Student Mobility in Milwaukee:

The Effects of School Transfers on Mobile and Non-Mobile Students

Susanna Loeb

Stanford University

Jon Valant

Stanford University

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Abstract

This article explores student mobility in the Milwaukee Public Schools (MPS) and its effects on student achievement. An urban district with plentiful opportunities for school choice, Milwaukee has a transient student population. From 2003-04 through 2007-08, 11% of MPS students switched schools or left the district between the fall and spring of a given school year, while 22% were mobile between the spring of one year and the fall of the following year. Using both student fixed-effects and instrumental variables approaches, we examined the effects of this mobility on both the students who moved and their classmates who did not. We found evidence that mobile students' test score gains dropped immediately after they switched schools, but these students typically recovered their losses in the subsequent year. We detected modest but statistically significant negative effects of grade-level turnover on non-mobile students' academic achievement.

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A family's decision to move a child from one school to another has deep and broad implications. For the mobile child, it means adjusting to new people, policies, and procedures while transitioning away from a familiar setting. For the child's new teacher, it means quickly gathering information to integrate the child into the classroom's activities and culture. For the child's new classmates, it means acclimating to a new personality who might draw disproportionately on their teacher's time and resources. Finally, for the child's past and present schools, it means handling disruptions ranging from fluctuating class sizes and teacher frustrations to textbook exchanges and immunization paperwork.

This article examines student mobility in Milwaukee. We begin by describing mobility within and between academic years, focusing only on non-promotional moves when the child is not graduating from elementary to middle school or middle to high school. We then estimate mobility's effects on the academic outcomes of both mobile students and their non-mobile classmates. Although intuitively, mobility might seem like a plainly negative phenomenon, neither academic theory nor prior research offers a definitive answer as to whether mobility, on balance, is more disruptive than productive. On the one hand, student transfers create real educational, social, and administrative challenges; on the other, they could produce a better sorting of students such that families leave bad situations for better ones. In a city like Milwaukee, where families have extraordinary discretion over which schools their children attend – and opportunities to revisit those choices during and between years – the effects of mobility can be especially consequential.

Although many researchers have studied student mobility, its effects remain inadequately understood due to the methodological hurdles confronting this work. This study contributes to the existing literature in several ways. First, it distinguishes between students who transfer within and between years, and between students who change and do not change residences at the time of their school transfers. Second, it uses both student fixed-effects and instrumental variables strategies to reduce possible omitted variable bias and more carefully describe the mechanisms at work. Third, it estimates mobility's causal effects on non-mobile students' achievement. Finally, it utilizes the unusual timing of Wisconsin's state testing program to examine differences in the short-term and long-term effects of switching schools.

The causes and effects of student mobility

Students change schools for many reasons. Perhaps the most innocuous of these is passing through the final grade in one's school and advancing to middle school or high school. We do not treat these "promotional" movers as mobile and do not examine the effects of such moves. Among non-promotional movers, residential relocation is a common cause of student mobility. The U.S. Census Bureau estimated that 12% of the American population changed its place of residence over a one-year period from 2007 to 2008, including 13% of five-to-nine-year olds and 10% of ten-to-14-year olds (U.S. Census Bureau, 2010a). Relocation is especially common among Americans in low-income households (U.S. Census Bureau, 2010b), of which there are many in Milwaukee. Examining the reasons behind student mobility in Chicago, Kerbow (1996) noted that 58% of mobile sixth-grade students reported that their school change was accompanied by residential relocation. An examination of NELS data yielded similar findings, as 58% of parents reported a change of residence as a reason for school transfers between grades eight and twelve (Rumberger, Larson, Ream, & Palardy, 1999).

Although residential relocation accounts for a sizable portion of student mobility, many students switch schools without relocating and some residential movers change residences precisely to find a better school.[1] For some, school transfers follow a careful consideration of the options available and how they compare to the status quo; for others, a change in school or family circumstances requires an immediate, unexpected switch (e.g., because of divorce, job loss, or personality conflicts with teachers or peers). The mobility literature now distinguishes between “strategic” moves, which are deliberate and planned, and “reactive” moves, which are not (Rumberger et al., 1999). This distinction is helpful, and we refer to it at several points in this article. For example, it might help to reconcile differences in the theoretical treatment of student mobility by researchers of different disciplines. Economists tend to emphasize the benefits of a strategic type of Tiebout sorting in which families migrate toward the schools best suited for their children, leading to an efficient provision of educational services (Hanushek, Kain, & Rivkin, 2004). Psychologists, sociologists, and educational scholars, however, often worry about the effects of reactive mobility on children’s emotional wellbeing (Rhodes, 2000), families’ access to social capital and attachment to schools (Coleman, 1988; South, Haynie, & Bose, 2007), and teachers’ instruction, classroom management, and morale (Lash & Kirkpatrick, 1990; Lash & Kirkpatrick, 1994). The distinction between “strategic” and “reactive” mobility might be helpful in understanding why some students benefit from school changes and others do not.

Additional complexity in thinking about student mobility comes from the subtly different ways in which school transfers can affect students. A mobile student experiences separation from a particular prior school setting, the uncertainty associated with being new to an

[1] NELS survey responses suggested that over one-fourth of those who cited residential change as the reason for school change moved in order to enroll their children in a particular school or program (Rumberger et al., 1999).

environment of unfamiliar faces and procedures, and integration into a particular new school setting. The effects of mobility therefore arise through a combination of a general experience of upheaval and the particular differences between one's prior and new school (e.g., in academic rigor). Furthermore, these effects can be conflated with effects from other significant life events that often accompany school transfers. Disentangling the effects of these phenomena is difficult, both theoretically and methodologically, but our data provide some useful opportunities to do so.

Prior Research

Scholars from a great number of decades and disciplines have studied the effects of student mobility. Of greatest relevance to this paper are studies that described the movement between schools, estimated the effects of mobility on mobile students, and estimated the effects of mobility on other, non-mobile students.

Describing the extent of student mobility[2]

Most American children undergo at least one non-promotional school change during their academic careers. Rumberger et al. (1999) showed that 61% of students enrolled in 8th grade in 1988 switched schools at least once between grades 1 and 12, with well over half of the mobile students making two or more non-promotional school changes. This is consistent with a U.S. General Accounting Office finding that over 40% of all third graders had changed schools at least once since the first grade (U.S. General Accounting Office, 1994). Examining mobility at the state level, Hanushek, Kain, and Rivkin (2001) found that roughly one-third of Texas public school students changed schools, non-promotionally, between grades 4 and 7. At the city level, 36% of public school 6th graders in Chicago switched schools at least once over a two-year period in the 1990s (Kerbow, 1996). This same study showed that the average Chicago

[2] We caution against comparing mobility rates across studies, as researchers have used an assortment of methods for defining which students are mobile (see Ligon & Paredes, 1992).

elementary school saw 20% of its students leave between September 1993 and September 1994.

The incidence of student mobility is not uniform across the population. Researchers have consistently found greater mobility rates among low-income, minority, and urban students and for schools serving disproportionately large numbers of these students (Rumberger, 2003). For example, the study of mobility in grades 4-7 in Texas showed that Black and Hispanic students were considerably more likely to move at least once than White students, while low-income students were considerably more likely to move than non-low-income students (Hanushek, Kain, & Rivkin, 2001). At the school level, a study of mobility in North Carolina found that urban schools experienced much higher rates of turnover than suburban and rural schools, and schools with the greatest concentrations of minority and low-income students experienced particularly high turnover (Xu, Hannaway, & D'Souza, 2009). The North Carolina study also uncovered high turnover rates in districts with magnet schools or choice-based enrollment, which is particularly relevant to this study of Milwaukee, where families have considerable discretion over which schools their children attend (and where barriers to moving between schools are low).

The effects of mobility on mobile students' academic outcomes

Perhaps the most commonly addressed question in the mobility literature is that of the effect of changing schools on the mobile student's academic outcomes. The majority of research on the topic suggests that mobility is harmful to students, but these findings are not universal and the studies vary in the reasonableness of their causal assertions.

Given that mobile students, on average, tend to be less advantaged than non-mobile students, we should expect their post-move (and pre-move) test scores to be lower than those of non-movers. Not surprisingly then, analyses that have not controlled for these differences have typically found that mobile students score lower on standardized tests after moving than their non-mobile classmates. Once controls have been introduced for observable variables correlated with both outcomes and propensity to change schools, the results have been more modest. Temple and Reynolds (1999) studied low-income Black children in Chicago and explained that frequent movers score far below more stable students on math and reading exams, but half of that difference was accounted for with an assortment of control variables (and considerably more than half when controlling for prior scores). Upon including covariates related to student background, a few studies found statistically insignificant effects of mobility (e.g., Alexander, Entwisle, & Dauber, 1996) or, at times, positive effects (Swanson & Schneider, 1999). In general, though, researchers have found slightly negative effects of mobility on student outcomes (e.g., Rumberger et al., 1999; Ingersoll, Scamman, & Eckerling, 1989; Lee & Smith, 1999), with effects particularly large for urban students (e.g., Xu, Hannaway, & D'Souza, 2009) and those who change schools regularly (Hartman, 2002).

Two more recent trends in the research on student mobility have helped to improve our understanding of mobility's effects. The first is the use of student fixed-effects models. Including student fixed-effects helps to combat problems that arise if mobile and non-mobile students differ in ways that are unobservable to researchers and correlated with test scores. For example, if parents who move their children from school to school are particularly unconcerned (or concerned) about their children's achievement on standardized tests, then even a model with numerous controls related to student background will likely produce biased estimates. Student fixed-effects models, which compare a student's test score gains in the year(s) immediately following a move to that student's typical gains in non-mobile years, help to remove any time-invariant, unobservable factors. Two recent papers employed a student-fixed effects strategy:

Hanushek, Kain, and Rivkin (2004) used it to examine statewide Texas data, while Xu, Hannaway, and D'Souza (2009) used it to examine statewide North Carolina data. Both of these studies also benefited from a second recent trend in student mobility literature: finding observable proxies for "strategic" and "reactive" movers. By differentiating between cross-district (strategic) moves and within-district (reactive) moves, these studies found that mobility was considerably less harmful for strategic, cross-district movers than for reactive, within-district movers. This finding is consistent with interview findings from California (Rumberger et al., 1999). Perhaps surprisingly, though, Hanushek et al. (2004) did not find evidence that within-year moves, generally thought to be reactive, are more harmful than between-year moves.

In sum, methodological advances have produced more compelling estimates of the causal effects of switching schools on the mover's academic outcomes, and distinctions in theory between "strategic" and "reactive" movers have helped to illuminate some of the nuances in understanding which mobile students experience the greatest harm. Still, opportunities for advancement remain. Student fixed-effects models, although an improvement, are imperfect, since they cannot capture the influence of time-variant, unobservable, student-level factors. This limitation is important given that so many school transfers arise from major life events. Disentangling the effect of a school switch from the effect of whatever prompted that school switch remains difficult.

The effects of mobility on non-movers' academic outcomes

A great deal of quantitative research has assessed the effects of student mobility on mobile students; surprisingly little has assessed the effects of mobility on mobile students' classmates. Rumberger et al. (1999) plotted average 10th-grade math scores against mobility rates for 51 California high schools and demonstrated that mobility rates are negatively correlated with scores. The authors reported that the relationship persisted even after they controlled for the effects of student socioeconomic status. In the same study, the researchers interviewed teachers about how mobility affects classroom instruction and school-wide climate and logistics, and they found teachers reporting "profoundly negative" impacts on teaching through mobility's effects on teacher morale, lesson presentation and continuity, and group activities within the classroom (Rumberger et al., 1999, p. 57). These same teachers reported negative school-wide effects on resources, climate, logistics, and performance on standardized tests (p. 58).

Hanushek, Kain, and Rivkin (2004) carefully assessed the effects of mobile students on their 4th-7th-grade peers in Texas during the mid-1990s. By exploiting the longitudinal nature of their data, they used year-to-year fluctuations in the turnover within school-grades to identify the likely effect of mobility on students' math scores on the Texas Assessment of Academic Skills (TAAS). They found that a one standard deviation increase in the proportion of new students (approximately eleven percentage points) is associated with a drop of approximately 0.013 standard deviations in average achievement. Although this single-year magnitude is small, they argued, the cumulative effect of several years in a high-mobility school could be substantial. Additionally, they found evidence that the negative consequences are particularly severe for low-income and Black students.

Research Questions

Despite receiving considerable attention from researchers, the topic of student mobility has several questions that remain unanswered. These questions are especially pertinent at a time when families relocate frequently and increased opportunities for school choice are removing barriers to movement between schools. Using administrative data from 2003-04 through 2007-08 in Milwaukee, we examine the following questions:

- How common is student mobility among Milwaukee students and schools, and how does it vary by student and school characteristics?
- What is the effect of moving on the mobile student's academic achievement?
- What is the effect of moving on the mobile student's classmates' academic achievement?

Data & Background

This study used data collected by the Milwaukee Public Schools (MPS) for the school years 2003-04 through 2007-08. We focused on children in grades 1-12. We excluded kindergarteners from these analyses in order to prevent ourselves from counting as mobile children who stayed in their preschools through kindergarten and then switched to an elementary school for 1st grade. Students were identified with unique scrambled IDs, and every student had a separate observation for each school year that he or she appeared in the data (for a maximum of five years). The data included the school in which the child was enrolled in the fall, winter, and spring of that academic year, enabling us to distinguish between students who changed schools within an academic year and students who changed schools between them.

In addition to basic demographic and enrollment information, the dataset contains scores for Wisconsin's state assessment, the Wisconsin Knowledge Concepts Examination (WKCE). Since the 2005-06 school year, the exam has been administered to students in grades 3-8 and 10. We also had WKCE scores for 4th- and 8th-grade students for each year that we observed, as the exam has been used as one criterion for making promotion decisions for students entering the 5th and 9th grades (Wisconsin Department of Public Instruction, 2009). With two exceptions, TerraNova scores were available for the years and grades (3rd-10th) in which the WKCE was not administered.[3] TerraNova assessments, developed by CTB/McGraw-Hill, are standardized exams spanning a wide range of academic subjects. For our purposes, we used TerraNova math and reading scores when WKCE math and reading scores are not available. We believe this is reasonable since both the WKCE and TerraNova were administered to virtually all public school students in our sample (in the relevant grades), and a study of the score compatibility of TerraNova and WKCE scores found the scores to be reasonably comparable (CTB/McGraw-Hill, 2003). With both the WKCE and TerraNova, we standardized scale scores by year, grade, and subject. Importantly, both of these exams have been administered in the fall, meaning that students experienced only a couple of months at their new grade level before being assessed.

Our dataset contains information on students in many types of schools other than just traditional public schools in MPS. Milwaukee is a city that affords plentiful opportunities for parents to choose schools for their children. Families have long been able to choose from: voucher-supported private schools through the Milwaukee Parental Choice Program (MPCP); private schools not participating in MPCP; charter schools classified as MPS instrumentality (with MPS-employed staff), MPS non-instrumentality (with non-MPS-employed staff), or independent; schools in other districts through statewide open enrollment policies and a voluntary racial integration program (Chapter 220); MPS "partnership" schools for students identified as struggling or at-risk of dropping out; and an assortment of traditional public schools available to students across the city. This dataset contains complete information about students enrolled in traditional public schools, instrumentality charter schools, and partnership schools but partial information for the rest. For example, we observed only a subset of private school students and knew only the districts (not the individual schools) that open enrollment students

[3] We do not have test score information for 9th-grade students in the 2006-07 and 2007-08 school years.

attended. For data purposes, this means that all of our calculations of mobility's frequency and effects included only students who were enrolled in a traditional MPS public school, MPS instrumentality charter school, or partnership school. We used the incomplete information for students enrolled elsewhere only to obtain more accurate data on the whereabouts of students who transferred from MPS. Table 1 provides demographic and background information for students in our data who attend a traditional public, instrumentality charter, or partnership school in the fall.

The abundance of choice in Milwaukee could have interesting implications for the incidence and effects of mobility. To the extent that it has made Milwaukee families more active and frequent school shoppers and removed barriers to switching between schools, we might expect to find more strategic movers than we would expect to find in similar urban areas (unless families make careful initial choices and see no need for later moves). Furthermore, if Milwaukee parents are atypically careful to choose schools that they believe suit their children well, we might expect a drop in the reactive mobility that results from bad matches between students and schools. Alternatively, the low barriers to moving may lead to more reactive moving as a result of minor dissatisfactions.

Methods

Quantifying the extent of mobility

Ligon and Paredes (1992) lamented the lack of consistency across districts' and states' mobility calculations. Unfortunately, a clear consensus has not emerged since then, particularly about how to classify movers and non-movers when confronted with unwieldy administrative datasets. Our data enable us to see the school in which a student was enrolled in the fall, winter, and spring of each school year, and we were consistent and conservative in the way that we handled missing data.

We define *within-year mobility* as being enrolled in a traditional MPS public school, MPS instrumentality charter school, or MPS partnership school in the fall of a school year and then leaving that school before the spring of that same school year. "Leaving" could entail switching to another school or dropping out entirely. To calculate the overall within-year mobility rate for our data, we used the number of students who left between the fall and spring of year *t* as our numerator. The denominator was the total number of students enrolled in one of these schools in the fall of year *t*. Thus, students missing a school ID or enrolled in other types of schools (e.g., private and non-instrumentality charter schools) in the fall of year *t* were omitted from these mobility rate calculations altogether.

We define *cross-year mobility* as being enrolled in a traditional MPS public school, MPS instrumentality charter school, or MPS partnership school in the fall of a school year and then leaving that school before the fall of the following year. Note that students who were classified as within-year mobile were also classified as cross-year mobile. Although our rationale for determining the numerator and denominator was virtually identical for within-year and cross-year mobility, this required an additional step. We needed to omit "promotional movers" who could not stay in the same school the following year. To identify these movers, we manually entered the final grade available at each school at the time.[4] Students in promotional grades were omitted from both the numerator and denominator of cross-year mobility calculations. In this case, our numerator was the number of students in non-promotional grades in the fall of year *t*

[4] In general, this is simply the most advanced grade offered at the school in that particular year. However, in rare cases when the school added a grade each year (e.g., offers K-2nd in 2003, K-3rd in 2004, K-4th in 2005), we called a student mobile if he or she was in the most advanced grade but departed before the school reached the final grade that it served.

who departed before the fall of year *t+1*. Our denominator was the total number of students enrolled in non-promotional grades in the fall of year *t*.

We define *between-year mobility* as being in one school in the spring of year *t* and then a different school in the fall of year *t+1* without an accompanying school level promotion (e.g., middle school to high school). Between-year movers were identified consistent with the logic used for within- and cross-year movers.

Mobility was determined at the student level. Since each student has a separate observation for each school year, one could be classified as mobile in one year and non-mobile the next. However, we did not double-count observations. If a student changed schools between the fall and winter and then changed schools again between the winter and spring, we treated the observation as mobile (both within-year and cross-year) but only counted it once. In terms of handling missing school IDs, we were careful to allow for some human error. If a student was missing a school ID for one time period (e.g., the winter of year *t*) but showed up again at her previous school in the following period (e.g., spring of year *t*), we assumed the missing school ID was a data entry error and treated the student as non-mobile. However, if a student's school ID was missing for two consecutive time periods, we treated her as not enrolled in either of those periods. Our guiding rationale was "once is a fluke, twice is a pattern."

In sum:

- within-year movers switched schools between the fall and spring of year *t*;
- cross-year movers switched schools between the fall of year *t* and the fall of year *t+1*; and
- between-year movers switched schools between the spring of year *t* and the fall of year *t+1*.

If anything, we believe that we undercounted the proportion of Milwaukee students who are mobile. For example, by omitting all students who did not have a school ID in the fall term from our within-year and cross-year calculations, we omitted a group that might have been atypically inclined to move into, out of, and between schools.

Estimating the effects of mobility on the mobile student

Credible estimates of the causal effect of a move on a student's test scores are elusive. The simplest, most naïve approach would simply compare the outcomes of mobile students and non-mobile students. The concern here, of course, is that movers differ from non-movers in ways that could be correlated with academic achievement (e.g., family wealth or income). A partial solution to this problem is to control for a set of observable characteristics that might correlate with achievement. We perform these analyses, regressing students' standardized test scores on variables representing mobility as well as some useful observable covariates (e.g., free or reduced lunch status). However, this, too, will be inadequate if there are differences between movers and non-movers that correlate with test scores and are not captured by our data. For example, if movers are, on average, more (or less) motivated to perform well in school and on standardized tests, our previous model with observable control variables will produce biased estimates of the effects of mobility on achievement. Since our Milwaukee data are longitudinal, spanning the 2003-04 through 2007-08 school years, we could utilize a student fixed-effects strategy to address some of the problems presented by unobservable variables. We used the model:

(1) $$\Delta A_{it} = A_{it} - A_{i,t-1} = \beta_0 + \beta_1 M_{it} + X_{it}\beta_2 + \delta_i + \varepsilon_{it}$$

where A_{it} represents the achievement (standardized test score) of student i in year t, M_{it} represents dummy variables for whether the student was mobile (within a year or between years),

X_{it} represents a vector of time-varying control variables, δ_i represents student fixed-effects, and ε_{it} represents random error. Our time-varying controls included the student's grade level and characteristics of her school in the fall of year *t*. We clustered standard errors at the grade-by-school-by-year level to address the possibility that families within schools are apt to think and act together when deciding whether and where to move their children.

In these analyses, we focused on within-year and between-year mobility. Between-year mobility differs from cross-year mobility in that it only includes students who transfer between the spring of year *t* and the fall of year *t+1*. This way, we could estimate the effects of moving during a school year and after a school year, separately.

The fact that the WKCE and TerraNova were administered in the fall has important consequences for our analyses. For students mobile within a school year – i.e. those switching schools or dropping out between the fall of year *t* and the spring of year *t* – we used the student's test scores in the fall of year *t+1* to gauge the effect of mobility. In other words, we compared the student's gains from year *t* to year *t+1* to her gains in prior years to estimate the impact of the move. For students mobile between years – i.e. those switching schools between the spring of year *t* and the fall of year *t+1* – there was greater ambiguity about which score to use. Since these tests are administered so early in the year, we should expect the score from the fall of year *t+1* to provide good estimates of the immediate effect of the move on the child's achievement (e.g., psychological disruptions that affect test-taking). Scores from year *t+2*, however, provide better estimates of the long-term effects of the move, including those related to differences in school quality. Given that changes from both year *t* to year *t+1* and year *t* to year *t+2* are of interest to schools and policymakers, we included them both. There is some evidence that academic losses immediately following school changes are partially offset by recovery in subsequent years (Kerbow, 1996).

The strength of the student fixed-effects model is that it enables us to control for time-invariant characteristics of students that might otherwise bias our estimates of the causal effect of mobility on achievement. One of its key limitations, though, is its inability to control for time-variant characteristics of students that are correlated with both mobility and achievement. In this context, this is an important limitation. For many students undergoing a school change, it is quite possible that something dramatic happened prior to the school change that influenced their decisions to switch schools and their academic performance. This could include events at home, like residential relocation or a change in family structure, or events at school, like a conflict with classmates or school staff. Failure to account for these time-variant factors would keep us from disentangling the effects of a move from the effects of whatever prompted the move and would make our findings from equation (1) biased.

Confronted with this methodological challenge, we looked to our data for a possible instrumental variable for student mobility. An instrument that is correlated with whether a student moves to a different school and uncorrelated with the error term in Equation (1) could produce an unbiased estimate of the effect of mobility on achievement. For this, we turned to the unconventional grade structures of some Milwaukee schools during this period. Most Milwaukee schools end in grades 5, 8, or 12. However, four schools during this period ended in grade 6 without plans to introduce a 7th grade in the following year. With no schools beginning in 7th grade during this period, these departing 6th graders had to enroll in a school in which the majority of the students had already been together for at least one year. These movers, in other words, become the "new kids" in their schools and likely shared many of the experiences of cross-year movers who leave for “strategic” or “reactive” reasons. They stepped into an unfamiliar environment in which they were known neither by their teachers nor their peers, though these teachers and students were well acquainted with one another. Importantly, their movement was much less likely to have been triggered by a sudden, powerful event in their lives that could have influenced their later achievement. Thus, they may provide an opportunity to obtain a truer estimate of the effect of the move itself on achievement, apart from the effects of whatever prompted that move. We use a dummy variable that represents whether one’s school ends in 6th grade (interacted with being a 6th grader) as an instrument for between-year mobility.

Instrumental variables must satisfy several assumptions in order to provide plausibly causal estimates of the effect of a treatment on an outcome of interest. Among these assumptions, they must have a non-zero affect on the independent variable of interest (in our case, cross-year mobility) and they must affect the dependent variable (test score change) only through that independent variable. Consider the first criterion (instrument relevance). In this case, the relevance of the instrument is unambiguous. Sixth-grade students in schools that do not have a 7th grade in the following year cannot progress to 7th grade and remain in the same school. Clearly, this instrument – being in a school that has a 6th grade but no 7th grade – affects the likelihood that a 6th grader changes schools the following year. The second criterion requires more careful examination. The question here is whether attending a school that ends in 6th grade should affect one’s trajectory of test scores via a mechanism other than between-year mobility.

Since we are comparing students’ gains following the move to their gains prior to it, our chief concern is unobservable differences that affect test score *trajectories* between those in and not in schools that end in 6th grade. For example, if we believe that children attending elementary schools through 6th grade are particularly capable of handling school transitions smoothly (i.e. doing well on standardized tests after the move), then our instrument might be problematic. In order for this to be so, however, these students must handle these school transitions atypically well after controlling for prior achievement. This seems unlikely to us, since parents would have to accurately assess their children's relative ability to handle transitions – and act on those assessments – after only observing their children at very young ages. Another possible objection is that schools ending in 6th grade might better prepare 6th graders for a transition (than, say, schools ending in 8th grade would prepare their departing 6th graders for a transition). If this phenomenon exists, it would lead us to underestimate the initial negative effects of mobility.

Estimating the effects of mobility on the mobile student's classmates

Thus far, our discussion of effects has focused on the impact of mobility on mobile students. However, we are also interested in the effect of school-switching on non-mobile students, as understanding these externalities is essential to grasping the full consequences of student mobility. In order to estimate these effects, we took a similar approach to the one described for estimating the effects on the mobile. We created a measure of the degree of classmate (or, more accurately, "grade-mate") turnover experienced by non-mobile students. We divided the number of students who newly entered a grade between the fall and spring of a given year by the number of students who were enrolled in the fall. We believe that this is an appropriate measure of turnover, as we would generally expect newly enrolling students to be more disruptive than departing students.

First, we modeled the extent to which students' test performance differed when they were in grades (in a particular school and year) in which the turnover was higher. Although we controlled for various student and school characteristics and included grade and year fixed-effects, we initially did not include school fixed-effects. We clustered the standard errors at the grade-by-school-by-year level to account for the fact that mobility, the variable of interest, was measured at this level and students were nested within these groups.

The bias concern in estimating the causal effect of mobility in the simple model described above is that some schools and grades might experience higher mobility because they are "worse" than other schools and grades. To at least partially address this endogeneity and omitted variables bias, we added school indicators in addition to the indicators for each grade and year. Equation 2 describes this model.

$$\Delta A_{igst} = \beta_0 + \beta_1 M_{gst} + X_{\mathrm{igst}}\beta_s + \delta_{\mathrm{t}} + \gamma_{\mathrm{gs}} + \varepsilon_{\mathrm{igst}} \quad (2)$$

A non-mobile student's change in math achievement, ΔA_{igst}, is a function of the turnover in her grade in year *t*, M_{gst}, her characteristics in year *t*, as well as time and school-by-grade fixed-effects. (Prior to this model we ran an identical model but with school fixed-effects and grade fixed-effects separately rather than school-by-grade fixed-effects.) The identification of the mobility effect in this model comes from atypical mobility a given grade in a given school has from one year to the next.

Findings

The extent of mobility in Milwaukee

We began our analyses by describing mobility in Milwaukee's schools. We found a city in which students move frequently from one school to the next, with disadvantaged students – and the schools that disproportionately serve them – experiencing mobility most intensely.

Across all years, grades, and schools, 10.7% of the students enrolled in an MPS traditional public, instrumentality charter, or partnership school in the fall switched schools or left the system before the end of the school year. These are students who moved between the fall of year *t* and the spring of year *t*. As indicated in Table 2, within-year mobility rates were particularly high for high school students, Black students, and those eligible for free lunch (our proxy for low income). For the students whom we observed a change in schools between the fall and spring, we observed a change in home address for 30.5% of them, no change in home address for 35.7%, and missing or inconclusive data for 33.8%.

The between-year mobility rate was stunningly high, as well. We found that 22.0% of students enrolled in an MPS traditional public, instrumentality charter, or partnership school in the spring of year *t* departed that school before the fall of the following school year (excluding promotional moves). Like the within-year movers, this group consisted disproportionately of low-income and Black students. Of the full sample, 22.8% of free lunch-eligible and 25.9% of Black students changed schools or left the system between the spring of year *t* and the fall of year *t+1*.

The higher mobility rates for low-income and Black students suggest that schools serving disproportionately large numbers of these students should experience particularly high turnover. This was generally the case. Table 3 provides the probability that a student changed schools within and across years depending on the characteristics of that child's school.[5] For example, for a child enrolled in a school with a student body that was over 86.4% Black (as was the case for approximately one-fourth of MPS students), there was a 35.7% chance that the child switched schools or left the system between the fall of year *t* and the fall of year *t+1*.

In order to obtain a fuller understanding of which Milwaukee students were mobile, we ran logistic regressions of within-year mobility and between-year mobility on an assortment of

[5] Note that we constructed the quartiles in Table 3 such that approximately one-fourth of the student observations fit into each quarter, not necessarily one-fourth of the schools.

student characteristics. Table 4 shows a persistent relationship between one's fall math score and whether the child changes schools that year, with low scorers particularly likely to switch. Not surprisingly, those whose home addresses changed between the fall and spring were much more likely to switch schools than those whose addresses remained the same. Black students also were significantly more likely than White students to switch schools controlling for the other variables in the model. Table 5 replicates Table 4 but for between-year movers (spring to fall). Here, too, we see that low-scoring students (on the fall math assessment), those undergoing residential relocation, and Black students were particularly likely to switch schools. Free or reduced lunch eligibility was associated with a higher probability of switching schools, given the other variables in the model, while ELL status was negatively associated with school transfers.

With all of this movement around Milwaukee schools, where did students go? If we believe that most of the mobility is strategic, we would expect students to move to "better" schools (or at least schools better equipped to meet their needs). On the other hand, if we believe that most of the mobility is reactive, it is difficult to predict whether movement generally will be toward stronger or weaker schools. Looking to the data, we saw a divide (see Tables 6-9). Within-year movers, who switched schools between the fall of year t and spring of year t, tended to move to schools with lower mean test scores than their previous schools. Between-year movers, who switched schools between the spring of year t and fall of year $t+1$, tended to move to schools with higher mean test scores than their previous schools. This provides mild support for the suggestion in the mobility literature that cross-year movers are generally more strategic than within-year movers.

In examining the extent of mobility in Milwaukee, we found a highly transient population. Although mobility rates were high for all students both within years and across years, they were particularly high for low-income, low-scoring, and Black students and for the schools that serve high proportions of these students. Finally, whereas between-year movers tended to transfer to schools with higher mean test scores, within-year movers tended to transfer to schools with lower scores.

The effects of mobility on the mobile student

Our second set of analyses related to the effects of changing schools on mobile students.[6] Here, we worked through an assortment of methods in order to isolate, as best we could, the causal effect of mobility upon achievement. We saw evidence that past studies that did not use longitudinal data might have overstated the negative consequences of switching schools. In fact, although it seems that there is an immediate cost to a child's learning associated with switching schools, there might be longer-term benefits to being in a better schooling situation.

In Table 10, we show estimates from regressing math scores in year $t+1$ on whether the child was mobile during or after year t and an assortment of control variables that do not include the student's prior test scores. The most naïve estimates of all, from specifications (1) and (3) (for within-year and between-year mobility, respectively), suggest enormously negative effects of moving on test scores. These effects, of course, are overstated, since movers tend to be disadvantaged relative to non-movers. Specifications (2) and (4) introduce student- and school-level control variables but do not contain controls for prior scores. They reduce the estimated effects of mobility substantially but still suggest a powerful negative impact upon the mover. Importantly, though, these specifications control for only a crude set of observable characteristics. Without including prior test scores, our ability to make causal inferences is limited.

[6] We focus on math from this point forward for simplicity (and because it has been argued to be more school-dependent than reading), but we intend to return to mobility's effects on reading scores later.

Table 11 shows that incorporating prior scores sharply changes the interpretation. Here, the dependent variable is the change in standardized test scores between years *t* and *t+1*. The variable of interest is “mobile within year” for specifications (1) and (2) and “mobile between years” for specifications (3) and (4). Contrary to the findings from Table 10, we now see no negative effects of a within-year move on a child’s gains in the following year. If these estimates are unbiased, then children switched schools and perhaps had ample time to get comfortable in their new settings before the next autumn’s assessments. They left one school between the fall and spring of year *t* and adjusted quickly enough that their gains between years *t* and *t+1* were not compromised by the move.

The same cannot be said of between-year movers in these models. These are children who moved between the spring of year *t* and the fall of year *t+1*. These children, who did not have as much time to adjust to their new schools (nor did their new schools have as much time to adjust to them), saw lower gains between years *t* and *t+1* than usual. With both a model that includes an assortment of student- and school-level controls and another model that contains student fixed-effects and a control for one’s initial school’s achievement, we observed that switching schools between years is associated with a drop of about 0.05 standard deviations in math test score gains.

It is plausible, however, that this was a temporary drop in scores, since the test was administered shortly after these students arrived in their new schools. Thus, we compared the effects of a move (between years *t* and *t+1*) on these students’ gains between years *t* and *t+1* to their gains between years *t* and *t+2*. Table 12 shows gains in standardized math scores between years *t* and *t+1* and years *t* and *t+2* for students with test scores available in years *t*, *t+1*, and *t+2*. We found that after seeing their scores drop in the year after the move, these students recovered their losses by the following year. In fact, using our preferred student fixed-effects model (Table 12, Specification 4), we found weak evidence that students not only recovered all of their losses in the subsequent year, but their cumulative gains were actually better than we might have expected without the move.

We believe that the estimates provided in Tables 11 and 12 provide reasonably good approximations of the effect of mobility on math gains, particularly in the student fixed-effects models. However, methodological concerns remain. For example, even a student fixed-effects approach cannot pick up on time-variant, unobservable events that might affect a child's scores. Perhaps a powerful life event, like residential relocation or divorce, occurred just prior to the move and made the move's effect look misleadingly negative. Alternatively, maybe escaping a terrible situation had such strong positive effects that it made the move's effect look misleadingly positive. This is where using an instrument for switching schools can be valuable. We used the interaction between being in 6th grade and attending a school that ends in 6th grade as an instrument for switching schools. Doing so, we found significantly negative effects (one-tenth of a standard deviation) of moving between the spring of year t and fall of year $t+1$ on math gains between t and $t+1$ (see Table 13, Model 1). When we compared changes from years t to $t+1$ to those from years t to $t+2$, we saw a familiar pattern: an immediate loss to math score gains that students recovered in the subsequent year.

Our core assumptions related to this instrumental variable were that being a 6th grader in a school that ends in 6th grade made one more likely to change schools the next year (self-evidently true; F-statistic > 3000) and that being a 6th-grade student in one of these schools only affected test score gains through the school change itself (argued above). We caution against comparing the effects found using the instrument to the effects found in our other longitudinal analyses, as the instrument draws its estimates only from 6th-grade students.[7]

Taken together, these findings tell a coherent story of the effects of mobility on transient Milwaukee students. There does, in fact, seem to be an immediate negative effect to moving, as students' math score gains slip, sometimes sharply, when students change schools in the summer before the fall assessment. However, in the longer term, these students seem to recover. This is true for between-year movers, who overcome their immediate losses in the subsequent year, and for within-year movers, who seem to have enough time to adjust to their new settings that they do not experience a loss in gains by the following fall. The findings potentially reconcile the different theories that predict the effects of mobility (the benefits suggested by Tiebout-influenced economists and the costs suggested by educational scholars, psychologists, and sociologists). There is a real short-term cost to switching schools in the time that it takes a student to get to know a school and a school to get to know a student. However, that cost subsides, perhaps because the student ultimately finds himself or herself sorted into a school that better meets the student's needs.

The effects of mobility on the mobile student's classmates

When a student transfers to a new school, the change not only affects that child but also the child's past and present classmates. Although we might expect these effects to be less powerful than the direct effects on the mobile student, we cannot fully appreciate mobility's impact without assessing these indirect effects.

After calculating the degree of turnover for every grade in every school in every year (discussed above), we examined how differences in these turnover rates relate to math score

[7] We did not find clear evidence that 6th-grade transfers are more damaging than other transfers. Point estimates from our student fixed-effects models of the relationship between mobility and math gains were slightly more negative for 6th-grade students, but the interaction between being a 6th grader and being mobile was not significant.

gains for non-mobile students during that same period. We used three separate models. The first utilized grade and year fixed-effects and a series of school and student control variables. The second used school, grade, and year fixed-effects and a series of student-level control variables. The third used *school-by-grade* and year fixed-effects, and the same series of student-level control variables. The third, which appears as specification (3) in Table 14, is our preferred model, as it identifies effects from atypical mobility from one year to the next within a given grade and school.

One might expect higher levels of turnover to compromise stable students' learning, since these new students may disrupt classroom procedures and culture and draw disproportionately on teachers' time and school resources. In fact, we find modestly – but statistically significantly – negative effects. Specification (3) suggests that a one percentage point increase in the defined turnover rate is associated with a reduction in math gains of approximately 0.0023 standard deviations between that fall and the following fall.

Conclusion

The consequences of student mobility make for difficult terrain for researchers, both theoretically and methodologically. Theoretically, mobility might seem like a positive force to an economist who believes that actively mobile families sort themselves optimally. On the other hand, psychologists and sociologists might worry about the disruptive effects for both mobile and non-mobile students. Methodologically, the effects of mobility on both mobile students and their classmates are notoriously difficult to measure, as student mobility comes wrapped in a complicated set of social phenomena that create myriad possibilities for bias.

For this study, we described mobility in the Milwaukee Public Schools and estimated its effects on mobile and non-mobile students. We found remarkably high rates of mobility, particularly for low-income and Black students and the schools that disproportionately served them. Our sample consisted of students in grades 1-12 in Milwaukee's traditional public, instrumentality charter, and partnership schools during the 2003-2004 through 2007-2008 school years. Of the students enrolled in the fall of one of these years, 10.7% switched schools or left the system by the spring of that year and 30.3% transferred or left by the fall of the following year. There were patterns amid this constant shuffling. For example, students who transferred between academic years were more likely to choose schools with higher average test scores (than their previous schools) than students who transferred during school years. This supports the argument from the student mobility literature that between-year moves are generally more "strategic" while between-year moves are more "reactive."

Maybe surprising given this strategic-reactive distinction, but consistent with other recent research (Hanushek, Kain, and Rivkin, 2004), we found that within-year moves were no more harmful to mobile students than between-year moves. In fact, our preferred models found no evidence that switching schools between the fall and spring of a school year (within-year mobility) compromised mobile students' math gains through the following fall. Perhaps these movers were escaping such academically unproductive situations that the benefits from a fresh classroom environment negated the disruptive costs of the move. On the other hand, we saw immediate negative effects for between-year movers (those who moved between the end of one academic year and the beginning of the next). Our student fixed-effects models suggested that switching schools between years was associated with a loss of approximately 0.05 standard deviations in math score gains when students were assessed in the fall following the move (and even sharper losses for 6th-grade movers identified in our instrumental variables analysis). However, these mobile students tended to make up for these losses by the time they were tested again in the following fall.

The unusual timing of Wisconsin's (autumn) testing program during these years might provide important insight here. In most prior studies that noted differences between within-year and between-year movers, within-year movers were tested more immediately after their moves, since state assessments tend to be administered in the spring. Here in Milwaukee, though, within-year movers had more time than between-year movers to acclimate to their new schools before their first round of testing in their new schools. Perhaps as a result, their outcomes were less negative than one might expect given the proposed "reactive"-"strategic" distinction between within-year and between-year movers.

Taken together, these findings provide a coherent explanation for the effects of mobility that helps to reconcile the theoretical conflict discussed above. There appears to be an immediate cost in math test gains to switching schools, but these losses can be overcome in the longer term as the student grows more familiar with the new school. In this sense, the distinction between "strategic" and "reactive" mobility could be less helpful than one between short-term and long-term effects. Whereas those concerned about the immediate disruptions associated with moving to a new school seem to be rightfully concerned, those who argue that a more optimal sorting should be beneficial seem to be rightfully encouraged. The costs of disruption seem most acute in the short term; the benefits of sorting seem to develop more gradually.

Mobility touches not just the mobile student but also that student's new (and former) classmates. A new student introduces demands on a teacher's time and uncertainty to classroom social dynamics. When we examined the effects of a grade within a school having an unusually large number of students transfer in, we found evidence that the non-mobile students in that grade are negatively affected. Although the effects we find are reasonably small, the fact that we have grade-level measures rather than class-level measures could mean that we are underestimating the true effect. Furthermore, as Hanushek, Kain, and Rivkin (2004) argue, students exposed to highly mobile classmates year after year might be only mildly affected in any given year, but the aggregate effects might not be nearly so mild.

In “Life of Theseus,” the Greek philosopher Plutarch described an Athenian ship that saw its planks replaced, one-by-one, until not a single plank remained from its original construction. To Plutarch, this represented a fundamental question about identity. To one concerned about today’s urban schooling, it offers a metaphor for the constant shuffling of students in and out of schools. A student with enough stability – and perhaps good fortune – to remain in one school from its youngest grade to its oldest grade will observe a gradual exchange of planks such that the ship that arrives at graduation might be entirely different from the one that departed. As we have seen from Milwaukee, however, this constant reshuffling of students may not be entirely bad. Mobility’s ultimate effects on movers and non-movers alike may depend on whether the immediate costs in disruption or the potential long-term benefits of better sorting prove more powerful. Schools and policymakers that successfully ease the transitions of students into new schools and protect non-mobile students from any related disruptions – while at the same time preserving opportunities for families to move their children from bad schooling situations to better ones – might be able to minimize these costs and maximize the benefits.

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Table 1

Enrollment in Milwaukee Public Schools from 2003-2007 (grades 1-12), by race, free/reduced lunch eligibility, and ELL status

Year	Enrollment	Race / ethnicity (%)				Free/reduced lunch eligible	ELL
		Black	Hispanic	White	Other	(%)	(%)
2003-04	80,725	60.5	17.5	14.5	7.6	77.2	8.7
2004-05	79,839	60.3	18.5	13.7	7.5	77.8	8.5
2005-06	78,235	60.1	19.2	13.0	7.7	75.2	7.5
2006-07	75,471	59.6	20.1	12.5	7.8	86.2	7.8
2007-08	72,081	59.3	20.6	12.0	8.0	82.0	8.6
Total	386,351	60.0	19.1	13.2	7.7	79.6	8.2

Note: Enrollment figures describe student observations (one per year; up to five per student) for which the student's fall school is identified in the data and the student attends an MPS public, instrumentality charter, or partnership school.

Table 2

Probability of switching schools at least once within, between, and across a given year, by student-level characteristics

	Percent mobile within year (fall of year *t* through spring of year *t*)	Percent mobile between years (spring of year *t* through fall of year *t+1*)	Percent mobile across years (fall of year *t* through fall of year *t+1*)
All observations	10.7%	22.0%	30.3%
By grade level			
1st grade	7.4%	19.9%	25.8%
2nd grade	7.3%	21.1%	26.7%
3rd grade	7.1%	19.2%	24.9%
4th grade	7.5%	18.8%	24.8%
5th grade	7.1%	32.5%	38.6%
6th grade	8.4%	23.0%	29.5%
7th grade	9.6%	22.0%	29.5%
8th grade	9.8%	39.9%	63.6%
9th grade	17.5%	24.8%	37.9%
10th grade	14.4%	21.0%	32.3%
11th grade	13.0%	19.2%	29.5%
12th grade	8.4%	N/A	N/A
By race/ethnicity			
Black	12.1%	25.9%	35.3%
Hispanic	7.5%	16.2%	23.0%
White	5.8%	15.8%	21.1%
Other	7.0%	18.3%	24.5%
By sex			
Female	9.1%	21.6%	29.2%
Male	10.8%	22.4%	31.3%
By free/reduced lunch eligibility			
Eligible	10.2%	22.8%	31.1%
Ineligible	9.1%	18.7%	26.7%
By ELL status			
ELL	6.1%	15.6%	21.2%
non-ELL	10.3%	22.6%	31.1%

Notes: The appropriate interpretation is, "Of the students who were enrolled in a traditional public, instrumentality charter, or partnership school in the __ of year __, *x*% of them left that school by the __ of year __." Sample consists of all observations for students enrolled in the Milwaukee Public Schools in the fall of a given year between 2003-04 and 2007-08. See "Data & Background" and "Methods" sections for details about how students were classified.

Table 3

Probability of switching schools at least once within, between, and across a given year, by school-level characteristics

	Percent mobile within year (fall of year *t* through spring of year *t*)	Percent mobile between years (spring of year *t* through fall of year *t+1*)	Percent mobile across years (fall of year *t* through fall of year *t+1*)
By school type			
Elementary	7.7%	21.6%	27.1%
Middle	11.3%	27.1%	36.6%
High	14.0%	21.7%	32.4%
Combined Elementary/Secondary	11.2%	26.5%	33.2%
By percentage of students eligible for free/reduced lunch			
Quartile 1 (< 67% of school)	11.5%	19.2%	25.6%
Quartile 2 (67% - 79.5%)	11.7%	24.2%	32.6%
Quartile 3 (79.5% - 88.2%)	10.3%	25.7%	33.0%
Quartile 4 (> 88.2% of school)	9.2%	23.6%	30.2%
By percentage of students Black			
Quartile 1 (< 27% of school)	7.2%	16.7%	22.2%
Quartile 2 (27% - 64.5%)	9.3%	18.4%	24.9%
Quartile 3 (64.5% - 86.4%)	12.2%	27.6%	35.4%
Quartile 4 (> 86.4% of school)	14.1%	30.3%	38.8%
By percentage of students Black or Hispanic			
Quartile 1 (< 68.3% of school)	7.6%	17.1%	22.5%
Quartile 2 (68.3% - 82.5%)	10.1%	21.1%	28.5%
Quartile 3 (82.5% - 90.6%)	12.8%	27.1%	35.4%
Quartile 4 (> 90.6% of school)	12.6%	28.1%	35.7%
By percentage of students ELL			
Quartile 1 (< 0.38% of school)	12.9%	27.0%	34.9%
Quartile 2 (0.37% - 1.52%)	9.7%	21.9%	28.6%
Quartile 3 (1.52% - 7.2%)	10.8%	23.7%	30.6%
Quartile 4 (> 7.2% of school)	9.1%	19.5%	26.6%

Notes: The appropriate interpretation is, "Of the students who were enrolled in a traditional public, instrumentality charter, or partnership school of this type in the __ of year __, *x*% of them left that school by the __ of year __." Sample consists of all observations for students enrolled in the Milwaukee Public Schools in the fall of a given year between 2003-04 and 2007-08. See "Data & Background" and "Methods" sections for details about how students were classified. Schools only were included if they contained at least 50 observations (students) in that particular year. Each quartile contains a roughly equal number of student observations, not necessarily a roughly equal number of schools.

Table 4

Odds ratios for logistic regressions of within-year mobility on various student characteristics

Dependent variable: Within-year mobility

Specification	(1)	(2)	(3)
Eligible for free/reduced lunch	1.169***	1.496***	1.683***
	[0.034]	[0.107]	[0.170]
ELL	0.730***	0.856	0.766~
	[0.030]	[0.099]	[0.124]
Female	0.814***	0.986	1.037
	[0.011]	[0.044]	[0.060]
Black	2.237***	1.187*	1.537**
	[0.077]	[0.104]	[0.193]
Hispanic	1.457***	1.014	1.142
	[0.061]	[0.099]	[0.170]
Other race/ethnicity	1.271***	1.134	1.388*
	[0.055]	[0.122]	[0.201]
Standardized math score (fall of year *t*)		0.790***	0.769***
		[0.021]	[0.025]
Standardized math gain (fall *t-1* to fall *t*)		0.957	0.932
		[0.035]	[0.041]
New address (fall *t* to spring *t*)			3.192***
			[0.207]
Observations	425,304	114,480	113,618

Notes: Standard errors appear in brackets and are clustered by school, grade, and year. Only students enrolled in MPS public, instrumentality charter, and partnership schools are included (enrolled in the fall of year *t* for within-year mobility). Students whose race/ethnicity is listed as Caucasian are omitted.

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.10

Table 5

Odds ratios for logistic regressions of between-year mobility on various student characteristics

Dependent variable: Between-year mobility

Specification	(1)	(2)	(3)
Eligible for free/reduced lunch	1.028	1.234***	1.212***
	[0.046]	[0.049]	[0.065]
ELL	0.877**	0.801**	0.764*
	[0.038]	[0.063]	[0.083]
Female	0.960***	0.971	0.977
	[0.011]	[0.018]	[0.023]
Black	1.913***	1.409***	1.703***
	[0.081]	[0.083]	[0.143]
Hispanic	1.111*	0.922	1.042
	[0.049]	[0.056]	[0.090]
Other race/ethnicity	1.194***	1.025	1.231*
	[0.054]	[0.064]	[0.102]
Standardized math score (fall of year *t*)		0.804***	0.797***
		[0.014]	[0.017]
Standardized math gain (fall *t-1* to fall *t*)		1.060***	1.058**
		[0.017]	[0.021]
New address (fall *t* to spring *t*)			2.754***
			[0.130]
Observations	339,612	87,207	80,442

Notes: Standard errors appear in brackets and are clustered by school, grade, and year. Only students enrolled in MPS public, instrumentality charter, and partnership schools are included (enrolled in the spring of year *t* for between-year mobility). Students whose race/ethnicity is listed as Caucasian are omitted.

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.10

Table 6

Percentage of students mobile within a year (fall of year t to spring of year t) and between years (spring of year t to fall of year t+1) who moved to schools with higher (or lower) mean standardized math scores

	n	Transferred to school with higher mean math score	Transferred to school with lower mean math score
Students mobile within years	24,805	47.4	52.6
Students mobile between years	31,218	54.7	45.3

Note: Mobile students appear in this table only if we observe their pre- and post-move schools' mean assessment scores.

Table 7

Percentage of students mobile within a year (fall of year t to spring of year t) and between years (spring of year t to fall of year t+1) who moved to schools with higher (or lower) mean standardized reading scores

	n	Transferred to school with higher mean reading score	Transferred to school with lower mean reading score
Students mobile within years	25,084	48.2%	51.8%
Students mobile between years	31,106	51.5%	48.5%

Note: Mobile students appear in this table only if we observe their pre- and post-move schools' mean assessment scores.

Table 8

Of those who transferred to schools with much higher or much lower average math scores than their previous schools (at least 0.25 standard deviations different), the percentage of those mobile who transferred to much higher-scoring vs. much lower-scoring schools

	N	Transferred to school with much higher mean math score	Transferred to school with much lower mean math score
Students mobile within years	14,131	46.9%	53.1%
Students mobile between years	16,929	55.1%	44.9%

Note: Mobile students appear in this table only if we observe their pre- and post-move schools' mean assessment scores.

Table 9

Of those who transferred to schools with much higher or much lower average reading scores than their previous schools (at least 0.25 standard deviations different), the percentage of those mobile who transferred to much higher-scoring vs. much lower-scoring schools

	N	Transferred to school with much higher mean reading score	Transferred to school with much lower mean reading score
Students mobile within years	12,590	47.6%	52.4%
Students mobile between years	14,748	52.9%	47.1%

Note: Mobile students appear in this table only if we observe their pre- and post-move schools' mean assessment scores.

Table 10

Naively estimated effects of within-year mobility and between-year mobility on mobile student's math score in the following year, using various controls (but not prior scores)

Dependent variable: Standardized math score in year *t+1*

	Within-year mobility (fall of year *t* through spring of year *t*)		Between-year mobility (spring of year *t* through fall of year *t+1*)	
	Without any controls	Controlling for student and school characteristics (with grade fixed-effects)	Without any controls	Controlling for student and school characteristics (with grade fixed-effects)
Specification	(1)	(2)	(3)	(4)
Mobile within year (fall *t* to spring *t*)	-0.408***	-0.197***		
	[0.030]	[0.028]		
Mobile between years (spring *t* to fall *t+1*)			-0.294***	-0.083***
			[0.015]	[0.010]
Female		0.006		0.005
		[0.005]		[0.006]
Eligible for free or reduced lunch		-0.236***		-0.221***
		[0.009]		[0.009]
ELL		-0.221***		-0.198***
		[0.015]		[0.016]
Black		-0.476***		-0.470***
		[0.013]		[0.013]
Hispanic		-0.188***		-0.201***
		[0.012]		[0.013]
Other race/ethnicity		-0.079***		-0.093***
		[0.014]		[0.014]
School mean math score (fall/spring *t*)		0.666***		0.677***
		[0.016]		[0.015]
Observations	169,228	168,609	155,593	154,714

Notes: Standard errors appear in brackets and are clustered by school, grade, and year. Only students enrolled in MPS public, instrumentality charter, and partnership schools are included (enrolled in the fall of year *t* for within-year mobility and the spring of year *t* for between-year mobility). Students whose race/ethnicity is listed as Caucasian are omitted.

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.10

Table 11

Estimated effects of within-year mobility and between-year mobility on mobile student's math score gains from years t to t+1, with and without student fixed-effects

Dependent variables: Gain in standardized math scores between year *t* and year *t+1*

	Within-year mobility (fall of year *t* through spring of year *t*)		Between-year mobility (spring of year *t* through fall of year *t+1*)	
	Grade fixed-effects but no student fixed-effects	Grade fixed-effects and student fixed-effects	Grade fixed-effects but no student fixed-effects	Grade fixed-effects and student fixed-effects
Specification	(1)	(2)	(3)	(4)
Mobile within year (fall *t* to spring *t*)	0.039	0.068		
	[0.034]	[0.055]		
Mobile between years (spring *t* to fall *t+1*)			-0.053***	-0.055***
			[0.011]	[0.011]
Female	0.015**		0.016***	
	[0.004]		[0.004]	
Eligible for free or reduced lunch	-0.043***		-0.033***	
	[0.007]		[0.007]	
ELL	0.004		0.010	
	[0.013]		[0.013]	
Black	-0.047***		-0.034**	
	[0.011]		[0.011]	
Hispanic	0.003		0.001	
	[0.010]		[0.010]	
Other race/ethnicity	0.001		0.002	
	[0.010]		[0.010]	
School mean math score (fall/spring *t*)	-0.101***	-0.294***	-0.078***	-0.258***
	[0.016]	[0.013]	[0.015]	[0.016]
Observations	134,773	134,773	117,912	117,912

Notes: Standard errors appear in brackets and are clustered by school, grade, and year. Only students enrolled in MPS public, instrumentality charter, and partnership schools are included (enrolled in the fall of year *t* for within-year mobility and the spring of year *t* for between-year mobility). Students whose race/ethnicity is listed as Caucasian are omitted.

*** p<0.001, ** p<0.01, * p<0.05, ~ p<0.10

Table 12

Estimated effects of between-year mobility on mobile student's math score gains from years t to t+1 and t to t+2, with and without student fixed-effects

*Note that the dependent variables differ. In models (1) and (2), the dependent variable is test score gain between years *t* and *t+1*; in models (3) and (4), the dependent variable is test score gain between years *t* and *t+2*.

	Gain in standardized math scores between year *t* and year *t+1* for those with a math score present in years t, t+1, and t+2		Gain in standardized math scores between year *t* and year *t+2* for those with a math score present in years t, t+1, and t+2	
	Grade fixed-effects but no student fixed-effects	Grade fixed-effects and student fixed-effects	Grade fixed-effects but no student fixed-effects	Grade fixed-effects and student fixed-effects
Specification	(1)	(2)	(3)	(4)
Mobile between years (spring *t* to fall *t+1*)	-0.050*	-0.086**	-0.048*	0.045~
	[0.020]	[0.028]	[0.021]	[0.025]
Female	0.004		0.027**	
	[0.009]		[0.009]	
Eligible for free or reduced lunch	-0.023*		0.004	
	[0.011]		[0.014]	
ELL	0.046*		0.001	
	[0.020]		[0.032]	
Black	-0.021		0.013	
	[0.016]		[0.020]	
Hispanic	-0.008		0.079**	
	[0.017]		[0.023]	
Other race/ethnicity	0.019		0.031	
	[0.017]		[0.023]	
School mean math score (spring *t*)	-0.034	-0.355***	0.125***	0.332***
	[0.022]	[0.056]	[0.030]	[0.052]
Observations	28,466	28,466	28,466	28,466

Notes: Standard errors appear in brackets and are clustered by school, grade, and year. Only students enrolled in MPS public, instrumentality charter, and partnership schools in the spring of year *t* are included. Students whose race/ethnicity is listed as Caucasian are omitted.

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, ~ $p<0.10$

Table 13

Estimated effect of switching schools between years t and t+1 on mobile student's math score gains from t to t+1 and t to t+2, using attendance in a school that ends in 6th grade (while a 6th grader) as an instrument

*Note that the dependent variables differ. In model (1) and model (2), the dependent variable is test score gain between years *t* and *t+1*; in model (3), the dependent variable is test score gain between years *t* and *t+2*.

	Gain in standardized math scores between year *t* and year *t+1*	Gain in standardized math scores between year *t* and year *t+1* Only includes students in specification (3) for comparison	Gain in standardized math scores between year *t* and year *t+2*
	Using 6th grader in end-in-6th school as an instrument; includes student and grade fixed-effects		
Specification	(1)	(2)	(3)
Switched schools between spring of year *t* and fall of year *t+1*	-0.100*	-0.124~	-0.023
	[0.049]	[0.071]	[0.069]
Observations	151,582	40,255	40,255

Notes: Standard errors appear in brackets. Only students enrolled in MPS public, instrumentality charter, and partnership schools in the spring of year *t* are included.

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, ~ $p<0.10$

Table 14

Estimated effects of grade-level turnover (new student) rates on non-mobile students' math gains from year t to year t+1

Dependent variable: Gain in standardized math scores between year *t* and year *t+1*

	With grade and year fixed-effects	With school, grade, and year fixed-effects	With school-by-grade and year fixed-effects
Specification	(1)	(2)	(3)
# of students entering grade between fall and spring divided by fall enrollment	-0.0034***	-0.0009~	-0.0023***
	[0.0008]	[0.0005]	[0.0006]
Female	0.0162***	0.0149***	0.0157***
	[0.0045]	[0.0041]	[0.0040]
Eligible for free or reduced lunch	-0.0087	-0.0150*	-0.0149*
	[0.0057]	[0.0060]	[0.0059]
ELL	0.0231~	0.0239**	0.0258**
	[0.0135]	[0.0090]	[0.0089]
Black	-0.0197*	0.0109	0.0121
	[0.0096]	[0.0076]	[0.0075]
Hispanic	0.0275**	0.0122	0.0120
	[0.0094]	[0.0086]	[0.0085]
Other race/ethnicity	0.0127	0.0240*	0.0226*
	[0.0097]	[0.0096]	[0.0095]
School mean math score (fall *t*)	-0.1477***		
	[0.0204]		
School free/reduced lunch % (fall *t*)	-0.3473***		
	[0.0506]		
Observations	116,689	116,772	116,772

Notes: Clustered standard errors appear in brackets. This includes only non-mobile students, leaving out those who change schools for promotional and non-promotional reasons. Only students enrolled in MPS public, instrumentality charter, and partnership schools during the fall of year *t* appear. Students whose race/ethnicity is listed as Caucasian are omitted.

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, ~ $p<0.10$