Year-by-Year and Cumulative Impacts of Attending a High-Mobility Elementary School on Children's Mathematics Achievement in Chicago

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Abstract

High rates of residential and school mobility are a pervasive aspect of life among the inner-city poor in contemporary America. We know that families of urban children living in poverty frequently move, usually over short distances. Moving across school catchment boundaries increases the likelihood that a residential move will trigger a school move. School moves also occur as families seek better schools, children are expelled from their current school, or because their schools are closed. Many researchers have attempted to clarify the impact of frequent school moves on children's academic learning. In this paper we ask a related but different question: whether attending a school characterized by high levels of student mobility depresses learning in the general student population. In particular, we ask whether and to what extent influxes of new students during the school year ("within-year in-migration") affect students' mathematics achievement each year and cumulatively during the elementary years. We study a cohort of more than 20,000 students tested annually and attending more than Chicago elementary schools between 1997-2000. We find that African American students and students attending high-poverty schools are at greatest risk of attending schools characterized by high within-year in-migration. Using year-by-year analysis of covariance and propensity score stratification, we find small negative annual effects of within-year in-migration. Using inverse-probability of treatment weighting, we find some evidence that these effects accumulate. However, these findings do not hold under fixed effects specifications. We find no evidence that the effects depend upon prior math ability or student mobility, though we cannot test the interaction with student mobility powerfully. The fact that findings depend upon methods of identification poses questions for continued research.

1. Introduction

By the time they reach third grade, almost half of the US children entering kindergarten in 1998 had changed schools at least once. Children from ethnic minority families and low-income families are especially likely to change schools (Burkham, Lee, and Dwyer, 2009). Kerbow (1996) reported that only 38% of Chicago elementary students attended the same school through their elementary years. Residential mobility, which is particularly high in the US as compared to other developed nations, often leads to school mobility, and residential mobility is particularly high among disadvantaged urban families (de la Torre and Gwynne, 2009; Kerbow, 1996; Hanushek, Kain, and Rivkin, 2004). Of course, children change schools not only because their residence changes, but to seek better schools, because their schools close, or because, in rare cases, of expulsion.

A large body of evidence suggests that changing schools is statistically associated with low achievement (see review by Reynolds, 2009). There are several possible explanations for this association. First, the association may be causal. It may be that frequent school changes disrupt instructional continuity, undermining sustained opportunities to learn. It also may be that school changes undermine children's capacity to become integrated in social networks, and the resulting isolation or alienation may undermine school engagement and motivation to learn. Second, the association may be spurious causally. After all, we know that those who move frequently are disadvantaged in other ways that predict low achievement; and we suspect that frequent residential moves that often trigger school moves may themselves undermine learning. Moreover, changing schools may be a response to a student's difficulties rather than a cause. Isolating the causal effect of school change is also difficult because the disruptive effect of changing schools may be offset if the move is to a better school or amplified if the move is to a worse school.

It is not surprising, then, that evidence regarding the effects of changing schools on student learning is mixed. Kerbow (1996) found that the initial negative effects associated with changing schools faded out by two years as long as students did not experience a second move during that time. Indeed, a key claim in Reynolds' meta-analysis (2009) is that frequent moves are especially harmful. Other careful studies find that once student demographic background and prior achievement are controlled, associations between student mobility and test cores diminish to non-significance (Alexander and Entwistle, 1996; Hanushek, Rivkin, and Kain, 2004; see Rumberger's 2003 review).

It is quite plausible, however, that if policies were enacted to stabilize school membership or to ameliorate its effects, school leaders and teachers who could then count on a stable population could become more effective. For example, children moving across school catchment areas could be allowed or encouraged to stay in the school they had attended; and school districts might enact common curricula so that school changes are not associated with drastic changes in curricular content. Influxes of new students during the year would then plausibly by less common and less disruptive when they do occur. Moreover, school improvement may help reduce mobility and its effects. There is some evidence that increased school quality reduces the number of school changes young children experience and that part of the positive effect of intending high quality schools is attributable to their reduced mobility rates (Reynolds, 2009).

1.1 Purpose of This Paper

In sum, a growing number of studies have produced mixed results concerning the impact of moving on children's learning. In this paper, we ask a related but different question: whether attending a “high-mobility school” undermines student achievement in mathematics during grades K-8. In particular, it may be that when large numbers of students enter a school during the year, teachers will experience difficulty sustaining effective instruction (Lash and Kirkpatrick, 1990, Kerbow, 1996). For example, it may be difficult to sustain a reasonable pace of mathematics instruction when new students appear in a classroom during the academic year. As a result, non-mobile as well as mobile students might suffer diminished learning opportunities when attending high-mobility schools. Moreover, an influx of new students may disrupt pro-academic social networks. Haynie, Scott and Bose (2006) and Scott and Haynie (2004) found that schools with high mobility rates were more likely than other schools to be characterized by anti-social peer networks and that such schools had high drop out rates. Faith and Leventhal (2005) found that highly mobile students were more likely than others to be victimized by violence. Wood and Halfen (1993) noted high levels of behavior problems in schools with high mobility.

Associations between school mobility rates and student achievement need not be causal, of course. Research reviewed above shows clearly that mobile students are at high risk of having difficulties in school for a variety of reasons; it may be that schools characterized by high mobility display low average achievement because the students they serve are multiply disadvantaged. Hanushek, Kain, and Rivkin’s (2004) article is notable in taking pains to identify the causal effect school mobility. They found that attending schools characterized by high levels of within-year in-migration statistically significantly reduced the test scores of students attending those schools. Thus, such within-year school-level mobility imposed a cost on non-mobile students. This impact was larger for poor and minority students than for their more advantaged peers. While the effects in any one year were small, the authors reasoned that, based on their model, the cumulative effects of multiple years of exposure would be substantial.

Hanushek et al. (2004) exploited variation between cohorts within the same school and grade to identify the causal effect of school mobility. Specifically, they controlled school-by-grade and school-by-year fixed effects in addition to observed teacher covariates. In this paper, we build on this work by studying "student careers in school mobility." We are interested in the exposure to school-level mobility that students experience during grades 2-4 in Chicago and in estimating the annual and cumulative effects of school mobility, defined as the rate of student in-migration during the academic year. Thus, our definition of "the treatment" is the same as that in Hanushek et al., but we want to explicitly study the cumulative effects that Hanushek et al.'s model predicts. This goal requires a different identification strategy from theirs, and we consider the problem of identification subsequently.

We also ask whether the impact of school mobility varies across students. Hanushek et al. found that poor and minority students suffered more than others from school-level mobility. However, there is also reason to suspect that high-achieving math students would especially suffer, particularly if teachers are forced to slow down the pace of curriculum coverage to accommodate students who enter their classes during the year. We are also interested in whether "stable students," that is, students who do not move, experience effects similar to those of mobile students.

2.2 School-Level Mobility in Chicago

Our causal variable of interest will be a school's rate of in-migration during the academic year, which we shall henceforth refer to as "school mobility" for simplicity, and is defined as

$$\text{School mobility=rate of within year in-migration=}\frac{n_j \ new}{n_j \ in \ May}, \qquad (1)$$

where n_j *new* is the number of students who joined the school after the beginning of the school year and n_j *in May* is the school's enrollment in May of that year.

If high levels of school mobility thus defined suppress student achievement, then many students in Chicago will have been affected. Rates of school mobility have been quite high there, though they have decreased slowly over time. In 1995 the school mobility rate was, on average, 11.4%, declining to 7.1% in 2007. For African American students the rates have been higher and have declined less, from 12.5% in 1995 to 9.5% in 2007 (de la Torre and Gwynne, 2009). The standard deviation of school mobility in a given year has hovered around .05. Given the slightly positively skewed distribution of this variable (see below), it is plausible to find students, particularly African American students, attending schools in which as many as 18% of the students enrolled at the end of the school year had migrated into the school during the year. Between year in-migration rates resulting from transfers during the summer are even more numerous and high rates of such mobility may also affect achievement. However, we reason that a high influx of students during the school year is especially challenging, and so we focus in this paper on this form of school mobility.

The overarching questions for our study, then, is whether attending a school with a high mobility rate so defined depresses students' mathematics achievement during the elementary years and whether these effects cumulate over time. (A subsequent paper will consider the transition into middle school and the middle school years.) We shall address a sequence of closely related questions:

1. How do "student careers" in school mobility vary as a function of student background between grades 2 and 4 in Chicago? To answer this question, we will study individual

trajectories in exposure to school mobility, enabling us to see how much students vary in exposure by year and cumulatively.

2. What are the impacts on average, of exposure to school-level mobility by year and cumulatively?

3. Do these impacts vary by prior mathematics achievement and student-level mobility status?

4. How sensitive are our conclusions to alternative model specifications and assumptions?

2. Sample and Data

Our analyses focus on a single cohort of more than 20,000 students tested annually and attending over 400 Chicago schools between 1997-2000, generating 47,692 time-series observations[1]. During these years, most students progressed from grades 1-4. Most students are either African-American or Hispanic, with a minority of White or Asian background.

The outcome variable is the mathematics sub test of the Iowa Test of Basic Skills. This outcome has been equated over years such that the scores represent the log-odds of a correct response to an item of average difficulty.

Time-varying covariates include age, concentration of poverty of the block group of the student's residence, "social status" of the block group (a composite of percent professional and percent with a college education on the block group), the concentration of poverty status of the school attended, the social status of the school population (an aggregate across the block groups in which the students reside), the ethnic composition of the school, the school's percent of students with limited English proficiency, the school's average class size, and the school's status

[1] Analytic sample sizes vary somewhat according to the statistical analysis methods described in Section 4.

as a charter or magnet schools. Time-invariant variables include gender, ethnicity, and eligibility and prior math and reading test scores.

Table 1 describes the sample based on 24,826 students in the 1998-1999 school year when most of these students were in second grade. We see that most students are Black (73%) or Hispanic (18%). The schools these students attend have high concentrations of poverty – with 90% of the students eligible for reduced price or free lunch. We have data on the level of concentrated disadvantage of the block group in which each child lives as well as a measure of that block group's "social status" as indicated by the fraction of adults in the block group who graduated from college and the fraction holding a professional or managerial job. These child-specific local neighborhood indicators have been aggregated to the school level to generate measures of the school's social composition. Reading and mathematics sub-tests of the Iowa Test of Basic Skills are available each year from 1997-2000.

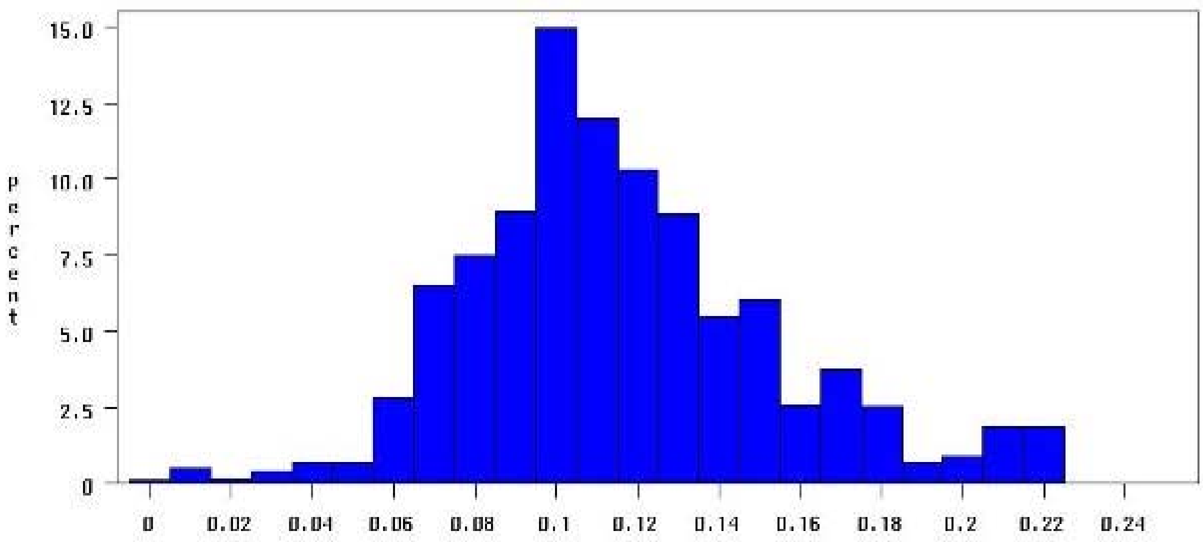
Table 1: Description of the Sample

Variable	Mean	SD	Minimum	Maximum
Math Score	-2.18	0.95	-6.70	2.96
Reading Score	-2.21	1.19	-6.93	2.50
Age	6.71	0.46	6	8
Average Class Size	24.87	2.83	17	45
Neighborhood Concentrated Poverty	0.42	0.73	-2.95	4.92
Neighborhood Social Status	-0.33	0.64	-2.51	3.18
School Mean Concentrated Disadvantage	1.49	0.48	0.51	2.86
School Mean Neighborhood Concentrated Poverty	0.42	0.61	-0.93	2.48
School Mean Neighborhood Social Status	-0.33	0.45	-1.52	1.51
School Percent Asian	1.89	6.50	0	58.30
School Percent Black	70.52	40.27	0	100
School Percent Hispanic	21.50	31.87	0	99.5
School Percent LEP students	11.85	17.67	0	77.20
School Percent Low Income	90.63	9.32	11.10	99.60
School Percent White	5.98	13.44	0	77.60
School Size	788.37	325.68	50	2687
School-level In-Mobility	0.12	0.04	0	0.22
Student has Birthday Before September	0.65	0.48	0	1
Student is Asian	0.02	0.14	0	1
Student is Black	0.73	0.44	0	1
Student is Female	0.50	0.50	0	1
Student is Hispanic	0.18	0.39	0	1
Student is White	0.09	0.28	0	1
Student Moved During School Year	0.08	0.28	0	1
Student Repeated a Grade	0.04	0.21	0	1

The "treatment" or "exposure" of interest is the intensity of school-level mobility defined as the percent of students who moved in during the year (see Equation 1). As Table 1 shows, the mean is 12% in 1998 with a standard deviation of 4% (this increases to 5% in later years). Figure

1 displays the distribution of school mobility so defined during the 1998-1999 academic year. The histogram shows that some children attended schools with very few or even no in-movers while we can easily find students attending schools where 18% or more of their classmates were in-movers. So the student exposure to school mobility varies significantly. We shall consider variation within students on school mobility in the results section.

Figure 1: Distribution of school mobility across the sample during the 1998-1999 school year.



3. Causal Model

We are interested in the estimating the average effect of a given "dosage" of a continuous variable z_h relative to an alternative dosage $z_h^/$ of school mobility during academic year h on student mathematics achievement. If a student is exposed to dosage z_h, that student will display math achievement $Y(z_h)$ whereas if exposed to dosage $z_h^/$, the outcome or "response" will be $Y(z_h^/)$. The causal effects of interest have the form $Y(z_h) - Y(z_h^/)$. In principle, the range of dosages is constrained to $0 \le z_h \le 1$ by our definition of school mobility rate (Equation 1).

However, the range of plausible dosages to which children of varied background might actually be exposed is an empirical question that we address in Section 5.

We are also interested in the cumulative effects, on average, of exposure to school mobility, that is, the effects of $z_t = \sum_{h=1}^{t} z_h$, the exposure experienced up to (and including) year *t* in achievement Y_t in year *t*. The potential outcomes at year *t* are $Y(z_t)$ and the causal effects of interest are $Y(z_t) - Y(z_t^{/})$ where $z_t^{/} = \sum_{h=1}^{t} z_h^{/}$.

After exploring the plausible dosages and dose-response associations, we have settled on a linear dose-response relationship as a reasonable approximation. Thus, we summarize the time-specific causal effects as $\Delta = [Y(z_h) - Y(z_h^{/})]/(z_h - z_h^{/})$ for $z_h \neq, z_h^{/}$. Similarly, the cumulative effects are linear, $\Omega = [Y(z_t) - Y(z_t^{/})]/(z_t - z_t^{/})$ for $z_t \neq, z_i^{/}$. Note that Δ and Ω are person-specific random variables having population means $E_p(\Delta) = \delta_p$ and $E_p(\Omega) = \omega_p$ where the expectation is taken over units within some population or sub-population *p*.

For some of our analyses, our model assumes not only that the dose-response model is linear for any given year but that the slope coefficient Δ is, for each student, invariant over the elementary years. It also assumes no interaction between the dosage in one year and the dosage in the next. We can probe these assumptions by estimating effects year by year as we shall. The cumlative effects model assumes no decay in effects: an effect one year “sticks with” the student in subsequent years. We can test this by comparing the size the estimated cumulative effects to the sum of the effects estimated year by year.

4. Identification of Causal Effects

In non-experimental studies like ours, causal inference is challenging. We know that schools in Chicago that experience high levels of student mobility serve somewhat disadvantaged populations (de la Torre and Gwynne, 2009). Nevertheless, it may be that schools experiencing high influxes of students during the year are more effective or at least more attractive than schools that mobile students might otherwise choose. So selection bias poses one challenge to valid causal inference. We shall use alternative strategies (analysis of covariance and adjustment via the propensity score) to adjust for observed confounders when we estimate year-by-year effects. We shall also estimate fixed effects models that eliminate observed and unobserved time-invariant confounding.

Given our aim of understanding the cumulative effects of exposure to high school mobility, another challenge is reciprocal causation over time. The parents of a child suffering the negative effects of attending a high mobility school may decide to transfer schools, possibly increasing or decreasing the risk of attending another school with high mobility. Thus, the outcomes of earlier dosages of school mobility may change the expected dosage of school mobility experienced in the future. The fixed effects specification, effective in removing time-invariant confounding, is not ideally suited to the presence of such time-varying confounding. Robins, Hernan, and Brumback (2000) proposed a marginal structural means model with inverse probability of treatment weighting to cope with this problem, and Hong and Raudenbush (2008) extended this to the case where students move across classrooms or schools over time. We shall employ this strategy as well.

Given its advantage in removing time-invariant confounding, and despite its shortcomings in adjusting for time-varying confounding, we shall also estimate fixed effects models. Using the fixed effects approach, an additional challenge arises when the assumption of an interval scale metric fails. Under the assumption of interval scaling, a unit of growth reflects the same amount of learning regardless of a student's initial status. In preliminary analyses, we found that students whom we expect to score highly in mathematics based on their past do exhibit high average achievement in the future, but their rates of growth are, on average, lower than are those of less able children. In essence, high initial ability is associated with low rates of growth. While this might reflect a citywide policy of providing more attention to the low achievers than to the high achievers, we suspect instead that this result stems from the scaling of the ITBS math achievement test used in our Chicago data. A failure to account for these expected differences in growth rates introduces a negative bias into the fixed-effects estimates: low scoring students, who are exposed to comparatively high rates of school mobility, can be expected to show comparatively *high* rates of learning on ITBS! A failure to adjust for these differences in expected learning rates makes the highly-mobile students appear to be growing comparatively well. The conventional fixed effects model does not control for heterogeneity in age-related growth rates. To solve this problem we introduce the idea of controlling for the "prognosis score," that is, a student's the expected future outcome based on all available past information. We shall estimate fixed effects models within 10 strata defined by the prognosis score in order to remove this bias. Within each stratum, adjustment is made for stratum-specific average growth rates in the absence of treatment.

In the next four sections we consider our analytic strategies in more detail.

4.1 Year-by-Year Effects: Two-level Analysis of Covariance

We shall estimate the average impact of school mobility at times h=1 (1998), h=2 (1999) and h = 3 (2000) by regressing the outcome at the spring of year h on relevant pre-treatment covariates and dosage level. Within any given year, students are nested within schools. We adopt a two-level model (students nested within schools) using school-level random effects to insure efficient point estimates and standard errors. We also compute robust standard errors as a check on model specification.

Define for each student *i* attending school *j* at time *h* the variable L_{hij} which contains all of the available information about that student ***prior to*** receipt of the treatment at time *h*. Define X_{hij} as a vector of covariates available prior to receipt of the treatment at time *h*. Define Z_{hij} as the school mobility level or dosage experienced by that student at time *h*. Define the observed outcome at time *h* as Y_{hij}. The pre-treatment covariates at time *h* can then be gathered into the vector L_{hij} that captures a student's history of covariates, treatments, and outcomes, where

$$\begin{aligned} L_{1ij} &= \{X_{1ij}, Z_{0ij}, Y_{0ij}\} \ (for\ 1998) \\ L_{2ij} &= \{X_{1ij}, X_{2ij}, Z_{0i}, Z_{1i}, Y_{0ij}, Y_{1ij}\} \ (for\ 1999) \\ L_{3ij} &= \{X_{1ij}, X_{2ij}, X_{3ij}, Z_{0i}, Z_{1i}, Z_{2i}, Y_{0ij}, Y_{1ij}, Y_{2ij}\} \ (for\ 2000). \end{aligned} \tag{2}$$

The two-level analysis of covariance model is then

$$Y_{hij} = \beta_{0h} + \beta_{1h}^{T} L_{hij} + \delta_h Z_{hij} + u_{hj} + \varepsilon_{hij}. \tag{3}$$

where β_{0h} is the intercept at year *h*, β_{1h} is a vector of regression coefficients associated with elements of L_{hij}; δ_h is the regression coefficient linking exposure Z_{hij} to the outcome given L_{hij}; u_{hj} is a school-specific random effect assumed independently distributed across schools with variance τ_h^2; and ε_{hij} is a child-specific random effect assumed independently distributed across children within schools and across schools and with variance σ_h^2.

Equation 3 identifies the average causal effect on two key assumptions:

(i) The ANCOVA model holds in the potential outcomes, that is, $Y(z_h) = \beta_{0h} + \beta_{1h}^T L_h + \delta_h z_h + u_h(z_h) + \varepsilon_h(z_h)$. This means that the potential outcome is linear in L_h at every level of z_h with constant slope β_{1h} so that $E[u_h(z_h)] = E[\varepsilon_h(z_h)] = 0$ at every combination of L_h and z_h.

(ii) Treatment assignment is independent of the potential outcomes given L_h. This is the assumption of "strongly ignorable treatment assignment" based on observed covariates (Rosenbaum and Rubin, 1983), that is $Z_h \perp Y(z_h) \mid L_h$.

Applying these assumptions to Equation 3, we have

$$\begin{aligned} E(Y_{hij} \mid Z_{hij} = z_{hij}, L_{hij}) &= \beta_{0h} + \beta_{1h}^T L_{hij} + \delta_h z_{hij} + E(u_{hj} \mid Z_{hij} = z_{hij}, L_{hij}) + E(\varepsilon_{hij} \mid Z_{hij} = z_{hij}, L_{hij}) \\ &= E[Y_{hij}(z_{hij})] = \beta_{0h} + \beta_{1h}^T L_{hij} + \delta_h z_{hij} + E(u_{hj} \mid L_{hij}) + E(\varepsilon_{hij} \mid L_{hij}) \end{aligned} \quad (4)$$

Note it is not essential that our estimate of β_{1h} be unbiased so that we need not have $E(u_{hj} \mid L_{hij}) = E(\varepsilon_{hij} \mid L_{hij}) = 0$; however, covariates not in the model and therefore represented in $u_{hj}, \varepsilon_{hij}$ must not be linearly associated with *Z* within levels of *L*. Assumption (i) is strong but can to some extent by checked against alternative specifications. Assumption (ii) is strong and cannot be checked. However, as *h* increases from 1 to 3, we accumulate on each student achievement histories captured in L_h in math and reading, school mobility histories, neighborhood residential histories in terms of the levels of concentrated disadvantage and professional/managerial density of the local block group of the student, school composition histories in terms of the school means of the levels of concentrated disadvantage, and several other covariates. If these increasingly comprehensive pre-treatment histories help us reduce bias, and if the treatment effects are always null, we would likely see smaller estimated coefficients for school mobility as *h* increases.

4.2 Propensity Score Stratification

An important criticism of adjustment by means of the analysis of covariance is that it can use the linearity assumption to generate misleading causal inferences when the data lack meaningful comparative information. For example it may in our case that students exposed to very high mobility schools are systematically different from other students on observed covariates related to mobility. This "failure of common support" is not readily visible to the analyst using ANCOVA (Rubin, 1997), though of course it does inflate standard errors. Closely related to the linear model assumption is the assumption of an interval scale metric (Reardon and Raudenbush, 2009). As mentioned, the linearity assumption requires that we believe that a one-point gain in the outcome at every level of the pre-treatment outcome distribution represents the same amount of learning. In the absence of common support, a failure of this assumption can substantially distort results. In contrast, in the presence of common support, inferences are more robust in the face of a failure of the assumption of an interval scale because there will be no association between prior achievement and the dosage of school mobility received within a stratum. A second criticism of ANCOVA is that the aim of adjusting for potentially large numbers of pre-treatment covariates can generate instability in coefficient estimates and encourage the comparison of many models, each with a different estimate of the treatment effect.

For binary treatments, matching or finely stratifying on the propensity score – the conditional probability of treatment group assignment given pre-treatment covariates – addresses these problems of ANCOVA while nonetheless still requiring assumption (ii) of ignorable treatment assignment given the observed prior history. Most applications of this logic involve binary treatments, whereas our study uses an approximately continuous dosage measure. Imai and van Dyk (2004) extended the logic of propensity score stratification to the case of continuous treatment regimes, and we adopt their methods here. Define $P_h(L_h) = E(Z_h \mid L_h)$ as the propensity function or propensity score, in effect, the expected dosage given the past history as captured in L_h. The key result is that, in large samples, conditioning on the one-dimensional function $P_h(L_h)$ is equivalent to conditioning on the high-dimensional covariate L_h. Thus, if treatment assignment is strongly ignorable given L_h (assumption (ii) above) then it is also strongly ignorable given $P_h(L_h)$. An additional assumption is that the propensity score is correctly specified. However, that assumption can be checked and virtually assured to hold by checking balance, that is checking to see whether the association between Z_h and each element of L_h is null within strata sharing the same or very similar $P_h(L_h)$. We can rectify a failure of balance by either re-stratifying or dropping strata that fail to produce balance. Dropping cases may seem problematic, but the cases so dropped are those that fail the test of common support and therefore contain no causal information. Doing so, of course, modifies the target population for causal inference.

Our estimation model for the propensity score analysis is

$$Y_{hij} = \gamma_{0h} + \delta_h Z_{hij} + \sum_{s=1}^{S-1} \gamma_s D_{shij} + u_{hj} + \varepsilon_{hij} \qquad (5)$$

where D_{shij} is a dummy variable indicating membership of student *i* attending school *j* at time *h* and in propensity stratum *s*, for *s*=1,…,*S*-1; and γ_s is a stratum-specific fixed effect. Under the assumption of strongly ignorable treatment assignment given propensity stratum membership, $E(u_h \mid Z_h = z_h, L_h) = E(u_h \mid L_h)$ and $E(\varepsilon_h \mid Z_h = z_h, L_h) = E(\varepsilon_h \mid L_h)$ and so Equation (5) identifies the causal effect, δ_h. Covariates can be included on the right-hand-side of Equation 5 to increase precision.

4.3. Inverse Probability of Treatment Weighting with Continuous Treatment

One of our primary aims is to assess the cumulative effect of exposure to high school mobility. Small effects estimated in a single year may accumulate into large effects, but empirically testing this proposition requires longitudinal data on exposure to school mobility. As mentioned, a key challenge is that responses to earlier outcomes can change the risk of exposure to school mobility in the future while also predicting future outcomes. Robins, Hernan, and Brumbeck (2000) discuss the application of inverse probability of treatment weighting to the case of a repeated cumulative dosage. The key idea is that observed time-varying confounders are controlled through weighting rather than by incorporating these confounders as explanatory variables. The key assumptions are two:

(i) Sequentially strongly ignorable treatment assignment requires that the dosage of school mobility received at each time *h* is independent of all future potential outcomes at a given time given past observables. That is $Z_h \perp Y(z_h), Y(z_{h+1}),..., Y(z_T) \mid L_h$ for *h*=1,...,*T*-1 where *T* is the number of time-series observations.

(ii) As in the case of propensity score stratification, we correctly specify the propensity function $P_h(L_h) = E(Z_h \mid L_h)$.

(iii) We shall also assume for simplicity that yearly dose-response relationships are equal and additive.

Hong (2009) showed that a failure of common support will generally cause bias in applications of IPTW. We therefore use only those cases in the IPTW analysis that we found to meet the common support test in the propensity score analysis described just above.

The logic of IPTW works as follows. First, write down the model one would estimate if the treatment had been assigned at random with equal probability for all at each time point. Next, define the IPT weights. Third, analyze the "as if randomized" model but with weighting.

4.3.1. Analytic Model for the Outcome. If school mobility had been assigned to students at random each year, we would estimate a cross-classified random effects model wherein time-series observations are cross classified by the child and the school attended. We formulate first a within-cell model that describes the association between time-varying predictors and outcomes for each "cell" defined by the cross-classification of students and schools. The parameters of this model can, in principle, vary over children and schools as a function of explanatory variables and random effects.

Level 1 (between time-points within student-by school "cells"):

$$Y_{ti} = \pi_{0ti} + \pi_{1i}(Age_{ti}) + \pi_{2i}\sum_{h=1}^{t} Z_{hi} + e_{ti}, \qquad e_{ti} \sim N(0,\sigma^2) \tag{6}$$

where Y_{ti} is the math outcome for child *i* at time *t*; π_{0ti} is an intercept that will change over time and as a function of which schools the child has attended by time *t*; π_{1i} is the annual growth rate of child *i* if that child attends schools of average effectiveness; π_{2i} is a treatment effect that might in principle vary by student; Z_{hi} is the mobility rate for the school child *i* attends in year h; and e_{ti} is a random error identically and independently distributed $N(0,\sigma^2)$.

Level-2 (between cells):

$$\pi_{0ti} = \theta_0 + b_{00i} + \sum_{h=0}^{t} c_{j_{hi}}$$
$$\pi_{1i} = \theta_1 + b_{10i} \quad (7)$$
$$\pi_{2i} = \omega.$$

Here θ_0 is the overall average intercept and b_{00i} is a child-specific random effect; θ_1 is the overall average growth rate per year and b_{10i} is a child-specific random effect on the growth rate; ω is the average cumulative effect of school mobility; and $c_{j_{hi}}$ is the random effect associated with school j_{hi}, the school child *i* attends at time *h*. Note that these school random effects cumulate. For the random effects we assume

$$\begin{bmatrix} b_{00i} \\ b_{10i} \end{bmatrix} \sim N\left[\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \tau_{b00} & \tau_{b01} \\ \tau_{b10} & \tau_{b11} \end{pmatrix}\right], \qquad c_{j_{hi}} \overset{iid}{\sim} N(0, \tau_c^2).$$

The logic may be clearer if we look at the combined model and how it changes over three years:

$$Year\ 0\ (1997): Y_{0i} = (\theta_0 + b_{00i}) + c_{00\,j_{0i}} + e_{0i}$$
$$Year\ 1\ (1998): Y_{1i} = (\theta_0 + b_{00i}) + (\theta_1 + b_{10i})(age)_{1i} + \omega Z_{1i} + (c_{00\,j_{0i}} + c_{00\,j_{1i}}) + e_{1i}$$
$$Year\ 2\ (1999): Y_{2i} = (\theta_0 + b_{00i}) + 2(\theta_1 + b_{10i})(age)_{2i} + \omega(Z_{1i} + Z_{2i}) + (c_{00\,j_{0i}} + c_{00\,j_{1i}} + c_{00\,j_{2i}}) + e_{2i}$$
$$Year\ 3\ (2000): Y_{3i} = (\theta_0 + b_{00i}) + 3(\theta_1 + b_{10i})(age)_{3i} + \omega(Z_{1i} + Z_{2i} + Z_{3i}) + (c_{00\,j_{0i}} + c_{00\,j_{1i}} + c_{00\,j_{2i}} + c_{00\,j_{3i}}) + e_{3i}$$
(8)

Under this specification, the gain a student experiences during each year *h* (apart from within-child noise e_{ti}) is $\theta_1 + b_{10i} + \omega Z_{hi} + c_{00\,j_{hi}}$, the sum of three pieces: the child's person specific annual growth rate, $\theta_1 + b_{10i}$, the school's "value added" $c_{00\,j_{hi}}$, and the cumulating treatment effect ωZ_{hi}.

4.3.2. Defining the weights. Equations 6 and 7 would identify the cumulative effect of school mobility under sequential random assignment of students to doses of school mobility (and, of course, assuming our simple causal structure). However, in fact, students will be assigned doses of school mobility with varied probabilities depending on their past experiences and background characteristics. To solve this problem, the IPTW approach borrows the idea of weighting from sample survey research: those who are "over-represented" at each dosage level will be weighted down.

Specifically, for each student i at time h define the h^{th} "pieces" of the weight sp_{hi}

$$sp_{0i} = 1, \quad sp_{1i} = \frac{f(z_{1i})}{f(z_{1i} \mid L_{1i})}, \quad sp_{2i} = \frac{f(z_{2i} \mid Z_{1i})}{f(z_{2i} \mid L_{2i})}, \quad sp_{3j} = \frac{f(z_{3i} \mid Z_{1i}, Z_{2i})}{f(z_{3i} \mid L_{3i})}. \tag{9}$$

Then the stabilized weights for at time t is $sw_{ti} = \prod_{h=1}^{t} sp_{hi}$. Thus, for times 1,2, and 3 we have

$$sp_{0i} = 1, \; sw_{1j} = sp_{1j}, \; sw_{2j} = sp_{1j} * sp_{2j} \; sw_{3j} = sp_{1j} * sp_{2j} * sp_{3j}. \tag{10}$$

Robins (2000) proved that maximization of the score function weighted by weights of the type in Equation 10 would yield consistent estimates of treatment effects under our assumptions. Hong and Raudenbush (2008) extended this approach to multilevel data, specifically to the case of cross-classified random effects, the design of the current study. They showed how to apply the weights and derive consistent model-based standard errors in this case. The model-based standard errors are important because Huber-White robust standard errors are not readily available in the case of students moving across schools because the vector of residuals for each student is potentially correlated with the vector of residuals for every other student.

4.3.3 **Evaluating the density functions** $f(z_h \mid L_h)$. If a treatment z_h is binary, P_h =Prob(Z_h=z_h|L_h) for z_h=0,1. Thus, cases are weighted inversely proportional to the probability of being selected into the treatment they actually received. Cases over-represented by selection bias are thus down-weighted. The numerators of s_{ph} are the probabilities under randomization, that is, with no dependence on L_h . In contrast, when Z_h is a continuous variable taking on values z_h, $f(z_h \mid L_h)$ is the conditional probability density function evaluated at z_h. To implement this approach, one must therefore adopt a continuous density function to represent the distribution of dosages given L_h. After examining the residuals from our model of $P(z_h \mid L_h) = E(z_h \mid L_h)$, we selected the normal density function for *f*. The resulting weighted analysis using Equations 6 and 7 will provide us with our estimate of the cumulative effects of school mobility.

4.4. Fixed Effects Models with Adjustment for the Prognosis Score and Time-Varying Covariates

While carrying some potential disadvantages discussed earlier, the fixed effects model offers the well-known important advantage of removing all time-invariant confounding from estimates of the treatment effect. We estimated two kinds of fixed effects models to supplement our year-by-year estimates via ANCOVA and the propensity score: student fixed effects models with adjustment for time- varying covariates defined at the student and school-levels; and models with student and school fixed effects with adjustment for time-varying student-level covariates. We estimated these without and with a further adjustment for the prognosis score as discussed below. Key time-varying student-level covariates are the concentrated disadvantage and social status of the block group in which the student resides. Key time-varying school-level covariates are the concentrated disadvantage and mean social status of the school attended.

Raudenbush (2009) proved that the conventional fixed effects model (e.g., Mundlak, 1978) is equivalent to a random effects model in which explanatory variables are centered about their unit means. He extended this principle to an arbitrary number of fixed effects dimensions and an arbitrary number of levels of clustering, and showed that this method of "adaptive centering with random effects" offers certain advantages in terms of flexibility and computational ease.

4.4.1 Student fixed effects model. We estimated the model

$$Y_{hi} = \beta_0 + \beta_1(age_{hi} - \overline{age}_i) + \delta(Z_{hi} - Z_{\cdot i}) + \beta_2^T(X_{hi} - \overline{X}_{\cdot i}) + b_i + c_{j_{hi}} + \varepsilon_{hi}, \tag{11}$$

where X_{hi} is a vector of covariates observed at time *h* for student *i* and $\overline{X}_{\cdot i}$ is the student-specific mean of those covariates over the T_i occasions on which student *i* was observed, that is $\overline{X}_{\cdot i} = \sum_{h=1}^{T_i} X_{hi} / T_i$. Similarly, $(age_{hi} - \overline{age}_i)$ is person-mean centered age at time *h*, and $Z_{hi} - Z_{\cdot i}$ is the person-mean centered school mobility at time *h*. The model includes zero-mean random effects $b_i, c_{j_{hi}}, \varepsilon_{hi}$ defined between students, within-students, and between schools, respectively. We also estimated models that were quadratic in age and that allowed the age coefficient to vary randomly over children. The identifying assumptions have a similar form to those used for the ANCOVA: (i) linearity in age, school mobility, and covariates, *X*; and ignorable treatment assignment given time-varying covariates $(X_{hi} - \overline{X}_{\cdot i})$. Note that the person-mean centered dosage $(Z_{hi} - Z_{\cdot i})$ is orthogonal to the person-specific random effect b_i by construction because $\sum_{i=1}^{n}\sum_{h=1}^{T_y} b_i(Z_{hi} - Z_{\cdot i}) = \sum_{i=1}^{n} b_i \sum_{h=1}^{T_y}(Z_{hi} - Z_{\cdot i}) = 0$. The more problematic assumption is that the school-specific and child-specific random effects are conditionally mean-independent of dosage given the covariates, that is:

$$\begin{aligned} &E[c_{j_{hi}} \mid (age_{hi} - \overline{age}_i), (X_{hi} - \overline{X}_{\cdot i}), (Z_{hi} - Z_{\cdot i})] = E[e_{hi} \mid (age_{hi} - \overline{age}_i), (X_{hi} - \overline{X}_{\cdot i}), (Z_{hi} - Z_{\cdot i})] \\ &= E[c_{j_{hi}} \mid (age_{hi} - \overline{age}_i), (X_{hi} - \overline{X}_{\cdot i})] = E[e_{hi} \mid (age_{hi} - \overline{age}_i), (X_{hi} - \overline{X}_{\cdot i})] = 0. \end{aligned} \tag{12}$$

This assumption depends on the comprehensiveness of the time-varying child-specific and school-specific covariates. We can relax this assumption by controlling for school fixed effects, in which case we need only assume

$$E[e_{hi} \mid (age_{hi} - \overline{age}_i), (X_{hi} - \overline{X}_{\cdot i}), (Z_{hi} - Z_{\cdot i})] = E[e_{hi} \mid (age_{hi} - \overline{age}_i), (X_{hi} - \overline{X}_{\cdot i})] = 0. \quad (13)$$

4.4.1 Student and school fixed effects model. We therefore further elaborated the model to include school fixed effects. This is achieved by "double centering" of all explanatory variables, as

$$\begin{aligned} Y_{hi} = \beta_0 + \beta_1(age_{hi} - a\hat{g}e_i - a\hat{g}e_{j_{hi}} - \overline{age}) + \delta_h(Z_{hi} - \hat{Z}_i - \hat{Z}_{j_{hi}} + \overline{Z}) \\ + \beta_2^T(X_{hi} - \hat{X}_i - \hat{X}_{j_{hi}} + \overline{X}) + b_i + c_{j_{hi}} + \varepsilon_{hi} \end{aligned}. \quad (14)$$

The quantity $(X_{hi} - \hat{X}_i - \hat{X}_{j_{hi}} + \overline{X})$ can be regarded as the residual obtained from regressing X_{hi} on the set of indicators for students and the set of indicators for schools. It is in fact an interaction contrast in which, and, in a balanced design, would be $(X_{hi} - \overline{X}_i - \overline{X}_{j_{hi}} + \overline{X})$. Raudenbush (2009) shows a simple method of computing these, which can be important with over 400 schools and 20,000 students. The variables age_{hi} and Z_{hi} are defined similarly.

4.4.2 The importance of the prognosis score. We found that age-elated growth rates in mathematics achievement were negatively associated with our best estimates of a child's initial status at a given time point. We do not suspect this reflects solely from regression, or do we believe that the test has a "ceiling." However, our strategy is designed to cope with this problem whatever the cause. We collect all data from the initial time point *h*=0 (1997) until *h*=*t* (1998) to generate our best estimate of each child's ability at time *t*. Specifically, for each child *i* nested within school *j* at time *h*, we estimate the three-level hierarchical linear model $Y_{hij} = \pi_{0ij} + \pi_{1ij}(age_{hij} - age_{tij}) + \beta_1^T L_{hij} + e_{tij}$ where π_{0ij}, π_{0ij} are, respectively the person-specific intercept and linear growth rate, and can vary randomly over students within schools and over schools. Under this model, π_{0ij} is the true mathematics achievement at time *t* and we therefore define $E(\pi_{0ij} \mid Y_{0ij}, Y_{1ij}, ..., Y_{tij}, L_{0ij}, L_{1ij}, ..., L_{tij}) \equiv A_{tij}$ as the "prognosis score." We prefer this approach to stratifying on Y_{tij} itself, which includes measurement error. We then stratify the sample into deciles of *A* and re-formulate our fixed effects models. For example, the student fixed effects model becomes

$$Y_{hi} = \left[\sum_{d=1}^{D=10} M_{dhi} [\beta_{0d} + \beta_{1d}(age_{hi} - \overline{age}_i) + \delta_d (Z_{hi} - Z_{\cdot i}) \right] + \beta_2^T (X_{hi} - \overline{X}_{\cdot i}) + b_i + c_{j_{hi}} + \varepsilon_{hi}. \quad (15)$$

We are thus estimating intercepts, age slopes, and treatment effects within each of 10 strata M_{dhi}, d=1,…,D=10 for 1999 and 2000.

5. Results

5.1 Student Careers in School Mobility: Which Students are Exposed to High Mobility Schools, and When?

A key aim of this paper is to estimate a "dose-response" function: how a percentage point increase in school-level mobility affects student mathematics achievement during a year and cumulatively over years. To understand the practical importance of such results requires that we understand the size of the "doses" students receive. It is also of interest to policy to know which kinds of students in which kinds of schools are at highest risk of receiving high doses, if indeed the dose-response relationship is non-zero. Finally, the fixed effects estimates rely depend on within-student differences in dose over time and so it becomes important to understand how much variation in doses exists within children and how much lies between children.

To answer these questions, we formulated a series of "growth models" in which the outcome is repeatedly measured school mobility. Within each child, the growth model is linear over the four years 1997-2000,

$$Z_{hi} = \alpha_{0i} + \alpha_{1i}(age_{hi} - \overline{age}) + e_{hi}, \quad e_{hi} \sim N(0, \sigma_w^2) \tag{16}$$

where Z_{hi} is the school mobility experienced by child *i* in year *h*, $(age)_{hi}$ is the age of child *i* in that year, $(age_{hi} - \overline{age})$ is the age of child *i* in year *h* centered around the grand mean, and e_{hi} is a within-child random effects having variance σ_w^2. Given these definitions, α_{0i} is child *i*'s expected school mobility rate evaluated at the sample mean age, and α_{1i} is child *i*'s growth rate. Looking across children, we then have the model

$$\begin{aligned} \alpha_{0i} &= \gamma_{00} + \sum_{q=1}^{Q} \gamma_{0q}(X_{qi} - \overline{X}_q) + u_{q0i} \\ \alpha_{1i} &= \gamma_{10} + \sum_{q=1}^{Q} \gamma_{1q}(X_{qi} - \overline{X}_q) + u_{q1i}, \end{aligned} \qquad \begin{bmatrix} u_{q0i} \\ u_{q1i} \end{bmatrix} \sim N\left[\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \tau_{q00} & \tau_{q01} \\ \tau_{q10} & \tau_{q11} \end{pmatrix}\right], \tag{17}$$

Here time-invariant child characteristics X_{qi}, centered around their grand means $\overline{X}_q$, predict the child-specific average school mobility, α_{0i} and the child-specific annual increases in school mobility, α_{1i}, with unobserved differences between students captured in the random effects, u_{q0i}, u_{q1i}. It is also of interest to include time-varying covariates in the child-specific equation (10).

We found the average level of school mobility over the four-year period to be $\hat{\gamma}_{00} = .114$, so that we can expect about 11.4 percent of the students in a typical school at the end of the year to have been in-movers during the year. The mobility level diminished slowly (but highly statistically significantly) over time at a rate of about $\hat{\gamma}_{10} = -.0023$ per year, so that the overall rate was about a percentage point lower in 2000 than in 1997. Black students experienced significantly higher mobility rates than did others, or about 12.0 per cent, on average, and the rate of decline for Blacks was also slightly but statistically significantly less than for students of other ethnic groups.

About half of the total variance in school mobility lies within children ($\hat{\sigma}_w = 0.028$) and half between children ($\sqrt{\hat{\tau}_{q00}} = 0.027$). The single-most important time-varying covariate was the school mean level of concentrated disadvantage (recall this is the average level of neighborhood disadvantage experienced by a child in a given school). Attending a school one standard deviation above average in neighborhood disadvantage was associated with nearly a percentage point increase in school mobility. The single most important child-level covariate was the race of the child, with Blacks experiencing more school-level mobility as mentioned.

The within-child variation in dosages is small but not negligible. One could plausibly find thousands of children whose school mobility rate increased (or decreased) about 3 percentage points in a given year, and one could plausibly find hundreds of students whose school mobility rate increased 5 percentage points per year out of our analytic sample of around 20,000 students per year.

5.2 Year by Year Estimates via ANCOVA

Recall that we estimated effects of school mobility one year at a time using ANCOVA and propensity score stratification. The ANCOVA yield dose-response estimates of $\hat{\delta}_{1998} = -0.48$, se=.40; $\hat{\delta}_{1999} = -.33$, se= 0.27 and $\hat{\delta}_{2000} = -0.65$, se= 0.25 for 1998, 1999, 2000, respectively (see Table 2). To gauge the size of these effects, recall that the standard deviation of the school mobility variable is approximately 0.05; and the standard deviation of the outcome is 1.03, 1.11, and 1.16, respectively in 1998, 1999, and 2000. Thus, according to these models, a one-standard deviation increase in school mobility would be associated with reductions, on average, of .023, 0.014, and 0.024 standard deviations in math achievement in the three years, respectively.

Table 2: Results of two-level analyses of covariance for 1998, 1999, 2000

Variables	**1998~**	**1999 ~**	**2000 ~**
Intercept	-1.94 (.040)***	-1.325 (.011)***	-.651 (.010)***
% Low Inc (t)	.0007 (.002)	-.0001 (.002)	.00005 (.001)
% White (t)	.0005 (.002)	.0003 (.002)	.002 (.002)
% Black (t)	-.001 (.001)	-.0004 (.001)	.0001 (.001)
Mean ConDis (t)	.017 (.066)	.037 (.056)	.066 (.046)
School in-mob (t)	-.408 (.395)	-.329 (.265)	-.646 (.248)***
Size (t)	.000003 (.0001)	-.00006 (.00004)	-.00003 (.00003)
% LEP (t)	-.003 (.003)	-.0008 (.002)	.002 (.002)
Mean Math (t-1)	-.066 (.044)	-.0046 (.037)	-.015 (.030)
Mover (t-1)	-.0004 (.022)	-.007 (.021)	-.013 (.018)
Reading (t-1)	.120 (.008)***	.195 (.008)***	.137 (.007)***
Math (t-1)	.512 (.013)***	.606 (.010)***	.670 (.007)***
ConDis (t-1)	-.046 (.035)	-.149 (.028)	-.029 (.023)
ConDis (t)	-.018 (.035)	-.003 (.026)	-.033 (.021)
School in-mob (t-1)	-.108(.316)	-.415 (.251)*	-.099 (.178)
% Black (t-1)	.0001 (.001)	-.0004 (.0009)	.0005 (.0007)
% White (t-1)	-.001 (.002)	-.001 (.001)	-.001 (.001)
% LowInc (t-1)	-.003 (.002)**	-.002 (.001)	-.001 (.001)
Size (t-1)	-.00006 (.00004)	.00006 (.00003)**	.00002 (.00003)
Mean ConDis (t-1)	-.032 (.056)	-.022 (.049)	-.070 (.040)*
% LEP	.001 (.002)	.0009 (.001)	.001 (.001)
Female	-.059 (.011)***	.017 (.010)*	.063 (.008)***
DAge (Jan-Sep B)	-.038 (.038)	-.058 (.022)**	-.075 (.018)***
Other (white/asian)	.154 (.027)***	.196 (.026)***	.087 (.019)***
Hispanic	.095 (.022)***	.114 (.021)***	-.088 (.017)***
Repeater	-.048 (.097)	-.158 (.026)***	-.016 (.020)
Grade – 1	-.303 (.105)***	-.154 (.029)***	-.024 (.024)
Grade – 2		-.053 (.071)	-.393 (.048)***
Grade – 3			-.542 (.268)*

The explanatory power of the models is quite large: most of the between-school variance is accounted for by the model and the total variance explained is 56%, 61%, and 69% over the three years, respectively. By far the most important predictors in the model are prior math and reading scores.

Table 3: Variance Explained by ANCOVA Models

Variance component	**1998**	**1999**	**2000**
Unconditional within school, $\hat{\sigma}^2$	0.882	1.014	1.120
Unconditional between school $\hat{\tau}^2$	0.185	0.211	0.223
Unconditional total	1.067	1.225	1.343
Conditional within school $\hat{\sigma}^2_{residual}$	0.393	0.438	0.389
Conditional between school $\hat{\tau}^2_{residual}$	0.076	0.033	0.025
Conditional total	0.469	0.471	0.414
Reduction	**56%**	**61%**	**69%**

5.2 Year-by -Year Estimates via Propensity Score Stratification

As discussed in the section on identification of causal effects, causal inference based on the ANCOVA model requires a linearity assumption in addition to the assumption of strongly ignorable treatment assignment. The linearity assumption can lead to inferences based on unjustified extrapolation when students experiencing particularly high or low levels of school mobility differ on observed covariates from other students. The ANCOVA also can risk overfitting the model for the outcome, though that problem is not so pronounced when sample sizes are large as in our case.

We used stratification on the expected "dosage" or propensity score as a semi-parametric alternative to ANCOVA, separately for 1998, 1999, and 2000. For each year we built a model to predict Z_h, school mobility in year h, as a function of history on covariates, prior treatments, prior outcomes, prior residential, and prior school characteristics, collected in the vector, L_h. Call this predicted dosage of school mobility the propensity score. Next, we stratified cases on the propensity score and, within strata, checked the association between the school mobility and each covariate including the propensity score itself. We stratified more finely when balance was not achieved; typically we found cases at the extremes that could not be balanced. We removed these cases as exemplifying regions of the data failing to achieve common support. We ended up with 16 strata in 1998, 198 strata in 1999, and 21 strata in 2000. We found no evidence against the null hypothesis of homogeneity of association between school mobility and mathematics achievement within strata in any year. We therefore estimated Equation 5 but controlling for prior reading and math achievement to improve precision. Results are summarized in Table 4 (complete output is in Appendix A).

Table 4: Summary of Results of Propensity Score Analysis

	1998	1999	2000
Estimate $\hat{\delta}_h$	-0.86	-0.50	-0.47
Standard error	(0.34)	(0.26)	(0.25)
Within-school variance, $\hat{\sigma}^2$	0.733	0.498	0.418
Between-school variance, $\hat{\tau}^2$	0.058	0.040	0.031
Number of strata	16	18	18

5.2.1. Estimated average effects. As in the case of ANCOVA, the point estimates are all negative, but now all are statistically significant by conventional standards. The biggest discrepancy is in 1998, where the propensity score estimate is noticeably larger than that provided by ANCOVA. The associated standardized coefficients would be in the neighborhood of -.042, -.020, and -.018. The 1998 effects are larger than those we estimated by means of ANCOVA, but, given the size of the standard errors in each case, the general picture looks similar.

5.2.2 Interaction between school mobility and math ability. We had hypothesized more severe negative effects for students of relatively high ability based on the supposition that teachers coping with an influx of new and typically disadvantaged students would be forced to review previously taught material excessively. To test this hypothesis, we classified students within each school at each time as "high" on math achievement if their prior achievement was above the school mean and "low" if it was below the school mean. We then re-ran the propensity score models but adding this indicator by itself and in interaction with school mobility. Power for the test of interaction was quite high yet we found no hint of evidence of an interaction. Under the assumptions of the propensity score model, the results suggest that high and low achievers so defined are similarly negative affected by school mobility.

5.2.3 Interactions with student mobility status. It would be interesting to discern whether high school mobility affects mobile and non-mobile students differently. Moreover, from a methodological standpoint, we might worry that our models are picking up effects of residential or school mobility rather than of school mobility. If so, the effects should exist only for the movers. Unfortunately, our data provide low statistical power to detect interactions between school-level mobility and child mover status. For example, in 2000, 587 students changed residence but not school during the year; 373 changed schools but not residence, and 1437 changed both schools and residence. There were 20,147 students who changed neither residence nor school. The samples sizes of movers are not tiny, but the effect sizes of interest here are small, which limits our power. Nevertheless we did test interactions between school-level mobility and child mobility and found no significant interaction. However, the results of this test were interesting in revealing that the effects of school mobility on stayers were uniformly negative and statistically significant; indeed our causal identification comes mainly from stayers. We conclude that, under our model assumptions, high school in-mobility during the years reduces, by a small amount, the average achievement of stable students.

5.3 Cumulative effects via IPTW

Hanushek, Cain, and Rivkin reported significant negative effects of school-level mobility on mathematics achievement in Texas. Their definition of school level mobility was similar to ours – fractions of students who migrate into a school during the school year. They found year-specific effects of around -.013 standard deviations of the outcome for a standard deviation difference in school mobility. They reasoned that although such effects are small, they would be practically significant if they accumulate over time. We assessed cumulative effects in our data using a marginal mean structural model with inverse probability of treatment weighting (IPTW).

Our estimates based on Equation 8 suggest an average cumulative effect of -0.38, s.e. = 0.20 (Table 5; see Appendix B for full output). Although the confidence interval is quite wide, it is interesting that our point estimate based on the cumulative effects model is quite similar to the average of the year-by-year point estimates reported for ANCOVA and propensity score stratification, providing suggestive evidence that the annual effects described above do accumulate.

Table 5a: Estimates of Regression Coefficients

Fixed Effect	Coefficient	Standard error	*t*-ratio	Approx. *d.f.*	*p*-value
INTERCEPT, θ_0	-1.524899	0.048207	-31.632	8874	<0.001
Annual Growth, θ_1	0.729185	0.025346	28.769	19204	<0.001
School Mobility, ω	-0.381361	0.202167	-1.886	8874	0.059

Table 5b: Estimation of within-child and level-1 variance components:

Variance Component	Estimated Standard Deviation	Estimated Variance
Child-specific intercepts	**0.835**	**0.697**
Child-specific growth rates	**0.216**	**0.047**
Cumulative school effects	**0.120**	**0.014**
Within children within schools	**0.488**	**0.238**

5.4 Fixed Effects Models

5.4.1 Student random effects. We used data from 1997 and 1998 to compute the "prognosis score," the expected "true" achievement score in 1998 given all available data (including the 1998 test score). We then subdivided the sample on 10 deciles of this prognosis score. We then estimated a random effects model for the two final years 1999 and 2000; the model allows the expected annual growth rate and the effect of school mobility to vary by stratum. The results are summarized in Table 6 (full output in Appendix C). The within-stratum point estimates are uniformly negative. We tested the null hypothesis that all 10 effects were null using a multivariate Wald test; we rejected this hypothesis (chi-square=28.16, df=10, p=.002). Next we tested the null hypothesis of homogeneity of effects across strata; we retained this hypothesis (chi-square = 5.39, df=9, p >.500). This gave further support to the inference that the association between school mobility and mathematics achievement does not depend on prior ability. Finally, we tested the null hypothesis that the average effect of school mobility (that is, the effect pooled within the 10 strata) was null; we rejected this null hypothesis (chi-square = 22.85, df=1, p <.001).

Table 6: Estimated effects of school mobility controlling for student random effects, time-varying covariates, age, prognosis stratum, prognosis stratum by age, and time-varying covariates measured on schools and children.

Prognosis Decile	Estimated Effect	Standard Error	t-ratio
1	-0.302	0.328	-0.92
2	-0.519	0.327	-1.59
3	-0.765	0.353	-2.17
4	-0.911	0.359	-2.54
5	-0.338	0.337	-1.00
6	-0.190	0.364	-0.52
7	-0.518	0.357	-1.45
8	-0.788	0.338	-2.33
9	-0.892	0.345	-2.59
10	-0.301	0.360	-0.84

5.4.2. Student fixed effects. We estimated essentially the same model but now removing student-level covariates and including student fixed effects. The within stratum estimates were quite imprecise (Table 7). We tested the null hypothesis that all 10 stratum-specific effects were null, and retained that hypothesis, chi-square =10.77, df=10, p = 0.38. Nor did we find any evidence of heterogeneity of effect. Noting that many of these within-stratum effects had changed sign, we estimated a very simple fixed effects model with no covariates and no prognosis strata in order to minimize collinearity. We found an average effect of $\hat{\omega} = 0.23,$ se=0.16 (see Appendix D for details). This point estimate was nearly identical to the average of the stratum effects displayed in Table 7.

Table 7: Estimated effects of school mobility controlling for student fixed effects, time-varying covariates, age, prognosis stratum, prognosis stratum by age, and time-varying covariates measured on schools and children.

Prognosis Decile	Estimated Effect	Standard Error	*t*-ratio
1	0.722782	0.513258	1.408
2	0.609669	0.462320	1.319
3	-0.267633	0.490547	-0.546
4	-0.767319	0.511009	-1.502
5	0.600982	0.456364	1.317
6	0.486604	0.497063	0.979
7	0.100551	0.567112	0.177
8	0.057760	0.468358	0.123
9	0.342504	0.508170	0.674
10	0.599850	0.492026	1.219

5.4.2 Student and school fixed effects. Models with student and school fixed effects gave similar results to those of the student fixed effects model, with positive but non-significant points estimates for the average effect of school mobility.

6. Discussion

About 11 percent of the students attending a typical Chicago Public School in the spring of 1998 had moved into that school during the year. However, that number varied substantially across schools. Some schools experienced little such mobility while in others, as many as 18 percent of the schools students had moved in during the year. This means it would not have been unusual to locate classrooms in which 4 to 6 of the students had entered the school during the school year. Past research cited earlier suggests such in-migration during the year makes it hard for teachers to sustain coherent instruction at a reasonably fast pace. Moreover, there is evidence that a large influx of new students during the year can undermine the social networks that enable teachers and students to support sustained school-related effort.

An important question, then, is whether and to what extent such within-year mobility undermines learning of all students – including non-movers -- and, relatedly, whether such effects accumulate over time. If the cumulative effects are appreciable, it would be sensible to focus the attention of policy makers on strategies for reducing school mobility or protecting children from its effects.

Assessing the impact of school-level mobility thus defined is challenging, and we adopted several analytic strategies. Year-by-year estimation strategies (analysis of covariance and propensity score stratification) can reduce bias by controlling all observed information about the past while avoiding problems of reciprocal causation. However, such strategies cannot assess the cumulative effects of school mobility. To test for cumulative effects, we adopted the marginal mean model of Robins, Hernan, and Brumback (2000) as extended to longitudinal clustered data by Hong and Raudenbush (2008). This method sequentially controls for past covariates, school mobility, and outcomes by inverse-probability-of-treatment weighting. We also estimated fixed effects models that pool data over time to provide a powerful test of school mobility effects while also removing unobserved time-invariant confounding.

The results of the first three strategies were convergent, but contradicted by the fixed effects estimates. We have assembled these for convenience in Table 8. To interpret these results, recall that our measure of school mobility is a proportion with a mean of about 0.11 and a standard deviation of about 0.05; and our outcome has a standard deviation of 1.07, 1.22, and 1.33 in 1998, 1999, and 2000, respectively. So, for example, our 1999 estimate of the school mobility effect based on propensity score stratification is 0.50; if school mobility increases 10%, we expect a decrease of 0.050 units in math achievement, a little less than 5 percent of a standard deviation.

Table 8: Summary of estimates across years and methods

	1998	1999	2000
Year by year (Ancova)	-0.41 (0.32)	-0.33 (0.25)	-0.65 (0.18)
Year by year (Propensity stratification)	-0.86 (0.34)	-0.50 (0.26)	-0.47 (0.25)
Cumulative (IPTW)	-0.38 (0.20)	-0.38 (0.20)	-0.38 (0.20)
Year by year (Fixed Effects)		0.24 (0.16)	0.24 (0.16)

The point estimate by IPTW of -0.38 is an estimated cumulative effect while the others are effects over a single academic year. The IPTW estimate is a bit smaller than most of the estimates of the annual effects, particularly those for 2000 which appear to have the greatest precision. Interestingly, if we translate this cumulative effect estimate into a standardized regression coefficient, we obtain -0.016, a number remarkably similar to that found in Hanushek, Kain, and Rivkin (2003) who analyzed annual effects using data from Texas.

The failure to find convergent estimates across identification strategies is troubling. There are several possible interpretations. Perhaps the most plausible explanation is that the observed covariates relied upon by the Ancova, propensity score, and IPTW strategies are insufficient to remove time-invariant confounding. If we believe the fixed effects results, we would suspect that students attending schools characterized by high mobility who are observationally similar to students attending low-mobility schools are disadvantaged on unobservables, causing a negative bias in the estimated effects of school-level mobility.

Yet the fixed effects specifications are also open to criticism. The key problem involves time-varying confounding. A failure to include relevant time-varying confounders could cause a bias. For example, families might wish to transfer their children into schools located in improving neighborhoods. If improvement in the local neighborhood increases achievement, the impact of school mobility would appear positive even if it is not. On the other hand, controlling time-varying covariates within the fixed effects model could also cause a problem, as discussed earlier. This concern about time-varying confounding was key to the rationale for inverse-probability of treatment weighting, though that method is vulnerable to a criticism of failing to control unobserved time-invariant confounding.

One solution is to find a source of exogenous variation such as the closing of local schools or the demolition of local housing projects that generate high school-level mobility. While this strategy holds some promise, a caution is that, even by the most optimistic of our estimates, the impact of school mobility is small. This means very large data sets are required to identify it. We shall investigate prospects of using data from Chicago to exploit natural experiments such as school closings with an eye toward whether such analyses would yield large enough sample sizes.

Another option is to adopt identification based on comparing cohorts of students within the same grade and school as in Hanushek et al. (2004). This could be important in estimating year-by-year effects, but probably not in estimating cumulative effects.

Finally, we are now carrying out analysis using finer-grained measures of school-level mobility. The measures in the current study at taken at the overall school level. We have now obtained grade-by-grade measures. These may produce more robust results.

In the face of these methodological challenges, the implications for policy are quite limited. Nevertheless, our results cast doubt on the supposition that the impacts of school-level mobility are large. Even our optimistic estimate of the average cumulative effect is equivalent to a standardized regression coefficient with a magnitude of about -0.016. Practically this means that if a school's mobility rate were to increase from the citywide average of about 11% to 16 %, placing that school above the 80 percentile citywide, student achievement would decline by about 1.6 percent of one standard deviation. Such an effect would be non-negligible if it were to accumulate over time, as our IPTW results suggest, but it would be a modest effect. Let's see what this would mean for African-American students.

African American children are at the highest risk of experiencing large influxes of new classmates during the year. Rates of school mobility rates have fallen more significantly for non-African than for African-American students, so that the current gap has increased to about 5 percent, similar to the standard deviation in our study.

To reduce school mobility a standard deviation for African American students during these years would take it from about 12 percent per year down to about 7 percent – near the current citywide average for non-African American students in Chicago (de la Torre and Gwynne, 2009). If the cumulative effects we estimated held for the entire span of the elementary years, from kindergarten to grade 5 (an admitted extrapolation from an optimistic estimate), closing the "mobility gap" in this way would increase African American test scores by almost one-tenth of a standard deviation. Whether this is a lot or a little depends, of course, on the cost of reducing school mobility for African Americans. And that depends upon what policy options might be available for doing so.

Crafting policy options to reduce school mobility, which is especially salient for African American children, is a complex problem. Reducing housing instability would seem an especially expensive way to reduce school mobility, though such an option may have other important benefits. Encouraging families who move short distances to stay in the same school would seem a lot cheaper, though we don't know how effective such a strategy might be. Improving school quality would likely encourage parents to avoid changing schools, though the cost of improving school quality is a question of seminal importance and a complex one. Finally, policies to ameliorate the impact of school mobility are worth considering. Increasing curricular and instructional conformity across schools would require strong district leadership and would seem to run counter to the current trend of encouraging schools to experiment with novel governance and instruction. So the problem of crafting policies to reduce mobility or ameliorate its effects is complex.

In sum, our results are mixed but suggest that these effects are small. While we purposely decided to begin our work in this area with the careful study of a single cohort of students at the elementary level, we also intend to study the impacts of school-level mobility at the secondary level. Prior research about the negative effects of mobility on social networks may prove more important at the secondary than at the elementary level, and effects on anti-social behavior may be manifest at that level. This research will give a clearer picture of the cumulative effects of school-level mobility over the public school careers of urban students.

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Appendix A: Model Estimates of Annual Effects of School Mobility Based on Propensity Scores

1998

Predictor	Estimated Coefficient	Standard Error	T-ratio
Intercept	−2.173490	0.013826	−157.202
School Mobility	−0.861094	0.335696	−2.565
Reading Pretest	0.148556	0.022016	6.748
Math Pretest	0.444360	0.023128	19.213
Propensity score	−4.922045	2.512674	−1.959
Stratum 2	0.015917	0.047605	0.334
Stratum 3	0.060279	0.056357	1.070
Stratum 4	0.128620	0.064496	1.994
Stratum 5	0.098285	0.075297	1.305
Stratum 6	0.102343	0.081317	1.259
Stratum 7	0.115323	0.092084	1.252
Stratum 8	0.089314	0.102227	0.874
Stratum 9	0.150490	0.112465	1.338
Stratum 10	0.252146	0.121009	2.084
Stratum 11	0.140444	0.129206	1.087
Stratum 12	0.171163	0.139540	1.227
Stratum 13	0.265001	0.154577	1.714
Stratum 14	0.270600	0.172913	1.565
Stratum 15	0.339481	0.189178	1.795
Stratum 16	0.351639	0.227296	1.547

Final estimation of variance components:

Random Effect	Standard Deviation	Variance Component	df	Chi-square	P-value
Between School	0.24107	0.05811	428	2417.27015	0.000
Between children	0.73319	0.53756			

1999

Predictor	Estimated Coefficient	Standard Error	T-ratio
INTRCPT2, G00	-1.538452	0.011826	-130.087
TIN1999, G01	-0.499425	0.263531	-1.895
Reading pretest	0.083237	0.035296	2.358
Math pretest	0.701745	0.029770	23.572
Propensity score	2.889036	4.116305	0.702
Stratum 2	-0.035779	0.035929	-0.996
Stratum 3	-0.133131	0.046573	-2.859
Stratum 4	-0.076766	0.058251	-1.318
Stratum 5	-0.108673	0.068527	-1.586
Stratum 6	-0.162653	0.080002	-2.033
Stratum 7	-0.182538	0.092055	-1.983
Stratum 8	-0.130513	0.105367	-1.239
Stratum 9	-0.173430	0.114521	-1.514
Stratum 10	-0.196589	0.129205	-1.522
Stratum 11	-0.218870	0.139601	-1.568
Stratum 12	-0.230340	0.156506	-1.472
Stratum 13	-0.212849	0.171668	-1.240
Stratum 14	-0.234595	0.192029	-1.222
Stratum 15	-0.256995	0.210839	-1.219
Stratum 16	-0.268386	0.234872	-1.143
Stratum 17	-0.283581	0.277801	-1.021
Stratum 18	-0.359330	0.359005	-1.001

Final estimation of variance components:

Random Effect	Standard Deviation	Variance Component	df	Chi-square	P-value
Between Schools	0.19994	0.03998	426	2169.15094	0.000
Between Childen	0.70593	0.49833			

2000

Predictor	Estimated Coefficient	Standard Error	T-ratio
Intercept	-0.896126	0.010664	-84.036
School Mobility	-0.472317	0.253570	-1.863
Reading pretest	0.169174	0.007294	23.195
Math pretest	0.709197	0.007727	91.776
Propensity score	1.417612	1.301516	1.089
Stratum 2	-0.016226	0.026529	-0.612
Stratum 3	-0.033400	0.030078	-1.110
Stratum 4	-0.050339	0.031381	-1.604
Stratum 5	-0.025634	0.033747	-0.760
Stratum 6	-0.037701	0.035289	-1.068
Stratum 7	-0.026141	0.038845	-0.673
Stratum 8	-0.041637	0.037360	-1.114
Stratum 9	-0.072587	0.042258	-1.718
Stratum 10	-0.052480	0.043167	-1.216
Stratum 11	-0.039430	0.045958	-0.858
Stratum 12	-0.078125	0.049296	-1.585
Stratum 13	-0.115497	0.052109	-2.216
Stratum 14	-0.100777	0.058657	-1.718
Stratum 15	-0.129543	0.061691	-2.100
Stratum 16	-0.129187	0.065360	-1.977
Stratum 17	-0.102998	0.071502	-1.440
Stratum 18	-0.172119	0.080733	-2.132
Stratum 19	-0.166392	0.087088	-1.911
Stratum 20	-0.162893	0.108360	-1.503

Final estimation of variance components:

Random Effect	Standard Deviation	Variance Component	df	Chi-square	P-value
Between Children	0.17513	0.03067	417	1985.45199	0.000
Within Children	0.64693	0.41852			

Appendix B: Model Estimates of Cumulative Effects of School Mobility Based on Inverse Probabilty of Treatment Weighting

Final estimation of fixed effects:

Predictor	Coefficient	Standard error	*t*-ratio
INTERCEPT, θ_0	-1.524899	0.048207	-31.632
Age	0.729185	0.025346	28.769
School Mobility	-0.381361	0.202167	-1.886

Final estimation of child and level-1 variance components:

Random Effect	Standard Deviation	Variance Component	*d.f.*	χ^2	*p*-value
intercept	0.83486	0.69700	15586	130876.49042	<0.001
age	0.21640	0.04683	15586	20416.60680	<0.001
level-1	0.48821	0.23835			

Final estimation of column level variance components:

Random Effect	Standard Deviation	Variance Component	*d.f.*	χ^2	*p*-value
intercept	0.12028	0.01447	400	2181.10453	<0.001

Appendix C: Model Estimates of Annual Effects of School Mobility Based on Random Effects Models with Stratification on the Prognosis Score

Predictor	Coefficient	Standard error	*t*-ratio
Decile 1	-5.669592	0.148865	-38.085
Decile 2	-5.683606	0.146088	-38.905
Decile 3	-5.926712	0.149921	-39.532
Decile 4	-5.486567	0.151446	-36.228
Decile 5	-5.654233	0.154668	-36.557
Decile 6	-5.574752	0.158125	-35.255
Decile 7	-5.232839	0.163006	-32.102
Decile 8	-4.833804	0.161477	-29.935
Decile 9	-4.399306	0.163644	-26.883
Decile 10	-3.587017	0.164532	-21.801
% Black	-0.003334	0.000344	-9.686
Con. Disadv.	-0.077103	0.015515	-4.970
LEP	-0.003528	0.000776	-4.543
LINC	-0.000321	0.000649	-0.495
Mean Con Dis.	-0.013411	0.022775	-0.589
Class size	0.000004	0.000017	0.250
School Mob, Dec. 1	-0.302107	0.328101	-0.921
School Mob, Dec 2	-0.518961	0.327019	-1.587
School Mob, Dec. 3	-0.765359	0.352860	-2.169
School Mob, Dec. 4	-0.911101	0.359180	-2.537
School Mob, Dec. 5	-0.337679	0.336511	-1.003
School Mob, Dec. 6	-0.190211	0.363862	-0.523
School Mob, Dec. 7	-0.518177	0.356508	-1.453
School Mob, Dec. 8	-0.787597	0.338476	-2.327
School Mob, Dec. 9	-0.892275	0.344548	-2.590
School Mob, Dec 10	-0.300846	0.360126	-0.835
Repeater	-0.158796	0.013295	-11.944
Age Dec. 1	0.513352	0.015986	32.113
Age Dec. 2	0.564177	0.015309	36.853
Age Dec. 3	0.628270	0.015655	40.131
Age Dec. 4	0.593267	0.015323	38.718
Age Dec. 5	0.624454	0.015608	40.009
Age Dec. 6	0.630382	0.015924	39.586
Age Dec. 7	0.616166	0.016374	37.630
Age Dec. 8	0.591860	0.016237	36.451
Age Dec. 9	0.571404	0.016347	34.955
Age Dec. 10	0.529789	0.016238	32.626
Mover	-0.087439	0.014490	-6.034
School Size	-0.006782	0.001930	-3.513
Magnet	0.154250	0.045553	3.386
Charter	0.072982	0.060055	1.215

Appendix D: Student Fixed Effects Estimates

Predictor	**Estimate Coefficient**	**Standard error**	*t*-ratio
Intercept	-1.128503	0.007612	-148.256
Student mobility	0.234781	0.157126	1.494
Age	0.715026	0.005537	129.145