quality, for better or worse; and second, a student incurs costs in switching to a new school, such as learning new rules, getting to know new peers and teachers, and other adjustments.[17] Again, because in the current paper we are more interested in the external effects of peer mobility than in the effects of own school changes, we do not claim to fully disentangle the respective effects of these two mechanisms. However, the presence of both pathways will be kept in mind when interpreting effects of own school changes under the various specifications.

The school quality term, SQ_{sgt}, captures the influence of factors such as the effectiveness of the school principal, the quality of the school's physical plant, average teacher effectiveness at the school for the given grade level and year, and average peer "quality" at the school-grade-year level, where quality refers to any cognitive or noncognitive attributes of students, whether fixed or time-varying, that influence nearby students' scores. In addition to these factors, the overall rate of student turnover at the school, by grade and year, may affect school quality for all students regardless of their individual mobility status, because the process of assimilating new students places demands on school resources that detract from their ability to produce

needless complications.

[17]Unlike HKR, we do not let the switching cost depend on the aggregate student mobility level of the school being entered, nor do we allow school quality to vary systematically within a school with individual mobility status. Allowing for such dependencies would require including interaction terms between individual mobility status and school-wide mobility rates, as well as between individual mobility and school fixed effects in the relevant specifications.

achievement growth for the average student. Similarly, we consider whether the school-wide foreclosure rate—again at the grade-by-year level—imposes negative effects on achievement regardless of own foreclosure status, due to a possible drain on school resources incurred in helping some students cope with fallout from foreclosures.

To illustrate the key issues in identifying spillovers from peer mobility and peer foreclosures, we express school quality in the following manner:

$$SQ_{sgt} = \boldsymbol{a_0}'\overline{\boldsymbol{M}}_{\boldsymbol{sgt}} + \boldsymbol{b}'\overline{\boldsymbol{X}}_{\boldsymbol{sgt}} + \theta_{sgt} + \boldsymbol{a_1}'\overline{\boldsymbol{M}}_{\boldsymbol{sg,t-1}} + \mu\theta_{sg,t-1} \qquad \textbf{(2)}$$

In the above, $\overline{\boldsymbol{M}}_{\boldsymbol{sgt}}$ refers to the vector of school-wide mobility rates by grade and year, including, for example, the share of students at school *s* in grade *g* and year *t* who transferred into the school between October and June as a result of a residential move. (Recall that this share is calculated as the number of such in-moves divided by total March enrollment at the given school for that grade and year.) Similarly, the vector includes rates of school-change owing to other reasons as well as to the share of students in the school-grade-year group (enrolled as of October) who experienced a foreclosure petition at any point in the given academic year.

The term $\overline{\boldsymbol{X}}_{\boldsymbol{sgt}}$ refers to a vector of additional, observed, time-varying characteristics of the school's student population by grade and year, such as the share of students classified as LEP, the share classified as special education students, the share who are low income, the share entering low-income status in the given year, and the racial and gender composition. These peer traits may affect individual achievement directly, for example if having a higher share of LEP peers leads to slower gains in English proficiency, controlling for own LEP status, and/or because these variables may proxy for unobserved characteristics or behaviors that exert peer effects on achievement. For example, students who enter low-income status in a given school year may be subject to additional stress and, as a result, may display disruptive behaviors in school that harm other students' outcomes.

We also include the change in enrollment (for the given grade level and school) between October and June of a given school year, because effects of changes in class size, if uncontrolled, could confound mobility externalities. (Changes in grade-level enrollment should predict changes in average class size within the grade level at the school.) The term θ_{sgt} represents the contribution to achievement of unobserved factors at the school-grade-year level. These would include the quality of the school's administration and its overall curriculum, average teacher effectiveness, and average peer quality to the extent that this is not captured by the observed peer characteristics. Some of these factors may be common to the school at all grade levels and years, but in principle we allow at least some relevant unobserved aspects of school quality to vary across the school-grade-year unit.

Ignoring for the moment the terms $\overline{\boldsymbol{M}}_{\boldsymbol{sg,t-1}}$ and $\mu\theta_{sg,t-1}$, an identification issue arises because the school mobility variables $\overline{\boldsymbol{M}}_{\boldsymbol{sgt}}$ may be correlated with the unobserved school quality factors in θ_{sgt}. (The same identification issue affects the factors in $\overline{\boldsymbol{X}}_{\boldsymbol{sgt}}$, but identification of those effects is not our primary focus.) For example, there may be relatively fixed factors, such as financial instability among the population a school draws upon, that contribute both to persistently higher student turnover—for example, because financial instability leads to residential transience and related school changes—and to lower achievement among all students, regardless of individual mobility status in a given year. Financial instability may also predict a higher frequency of behavior-related turnover at the school as well as lower test scores among both school-changers and nonchangers. In addition, the frequency of preference-based moves into the school may be influenced by the school quality itself, as indicated by average test scores at the school or reports on teacher effectiveness.

In addition to fixed, inter-school differences that confound mobility externalities, increases (decreases) in student mobility at a school may be simultaneous with other, unobserved shocks to school quality that harm (help) student achievement, either because changes in school quality induce changes in mobility or because changes in underlying factors—such as a widespread economic shock affecting many students at a school or group of schools—induce both changes in mobility and changes in test scores for movers and nonmovers alike.

In the case of a widespread economic shock, confounding could occur because the individual shock, ε_{it}, predicts mobility at the school-grade-year level, even conditioning on individual mobility status, because not all students affected by the shock will experience a mobility event. Because we allow individual lagged shocks to affect the current outcome, confounding could also occur if the lagged individual shock, $\varepsilon_{i,t-1}$, predicts current mobility at the school-grade-year level.

We allow for the possibility that lagged school changes and/or lagged foreclosures among the current peer group, indicated by $\bar{\boldsymbol{M}}_{\boldsymbol{sg,t-1}}$, impose externalities on a student's current outcome. For example, peers who experienced a school change last year may still be adjusting to that change in the current year and, if so, they may exhibit behaviors that detract from the current learning environment. As discussed below, the lagged peer mobility measures are calculated as the share of a student's *current* peer group who experienced a given transition (for example, a residential-move school change) during the *previous* academic year.

Finally, the term $\mu\theta_{sg,t-1}$ refers to effects on current achievement of other prior shocks to the current peer group, such as parental divorce or job loss in the preceding year. Such prior shocks may continue to affect peer behavior in the current period, possibly producing external effects on individual achievement, and the shocks may also predict peer mobility in either the previous or current period. Therefore, this term represents another source of confounding of peer mobility effects, whether current or lagged, which we discuss in the context of various empirical specifications below.

Controlling for school quality, including both fixed and time-varying components, is therefore an important step in identifying mobility spillovers. We approach this by including numerous observed, time-varying indicators of school quality, and, in some models, by also including either fixed effects for schools or pairwise fixed effects at the level of the school-by-grade and/or school-by-year. In addition, the administrative information we observe concerning the reasons for individual school changes is useful for identifying the external effects of school-level mobility, as well as for identifying the own effects of a school change, for reasons discussed above.

III. Empirical Results

A. Student-level analysis

We preface our empirical analysis by examining the influence on individual test scores of student-level factors, in particular the indicators of individual school changes and foreclosures. This preliminary analysis constitutes a modified version of the analysis from our previous paper (Bradbury, Burke, and Triest 2013). At the same time, these regressions enable us to assess the extent to which combined factors at the level of the school-by-grade-by-year (SGY) contribute to variance in individual scores, using, in turn, SGY fixed effects and SGY random effects.[18] Following this analysis, in section III.B, we unpack the generic SGY-level variance with an eye to assessing the extent of externalities of (SGY-level) peer mobility rates and peer foreclosure rates, still retaining the SGY-level random effects.[19]

In Tables 2 and 3, columns 1–3 report the student-level coefficients from these preliminary regressions. In the models in columns 1 and 2, we include fixed effects for the SGY combination, while the model in column 3 substitutes SGY random effects, paving the way for subsequent models—results reported in columns 4 through 9—that also include specific SGY-level factors. (Coefficients on the SGY-level factors in these same models are shown in columns 4 through 9 of Tables 5 and 6, discussed below.) As in the previous paper, we include the student's lagged (same-subject) test score as an explanatory variable, and control for numerous observed individual characteristics, a complete list of which appears in Table 1.

One significant difference from the previous paper is that we include indicators for each of the three different reasons for within-year (October-to-June) nonstructural school changes, rather than a single indicator for any within-year nonstructural school change. As seen in column 1 of

[18] Analysis in the previous paper includes neighborhood (that is, census tract) fixed effects as well as school fixed effects. We focus on school-based influences in this paper because our previous results indicated that between-school test-score variation is much greater than between-neighborhood variation, given individual student covariates.

[19] Because MCAS tests are given only in selected grades, and because different schools within Boston consist of different ranges of grade levels (for example, K–5 vs. K–8), comparisons between schools, as well as comparisons of within-school vs. between-school variation, must be made at the school-by-grade level (or lower) in order to control for effects of grade composition by school on average scores.

Tables 2 and 3, we observe small, negative coefficients on the lead and on the lag of an individual foreclosure petition. As in the previous paper, we interpret these results as indicating the presence of some common factor, such as an economic shock affecting the student's family, that likely caused both the foreclosure and the poor test scores. The estimated coefficients on the three (mid-year) school-change indicators demonstrate that students changing schools for any reason have lower test scores than those who do not. The discretionary-school-change indicator obtains the smallest (closest to zero) coefficient, perhaps because some of the children moving for these reasons actually do better at the new school, which is preferred by them or their parents. The residential-move-school-change indicator obtains a larger negative coefficient, suggesting that students who move for address-change reasons have difficulty settling into the new school and/or are less likely to experience offsetting improvements in school quality. The other-school-change indicator obtains an even more negative coefficient, perhaps because some of these students changed schools on account of behavioral, medical, and safety problems, which may also cause them to perform less well in school.

Column 2 in Tables 2 and 3 adds lags of the October-to-June individual school-change-by-reason indicators. These estimates suggest that students who experience a school change may not adjust fully in the year in which they change schools: the estimated coefficient on each of the lags is negative and yet each is smaller in absolute value than the coefficient on the corresponding contemporaneous school-change indicator, and not all of the lagged effects are statistically significant. These results could reflect the persistent effect of a lagged shock that also precipitated the lagged school change, as indicated by the term $\lambda\varepsilon i,t\text{-}1$ in equation (1) above.

In the models in columns 1 and 2, fixed effects for the SGY combination capture all school quality factors at the SGY level, whether observed or unobserved, referred to in equation (2) above. Considering the full range of the estimated SGY fixed effects from the model in column 1, the analysis suggests that a student's school, grade, and year combination may account for up to 2 to 2½ points (standard deviations) in his or her normed test score.[20]

Column 3 of Tables 2 and 3 includes SGY random effects in place of the SGY fixed effects. The coefficient estimates on the student-level factors in column 3 are mostly quite similar to the corresponding coefficient estimates from column 2, results suggesting that the unobserved effects at the SGY level are orthogonal to most of the observed individual factors. For example, the estimated coefficients in column 3 on the foreclosure petition, on each of the October-to-June school-change-by-reason indicators, and on the lagged dependent variable are all either roughly equal to or slightly larger (in absolute value) than the corresponding estimates in column 2.

B. Testing for external effects of peer foreclosures and peer mobility

Within a model that includes SGY-level random effects, we can investigate the impact on individual test scores of foreclosure prevalence and mobility rates among peers at the same school by grade and year, controlling for the numerous observed individual factors as well as for numerous factors at the SGY level, described below. For identification of the mobility and foreclosure externalities—as well as for identification of effects of other observed factors at the SGY level—we must assume that the unobserved factors at the SGY level are orthogonal to the observed, SGY-level factors, conditional on the included controls in any given model. We have already discussed, in the context of the theoretical model above, some circumstances in which such an assumption would be violated. We revisit these identification issues within the context of the specific empirical models described below.

Table 4 reports means of the SGY variables in the two regression samples (Math and ELA, respectively); these variables are denoted with the prefix SGY. The SGY attributes we investigate first relate to SGY aggregates of the student characteristics of interest, such as foreclosure petitions and reasons for changing schools during the school year.

Column 4 in Tables 2 and 3 reports regression estimates when we add two variables to column

[20] The range from the 10th to 90th percentile of the estimated school-grade-year fixed effect is much smaller—about two-thirds of a point; for comparison, the unconditional range across SGYs in mean test score is 3.27 standard deviations for math and 2.95 for ELA.

3's equation,[21] one representing the prevalence of foreclosure petitions among students in the SGY as of October and the other tallying students entering the SGY between October and June who experienced a foreclosure petition. Specifically, the SGY foreclosure variable measures the fraction of students enrolled in the school and grade in October of a given year who experienced a foreclosure petition at their home address at some time during the same school year (July 1 through June 30); the SGY foreclosure-school-change variable measures the fraction of March enrollment at each school who both (i) experienced a foreclosure petition at some point between the previous July 1 and the subsequent June 30 and (ii) entered the school between October and June.[22]

These two variables should help us find out whether (or the degree to which) a high prevalence of foreclosures among the October student body or a post-October influx of students affected by foreclosure within a SGY affects the test scores of all children in the SGY. Such effects could occur via student-to-student contact, in the sense that students whose school friends experience foreclosure may be concerned about them, be less able to pay attention to schoolwork, and do less well in school. Alternatively, students experiencing foreclosure and its related stresses may create additional classroom disruptions and/or require special attention from teachers, thereby reducing the quality and quantity of instruction received by other students. At the school level, foreclosure-affected students could absorb school-wide administrative resources (for example, principals' and/or counselors' time), similarly reducing the effectiveness of educational services received by other students. Even less directly, a high prevalence of foreclosures among students in the October student body may generate exits from the school, which could make teaching and learning more difficult for everyone in an affected SGY. (Recall that this potential mechanism is separate from October-to-June influxes of students affected by foreclosures, which are captured by a separate variable in our analysis.)

The coefficients on the SGY variables are reported in Tables 5 and 6. (For these same models, the coefficients on the student-level variables are seen in the corresponding columns of Tables 2 and

[21] Estimated coefficients for these two added variables are reported in Tables 5 and 6.

[22] Note that these foreclosure-and-move-in students are not counted in the school's tally of October students experiencing foreclosure.

3). The SGY foreclosure variable in column 4 obtains a negative coefficient which is significantly different from zero in the math (Table 5) equation and marginally significant in the ELA (Table 6) equation. The SGY foreclosure-school-change variable obtains an insignificant negative coefficient in both equations. Note also that the estimated coefficients on the individual student foreclosure variables (lead, lag, and contemporaneous) in column 4 of Tables 2 and 3 are very similar to the corresponding coefficient estimates in column 3 of the same tables.

These results indicate that students in a school are somewhat negatively affected by the foreclosure experiences (and possible school-change *exits*) of the students enrolled in the same school with them in October, but unaffected by an influx of foreclosed-upon students that occurs during the school year.

Column 5 of Tables 2, 3, 5, and 6 adds variables that measure the fraction of students who moved into the school between October and June for the three groups of reasons. These three school-change-by-reason variables are constructed in such a way that their sum represents the fraction of March enrollment at the school that entered the school between October and June; that is, they jointly reflect the prevalence of students moving into the school and grade as well as reasons for them.

The results in column 5 document a negative relationship between the prevalence of any of these types of move-in activity at a school and individual students' test scores. In both the math and ELA equations, the SGY residential-move-school-change variable obtains the largest negative coefficient, which is significantly different from zero, suggesting that school in-moves associated with ostensibly exogenous residential moves at the school-by-grade level impose negative externalities on other (nonmoving) students' test scores in the same school year. Inclusion of the school-change variables reduces the size of the coefficient estimate on SGY foreclosure prevalence in the ELA equation, making it statistically indistinguishable from zero, but does not affect the SGY foreclosure-prevalence coefficient in the math equation.

Both the SGY discretionary-school-change variable and the SGY other-school-change variable also obtain negative and significant coefficients in the math equation, suggesting that the in-moves of students who enter a school for reasons other than residential moves are also disruptive to other students, albeit to a lesser degree than SGY residential-move school changes. Neither of the school-change variables for reasons other than residential moves obtains a statistically significant coefficient in the ELA equation. Column 5 also includes a measure of net enrollment change between October and June at the SGY level to separate the potentially disruptive effects of turnover—captured by the school-change-by-reason variables, which are gross measures—from the effects of net addition or loss of students associated with changes in class size. The estimated coefficient on the net-change variable is indistinguishable from zero in both equations.

Column 6 of Tables 2, 3, 5, and 6 adds to the column 5 regressions many student characteristics aggregated to the SGY level in order to control for other aspects of school quality (by grade and year) that, if omitted, could confound estimates of spillovers from peer mobility. In equation (2) above, these factors are denoted $\boldsymbol{\overline{X}_{sgt}}$**.** We do not report the coefficients on the SGY characteristics in Tables 5 and 6; they (and other control-variable coefficients) are reported in the appendix tables.

When the SGY characteristics are included, the coefficient on the SGY residential-move-school-change variable becomes smaller in both the math and ELA equations, and retains its significance in the math equation but not in the ELA equation. Furthermore, including the other SGY characteristics reduces the estimated coefficient on SGY foreclosure in the math equation to a value statistically indistinguishable from zero. This change suggests that any effects of foreclosure prevalence on individual scores actually reflect the influence of other observed peer characteristics that predict foreclosure prevalence, and/or that effects of SGY foreclosure actually indicate peer effects stemming from unobserved economic shocks—denoted θ_{sgt} and $\theta_{sg,t-1}$ in equation (2) above—which are better proxied by the peer characteristics than by peer foreclosures.[23] The coefficients on "SGY low-income status"and "SGY entered low-income status" are negative and statistically significant at the 5 percent level or better in both the math

[23] In our earlier research, we find that a similar pattern holds at the individual level. The estimated coefficients on individual foreclosure indicators become weaker, but remain negative, when we include individual student characteristics as controls.

and ELA regressions, providing support for this interpretation.[24]

Column 7 of Tables 2, 3, 5, and 6 includes SGY-level lagged values of the three school-change-by-reason variables. These lags reflect the previous year's school-change experiences of the current students in a given SGY. The lagged SGY residential-move-school-change variable obtains a negative coefficient that is significantly different from zero in the ELA equation but not in the math equation, suggesting an additional negative spillover or externality effect on students' ELA test scores in a given year of a concentration of students who changed schools for reason of a residential move during the *previous* school year. Even as the coefficient on the SGY lagged residential-move-school-change variable in the ELA equation is large and significant, its contemporaneous counterpart is neither. These results suggest a somewhat different mechanism for school-change influences on ELA test scores than on math scores, in that it appears not to be the disruption of this year's class by turnover per se, but rather the disruption of having a concentration of (this-year) grade-mates who may be unsettled on account of prior-year school changes.

Columns 8 and 9 of Tables 2, 3, 5, and 6 include SGY-level lagged test-score percentiles as ostensibly exogenous indicators of pre-existing "ability" of current student peers in equations without and with the SGY lagged school-change-by-reason variables.[25] The SGY lagged test-score percentile variables represent the 20th, 50th, and 80th percentiles of the previous year's test scores[26] of the student body of the current year school-by-grade. These scores are worthwhile controls because they help to control for (at least) two elements of peer group quality, which, if omitted, could confound estimates of mobility spillovers. One such element is the fixed cognitive ability of peers, which could be predictive of mobility while at the same time delivering achievement spillovers—for example, if low-aptitude peers (as opposed to high-aptitude peers) are both more likely to move (for example, in search of a better fit or for behavioral reasons) and less likely to impart knowledge spillovers to their peers. Another factor

[24] See tables A.1 and A.2 in the appendix.

[25] That is, column 8 adds the percentile measures to the equation in column 6 and column 9 adds them to the equation in column 7, the difference being the exclusion or inclusion of lagged SGY school-change variables.

[26] For 10th-grade students, these are the test scores from two years earlier; there are no 9th-grade tests.

embedded in lagged peer scores is the unobserved shocks these students suffered in the previous year, which might predict both the tendency to switch schools in the current year and the ongoing stressors in the current period that negatively affect other students' outcomes. The inclusion of the lagged percentile measures has very little impact on the estimated effect of SGY residential-move school changes and SGY lagged residential-move school changes on student test scores, indicating that these effects do not merely reflect spillovers stemming from either permanent or temporary aspects of peer ability that correlate with peer mobility status.

C. Overview of baseline school-grade-year results

The regression results reported in Tables 2, 3, 5, and 6 begin by analyzing the effects of foreclosures and school changes on the test scores of individual students who undergo these events, and then go on to analyze spillover effects of such events on the test scores of other students in the same school, grade, and year. Using administrative data concerning the reasons for individual school changes allows us to distinguish school changes associated with residential moves from changes resulting from parents' or student's requests (discretionary) and from changes occurring for other reasons, such as BPS staff-initiated moves to schools offering more appropriate bilingual or special education programs. We argue that residential-move school changes are more likely to be exogenous to unobserved SGY characteristics (such as teacher quality) in the context of Boston's school assignment system, which has a very loose link between residential location and school assignment.[27] These regressions provide evidence that students attending a SGY with higher prevalence of residential-move school changes suffer somewhat lower math test scores than students attending SGYs with a more stable student body.[28] This spillover effect holds up as additional controls are added, including other SGY characteristics and a measure of peer ability (lagged test score percentiles). For ELA test scores, we observe similarly robust negative spillover effects from lagged residential-move school changes among current peers, suggesting that peer adjustments to a new school and their

[27] In particular, parents' choice of residential location does not guarantee that their child will attend a specific school. Thus, the school changes resulting from residential moves are not likely to have been caused by parents seeking better schools within BPS.

[28] We find no spillover effect of foreclosures at the school-grade-year level beyond what is subsumed into the residential-move-school-change variable.

associated fall-out or spillovers may occur over an extended period. (However, the effects of one's own lagged residential-move school change on the ELA test score, while negative, are not statistically significant.) The fact that spillover effects of residential-move school changes appear to be larger and/or more immediate for math scores than for ELA scores is consistent with earlier research that finds stronger effects of a variety of school interventions on math scores than on ELA scores.[29]

Although we do not believe that residential-move school changes are *caused by* differences in unobserved school quality, our spillover results might still be biased if these two factors are correlated for other reasons. For example, lower school quality may be associated with both higher residential-move-induced turnover and worse test-score outcomes. To test our identification further, we undertake a series of robustness checks in the next section of the paper.

IV. Robustness Checks

In this section, we investigate the extent to which our principal results and conclusions hold up in the face of alternative specifications of the SGY effects. We first investigate whether school-change exits by reason are similar in their effects to the school in-move variables included above, as a way of testing the validity of the hypothesized mechanisms of in-mobility externalities. Then we examine specifications that include one-period leads of the SGY school-change-by-reason variables along with the lags shown in the earlier tables, as a way to assess the robustness of our effects to unobserved shocks that precipitate school changes. For similar reasons, we also test specifications that include leads of percentiles of the SGY test-score

[29] For example, Fryer (2014) finds implementation of "best practices" in traditional public schools significantly increases student math achievement in treated elementary and secondary schools and has little effect on reading achievement. He cites earlier evidence of other policies' exhibiting similar disparities in impact and offers two possible explanations for these findings. One is that that "the critical period for language development occurs early in life, while the critical period for developing higher cognitive functions extends into adolescence." Another is that "reading scores are influenced by the language spoken when students are outside of the classroom," while math scores are tied more closely to school activities. [Both quotations are taken from page 43 of Fryer's "advance access accepted manuscript" downloaded from http://qje.oxfordjournals.org/ at Federal Reserve Bank of Boston Research Library on July 17, 2014.]

distributions along with the lags included above. Finally, in order to further control for unobserved, common confounders at various levels, we test specifications that include fixed effects at the level of the school, or combinations of pairwise fixed effects at the level of the grade-by-year and/or the school-by-grade and/or the school-by-year.

A. School out-moves compared with school in-moves

Student mobility is measured in terms of students entering a school between October and June. One might instead ask how mobility measured by the prevalence of student exits from a school affects the rest of the student body. Tables 7 and 8 report SGY coefficient estimates for versions of the equations in columns 7–9 of Tables 2, 3, 5, and 6, substituting student out-move measures by reason for student in-move measures (or adding the former measures to equations including the latter measures). It seems likely that students' performance would be adversely affected by departures of some of their classmates during the school year, although for reasons other than a strain on school resources associated with assimilating new students. If out-moves have effects that are very similar to effects of in-moves, there might be reason to suspect that both in-moves and out-moves are merely capturing (the same) characteristics or circumstances of the broader student population that also predict variation in test scores.

Columns 4–6 (Tables 7 and 8) report estimates for coefficients on variables measuring the prevalence of out-moves by reason (for comparison, columns 1–3 of Tables 7 and 8 repeat columns 7–9 of the Tables 5 and 6, which include the in-move measures).[30] In the math equation, the prevalence of school exits for reasons of residential moves appears to have had no effect on test scores of other students, while exits for discretionary reasons obtain a strong negative coefficient and exits for other reasons are weakly negatively associated with other students' test scores. When both in-moves and out-moves by reason are included (columns 7–9 of Tables 7 and 8), the coefficients on the out-move variables are similar to those in columns 4–6. At the same time, the residential-move-school-change in-move variable becomes weaker but

[30] Note that the SGY ***lagged*** school-change-by-reason variables (included in all but columns 2, 5, and 8 of Tables 7 and 8) are the same for in-moves and out-moves. Recall that the lagged measures tally the prevalence of school changes during the previous school year (October to June) of all students in the current SGY student body.

remains significantly different from zero, while the other two in-move variables become indistinguishable from zero.

The lack of effect of school exits because of residential moves, in contrast with the significant effects of in-moves for the same reason, reinforces both the exogeneity of the residential-move-school-change variable and the notion that integrating new students (in-moves) is more disruptive to teaching and learning than adapting to loss of classmates. The apparent lack of robustness of the effects of in-moves driven by discretionary and other reasons to the inclusion of out-moves suggests that these latter types of in-moves are at least partly endogenous to unobserved SGY factors.

In the ELA equation (Table 8), the estimated coefficients on the out-move variables are never significantly different from zero. This holds regardless of whether or not we also include the in-move school-change-by-reason variables—results without in-moves are shown in columns 4–6 of Table 8 and results including in-moves are shown in columns 7–9. These ELA results seem consistent with the lack of significant effects of the corresponding contemporaneous in-move school-change variables in the earlier results (repeated in columns 1–3 of Table 8). Meanwhile, the significant negative effect of lagged SGY residential-move-school-changes on ELA test scores persists when the contemporaneous out-moves variables are included (columns 4, 6, 7, and 9).

B. Inclusion of leads of the SGY school change variables

Our interpretation of the negative coefficients on the SGY residential-move-school-change variable is that classroom disruptions associated with students entering a school following a residential move impose negative externalities on other students, resulting in lower test scores even among students who did not change schools. An alternative hypothesis, however, is that the SGY residential-move-school-change variable has no direct causal effect on test scores, but has a negative coefficient because it is correlated with unobserved variables that are associated with lower student test scores. Unobserved confounders were discussed at length in section II above. These include common economic shocks affecting many students in a SGY group, shocks that could induce both an uptick in SGY-level school changes and declines in test scores for movers and nonmovers alike. In the context of our conceptual model, confounding could occur in this case because the individual shock, ε_{it}—or its lag, $\varepsilon_{i,t\text{-}1}$—predicts mobility at the SGY level, even conditioning on individual mobility status, because not all students experiencing a shock will change schools. Alternatively, or in addition, confounding could occur if the shocks to peers, captured in the terms θ_{sgt} and $\theta_{sg,t-1}$, predict SGY mobility AND induce behaviors that impose negative externalities on students not directly affected by the shock, behaviors that would arise regardless of whether the affected students changed schools or not.

One way to test for such confounding shocks is to include one-year *leads* of the SGY school-change-by-reason variables in the test-score regressions. We construct these leads analogously to the lags of these variables (described in the previous section and included in the specifications reported in columns 7 and 9 of Tables 2, 3, 5, and 6); therefore, they describe the subsequent-year school changes of the current student body in a given SGY. Future school changes of current peers cannot have a direct causal influence on current test scores, but they could exhibit spurious effects that reflect shocks in the current period, such as those described just above, which harm current test scores within the SGY group and precipitate or predict the future school changes at the SGY level, even after conditioning on future school changes at the individual level. Therefore, including the leads in the regressions provides a falsification test for the effects of current and lagged peer mobility. If the future SGY school-change coefficients are statistically significant, conditional on the contemporaneous and lagged school-change variables, we should suspect that any apparent effects of current SGY school changes are actually driven by omitted variables.

Tables 9 (math) and 10 (ELA) display estimated coefficients from specifications including leads of the SGY school-change variables. As in the earlier SGY-level tables, we display the coefficients on only selected SGY variables (foreclosures, school change, and peer test scores), but a much broader group of student-level and SGY-level variables are controlled for in the underlying regressions (as indicated in the bottom panel of the tables). Column 1 of these tables does not include leads of the school-change variables, and is displayed to provide a benchmark for comparison with the other columns. Column 1 is very similar to the specification displayed in column 7 of Tables 5 and 6, although the regressions underlying Tables 9 and 10 condition on leads of the student-level school-change variables, while those in Tables 5 and 6 do not; these additional student-level controls, which control for current shocks to the individual insofar as these predict future individual school changes, have very little effect on the SGY-level coefficients of interest.

Column 2 of Tables 9 and 10 reports coefficient estimates from regressions that add one-year leads of the SGY school-change variables to the specification. Our main coefficients of interest are those on the SGY residential-move-school-change variables. We first discuss the results for the math test-score regressions (Table 9) and then turn to the ELA test-score regressions (Table 10). The magnitude of the contemporaneous SGY residential-move-school-change variable coefficient in the math regression decreases only slightly when the lead of the variable is added to the specification, and it remains highly significant. As before, the coefficient on the lagged variable is negative and not significantly different from zero. Most important, the coefficient on the lead of the SGY residential-move-school-change variable is positive and not even marginally statistically significant, indicating that the causal interpretation passes this falsification test.

Interestingly, the coefficient on the lead of the SGY discretionary-school-change variable is negative and marginally statistically significant, indicating that giving a causal interpretation to this effect fails the falsification test. Having peers who will make discretionary school changes next year is associated with lower student test scores. These results can be taken to suggest that these peers are a disruptive influence because they are experiencing adverse circumstances in the current year that will spur them to change schools in the following year. Alternatively, resources at the school for that grade may have been hit by a negative shock that year—such as a spike in teacher turnover—that negatively affected student outcomes and caused students or parents to request discretionary school changes, some/many of which may not come to pass until the following school year. That is, this lead variable may be a proxy for the endogenous discretionary exit rate of the current student body.

The effect of adding leads of the SGY school-change variables to the ELA test score regression, reported in Table 10, are qualitatively similar to those described above for the math test score regression. In the ELA regression, the contemporaneous SGY residential-move-school-change coefficient remains insignificant and the lagged SGY residential-move-school-change coefficient retains its significance when the lead of the variable is added. As in the math test score regression, the lead of the SGY residential-move-school-change variable is positive and far from significant; the causal interpretation again passes the falsification test. At the same time, the lead of other-school-changes does not pass the falsification test; the reverse causation or common shock interpretation offered above for discretionary school-changes in the math equation may apply here as well.

As a further robustness check, we conduct the same exercise of adding leads of the SGY school-change variables starting from a specification that already includes the SGY lagged test-score percentile variables (and lagged school-change variables). The results of this estimation are shown in columns 3 (base case) and 4 (adding the leads of the SGY school-change variables) of Tables 9 (Math) and 10 (ELA). The results of this robustness test are very similar to those described above.

C. Inclusion of leads of the SGY peer test score percentiles

The next set of robustness checks adds leads of the SGY 20th, 50th, and 80th percentiles of the test-score distribution to the regression specification. We include these leads as a further check against the concern that the SGY residential-move-school-change variable may be correlated with unobserved SGY-level characteristics or shocks that affect student test performance. The lagged test-score percentiles that we have already included in some of the specifications discussed above control for the scholastic achievement of peers prior to the start of the school year. Conditional on the lagged percentiles, the leads of the percentiles capture the effect of unobserved (by us) contemporaneous SGY influences on student achievement that persist beyond the current year. The performance of this year's peers on next year's test reflects in part the influence of current SGY quality, and so helps to further purge the SGY school-change coefficients of omitted variable bias.

Table 11 displays the result of this exercise for the math test-score regressions, and Table 12 for the ELA test-score regressions. Column 1 shows the base specification (equivalent to column 6 of Tables 5 and 6, but with fewer observations because inclusion of the leads of test scores requires that we drop 10^{th}-grade students). Column 2 adds percentiles of the SGY lagged test scores to the specification (as in column 8 of Tables 5 and 6, but again with fewer observations); column 3 adds leads of the SGY test scores to the specification; column 4 adds lags of the SGY school-change variables. Not surprisingly, the estimated coefficients on the leads of all three SGY test-score percentiles are positive and highly significant in both the math and the ELA regressions.

In the math test score regressions (Table 11), inclusion of leads of the SGY test-score percentiles brings down the magnitude of the estimated coefficient on SGY residential-move-school-changes somewhat, but it remains significant at better than the 1-percent level. In the ELA test-score regression (Table 12), the magnitude of the estimated SGY residential-move-school-change coefficient increases slightly (but remains far from significance) and the SGY lagged residential-move-school-change coefficient is smaller (but still significant) when leads of the SGY test-score percentiles are included.

Overall, including the leads of the SGY test-score percentiles provides further support for a causal interpretation of the SGY residential-move-school-change coefficients. The modest reduction in the magnitude of the SGY residential-move-school-change coefficient when conditioned on leads of the SGY test-score percentiles may indicate that the SGY residential-move-school-change coefficient is overstated to a slight degree due to omitted SGY characteristics.

D. Inclusion of fixed effects

The final set of robustness checks that we conduct is to examine how our estimates change as we introduce various combinations of fixed effects. Once again, the motivation is to provide additional controls for omitted variables, particularly latent SGY-level factors that may be correlated with the SGY school-change variables. Our main coefficients of interest are on variables measured at the SGY level, so we cannot include SGY fixed effects and still identify those coefficients. However, we can introduce pairwise combinations of grade-by-year, school-by-grade, and school-by-year fixed effects without losing identification, at least in theory.[31] In practice, some of the fixed-effects specifications appear to be hindered by lack of variation in the SGY-level variables within, for example, a given school-by-grade combination, because we observe a given school-by-grade only in a few years.

Our fixed-effects robustness checks are presented in Tables 13 (math) and 14 (ELA). The first column presents our baseline specification, which includes academic year (or just "year") and test grade (or just "grade") fixed effects; column 2 drops the separate year and grade effects, but adds pairwise grade-by-year fixed effects; column 3 adds school fixed effects to the column 2 specification. The next two columns follow this pattern, with a pairwise fixed effect combined with the remaining one-way fixed effect; that is, column 4 includes both school-by-year and grade fixed effects; the column 5 specification includes school-by-grade and year fixed effects. Column 6 reports estimates when all three pairwise fixed effects are included, and the final column (7) reverts to the simpler, additive, year, grade, and school fixed effects.

The SGY residential-move-school-change coefficient changes relatively little in the math test-score regressions (Table 13) as grade-by-year fixed effects (of which there are 21 for math) replace the separate, additive, grade and year effects, moving from column 1 to 2. The same is true of the lagged residential-move-school-change coefficient in the ELA equation as (in this case 20) grade-by-year fixed effects are substituted in column 2 of Table 14. The coefficients on SGY residential-move-school-change variables (lagged for ELA) weaken considerably but retain at least marginal significance in both math and ELA when school fixed effects (131 schools) are added, moving from column 2 to column 3.

However, when school-by-year and grade fixed effects[32] are included in the specification (column 4), the magnitudes of the estimated coefficients of interest are smaller than in the base

[31] This pairwise fixed effects approach is also taken by Hanushek, Kain, and Rivkin (2004), although they do not also include multilevel random effects.

[32] There are 298 school-by-grade combinations in math and 297 in ELA.

case, and become indistinguishable from zero in the math equation. Inclusion of school-by-grade plus year fixed effects[33] (column 5) slashes the size of the relevant ELA coefficient, leaving the estimated residential-move-school-change coefficients indistinguishable from zero in the ELA as well as in the math equations. And when all three pairwise fixed effects are included (column 6), many of the coefficients change even more, in some cases reversing signs. Our interpretation of the column 4–6 estimates is that the school-by-year or school-by-grade pairwise fixed effects are over-fitting the data, leading to parameter instability.[34]

The column 7 specification, which includes additive year, test grade, and school fixed effects, produces estimates that are similar to those in column 3. As noted above, these results are weaker than the specifications in columns 1 and 2, which do not include school fixed effects; the point estimate of the SGY residential-move-school-change coefficient (lagged coefficient in ELA) in column 7 (or 3) is only half as large as in columns 1 and 2. Overall, the specifications in Tables 13 and 14 indicate that unobserved factors at the level of the school—such as persistent characteristics of the underlying population—account for some, but not all, of the observed relationship between test scores and residential-move-school-changes.

V. Discussion and Conclusion

This paper investigates the degree to which there are spillover effects on other students of attending schools with a concentration of students experiencing foreclosure at home. Given the finding in our earlier research that the only significant effects of foreclosure on individual students' test scores seem to operate via school changes that are precipitated by foreclosure-induced residential moves, we focus on the extent of spillover effects associated with student mobility, and particularly on mobility induced by residential moves, whether occasioned by foreclosure or by other events.

[33] There are 499 non-empty school-by-year cells in math and 443 in ELA.

[34] Of the 131 schools, 29 have only one grade (for tests)—these are mostly high schools; 94 schools have two or fewer test grades, and only 15 schools have more than three of the six test grades. By contrast, only 15 schools report test scores for fewer than four school years. Recall that there are only 952 total SGYs in math (959 in ELA), out of an arithmetically possible tally of 3,144 (131 schools by 6 test grades by 4 school years); these "gaps" leave very little variation across SGYs to be explained within some of the pairwise cells.

We find reasonably robust evidence that higher student mobility during the school year, induced in particular by residential moves, imposes significant negative spillover effects on the test scores of the general student body (including children who have not changed schools) in the receiving school-by-grade during the current year (math) or the following year (ELA), controlling for a student's own mobility status and numerous other (fixed and time-varying) individual and school-level factors. We argue that in the context of the Boston Public Schools' school-assignment policy (during the years of our study), parents cannot change residences within the city with any realistic hope of obtaining a higher-quality school for their children. In addition, we provide a number of robustness checks, including falsification tests, to bolster the exogenous interpretation of residential-move school changes.

We find no robust, direct effects of a school's own foreclosure rate on the test scores of individual students at the school. Rather, foreclosures may exert negative externalities by contributing to residential moves and concomitant school changes, and therefore their effects may show up at schools receiving the foreclosure-displaced students rather than at the schools that experience high foreclosure rates under our measure. Furthermore, our results are consistent with the possibility that shocks to some families' finances—shocks that may precipitate foreclosure in some cases—could exert negative effects on the academic performance of peers. In particular, we observe that (1) a school's (SGY) foreclosure rate exerts negative effects on individual test scores in relatively uncontrolled models, (2) these effects become insignificant when we include, among other factors, the share of low-income peers and the share of peers that entered low-income status during the year, and (3) both of these more-direct measures of peer financial distress exert statistically significant, negative effects on both math and ELA test scores. Therefore, even if policies are adopted to reduce October-to-June student school changes stemming from foreclosures, the deterioration of family income and stability that underlies many foreclosures could still have negative effects on the performance of peers whose families are not directly subject to financial shocks.

The findings of negative spillovers stemming from residential-move school changes among peers—along with our evidence that such school changes negatively affect the school-changers themselves—suggest that the Boston Public Schools and other school systems could uniformly improve student performance by acting to reduce the incidence of such school moves or by attempting to mitigate the negative spillovers. The former might be accomplished by allowing students to finish out the school year at their current school even after a residential move; such a policy, however, would increase costs to the degree that it involved additional transportation expenses. By the same token, providing extra counseling and support for students, teachers, and administrators in schools and grades with many incoming students might lessen the disruptions associated with higher turnover, although such interventions are also not costless. This paper's results suggest that added transportation and/or staffing costs should be weighed against potentially measurable positive effects on student test scores of such system-wide interventions.

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Table 1: Summary Statistics of Student Level Variables

	Math Sample		ELA Sample	
	Mean	Std. Dev.	Mean	Std. Dev.
Lag Foreclosure Petition	0.0181	0.133	0.0188	0.136
Foreclosure Petition	0.0230	0.150	0.0235	0.151
Lead Foreclosure Petition	0.0258	0.158	0.0254	0.157
Residential Move	0.124	0.330	0.127	0.333
Discretionary-School-Change	0.00624	0.0788	0.00619	0.0784
Res-Move-School-Change	0.00543	0.0735	0.00596	0.0769
Other-School-Change	0.00898	0.0943	0.00909	0.0949
Lag Discretionary-School-Change	0.00737	0.0855	0.00736	0.0855
Lag Res-Move-School-Change	0.00947	0.0969	0.0103	0.101
Lag Other-School-Change	0.0131	0.114	0.0127	0.112
Lead Discretionary-School-Change	0.00916	0.0953	0.00916	0.0953
Lead Res-Move-School-Change	0.00403	0.0633	0.00461	0.0677
Lead Other-School-Change	0.0129	0.113	0.0141	0.118
Normed Math	-0.444	1.069		
Lag Normed Math	-0.477	1.071		
Normed ELA			-0.557	1.121
Lag Normed ELA			-0.600	1.143
Summer School Change Indicators				
Nonstructural School Change (Jul 1 to Oct 1)	0.0507	0.219	0.0550	0.228
Structural School Change	0.141	0.348	0.138	0.345
School Change Discontinuity	0.0117	0.108	0.00661	0.0810
Student Characteristics				
Male	0.505	0.500	0.507	0.500
White	0.136	0.343	0.135	0.341
Asian	0.0934	0.291	0.0916	0.288
Hispanic/Latino	0.334	0.472	0.334	0.472
Native American	0.00386	0.0620	0.00378	0.0614
Multi-race	0.00506	0.0710	0.00517	0.0717
LEP	0.145	0.353	0.151	0.358
Special Education	0.207	0.405	0.212	0.409
Low-income	0.776	0.417	0.785	0.411
Entered Low-income Status	0.0659	0.248	0.0641	0.245
Exited Low-income Status	0.0815	0.274	0.0783	0.269
Owner (Name Match)	0.186	0.389	0.183	0.387
Condo	0.0157	0.124	0.0156	0.124
1 Family	0.144	0.351	0.142	0.349
4+ family	0.164	0.370	0.167	0.373
Exempt	0.218	0.413	0.221	0.415
Other	0.0374	0.190	0.0373	0.190
No Scaled Score (ELA)	0.00290	0.0538	0.00338	0.0580
No Scaled Score (Math)	0.00207	0.0454	0.00184	0.0429
Test Grade 3	0	0	0	0
Test Grade 4	0.152	0.359	0.211	0.408
Test Grade 5	0.189	0.392	0.196	0.397
Test Grade 6	0.138	0.344	0.142	0.349
Test Grade 8	0.156	0.362	0.218	0.413
Test Grade 10	0.173	0.378	0.0875	0.283
2005/2006	0.144	0.351	0.162	0.369
2007/2008	0.285	0.451	0.294	0.456
2008/2009	0.283	0.450	0.291	0.454
Observations	54145		52056	

Table 2: Math Test Score Regressions — Student Level Coefficients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lag Foreclosure Petition	-0.032^{+}	-0.032^{+}	-0.034^{+}	-0.034^{+}	-0.034^{+}	-0.034^{+}	-0.034^{+}	-0.034^{+}	-0.034^{+}
	(0.019)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Foreclosure Petition	-0.019	-0.019	-0.019	-0.018	-0.018	-0.018	-0.018	-0.018	-0.018
	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Lead Foreclosure Petition	-0.054***	-0.054***	-0.054***	-0.053***	-0.053***	-0.053***	-0.053***	-0.053***	-0.053***
	(0.016)	(0.016)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Residential Move	0.009	0.009	0.009	0.009	0.009	0.010	0.009	0.010	0.009
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Discretionary-School-Change	-0.149***	-0.148***	-0.152***	-0.152***	-0.149***	-0.150***	-0.149***	-0.149***	-0.149***
	(0.032)	(0.032)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Res-Move-School-Change	-0.237***	-0.235***	-0.239***	-0.239***	-0.233***	-0.234***	-0.234***	-0.234***	-0.234***
	(0.039)	(0.039)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)
Other-School-Change	-0.271***	-0.269***	-0.270***	-0.270***	-0.267***	-0.268***	-0.268***	-0.269***	-0.269***
	(0.031)	(0.031)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
Lag Discretionary-School-Change		-0.055^{+}	-0.055^{+}	-0.054^{+}	-0.054^{+}	-0.054^{+}	-0.054^{+}	-0.054^{+}	-0.054^{+}
		(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Lag Res-Move-School-Change		-0.038	-0.040	-0.040	-0.039	-0.039	-0.037	-0.039	-0.037
		(0.026)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
Lag Other-School-Change		-0.032	-0.030	-0.031	-0.029	-0.028	-0.031	-0.028	-0.031
		(0.026)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Lag Normed Math	0.696***	0.695***	0.697***	0.697***	0.697***	0.696***	0.696***	0.695***	0.695***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.284***	0.285***	-0.095***	-0.080**	-0.048	0.188^{+}	0.196*	0.200^{+}	0.202*
	(0.044)	(0.044)	(0.028)	(0.029)	(0.030)	(0.098)	(0.098)	(0.103)	(0.103)
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Fixed Effects	Yes	Yes	No	No	No	No	No	No	No
SGY Characteristics	No	No	No	No	No	Yes	Yes	Yes	Yes
Test Grade	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Year	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Random Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Note: See Table 5 for additional included SGY-level variables.									
R-squared	0.740	0.740							
Observations	54145	54145	54145	54145	54145	54145	54145	54145	54145

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3: ELA Test Score Regressions — Student Level Coefficients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lag Foreclosure Petition	-0.063**	-0.064**	-0.067**	-0.066**	-0.067**	-0.066**	-0.066**	-0.066**	-0.066**
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Foreclosure Petition	0.016	0.017	0.016	0.018	0.018	0.017	0.017	0.017	0.017
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Lead Foreclosure Petition	-0.015	-0.015	-0.016	-0.016	-0.016	-0.016	-0.016	-0.016	-0.016
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Residential Move	0.002	0.002	0.002	0.002	0.002	0.003	0.003	0.002	0.002
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Discretionary-School-Change	-0.106**	-0.106**	-0.106**	-0.107**	-0.104**	-0.103**	-0.103**	-0.103**	-0.103**
	(0.038)	(0.038)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)
Res-Move-School-Change	-0.155***	-0.152***	-0.158***	-0.158***	-0.153***	-0.154***	-0.154***	-0.154***	-0.154***
	(0.043)	(0.043)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)
Other-School-Change	-0.256***	-0.254***	-0.259***	-0.259***	-0.257***	-0.258***	-0.257***	-0.258***	-0.258***
	(0.037)	(0.037)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Lag Discretionary-School-Change		-0.059^{+}	-0.056^{+}	-0.055^{+}	-0.055^{+}	-0.055^{+}	-0.058^{+}	-0.055^{+}	-0.058^{+}
		(0.035)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Lag Res-Move-School-Change		-0.040	-0.046^{+}	-0.046^{+}	-0.045	-0.044	-0.038	-0.044	-0.038
		(0.031)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Lag Other-School-Change		-0.080**	-0.080**	-0.080**	-0.079**	-0.079**	-0.080**	-0.079**	-0.080**
		(0.031)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
Lag Normed ELA	0.634***	0.633***	0.636***	0.636***	0.635***	0.634***	0.634***	0.633***	0.633***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.098^{+}	0.100^{+}	-0.019	-0.006	0.026	0.194*	0.202*	0.181^{+}	0.188^{+}
	(0.052)	(0.052)	(0.030)	(0.031)	(0.032)	(0.098)	(0.097)	(0.102)	(0.102)
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Fixed Effects	Yes	Yes	No	No	No	No	No	No	No
SGY Characteristics	No	No	No	No	No	Yes	Yes	Yes	Yes
Test Grade	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Year	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Random Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Note: See Table 6 for additional included SGY-level variables.									
R-squared	0.696	0.696							
Observations	52056	52056	52056	52056	52056	52056	52056	52056	52056

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 4: Summary Statistics of School-Grade-Year (SGY) Level Variables

	Math Sample		ELA Sample	
	Mean	Std. Dev.	Mean	Std. Dev.
SGY Foreclosure	0.0237	0.0229	0.0238	0.0234
SGY Foreclosure-School-Change	0.000997	0.00378	0.00102	0.00382
SGY Net Enrollment Change	0.0708	0.0583	0.0715	0.0568
SGY Discretionary-School-Change	0.00797	0.0120	0.00786	0.0120
SGY Res-Move-School-Change	0.0109	0.0146	0.0112	0.0148
SGY Other-School-Change	0.0134	0.0185	0.0135	0.0190
SGY Discretionary-School-Change - Out	0.00850	0.0114	0.00793	0.0112
SGY Res-Move-School-Change - Out	0.00864	0.0130	0.00921	0.0134
SGY Other-School-Change - Out	0.0156	0.0170	0.0156	0.0171
SGY Lag Discretionary-School-Change	0.00772	0.0131	0.00742	0.0126
SGY Lag Res-Move-School-Change	0.0103	0.0160	0.0112	0.0164
SGY Lag Other-School-Change	0.0157	0.0222	0.0151	0.0223
SGY Lead Discretionary-School-Change	0.0105	0.0140	0.0100	0.0139
SGY Lead Res-Move-School-Change	0.00474	0.0101	0.00522	0.0105
SGY Lead Other-School-Change	0.0156	0.0180	0.0163	0.0190
SGY Lag Math P20	-1.299	0.767		
SGY Lag Math P50	-0.470	0.674		
SGY Lag Math P80	0.304	0.541		
SGY Lead Math P20*	-1.310	0.731		
SGY Lead Math P50*	-0.507	0.704		
SGY Lead Math P80*	0.284	0.641		
SGY Lag ELA P20			-1.478	0.782
SGY Lag ELA P50			-0.510	0.596
SGY Lag ELA P80			0.263	0.444
SGY Lead ELA P20**			-1.418	0.750
SGY Lead ELA P50**			-0.472	0.589
SGY Lead ELA P80**			0.268	0.460
SGY Characteristics				
SGY LEP	0.156	0.140	0.165	0.143
SGY Low-income	0.754	0.167	0.767	0.163
SGY Special Education	0.213	0.113	0.216	0.111
SGY White	0.142	0.153	0.140	0.152
SGY Hispanic/Latino	0.337	0.200	0.339	0.206
SGY Asian	0.0878	0.121	0.0868	0.124
SGY Male	0.514	0.0713	0.517	0.0688
SGY Entered Low-income Status	0.0656	0.0392	0.0633	0.0378
SGY Exited Low-income Status	0.0768	0.0471	0.0726	0.0440
SGY No Scaled Score (Math)	0.00897	0.0167	0.00916	0.0168
SGY No Scaled Score (ELA)	0.00788	0.0138	0.00767	0.0138
Observations	54145		52056	

Notes: * Sample size is 42067, ** Sample size is 44828

Table 5: Math Test Score Regressions — School-Grade-Year (SGY) Level Coefficients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SGY Foreclosure				-0.686*	-0.680*	-0.235	-0.211	-0.242	-0.218
				(0.316)	(0.313)	(0.321)	(0.322)	(0.321)	(0.321)
SGY Foreclosure-School-Change				-0.490	0.782	0.842	1.020	0.774	0.934
				(1.786)	(1.796)	(1.771)	(1.771)	(1.769)	(1.768)
SGY Discretionary-School-Change					-1.253*	-1.093+	-1.003	-1.157+	-1.072+
					(0.627)	(0.628)	(0.629)	(0.628)	(0.629)
SGY Res-Move-School-Change					-2.449***	-2.022***	-2.026***	-2.010***	-2.019***
					(0.578)	(0.585)	(0.586)	(0.586)	(0.586)
SGY Other-School-Change					-1.018*	-0.570	-0.888+	-0.619	-0.936+
					(0.441)	(0.479)	(0.504)	(0.480)	(0.505)
SGY Lag Discretionary-School-Change							-0.033		-0.047
							(0.512)		(0.512)
SGY Lag Res-Move-School-Change							-0.561		-0.529
							(0.438)		(0.442)
SGY Lag Other-School-Change							0.767*		0.751*
							(0.346)		(0.346)
SGY Lag Math P20								0.065*	0.063*
								(0.028)	(0.028)
SGY Lag Math P50								-0.059	-0.064
								(0.046)	(0.046)
SGY Lag Math P80								0.018	0.025
								(0.040)	(0.040)
Net Enrollment Change					0.294	0.442*	0.463*	0.499*	0.520*
					(0.206)	(0.219)	(0.218)	(0.221)	(0.220)
Std. Deviation of Constant			-1.434***	-1.437***	-1.455***	-1.480***	-1.484***	-1.484***	-1.487***
			(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Std. Deviation of Residual			-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Fixed Effects	Yes	Yes	No	No	No	No	No	No	No
SGY Characteristics	No	No	No	No	No	Yes	Yes	Yes	Yes
Test Grade	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Year	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Random Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Note: See Table 2 for additional included student-level variables.									
R-squared	0.740	0.740							
Observations	54145	54145	54145	54145	54145	54145	54145	54145	54145

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows:
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6: ELA Test Score Regressions — School-Grade-Year (SGY) Level Coefficients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SGY Foreclosure				-0.533+	-0.499	0.154	0.166	0.127	0.140
				(0.308)	(0.306)	(0.310)	(0.309)	(0.309)	(0.308)
SGY Foreclosure-School-Change				-0.651	0.469	0.392	0.905	0.303	0.784
				(1.773)	(1.789)	(1.735)	(1.727)	(1.728)	(1.721)
SGY Discretionary-School-Change					-0.619	-0.558	-0.523	-0.669	-0.624
					(0.608)	(0.599)	(0.598)	(0.598)	(0.597)
SGY Res-Move-School-Change					-1.279*	-0.658	-0.542	-0.614	-0.528
					(0.562)	(0.559)	(0.558)	(0.561)	(0.559)
SGY Other-School-Change					-0.083	0.032	0.038	-0.026	-0.039
					(0.418)	(0.450)	(0.473)	(0.449)	(0.472)
SGY Lag Discretionary-School-Change							0.946+		0.923+
							(0.516)		(0.515)
SGY Lag Res-Move-School-Change							-1.432***		-1.329**
							(0.413)		(0.415)
SGY Lag Other-School-Change							0.203		0.233
							(0.340)		(0.339)
SGY Lag ELA P20								0.067**	0.060*
								(0.025)	(0.024)
SGY Lag ELA P50								-0.056	-0.057
								(0.043)	(0.043)
SGY Lag ELA P80								0.037	0.039
								(0.042)	(0.042)
Net Enrollment Change					-0.213	-0.056	-0.053	0.027	0.019
					(0.205)	(0.212)	(0.211)	(0.213)	(0.212)
Std. Deviation of Constant			-1.485***	-1.488***	-1.500***	-1.548***	-1.558***	-1.555***	-1.563***
			(0.029)	(0.029)	(0.029)	(0.030)	(0.030)	(0.030)	(0.030)
Std. Deviation of Residual			-0.471***	-0.471***	-0.471***	-0.471***	-0.471***	-0.471***	-0.471***
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Fixed Effects	Yes	Yes	No	No	No	No	No	No	No
SGY Characteristics	No	No	No	No	No	Yes	Yes	Yes	Yes
Test Grade	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Year	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Random Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Note: See Table 3 for additional included student-level variables.									
R-squared	0.696	0.696							
Observations	52056	52056	52056	52056	52056	52056	52056	52056	52056

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows:
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 7: Math Test Score Regressions, Including Out Mobility — School-Grade-Year (SGY) Level Coefficients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SGY Foreclosure	-0.211	-0.242	-0.218	-0.192	-0.218	-0.196	-0.231	-0.248	-0.228
	(0.322)	(0.321)	(0.321)	(0.322)	(0.321)	(0.322)	(0.322)	(0.321)	(0.321)
SGY Foreclosure-School-Change	1.020	0.774	0.934	0.502	0.342	0.434	1.014	0.797	0.955
	(1.771)	(1.769)	(1.768)	(1.778)	(1.775)	(1.776)	(1.768)	(1.767)	(1.768)
SGY Discretionary-School-Change	-1.003	-1.157^{+}	-1.072^{+}				-0.459	-0.552	-0.459
	(0.629)	(0.628)	(0.629)				(0.583)	(0.580)	(0.582)
SGY Res-Move-School-Change	-2.026***	-2.010***	-2.019***				-1.190*	-1.145*	-1.144*
	(0.586)	(0.586)	(0.586)				(0.504)	(0.509)	(0.509)
SGY Other-School-Change	-0.888^{+}	-0.619	-0.936^{+}				-0.225	0.072	-0.219
	(0.504)	(0.480)	(0.505)				(0.429)	(0.406)	(0.431)
SGY Discretionary-School-Change - Out				-1.297*	-1.316*	-1.249*	-1.048^{+}	-1.059^{+}	-1.000^{+}
				(0.602)	(0.600)	(0.601)	(0.602)	(0.601)	(0.602)
SGY Res-Move-School-Change - Out				0.227	0.133	0.180	0.393	0.309	0.344
				(0.556)	(0.555)	(0.556)	(0.559)	(0.558)	(0.559)
SGY Other-School-Change - Out				-0.903*	-0.862^{+}	-0.881^{+}	-0.803^{+}	-0.782^{+}	-0.795^{+}
				(0.449)	(0.450)	(0.450)	(0.449)	(0.450)	(0.450)
SGY Lag Discretionary-School-Change	-0.033		-0.047	-0.064		-0.089	0.015		-0.007
	(0.512)		(0.512)	(0.512)		(0.512)	(0.512)		(0.513)
SGY Lag Res-Move-School-Change	-0.561		-0.529	-0.538		-0.506	-0.447		-0.435
	(0.438)		(0.442)	(0.444)		(0.448)	(0.443)		(0.447)
SGY Lag Other-School-Change	0.767*		0.751*	0.681*		0.656*	0.755*		0.731*
	(0.346)		(0.346)	(0.328)		(0.329)	(0.346)		(0.346)
SGY Lag Math P20		0.065*	0.063*		0.060*	0.057*		0.056*	0.054^{+}
		(0.028)	(0.028)		(0.029)	(0.028)		(0.028)	(0.028)
SGY Lag Math P50		-0.059	-0.064		-0.049	-0.052		-0.052	-0.056
		(0.046)	(0.046)		(0.046)	(0.046)		(0.046)	(0.046)
SGY Lag Math P80		0.018	0.025		0.010	0.015		0.007	0.013
		(0.040)	(0.040)		(0.040)	(0.040)		(0.040)	(0.040)
Net Enrollment Change	0.463*	0.499*	0.520*	-0.006	0.057	0.030			
	(0.218)	(0.221)	(0.220)	(0.154)	(0.155)	(0.156)			
Std. Deviation of Constant	-1.484***	-1.484***	-1.487***	-1.481***	-1.480***	-1.484***	-1.485***	-1.484***	-1.487***
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Std. Deviation of Residual	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Random Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Note: See Table 2 for additional included student-level variables.									
R-squared									
Observations	54145	54145	54145	54145	54145	54145	54145	54145	54145

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8: ELA Test Score Regressions, Including Out Mobility — School-Grade-Year (SGY) Level Coefficients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SGY Foreclosure	0.166	0.127	0.140	0.160	0.129	0.139	0.132	0.103	0.113
	(0.309)	(0.309)	(0.308)	(0.308)	(0.308)	(0.307)	(0.308)	(0.308)	(0.307)
SGY Foreclosure-School-Change	0.905	0.303	0.784	0.681	0.085	0.555	0.668	0.126	0.600
	(1.727)	(1.728)	(1.721)	(1.726)	(1.727)	(1.722)	(1.719)	(1.720)	(1.714)
SGY Discretionary-School-Change	-0.523	-0.669	-0.624				-0.546	-0.587	-0.569
	(0.598)	(0.598)	(0.597)				(0.555)	(0.553)	(0.553)
SGY Res-Move-School-Change	-0.542	-0.614	-0.528				-0.437	-0.390	-0.352
	(0.558)	(0.561)	(0.559)				(0.477)	(0.485)	(0.482)
SGY Other-School-Change	0.038	-0.026	-0.039				-0.008	0.029	-0.008
	(0.473)	(0.449)	(0.472)				(0.402)	(0.379)	(0.402)
SGY Discretionary-School-Change - Out				-0.908	-0.945	-0.843	-0.881	-0.897	-0.807
				(0.586)	(0.588)	(0.586)	(0.587)	(0.589)	(0.587)
SGY Res-Move-School-Change - Out				-0.172	-0.331	-0.142	-0.086	-0.248	-0.069
				(0.528)	(0.526)	(0.526)	(0.531)	(0.529)	(0.529)
SGY Other-School-Change - Out				-0.612	-0.594	-0.590	-0.596	-0.573	-0.571
				(0.423)	(0.425)	(0.423)	(0.423)	(0.425)	(0.423)
SGY Lag Discretionary-School-Change	0.946^{+}		0.923^{+}	0.957^{+}		0.923^{+}	1.003^{+}		0.973^{+}
	(0.516)		(0.515)	(0.514)		(0.513)	(0.515)		(0.515)
SGY Lag Res-Move-School-Change	-1.432^{***}		-1.329^{**}	-1.366^{**}		-1.287^{**}	-1.340^{**}		-1.257^{**}
	(0.413)		(0.415)	(0.416)		(0.417)	(0.417)		(0.418)
SGY Lag Other-School-Change	0.203		0.233	0.276		0.287	0.237		0.258
	(0.340)		(0.339)	(0.320)		(0.320)	(0.339)		(0.339)
SGY Lag ELA P20		0.067^{**}	0.060^{*}		0.064^{**}	0.058^{*}		0.065^{**}	0.059^{*}
		(0.025)	(0.024)		(0.024)	(0.024)		(0.024)	(0.024)
SGY Lag ELA P50		-0.056	-0.057		-0.057	-0.059		-0.058	-0.060
		(0.043)	(0.043)		(0.043)	(0.043)		(0.043)	(0.043)
SGY Lag ELA P80		0.037	0.039		0.032	0.035		0.034	0.037
		(0.042)	(0.042)		(0.042)	(0.042)		(0.042)	(0.042)
Net Enrollment Change	-0.053	0.027	0.019	-0.124	-0.080	-0.080			
	(0.211)	(0.213)	(0.212)	(0.150)	(0.152)	(0.152)			
Std. Deviation of Constant	-1.558^{***}	-1.555^{***}	-1.563^{***}	-1.560^{***}	-1.557^{***}	-1.565^{***}	-1.561^{***}	-1.558^{***}	-1.566^{***}
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Std. Deviation of Residual	-0.471^{***}	-0.471^{***}	-0.471^{***}	-0.471^{***}	-0.471^{***}	-0.471^{***}	-0.471^{***}	-0.471^{***}	-0.471^{***}
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Random Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Note: See Table 3 for additional included student-level variables.									
R-squared									
Observations	52056	52056	52056	52056	52056	52056	52056	52056	52056

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 9: Math Test Score Regressions, Adding Leads of SGY School Changes — School-Grade-Year (SGY) Level Coefficients

	(1)	(2)	(3)	(4)
SGY Foreclosure	-0.205	-0.203	-0.213	-0.209
	(0.322)	(0.321)	(0.321)	(0.320)
SGY Foreclosure-School-Change	0.942	0.749	0.860	0.679
	(1.770)	(1.766)	(1.767)	(1.763)
SGY Discretionary-School-Change	-0.993	-0.913	-1.063+	-0.982
	(0.629)	(0.628)	(0.629)	(0.628)
SGY Res-Move-School-Change	-2.025***	-1.931***	-2.017***	-1.923**
	(0.585)	(0.586)	(0.585)	(0.587)
SGY Other-School-Change	-0.895+	-0.843+	-0.942+	-0.886+
	(0.504)	(0.503)	(0.505)	(0.504)
SGY Lag Discretionary-School-Change	-0.043	-0.111	-0.058	-0.126
	(0.512)	(0.511)	(0.512)	(0.512)
SGY Lag Res-Move-School-Change	-0.545	-0.456	-0.510	-0.414
	(0.438)	(0.439)	(0.442)	(0.443)
SGY Lag Other-School-Change	0.754*	0.713*	0.738*	0.695*
	(0.346)	(0.346)	(0.346)	(0.346)
SGY Lead Discretionary-School-Change		-1.013+		-0.986+
		(0.518)		(0.518)
SGY Lead Res-Move-School-Change		0.237		0.160
		(0.642)		(0.642)
SGY Lead Other-School-Change		-0.629		-0.624
		(0.411)		(0.411)
SGY Lag Math P20			0.063*	0.061*
			(0.028)	(0.028)
SGY Lag Math P50			-0.062	-0.056
			(0.046)	(0.046)
SGY Lag Math P80			0.025	0.019
			(0.040)	(0.040)
Net Enrollment Change	0.468*	0.469*	0.525*	0.526*
	(0.218)	(0.219)	(0.220)	(0.221)
Std. Deviation of Constant	-1.485***	-1.489***	-1.488***	-1.492***
	(0.027)	(0.027)	(0.027)	(0.027)
Std. Deviation of Residual	-0.598***	-0.598***	-0.598***	-0.598***
	(0.003)	(0.003)	(0.003)	(0.003)
Student Characteristics	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes
SGY Characteristics	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes
School Year	Yes	Yes	Yes	Yes
SGY Random Effects	Yes	Yes	Yes	Yes
Note: See Table 2 for additional included student-level variables.				
R-squared				
Observations	54145	54145	54145	54145

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 10: ELA Test Score Regressions, Adding Leads of SGY School Changes — School-Grade-Year (SGY) Level Coefficients

	(1)	(2)	(3)	(4)
SGY Foreclosure	0.175	0.186	0.148	0.162
	(0.309)	(0.308)	(0.308)	(0.307)
SGY Foreclosure-School-Change	0.854	0.711	0.733	0.599
	(1.726)	(1.724)	(1.721)	(1.719)
SGY Discretionary-School-Change	-0.512	-0.467	-0.614	-0.568
	(0.598)	(0.598)	(0.597)	(0.597)
SGY Res-Move-School-Change	-0.553	-0.545	-0.538	-0.530
	(0.558)	(0.559)	(0.559)	(0.560)
SGY Other-School-Change	0.015	0.015	-0.062	-0.063
	(0.473)	(0.472)	(0.472)	(0.471)
SGY Lag Discretionary-School-Change	0.939+	0.898+	0.916+	0.877+
	(0.516)	(0.515)	(0.515)	(0.515)
SGY Lag Res-Move-School-Change	-1.414***	-1.345**	-1.309**	-1.244**
	(0.413)	(0.415)	(0.415)	(0.416)
SGY Lag Other-School-Change	0.194	0.195	0.225	0.220
	(0.340)	(0.340)	(0.339)	(0.339)
SGY Lead Discretionary-School-Change		-0.248		-0.293
		(0.505)		(0.503)
SGY Lead Res-Move-School-Change		0.265		0.146
		(0.601)		(0.603)
SGY Lead Other-School-Change		-0.767+		-0.717+
		(0.401)		(0.400)
SGY Lag ELA P20			0.061*	0.060*
			(0.024)	(0.024)
SGY Lag ELA P50			-0.057	-0.057
			(0.043)	(0.043)
SGY Lag ELA P80			0.039	0.037
			(0.042)	(0.042)
Net Enrollment Change	-0.048	-0.031	0.025	0.041
	(0.211)	(0.211)	(0.212)	(0.213)
Std. Deviation of Constant	-1.558***	-1.561***	-1.564***	-1.567***
	(0.030)	(0.030)	(0.030)	(0.030)
Std. Deviation of Residual	-0.472***	-0.472***	-0.472***	-0.472***
	(0.003)	(0.003)	(0.003)	(0.003)
Student Characteristics	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes
SGY Characteristics	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes
School Year	Yes	Yes	Yes	Yes
SGY Random Effects	Yes	Yes	Yes	Yes
Note: See Table 3 for additional included student-level variables.				
R-squared				
Observations	52056	52056	52056	52056

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 11: Math Test Score Regressions, Adding Leads of Peer Test Score Percentiles — School-Grade-Year (SGY) Level Coefficients

	(1)	(2)	(3)	(4)
SGY Foreclosure	-0.213	-0.213	0.069	0.052
	(0.330)	(0.330)	(0.288)	(0.288)
SGY Foreclosure-School-Change	0.661	0.549	0.373	0.490
	(1.792)	(1.790)	(1.564)	(1.562)
SGY Discretionary-School-Change	-0.833	-0.908	-0.850	-0.851
	(0.657)	(0.657)	(0.573)	(0.575)
SGY Res-Move-School-Change	-1.643**	-1.681**	-1.431**	-1.460**
	(0.604)	(0.604)	(0.525)	(0.525)
SGY Other-School-Change	-0.243	-0.315	0.039	-0.203
	(0.524)	(0.526)	(0.465)	(0.497)
SGY Lag Discretionary-School-Change				0.558
				(0.492)
SGY Lag Res-Move-School-Change				-0.197
				(0.401)
SGY Lag Other-School-Change				0.487
				(0.336)
SGY Lag Math P20		0.060*	0.011	0.008
		(0.029)	(0.028)	(0.028)
SGY Lag Math P50		-0.071	-0.169***	-0.169***
		(0.049)	(0.043)	(0.043)
SGY Lag Math P80		0.025	-0.075+	-0.072+
		(0.043)	(0.041)	(0.041)
SGY Lead Math P20			0.154***	0.156***
			(0.032)	(0.033)
SGY Lead Math P50			0.164***	0.158***
			(0.041)	(0.042)
SGY Lead Math P80			0.171***	0.176***
			(0.038)	(0.038)
Net Enrollment Change	0.329	0.384	0.337	0.347+
	(0.235)	(0.237)	(0.207)	(0.207)
Std. Deviation of Constant	-1.488***	-1.491***	-1.685***	-1.689***
	(0.030)	(0.030)	(0.033)	(0.033)
Std. Deviation of Residual	-0.592***	-0.592***	-0.583***	-0.583***
	(0.003)	(0.003)	(0.003)	(0.003)
Student Characteristics	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes
SGY Characteristics	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes
School Year	Yes	Yes	Yes	Yes
SGY Random Effects	Yes	Yes	Yes	Yes
Note: See Table 2 for additional included student-level variables.				
R-squared				
Observations	44265	44265	42067	42067

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All columns exclude 10th grade students.

Table 12: ELA Test Score Regressions, Adding Leads of Peer Test Score Percentiles — School-Grade-Year (SGY) Level Coefficients

	(1)	(2)	(3)	(4)
SGY Foreclosure	0.147	0.118	-0.086	-0.086
	(0.320)	(0.319)	(0.290)	(0.289)
SGY Foreclosure-School-Change	0.326	0.190	0.117	0.364
	(1.774)	(1.769)	(1.615)	(1.611)
SGY Discretionary-School-Change	-0.666	-0.759	-1.024+	-1.004+
	(0.621)	(0.619)	(0.564)	(0.565)
SGY Res-Move-School-Change	-0.558	-0.548	-0.684	-0.648
	(0.578)	(0.580)	(0.526)	(0.526)
SGY Other-School-Change	0.005	-0.063	-0.147	-0.232
	(0.475)	(0.474)	(0.436)	(0.462)
SGY Lag Discretionary-School-Change				0.763
				(0.496)
SGY Lag Res-Move-School-Change				-0.815*
				(0.395)
SGY Lag Other-School-Change				0.322
				(0.336)
SGY Lag ELA P20		0.065*	-0.005	-0.007
		(0.025)	(0.026)	(0.026)
SGY Lag ELA P50		-0.065	-0.126**	-0.128**
		(0.045)	(0.041)	(0.041)
SGY Lag ELA P80		0.041	-0.053	-0.049
		(0.044)	(0.041)	(0.041)
SGY Lead ELA P20			0.178***	0.175***
			(0.027)	(0.027)
SGY Lead ELA P50			0.079*	0.078+
			(0.040)	(0.040)
SGY Lead ELA P80			0.217***	0.217***
			(0.040)	(0.040)
Net Enrollment Change	-0.024	0.053	0.140	0.141
	(0.221)	(0.223)	(0.203)	(0.203)
Std. Deviation of Constant	-1.533***	-1.538***	-1.685***	-1.692***
	(0.031)	(0.031)	(0.034)	(0.034)
Std. Deviation of Residual	-0.463***	-0.463***	-0.458***	-0.458***
	(0.003)	(0.003)	(0.003)	(0.003)
Student Characteristics	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes
SGY Characteristics	Yes	Yes	Yes	Yes
Test Grade	Yes	Yes	Yes	Yes
School Year	Yes	Yes	Yes	Yes
SGY Random Effects	Yes	Yes	Yes	Yes
Note: See Table 3 for additional included student-level variables.				
R-squared				
Observations	46995	46995	44828	44828

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All columns exclude 10th grade students.

Table 13: Math Test Score Regressions, Comparing Combinations of Fixed Effects — School-Grade-Year (SGY) Level Coefficients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SGY Foreclosure	-0.211	-0.161	-0.171	-0.509	-0.079	-0.495^{+}	-0.186
	(0.322)	(0.322)	(0.303)	(0.315)	(0.264)	(0.257)	(0.303)
SGY Foreclosure-School-Change	1.020	1.216	0.726	-1.927	0.795	-1.916	0.564
	(1.771)	(1.775)	(1.538)	(1.607)	(1.371)	(1.206)	(1.539)
SGY Discretionary-School-Change	-1.003	-0.934	-0.169	-0.377	0.100	0.143	-0.134
	(0.629)	(0.627)	(0.561)	(0.583)	(0.499)	(0.493)	(0.565)
SGY Res-Move-School-Change	-2.026***	-1.978***	-0.985^{+}	-0.543	-0.409	0.690	-0.989^{+}
	(0.586)	(0.584)	(0.551)	(0.558)	(0.501)	(0.453)	(0.556)
SGY Other-School-Change	-0.888^{+}	-0.820	-0.427	0.752	-0.400	0.909^{+}	-0.393
	(0.504)	(0.504)	(0.527)	(0.634)	(0.460)	(0.491)	(0.528)
SGY Lag Discretionary-School-Change	-0.033	0.039	0.715	0.628	0.047	-0.180	0.616
	(0.512)	(0.513)	(0.455)	(0.494)	(0.395)	(0.392)	(0.456)
SGY Lag Res-Move-School-Change	-0.561	-0.640	-0.277	-0.228	0.050	-0.019	-0.174
	(0.438)	(0.438)	(0.400)	(0.443)	(0.346)	(0.345)	(0.402)
SGY Lag Other-School-Change	0.767*	0.698*	0.185	-0.409	0.192	-0.538^{+}	0.289
	(0.346)	(0.347)	(0.337)	(0.361)	(0.310)	(0.321)	(0.337)
Net Enrollment Change	0.463*	0.444*	0.314	0.207	0.099	-0.254	0.275
	(0.218)	(0.218)	(0.210)	(0.228)	(0.185)	(0.182)	(0.212)
Std. Deviation of Constant	-1.484***	-1.492***	-1.777***	-2.180***	-2.140***	-4.188***	-1.762***
	(0.027)	(0.028)	(0.031)	(0.039)	(0.037)	(0.507)	(0.031)
Std. Deviation of Residual	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	No	No	Yes	No	No	Yes
School Year	Yes	No	No	No	Yes	No	Yes
School Fixed Effects	No	No	Yes	No	No	No	Yes
Grade–Year Fixed Effects	No	Yes	Yes	No	No	Yes	No
School–Year Fixed Effects	No	No	No	Yes	No	Yes	No
School-Grade Fixed Effects	No	No	No	No	Yes	Yes	No
SGY Random Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Note: See Table 2 for additional included student-level variables.							
R-squared							
Observations	54145	54145	54145	54145	54145	54145	54145

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 14: ELA Test Score Regressions, Comparing Combinations of Fixed Effects — School-Grade-Year (SGY) Level Coefficients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SGY Foreclosure	0.166	0.233	0.118	-0.555^{+}	0.185	-0.370	0.065
	(0.309)	(0.308)	(0.295)	(0.315)	(0.247)	(0.264)	(0.295)
SGY Foreclosure-School-Change	0.905	1.088	-0.147	-3.294*	-0.104	-2.581*	-0.237
	(1.727)	(1.726)	(1.529)	(1.619)	(1.293)	(1.253)	(1.526)
SGY Discretionary-School-Change	-0.523	-0.499	-0.173	-0.606	0.167	0.360	-0.189
	(0.598)	(0.594)	(0.545)	(0.567)	(0.465)	(0.474)	(0.548)
SGY Res-Move-School-Change	-0.542	-0.561	0.527	0.820	0.844^{+}	1.757***	0.552
	(0.558)	(0.554)	(0.531)	(0.543)	(0.462)	(0.454)	(0.535)
SGY Other-School-Change	0.038	-0.047	-0.224	1.182*	0.101	1.761***	-0.177
	(0.473)	(0.472)	(0.524)	(0.592)	(0.442)	(0.480)	(0.523)
SGY Lag Discretionary-School-Change	0.946^{+}	0.889^{+}	1.247**	1.624**	0.583	-0.292	1.296**
	(0.516)	(0.513)	(0.464)	(0.494)	(0.388)	(0.405)	(0.465)
SGY Lag Res-Move-School-Change	-1.432***	-1.433***	-0.830*	-1.170**	-0.282	0.099	-0.827*
	(0.413)	(0.412)	(0.392)	(0.415)	(0.322)	(0.328)	(0.393)
SGY Lag Other-School-Change	0.203	0.227	0.046	-0.704^{+}	-0.279	-1.003**	0.042
	(0.340)	(0.339)	(0.348)	(0.386)	(0.301)	(0.327)	(0.348)
Net Enrollment Change	-0.053	-0.048	0.048	0.233	-0.267	-0.466**	0.064
	(0.211)	(0.209)	(0.206)	(0.218)	(0.174)	(0.181)	(0.207)
Std. Deviation of Constant	-1.558***	-1.569***	-1.833***	-2.198***	-2.329***	-22.905***	-1.821***
	(0.030)	(0.030)	(0.034)	(0.043)	(0.050)	(0.642)	(0.034)
Std. Deviation of Residual	-0.471***	-0.471***	-0.471***	-0.471***	-0.471***	-0.473***	-0.471***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Summer School Change	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test Grade	Yes	No	No	Yes	No	No	Yes
School Year	Yes	No	No	No	Yes	No	Yes
School Fixed Effects	No	No	Yes	No	No	No	Yes
Grade–Year Fixed Effects	No	Yes	Yes	No	No	Yes	No
School–Year Fixed Effects	No	No	No	Yes	No	Yes	No
School-Grade Fixed Effects	No	No	No	No	Yes	Yes	No
SGY Random Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Note: See Table 3 for additional included student-level variables.							
R-squared							
Observations	52056	52056	52056	52056	52056	52056	52056

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix A.1 — Math Test Score Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lag Foreclosure Petition	-0.032+	-0.032+	-0.034+	-0.034+	-0.034+	-0.034+	-0.034+	-0.034+	-0.034+
	(0.019)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Foreclosure Petition	-0.019	-0.019	-0.019	-0.018	-0.018	-0.018	-0.018	-0.018	-0.018
	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Lead Foreclosure Petition	-0.054***	-0.054***	-0.054***	-0.053***	-0.053***	-0.053***	-0.053***	-0.053***	-0.053***
	(0.016)	(0.016)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Residential Move	0.009	0.009	0.009	0.009	0.009	0.010	0.009	0.010	0.009
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Discretionary-School-Change	-0.149***	-0.148***	-0.152***	-0.152***	-0.149***	-0.150***	-0.149***	-0.149***	-0.149***
	(0.032)	(0.032)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Res-Move-School-Change	-0.237***	-0.235***	-0.239***	-0.239***	-0.233***	-0.234***	-0.234***	-0.234***	-0.234***
	(0.039)	(0.039)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)
Other-School-Change	-0.271***	-0.269***	-0.270***	-0.270***	-0.267***	-0.268***	-0.268***	-0.269***	-0.269***
	(0.031)	(0.031)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
Lag Discretionary-School-Change		-0.055+	-0.055+	-0.054+	-0.054+	-0.054+	-0.054+	-0.054+	-0.054+
		(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Lag Res-Move-School-Change		-0.038	-0.040	-0.040	-0.039	-0.039	-0.037	-0.039	-0.037
		(0.026)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
Lag Other-School-Change		-0.032	-0.030	-0.031	-0.029	-0.028	-0.031	-0.028	-0.031
		(0.026)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Lag Normed Math	0.696***	0.695***	0.697***	0.697***	0.697***	0.696***	0.696***	0.695***	0.695***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Nonstructural School Change (Jul 1 to Oct 1)	-0.014	-0.013	-0.014	-0.014	-0.013	-0.014	-0.014	-0.014	-0.014
	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Structural School Change	-0.060+	-0.060+	-0.049+	-0.048+	-0.043	-0.054*	-0.054*	-0.058*	-0.058*
	(0.036)	(0.036)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
School-Change Discontinuity	-0.027	-0.027	-0.051	-0.051	-0.044	-0.049	-0.048	-0.048	-0.046
	(0.042)	(0.042)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)
Male	0.003	0.003	0.002	0.002	0.002	0.003	0.003	0.003	0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
White	0.105***	0.105***	0.109***	0.109***	0.108***	0.105***	0.105***	0.105***	0.105***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Asian	0.248***	0.248***	0.253***	0.252***	0.251***	0.248***	0.248***	0.248***	0.248***
	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Hispanic/Latino	0.043***	0.043***	0.046***	0.046***	0.045***	0.043***	0.043***	0.043***	0.043***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Native American	0.009	0.009	0.005	0.005	0.005	0.004	0.004	0.004	0.004
	(0.032)	(0.032)	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)
Multi-race	0.055+	0.055+	0.055	0.055	0.055	0.054	0.053	0.054	0.053
	(0.032)	(0.032)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)
LEP	-0.027***	-0.027***	-0.025***	-0.025***	-0.026***	-0.027***	-0.027***	-0.027***	-0.027***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Special Education	-0.263***	-0.263***	-0.263***	-0.263***	-0.262***	-0.262***	-0.262***	-0.262***	-0.262***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Low-income	-0.035***	-0.035***	-0.038***	-0.038***	-0.037***	-0.035***	-0.035***	-0.035***	-0.035***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)

Entered Low-income Status	-0.047***	-0.047***	-0.049***	-0.049***	-0.049***	-0.046***	-0.046***	-0.046***	-0.046***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Exited Low-income Status	0.010	0.010	0.009	0.009	0.009	0.009	0.009	0.009	0.009
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Owner (Name Match)	0.028***	0.028***	0.029***	0.029***	0.029***	0.028***	0.028***	0.028***	0.028***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Condo	0.004	0.005	0.003	0.002	0.002	0.002	0.002	0.002	0.002
	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
1 Family	0.009	0.009	0.011	0.011	0.011	0.010	0.010	0.010	0.010
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
4+ family	-0.013+	-0.013+	-0.012+	-0.013+	-0.013+	-0.013+	-0.013+	-0.013+	-0.013+
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Exempt	-0.014*	-0.014*	-0.013+	-0.014*	-0.014*	-0.014*	-0.014*	-0.014*	-0.014*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Other	0.001	0.001	0.002	0.002	0.002	0.001	0.001	0.001	0.001
	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
No Scaled Score (ELA)	-0.170**	-0.169**	-0.169***	-0.169***	-0.169***	-0.168***	-0.168***	-0.168***	-0.168***
	(0.054)	(0.054)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)
No Scaled Score (Math)	-0.916***	-0.915***	-0.916***	-0.917***	-0.915***	-0.915***	-0.916***	-0.916***	-0.917***
	(0.061)	(0.061)	(0.053)	(0.053)	(0.053)	(0.053)	(0.053)	(0.053)	(0.053)
2005/2006			0.066*	0.058*	0.047+	0.057*	0.060*	0.055+	0.057*
			(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
2007/2008			0.043+	0.049*	0.048*	0.046*	0.049*	0.046*	0.048*
			(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
2008/2009			0.005	0.006	0.005	0.003	0.006	0.003	0.006
			(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
SGY Foreclosure				-0.686*	-0.680*	-0.235	-0.211	-0.242	-0.218
				(0.316)	(0.313)	(0.321)	(0.322)	(0.321)	(0.321)
SGY Foreclosure-School-Change				-0.490	0.782	0.842	1.020	0.774	0.934
				(1.786)	(1.796)	(1.771)	(1.771)	(1.769)	(1.768)
SGY Discretionary-School-Change					-1.253*	-1.093+	-1.003	-1.157+	-1.072+
					(0.627)	(0.628)	(0.629)	(0.628)	(0.629)
SGY Res-Move-School-Change					-2.449***	-2.022***	-2.026***	-2.010***	-2.019***
					(0.578)	(0.585)	(0.586)	(0.586)	(0.586)
SGY Other-School-Change					-1.018*	-0.570	-0.888+	-0.619	-0.936+
					(0.441)	(0.479)	(0.504)	(0.480)	(0.505)
SGY Lag Discretionary-School-Change							-0.033		-0.047
							(0.512)		(0.512)
SGY Lag Res-Move-School-Change							-0.561		-0.529
							(0.438)		(0.442)
SGY Lag Other-School-Change							0.767*		0.751*
							(0.346)		(0.346)
SGY Lag Math P20								0.065*	0.063*
								(0.028)	(0.028)
SGY Lag Math P50								-0.059	-0.064
								(0.046)	(0.046)
SGY Lag Math P80								0.018	0.025
								(0.040)	(0.040)

SGY LEP						0.022	0.024	0.044	0.043
						(0.078)	(0.078)	(0.081)	(0.081)
SGY Low-income						-0.309***	-0.306***	-0.283**	-0.279**
						(0.091)	(0.091)	(0.094)	(0.093)
SGY Special Education						-0.047	-0.110	-0.015	-0.080
						(0.085)	(0.089)	(0.087)	(0.092)
SGY White						0.050	0.044	0.029	0.027
						(0.073)	(0.073)	(0.077)	(0.076)
SGY Hispanic/Latino						0.154**	0.155**	0.148**	0.149**
						(0.053)	(0.053)	(0.054)	(0.054)
SGY Asian						0.182*	0.177*	0.171+	0.167+
						(0.088)	(0.087)	(0.094)	(0.093)
SGY Male						-0.111	-0.119	-0.085	-0.095
						(0.111)	(0.111)	(0.112)	(0.111)
SGY Entered Low-income Status						-0.487*	-0.493*	-0.479*	-0.485*
						(0.217)	(0.217)	(0.218)	(0.217)
SGY Exited Low-income Status						0.018	0.023	0.000	0.006
						(0.181)	(0.181)	(0.181)	(0.181)
SGY No Scaled Score (Math)						0.389	0.249	0.349	0.202
						(0.561)	(0.565)	(0.562)	(0.566)
SGY No Scaled Score (ELA)						-0.788	-0.747	-0.742	-0.701
						(0.644)	(0.642)	(0.643)	(0.641)
Net Enrollment Change					0.294	0.442*	0.463*	0.499*	0.520*
					(0.206)	(0.219)	(0.218)	(0.221)	(0.220)
Constant	0.284***	0.285***	-0.095***	-0.080**	-0.048	0.188+	0.196*	0.200+	0.202*
	(0.044)	(0.044)	(0.028)	(0.029)	(0.030)	(0.098)	(0.098)	(0.103)	(0.103)
Std. Deviation of Constant			-1.434***	-1.437***	-1.455***	-1.480***	-1.484***	-1.484***	-1.487***
			(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Std. Deviation of Residual			-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
SGY Fixed Effects	Yes	Yes	No	No	No	No	No	No	No
Test Grade	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Random Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.740	0.740							
Observations	54145	54145	54145	54145	54145	54145	54145	54145	54145

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows:
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix A.2 — ELA Test Score Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lag Foreclosure Petition	-0.063**	-0.064**	-0.067**	-0.066**	-0.067**	-0.066**	-0.066**	-0.066**	-0.066**
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Foreclosure Petition	0.016	0.017	0.016	0.018	0.018	0.017	0.017	0.017	0.017
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Lead Foreclosure Petition	-0.015	-0.015	-0.016	-0.016	-0.016	-0.016	-0.016	-0.016	-0.016
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Residential Move	0.002	0.002	0.002	0.002	0.002	0.003	0.003	0.002	0.002
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Discretionary-School-Change	-0.106**	-0.106**	-0.106**	-0.107**	-0.104**	-0.103**	-0.103**	-0.103**	-0.103**
	(0.038)	(0.038)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)
Res-Move-School-Change	-0.155***	-0.152***	-0.158***	-0.158***	-0.153***	-0.154***	-0.154***	-0.154***	-0.154***
	(0.043)	(0.043)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)
Other-School-Change	-0.256***	-0.254***	-0.259***	-0.259***	-0.257***	-0.258***	-0.257***	-0.258***	-0.258***
	(0.037)	(0.037)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Lag Discretionary-School-Change		-0.059+	-0.056+	-0.055+	-0.055+	-0.055+	-0.058+	-0.055+	-0.058+
		(0.035)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Lag Res-Move-School-Change		-0.040	-0.046+	-0.046+	-0.045	-0.044	-0.038	-0.044	-0.038
		(0.031)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Lag Other-School-Change		-0.080**	-0.080**	-0.080**	-0.079**	-0.079**	-0.080**	-0.079**	-0.080**
		(0.031)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
Lag Normed ELA	0.634***	0.633***	0.636***	0.636***	0.635***	0.634***	0.634***	0.633***	0.633***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Nonstructural School Change (Jul 1 to Oct 1)	-0.017	-0.015	-0.014	-0.015	-0.014	-0.015	-0.015	-0.015	-0.015
	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Structural School Change	0.008	0.007	-0.032	-0.032	-0.025	-0.030	-0.028	-0.035	-0.032
	(0.044)	(0.044)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
School-Change Discontinuity	-0.003	-0.004	-0.009	-0.011	-0.002	-0.012	-0.012	-0.010	-0.011
	(0.057)	(0.057)	(0.057)	(0.057)	(0.057)	(0.056)	(0.056)	(0.056)	(0.056)
Male	-0.086***	-0.086***	-0.086***	-0.086***	-0.086***	-0.086***	-0.086***	-0.086***	-0.086***
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
White	0.084***	0.084***	0.094***	0.094***	0.093***	0.083***	0.083***	0.084***	0.084***
	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Asian	0.172***	0.172***	0.179***	0.179***	0.177***	0.171***	0.171***	0.171***	0.171***
	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)
Hispanic/Latino	0.030***	0.030***	0.036***	0.036***	0.035***	0.030***	0.030***	0.031***	0.030***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Native American	0.041	0.040	0.037	0.037	0.037	0.035	0.035	0.035	0.035
	(0.042)	(0.042)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)
Multi-race	0.039	0.039	0.038	0.037	0.038	0.035	0.035	0.035	0.035
	(0.034)	(0.034)	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)
LEP	-0.143***	-0.143***	-0.141***	-0.141***	-0.141***	-0.142***	-0.142***	-0.143***	-0.143***
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Special Education	-0.402***	-0.401***	-0.399***	-0.399***	-0.399***	-0.400***	-0.400***	-0.401***	-0.401***
	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Low-income	-0.056***	-0.056***	-0.060***	-0.060***	-0.059***	-0.055***	-0.055***	-0.055***	-0.055***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)

Entered Low-income Status	-0.044***	-0.044***	-0.049***	-0.049***	-0.048***	-0.043**	-0.043**	-0.044**	-0.044**
	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Exited Low-income Status	0.017	0.017	0.017	0.017	0.017	0.016	0.016	0.016	0.016
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Owner (Name Match)	0.032***	0.032***	0.034***	0.034***	0.033***	0.032***	0.032***	0.032***	0.032***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Condo	0.013	0.014	0.010	0.010	0.009	0.009	0.010	0.010	0.010
	(0.021)	(0.021)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
1 Family	-0.003	-0.003	-0.001	-0.001	-0.001	-0.003	-0.003	-0.003	-0.003
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
4+ family	-0.012	-0.013	-0.013	-0.013	-0.013	-0.013	-0.013	-0.013	-0.013
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Exempt	-0.017*	-0.017*	-0.018*	-0.019*	-0.019*	-0.019*	-0.019*	-0.019*	-0.019*
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Other	-0.001	-0.001	0.002	0.002	0.002	0.001	0.001	0.001	0.001
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
No Scaled Score (ELA)	-1.590***	-1.587***	-1.589***	-1.588***	-1.588***	-1.589***	-1.589***	-1.590***	-1.590***
	(0.075)	(0.075)	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)
No Scaled Score (Math)	-0.112	-0.109	-0.106	-0.106	-0.104	-0.106	-0.106	-0.106	-0.106
	(0.079)	(0.079)	(0.065)	(0.065)	(0.065)	(0.065)	(0.065)	(0.065)	(0.065)
2005/2006			-0.014	-0.021	-0.023	-0.004	0.002	-0.001	0.004
			(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
2007/2008			0.030	0.035	0.036	0.031	0.034	0.030	0.033
			(0.023)	(0.023)	(0.023)	(0.022)	(0.022)	(0.022)	(0.022)
2008/2009			-0.002	-0.001	0.002	-0.005	-0.000	-0.008	-0.003
			(0.023)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
SGY Foreclosure				-0.533+	-0.499	0.154	0.166	0.127	0.140
				(0.308)	(0.306)	(0.310)	(0.309)	(0.309)	(0.308)
SGY Foreclosure-School-Change				-0.651	0.469	0.392	0.905	0.303	0.784
				(1.773)	(1.789)	(1.735)	(1.727)	(1.728)	(1.721)
SGY Discretionary-School-Change					-0.619	-0.558	-0.523	-0.669	-0.624
					(0.608)	(0.599)	(0.598)	(0.598)	(0.597)
SGY Res-Move-School-Change					-1.279*	-0.658	-0.542	-0.614	-0.528
					(0.562)	(0.559)	(0.558)	(0.561)	(0.559)
SGY Other-School-Change					-0.083	0.032	0.038	-0.026	-0.039
					(0.418)	(0.450)	(0.473)	(0.449)	(0.472)
SGY Lag Discretionary-School-Change							0.946+		0.923+
							(0.516)		(0.515)
SGY Lag Res-Move-School-Change							-1.432***		-1.329**
							(0.413)		(0.415)
SGY Lag Other-School-Change							0.203		0.233
							(0.340)		(0.339)
SGY Lag ELA P20								0.067**	0.060*
								(0.025)	(0.024)
SGY Lag ELA P50								-0.056	-0.057
								(0.043)	(0.043)
SGY Lag ELA P80								0.037	0.039
								(0.042)	(0.042)

SGY LEP						-0.014	-0.024	0.059	0.039
						(0.074)	(0.074)	(0.080)	(0.080)
SGY Low-income						-0.346***	-0.339***	-0.300**	-0.300***
						(0.089)	(0.088)	(0.091)	(0.091)
SGY Special Education						0.040	0.028	0.124	0.097
						(0.084)	(0.088)	(0.091)	(0.094)
SGY White						0.224**	0.203**	0.192**	0.178*
						(0.070)	(0.070)	(0.073)	(0.073)
SGY Hispanic/Latino						0.176***	0.177***	0.156**	0.160**
						(0.050)	(0.050)	(0.050)	(0.050)
SGY Asian						0.162*	0.150+	0.129	0.122
						(0.083)	(0.082)	(0.084)	(0.084)
SGY Male						-0.007	-0.020	0.026	0.009
						(0.109)	(0.109)	(0.110)	(0.109)
SGY Entered Low-income Status						-0.704***	-0.723***	-0.662**	-0.687**
						(0.213)	(0.212)	(0.215)	(0.213)
SGY Exited Low-income Status						0.092	0.118	0.094	0.120
						(0.179)	(0.178)	(0.179)	(0.178)
SGY No Scaled Score (Math)						0.807	0.859	0.795	0.829
						(0.543)	(0.545)	(0.541)	(0.543)
SGY No Scaled Score (ELA)						-0.350	-0.368	-0.325	-0.344
						(0.624)	(0.620)	(0.622)	(0.619)
Net Enrollment Change					-0.213	-0.056	-0.053	0.027	0.019
					(0.205)	(0.212)	(0.211)	(0.213)	(0.212)
Constant	0.098+	0.100+	-0.019	-0.006	0.026	0.194*	0.202*	0.181+	0.188+
	(0.052)	(0.052)	(0.030)	(0.031)	(0.032)	(0.098)	(0.097)	(0.102)	(0.102)
Std. Deviation of Constant			-1.485***	-1.488***	-1.500***	-1.548***	-1.558***	-1.555***	-1.563***
			(0.029)	(0.029)	(0.029)	(0.030)	(0.030)	(0.030)	(0.030)
Std. Deviation of Residual			-0.471***	-0.471***	-0.471***	-0.471***	-0.471***	-0.471***	-0.471***
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
SGY Fixed Effects	Yes	Yes	No	No	No	No	No	No	No
Test Grade	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SGY Random Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.696	0.696							
Observations	52056	52056	52056	52056	52056	52056	52056	52056	52056

Notes: Standard errors (in parentheses) are clustered by student identification number. Significance is denoted as follows:
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.