

Reducing School Mobility: A Randomized Trial of a Relationship- Building Intervention

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Student turnover has many negative consequences for students and schools, and the high mobility rates of disadvantaged students may exacerbate inequality. Scholars have advised schools to reduce mobility by building and improving relationships with and among families, but such efforts are rarely tested rigorously. A cluster-randomized field experiment in 52 predominantly Hispanic elementary schools in San Antonio, Texas, and Phoenix, Arizona, tested whether student mobility in early elementary school was reduced through Families and Schools Together (FAST), an intervention that builds social capital among families, children, and schools. FAST failed to reduce mobility overall but substantially reduced the mobility of Black students, who were especially likely to change schools. Improved relationships among families help explain this finding.

KEYWORDS: school mobility, social capital, race/ethnicity, experiment, FAST

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Introduction

Moving to a different school is very common among children in the United States. Following a cohort of kindergarteners from 1998 to 2007, the U.S. Government Accountability Office (Ashby, 2010) reported that 31% changed schools once, 34% changed schools twice, 18% changed schools three times, and 13% changed schools four or more times before entering high school. Mobility tends to be highest in urban schools with disadvantaged populations, especially at the elementary school level, and the South and West regions (as opposed to the Northeast and Midwest) have the highest percentage of schools with high mobility (Ashby, 2010; Rumberger, 2003). A wide body of evidence suggests that student mobility poses serious problems for mobile students as well as their schools, teachers, and even nonmobile peers (Fleming, Harachi, Catalano, Haggerty, & Abbott, 2001; Gasper, DeLuca, & Estacion, 2010, 2012; Kerbow, 1996; Parke & Kanyongo, 2012; Rumberger, 2003; Temple & Reynolds, 1999). The high levels of student turnover in many underperforming schools also hinder reform efforts, as it is difficult to sustain progress with transient students.

Schools clearly have an interest in curtailing disruptive mobility among students, but can they do anything about it? If students are moving due to family or economic circumstances, the answer may be no. But if students change schools because of dissatisfaction with schools, the answer is yes. Researchers have suggested a variety of ways by which schools can increase their “holding power” and reduce student mobility (Kerbow, Azcoitia, & Buell, 2003; Rumberger, 2003; Temple & Reynolds, 1999). Many of these suggestions relate to building relationships of trust, or social capital, within the school community. However, such efforts rarely have been evaluated rigorously (for an exception, see Fleming et al., 2001).

In this study, we examine the effects on student mobility of a school-based intervention designed to build relationships of trust among children, families, and schools. These relationships may be an important mechanism by which schools can reduce student mobility.¹ Our study specifically focuses on mobility between first and third grades in 52 predominantly low-income and Hispanic schools in Phoenix, Arizona, and San Antonio, Texas. This population is particularly interesting, both because the early elementary grades represent a critical time in child development and because mobility tends to be especially high in this population. Since the intervention was randomly assigned to schools, the findings provide some of the strongest evidence to date on the potential of schools to reduce student mobility by building social capital among members of the school community.

School Mobility: Trends, Effects, and Distinctions

School mobility is fairly common nationally, and it is also prevalent in Arizona and Texas, where the research sites for this study are located. In Arizona between 2004 and 2008, more than a quarter of all students changed schools at least once, and mobility rates were highest among elementary students (Fong, Bae, & Huang, 2010). In Texas, one third of children in public schools moved at least once between fourth and seventh grades, not including compulsory moves related to the transition from elementary to middle school (Hanushek, Kain, & Rivkin, 2004).

Effects of School Mobility

Although some types of school moves can have positive effects, most are associated with a range of negative outcomes, including lower test score gains in reading and mathematics, grade retention, lower self-esteem, trouble fitting into schools, dropping out, and even adult substance abuse (Gasper et al., 2012; Grigg, 2012; Parke & Kanyongo, 2012; Reynolds, Chen, & Herbers, 2009; Rumberger, Larson, Ream, & Palardy, 1999; Swanson & Schneider, 1999; Wood, Halfon, Scarlata, Newacheck, & Nessim, 1993). In a meta-analysis of 26 studies of school mobility, Mehana and Reynolds (2004) estimated a 3- to 4-month performance disadvantage in math and reading achievement for mobile students.

Beyond the impact on individuals, there are spillover effects in high-mobility schools, as student turnover affects not only movers but also the nonmovers whose classrooms and schools are disrupted (Hanushek et al., 2004). About 11.5% of schools serving kindergarten through eighth grade have at least 10% of their students leave during the school year (Ashby, 2010). In these high-mobility schools, even nonmobile students exhibit lower levels of school attachment, weaker academic performance, and higher dropout rates (South, Haynie, & Bose, 2007). At the school level, high mobility promotes chaos, decreases teacher morale, and increases administrative burdens (Rumberger, 2003; Rumberger et al., 1999). At the classroom level, high student turnover frustrates teachers, compromises long-term planning, and leads teachers to develop a more generic teaching approach (Lash & Kirkpatrick, 1990). Instead of addressing individual student needs, teachers slow the pace of instruction and become more review-oriented (Kerbow, 1996). This means that after only a few years, students attending high-mobility schools are exposed to considerably less information than those attending schools with lower mobility rates.

These school-level mobility effects are not trivial. While they have the potential to harm all students, there is evidence that they are worse for poor and minority students, contributing to racial and socioeconomic gaps in achievement (Hanushek et al., 2004). Furthermore, school reform efforts

usually assume that students will remain in a specific school long enough for reforms to take effect, but schools in need of reform often have the highest rates of student turnover (Kerbow, 1996). High rates of student mobility are so problematic that some schools have implemented programs to discourage families from moving, such as Chicago's "Staying Put" project (Kerbow et al., 2003).

Causal Inference

Researchers warn of potentially spurious relationships between mobility and student outcomes, since the families most likely to move are often the most disadvantaged (Gasper et al., 2010, 2012). In general, students of low socioeconomic status are more mobile than their more advantaged peers, Black and Hispanic students are more mobile than their White and Asian American peers, and students from single-parent or stepparent families are more mobile than those from traditional two-parent families (Alexander, Entwisle, & Dauber, 1996; Ashby, Hanushek et al., 2004; Burkam, Lee, & Dwyer, 2009; Nelson, Simoni, & Adelman, 1996; Rumberger, 2003; Rumberger & Larson, 1998). Although a causal interpretation of findings on mobility effects remains a challenge because of the many common factors associated with school moves and child outcomes, studies that attempt to disentangle the effects of these confounders consistently find student mobility to have negative consequences, both for the students who change schools and for high-mobility schools (Rumberger, 2003). The evidence is sufficient to warrant examination of why families change schools and how schools can address this issue.

Types of School Mobility

It is important to distinguish among different types of school moves. Some types are more common than others, some are more likely to have negative consequences than others, and some potentially can be addressed by schools while others likely cannot. Researchers have made such distinctions along four dimensions: (a) whether a school move is accompanied by a residential move, (b) when the move occurs, (c) whether the move is voluntary, and (d) if it is voluntary, whether the move is dictated by a negative life event.

First, residential mobility is very common in the United States; 22% of the U.S. population moved between 2008 and 2009, and two-thirds of these moves occurred within the same county (U.S. Census Bureau, 2009). Residential and school mobility are closely linked. Approximately two-thirds of secondary school changes are associated with a residential move (Rumberger & Larson, 1998), and these moves may be more detrimental than simply changing schools. Residentially mobile adolescents have been found to have school-based friendships characterized by weaker academic

performance and lower expectations, less school engagement, and higher rates of deviance (Haynie, South, & Bose, 2006a). They also tend to have higher rates of violent behavior and among adolescent girls, a higher likelihood of attempted suicide (Haynie et al., 2006b; Haynie & South, 2005). Not surprisingly, residential mobility is also associated with reduced achievement in elementary and middle school (Voight, Shinn, & Nation, 2012). Although our data permit an examination of residential mobility, we include it only as a supplementary analysis because residential mobility was not affected by our school-based intervention and controlling for residential mobility did not alter our findings.

Second, the timing of school moves matters because moving during the academic year is more disruptive than moving during the summer (Hanushek et al., 2004). The student's age and grade level also matter; moving during early elementary school is associated with worse outcomes than moves that occur later in the schooling process, especially when school changes are frequent (Burkam et al., 2009). Our study cannot differentiate between academic year and summer moves, but we are able to examine mobility during the early elementary grades, a critical period in child development that few studies of school mobility have explored.

Third, scholars have distinguished between compulsory and noncompulsory school changes. While the majority of school mobility occurs for noncompulsory reasons, compulsory moves, such as the transition from elementary to middle school or from middle school to high school, affect all students and are built into the structure of schooling. These moves are generally less disruptive than noncompulsory moves because school systems are set up for these transitions and all grade-equivalent students experience them together, but they are not free of negative consequences (Grigg, 2012). Because we focus on Grades 1 through 3, our study is not complicated by compulsory moves, so we focus on an effort to curtail noncompulsory (voluntary) school changes.

Finally, voluntary school changes can be subdivided into strategic and reactive moves. Strategic moves historically have been more prevalent among White or socioeconomically advantaged families and are based on a family's choice to seek out a higher quality or better fitting school (also known as "Tiebout" mobility, named after C. M. Tiebout). Reactive moves occur in response to negative events, are more common among minorities and disadvantaged families, and are the type of move most frequently associated with harmful consequences (Fantuzzo, LeBoeuf, Chen, Rouse, & Culhane, 2012; Hanushek et al., 2004; Warren-Sohlberg & Jason, 1992). Some reactive moves are school related, such as those motivated by dissatisfaction with a school's social or academic climate, conflict with students or teachers, or disciplinary problems and expulsions (Kerbow, 1996). Others are not motivated by school-related factors, but instead by negative life events such as family disruption, dissolution, or economic hardship. This

distinction suggests that schools have the potential to curtail certain moves but are unlikely to influence others. It also explains why some school changes are associated with positive effects, yet (most) others are not. Our data do not allow us to differentiate between strategic and reactive moves, but prior research shows that reactive mobility is high in predominantly low-income and minority urban populations, so it is very likely that most—though certainly not all—mobility in our sample is reactive rather than strategic (Alexander et al., 1996; Fong et al., 2010; Hanushek et al., 2004; Kerbow, 1996). To the extent that a school-based intervention can reduce mobility, it is likely to be through deterring school-related reactive moves.

Heterogeneity in School Mobility

School mobility rates differ according to the characteristics of schools, where those with the highest levels of mobility are also the most disadvantaged and tend to have larger proportions of minority and low-income students (Nelson et al., 1996). Again, this fits the profile of our sample of schools. Mobility rates also differ according to the characteristics of students. Differences in mobility along racial/ethnic lines have been studied extensively. Generally, Black and Hispanic students are more likely to change schools than White and Asian American students, due in part to greater economic disadvantage (Alexander et al., 1996). Blacks also tend to change schools more frequently than other race/ethnic groups, and frequent moves are associated with an increased risk of underachievement (Temple & Reynolds, 1999). Evidence that immigrant students and English language learners have above-average mobility rates is also troubling because mobility is associated with a longer time for achieving proficiency in English (Ashby, 2010; Fong et al., 2010; Mitchell, Destino, & Karam, 1997). Moreover, differences in Hispanic subpopulations leave open the possibility of heterogeneity in school mobility among Hispanics; Mexican Americans—who comprise the majority of our sample—display particularly high mobility rates (Ream, 2005).

Student characteristics and school characteristics also interact to affect mobility. School segregation research finds evidence of White flight from predominantly minority public schools (Clotfelter, 2001), evidence of segregation between Black and Hispanic students across the public and private sectors (Fairlie, 2002), and self-segregation of a variety of groups into charter schools (Garcia, 2008). Thus, it is important to examine differential mobility across racial/ethnic groups while keeping the racial composition of schools in mind.

The availability of school choice may play a role in differential mobility patterns as well. Recent data show that Blacks (24%) are more likely to enroll in chosen (as opposed to assigned) public schools than Hispanics (17%), Asian Americans (14%), or Whites (13%) (Grady, Bielick, & Aud, 2010).

Presumably, students are more likely to exercise choice when their families are dissatisfied with their assigned school or if a new school seems particularly promising. That Blacks have the highest rates of exercising school choice suggests that compared to other race/ethnic groups, they are either more dissatisfied with their assigned schools, more sensitive to school-related factors, more heavily recruited by choice schools, or have greater access to choice schools in their communities. With only two research sites and limited information on which schools mobile students attend, we cannot fully address the role of choice, but we do examine the extent to which proximity to charter schools influences mobility in our sample.

Thus, families change schools for a variety of reasons, including family or economic circumstances, aversion to certain groups of students, dissatisfaction or conflict with the school, or attraction to other schools, and these reasons are likely to vary according to the characteristics of students and their schools. This means that strategies to reduce mobility will be more or less effective across students and schools as well. Accordingly, it is important to examine heterogeneity, both in overall mobility rates and in the effects of mobility-reducing efforts, as we do in the following analyses.

School Mobility and Social Capital

Relations of trust between families and school personnel, or social capital, play an important role not only in explaining why school mobility can be detrimental, but also in identifying how schools can reduce mobility. Much research implicates social capital in the negative effects of changing schools; the disruption in relationships among students, school personnel, and parents that accompanies school moves helps explain why mobile students exhibit lower achievement (Coleman, 1988; Pribesh & Downey, 1999; Ream, 2005). However, the relationship between mobility and social capital is multidirectional; not only does mobility affect social capital, but social capital also affects mobility.

Reducing Mobility

Studies of residential mobility provide evidence that social networks play an important role in encouraging families to stay. Both nuclear and extended family ties deter long-range residential mobility, especially for racial/ethnic minorities and families of low socioeconomic status (Dawkins, 2006; Spilimbergo & Ubeda, 2004). Social ties with others living nearby deter long-distance mobility as well (Kan, 2007). Coleman (1988) lamented the decline in these informal sources of social capital and highlighted the need for formal organizations to take their place. Accordingly, there is evidence that local institutions such as churches and businesses can serve a socially integrating function that deters residential mobility (Irwin, Blanchard, Tolbert, Nucci, & Lyson, 2004).

Schools are an obvious candidate to serve this purpose with regard to school mobility. Researchers have suggested several ways for schools to encourage families to stay, many of which relate to building social capital. By improving their social and academic climates and making an effort to boost students' and their families' sense of membership in the school community, schools can increase parent engagement (Rumberger & Larson, 1998). Schools can also make themselves more attractive to students and their parents by implementing programs that promote positive relationships with families (Fleming et al., 2001; Kerbow, 1996; Kerbow et al., 2003; Rumberger, 2003; Rumberger et al., 1999). Thus, by making efforts to improve the number and quality of social relations among students, parents, and school personnel, and providing a space in which these networks can develop and operate, schools can aid in the production of social capital and possibly reduce student mobility.

The Intervention: Families and Schools Together (FAST)

Our study examines an intervention expected to reduce school mobility by enacting the aforementioned recommendations. Families and Schools Together (FAST) is an intensive 8-week multifamily afterschool program designed to empower parents, promote child resilience, and increase social capital—relations of trust and shared expectations—within and between families and among parents and school personnel. FAST is typically implemented in three stages: (a) active outreach to recruit and engage parents, (b) 8 weeks of multifamily group meetings at the school, followed by (c) 2 years of monthly parent-led meetings (FASTWORKS).² The 8 weekly sessions—which take place at the school—last approximately 2½ hours and follow a preset schedule, where about two-thirds of the activities center around building relationships between families and schools and the remainder target within-family bonding (Kratochwill, McDonald, Levin, Bear-Tibbetts, & Demaray, 2004). During each session, these activities include: family communication and bonding games, parent-directed family meals, parent social support groups, between-family bonding activities, one-on-one child-directed play therapy, and opening and closing routines modeling family rituals (see the Appendix in the online journal for a detailed description of each FAST activity). FAST activities are theoretically motivated, incorporating work from social ecological theory (Bronfenbrenner, 1979), family systems theory and family therapy (Minuchin, 1974), family stress theory (McCubbin, Sussman, & Patterson, 1983), and research in the areas of community development and social capital (Coleman, 1988; Dunst, Trivette, & Deal, 1988; Putman, 2000) in order to build social networks by strengthening bonds among families and schools (see Kratochwill et al., 2004, and www.familiesandschools.org for specific information about FAST activities and their theoretical framework). These research-based activities, adapted to

be culturally and linguistically representative, are led by a trained team that includes at least one member of the school staff in addition to a combination of school parents and community professionals from local social service agencies.

The FAST intervention has been successfully replicated and implemented across diverse racial, ethnic, and social class groups in urban and rural settings within 45 states and internationally (McDonald, 2002; McDonald et al., 1997). Several recent randomized controlled trials (RCTs), including one involving the sample studied here, demonstrate that FAST engages socially marginalized families with schools and school staff and improves the academic performance and social skills of participating children (Gamoran, Turley, Turner, & Fish, 2012; Kratochwill et al., 2004; Kratochwill, McDonald, Levin, Scalia, & Coover, 2009; Layzer, Goodson, Bernstein, & Price, 2001; McDonald et al., 2006). Each of these RCTs had a different study focus and explored the impact of FAST on children's educational and behavioral outcomes for samples that differed by geographic region and race/ethnicity of participants (Supplementary Table S1 in the online journal briefly summarizes these previous RCTs). Our study is unique in that it examines low-income predominantly Latino Southwestern communities, recruits all families rather than those of at-risk children, and is the first to investigate effects of FAST on school mobility.

Although FAST was not explicitly designed to reduce school mobility, its proven ability to build and enhance social relationships among members of the school community directly addresses one of the most important mechanisms by which schools can reduce mobility. FAST activities work to strengthen relationships among three specific types of networks: within families, between families within the same school community, and between families and school personnel. By developing and improving these types of relationships—and doing so within the physical boundaries of the school—FAST decreases school-related anxiety for both children and parents, reduces barriers to parent engagement, makes the school a more welcoming environment for families, and fosters the creation of parent networks within schools, where resources and social support can be exchanged (Kratochwill et al., 2004, 2009; Layzer et al., 2001; McDonald et al., 2006). Thus, FAST is just the sort of social capital-building organization advocated by Coleman (1988) and others to reduce school mobility.

The research on social capital, school mobility, and FAST suggests that the intervention could reduce school mobility for three reasons. First, building relationships among families within a school should increase parents' sense of membership in the school community and reduce mobility. Second, FAST makes schools central to the social networks of parents, providing physical space where these networks develop and operate and where families exchange resources and social support. Changing schools would result in a loss of this source of social capital. Third, increasing families'

familiarity with, and trust of, the school and school personnel by offering a new and informal context where parents can interact with school staff should reduce school moves driven by dissatisfaction, discomfort, or distrust. Thus, even though reducing mobility is not an explicit goal of the FAST intervention, it is for these reasons that we expect students in schools assigned to the FAST program to be less likely to change schools between Grades 1 through 3 than students in control schools. Moreover, we expect FAST to be particularly effective at reducing educationally motivated moves, such as those spurred by school dissatisfaction or feelings of isolation from the school community, which, as discussed previously, may be more likely for Black families. Since motives for changing schools likely vary across students, we anticipate heterogeneity in the effects of FAST on school mobility across different types of students. Because our sample of schools is relatively homogeneous, we expect less variation in effects across schools.

Data and Measures

Sample Recruitment and Randomization

We use data drawn from the Children, Families and Schools (CFS) study, a cluster-randomized controlled trial targeting first-grade students and their families in eligible elementary schools that agreed to randomization in Phoenix, Arizona, and San Antonio, Texas.³ These cities and schools were selected because of their high proportions of Hispanic students and students eligible for the national school lunch program, and our sample reflects these characteristics. Fifty-two elementary schools were randomly assigned to a treatment condition, with half selected to receive the intervention (26 FAST schools) and half selected to continue with business as usual (26 control schools). Randomization produced two comparable groups of schools with no statistically significant differences on pretreatment demographics or academic performance characteristics.

Participant data were collected during the students' first-grade year (2008–2009 for Cohort 1 and 2009–2010 for Cohort 2), with follow-ups at the end of Year 2 and a final survey in Year 3, when students were expected to be in third grade (2010–2011 for Cohort 1 and 2011–2012 for Cohort 2).⁴ Just below 60% of first-grade families consented to participate in the study, which limits the generalizability of our results to some extent, but since there were no statistically significant differences in the recruitment rates between FAST and control schools, our results should be unbiased. In FAST schools, 73% of families who consented attended at least one FAST session, and among those who attended at least one session, 33% “graduated” with a “full dose” of FAST, meaning that they began in week 1 or 2 and attended six or more of the eight sessions. On average, participants attended 35% of FAST sessions, and half the participants attended multiple sessions.

Fortunately, we are not missing any data related to treatment assignment, randomization, school mobility, or school characteristics. Thus, our analytic sample includes all 3,091 students who consented to the study and the 52 schools they attended in first grade. We discuss additional covariates and our handling of missing student data in the following.

Outcome and Key Independent Variables

The outcome is a binary indicator of whether a student was enrolled in a different school in third grade than he or she attended in first grade. School moves were identified using rosters provided by schools at the beginning of the first and third years and should be very accurate. Students retained in grade were also identified so as not to be incorrectly labeled as movers. The weakness of this measure is that we are unable to identify students who made multiple school moves or who changed schools but returned to their original school between first and third grade. We conducted both an intent-to-treat (ITT) analysis, which estimates the average treatment effect for those in schools assigned to FAST, and a complier average causal effect (CACE) analysis, which estimates the average treatment effect for those who actually complied (i.e., who graduated from FAST by attending one of the first two sessions and at least six of the total eight sessions). The key independent variable in the ITT analysis is a school-level treatment indicator, and the key independent variable in the CACE analysis is an individual-level indicator of graduating from FAST.

Control Variables

The randomization of FAST occurred within three districts in Phoenix and two randomization blocks in San Antonio, so estimating an unbiased average treatment effect requires controlling for these units of randomization.⁵ These controls were included at the school level in our analyses. Additional controls can increase statistical power and correct for pretreatment differences that may arise in spite of randomization. At the school level, we included the size of the school; the proportion of students receiving subsidized lunch; the proportion of students identified as Hispanic, Black, White, Other (Asian or American Indian); English language learners; and the proportion of third graders scoring proficient on state assessments in reading (all based on the 2008–2009 school year). Because school choice may play a role in school mobility, we also included measures of the number of charter elementary schools located within three miles of each school in Year 1 of the study and the change in the number of such schools between Years 1 and 3.

At the student level, we included each student's age, a log-transformed measure of travel time (in minutes) from home to school, indicators of the student's gender and race/ethnicity (Hispanic, Black, White, or Other),

and indicators of whether the student was an English language learner, a recipient of special education services, or eligible for the national school lunch program. We also conducted supplementary analyses that incorporate information on participants' residential mobility, which we discuss at the end of the results section.

Since FAST is expected to reduce mobility by building social capital among families and between families and schools, several pretreatment measures of parent-reported social capital were also included. These include parent reports of the number of school staff they felt comfortable approaching (staff contacts), the number of parents of their child's friends they knew (intergenerational closure; Coleman, 1988), the degree to which they agreed that they shared expectations for their child with other parents, whether they regularly discussed school with their child, and whether they regularly participated in school activities. Two additional scales were constructed from a battery of questions. The first is a parent-staff trust scale constructed from four items related to parents' perceived trust of school staff ($\alpha = 0.86$). The second is a parent-parent involvement scale ($\alpha = 0.91$) measuring how involved each parent was with other parents at the school, in terms of exchanging favors and social support. Together, these measures provide information on both the quantity and quality of relationships between families in the community, as well as between families and schools. More details on these social capital indicators and scale construction are provided in the Appendix in the online journal.

Missing Data

Supplementary Table S2 in the online journal summarizes the raw student-level data, including the number of observations for each variable. We used multiple imputation procedures to impute missing data values for student-level covariates in order to maximize the use of available information and minimize bias (Royston, 2005; Rubin, 1987; von Hippel, 2009).⁶ We created five imputed data sets using `-ice-` in Stata 12, analyzed each individually, and derived final estimates adjusted for variability between these data sets.⁷

Method and Analysis

Intent-to-Treat Analysis

Because the outcome is a dichotomous indicator of whether each student changed schools between Years 1 and 3, and treatment assignment occurred at the school level, we used a two-level logistic regression approach, as described by Raudenbush and Bryk (2002). For the ITT analysis, the comparison is based on school assignment to the treatment versus control condition rather than actual receipt of the treatment, which varied

among participants. It should be noted that the ITT effect encompasses the total average effect of treatment assignment, including any effects driven by participation in the FAST sessions, subsequent FASTWORKS meetings over the next 2 years, as well as any spillover effects to families who did not participate. The null model, shown in Equation 1, partitions the variance in the log-odds of mobility into within- and between-school components. There is no within-school error term because logistic regression predicts probabilities rather than expected values, and the error is a function of these predicted probabilities. The between-school error term, u_{0j} , represents each school's deviation from the grand mean (γ_{00}) and is used to estimate between-school variability.

$$\eta_{ij} = \log\left(\frac{\phi_{ij}}{1-\phi_{ij}}\right) = \gamma_{00} + u_{0j}, \tag{1}$$

To estimate the unbiased ITT effect of FAST, we added the treatment indicator (FAST) along with controls for the units of randomization (RAND) to the second-level model, as shown in Equation 2.⁸

$$\eta_{ij} = \log\left(\frac{\phi_{ij}}{1-\phi_{ij}}\right) = \gamma_{00} + \gamma_{01}FAST + \boldsymbol{\gamma}RAND + u_{0j}, \tag{2}$$

In further specifications, we added the pretreatment student-level and school-level covariates listed previously. Throughout, we used random-intercept models, which hold the effects of all student-level predictors fixed, meaning they do not vary across schools. To examine heterogeneous effects of FAST on mobility, we also included cross-level interactions of the treatment with selected student-level covariates, including race/ethnicity, gender, travel time to school, survey language, free/reduced lunch status, English Language Learner status, and special education status. These cross-level interactions permit nonrandomly varying slopes for student-level predictors.⁹ Similarly, we examined school-level interactions between FAST and school characteristics, although with only 52 fairly homogeneous schools, our study is underpowered to detect school-level interactions.

Complier Average Causal Effect Analysis

If FAST affects school mobility, this should be especially true for students who comply with their treatment assignment and actually attend the FAST sessions, which are the core of the intervention. However, since compliance cannot be randomly assigned, quasi-experimental methods are required to estimate the effect for compliers. Families in the treatment group who attended the sessions are likely to be less prone to move than families in the treatment group who did not attend the sessions. To account for selection bias, we must compare the compliers from the treatment group to those

in the control group who would have complied, had they been offered the treatment.

Our approach views compliers as a latent class of individuals that is observed for the treatment group but unobserved for the control group. By using observed data on the compliance of the treatment group and observed pretreatment predictors of compliance for all participants, we are able to identify members of the control group who would have been most likely to comply if they had been given the opportunity. We examined several specifications of the compliance model and present the one that best distinguishes compliers and noncompliers. The compliance model was estimated simultaneously with a multilevel model predicting school mobility, similar to those used in the ITT analysis. This model assumes that FAST affected only those who complied with the treatment, and it estimates the complier average causal effect (Muthén & Muthén, 2010). We provide more information in the results section, and further details are available from the authors upon request.

Results

Table 1 summarizes school-level descriptive statistics by treatment and shows that there were no statistically significant differences in school characteristics across the two conditions, as expected under random assignment. Post-imputation student-level descriptive statistics, by treatment, are summarized in Table 2. The students in our sample reflect the demographic composition of the schools. About 15% of the sample was White, just over 70% was Hispanic, and nearly 10% was Black, while less than 5% made up a combination of other race/ethnic groups. We did find statistically significant differences between FAST and control schools at the individual level for some covariates. Students in FAST schools lived farther away from their schools and were more disadvantaged on most pretreatment measures of social capital (the lone exception was parent-staff trust, which favored the FAST group). It is unclear whether these differences were due to chance, differential selection to participate in our study, or an effect of treatment assignment on survey responses relating to social capital. In any case, it is important to consider these differences and account for them in our analyses. Although this study does not focus on FAST effects on social capital per se, we note that FAST did significantly boost social capital between the beginning and end of first grade (Supplementary Table S2 in the online journal; Gamoran et al., 2012).

Intent-to-Treat Results

The results of our ITT analysis are summarized in Table 3. Coefficients are presented on the logit scale, so positive coefficients correspond to a higher likelihood of changing schools, and negative coefficients

Table 1
School-Level Descriptive Statistics by Treatment

	Control (<i>N</i> = 26)		FAST (<i>N</i> = 26)		FAST-Control	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	Mean Difference	<i>p</i> Value
Proportion proficient, math	0.74	(0.21)	0.74	(0.18)	0.00	.82
Proportion proficient, reading	0.74	(0.22)	0.75	(0.19)	0.01	.89
School size	709.2	(181.3)	702.5	(184.0)	-6.69	.59
Proportion Black	0.10	(0.09)	0.10	(0.09)	0.00	.29
Proportion American Indian	0.01	(0.02)	0.01	(0.02)	0.00	.84
Proportion Asian	0.02	(0.02)	0.02	(0.02)	0.00	.31
Proportion Hispanic	0.75	(0.18)	0.72	(0.21)	-0.02	.49
Proportion White	0.13	(0.16)	0.15	(0.18)	0.02	.81
Proportion free/reduced lunch	0.77	(0.18)	0.76	(0.18)	-0.01	1.00
Charter schools within 3 miles, Year 1	3.92	(2.98)	3.77	(2.57)	-0.15	.84
Charter schools within 3 miles, change Year 3 – Year 1	0.92	(1.65)	0.65	(1.16)	-0.27	.50

Note. No missing school data; *p* value based on two-tailed *t* test with unequal variances.

correspond to a lower likelihood of changing schools. A full table with standard errors is included in Supplementary Table S3 in the online journal. The null model (Column 1) estimates a between-school variance of .156. The latent intraclass correlation (which uses $\pi^2/3$ as the within-group variance in multilevel logit models) is .047; in other words, less than 5% of the variance in the probability of changing schools occurred between schools. This model also estimates the typical student in the typical school’s probability of changing schools to be .380. This is roughly equal to the proportion of students in our sample making a school change and is comparable to prior studies of student mobility in early elementary school. Thus, overall levels of school mobility were quite high in our sample but consistent with prior studies, and there was not much variability in mobility among schools.

Column 2 shows the unbiased average ITT effect of FAST on mobility, which is small, positive, and nonsignificant. According to this model, the predicted probability of the average student in a FAST school changing schools was about .39, compared to .37 for students in control schools. There were no statistically significant differences in mobility across the units of randomization, and further analyses found no evidence of heterogeneous

Table 2
Student-Level Descriptive Statistics by Treatment, Imputed

	Control		FAST		FAST-Control Mean Difference
	(N = 1,497)		(N = 1,594)		
	M	SD	M	SD	
Changed schools	0.37	(0.48)	0.39	(0.49)	0.03
Spanish pretest	0.29	(0.45)	0.27	(0.44)	-0.02
Female	0.50	(0.52)	0.51	(0.50)	0.02
Time to school (minutes)	3.22	(3.89)	4.38	(5.90)	1.15*
Age (years)	7.08	(0.46)	7.09	(0.48)	0.01
Black	0.09	(0.33)	0.09	(3.64)	0.00
Hispanic	0.75	(0.45)	0.72	(0.45)	-0.03
White	0.14	(0.35)	0.16	(0.37)	0.02†
Other	0.02	(0.17)	0.02	(0.26)	0.00
Free/reduced lunch	0.79	(0.42)	0.79	(0.42)	0.00
English language learner	0.27	(0.50)	0.26	(0.44)	-0.01
Special education	0.10	(0.33)	0.11	(0.33)	0.01
Number of staff contacts	3.87	(1.81)	3.71	(1.85)	-0.16*
Shared expectations with parents	2.32	(1.13)	2.22	(1.10)	-0.10*
Intergenerational closure	3.09	(2.17)	2.77	(2.15)	-0.32*
Talk to child about school	4.75	(0.70)	4.69	(0.75)	-0.07*
Participate at school activities	3.97	(1.14)	3.83	(1.16)	-0.14*
Parent-staff trust	-0.03	(0.88)	0.03	(0.80)	0.06*
Parent-parent involvement	0.06	(0.85)	-0.05	(0.82)	-0.11*
Graduated FAST (6+ sessions)	—	—	0.24	(0.43)	—
Percentage FAST sessions attended	—	—	0.35	(0.35)	—

Note. Estimates combined across five imputations.

†*p* < .10. **p* < .05.

FAST effects (interactions) across districts or randomization blocks. In short, the findings suggest that on average, attending a school assigned to FAST did not reduce school mobility.

Earlier we reported some pretreatment differences in student characteristics between treatment conditions. Specifically, students in FAST schools tended to report lower levels of social capital prior to treatment and to live farther from school than students in control schools. Column 3 shows the estimates after controlling for pretreatment student background and social capital variables. The FAST effect is even smaller and continues to be indistinguishable from zero, further suggesting that there was no main effect of FAST on school mobility. Not surprisingly, students who lived farther from their school were more likely to change schools, and mobility was lower for students whose parents knew more of their friends' parents

Table 3
Two-Level Logistic Regression Estimates: School Mobility

	(1) Null	(2) Avg. ITT	(3) Student Covariates	(4) School Covariates	(5) Student Interactions
Intercept variance (between schools)	0.156	0.121	0.130	0.045	0.045
Intercept	-0.490*	-0.551*	-1.223*	-3.994*	-4.367*
FAST		0.090	0.012	-0.020	0.427
District 1		0.038	0.105	-0.099	0.004
District 2		0.238	0.115	-0.129	-0.105
District 3		0.173	0.086	0.515	0.532
Block 2		-0.226	-0.286	-0.241	-0.245
Log (time to school)			0.425*	0.368*	0.580*
Age (standardized)			-0.001	0.013	0.013
Spanish pretest			0.146	0.104	0.077
Female			0.088	0.080	-0.015
English language learner			-0.139	-0.097	-0.178
Special education			-0.153	-0.179	-0.391†
Black			0.436*	0.399*	0.743*
White			0.348*	0.360*	0.408*
Other			0.186	0.231	0.285
Free/reduced lunch			0.323*	0.338*	0.389*
Number of school contacts			-0.018	-0.019	-0.022
Shared expectations with parents			-0.037	-0.043	-0.041
Talk to child about school			0.066	0.065	0.065
Intergenerational closure			-0.131*	-0.131*	-0.115*
Participate at school activities			0.003	0.015	0.010
Parent-staff trust (standardized)			-0.044	-0.057	-0.091
Parent-parent involvement (standardized)			0.013	0.019	0.023
Missing demographics			0.559†	0.587†	0.585†
Missing pretest survey			0.235	0.239	0.224
School size				0.001*	0.001†
Percentage Black				0.803	0.872
Percentage White				1.456	1.480
Percentage free/reduced lunch				1.674	1.704
Percentage proficient, reading				0.618	0.780
Charter schools, Year 1				0.051†	0.042
Charter school growth, Years 1–3				-0.159*	-0.144†
FAST × Log(Time)					-0.339*
FAST × Spanish Survey					0.040
FAST × Female					0.198
FAST × Special Education					0.420
FAST × Black					-0.649*
FAST × English Language Learner					0.202
FAST × White					-0.094
FAST × Other					-0.051
FAST × Free/Reduced Lunch					-0.086
FAST × Intergenerational Closure					-0.025
FAST × Parent-Staff Trust					0.074

Note. $N = 3,091$ students, $J = 52$ schools. Estimates combined across five imputations. Full table with standard errors and school interactions provided in supplement in the online journal. Avg. ITT = average intent-to-treat.

† $p < .10$. * $p < .05$.

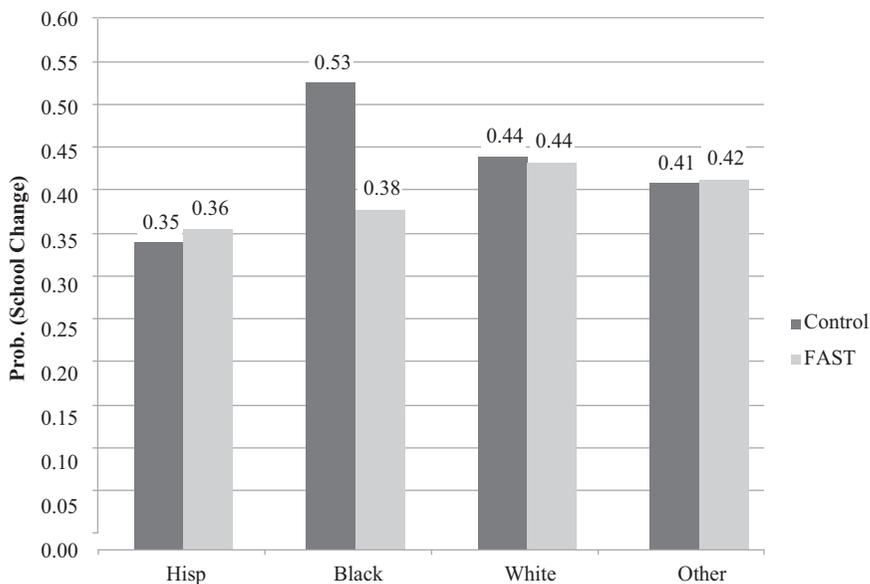
at the beginning of first grade, suggesting that more intergenerational closure was associated with less school mobility. Together, these findings imply that pretreatment differences in social capital and distance to school do not substantially bias FAST effects, but if anything, the bias is upward, making mobility in FAST schools appear higher than it should be.

There were also differences in mobility by race/ethnicity and subsidized lunch status. School mobility was higher among Black and White students than Hispanic students, and it was higher among students who qualified for free or reduced-price lunch than those who did not. The corresponding predicted probabilities of changing schools were .35 for Hispanic students, .46 for Black students, .43 for White students, and .39 for students in the Other category. Students qualifying for free or reduced-price lunch had a .39 predicted probability of making a school change, compared to .31 for students who did not qualify for subsidized lunch.

The FAST effect estimates do not change after accounting for pretreatment school characteristics (Column 4), which is not surprising considering there were no school-level differences in observed characteristics across treatment conditions. Mobility was significantly higher in larger schools and may have been higher in schools with more Whites and more students qualifying for free/reduced-price lunch. It also appears that mobility was higher in schools with more charter schools located nearby at the beginning of the study, but lower for schools that experienced a growth in nearby charter schools. We found no evidence of treatment effect interactions with any of these school characteristics (Supplementary Table S3 in the online journal).

Column 5 examines interactions of FAST with selected student characteristics. The significant negative interaction with time to school suggests that although living farther from school was associated with higher mobility, this association was significantly weaker in FAST schools. There are no significant interactions with survey language, gender, English language learner status, or free or reduced-price lunch status, suggesting that FAST was equally ineffective in reducing school mobility for these groups of students in our sample. Of the interactions with race/ethnicity, there is a negative and statistically significant interaction for Blacks and smaller negative interactions for Whites and Others that do not reach statistical significance. Figure 1 translates these estimates into predicted probabilities. The substantial effect of FAST on school mobility for Black students is particularly striking considering their high mobility rates. Net of all other covariates, Black students in control schools were more likely to move (.53) than not, but in FAST schools their probability of moving was much lower (.38), bringing them to par with other non-Hispanics and nearly equal to Hispanics, who had the lowest school mobility rates in our sample.

Exploring the FAST effect on Black mobility. The significant reduction of mobility for Black students warrants further exploration, so we took several



Estimates based on Table 3, Column 5, average on other covariates except ELL status and survey language.

Figure 1. School mobility by treatment and race/ethnicity.

measures to examine the robustness of this finding and found convincing evidence that FAST reduced school mobility for Black students. First, we examined pretreatment descriptive statistics by treatment for the Black subsample. There were no statistically significant differences in school characteristics (averages weighted by Black enrollment), although Black students in FAST attended schools with fewer Hispanics and fewer charter schools nearby (Supplementary Table S4 in the online journal). There were also no statistically significant differences in student characteristics by treatment among Blacks, and the patterns of these differences were similar to those reported for the overall sample (Supplementary Table S5 in the online journal). Second, we added interactions of FAST with other pretreatment variables, such as indicators of social capital, the racial composition of the school, and the proximity to charter schools (available upon request). Though Blacks were more likely to move from predominantly Hispanic schools and when there were more charter schools nearby, these interactions were not statistically significant, and allowing for them did not explain the Black by FAST interaction. Another threat to validity is that in logistic regressions, coefficients are scaled relative to the variance of the error term, so this interaction could potentially be an artifact of differences in unobserved factors related to mobility among Blacks (Allison, 1999), but

models that estimated a unique variance for Blacks found no significant evidence of this unobserved heterogeneity, and allowing for it did not alter our findings. Thus, we are convinced that Black families in this sample were indeed very likely to change schools and that FAST substantially reduced their propensity to move.

We suggest two plausible explanations for the particularly high levels of Black mobility and the FAST effect reducing Black mobility. First, suppose Black families were more likely to change schools out of dissatisfaction related to poor relationships with schools and FAST improved these relationships. The variable most relevant to this explanation is the “parent-staff trust” scale measured in the posttreatment parent survey. Second, suppose Black families were more likely to change schools because they felt isolated from other families in the school community, but FAST helped these families build relationships with others at the school. The variable most relevant to this explanation is posttreatment intergenerational closure. We tested these explanations among families completing a posttreatment survey (roughly two-thirds of our sample) by adding each of these social capital measures, as well as its interactions with FAST, Black, and three-way interactions with FAST and Black, to simplified models using a package designed to test mediation in logistic regressions (Kohler, Karlson, & Holm, 2011).¹⁰

The results of this analysis are presented in Table 4. The reduced model shows the FAST and Black main effects and the Black by FAST interaction without allowing them to be correlated with the mediators (in gray), and the full model shows the extent to which these effects are mediated when they are allowed to be correlated. The final two columns show the percentage of the Black main effect and Black by FAST interaction that are explained by each mediator. The results suggest that the Black by FAST interaction is almost totally explained by the three-way interaction of Black, FAST, and intergenerational closure. Thus, it was not simply that FAST boosted intergenerational closure, or that intergenerational closure had a larger impact on Black mobility, but that the intergenerational closure promoted by FAST had a particularly strong impact on reducing Black mobility.

It should be stressed that this analysis is nonexperimental in that mediators are not randomized, so we treat this as an exploratory procedure. Nonetheless, the findings fit a story in which Black families were socially isolated from other parents in these schools, but FAST helped bring them into parental networks and reduced their propensity to change schools.

Complier Average Causal Effect Results

The CACE analysis focuses on the effects of FAST for those families who actually complied with the treatment assignment and graduated from FAST.

Table 4
Mediators of Black-by-FAST Interaction

	Reduced		Full		Percentage Black Explained	Percentage Black × FAST Explained
	Coefficient	SE	Coefficient	SE		
Intercept	-0.465*	(0.127)	-0.362*	(0.143)	—	—
Black	0.840*	(0.244)	0.443	(0.411)	—	—
FAST	0.167	(0.151)	0.121	(0.224)	—	—
Black × FAST	-0.788*	(0.399)	-0.035	(0.554)	—	—
Pre: intergenerational closure	-0.130*	(0.027)	-0.085*	(0.032)	—	—
Pre: parent-staff trust	-0.106*	(0.052)	0.001	(0.064)	—	—
<i>Mediators (Posttreatment)</i>						
Intergenerational closure	-0.076*	(0.036)	-0.076*	(0.036)	1.5%	-2.1%
FAST × Intergenerational Closure	0.019	(0.051)	0.019	(0.051)	0.2%	1.5%
Black × Intergenerational Closure	0.103	(0.119)	0.103	(0.119)	35.5%	-0.5%
Black × FAST × Intergenerational Closure	-0.228	(0.166)	-0.228	(0.166)	-0.2%	84.5%
Parent-staff trust	-0.191*	(0.065)	-0.191*	(0.065)	4.9%	6.3%
FAST × Parent-Staff Trust	0.040	(0.085)	0.040	(0.085)	0.1%	0.5%
Black × Parent-Staff Trust	-0.190	(0.199)	-0.190	(0.199)	5.3%	2.8%
Black × FAST × Parent-Staff Trust	0.157	(0.316)	0.157	(0.316)	0.0%	2.6%

Note. $N = 1,935$ students with posttreatment surveys, $J = 52$ schools. Standard errors adjusted for within-school clustering displayed in parentheses. In reduced model, gray cells indicate that variables have been residualized of the pretreatment variables.

* $p < .05$.

Table 5
Compliance Model

	Full Sample		Black Sample	
	(N = 3,091, J = 52)		(N = 283, J = 49)	
	Coefficient	SE	Coefficient	SE
Log(time to school)	-0.067	(0.110)	0.473	(0.416)
Spanish survey	0.490*	(0.180)	—	—
White	-0.320	(0.200)	—	—
Free/reduced lunch	-0.197	(0.196)	0.269	(0.500)
English language learner	0.234	(0.209)	—	—
Special education	-0.396*	(0.202)	-0.593	(0.992)
Age (standardized)	-0.164*	(0.062)	-0.512*	(0.248)
Number of school contacts	-0.022	(0.039)	-0.106	(0.148)
Shared expectations with parents	0.051	(0.077)	0.020	(0.277)
Talk to child about school	-0.073	(0.085)	-0.138	(0.463)
Intergenerational closure	0.101*	(0.029)	0.149	(0.146)
Participate at school activities	0.197*	(0.060)	0.154	(0.270)
Parent-staff trust (standardized)	-0.059	(0.086)	-0.193	(0.278)
Parent-parent involvement (standardized)	-0.086	(0.071)	0.112	(0.398)
School size	-0.001	(0.001)	-0.002	(0.002)
Percentage White	-1.174	(1.267)	-5.713	(4.177)
Percentage free/reduced lunch	-1.681	(1.052)	-4.994	(3.975)
Intercept	0.250	(1.169)	3.300	(4.135)

Note. Twenty-five percent of full sample and 16% of Black sample classified as compliers. Estimates combined across five imputations.

* $p < .05$.

Given the striking findings presented previously, we estimated compliance models for the full sample and for the subsample of Blacks.¹¹ Table 5 shows the estimates of the preferred compliance models, which classify 25% of the full sample and 16% of the Black sample as compliers or would-be compliers.¹²

The results from the CACE analysis, shown in Table 6, are practically identical to those provided by the ITT analysis. FAST had no overall effect for compliers; the predicted probability of changing schools for compliers in FAST schools was .41, compared to .40 for would-be compliers in control schools. Echoing earlier findings, mobility was higher among Blacks and Whites than among Hispanics and lower for those with higher initial levels of intergenerational closure. Because the finding of reduced mobility for Blacks in the FAST group was so intriguing, we also estimated a CACE model on the subsample of Blacks in the study. The results indicate a huge FAST

Table 6
Complier Average Causal Effect Model Estimates: School Mobility

	Full Sample		Black Sample	
	<i>(N = 3,091, J = 52)</i>		<i>(N = 283, J = 49)</i>	
	Coefficient	SE	Coefficient	SE
FAST	0.040	(0.460)	-12.947*	(3.219)
Log(time to school)	0.421*	(0.084)	-0.110	(0.274)
Age (standardized)	-0.003	(0.045)	-0.174	(0.137)
Spanish pretest	0.155	(0.118)	—	—
Female	0.084	(0.088)	0.312	(0.353)
English language learner	-0.109	(0.136)	—	—
Special education	-0.128	(0.177)	0.256	(0.595)
Black	0.508*	(0.186)	—	—
White	0.421*	(0.124)	—	—
Other	0.391	(0.417)	—	—
Free/reduced lunch	0.312*	(0.120)	1.167*	(0.417)
Number of school contacts	-0.021	(0.021)	0.022	(0.082)
Shared expectations with parents	-0.036	(0.053)	-0.405*	(0.172)
Talk to child about school	0.056	(0.056)	0.067	(0.281)
Intergenerational closure	-0.126*	(0.022)	-0.388*	(0.110)
Participate at school activities	0.008	(0.041)	-0.056	(0.163)
Parent-staff trust (standardized)	-0.043	(0.039)	0.025	(0.157)
Parent-parent involvement (standardized)	0.012	(0.052)	0.676*	(0.225)
School size	0.001	(0.000)	—	—
Percentage Black	0.446	(0.649)	—	—
Percentage White	0.746	(1.320)	—	—
Percentage free/reduced lunch	1.422	(1.219)	—	—
Percentage proficient, reading	0.362	(0.818)	—	—
District 1	0.027	(0.372)	0.412	(0.677)
District 2	0.176	(0.500)	0.027	(0.637)
District 3	0.118	(0.299)	-0.193	(0.570)
Block 2	-0.198	(0.250)	0.696	(0.633)
Intercept	-3.337*	(1.452)	13.310*	(3.001)

Note. Twenty-five percent of full sample and 16% of Black sample classified as compliers. Estimates combined across five imputations.

**p* < .05.

effect on reducing school mobility for Black compliers. While the predicted probability of changing schools for Black compliers in FAST schools was .43, it was almost 1.0 in control schools, as practically all Black would-be compliers in these schools moved.¹³ Other estimates suggest potential reasons for this effect. For Blacks, pretreatment parent-parent social capital

measures were especially important predictors of reduced mobility. In particular, both higher levels of shared expectations with other parents and intergenerational closure were significantly associated with lower probabilities of school mobility. This further supports the hypothesis that increased parent-parent social capital played an important role in lowering the mobility of Black students.

Supplementary Analyses: Residential Mobility

Given the close relationship between school mobility and residential mobility, we conducted supplementary analyses incorporating data on participants' residential moves. ITT models similar to those presented previously provided no evidence of FAST effects on residential mobility overall or for any subgroup of students. We also found that controlling for residential mobility did not alter the FAST effect on school mobility, and there was no evidence of an interaction to suggest FAST effects on school mobility differed between families who did or did not move residences.

Discussion

This study provides a rare experimental evaluation of a social capital-building intervention hypothesized to reduce student mobility in early elementary school, a significant period when moving is particularly harmful. School mobility rates were high in our sample but consistent with previously published reports; the probability of a first-grader changing schools by third grade was nearly 40%. FAST was expected to reduce mobility due to program components that build and improve relationships between families and among families and schools. This social capital was theorized to improve families' perceptions of the school's commitment or effectiveness and increase families' identification with the school community, making them less likely to leave.

For the majority of students in our sample of predominantly low-income Hispanic schools, FAST had no effect on mobility. There was evidence, however, of heterogeneity in treatment effects. First, Black students had especially high rates of school mobility, but FAST reduced their probability of changing schools between first and third grade by 29%. This effect held up across a variety of robustness checks, and it was even larger for those who complied with the treatment and graduated from FAST. Given recent reports that Blacks are more likely to exercise school choice than other groups (Grady et al., 2010), it is possible that FAST helped reduce school dissatisfaction among Black families in our sample by building social capital between families and schools. It is also possible that Black mobility was high because Black families felt socially disconnected from families in these predominantly Hispanic schools, but FAST aided in their integration into these communities. Our evidence, though tentative given the

nonexperimental nature of the mediation analysis, favors the second explanation. The intergenerational closure promoted by FAST was particularly beneficial for Black families in terms of reducing mobility, and the CACE analyses offered further evidence that parent-parent relationships were an especially important deterrent to mobility for Blacks.

Second, although students who lived farther from their schools were considerably more likely to change schools than others, this association was significantly weaker in FAST schools. It is plausible that children who lived farther away from school were more mobile because their families were less connected with the community of families at their child's school, but FAST helped incorporate them into school networks and communities, making them less likely to move. Unfortunately, further analyses were unsuccessful in supporting this hypothesis or any other social capital-related explanation of this finding.

The heterogeneity in mobility rates among race/ethnic groups is also worth revisiting. The high rates of mobility among Blacks in this sample align with prior findings, but the high rates of White mobility and the lower rates of Hispanic mobility are atypical. Similar trends have been documented in predominantly minority schools (Nelson et al., 1996) and could be related to the schools' racial composition. Whites may exhibit higher levels of school mobility in predominantly Hispanic areas due to White flight or the types of strategic moves documented elsewhere (Hanushek et al., 2004). When viewed alongside the high mobility of all non-Hispanics in this study, another explanation is that non-Hispanic students and their families feel out of place in predominantly Hispanic schools. However, we found minimal variation in White mobility rates across schools, so our data provide no evidence on such speculation.

The experimental design of our study supports the causal claim that FAST reduced mobility for Black families in our sample. This is an important finding given the role of high Black mobility in the persistence of racial achievement gaps (Hanushek et al., 2004) and the accompanying long-term consequences of these achievement gaps for Black students' later schooling, occupational, and labor market outcomes compared to their White and Asian counterparts (Jencks & Phillips, 1998; Magnuson & Waldfogel, 2008). Whether parent-parent social capital is the true causal mediator of this effect is less certain. Because the mediators we tested were not randomly assigned, unobserved factors that affect both the mediator and school mobility could lead to bias. There is no way to rule this out, but our results did hold after controlling for pretreatment measures of our mediators. Ultimately, intensive qualitative research may be required to uncover the reasons families change schools and to understand why programs like FAST have heterogeneous effects.

While our results are illuminating, there are important limitations. Data constraints prevent us from drawing stronger inferences about why FAST

decreased school mobility for Blacks or why it failed to reduce mobility for other students. Our sample is not representative of schools nationally, and the 60% of consenting families may not be representative of all families in these schools, so we encourage future research to examine the generalizability of these findings to other contexts; if similar school-based programs can promote social capital among parents within a school community and reduce mobility, this could benefit many students and schools. It may be important to examine the timing of school moves as well; convincing families to delay school changes until the summer could be beneficial, but we are unable to identify the timing of moves in our data. On a related note, it is unclear whether these findings would hold if we examined mobility over a longer period of time. FAST may have simply delayed the mobility of Black students, a short-term effect that could fade over time. Conversely, FAST could reduce mobility beyond third grade if the effects of social capital accumulate over time. We are also unable to differentiate between the two types of noncompulsory moves—strategic and reactive—as we do not know why children changed schools. School moves may have been beneficial for some students and harmful to others, but the high rates of mobility in our sample were almost certainly disruptive to schools. Understanding the different motivations behind school moves is a next step toward understanding how schools or policymakers could address student turnover.

To conclude, school mobility is an important outcome to be studied in its own right, and very little published research has examined efforts to curtail it or mitigate its negative consequences (Alexander et al., 1996; Kerbow et al., 2003; Nelson et al., 1996). Our study provides rigorous evidence that building relationships between and among families and schools may significantly reduce mobility for Black students in predominantly Hispanic schools. It is possible that these types of interventions also reduce mobility for other groups of students in schools with different racial/ethnic compositions, which we encourage future work to explore. We also encourage researchers studying the effects of educational programs and reforms to examine their impact on school mobility, as this is only the second experimental study to test ways in which schools can reduce student turnover (Fleming et al., 2001). Finally, we urge researchers to move beyond simply exploring the effects of mobility and to examine its causes as well as potential ways to prevent unnecessary and harmful moves or to mitigate their negative consequences. Social capital theory may be a critical element in these pursuits.

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¹We use the terms *student mobility* and *school mobility* interchangeably to refer to any noncompulsory school change a student makes over the time period studied.

²In this study, a trained team of parents and professionals recruited participants and led FAST sessions for families of first graders in each treatment school for 8 weeks.

³Given the large number of schools participating in the study over the two sites, a staggered implementation was necessary. Two consecutive cohorts of first graders were each divided between three seasons (fall, winter, spring). Schools were selected to have at least 25% of students from low-income families and 25% of Hispanic origin. More details about the randomized controlled trial (RCT) design and implementation are available upon request.

⁴Students retained in first or second grade were also included.

⁵Schools in San Antonio were blocked by school-level percentage free and reduced-price lunch. The two blocks were within one large school district.

⁶Multiple imputation is the preferred method of handling missing data among many researchers, but our results are unlikely to depend on the particular strategy used. No students were missing the outcome variable or treatment indicator, there were low levels of missingness on imputed covariates, and findings were practically identical when we used listwise deletion.

⁷Interactions and variable transformations were created prior to imputation. School fixed effects were included in imputation models to address the multilevel nature of the data. Analyses include indicators for students missing pretest or demographic variables.

⁸There was no evidence of cohort and season of implementation effects or interactions.

⁹We also estimated models allowing for randomly varying slopes for key student-level predictors (race, free/reduced-price lunch). Only the slope for “Black” varied across schools, but allowing for this variation did not change any of our conclusions, and we were unsuccessful in explaining much of this variation, so these results are not presented.

¹⁰Testing mediators in logistic regression analyses requires special techniques. Logistic regression coefficients are scaled relative to the unobserved variance in the outcome, which changes when covariates are added to a model. The solution is to explain the same amount of variability across all models so that changes in coefficients are due solely to their relationships with mediators (Kohler, Karlson, & Holm, 2011). We use the `-khhb-` program in Stata to conduct our mediation analyses.

¹¹This model excludes the student-level covariates that were irrelevant to the Black sample (the race dummies and language variables), as well as several school-level covariates because of the lower statistical power resulting from a smaller sample. The findings hold when the school-level variables are included, but standard errors are larger.

¹²We examined the robustness of our findings to the specification of compliance. Our preferred model defines compliance in terms of the FAST program’s official definition of “graduation” as attending at least six of the eight weekly sessions. Using lower cut-offs such as two or four sessions yields higher compliance rates and produces qualitatively similar but less precise results than those presented here.

¹³Although the especially high rates of mobility for Black would-be compliers seem odd, they were robust across different specifications of both the compliance and outcome models. It is also important to keep in mind that there were only about 15 Black compliers in each treatment condition, so 14 or 15 of them moving is not implausible given the high mobility of Black students.

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