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# Disruption versus Tiebout improvement: the costs and benefits of switching schools

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**Abstract**

Most students change schools at some point in their academic careers, but some change very frequently and some schools experience a great deal of turnover. While many argue that mobility harms students, economists tend to emphasize Tiebout type moves to procure better school quality (SQ). This paper disentangles the disruption effects of moves from changes in SQ. Importantly, it identifies the negative externality movers impose on other students. Student turnover is shown to entail a substantial cost for movers and non-movers alike. This cost appears to be larger for lower income and minority students who typically attend much higher turnover schools.
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Switching schools is a common occurrence for children in the United States. In Texas public schools, for example, one-third of all children switch schools at least once between grades 4 and 7 excluding changes due to the transition from elementary to middle school. Common perceptions of the implications of school moves, however, differ dramatically by the underlying perspective. Moves dictated by divorce, job loss, or similar events would be expected to disrupt academic progress, while "Tiebout" mobility, with parents changing districts in pursuit of higher quality schools or better matches for their children, would

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generally be thought of as achievement-enhancing. Frequent school changers, such as children of migrant workers and those who live in economically disadvantaged families, evoke particular concern. The combination of school instability with the pressures of economic disadvantage and limited community roots might be expected to diminish seriously prospects for academic success.

Prior research suggests that mobility is on average harmful to students, though the evidence is mixed and rarely considers differences by type of move. In many studies the relationship is not statistically significant, and some papers report a positive relationship between achievement and mobility (see Alexander et al., 1996). Kerbow (1996) finds that most students recover fully 2 years following a move but that frequent movers lose ground relative to other students. Yet interpretation of these results is complicated by the fact that movers and non-movers appear to differ along a number of dimensions related to school performance. Movers, particularly those who move multiple times, tend to have lower family income, to be Black or Hispanic, and to have lower initial achievement. Thus several recent studies unsurprisingly show that controls for family background and pre-move achievement levels reduce the magnitude and statistical significance of moving effects, often to the point that hypotheses of no mobility effects cannot be rejected[1].

There is even less evidence on the change in school quality (SQ) following a move despite the emphasis on SQ differences in many theoretical models of neighborhood and school choice (e.g. Fernandez and Rogerson, 1997; Epple and Romano, 1998; Nechyba, 2000). One problem is the difficulty of disentangling the effects of switching schools per se from concurrent changes in SQ. Kain and O'Brien (1998) suggest that moves that ultimately benefit students might appear costly if achievement is measured only in the year following the move. A recent study by Cullen et al. (2000) does examine the change in SQ for students who choose to take advantage of open enrollment and opt out of their neighborhood school in Chicago. Except for students attending a trade school, there is little or no evidence that students systematically procure better SQ by choosing to travel to a non-neighborhood school—at least when quality is measured by student outcomes.

Finally, a less discussed aspect of mobility is its effect on other students. High student turnover can disrupt orderly teaching and curriculum development, implying potentially serious externalities from mobility. Because movers attend schools with higher turnover on average, it is important to separate the direct effects of moving from the effects of aggregate school turnover.

This paper investigates the heterogeneous effects of different kinds of moves on students and their schoolmates using the extraordinarily rich data set constructed under the UTD Texas Schools Project. The large number of movers in this data set, which follows several entire cohorts of Texas elementary school students for a number of grades, permits detailed investigation of different types of moves and their implications for specific demographic

[1] Alexander et al. (1996) and Ingersoll et al. (1989) both find that controls for family socio-economic status diminish estimated moving effects, and Alexander et al. find that control for initial academic achievement generally produces estimated moving effects that are not significantly different from zero. Kain and O'Brien (1998), however, find significant negative impacts of all kinds of mobility, even after conditioning on initial achievement.

groups. These data also make possible the identification of mobility effects through comparisons of academic performance before and after a move for the same student, a superior approach to relying solely on comparisons between movers and non-movers.

The analysis undertakes an investigation of several facets of student mobility that have entered into past research and policy discussions including the timing of a move (prior to or during the school year), whether or not a student switches districts, the effects of multiple moves in a year, and the effects of high student turnover in a school. Particular attention is focused on differences by race, ethnicity, and income because of the heterogeneity in mobility propensity, family circumstances, and SQ.

The investigation of mobility effects begins with the development of an empirical model that highlights the difficulty of separating changes in SQ from correlated changes in family circumstances and from normal disruptions following a transfer to a new school. Under reasonable assumptions, however, changes in SQ can be identified from the time pattern of achievement gains following a move. Moreover, classification of moves on the basis of timing, frequency, and distance highlights the types of moves that tend to lead to larger SQ improvements. The results suggest that students switching districts on average procure higher SQ, while those switching schools within district and especially those moving more than once in a school year appear to suffer short run losses and obtain no significant improvements in SQ.

The methods used in this paper permit the identification of the negative externality movers impose on other students—perhaps the most important aspect for policy. The estimation of such peer group externalities has been problematic because of the difficulty controlling for other confounding individual and aggregate school factors. Externality estimates show that aggregate school turnover hurts movers and non-movers alike, and these costs appear to be larger for minority and lower income students.

## 1. A model of mobility effects

Why does a family change schools? Presumably the current school was not selected at random, meaning that something must have changed to prompt a family to relocate. Of course that something may have nothing to do with education given the close link between the choices of school and residence. Rather changes in employment opportunities, family structure, or some other factor may precipitate a move. Tabulations from the NLSY show that movers experience higher rates of job change, divorce, and other changes in family structure than non-movers. Among economically disadvantaged students, for example, the prevalence of a divorce or a job loss by the father was more than 50% greater in the year of a school move than in years of non-moves[2]. The notion that seeking out a different school does not drive most moves is further supported by evidence from the Current Population Survey (CPS). For within county movers, 26% move for family-related reasons, 6% for work-related reasons, and 65% for housing-

[2] We are indebted to Pat Reagan and Qing Liu for constructing the necessary data set and for producing the tabulations from the NLYS.

related reasons of which the two largest categories are wanting a new or better house and wanting to own a house; see Schacter (2001a,b). Interestingly, the CPS does not even ask whether families move to procure better schooling or other public services, though many or even most families with children likely consider the quality of public schools in making their housing choices.

Yet whatever the cause of a move, the change will affect students in a number of ways. They will have to adjust to a new school and possibly a new neighborhood, and the new school may be better or worse than the previous one. Moreover, moves resulting from a change in parental employment, earnings, or family structure may exert a direct effect on academic performance.

The multiple move related changes make it difficult to isolate the contributions of SQ and the other factors to the academic changes experienced by movers. If SQ were easily measured, it would be straightforward to identify its importance. However, a substantial body of research demonstrates the difficulty of inferring SQ from observable school characteristics such as class size, per-student expenditure, or other teacher and school variables. Preferred measures of SQ are based on the value-added of schools to achievement and other student outcomes, but their use requires a mechanism to separate the change in SQ from other move related factors.

We begin with a general model of housing choice that provides a framework for considering the impact of moving. Subsequently we develop a more detailed model of achievement growth that can be used to decompose changes in academic performance into SQ and transactions costs components.

### 1.1. Housing choice

Consider the locational equilibrium of a household that resides at location $d^*$. The household will be in equilibrium at location $d^*$ if:

$$\int_H E[U(X_\tau^{d*},\ \mathrm{SQ}_\tau^{d*},\ O_\tau^{d*}\,|y,\ F,\ p_{d*})]\mathrm{d}\tau > \int_H E[U(X_\tau^{d},\ \mathrm{SQ}_\tau^{d},\ O_\tau^{d}\,|y,\ F,\ p_{d},\ c(d\,|\,d^*))]\mathrm{d}\tau \quad \text{for } d\in\{d\} \tag{1}$$

where, expected utility is accumulated over the relevant planning horizon, $H$, and the current location, $d^*$, is compared with all feasible locations in the choice set $\{d\}$. $X$ is a composite of goods and services, SQ is school quality, and $O$ is other location specific attributes of consumption including housing and other governmental services. Utility is conditioned by income ($y$), family characteristics and job location ($F$), and prices (especially for housing) within the jurisdiction. The expected utility at other locations is also affected by the costs, $c$, that must be incurred when moving from the current location, $d^*$. Largely static variants of this lie behind general theories of urban location decisions, the quality of local public services, and the demand for local government services (cf. Straszheim, 1987; Tiebout, 1956; Wildasin, 1987). In the simplest models a household optimizes Eq. (1) across all of the feasible locations within its choice set, given complete information for all periods.

Yet in reality, predictions of the future often miss the mark, and households may decide to relocate because of changes in expected lifetime income, in family structure (additions of children, divorce or remarriage), in perceptions of the quality of local public services, in the distribution of employment opportunities, or in other factors. Even in the absence of prediction error, rising income may reduce borrowing constraints and expand opportunities. Note further that moving costs introduce inertia into the decisions, so that at any point in time a household might drift away from its current utility maximizing location and might not move until a time when the utility loss from $d^*$ compared with the next best alternative becomes large.

Importantly, across all families that change residence, some will be adjusting to better and more expensive housing, schools, and other attributes of location, while others will be moving in the opposite direction—and there is no presumption that moves are systematically associated with improvements (and added expenditure) in any specific dimension. Yet SQ concerns almost certainly play a central role in the location decisions of many families with young children, even if the move is precipitated by a change in income or employment opportunities. Schools are the largest element of local public expenditure and are widely recognized as being important determinants of future earnings and well-being; families tend to be on the rising portion of life-cycle income, lessening any borrowing constraints; family size and related housing demands are likely to be growing, putting households out of equilibrium; and tax prices to families are independent of the number of children who benefit from the schools.

Because of the complicated decision process involved in housing choice, it is generally infeasible—even with detailed surveys—to investigate directly the underlying motivations of families and how they relate to choices along the dimensions of location, housing stock, and government services. It is, however, possible to analyze the simpler question of whether families with children tend to realize SQ gains following a move.

### *1.2. Theoretical model of mobility effects*

Uncovering SQ outcomes for movers is complicated by the confluence of other factors that are correlated both with moves and with student achievement. Much of the past work on mobility has been correctly criticized for looking just at simple relationships between moving and current achievement, but even the inclusion of a variety of contemporaneous measures of family and school characteristics does not permit identification of the effects of moving. Because achievement depends upon the entire past history of family, community, and school inputs including mobility, the data requirements to model the entire achievement process are generally prohibitive. Our development here focuses on how longitudinal data on achievement and mobility can be used to identify parameters of interest.

Consider a stylized value-added model of achievement growth in which the change in annual learning ($\Delta A$) for student $i$ in year $t$ is a function of both fixed and time varying individual, family, and school factors:

$$\Delta A_{it} = A_{it} - A_{i,t-1} = \mathrm{SQ}_{it} + \gamma_i + \delta_{it} + \varepsilon_{it} \tag{2}$$

where, SQ indicates school quality, $\gamma$ captures all fixed family and individual influences on achievement growth, $\delta$ captures systematic influences that vary over time, and $\varepsilon$ is an idiosyncratic error. We concentrate on annual gains in achievement not only to relate the timing and pattern of moves directly to school outcomes but also to deal with the other well-known estimation problems[3].

Notice that SQ is always defined in terms of gains in student outcomes after controlling for other factors, because quantifiable measures of resources, peers, and other factors tend to explain little of the variation in actual instructional effectiveness. Nonetheless, since parental choices of schools may emphasize such inputs, the subsequent analysis also considers such characteristics.

It is important to recognize that mobility may affect the quality of education in a number of ways. In order to highlight these issues, SQ for student $i$ in school $s$ in year $t$ is decomposed into a common component for all students in the school ($\omega$) and a component ($\theta$) that is a function of mobility status ($m_{it}$):

$$\mathrm{SQ}_{it} = \omega(\tilde{\omega}_s,\ \bar{m}_{st}) + \theta(\bar{m}_{st},\ m_{it}) \tag{3}$$

Though there is considerable heterogeneity in education quality within schools (Rivkin et al., 2001), we think of the component ($\omega$) as an index of overall SQ. The key elements of $\omega$ are ($\tilde{\omega}$), which incorporates the quality of the staff, the available resources, peers, and the curriculum, and ($\bar{m}$), the level of mobility in the school[4]. This breakdown highlights the potential importance of aggregate mobility. More student turnover may harm the overall instructional program as increased time is spent bringing all students to the same point in the curriculum, developing normal procedures, integrating parents into school programs, and so forth.

Eq. (3) also makes explicit the fact that SQ for new entrants may differ from SQ for incumbent students, as denoted by $\theta$ which is a function of both individual and aggregate mobility. Schools, for example, may assign new entrants to worse (better) teachers, and schools likely vary in the time it takes to assimilate new entrants academically (possibly as a function of the aggregate entry rate, $\bar{m}_{st}$).

Mobility also affects achievement independent of SQ. Students must establish themselves in a new community, make new friends, and learn new "operating procedures" at school. This has the character of school specific human capital that is acquired on the task. In our framework, the time varying individual component ($\delta$) captures such mobility effects.

[3] Since the level of achievement at any point will be related to cumulative family and school inputs to the time, value-added models can circumvent problems of omitted or mismeasured past inputs. Some value-added models put lagged test score on the right hand side, which allows its coefficient to differ from one. However, the inclusion on the right hand side of an endogenous variable that is a noisy measure of achievement introduces a number of statistical problems. In any event, preliminary work showed that coefficients on variables of interest were not sensitive to the form of the value-added model. See Rivkin et al. (2001) for the development of a comprehensive model of education production.

[4] Our previous analyses of within school heterogeneity in teacher quality (Rivkin et al., 2001) is entirely consistent with this description of school quality where the fixed quality component ($\tilde{\omega}$) is the stable fixed component incorporating average teacher quality, resources, curriculum, and the like with individual classrooms deviating around this according to fluctuations in teacher quality.

The derivative of Eq. (2) reveals the net effect on school achievement associated with a move in year $t$:

$$\frac{\partial(\Delta A)}{\partial m_{it}} = \underbrace{\frac{\partial \omega}{\partial \tilde{\omega}}\frac{\partial \tilde{\omega}}{\partial m_{it}} + \frac{\partial \omega}{\partial \bar{m}}\frac{\partial \bar{m}}{\partial m_{it}}}_{\text{pure Tiebout}} + \underbrace{\frac{\partial \theta}{\partial \bar{m}}\frac{\partial \bar{m}}{\partial m_{it}} + \frac{\partial \theta}{\partial m_{it}}}_{\text{school assimilation}} + \underbrace{\frac{\partial \delta}{\partial m_{it}}}_{\text{disruption}} \tag{4}$$

The first two terms form the "pure Tiebout" effect, which indicates how a move relates to changes in overall SQ determined by school operations, peers and turnover; the third and fourth terms capture the treatment of new students and how it may vary with aggregate turnover; and the final term captures transition costs that are independent of the level of SQ.

The signs on the respective derivatives determine the change in learning following a move. As emphasized in the general discussion of moving behavior, students switch schools for a variety of reasons, only some of which are related to SQ. Some will experience a severe disruption; for others it will be minor. Some movers will transfer to better schools, others to worse schools. The sign and magnitude of a mobility coefficient estimated over the entire sample will depend upon the relative frequencies of particular changes in SQ, treatment of new entrants, and size of disruptions, while coefficients for specific types of moves and movers capture average changes for those subgroups.

Prior to discussing the estimation and identification of parameters, one final aspect of mobility is important. The previous descriptions were derived from the perspective of the potential mover. In particular, we have portrayed $\partial \bar{m}/\partial m_{it}$ as the difference in aggregate mobility rates between the sending and receiving schools. However, the addition of another mover directly increases $\bar{m}_{st}$, which in turn potentially affects SQ for all students and the ease of assimilation for other movers. This effect, presumably ignored by families in making their mobility decisions, involves a fundamental externality that we explicitly consider in the estimation and evaluation below.

### *1.3. Regression model*

The task of identifying the pathways through which mobility affects learning is quite difficult, because of the nonrandomness of moving. The active decision process from Eq. (1) underscores the fact that moves can be precipitated by a number of factors. For example, a change in family circumstances may both cause a school change and affect achievement directly ($\delta$). Consequently, it is quite easy to confound any change in SQ or the transactions cost of moving with the effects of divorce, job loss, or other factors that precipitate a move.

Our approach takes advantage of the availability of multiple test score observations per-student to isolate the change in SQ. Consider regression Eq. (5) in which achievement growth is regressed on an indicator variable ($m_{it}$) for whether the student moved at the

beginning of or during year $t$, an individual fixed effect ($\gamma_i$*), time varying individual factors ($x_{it}$), and a random error[5].

$$\Delta A_{it} = m_{it}\lambda + x_{it}\beta + \gamma_i{}^* + v_{it} \tag{5}$$

This generalized fixed effect model can be estimated with achievement growth data for two school years, $t$ and $t-1$, requiring 3 years of test information. Note that the removal of a student fixed effect means that the mobility effect is identified by changes over time in achievement gains for movers rather than cross-sectional differences between movers and non-movers. While this formulation is a noticeable improvement over many of the existing approaches that use a limited set of observable characteristics to control for differences between movers and non-movers, it still suffers from two major problems. First, unless confounding influences that precipitate moves are fully accounted for, the estimate of $\lambda$ will confound family shocks with the actual effects of moving. Second, as delineated in Eq. (4), $\lambda$ incorporates both the changes in SQ and transition cost of moving. Absent information about the separate components, both the interpretation and the relevance for policy purposes of direct estimation of $\lambda$ will be quite limited.

In order to relate moves to changes in SQ, more structure must be imposed on the model. Eq. (6) adds an additional mobility term ($m_{it}^*$) that indicates a school change sometime prior to the summer before the current school year. Specifically, $m_{it}^* = 1$ if $s_t = s_{t-1}$ and either $s_{t-1} \neq s_{t-2}$ or $m_{it-1}^* = 1$; otherwise $m_{it}^* = 0$:

$$\Delta A_{it} = m_{it}\lambda + m_{it}^*\lambda^* + x_{it}\beta + \gamma_i^* + v_{it} \tag{6}$$

The estimate of $\lambda$* captures the average difference in learning between school years following the year of a move and the year prior to the move[6].

What factors contribute to this longer run difference? In terms of Eq. (4), we argue that only the two "pure Tiebout" factors associated with differences in SQ systematically determine the size of $\lambda$*. This argument assumes that all assimilation and disruption costs including the acquisition of school specific human capital occur in the first school year (i.e. $E[\partial\delta/\partial m^*] = 0$) and that movers are treated as incumbent students after the first year (i.e. $E[\partial\theta/\partial m^*] = 0$). What remains is the persistent difference in SQ. It is natural in this framework to consider $\lambda$ to be the gross temporary effect on the rate of learning and $\lambda$* to be the steady state change in the rate of learning following a move.

This identification of the average change in SQ rests on three primary assumptions—which we subsequently test—related to the achievement patterns of non-move factors:

(A1) Students do not on average experience temporary losses in the year prior to the move. Because SQ is estimated by the rate of growth of achievement after a move compared with that before a move, a temporary decline in the rate of achievement

[5] Empirically, $m_{it} = 1$ if the student's school differs between test taking at $t$ and $t-1$ (i.e. $s_t \neq s_{t-1}$); $m_{it} = 0$ otherwise.

[6] Note that when $m^* = 1$, $m = 0$ and visa versa.

growth prior to the move would bias upward the estimated change in SQ[7]. We can directly investigate the existence of such temporary declines by including dummy a variable that identifies changes in achievement growth in the year prior to a move.

(A2) In the absence of any change in SQ, students tend to recover to pre-move achievement growth rates in the years following the year of the move. An alternative model, albeit one inconsistent with the underlying conceptual framework, is that students tend to return to "equilibrium" achievement levels, i.e. to the achievement that they would have had in the absence of any disruption. In this latter case, $\hat{\lambda}^*$ would confound the average change in SQ with the recovery from the disruption associated with the move.

(A3) Extraneous disruptions that directly affect achievement and that accompany a move do not have effects that persist past the academic year of a move. The student fixed effects capture any changes that persist throughout the entire sample period, but changes that accompany a move, persist in the subsequent years, and affect achievement directly would lead to biased estimation of the average change in SQ. While we believe that such shocks are probably uncommon, the exact nature of family inputs has received little attention. A variety of studies suggest that the important elements involve long run, non-pecuniary factors such as preferences toward education and family stability, though very severe income shocks almost certainly affect time allocation and achievement even in middle school[8]. The rarity of such dramatic changes suggests that the typical family relocation to take advantage of a superior job opportunity, while expanding economic capacity and resources available to purchase better SQ, has little direct effect on academic achievement[9]. Nonetheless, without better information about changes in family circumstances and their effects on achievement, this assumption remains open to further investigation.

A sensitivity test that separates $m^*$ into an indicator for a move 1 year prior and an indicator for a move 2 years prior provides direct evidence on assumptions 2 and 3. If students were to recover to the achievement level they would have had in the absence of disruption, average gains in the year following the move should be much larger than those in the second year following the move. Average gains in the year following the move would also be larger if positive shocks persisted into the subsequent year but diminished over time (and the opposite in the presence of persistent but dampening negative shocks). These patterns contrast with our assumed case where students recover to their original achievement growth path augmented by any change in SQ and where shocks do not persist

[7] This is exactly analogous to problems faced in the job training literature where a pre-program earnings dip may precipitate entry into training. Heckman and Smith (1999) discuss the implications of a pre-program decline for the estimation of job training program effects.

[8] See Behrman et al. (1980) for a discussion of research on the relationship between achievement and family background. Both Mare (1980) and Stinchcombe (1969) argue that exposure to cognitive stimuli and parental attitudes toward schooling are more important determinants of academic performance than income.

[9] Mayer (1997) discusses income effects. Hanushek (1992) finds that larger family size has a negative influence on achievement, other things being equal, but finds little indication of any influence of short run income changes after allowing for permanent income.

beyond the year of the move. For this, there would be little or no difference in average gains 1 and 2 years after the move. Note, nonetheless, that this test has no power against persistent shocks that begin in the period of the move and remain constant thereafter.

In order to examine further the validity of the estimates of $\lambda^*$ and to learn more about any systematic variation by move characteristics, we compute separate estimates by origin and destination community type. Specifically, we divide district switchers into four categories: urban to urban and suburban to urban (which we expect are less likely to produce average SQ improvements), and urban to suburban and suburban to suburban (which we expect are more likely to produce SQ improvements)[10].

It is crucial to recognize that specific values of $\hat{\lambda}^*$ are estimates of the average change in SQ for a subset of movers rather than some kind of pure treatment effect[11]. For simplicity, assume that there are two types of families: those motivated to move primarily because of nonschool factors and those motivated primarily by Tiebout SQ concerns. For the former group, the lower weight placed on school issues could lead to less investment in information about SQ and generally less systematic outcomes in terms of schools. For the latter, the opposite would hold.

In the absence of survey data on reasons for moving, families cannot be divided easily into mobility categories. However, we expect that school considerations on average receive more weight for district switchers, particularly those moving within a metropolitan area, than for either those moving within districts or those moving to a different metropolitan area. Within district movers face many unchanged school factors such as a common administration with set hiring policies, curricular choices, and operating rules, while moves between regions are most likely driven by job changes where typically the movers lack detailed information about schools[12]. We also divide families by race, ethnicity, and income. To the extent that these factors are correlated with move motivations, the cost of SQ, or family changes that affect SQ, the weighted averages of changes in SQ will vary according to the differing proportions in the population. Thus, common assertions about mobility can be tested in terms of the weighted average outcomes for alternative groupings of movers.

Once an estimate of the change in SQ is obtained from $\lambda^*$, it can be used to estimate the magnitude of the transitory disruption cost $\lambda - \lambda^*$. However, it is very difficult to separate this difference into the underlying factors described in Eq. (4). The components involve both adjustments in school and those unrelated to school. Even data on timing of events such as divorce or job loss—which we do not have—would present problems, because the events per se might not adequately indicate the time periods of any disruption. Therefore, we emphasize the general size of total disruption effects rather than the precise magnitudes or the attribution to different causes.

[10] Note that Texas districts are not coterminous with political jurisdictions, which have sometimes expanded greatly through annexation.

[11] The weighted average nature of the mobility coefficients helps to reconcile some of the previous variation in results across studies. Most of the prior studies rely on nonrepresentative samples and restrict attention to specific subsets of moves, such as moves within a single large district.

[12] Black (1999) and Weimer and Wolkoff (2001) do show, however, that within district school quality differences appear to be capitalized into house prices.

To this point we have focused on the identification of the effects of moving for students who switch schools, but it is also possible to identify the impact of average turnover on learning for movers and non-movers alike. Specifically, using the entire sample of students, we can estimate the impact of $\bar{m}_{st}$ on SQ[13]. Because turnover is undoubtedly correlated with unmeasured determinants of SQ, simply including $\bar{m}_{st}$ into Eq. (6) as an additional regressor is unlikely to generate a consistent estimate of the externalities. Therefore, we employ an alternative approach that adds school-by-grade and school-by-year fixed effects and teacher and school variables to Eq. (5), thereby identifying the effect of average turnover from within school differences in outcomes for different cohorts in different grades. A natural extension, concentrating on the timing of moves, divides entrants into those who arrive prior to the start of the school year and those who arrive during the year in order to see if midyear arrivals are more disruptive.

## 2. The UTD Texas schools microdata panel

The cornerstone of this research is the analysis of a unique microdata set of school operations constructed by the UTD Texas Schools Project, a project conceived of and directed by John Kain. The database tracks students as they progress through elementary and middle school; it measures student performance each spring; and it contains detailed information about schools and teachers. This analysis follows three consecutive cohorts for three academic years each. The oldest cohort attended 5th grade in 1994 and is followed through 7th grade in 1996. The next cohort is also followed from 5th to 7th grades, while the youngest cohort is followed from 4th to 6th grade. For each cohort there are over 200 000 students in over 3000 public schools. The large numbers of students who change schools and school districts are especially important for the methodology pursued here, as are the multiple cohorts which permit tracking of students who fall as far back as two grade levels behind their 4th grade classmates or who move ahead as many as two grade levels.

Beginning in 1993, the Texas Assessment of Academic Skills (TAAS) was administered each spring to eligible students enrolled in grades 3 through 8. Unique identifiers link the student records with the test data[14]. The criteria referenced tests evaluate student mastery of grade-specific subject matter and provide our measure of student outcomes. We use mathematics test scores in this paper, although the results are qualitatively quite similar for reading achievement. Each math test contains approximately 50 questions.

[13] As discussed previously, non-movers contribute nothing to estimation of the fixed school quality term in Eq. (5), since SQ is included in their individual fixed effect, $\gamma_i$*. Because $\bar{m}_{st}$ varies across time and cohorts, however, non-movers can be used to estimate the effect of student turnover on school quality.

[14] One important data consideration is the possibility that schools miscode student identifiers, which would tend to depress the number of movers within the public schools and overstate the percentage who exit Texas public schools. While there is no sure check for coding errors, the evidence suggests that other types of coding problems are quite minimal. Less than 1% of observations in 4th grade and less than one half of 1% of observations in 5th through 7th grades did not have unique identifiers in each cohort; note that a small number of duplicate records were deleted.

Because the number of questions and average percent right varies across time and grades, we transform all test results into standardized scores with a mean of zero and variance equal to one and included dummy variables for each grade-year combination. The regression results are robust to a number of transformations including the raw percentage correct.

The TAAS data are merged with attendance and teacher data. School attendance data provide information on school attended for each of six 6-week periods during the school year, enabling us to identify the approximate timing of all school switches and those students who move multiple times during an academic year. The teacher data provide information on average class size, teacher turnover, and teacher experience that is used as controls in the analysis of student turnover.

While the data contain a limited number of student, family, and program characteristics such as race, ethnicity, gender and eligibility for a free or reduced price lunch, the panel feature can be exploited to account implicitly for time invariant individual effects on achievement. Importantly, students who switch schools can be followed as long as they remain in a Texas public school.

School transitions ($m_{it}$) are constructed to exclude those that result from the structure of school districts[15]. In other words, transitions from elementary to middle school for students who remain in the same attendance zones are not considered moves, and dummy variables for highest and lowest grade in a campus capture the effects of such transitions. A dummy variable indicating a newly opened school is also included in all specifications.

## 3. Student mobility in Texas

Texas schools mirror those in other parts of the nation: switching schools is a regular part of academic life for many elementary school students, even when structural moves from elementary to middle school or to a newly constructed school are eliminated (as we do throughout)[16]. During the 3-year time frame that we follow students, roughly one third of those who remain in Texas public schools throughout the period move at least once. But move frequencies vary dramatically by ethnicity and income. Only 25% of students never eligible for a subsidized lunch move even once, and only 5% move multiple times[17]. In contrast, 40% of students ever eligible for a subsidized lunch move at least once, 10% move twice, and 7% move three or more times.

[15] Because information on change of residence is not available, a school change was considered a structural change if the transition was to a school attended by more than 30% of previous classmates. Such structural transitions combine transitions between middle school and junior high with changes in attendance zones including the opening of new schools.

[16] Summaries here rely on the more detailed descriptive statistics on mobility rates and patterns found in Hanushek et al. (2001).

[17] We define "never eligible" and "ever eligible" in terms of observed eligibility for any one of the years for which we have an individual student's data.

Table 1
Average annual probabilities of switching schools between grades 4 and 7, by type of move, income and ethnicity

| | Ever low income | | Ethnicity | | | All |
|---|---|---|---|---|---|---|
| | Yes | No | Black | Hispanic | White | |
| All school switches (%) | 26.4 | 17.6 | 30.2 | 24.0 | 19.6 | 22.8 |
| *By timing* | | | | | | |
| Prior to school year (%) | 14.9 | 12.4 | 18.4 | 14.1 | 12.2 | 13.9 |
| During school year (%) | 11.5 | 5.1 | 11.8 | 9.8 | 7.4 | 8.9 |
| Multiple moves[a] (%) | 4.4 | 1.2 | 4.8 | 3.4 | 2.4 | 3.1 |
| *By school location*[b] | | | | | | |
| Same district (%) | 10.7 | 5.3 | 14.7 | 9.7 | 5.7 | 8.4 |
| New district, same region (%) | 11.9 | 10.2 | 12.2 | 11.4 | 10.6 | 11.2 |
| New region (%) | 1.9 | 1.6 | 1.4 | 1.3 | 2.3 | 1.8 |
| Observations | 1 518 233 | 1 082 179 | 362 350 | 922 274 | 1 250 716 | 2 601 255 |

[a] Students moving multiple times during a year are a subset of all students moving during the school year.
[b] The distribution by school location does not sum to the total moves because multiple movers who go to more than one location are excluded from the distribution by location.

Table 1 provides additional detail on the timing and patterns of annual moves by income and ethnicity. On average 23% of all students switch to another Texas public school, and the income and black/white gaps in transition rates are approximately 10% points or 50%[18]. A surprising number of students (9%) move within the school year; of these, over one third move at least twice over the year. As with overall moves, blacks and lower income students are much more likely to switch schools during the academic year.

In terms of move destination, 8.4% of students transfer each year within the same district, 11.2% switch districts while remaining in the same geographic region, and 1.8% move to a school in a different region of Texas. (A final 1.4% of students engage in multiple moves that do not fall into a single destination category). The most pronounced differences by income and ethnicity occur in the probability of a within district switch: Blacks are almost three times as likely as whites (14.7–5.7%) to change schools within district and Hispanics are almost twice as likely as whites. Similarly, lower income students are roughly twice as likely to change schools within districts as higher income students. These income and ethnicity differences stem at least in part from the greater concentration of lower income and minority students in large urban districts.

These data reveal a great deal about school transition rates along a number of dimensions. Even so, we lack detailed information on family circumstances, so we cannot adequately distinguish between cases of "distress" (such as job loss or divorce), Tiebout moves in search of better schools, and the majority of moves that result from employment changes or other factors. But the national data make it clear that moving, whether within or across districts, is frequently accompanied by a variety of family changes.

[18] Roughly 7% of students exit the Texas public school system each year.

## 4. Effects of moving on mathematics achievement

The theoretical discussion highlights the problems in past research, where coefficients conflate the change in SQ, transactions cost of moving, and other accompanying changes and where inadequate data compound the estimation biases. This section demonstrates the importance of undertaking a more thorough treatment of the heterogeneous aspects of moving in order to isolate the change in SQ from other costs of moving.

Table 2 reports common cross-sectional estimates based on the level of student performance (column 1), simple annual gain models that do not account for the time pattern of moving related achievement gains (column 2), and our full panel estimates that distinguish immediate achievement changes from the subsequent growth after adjustment to a move (column 3). All specifications in this table and those below include indicators for the lowest grade in a campus, the final grade in a campus, a new school, subsidized lunch eligibility, and year-by-grade (to allow for different tests in the various years)[19]. Separate estimates (not shown) have been computed by ethnicity and income in order to investigate differences by demographic group, and the results are discussed in the text below.

In the simplest version found in panel A, moves of all types are collapsed together, and we obtain average effects across all move experiences. Not surprisingly given the observed transition patterns, the estimate in the first column shows that movers have significantly lower test scores than non-movers. On average, movers have math scores that are lower than those of non-movers by 0.17 standard deviations, matching some of the large estimates reported in previous analyses. But movers are not a random sample of all students, and this cannot be interpreted as the causal effect of moving. Simply controlling for their lower achievement before the move with the value-added specification in column 2 dramatically lowers the estimate of average moving costs to 0.027 standard deviations.

Of course, neither the level nor value-added estimates in columns 1 and 2 distinguish between the immediate disruption of a move and the longer-term ramifications related to any change in SQ. This requires the estimation of the full model described in Eq. (6), in which the coefficient for long run move effects ($\lambda^*$) captures the change in SQ for movers (the average "Tiebout effect"), while subtracting this coefficient from the coefficient on current mobility ($\lambda$) provides an estimate of the combined effects of the loss of school specific capital, other costs of disruption, school efforts to assimilate new entrants, and any coincidental influences of changes in family income or structure that may be associated with the move. (As noted, we are unable to disentangle the specific underlying factors). Column 3 reports estimates of both $\lambda$ and $\lambda^*$. The hypothesis that the average change in SQ is 0 cannot be rejected at any conventional level, while the average transactions cost is roughly $-0.01$ standard deviations and significantly different from 0[20].

[19] All standard errors (here and subsequently) are adjusted for the grouping of cohorts of students in schools, and the sample is restricted to students with valid test scores for four consecutive grades. Since the empirical analysis focuses on achievement growth differences across years, the four tests yield three observations for the students of each cohort. While students who miss one or more of the tests may differ from those sampled with complete testing, the inclusion of individual fixed effects in the preferred empirical analysis circumvents the most serious of these problems in ways that common cross-sectional analyses cannot.

[20] The average transition cost for lower income and minority students is somewhat larger, though still quite small in comparison to race and income gaps in achievement levels that tend to exceed 0.5 standard deviations.

Table 2
Estimated effects of moving on mathematics achievement test score gain during grades 4, 5, 6, and 7, by type and timing of move (absolute value of Huber–White adjusted $t$ statistics in parentheses)

| | | Score in highest grade | Annual gain | Annual gain with student fixed effects | Number of moves |
|---|---|---|---|---|---|
| *A. All moves combined* | | | | | 187 998 |
| | $\lambda$ | – 0.170 | – 0.027 | – 0.014 | |
| | | (24.53) | (10.35) | (3.09) | |
| | $\lambda^*$ | | | 0.006 | |
| | | | | (1.05) | |
| *B. Moves by timing and district pattern* | | | | | |
| Same region, new district | $\lambda$ | – 0.095 | – 0.005 | 0.009 | 46 616 |
| | | (10.02) | (1.25) | (1.42) | |
| | $\lambda^*$ | | | 0.025 | |
| | | | | (3.10) | |
| Same region, same district | $\lambda$ | – 0.088 | – 0.035 | – 0.024 | 99 580 |
| | | (7.61) | (8.63) | (3.87) | |
| | $\lambda^*$ | | | – 0.004 | |
| | | | | (0.33) | |
| New region, new district | $\lambda$ | 0.031 | 0.013 | 0.0106 | 20 601 |
| | | (2.83) | (2.64) | (1.21) | |
| | $\lambda^*$ | | | – 0.010 | |
| | | | | (0.90) | |
| Move during academic year | $\lambda$ | – 0.248 | – 0.013 | – 0.005 | 38 618 |
| | | (21.44) | (2.28) | (0.75) | |
| | $\lambda^*$ | | | 0.004 | |
| | | | | (0.45) | |
| Multiple moves in academic year | $\lambda$ | – 0.422 | – 0.053 | – 0.028 | 21 201 |
| | | (34.35) | (10.31) | (3.24) | |
| | $\lambda^*$ | | | 0.024 | |
| | | | | (1.92) | |
| Observations | | 493 777 | 1 482 141 | 1 482 141 | |

Note: the top and bottom panels represent separate regressions. All specifications include indicator variables for highest and lowest grades in a campus, new schools, student eligibility for a subsidized lunch, and grade-by-year. The first two columns are based on Eq. (5), and the third column is based on Eq. (6).

The top panel combines all moves together, but it is likely that SQ changes and moving costs vary systematically by type of move. In order to examine this possibility, the bottom panel of Table 2 divides moves on the basis of timing and location. Students who move a single time in a year are divided among district switchers who change regions, district switchers who remain in the same region, and those who move to a new school but remain in the same district. An additional variable indicates a move during the school year[21]. Finally, those who switch multiple times in a year are identified (regardless of school location). The preferred specification that includes student fixed effects and that separates achievement growth in the year of a move from that in subsequent years is found in column 3.

[21] Preliminary work that fully interacted the timing and location variables showed that the difference between pre-year and within-year move effects was very similar for both district switchers and those who moved within district. Subsequently we restricted the effects of within-year moves to be the same for all students

The estimates reveal significant differences by move timing and destination in both the average change in SQ and move associated transaction costs. For SQ ($\lambda$*), moving to a new district within region increases SQ by a statistically significant 0.025 standard deviations on average, while for all other categories of movers the estimated changes in SQ are small and not significant. Separate regressions by income and ethnicity (not shown) reveal little variation by income or ethnicity with the exception of blacks, for whom the average SQ quality change—even for moves across districts—is quite small and not significant. Importantly, because these estimates are computed over students whose families moved for a variety of reasons (including divorce), only some of which are associated with a search for better schooling, the magnitude (0.025) likely provides a lower bound estimate of the average improvement in SQ for 'Tiebout' movers who switch districts primarily to procure higher quality schooling.

Given that SQ considerations probably play a minor role in most decisions to relocate to a different region and that knowledge of schools in the new region may be quite limited, it is not surprising that average SQ remains largely unchanged following such a move. It is also not surprising that within district movers experience no systematic gain in SQ. This result is consistent with Cullen et al. (2000) and likely reflects the fact that all schools within a district share a common central administration, financing system, and other factors. Moreover, in comparison to district switches, within district moves are less costly for the family and may be undertaken with lower expected benefits.

The bottom panel results also show that within district movers experience larger disruptions on average than do students who change districts. Estimates of the transaction cost, obtained by subtracting the coefficient on $\lambda$* from the coefficient on $\lambda$, equal $-0.016$ for district switchers within region, 0 for region switchers, and $-0.24$ for students who switch schools within district. As expected, multiple movers experience the largest average adjustment costs ($-0.052$), though the estimates suggest that they also realize an average gain in SQ roughly comparable to that procured by district switchers. Surprisingly, the hypothesis that the timing of the move (i.e. moving during the school year) has no effect on the cost of moving cannot be rejected at any conventional level.

Transactions costs also vary by income and ethnicity (estimates not reported). Lower income students, particularly those who move multiple times and those who move within district, incur larger losses in the year of a move. This does not necessarily imply that transition costs of moving are higher for lower income students, because lower income students may experience a higher incidence of negative move-correlated shocks such as divorce. In terms of race and ethnicity, blacks moving within districts incur somewhat larger transition costs than Hispanics who in turn incur somewhat larger costs than whites.

### *4.1. Sensitivity tests*

The interpretation of the long run difference in achievement growth as the change in SQ depends, as noted above, on testable assumptions about nonmove impacts on achievement patterns. Two specifications are used to examine whether unobserved changes over time in family circumstances or student recovery from the disruption of moving contaminate the estimates. The first includes a full set of leads and lags for each type of move, and the second divides moves between districts by origin and destination community types. We

Table 3
Student fixed effect estimates of the change in achievement gains prior to and following a move (absolute value of Huber–White adjusted $t$ statistics in parentheses)

| | Same region: | | New region |
|---|---|---|---|
| | New district | Same district | |
| $\lambda_{-1}$ | − 0.005 | − 0.006 | − 0.020 |
| | (0.52) | (0.74) | (1.73) |
| $\lambda$ | 0.003 | − 0.032 | − 0.004 |
| | (0.35) | (3.67) | (0.34) |
| $\lambda_1^*$ | 0.019 | − 0.008 | − 0.025 |
| | (1.86) | (0.77) | (1.92) |
| $\lambda_2^*$ | 0.017 | − 0.020 | − 0.020 |
| | (1.33) | (1.52) | (1.20) |

Note: the $\lambda$'s in the table are estimated coefficients for a series of time specific indicator variables. $m_{-1}=1$ in year prior to move; $=0$ otherwise. $m=1$ in year of move; $=0$ otherwise. $m_1^*=1$ in year after move; $=0$ otherwise. $m_2^*=1$ in second year after move; $=0$ otherwise. All specifications are estimated with student fixed effects and include indicator variables for highest and lowest grades in a campus, new schools, student eligibility for a subsidized lunch, and grade-by-year.

also present evidence on changes in observable school characteristics, both because it is the more conventional approach to the measurement of SQ and because parents likely use such information when choosing a school.

Table 3 reports estimates of the average changes in achievement gains in the year prior to the move, the year of the move, the year following the move and the subsequent year by type of move[22]. This further disaggregation of move timing produces less precisely estimated parameters; nonetheless, the results are suggestive. Perhaps most important, there is little or no evidence either of a temporary dip in achievement growth prior to the move or of positive or negative shocks that persist beyond the year of the move for those switching districts within a region. The coefficient for achievement growth differences on the year before the move is very small and insignificant, and the coefficients for achievement effects 1 and 2 years after the move ($\lambda_1^*$ and $\lambda_2^*$) are also virtually identical. While these results do not rule out the possibility that constant, persistent shocks accompanying a move confound the estimated change in SQ, we believe that improvement in average SQ provides a more plausible explanation for the steady long-term average increase in achievement growth.

The tests for the other types of moves, however, are not as clear. For within district moves, the point estimates suggest the possibility of a pre-move dip. For those moving to a new region, there is a suggestion that the year prior to the move is associated with a drop in achievement growth (perhaps from divorces or job losses that play out in a subsequent move to a new area). Interestingly, in both cases, these would suggest that estimates of SQ changes ($\lambda^*$) are biased upward, supporting the finding of no changes in average SQ except for district switches within region.

The division of between district moves on the basis of origin and destination community types provides additional support for the finding of a positive average change

[22] Note that we do not have any achievement information 3 or more years following a move.

Table 4
Student fixed effect estimates of the change in achievement gains for between district moves by origin and destination community type (absolute value of Huber–White adjusted t statistics in parentheses)

| | From central city to: | | From suburban/rural to: | |
|---|---|---|---|---|
| | Central city | Suburban/rural | Central city | Suburban/rural |
| $\lambda$ | − 0.061 | − 0.007 | 0.007 | 0.021 |
| | (2.03) | (0.51) | (0.44) | (2.63) |
| $\lambda^*$ | − 0.041 | 0.030 | − 0.023 | 0.033 |
| | (0.94) | (1.86) | (1.00) | (3.20) |

Note: note that Texas districts are not coterminous with political jurisdictions, and there is more than one urban district in a number of metropolitan areas. All specifications are estimated with student fixed effects and include indicator variables for highest and lowest grades in a campus, new schools, student eligibility for a subsidized lunch, and grade-by-year.

in SQ. A priori one would expect average SQ to increase more for students moving outside of urban districts than for those moving to an urban district. Table 4 finds this precise pattern. The average gain in SQ exceeds 0.03 standard deviations for students moving to non-urban districts, while it is negative for students moving to urban districts. Interestingly, students who move from one suburban district to another achieve virtually the same average gain as those moving from a big city to its suburb.

Though the two sensitivity tests provide little or no reason to reject the SQ interpretation of the mobility estimates, it would be surprising if changes in teacher and peer characteristics observed by families were not at least somewhat consistent with the estimates based on academic achievement gains. Appendix A Table A.1 reports changes in teacher turnover rates, percent black, and average mathematics achievement by mobility category. The table shows that district switchers within region experience slightly larger decreases in teacher turnover and percent black and slightly larger increases in peer mathematics achievement than others, though the differences are small. In addition, the bottom panel of the table shows that among district switchers, those moving from urban to suburban schools experience the largest changes in observed school characteristics of the type typically associated with higher quality schools. On the other hand, the observed change in characteristics for suburban to suburban movers does not match common views on what constitutes an improvement in SQ (even though Table 4 demonstrates that the achievement gains of suburban to suburban movers equal those of urban to suburban movers). This suggests that "Tiebout movers" tend to have more sophisticated ways of judging SQ than contained in the simple descriptive statistics.

## 5. Mobility externalities

Even if the transition cost of moving is small, student turnover may adversely affect SQ because of the induced differences in skills, institutional knowledge, or curricular background accompanying the infusion of students with heterogeneous backgrounds. This disruption may be a particular problem for high turnover schools in which there are substantial variations in academic preparation and large numbers of students entering

during the school year. The possibility that turnover affects non-movers as well as movers is raised by many including Alexander et al. (1996) and Kerbow (1996), though neither study attempts to estimate the turnover externality.

Identification of the turnover externality is perhaps even more difficult than identification of the direct mobility effect, because sorting on the basis of school turnover rates is almost certainly more systematic than the differences between movers and non-movers. In fact, investigations of any type of peer group or external effect share such a concern.

We examine the external effects of mobility using a model that removes student as well as school-by-grade and school-by-year fixed effects and uses differences across cohorts in the change in turnover rates from one grade to the next to identify the effects of turnover. Consider a stylized school that offers both the 5th and 6th grade. Assume 20% of this year's 6th grade class are new to the school, while only 15% of the students were new last year when the students were in 5th grade. In comparison, last year's 6th grade class had only 15% new students in 6th grade but 23% new students in 5th grade. It is the within school difference in the change in percent new students ($20-15\%$ for the first cohort, $15-23\%$ for the second cohort) that we use to identify the external effects of turnover when we include school-by-grade fixed effects. By tracking such changes across the three cohorts in our analysis, we can also remove school-by-year fixed effects.

Controlling for individual fixed effects in gains and mobility, we believe that these cross cohort differences are orthogonal to other factors that systematically affect achievement gains.

Even systematic changes in school turnover as students age will not contaminate the estimates. For example, consider a school in a high poverty area in which mobility increases with age while at the same time relative performance declines. Controlling for only student and school fixed effects will leave the estimates susceptible to contamination by those factors that evolve over time and affect both mobility and achievement, such as a greater difficulty attracting teachers as students age. However, the removal of school-by-grade fixed effects eliminates such systematic changes. In addition, removal of school-by-year effects eliminates changes in school and district leadership, neighborhood environment, and other time-varying school factors that might be correlated with mobility within our sample (whether causally or not).

Importantly, some confounding school factors may not be eliminated by the removal of the fixed effects. One is average class size. If additional entrants tend to lead to increases in average class size, the negative effects of turnover may be confounded with the negative effects of larger average class sizes. A second is peer group quality. If new entrants have lower achievement and thus tend to reduce peer average quality, the omission of information on peer quality may bias upward the estimated effect of turnover per se. Finally, teacher turnover and inexperience may be positively correlated with student turnover, and their exclusion may also bias upward the estimated effect of turnover. There are some systematic changes in these factors, and we consider each in the analysis below.

Within each alternative specification, Table 5 reports two sets of turnover coefficients. The first groups all new students into a single category, while the second separates students who enter prior to the start of the school year from those who enter during the

Table 5
Estimated effects of proportion of students who are new to the class on achievement gain, by timing of entry (absolute value of Huber–White adjusted $t$ statistics in parentheses)

| | Score in highest grade | Annual achievement gains | | | |
|---|---|---|---|---|---|
| | | Without fixed effects | With student and school-by-grade fixed effects, class size, and teacher variables | Previous variables plus school-by-year fixed effects | Previous variables plus peer achievement |
| Specification | (1) | (2) | (3) | (4) | (5) |
| *A. Combined impact* | | | | | |
| Proportion new entrants | − 0.54 | − 0.18 | − 0.12 | − 0.12 | − 0.12 − 0.12 |
| | (11.46) | (10.36) | (3.60) | (2.86) | (3.08) (2.85) |
| Proportion new entrants moved | | | | | 0.00 |
| | | | | | (0.17) |
| *B. Impact by timing of entry* | | | | | |
| Proportion of students entering | − 0.15 | − 0.18 | − 0.06 | − 0.04 | − 0.04 |
| at start of year | (3.23) | (9.62) | (1.72) | (0.92) | (0.77) |
| Proportion of students entering | − 1.33 | − 0.02 | − 0.12 | − 0.14 | − 0.15 |
| during year | (18.10) | (0.37) | (1.94) | (2.43) | (2.69) |
| Observations | 493 777 | 1 482 141 | 1 200 769 | | |

Note: The top and bottom panels represent seperate regressions. All specifications include a full set of current and lagged move indicator variables by type and timing of moves, student eligibility for a subsidized lunch, and grade-by-year. Specifications 1 and 2 also include indicator variables for highest and lowest grades in a campus and new schools. Peer achievement calculations use achievement for the current group of students but from tests taken 2 years in the past; consequently 4th grade is dropped. Teacher characteristics include proportion of teachers with 0 years of experience and teacher turnover rate in the school. Missing information on teachers accounts for the lower sample size in the final three specifications. Specifications 4 and 5 each include the individual and school-by-grade fixed effects, class size and peer variables.

year. The possibility that turnover affects movers and non-movers differentially, perhaps because the effectiveness with which schools assimilate new entrants is a function of turnover, is also examined[23]. Note that all of these school effects are over and above the individual mobility estimates previously presented; all models include a full set of current and past student move indicators by type and timing of moves.

We begin with simple levels and gains specifications that group all entrants together (the top panel) before turning to more complete models. Not surprisingly higher turnover is strongly associated with a lower level of achievement (specification 1), but, as expected, the estimates decrease substantially, as additional controls are included. The estimated impact of overall school mobility in the level-form model is three times that found when simple value-added models are estimated (specification 2), reflecting the substantial sorting of students into schools. More adequately controlling for individual and school differences by adding individual and school-by-grade fixed effects, class size, proportion of new teachers, and the school's teacher turnover rate (specification 3) further reduce the estimated aggregate impact, demonstrating the confluence of overall school stability and other characteristics of individuals and schools[24].

In contrast, the subsequent allowance for school changes over time and for peer changes leaves the results unchanged. Incorporating school-by-year fixed effects (specification 4) produces no change in the point estimates, as turnover continues to exhibit a strong negative relationship with achievement. Specification 5 examines sensitivity to the inclusion of the average achievement of schoolmates to check on the possibility that the path of mobility effects comes from lowered peer quality[25]. The estimates on the turnover variables remain unchanged following the inclusion of the peer quality measure. Within this specification, we also present results distinguishing between the effects of aggregate mobility on movers and non-movers. We find no difference across these groups in the externalities of school instability, implying that the term for the effect of aggregate turnover on a school's ability to assimilate movers in Eq. (4) is unimportant on average. These extensions provide strong support for the interpretation that we have identified the causal impact of student turnover per se and not some correlated difference in teachers or peers.

The magnitude of the coefficient for overall proportion new students in the more complete specifications suggests that a 1 standard deviation increase in the proportion of students who are new to the school (an 11% point change) would reduce achievement by over 0.013 standard deviations. While a single year effect of this magnitude is not large,

[23] We also considered the possibility of nonlinear effects (not shown), but quadratic specifications provided little support for such nonlinearities.

[24] When class size and teacher variables are eliminated (not shown), there is virtually no change in the point estimate for overall student mobility—suggesting that these measured factors are not a source of bias in the estimation of the mobility externality.

[25] Hanushek et al. (2003) describes the estimation of peer group effects using the Texas data. We use peer average achievement of classmates lagged 2 years (but calculated for the current set of peers) to break the potential link due to unobserved differences in teacher quality and to the simultaneous affect students have on one another. As a result, a small number of observations are lost. Constructing average peer achievement based on their test scores for the previous grade (the average achievement of peers at the start of the school year) generates virtually identical results.

Table 6
Estimated effects of proportion of students who are new to the class on achievement gain, by timing of entry, income, and ethnicity (absolute value of Huber–White adjusted $t$ statistics in parentheses, $n = 1\,393\,153$)

| | Ever low income | | Race/ethnicity | | |
|---|---|---|---|---|---|
| | Yes | No | Black | Hispanic | Other |
| *A. Combined Impact* | | | | | |
| Proportion new entrants | − 0.22 | − 0.02 | − 0.30 | − 0.21 | − 0.04 |
| | (6.44) | (0.64) | (7.23) | (5.64) | (1.06) |
| *B. Impact by timing of entry* | | | | | |
| Proportion of students | − 0.14 | 0.03 | − 0.18 | − 0.14 | 0.01 |
| entering at start of year | (3.45) | (0.33) | (3.91) | (3.18) | (0.17) |
| Proportion of students | − 0.21 | 0.04 | − 0.30 | − 0.16 | − 0.04 |
| entering during year | (4.04) | (0.83) | (3.83) | (2.68) | (0.64) |

Note: the coefficients in the table come from four different regressions. Two regressions allow for separate effects by ethnicity, and two allow for separate effects by income. In each of these categories, one regression combines all new entrants into a single category (top panel) and the other separates entrants by timing of entry (bottom panel). All models include individual student, school-by-grade, and school-by-year fixed effects along with a full set of current and lagged move indicator variables by type and timing of moves, student eligibility for a subsidized lunch, and grade-by-year.

the sum total of 10 or 12 years of high turnover will have a substantial cumulative effect on learning for those students who attend high turnover schools year after year.

The bottom panel of Table 5 shows that all turnovers is not alike. The fixed effect estimates in specification 3 report that the negative effect of entrants during the school year is at least twice as large as the effect of entrants at the beginning of the school year. The gap is even larger for specifications that include school-by-year fixed effects, where the hypothesis that turnover prior to the year has no effect cannot be rejected at any conventional level. This conforms with prior expectations that mid-year entry would be much more disruptive.

Table 6 provides strong evidence that turnover effects differ substantially by income and ethnicity. The aggregate turnover effect is highly concentrated in students eligible at least once for a subsidized lunch, where the estimated impact is $-0.22$. For those never eligible the estimate is statistically insignificant with a virtually zero point estimate. This pattern holds even if turnover is disaggregated by timing of aggregate mobility. In the case of ethnicity, the effect of turnover for blacks ($-0.30$) is roughly one third larger than that for Hispanics ($-0.21$) and more than seven times larger than the effect for Whites ($-0.04$ and statistically insignificant). Again this pattern holds even if turnover is disaggregated by timing[26].

An important question relates to the extent that school turnover can account for income and race differences in academic achievement. The direct effects on movers cannot explain much of the income gap, because the disruption costs of moving are small and isolated in

[26] Given the somewhat higher turnover rates in schools attended by low income and minority students, one explanation for this pattern could be that the effect of turnover increases with the turnover rate. However, aggregate turnover specifications that include a quadratic term (not shown) do not find that the effect of turnover increases with the magnitude of turnover. The coefficient on the quadratic term is negative but small and insignificant.

the year of the move and both higher and lower income students obtain higher SQ on average following a between district move[27]. However, move externalities might account for a substantial portion of the achievement gaps given the larger turnover coefficients and more unstable schools attended by lower income and minority students. Specifically, the income difference in school turnover rates is 1.5% points, the black/white difference is 6.2% points, and the Hispanic/white difference is 2.3% points.

Higher school turnover reduces annual achievement gains for lower income students by roughly 0.005 standard deviations relative to higher income students; blacks and Hispanics lose roughly 0.015 and 0.005 standard deviations, respectively, relative to whites (based on the specifications that separate turnover by timing). Seven years of such turnover differences would contribute approximately 0.04 standard deviations to the 7th grade mathematics achievement gaps between income groups and between Hispanics and whites and roughly 0.10 standard deviations to the gap between blacks and white whites. Therefore, turnover differences would explain roughly 8% of the total income and Hispanic/white gaps in 7th grade mathematics achievement and 14% of the corresponding black/white gap. Given the large number of family, community, and school factors that affect achievement, this effect is far from trivial, particularly for blacks.

## 6. Conclusions

Consideration of school mobility is split between those emphasizing the disruption and harm to students and those emphasizing Tiebout choice of schools as a way that families can improve SQ. These opposing views have led to widely differing policy perspectives. At the same time, prior evidence on mobility effects has not reached a consensus on achievement implications, in large part we believe because the existing research has not identified or estimated the same parameters.

We develop a model of achievement growth that highlights the various avenues through which mobility can affect achievement. Our estimates suggest that moves across districts yield a small but significant improvement in average SQ for all demographic groups except for blacks. At the same time, moves within a district are associated with no significant changes in SQ but tend to involve noticeable short run costs—costs that are generally higher for poor and minority students. Importantly, the costs of moving are isolated in the year of the move, while the benefits for a student finding a better school accrue each year that the student remains in the new school.

The interpretation of our estimates does rely on assumptions about the nature and duration of changes in non-school factors that might relate to moving and might separately affect achievement. A series of sensitivity analyses generally supports our causal interpretation of average SQ changes but is not fully conclusive given our lack of direct evidence about what changes in other factors might be coincident with moving. In addition, since estimates of move effects are weighted averages across families moving for

[27] Some small differences by income in the pattern of within district moves also exist, but these are too small to have a significant effect on the average achievement differences given the coefficients related to individual mobility.

both school and non-school reasons, the coefficients likely underestimate the SQ gains for 'Tiebout' movers seeking a higher quality education.

Though families almost certainly ignore the effect of their moving on other students, the possibility of move externalities is quite real. Such externalities are notoriously difficult to identify and estimate in most circumstances—chiefly because of difficulties in distinguishing between individual influences and external effects when the circumstances of the potential externality involve individual choices and behavior. The most important methodological advance of this work is the development of an approach that permits credible separation of individual effects of moves from the aggregate impacts of high mobility schools.

We find that school mobility involves a clear externality. Student turnover, particularly student entry during the school year, reduces achievement gain, and the effects are felt by everyone in the school, not just those who themselves move. Importantly, the adverse effects of turnover appear to be larger for lower income and minority students, the very same students who experience higher average mobility rates and attend more unstable schools. The cumulative effects of turnover differences throughout elementary and secondary school add non-trivially to income, race, and ethnic gaps in achievement.

A policy challenge, which cannot be addressed here, is to devise schooling approaches that mitigate the academic losses due to school turnover. Whether, for example, such things as more standardized curricula, specialized transition and remedial programs for entering students, or more careful classroom placement of new students could help remain open questions.

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## Appendix A

Table A.1. Average changes in teacher and peer characteristics, by type and timing of move

| | % Teachers same as prior year | Peer average math achievement | % Black students |
|---|---|---|---|
| *Move status* | | | |
| Non-movers | – 0.5 | 0.00 | 0.2 |
| Movers | – 1.1 | 0.04 | – 0.8 |
| *Mover by location outcome* | | | |

| | | | |
|---|---|---|---|
| New district, same region | 0.2 | 0.04 | – 1.4 |
| New region | – 2.5 | 0.03 | – 0.6 |
| Move within district | – 1.8 | 0.04 | – 0.5 |
| *Timing of move* | | | |
| Move only once during year | – 0.9 | 0.01 | – 0.4 |
| Move more than once | – 0.6 | 0.01 | – 0.2 |
| *New district, same region* | | | |
| Urban to urban | 5.0 | – 0.02 | 0.4 |
| Urban to nonurban | 5.6 | 0.16 | – 8.9 |
| Nonurban to urban | – 4.1 | – 0.08 | 7.2 |
| Nonurban to nonurban | – 1.6 | 3.9 | – 1.2 |

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