Social Capital and Student Achievement: An Intervention-Based Test of Theory

Adam Gamoran, William T. Grant Foundation
Hannah K. Miller, Civilytics Consulting
Jeremy Fiel, Rice University
Jessa Lewis Valentine, DVP-PRAXIS LTD

An earlier version of this paper was presented at the annual meeting of the American Educational Research Association, Washington, DC, April 2016. The authors express their appreciation to Lynn McDonald, Ruth López Turley, Carmen Valdez, and many other partners in the larger project from which this paper is drawn. We are also grateful for constructive feedback from the editor of this journal and three anonymous referees. Research on the paper was supported by grants to the University of Wisconsin-Madison from the National Institute of Child Health and Human Development (grant no. 1R01HD051762-01A2) and the Institute of Education Sciences (grants no. R305C050055 and R305B090009). Findings and conclusions in the paper are the responsibility of the authors and do not necessarily represent the views of the supporting agencies.

Corresponding author: Adam Gamoran, William T. Grant Foundation, 60 East 42nd Street, 43rd floor, New York, NY 10165. Email: agamoran@wtgrantfdn.org Phone: 212-752-0071

Social Capital and Student Achievement: An Intervention-Based Test of Theory

Abstract

Social capital is widely cited as benefiting children's school performance, but close inspection of existing research yields inconsistent findings. Focusing on intergenerational closure among parents of children in the same school, this paper draws from a field experiment to test the effects of social capital on children's achievement in reading and mathematics. When children were in first grade, their schools were randomly assigned to an after-school family-based intervention that boosts social capital. A total of 52 schools in Phoenix and San Antonio, containing over 3,000 first graders, participated in the study, with half of the schools in each city assigned to the treatment group and half serving as no-treatment controls. Two years later, no differences in third grade achievement were evident between children who had been in treatment and control schools. By contrast, non-experimental analyses of survey-based measures of social capital suggested positive effects on achievement, indicating that naïve estimates based on survey measures may be upwardly biased by unobserved conditions that lead to both stronger ties among parents and higher test scores. This paper adds to a growing literature that raises doubts about the effects of this type of social capital for achievement outcomes among young children.

Social Capital and Student Achievement: An Intervention-Based Test of Theory

The relation between social capital and student achievement has long been of interest to sociologists of education, especially since Coleman (1988) theorized that social capital plays a key role in the cultivation of human capital in children. Much of the attention has centered on the concept and potential benefits of intergenerational closure, the degree to which children belong to closed social networks that include their parents, their friends, and the parents of their friends (Coleman 1988, 1990). When parents can surround their children with social networks characterized by trust and shared expectations, according to this view, they may be better positioned to share valuable information and to establish norms that help their children succeed in school.

A sizeable literature points to the promise of social capital for improving children's achievement, and multiple studies demonstrate a positive relationship between measures of intergenerational closure in particular and schoolchildren's academic performance (Carbonaro 1998; Dika and Singh 2002; Pong 1998; Sun 1998, 1999; Israel, Beaulieu and Hartless 2001; Kao and Rutherford 2007; Menes and Donato 2015). However, a growing evidence base casts doubt on this positive relationship, pointing to the correlational nature of existing evidence (Durlauf 2002; Mouw 2006; Morgan and Sørensen 1999; Pribesh and Downey 1999; Morgan and Todd 2009; Freeman and Condron 2011; Geven and van de Werfhorst 2020).

At the heart of these conflicting findings is a thorny empirical challenge that is intertwined with social capital's theoretical underpinnings. Social capital theory is notoriously difficult to operationalize, as the theory itself posits that people often selectively form relationships in the pursuit of anticipated resources or opportunities (Bourdieu 1986). Hence, it is hard to disentangle whether social capital promotes favorable educational outcomes, or whether resources and attributes that promote educational success attract more social capital (Dika and Singh 2002; Durlauf 2002; Mouw 2006). Prior studies of social capital effects on students' achievement rely on observational data and are thus vulnerable to selection bias, offering little purchase to discern the causal direction of the relation between strong social networks among families and children's performance in school (Dika and Singh 2002).

This study offers a novel perspective on the social capital debate, by using a school-randomized field experiment to stimulate social capital development and test for effects untainted by selection bias. Schools with large enrollments of low-income, Hispanic students were randomly assigned to an intervention—FAST (Families and Schools Together)—designed to boost social capital among families and between families and schools. Earlier analyses of these experimental data provide strong evidence that FAST strengthened the structure of parent-parent social networks as well as the quality of relationships among parents of first grade students within schools (Rangel, Shoji, and Gamoran 2020). We examine the effects of this social capital-building intervention two years later, on students' reading and mathematics achievement test scores in third grade.

Whereas most studies of the relation between families' social networks and children's academic outcomes have focused on middle or high school students, our study examines these relationships among families of elementary school children. Implementing the intervention in first grade leverages a key time in the educational career, when families are just beginning to form relations with other parents in the school community, and when parents have a substantial influence on children. Examining achievement in third grade focuses on academic growth in the key early childhood years while also giving social capital time to operate—e.g., by building relationships that facilitate information exchanges, norm development, interpersonal support, and social control. Because the multi-year, multi-family intervention was randomly assigned, the analysis can distinguish the effects of social capital from unobserved characteristics of families or schools that may both enhance connections among parents and children in the same school and boost children's achievement. Our paper adds new evidence to the debate over the relation between social capital and children's academic achievement, and our approach offers a powerful way to test a theoretical construct central to the sociology of education.

Social Capital and Schooling Outcomes

Scholars have explored the role of social capital in children's school success for more than three decades (Coleman 1988; Furstenburg and Hughes 1995; Bankston and Zhou 2002; Dika and Singh 2002; Dufur, Parcel, and Troutman 2014; Rodriguez Menés and Donato 2015; Putnam 2015; Dufur et al. 2016). The concept of social capital rose to prominence among sociologists in the 1980s with the contributions of Pierre Bourdieu (1986) and James Coleman (1988), who offered two distinct yet related theoretical frameworks (Portes 2000). Although both frameworks highlight the benefits of social ties for individuals or groups, Bourdieu's conceptualization of social capital is based in theories of social reproduction that emphasize not only the structure of networks, but also the quality of resources inhering in networks as well as an individual's ability to access those resources. For Bourdieu, social capital can accentuate social stratification because those with better access to resource-rich networks, and those with more power to access those resources within networks, can obtain greater benefits. In contrast, Coleman's conceptualization of social capital is based in rational choice theory and emphasizes the qualities of shared information, trust, and enforcement of norms that characterize these networks. Yet Coleman's approach also has functionalist roots, in that he defined social capital "by its function" as it "facilitates certain actions of individuals within the [social] structure"

(Coleman 1990, p. 302). For Coleman, social capital can mitigate stratification as social networks allow the resources of the network to benefit the variety of individuals who share in the network's ties. According to this view, social capital not only complements other forms of capital, but can actually help build human capital in circumstances where economic capital is in short supply (Coleman 1988).

Early on, research on social capital in education largely followed Coleman's approach (Dika and Singh 2002), likely because of Coleman's emphasis on the importance of families and schools for social capital effects on children, and because of his own empirical research on the topic (Coleman and Hoffer 1987). Coleman's concepts and measures remain prevalent in this field, and our approach is likewise largely consistent with Coleman's approach. His conception is well suited to our inquiry in light of our interest in whether it is possible to intentionally build social capital among families within schools, and by doing so, to enhance the school performance of children from marginalized communities. However, like other recent studies, our approach responds to a salient critique of Coleman's conception: his defining social capital by its function would preclude empirical assessment of its impact, because if the definition of social capital is that it "facilitates certain actions," one cannot *test* whether social capital is causally implicated (Portes 1998; Durlauf 2002). Instead, we distinguish between indicators of social capital such as the size, structure, and quality of relationships in a social network, and its potential payoff for child development (Sampson, Morenoff, and Earls 1998).

Thus in our conception—drawn largely from Coleman but avoiding the tautological logic of defining social capital by its function—when parents and educators belong to strong social networks characterized by trust and shared expectations for children, they can communicate more effectively and enforce norms that can help children succeed in school. A widely studied indicator of social capital, and a primary focus of this paper, is “intergenerational closure,” the extent to which children are embedded in closed social networks consisting of their friends, their parents, and the parents of their friends (Coleman 1988, 1990). Commonly measured by asking parents how many of the parents of their children’s friends they know (Carbonaro 1998), intergenerational closure is a marker of network structure, signaling both size and closure (Geven and van de Werfhorst 2020).[1] Other aspects of social capital may include the quality of relationships among parents of children in the same school, such as the degree of trust and shared expectations and the extent to which parents regularly interact (Bryk and Schneider 2002; Gamoran et al. 2012).

How might social capital among families in a school elevate children’s chances of academic success? Key mechanisms proposed in past research include a flow of useful information and social control. When families are isolated, they may lack access to information about how to help their children succeed in school. Information that flows through a social network enables parents to help their children, whether by advocating for teachers who provide the best support, understanding teachers’ expectations, or aiding children with homework (Horvat, Weininger, and Lareau 2003; Putnam 2015). Additionally, when parents are connected with other parents, they can act collectively to set and enforce norms for their children’s behavior (Carbonaro 1998). From nutrition to bedtime expectations to keeping up with schoolwork, establishing collective norms makes enforcing rules less stressful and more taken for granted.

For young children, who are the focus of this investigation, social capital derived from networks that include parents may be especially consequential for children’s schooling outcomes. Most studies of the relations between parents’ and families’ social networks and children’s academic outcomes have focused on middle or high school students. Yet as others have argued (Freeman and Condron 2011), parents' social networks may play an especially important role in early childhood before peer effects become more salient and influential as children enter adolescence.

Does Social Capital Boost Achievement?

Much of the research on social capital among families and its relation to children's achievement relies on data from a national survey of the high school class of 1992, which began with eighth graders in 1988, and which was the first large U.S. panel survey of youth to collect information on intergenerational closure (measured via parent surveys) and student achievement (measured with standardized achievement tests in math, reading, science, and history). Analyses of this survey, known as the National Educational Longitudinal Study of 1988, have reached contradictory conclusions. Despite using the same data, some studies have reported a positive relationship between intergenerational closure and students' achievement scores, particularly in math, while others have not. Analyses that focus on achievement growth (Carbonaro 1998; Morgan and Sørensen 1999; Pribesh and Downey 1999; Kao and Rutherford 2007) less often find positive effects compared to those that focus on differences in achievement at a point in time (Carbonaro 1998; Sun 1998, 1999; Israel, Beaulieu and Hartless 2001; Kao and Rutherford 2007). Research with more recent data seems to reflect the same pattern. For example, cross-sectional analyses of the 2006 Program for International Student Assessment (PISA) survey data exhibit positive associations between social capital and cognitive performance in science (Rodriguez Menés and Donato 2015), whereas longitudinal analyses of a U.S. national survey from the high school class of 2004 found no association between intergenerational closure and math score gains for public school students, although there was a positive association for Catholic school students (Morgan and Todd 2009). And using data on adolescents in Germany and the Netherlands, Geven and van de Werfhorst (2020) observed positive associations between intergenerational networks and school grades when comparing among students, but within-person estimates that controlled for all time-invariant unobserved confounders of closure and grades yielded null effects.[2]

Of particular note for the present study, which is focused on elementary school children, two studies using the Early Childhood Longitudinal Study—Kindergarten cohort (ECLS-K) also found no relationship between intergenerational closure and achievement gains among children in kindergarten and first grade (Condron 2009; Freeman and Condron 2011).[3] Freeman and Condron concluded that their study "adds to the mounting research suggesting that [intergenerational] closure is not an important predictor of students' learning (p. 541)." However, another study using the ECLS-K reported a significant and positive effect of intergenerational closure on reading gains (but not math gains) for a subset of children living in stepfamilies (Shriner, Mullis, and Schlee 2009).

In sum, while numerous studies suggest parent-parent social capital is beneficial to student achievement (Carbonaro 1998; Dika and Singh 2002; Pong 1998; Sun 1998, 1999; Israel, Beaulieu and Hartless 2001; Kao and Rutherford 2007; Menes and Donato 2015), other studies have surfaced unease with this conclusion, pointing to the correlational nature of existing evidence on social capital in general (e.g., Durlauf 2002; Mouw 2006) and intergenerational closure in particular (Morgan and Sørensen 1999; Pribesh and Downey 1999; Morgan and Todd 2009; Freeman and Condron 2011; Geven and van de Werfhorst 2020). With observational data, it is difficult to discern whether a positive association reflects an effect of social capital on schooling outcomes, or whether relationships among families thrive in schools where children are performing well (Dika and Singh 2002). Our study design responds to this limitation.

Research Questions, Study Design, and Methods

The most compelling design to test the causal effects of social capital would involve random assignment of families to varying conditions of social capital. Of course, this is neither feasible nor desirable. Instead, we adopted an intervention approach by randomly assigning schools to an intervention that strengthens relations among families within schools (Fiel, Shoji, and Gamoran 2015). The school-based intervention provides an exogenous "shock" to social capital that is independent of pre-existing relationships among families and prior levels of student academic performance. If the intervention is successful and conditions of random assignment are met, social capital theory as expressed by Coleman (1988) predicts that the intervention would positively affect schooling outcomes. Positive intervention effects would strengthen the credibility of social capital theory, whereas null or negative effects would challenge the theory.

While field-based experiments to test theory are relatively rare in sociology, they are increasingly common in economics (Duflo 2007). For example, Heckman's model of the importance of human capital investment in young children has been corroborated by experimental studies of high-quality early childhood education (Cunha and Heckman 2007). Likewise, the recent National Academies of Sciences, Engineering, and Medicine (NASEM 2019) report on a "roadmap to reduce child poverty" relies substantially on field experiments to show that children who grow up in poverty suffer in their cognitive and social development, and that tax and workforce interventions will reduce child poverty and improve both short-term developmental outcomes and long-term economic results, consistent with theories from psychology and economics. In a similar vein, Bannerjee and Duflo (2009) have crafted a renowned line of scholarship in development economics in which interventions put economic theory to the test.

Like human capital, social capital is a complex phenomenon that cannot be captured in a single intervention. But as economists do for human capital theory, sociologists can design an intervention whose effects can be predicted by social capital theory, and then "make direct comparisons between observations and the predictions of a theory" (Smith 1989 p.153) to interpret the results. This is our aim in the present study.

We recognize that findings *consistent with the predictions* of social capital theory will not *prove* social capital theory. Our approach responds to an endemic problem of social capital research in education (the difficulty of distinguishing cause from effect), but it does not solve all problems. Notably, the complexity of any attempt to build social capital in the field leaves open the possibility that results may reflect other aspects of the intervention in addition to the components intended to build social capital. As a test of social capital theory, therefore, our approach is not as strong as if we could actually randomize social capital itself. Nonetheless our study demonstrates the advantage of the intervention approach over prior work in the sociology of education. Prior research has often represented social capital with proxy measures such as family structure, social mobility, and extracurricular participation, which are at least as removed from social capital (if not more so) *and* which may reflect unobserved correlates that predict both the social capital proxy and school performance. By contrast, our approach offers more confidence that we are studying social capital effects because (a) the intervention was randomized; (b) qualitative research at an early stage of the study documented how the intervention builds social capital in ways that are consistent with social capital theory (Shoji et al. 2014); and (c) the focal intervention has demonstrated effects on widely accepted social capital indicators such as intergenerational closure and relationship quality among families (Rangel, Shoji, and Gamoran 2020).

Conversely, a finding of null or negative intervention effects would align with empirical studies that have raised doubts about whether social capital elevates achievement among young children (Condron 2009; Freeman and Condron 2011). Null or negative effect estimates, however, could be vulnerable to suppressor effects if negative intervention effects on achievement obscure positive social capital effects. It is therefore important to choose an intervention that not only builds social capital among families within the same school, but also has a documented record of positively enhancing child development, a consideration we have fulfilled in this study.

Following the logic of our intervention approach, our primary research question about social capital and achievement is an experimental one:

> **Research Question 1** (Experimental): Are third-grade test scores higher in schools that were randomly assigned to the social-capital-building intervention in first grade?

A limitation of the intervention approach is that not all families participate in the intervention that is offered to their schools. For this reason we supplement our primary analyses based on experimental data with a set of quasi-experimental analyses that test whether actual participation in the intervention improves achievement outcomes. The quasi-experiment uses random assignment to the intervention as an instrument to test the effects of participation. Its ability to yield causal estimates rests on the assumption that effects of random assignment could only occur via participation in the intervention, a reasonable assumption in this case. A quasi-experimental approach allows us to test whether students who actually participated in the intervention, and thus were most likely to have their social capital boosted, had improved academic outcomes. Consequently we pose a second, quasi-experimental question:

Research Question 2: (Quasi-experimental): Are third-grade test scores higher among students who actually participated in the social-capital-building intervention?

The Intervention: Families and Schools Together

To carry out the research, we selected a widely-used family-school engagement program known as Families and Schools Together (FAST). Designed to build relationships among parents, between parents and school staff, and between parents and children, FAST places children in an ecological context in which they are embedded in supportive social networks. FAST has a record of successfully engaging disadvantaged parents (McDonald et al. 2006; Caspe and Lopez 2006). Distinctive features include (a) a leadership team of parents, school staff, and professionals who are demographically representative of the local community; (b) adaptation of the program to the local cultural context; and (c) active outreach to draw parents and children into FAST. FAST consists of 8 weekly sessions led by a FAST leadership team in each school, followed by two years of monthly follow-up activities led by parents. In each city, a social service agency with past FAST experience implemented the FAST program for this study, supported by the national FAST non-profit organization.[4] FAST trainers used the FAST Program Integrity Checklist in each site to ensure that the program was implemented with fidelity (Shoji et al. 2014).

Elements of FAST. The 8 FAST sessions occur in "hubs" of 5-10 families and last approximately 2.5 hours each week, usually beginning in the late afternoon and encompassing the dinner hour. Sessions are led by the trained leadership team, and all family members are invited to attend.[5] The first activity of the first session consists of constructing a "Family Flag," in which children and parents talk about what symbols reflect their family and explain those symbols to other families. For the next activity, which is also the first activity in succeeding weeks, families share a meal together. Directed by their parents, children serve the food and clean up after the meal. After the meal, family members play games with one another at their family tables, as parents lead their children in taking turns, listening, and sharing feelings. This period also includes the "FAST Hello," when family members introduce themselves to other families, and communal singing of the "FAST Song." As Turley et al. (2017, p. 209) remarked, while the first activities of the evening are designed to strengthen relationships between parents and their own children, they "also contribute to trust, shared expectations, and shared values across families and between families and school staff. Singing together, playing games, and sharing a meal comprise a positive, shared experience for families and school staff, providing a basis for the development of relationships and reducing anxiety about the school context. Because families take turns providing meals for the entire FAST hub, they develop reciprocity in their relationships with each other, which engenders trust across the social network."

After the family meal and games, activities move towards building relationships among families, supporting intergenerational closure and reciprocity in the school community. Children move to a separate area to play and complete homework, while parents have "Buddy Time," a 15 minute period during which two parents from different families take turns talking with one another in a structured dialogue. Each has seven uninterrupted minutes to speak about his or her day. Parents are asked to listen without giving advice or passing judgment, which helps build trust and shared values within new friendships. Then, parents gather together for "Parent Group Time," joined by a leadership team member who participates as little as possible while parents are encouraged to take the lead in talking with one another. The aim of Parent Group Time is to create a peer network among parents to provide support for their children's success. This sets the stage for monthly meetings of parent-led activities following the end of FAST.

The next activity is "Special Play," which consists of 15 minutes of one-to-one parent-child interaction during which the child takes the lead in playing, and the parent devotes his or her full attention to the child. Parents are asked to follow four guidelines during Special Play: "(1) Don't boss. (2) Don't teach. (3) Don't judge. (4) Follow the child's lead." (Parents are also asked to repeat Special Play at home in between FAST sessions.) Finally, the entire group reassembles for closing activities that further strengthen relationships among families, including announcements about school and community activities, birthdays, and other special events, and a final communal activity such as creating a "rain storm" by snapping, clapping, and foot-stomping. As recounted by Turley et al. (2017, p. 210), "these closing activities are designed to reinforce togetherness through a routine and positive group experience."

FAST and social capital. Qualitative and quantitative evidence reported in previous papers based on these same data confirms that FAST triggered improvements in social capital in these school communities. In an interview-based qualitative study carried out in conjunction with the randomized trial, Shoji et al. (2014) identified four theory-based mechanisms through which FAST helped trigger social capital development among participating families. Two mechanisms were particularly salient in the Parent Time activity: responsive communication, in which parents reacted with interest or enthusiasm to other parents; and reciprocal communication, characterized by give and take among parents. These communication patterns helped establish a sense of bounded solidarity and reciprocity, conditions identified as supportive of social capital development (Portes 1998). Other mechanisms included the shared experience of attending FAST and engaging in its activities and rituals, and institutional linkages fostered with the school and community partners via the members of the FAST leadership team.

Likewise in the qualitative portion of a mixed-methods study, Rangel, Shoji, and Gamoran (2020) found that FAST not only helped parents get to know other parents more quickly than they would have otherwise, but that it also enabled parents to determine one another's trustworthiness, express care and respect for one another, and demonstrate reciprocity through mutual support. They concluded that FAST affected not only the structure of parent networks but the quality of relationships within the network.

Quantitative analyses of the randomized design have further established that, in these same data, FAST strengthened the ties among families (Gamoran et al. 2012; Shoji 2014; Turley et al. 2017). For example, intent-to-treat estimates yielded significant FAST effect sizes of 0.21, 0.14, and 0.11 for measures of intergenerational closure, shared expectations, and reciprocity in relationships among parents, respectively (Turley et al. 2017). Complier average causal effect estimates for students who completed FAST were even larger, with effect sizes of 0.54, 0.40, and 0.36 respectively.[6] Although FAST's effects on network structure (intergenerational closure) diminished over time, effects on the quality of relationships among parents (shared expectations and reciprocity) persisted through third grade: Rangel, Shoji, and Gamoran (2020) found that for intergenerational closure, a first-grade ITT effect size of 0.259 declined to 0.138 by third grade, but an effect of 0.167 in relationship quality increased to 0.199 by third grade.[7]

Moreover, in these data, children in schools assigned to FAST exhibited fewer internalizing behavior problems during first grade after the intervention, and these effects were pronounced for those who completed the FAST program (Turley et al. 2017). The question for the present study is whether FAST, as an engine of social capital, affected academic outcomes in third grade. Earlier randomized trials had indicated positive effects on at-risk children's behavioral and academic outcomes (Layzer et al. 2001; McDonald et al. 2006; Kratochwill et al. 2004, 2009). These prior studies, however, recruited only at-risk students, randomized at the student (or, in one case, the classroom) level, and measured achievement using teachers' subjective judgments of students' academic performance. Ours was the first study to recruit students universally, randomize at the school level, and assess performance on standardized tests, design elements that were key to assessing FAST as a driver of social capital effects.[8] For this paper, FAST was implemented in first grade and the follow-up ended when students were in third grade, the first year of statewide achievement testing.

Study Design

A total of 52 schools participated in the study, evenly divided between Phoenix and San Antonio. We selected these cities because they had social service agencies with experience implementing FAST, and because they had many schools with high proportions of low-income, Latino students, which was a special interest of the study in light of past research marking low-income Latino immigrant families' isolation from school communities (e.g., Valenzuela 1999). In Phoenix, three small districts participated, and in San Antonio, schools from one district were blocked into two groups based on proportions of students eligible for free and reduced-priced lunch. Each of these five units constituted blocks for randomization, and within each block, half the schools were assigned to treatment and half to control. Because the intervention was labor intensive, we further randomized schools to a two-year implementation cycle, with half of the treatment schools initiating FAST in 2008-2009, and half in 2009-2010.

A total of 3,084 families enrolled in the study, reflecting participation rates of about 60% in both treatment and control schools. Incomplete participation in the intervention may limit the generalizability of our findings but does not introduce bias into the treatment-control comparison. None of the families in control schools was exposed to FAST during the period of our study. Because assignment to FAST was by school, we have no reason to think that a family in a control school was affected by the assignment of other families to FAST, and vice versa. Likewise, the assignment of schools to the treatment group did not affect resource allocation patterns or other potential differences between treatment and control schools within districts. Of the families that consented to the study in treatment schools, 73% attended FAST at least once, and the average family attended 4 sessions (Shoji et al. 2014). Data collection continued through 2010-2011 and 2011-2012 respectively, following children from first to third grade. All 52 schools and 70% (2,165/3,084) of the children from the original sample participated in the third grade follow-up.

Measures. The dependent variables for this paper consist of student scores on high-stakes tests of reading and mathematics in third grade.[9] For each test, we use two measures. First, we obtained from districts students' scale scores, which we standardized—by state, test subject, and language in which the test was administered[10]—using the score mean and standard deviation of students in control schools. These scores capture the full range of student performance, but combining cases across states, years, and languages assumes the distribution of student scores is the same within each of these groups. We have no reason to expect otherwise within states, but we are less certain of the homogeneity of distributions across Texas and Arizona.

Second, districts provided a binary indicator of whether a student—based on his/her scale score—met reading and math proficiency standards as defined by the state. Although these proficiency scores convey less information about the range of achievement, they have two advantages. First, the value of "proficient" is common across states, and because state tests in Arizona and Texas had similar relations to scores on the National Assessment of Educational Progress, proficiency levels in the two states represent similar absolute levels as well (Bandeira de Mello, Blankenship, and McLaughlin 2009). The proficiency measure provides insight into changes in achievement for students who are on the margins of meeting statewide standards. Second, the data on proficiency levels are more complete than the scale scores. Approximately 14% of scale scores provided by districts were outside the range of legitimate scale scores for a particular test and are thus likely misreported.

Control variables included student race/ethnicity, gender, free or reduced price lunch status, English language learner status, and special education status drawn from district records. Language use was indicated by whether parents chose to respond to baseline surveys in English or Spanish.

Measures of social capital are consistent with prior studies and derive from parent surveys administered both prior to treatment and approximately six weeks post-treatment. The structure of social relations is measured by intergenerational closure, using the familiar measure of how many parents of their children's friends at this school parents know (Coleman 1988). The indicator is coded 0-6 for responses of "none" to "six or more." For some models we dichotomize the variable as 0 for none or one, and 1 for more than one parent known, based on the idea that, while being connected to at least a few parents may be very important, connections to additional parents may have diminishing returns. Freeman and Condron (2011) also ran supplemental models with intergenerational closure coded dichotomously; they reported little difference in results. Measures of the quality of social relations include indicators of the extent to which parents at the school interacted with one another in areas such as babysitting, carpooling, and sharing meals together. Parents responded to six items measured on a four-point scale from “not at all” to “a lot”; the items were summed and then the measure was standardized to have a mean of 0 and standard deviation of 1. In addition, the data include a single-item indicator of shared expectations among parents, measured by responses to a 1-4 scale on how much “Other parents share your expectations for your child”; response options were “none,” “a little,” “some” and “a lot.” Table 1 provides means and standard deviations of all variables. For some of our supplementary analyses, we also use factor analysis to create a composite parent-parent social capital scale that uses all of these items.

Models. We weighed several options to best match the analytic approach to the study design. The preferred approach is one that makes the fewest assumptions: simply examine the experimental effect of FAST assignment on student achievement outcomes to answer the first research question of whether third-grade test scores are higher in schools assigned to the social-capital-building intervention. Prior analyses have established that FAST primarily improved the quality and quantity of parent-parent relationships, which is not surprising given that FAST programming focused heavily on these relationships. Hence, we first estimate intent-to-treat (ITT) models that compare students who attended schools assigned to FAST to students in control schools. This approach minimizes potential problems of systematic selection among individuals into FAST participation as well as interference between treated and untreated units, which are in this case schools. Because schools were the unit of assignment to treatment, we fit two-level mixed-effects linear regression models with students clustered by the schools they attended in first grade; for dichotomous outcomes, we substitute logit models. Our main analyses use standard listwise deletion approaches (Acock 2005; Allison 2001). We also show results with multiple imputation in which we impute the independent variables using the mi impute chained procedure in Stata (StataCorp 2019).

All models include randomization controls; results are shown with and without additional pre-treatment controls. Although comparison of treatment and control schools revealed no differences in school composition, size, or average prior achievement (Gamoran et al. 2012), we did observe differences in pre-treatment social capital measures favoring control schools (i.e., study enrollment rates were higher among disconnected families in the treatment group; see Appendix Table A-1 and Turley et al. 2017). Controls for pre-treatment social capital adjust for this discrepancy and improve precision in the estimates.

Individuals in treatment schools were encouraged to participate in the FAST intervention, but they did so to varying degrees. Hence, we also estimate treatment-on-treated (TOT) models that account for individual students' participation. One model considers the "dosage" (number of FAST sessions) that students received, and another considers whether students "graduated" from FAST, where "graduation" is defined as attending 6 or more of the 8 FAST sessions. We estimate the TOT models using an instrumental variables approach (two-stage least squares) wherein the endogenous variable (either FAST dosage or FAST graduation) is instrumented using FAST-by-randomization block interactions (which are exogenous). This could help us detect treatment effects that might be suppressed by imperfect compliance and answers the second research question of whether third-grade test scores are higher among students who actually participated in the social-capital-building intervention. Again, we fit these models with and without pre-treatment controls.

An important question motivating this study was whether results from prior observational analyses of the effect of social capital on achievement might be upwardly biased by unobserved conditions that lead to both stronger ties among parents and higher test scores. Thus, we re-analyzed our data drawing on observational measures of social capital commonly used in non-experimental studies to examine how those results compare to our experimental findings. To further explore this contrast, we also briefly note findings from two quasi-experimental strategies that incorporate measures of social capital drawn from parent surveys. One applies a causal mediation analysis (Imai, Keele, and Tingley 2010) to estimate the portion of FAST ITT effects explained by a parent-parent social capital scale that combines the aforementioned parent-parent social capital measures. The other uses school-level treatment assignment (FAST-by-randomization block interactions) as instrumental variables to estimate the effects of individual students' post-treatment parent-parent social capital, using the same parent-parent social capital scale. Both of these supplementary specifications include pre-treatment social capital controls. These approaches entail more restrictive assumptions than the experimental approach. Causal mediation assumes sequential ignorability for the treatment (assignment is independent of mediator and outcome, a plausible assumption given random assignment) and for the mediator (a more challenging assumption that rests on inclusion of pre-treatment controls for the mediator). The instrumental variables approach requires the exclusion restriction, that FAST effects operate only through the social capital indicator, a challenging assumption given the rich, multifaceted nature of FAST and social capital. Still, these alternative specifications can help us make sense of differences that may emerge between experimental, quasi-experimental, and observational results.

Finally, we conduct additional analyses to assess the sensitivity of our findings to modeling assumptions. First, we check the sensitivity of our main results to survey nonresponse by introducing weights designed to adjust the sample for nonresponse to the third grade follow-up survey. The weights were calculated by predicting nonresponse using multilevel logistic regressions; the resulting models weight individual students by the inverse of their response propensity. Students who are missing item-level data on predictors included in the nonresponse model do not have nonresponse weights.

Second, we use a Heckman selection correction model to examine whether our results are robust to concerns about missing outcome data (Guo and Fraser 2010). Third-grade achievement data are only available for students who reach third grade two years after first grade and are attending school in their original district. Any students who were retained in first or second grade would have missing data on third-grade achievement, yet retention may be endogenous to the intervention (Fiel, Shoji, and Gamoran 2015). The selection model adjusts for missing third-grade test scores. Demographic variables and pre-treatment social capital are used to predict both achievement and selection, while the proportion of students at the school level who made adequate yearly progress (AYP) in math and reading in the prior year is used to predict achievement only, and randomization design indicators are used to predict selection only.

Results

Table 2 provides the main results for achievement in mathematics and reading. No matter whether the dependent variable is measured as the standardized test score or proficiency level, and whether we examine the ITT or TOT models, FAST exhibits no effects on test score outcomes. In the ITT analysis of math standardized scores, the effect size is -0.01 without pre-treatment controls and -0.03 with pre-treatment controls. The TOT effect sizes for math are -0.01 and -0.06 for the dosage models and 0.03 and -0.06 for the graduation models, respectively. In reading, the effect sizes are 0.05 without pre-treatment controls and 0.04 with pre-treatment controls in the ITT models, and the TOT effect sizes are 0.14 and 0.06 for the dosage models and

0.20 and 0.07 for the graduation models, respectively. None of these coefficients is statistically significant. For the indicators of whether students did or did not meet proficiency standards, the effect sizes range from -0.01 to 0.10 and, again, none is significant.

Re-analysis using imputed data for missing pre-treatment control variables yields virtually identical results, as seen in Appendix Table A-2. Moreover, interactions by race, ethnicity, gender, and free or reduced price lunch status are also insignificant (not shown). Based on these results, our analyses indicate that FAST—and by implication, social capital—has no effect on third-grade achievement.

Contrast with Analysis Based on Observational Data

Past research on social capital effects on achievement, particularly effects of intergenerational closure, has yielded inconsistent results. In reviewing this literature we detected a tendency for more rigorously controlled analyses to yield smaller or no effects, though the pattern was not universal. To consider this issue more fully we re-analyzed our data using measures of social capital drawn from parent surveys instead of the randomly-assigned FAST treatment. Table 3 reports these results. In contrast to our experimental findings, and yet consistent with some of the observational studies, this analysis yields statistically significant, positive social capital effects. Effects of the combined social capital scale are significant only in mathematics, with a small standardized effect of 0.07 with pre-treatment controls included in the model. Effects of the reciprocity scale alone are similar. Effects of shared expectations are considerably larger, however, as are effects of intergenerational closure when specified as a dichotomous contrast between parents who know zero or one parent of their children's friends, and parents who know two or more parents. Moreover, effects of shared expectations and intergenerational closure appear in both reading and math. Once again, the results are very similar for models that impute missing pretreatment control data (see Appendix Table A-3).

How are we to understand the positive effects of the survey social capital measures in contrast to the absence of experimental effects? The most likely explanation is that the survey measures are vulnerable to selection bias derived from unobserved predictors of both parent-reported social capital and student achievement. To further consider this explanation, we next examined causal mediation analyses that place the survey measures as mediators of FAST ITT effects on standardized achievement. Table 4 display the results, which yield no evidence of FAST effects operating through the composite parent-parent social capital scale. For example, with first grade post-treatment social capital as the mediator and reading achievement as the outcome, the causal mediation analysis shows a total effect of 0.058, consisting of a direct effect of 0.054 and a mediation effect of 0.004, none of which is statistically significant. The coefficients are even smaller and likewise non-significant for mathematics. Similarly, as displayed in the second and third panels of Table 4, the analyses fail to detect mediation effects through the social capital composite when drawn from second grade and third grade parent surveys. All the mediation effects are close to zero and precisely measured, suggesting no positive social capital effects were suppressed by other intervention effects and raising further doubt about the causal role of social capital on third grade achievement.[11]

We also fit models with FAST-by-randomization block interactions as instruments for post-treatment social capital effects on achievement (and controls for pre-treatment social capital), to leverage random assignment as an instrument for direct measures of social capital. However, these models were less informative because the standard errors were large and the coefficients were not robust across social capital indicators and outcomes.[12] In any case, none of the instrumented social capital effects on achievement were statistically significant. Overall, the additional analyses failed to yield evidence that changes in social capital triggered by FAST improved children's test scores. In light of experimental evidence of FAST effects on post-treatment social capital (Turley et al. 2017; Rangel, Shoji, and Gamoran 2020), the results suggest that social capital, at least as we have measured it, did not affect achievement. Rather, our findings suggest that the observational estimates of social capital effects may be upwardly biased, and reveal the value of the experimental approach to provide a more rigorous assessment of social capital theory.

Sensitivity Analyses

We conducted two additional analyses to assess the sensitivity of our results to sample specifications. First, we introduced weighting to adjust for sample nonresponse. Comparing Table 2 (main results) with Table 5 (weighted results) we observe no substantive differences. The weighted coefficients closely replicate the unweighted coefficients, and none is statistically significant. These comparisons suggest our main results are not biased by sample attrition. Second, we adjusted for selection to account for missing data on the dependent variable, which reflects students who were retained in grade and thus had no third grade test scores, as well as students who left the school districts. (Students who left the sample schools but remained in their districts were retained in the sample.) Here, too, the supplementary results lead to identical conclusions as the main results, in that the coefficients in Table 6 are all close to zero and are statistically insignificant.

Discussion and Conclusions

Since Coleman's (1988) foundational article on social capital and the creation of human capital, much attention and debate has focused on the effects of social capital on children's academic outcomes. This is particularly true with respect to Coleman's notion of intergenerational closure, reflecting the benefits children may derive from their parents' connections with parents of their peers. The mixed empirical evidence has been enough to sustain many scholars' conviction in the benefits of this form of social capital, while fueling skepticism among others. Much of the skepticism revolves around the difficulty of designing studies that can take into account differences among families that influence both their social capital and their children's outcomes (Durlauf 2002; Mouw 2006).

Our study adds to a growing literature challenging the notion that social capital boosts children's academic achievement with an experimental analysis that circumvents the potential selection bias plaguing observational studies. Given our novel approach to examining social capital effects, it is worth revisiting the logic. All first grade students in some schools were randomly assigned to the FAST intervention, and those in other schools were assigned to a business-as-usual control condition. As shown through prior studies using the same data, FAST significantly enhanced intergenerational closure and improved the quality of parents' relationships. If this sort of social capital improves children's academic achievement, then we would expect children in the FAST schools to fare better on achievement tests in third grade. They did not. This is the case for both math and reading tests, and the findings are robust to a variety of sensitivity analyses. By failing to confirm the expectations of theory, this study adds to growing skepticism about whether social capital is a potent strategy for boosting school success among young children.

Our results are consistent with other recent studies that question the effects of social capital on achievement. We noted at the outset that more rigorously controlled studies have tended to yield smaller effects of social capital on achievement. In well-controlled studies, the impact of intergenerational closure or similar constructs on achievement has generally ranged from 0 to about .2 in effect size units (Carbonaro 1998; Kao and Rutherford 2007; Morgan and Todd 2009; Condron 2009; Freeman and Condron 2011; Dufur, Parcel, and Troutman 2013; Geven and van de Werfhorst 2020),[13] and the observational analyses we report in Table 3 mostly fall within this range (larger for shared expectations). With FAST exhibiting ITT effects on social capital measures of about .11 to .26, and TOT effects as high as .54 (Turley et al. 2017; Rangel, Shoji, and Gamoran 2020), we were positioned to find significant effects of FAST on achievement. This seemed especially likely because, based on qualitative studies, FAST built social capital among parents in ways that were not necessarily captured by the survey measures, such as through responsive and reciprocal communication, bounded solidarity, and the shared experience of participating (Shoji et al. 2014). The opportunity both to capture social capital formation more fully and to eliminate selection bias through random assignment are strengths of our intervention approach. A limitation is that questions about our interpretation of intervention effects as social capital effects would have been unavoidable had we found positive effects of FAST on achievement, since as we noted earlier, FAST might operate in other ways in addition to building social capital. But in the absence of positive effects, and with no basis to expect suppressor effects, interpreting our results as inconsistent with the predictions of social capital theory appears to be our most logical conclusion.

Reconciling Our Results With Those of Previous Studies of FAST

It is important to reconcile apparent inconsistencies between this study and prior studies of FAST, including evidence from the same sample that FAST reduced children's behavioral problems (Turley et al. 2017). Despite recent attempts to link early behavioral outcomes with later academic results, it is plausible that social capital affects children's behavior but not their test scores. For example, McNeal (1999) and Domina (2005) examined a variety of parent-child, parent-school, and parent-parent connections, and argued that the social control generated when parents are networked with the parents of their children's friends is likely to affect behavior, whereas only connections that involve school staff are likely to affect both behavior and achievement. Perhaps this pattern is at play in our findings. Although earlier studies of FAST in other cities reported academic benefits, the outcome measures were teacher-reported judgments of students' academic performance, which may reflect behavior as well as academic skills (McDonald et al. 2006). A recent UK-based study found social-behavioral but not achievement effects of FAST, mirroring our results (Lord et al. 2018).

Our findings are a reminder that improving social-behavioral skills may not necessarily lead to higher cognitive performance, at least not in the medium term, and not among young children. In a definitive study of six large-scale databases, Duncan et al. (2007) showed that test scores among elementary students are not well predicted by social-behavioral skills in early childhood. Notably, the Early Childhood Longitudinal Study revealed zero effects of internalizing and externalizing behavior problems in kindergarten on reading and mathematics scores in third grade. In another, more recent study, an intervention known to elevate social and emotional learning in grade 2 yielded no impact on standardized test scores in grades 3-5 (Hart et al. 2020). Hence the inconsistent findings for FAST effects—with positive effects on behavioral outcomes and no effects on achievement scores two years later—may actually align with other research.[14]

Implications for Future Research on Social Capital and Achievement

Our findings point towards several new directions for research on social capital and school success. First, relationships among parents in children's peer networks are but one dimension of social capital, and it may still be the case that improving relationships between children and their parents or teachers, or between parents and school personnel, may impact children's academic outcomes. Although we examined measures related to other forms of social capital, the FAST intervention had weak effects on them (Shoji 2014). It is therefore possible that the type of social capital strongly engendered by FAST is not the sort that boosts achievement, and the sort that elevates test scores was not triggered by FAST. Raising doubts about achievement effects of the types of social capital instigated by FAST—relationships of trust and shared expectations in closed networks of parents and children—is significant because of its centrality to sociological theories of social capital. However, it does not close the book on understanding the relation between social capital and school performance.

Second, a possible explanation for why the inter-family networks strengthened by FAST did not improve children's academic achievement is that—while these networks may serve an important norm-enforcing function—they may not provide access to the resources necessary to alter children's success in school. The FAST intervention was fielded in schools with large enrollments of students from low-income families. Intergenerational closure has been characterized in some research as a "middle-class phenomenon" that mostly benefits children from advantaged families whose parents can draw upon one another's resource-rich networks (Horvat, Weininger, and Laureau, 2003). Resources inhering in the parental networks of lower-income families may have less potential to boost children's academic performance, a notion supported by some research suggesting that intergenerational closure boosts children's achievement in low-poverty schools but not in high-poverty schools (Fasang, Mangino, and Brückner 2014). Within diverse school settings, recent research points to the importance of having parent-parent networks that bridge social divisions, in addition to strong bonds forged within social groups, in order to generate equitable allocation of resources within schools (Murray, Domina, Petts, Renzulli, and Boylan 2020). Commonly-used quantitative measures of intergenerational closure, including the one employed in this study, do not shed light on the diversity of parent-parent networks nor on the extent to which these network may bridge social lines. Future studies can improve upon these measures.

Third, our findings for young children may not generalize to adolescents. On the one hand, parents' connections to the parents of their children's friends may be especially powerful when children are young and parents are better positioned to make use of information and exert social control, so the absence of achievement effects for young children is telling. On the other hand, precisely because peer networks become increasingly salient as children reach adolescence, intergenerational closure may be especially important in facilitating parents' access to information and social control over teenagers, and our evidence cannot speak to that possibility. This notion points to the need for further research on social capital with adolescents using designs that offer stronger causal warrant than the middle and high school surveys of the past.

It is reasonable to ask whether experiments such as this one provide useful information about the processes by which social capital operates naturally. However, if social capital can be a lever for change to improve children's lives, then it will likely be through interventions similar to the FAST program that such social engineering occurs. Over the past decades, countless interventions have been developed to strengthen relationships between parents in a community, to improve parent-school relationships, or to otherwise enhance parents' involvement in their children's education (Smith et al. 2020). These interventions differ in name, specific activities, dosage, and other features but share common underlying theoretical mechanisms. Both to advance science and to meet real-world challenges, researchers must identify the proposed underlying mechanism, such as social capital, and examine in the field whether the mechanism functions as predicted across interventions.

Indeed, new studies are essential because the problem that social capital formation seeks to address is more urgent than ever. As the events of 2020 demonstrated like perhaps no other year in decades, inequality faced by racial and ethnic minority and low-income communities persists at a staggering level with deep structural roots. This study took place in public schools with substantial enrollments of low-income Hispanic students, within two southwestern cities with large immigrant communities. Several scholars have documented the social disconnect between many Hispanic families and their school communities and speculated that such isolation contributes to the educational struggles of their children (Stanton-Salazar and Dornbusch 1995; Valenzuela 1999). This social isolation can be compounded by restrictive immigration policies that instill fear and distrust among immigrant families (Suárez-Orozco, Yoshikawa, Teranishi, and Suárez-Orozco 2011); such sentiments were documented for some of the immigrant families involved in the present study (Valdez, Padilla, and Valentine 2013). The question of whether school-based programs can build social capital, and whether such social capital can help children from marginalized communities succeed academically, remains pressing.

Notes

[1] Intergenerational closure refers to closure in a network comprised of children and parents from the same families. Although commonly measured in education research as closed networks among families within the same school, intergenerational closure does not assume closure among all families in the same class or school.

[2] All but one of these studies examined intergenerational closure and most used the NELS indicator of number of parents of children's friends known as reported by parent respondents. In the survey from Germany and the Netherlands analyzed by Geven and van de Werfhorst (2020), intergenerational networks were measured with a full sociometric map that asked students which classmates their parents know and with whose parents their own parents get together, divided by the sum of possible nominations. An exception is the PISA study by Rodriguez Menés and Donato (2015), which used a constellation of indicators related to social cohesion among families and students.

[3] The indicator of intergenerational closure was slightly different in the ECLS-K, asking parents to report the number of parents of their child's classmates that they talk to regularly by phone or in person.

[4] Information on the FAST national organization is available at: https://www.familiesandschools.org/

[5] Descriptions of FAST activities have been provided by McDonald and Frey (1999) and the research basis for each FAST activity was laid out by McDonald et al. (1997) and McDonald (2002).

[6] The complier average causal effect model uses data from the treatment group to simulate which members of the control group would have complied had they had the opportunity to do so. Effect estimates are based on comparisons of compliers in the treatment group to would-be compliers in the control group (Bloom 1984; Turley et al. 2017).

[7] Rangel, Shoji, and Gamoran (2020) reported metric coefficients, which for comparability we have converted to effect sizes by dividing the metric coefficients by their respective pooled standard deviations.

[8] Two subsequent studies have followed our design of universal recruitment and school-level randomization, with mixed results. A UK-based study of Key Stage 1 (ages 5-7) pupils reported positive effects on social-behavioral outcomes but no effects on achievement (Lord et al., 2018). A study of kindergarten students in Philadelphia yielded no experimental effects on first-grade test scores, but a positive quasi-experimental effect on reading in a small sample of students who complied with the treatment compared to a matched sample of controls (Bos et al., 2018).

[9] Arizona's statewide, standardized test in 2011 and 2012 was Arizona's Instrument to Measure Standards (AIMS). The statewide test administered in Texas changed between 2011 (Texas Assessment of Knowledge and Skills, or TAKS) and 2012 (State of Texas Assessments of Academic Readiness, or STAAR).

[10] Arizona's standardized test was administered in English only. Texas administered both an English and a Spanish version of its statewide tests.

[11] These results contrasted with causal mediation effects on first grade child behavior, which revealed statistically significant mediation effects for FAST effects on behavioral outcomes via a social capital composite (Turley et al. 2017, note 4).

[12] Model diagnostics indicated that FAST was an adequate instrument for social capital, with F-statistics of 11.66 and 15.446 for the social capital composite in math and reading, respectively, and 34.49 and 29.19 for intergenerational closure on its own. None of these analyses yielded significant effects on achievement.

[13] Fasang, Mangino, and Brückner's (2014) results translated to an effect size of .43 for the impact of intergenerational closure on high school grades, but only in low-poverty schools; the effect in high-poverty schools was actually negative. (Note that in this case and others in which coefficients were reported in their original metrics, we have converted them to effect size coefficients for comparability.)

[14] Of course, social-behavioral skills gained in early childhood may be vital for long-term life chances even if they do not affect cognitive skills in the short term (Kautz et al. 2014; Deming 2017).

Research Ethics

All human subjects research conducted for this paper was approved by an institutional review board. The research was performed in a way that is consistent with the ethical standards laid out in the 1964 Declaration of Helsinki and Section 12 of the American Sociological Association's Code of Ethics. All human subjects gave their informed consent prior to their participation in the research (children's parents, in the case of 5-8 year olds), and adequate steps were taken to protect participants' confidentiality.

References

Acock, Alan C. 2005. “Working with Missing Values.” *Journal of Marriage and Family* 67 (4): 1012–28.

Allison, Paul D. 2001. *Missing Data*. Thousand Oaks, CA: Sage

Bandeira de Mello, Victor, Charles Blankenship, and Don H. McLaughlin. 2009. *Mapping State Proficiency Standards Onto NAEP Scales: 2005-2007* (NCES 2010-456). Washington, DC: National Center for Education Statistics.

Banerjee, Abhijit V., and Esther Duflo. 2009. “The Experimental Approach to Development Economics.” *Annual Review of Economics* 1: 151-178.

Bankston, Carl L., and Min Zhou. 2002. “Social Capital as Process: The Meanings and Problems of a Theoretical Metaphor.” *Sociological Inquiry* 72: 285-317.

Bloom, Howard. 1984. “Accounting for No-Shows in Experimental Evaluation Designs.” *Evaluation Review* 8: 225-246.

Bos, Johannes, Elizabeth Spier, Victor Bandeira de Mello, Raquel Gonzales, and Fangyi Huang. 2018. *Investing in Innovation (i3) Validation Study of Families and Schools Together (FAST).* San Mateo, CA: American Institutes for Research.

Bourdieu, Pierre. 1986. “The Forms of Capital.” Pp. 241–58 in *Handbook of Theory and Research for the Sociology of Education*, edited by J. G. Richardson. New York: Greenwood.

Bryk, Anthony S., and Barbara L. Schneider. 2002. *Trust in Schools: A Core Resource for Improvement*. New York: Russell Sage Foundation.

Carbonaro, William. J. 1998. “A Little Help from My Friend’s Parents: Intergenerational Closure and Educational Outcomes.” *Sociology of Education* 71: 295-313.

Caspe, M., and Lopez, E. 2006. *Lessons from Family-Strengthening Interventions: Learning from Evidence-Based Practice*. Cambridge, MA: Harvard Family Research Project. Available at: http://www.hfrp.org/publications-resources/browse-our-publications/lessons-from-family-strengthening-interventions-learning-from-evidence-based-practice

Coleman, James S. 1988. "Social Capital in the Creation of Human Capital." *American Journal of Sociology* 94: S95–S120.

Coleman, James S. 1990. *Foundations of Social Theory*. Cambridge, MA: Harvard University Press.

Coleman, James S., and Thomas Hoffer. 1987. *Public and Private High Schools The Impact of Communities.* New York: Basic Books.

Condron, Dennis J. 2009. "Social Class, School and Non-School Environments, and Black/White Inequalities in Children's Learning." *American Sociological Review* 74:683-708.

Cunha, Flavio, and James Heckman. 2007. "The Technology of Skill Formation." *American Economic Review* 97: 31-47.

Deming, David. 2017. "The Growing Importance of Social Skills in the Labor Market." *Quarterly Journal of Economics* 132: 1593-1640.

Dika, Sandra L., and Kusum Singh. 2002. "Applications of Social Capital in Educational Literature: A Critical Synthesis." *Review of Educational Research* 72 :31-60.

Domina, Thurston. 2005. "Leveling the Home Advantage: Assessing the Effectiveness of Parental Involvement in Elementary School." *Sociology of Education* 78 :233-249.

Duflo, Esther. 2007. "Field Experiments in Development Economics." Pp. 322-348 in *Advances in Economic Theory and Econometrics: Theory and Applications* (Econometrics Society

Monographs no. 42), edited by Richard Blundell, Whitney K. Newey, and Torsten Persson. Cambridge, England: Cambridge University Press.

Dufur, Mikaela J., Parcel, Toby L., and Troutman, Kelly P. 2013. “Does Social Capital at Home Matter More than Capital at School? Social Capital Effects on Academic Achievement.” *Research in Social Stratification and Mobility* 31: 1-21.

Dufur, Mikaela J., Toby J. Parcel, John P. Hoffmann, and David B. Braudt. 2016. “Who Has the Advantage? Race and Sex Differences in Returns to Social Capital at Home and at School.” *Research in Social Stratification and Mobility* 45: 27-40.

Duncan, Greg J., Changelle J. Dowsett, Amy Claessens, Katherine Magnuson, Aletha C. Huston, Pamela Klebanov, Linda S. Pagani, Leon Feinstein, Mimi Engel, Jeanne Brooks-Gunn, Holly Sexton, Kathryn Duckworth, and Crista Japel. 2007. “School Readiness and Later Achievement.” *Developmental Psychology* 43: 1428-1446.

Durlauf, Steven N. 2002. “On the Empirics of Social Capital.” *Economic Journal* 112: F459–F479.

Fasang, Anette Eva, William Mangino, and Hannah Brückner. 2014. “Social Closure and Educational Attainment.” *Sociological Forum* 29: 137–164.

Fiel, Jeremy, Megan M. Shoji, M., and Adam Gamoran. 2015. An Intervention Approach to Building Social Capital: Effects on Grade Retention.” Pp. 262-291 in *The Handbook of Research Methods and Applications on Social Capital*, edited by Y. Li. Cheltenham, England: Edward Elgar Publishing.

Freeman, Kendralin J., and Dennis J. Condron. 2011. “Schmoozing in Elementary School: The Importance of Social Capital to First Graders.” *Sociological Perspectives* 54: 521-546.

Furstenberg, Jr., Frank M., and Mary E. Hughes. 1995. “Social Capital and Successful

Development among At-Risk Youth." *Journal of Marriage and the Family* 57: 580–592.

Gamoran, Adam, Turley, Ruth N. López Turley, Alyn Turner, and Rachel Fish. 2012. "Differences between Hispanic and Non-Hispanic Families in Social Capital and Child Development: First-Year Findings from an Experimental Study." *Research in Social Stratification and Mobility* 30: 97-112.

Geven, Sara, and Herman G. van de Werfhorst. 2020. "The Role of Intergenerational Networks in Students' School Performance in Two Differentiated Educational Systems: A Comparison of Between- and Within-Individual Estimates." *Sociology of Education* 93: 40-64.

Guo, Shenyang, and Mark W. Fraser. 2010. "Chapter 4. Sample Selection and Related Models." Pp. 85-125 in *Propensity Score Analysis: Statistical Methods and Applications*. Thousand Oaks, CA: Sage.

Hart, Susan Crandall, James Clyde DiPerna, Pui-Wa Lei, and Weiyi Cheng. 2020. "Nothing Lost, Something Gained? Impact of a Universal Social-Emotional Learning Program on Future State Test Performance." *Educational Researcher* 49 (1): 5-19.

Horvat, Erin M., Elliot B. Weininger, and Annette Lareau. 2003. "From Social Ties to Social Capital: Class Differences in the Relations between Schools and Parent Networks." *American Educational Research Journal* 40: 319-351.

Imai, Kosuke, Luke Keele, and Dustin Tingley. 2010. "A General Approach to Causal Mediation Analysis." *Psychological Methods* 15: 309–334.

Israel, Glenn D., Lionel J. Beaulieu, and Glen Hartless. 2001. "The Influence of Family and Community Social Capital on Educational Achievement." *Rural Sociology* 66: 43-68.

Kautz, Tim, James J. Heckman, Ron Diris, Bas ter Weel, and Lex Borghans. 2014. *Fostering* *and Measuring Skills: Improving Cognitive and Non-Cognitive Skills to Promote Lifetime Success*. Paris: OECD.

Kratochwill, Thomas R., McDonald, Lynn, Levin, Joel R., Young Bear-Tibbetts, Holly, and Michelle K. Demaray. 2004. Families and Schools Together: An Experimental Analysis of a Parent-mediated Multi-family Group Program for American Indian Children. *Journal of School Psychology* 42: 359-383.

Kratochwill, Thomas R., McDonald, Lynn, Levin, Joel R., Scalia, Phyllis A., and Gail Coover. 2009. "Families and Schools Together: An Experimental Study of Multi-family Support Groups for Children at Risk." *Journal of School Psychology* 47: 245-265.

Layzer, Jean I., Goodson, Barbara, Creps, Cindy, Werner, Alan, and Lawrence Bernstein. 2001. *National Evaluation of Family Support Programs, Volume B: Research Studies. Final Report.* Cambridge, MA: Abt Associates.

Lord, Pippa, Ben Styles, Jo Morrison, Richard White, Joana Andrade, Susie Bamford, Clare Lushey, Megan Lucas, and Robert Smith. 2018. *Families and Schools Together (FAST) Evaluation Report and Executive Summary*. London: Educational Endowment Foundation.

Kao, Grace, and Lindsay T. Rutherford. 2007. "Does Social Capital Still Matter? Immigrant Minority Disadvantage in School-Specific Social Capital and its Effects on Academic Achievement." *Sociological Perspectives* 50: 27-52.

Kautz, Tim, James J. Heckman, Ron Dirlis, Bas ter Weel, and Lex Borghans. 2014. *Fostering and Measuring Skills: Improving Cognitive and Non-Cognitive Skills to Promote Lifetime Success*. Paris: Organization for Economic Cooperation and Development.

McDonald, Lynne. 2002. *Research Background of Each FAST Activity for Multi-Family Group*

Meetings. Madison, WI: Wisconsin Center for Education Research, Madison.

McDonald, Lynn, and Heather E. Frey. 1999. *Families and Schools Together: Building Relationships*. Washington, DC: Office of Juvenile Justice and Delinquency Prevention. Available at: https://www.ncjrs.gov/pdffiles1/ojjdp/173423.pdf

McDonald, Lynn, Stephen Billingham, Tammy Conrad, Arthur Morgan, O. Nancy, and Estella Payton. 1997. "Families and Schools Together (FAST): Integrating Community Development with Clinical Strategy." *Families in Society* 78: 140–155.

McDonald, Lynn, D. Paul Moberg, Roger Brown, Ismael Rodriguez-Espiricueta, Nydia I. Flores, Melissa P. Burke, and Gail Coover. 2006. "After-School Multifamily Groups: A Randomized Controlled Trial Involving Low-Income, Urban, Latino Children." *Children and Schools* 18: 25–34.

McNeal, Ralph B. 1999. "Parental Involvement as Social Capital: Differential Effectiveness on Science Achievement, Truancy, and Dropping Out." *Social Forces* 78: 117-144.

Morgan, Stephen L., and Aage B. Sørensen. 1999. "Parental Networks, Social Closure, and Mathematics Learning: A Test of Coleman's Social Capital Explanation of School Effects." *American Sociological Review* 64: 661-681.

Morgan, Stephen L., and Jennifer J. Todd. 2009. "Intergenerational Closure and Academic Achievement in High School: A New Evaluation of Coleman's Conjecture." *Sociology of Education* 82: 267-286.

Mouw, Ted 2006. "Estimating the Causal Effects of Social Capital: A Review of Recent Research." *Annual Review of Sociology* 32: 79–102.

Murray, Brittany, Thurston Domina, Amy Petts, Linda Renzulli, and Rebecca Boylan. 2020. "'We're All In This Together': Bridging and Bonding Social Capital in Elementary

School PTOs." *American Educational Research Journal* 57: 2210-2244.

Pong, Suet-Ling. 1998. "The School Compositional Effect of Single Parenthood on 10th-Grade Achievement." *Sociology of Education* 71: 23-42.

Portes, Alejandro. 1998. "Social Capital: Its Origins and Applications in Modern Sociology." *Annual Review of Sociology* 24: 1–24.

Portes, Alejandro. 2000. "The Two Meanings of Social Capital." *Sociological Forum* 15: 1-12.

Pribesh, Shana, and Douglas B. Downey. 1999. "Why are Residential and School Moves Associated with Poor School Performance?" *Demography* 36: 521-534.

Putman, Robert D. 2015. *Our Kids: The American Dream in Crisis.* New York: Simon & Schuster.

Rangel, David, Megan Shoji, and Adam Gamoran. 2020. "Family Engagement and Social Capital: An Empirical Test with Families in Low-Income, Latino Communities." *American Educational Research Journal* 57: 2450-2484.

Rodriguez Menés, Jorge, and Luisa Donato. 2015. "Social Capital, Social Cohesion, and Cognitive Attainment." Pp. 324-343 in *The Handbook of Research Methods and Applications on Social Capital,* edited by Y. Li. Cheltenham, England: Edward Elgar Publishing.

Sampson, Robert J., Jeffrey D. Morenoff, and Felton Earls. 1999. "Beyond Social Capital: Spatial Dynamics of Collective Efficacy for Children." *American Sociological Review* 64: 633–660.

Shoji, Megan. 2014. "Nice to Meet You? Exploring the Development of Supportive Parent-School Relationships in Low-Income Latino Communities." Unpublished doctoral dissertation, Department of Sociology, University of Wisconsin-Madison.

Shoji, Megan, Anna R. Haskins, David E. Rangel, and Kia N. Sorensen. 2014. “The Emergence of Social Capital in Low-Income Latino Elementary Schools.” *Early Childhood Research Quarterly* 29: 600-613.

Shriner, Michael, Ronald L. Mullis, and Bethanne M. Schlee. 2009. “The Usefulness of Social Capital Theory for Understanding the Academic Improvement of Young Children in Stepfamilies over Two Points in Time.” *Journal of Divorce & Remarriage* 50: 445-458.

Smith, Tyler E., Susan M. Sheridan, Elizabeth M. Kim, Sunyoung Park, and S. Natasha Beretvas. 2020. "The Effects of Family-School Partnership Interventions on Academic and Social-Emotional Functioning: A Meta-Analysis Exploring What Works for Whom." *Educational Psychology Review* 32: 511-544.

Smith, Vernon L. 1989. "Theory, Experiment and Economics." *Journal of Economic Perspectives* 3: 151-169.

Stanton-Salazar, Ricardo D., and Sanford M. Dornbusch. 1995. “Social Capital and the Reproduction of Inequality: Information Networks among Mexican-Origin High School Students.” *Sociology of Education* 68: 116-35.

StataCorp. 2019. *Stata Multiple Imputation Reference Manual. Release 16.* College Station, TX: Stata Press.

Suárez-Orozco, Carola, Hirokazu Yoshikawa, Robert T. Teranishi, and Marcelo M. Suárez-Orozco. 2011. “Growing Up In the Shadows: The Developmental Implications of Unauthorized Status.” *Harvard Educational Review* 81: 438–472.

Sun, Yongmin. 1998. “The Academic Success of East-Asian-American Students: An Investment Model.” *Social Science Research* 27: 432-456.

Sun, Yongmin. 1999. “The Contextual Effects of Community Social Capital on Academic

Performance." *Social Science Research* 28: 403-426.

Tingley, Dustin, Teppei Yamamoto, Kentaro Hirose, Luke Keele, and Kosuke Imagi. 2014. mediation: R Package for Causal Mediation Analysis. *Journal of Statistical Software* 59: 1-38.

Turley, Ruth N. López, Adam Gamoran, Alyn T. McCarty, and Rachel Fish. 2017. Reducing Children's Behavior Problems through Social Capital: A Causal Assessment. *Social Science Research* 61: 206-217.

Valdez, Carmen R., Brian Padilla, and Jessa Lewis Valentine. 2011. "Consequences of Arizona's Immigration Policy on Social Capital Among Mexican Mothers With Unauthorized Immigration Status." *Hispanic Journal of Behavioral Studies* 35: 303-322.

Valenzuela, Angela. 1999. *Subtractive Schooling: U.S.-Mexican Youth and the Politics of Caring*. Albany: SUNY Press.

Table 1. Student-level Descriptive Statistics

	Mean	SD	N
FAST	0.50	-	2,165
Math standardized test score	-0.02	(1.02)	1,867
Reading standardized test score	0.02	(1.03)	1,854
Met proficiency level in math	0.72	-	2,165
Met proficiency level in reading	0.77	-	2,164
Race/ethnicity			
Hispanic	0.78	-	2,140
Non-Hispanic White	0.13	-	2,140
Black	0.07	-	2,140
Asian, American Indian, or other race	0.02	-	2,140
Female	0.50	-	2,163
Eligible for free or reduced price lunch	0.78	-	2,140
English language learner	0.26	-	2,145
Special education student	0.10	-	2,145
Parent completed baseline survey in Spanish	0.28	-	2,165
Intergenerational closure (linear)	3.45	(2.12)	1,493
Knew two or more parents of child's friends (dichotomous measure of intergenerational closure)	0.78	-	1,493
Reciprocity scale (standardized)	0.02	(1.01)	1,509
Shared expectations			1,479
Not at all	0.29	-	1,479
A little	0.21	-	1,479
Some	0.28	-	1,479
A lot	0.21	-	1,479

Note: Standard deviations are reported for continuous variables. All other variables are dichotomous. Social capital measures are collected in year 1 prior to treatment.

Table 2. Experimental Effects of FAST on Math and Reading Achievement

	Math Standardized Scores						Met Proficiency Level in Math					
	ITT		TOT Dosage		TOT Graduation		ITT		TOT Dosage		TOT Graduation	
FAST	-0.01	-0.03	-0.01	-0.06	0.03	-0.06	-0.01	-0.03	-0.01	-0.06	0.03	0.07
	(0.08)	(0.07)	(0.18)	(0.16)	(0.27)	(0.24)	(0.08)	(0.07)	(0.18)	(0.16)	(0.27)	(0.10)
Pretreatment controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	1,867	1,706	1,712	1,559	1,712	1,559	1,867	1,706	1,712	1,559	1,712	1,772
	Reading Standardized Scores						Met Proficiency Level in Reading					
	ITT		TOT Dosage		TOT Graduation		ITT		TOT Dosage		TOT Graduation	
FAST	0.05	0.04	0.14	0.06	0.20	0.07	0.05	0.04	0.14	0.06	0.20	0.08
	(0.08)	(0.07)	(0.17)	(0.15)	(0.25)	(0.20)	(0.08)	(0.07)	(0.17)	(0.15)	(0.25)	(0.09)
Pretreatment controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	1,854	1,695	1,700	1,549	1,700	1,549	1,854	1,695	1,700	1,549	1,700	1,772

Notes: Figures are regression coefficients, with standard errors in parentheses. All models include randomization controls. Pretreatment controls are race/ethnicity, sex, free or reduced price lunch eligibility, English language learner status, special education status, language of consent, and pretreatment social capital. ITT = intent-to-treat; TOT = treatment-on-treated.

Table 3. Regressions of Math and Reading Achievement on Survey Measures of Social Capital

	Math Standardized Score		Met Proficiency Level in Math		Reading Standardized Score		Met Proficiency Level in Reading	
Model 1: Social capital combined scale	0.09*	0.07*	0.05	0.00	0.06	0.03	0.12	0.10
	(0.03)	(0.03)	(0.07)	(0.08)	(0.03)	(0.03)	(0.08)	(0.09)
Model 2: Reciprocity scale	0.07*	0.05	0.03	0.00	0.05	0.03	0.10	0.08
	(0.03)	(0.03)	(0.07)	(0.07)	(0.03)	(0.03)	(0.07)	(0.08)
Model 3: Other parents share expectations								
A little	0.20*	0.21*	0.43*	0.49*	0.13	0.14	0.46*	0.53*
	(0.08)	(0.08)	(0.19)	(0.20)	(0.08)	(0.08)	(0.20)	(0.21)
Some	0.14	0.12	0.19	0.15	0.15*	0.12	0.61*	0.62*
	(0.07)	(0.07)	(0.17)	(0.18)	(0.07)	(0.07)	(0.19)	(0.20)
A lot	0.26*	0.21*	0.39*	0.26	0.25*	0.20*	0.49*	0.48*
	(0.08)	(0.08)	(0.19)	(0.21)	(0.08)	(0.08)	(0.20)	(0.22)
Model 4: Intergenerational closure (linear)	0.03*	0.03*	0.06	0.06	0.01	0.01	0.07*	0.07*
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.03)	(0.04)
Model 5: Intergenerational closure (dichotomous)	0.18*	0.18*	0.28	0.30	0.13	0.11	0.22	0.19
	(0.07)	(0.07)	(0.16)	(0.17)	(0.07)	(0.07)	(0.17)	(0.18)
Pretreatment controls	No	Yes	No	Yes	No	Yes	No	Yes
N	1,233	1,229	1,418	1,413	1,223	1,219	1,417	1,412

Notes: Each survey-based measure of social capital was entered in a separate two-level, mixed-effects regression model. Figures are regression coefficients, with standard errors in parentheses. All models include randomization controls. Pretreatment controls are race/ethnicity, sex, free or reduced price lunch eligibility, English language learner status, special education status, and language of consent.
*p<0.05

Table 4. Causal Mediation Effects of FAST on Achievement via Survey Measures of Social Capital

	First Grade (Post-treatment) Social Capital Combined Scale as Mediator					
	Math Standardized Scores			Reading Standardized Scores		
	Estimate	95% CI	P-value	Estimate	95% CI	P-value
Mediation effect	0.004	-0.002, 0.014	0.260	0.004	-0.002, 0.015	0.228
Direct effect	-0.028	-0.203, 0.150	0.732	0.054	-0.116, 0.222	0.534
Total effect	-0.024	-0.202, 0.150	0.770	0.058	-0.107, 0.225	0.498
Proportion mediated	-0.012	-0.646, 0.860	0.834	0.025	-0.650, 0.834	0.574
N		1142			1134	
	Second Grade Social Capital Combined Scale as Mediator					
	Math Standardized Scores			Reading Standardized Scores		
	Estimate	95% CI	P-value	Estimate	95% CI	P-value
Mediation effect	0.004	-0.006, 0.019	0.452	0.007	-0.003, 0.025	0.210
Direct effect	0.120	-0.054, 0.288	0.194	0.125	-0.068, 0.323	0.210
Total effect	0.124	-0.051, 0.295	0.178	0.132	-0.068, 0.333	0.192
Proportion mediated	0.016	-0.253, 0.433	0.558	0.040	-0.429, 0.483	0.342
N		752			747	
	Third Grade Social Capital Combined Scale as Mediator					
	Math			Reading		
	Estimate	95% CI	P-value	Estimate	95% CI	P-value
Mediation effect	0.003	-0.006, 0.015	0.498	0.003	-0.005, 0.015	0.490
Direct effect	-0.045	-0.231, 0.127	0.626	-0.007	-0.183, 0.176	0.926
Total effect	-0.042	-0.227, 0.129	0.644	-0.004	-0.179, 0.179	0.950
Proportion mediated	-0.006	-0.657, 0.631	0.854	0.000	-0.640, 0.607	0.996
N		770			765	

Notes: Analysis was conducted using the mediation package in R (Tingley et al. 2014). Models include randomization controls, pretreatment social capital, race/ethnicity, free or reduced price lunch eligibility, English language learner status, special education status, and language of consent. CI = confidence interval.

Table 5. Effects of FAST on Math and Reading Achievement Using Nonresponse Weights

	Math Standardized Scores						Met Proficiency Level in Math					
	ITT		TOT Dosage		TOT Graduation		ITT		TOT Dosage		TOT Graduation	
FAST	-0.01	-0.03	-0.03	-0.08	0.03	-0.07	0.00	0.00	0.06	0.05	0.11	0.09
	(0.08)	(0.08)	(0.19)	(0.17)	(0.27)	(0.26)	(0.04)	(0.03)	(0.07)	(0.07)	(0.10)	(0.10)
Pretreatment controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	1,815	1,706	1,663	1,559	1,663	1,559	2,100	1,963	1,896	1,772	1,896	1,772
	Reading Standardized Scores						Met Proficiency Level in Reading					
	ITT		TOT Dosage		TOT Graduation		ITT		TOT Dosage		TOT Graduation	
FAST	0.04	0.03	0.12	0.06	0.17	0.07	0.00	0.00	0.07	0.07	0.11	0.11
	(0.09)	(0.08)	(0.17)	(0.15)	(0.24)	(0.21)	(0.04)	(0.03)	(0.07)	(0.06)	(0.10)	(0.09)
Pretreatment controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	1,802	1,695	1,651	1,549	1,651	1,549	2,099	1,963	1,895	1,772	1,895	1,772

Notes: Figures are regression coefficients, with standard errors in parentheses. All models include randomization controls. Pretreatment controls are race/ethnicity, sex, free or reduced price lunch eligibility, English language learner status, special education status, language of consent, and pretreatment social capital. ITT = intent-to-treat; TOT = treatment-on-treated.

Table 6. Effects of FAST on Math and Reading Achievement With Selection Correction

	Math Standardized Scores					
	ITT		TOT Dosage		TOT Graduation	
FAST	-0.08	-0.05	-0.07	-0.05	0.03	-0.01
	(0.13)	(0.09)	(0.31)	(0.20)	(0.48)	(0.29)
Pretreatment controls	No	Yes	No	Yes	No	Yes
N	1,723	1,706	1,723	1,706	1,723	1,706
	Reading Standardized Scores					
	ITT		TOT Dosage		TOT Graduation	
FAST	-0.02	-0.02	0.03	-0.02	0.12	0.00
	(0.10)	(0.08)	(0.24)	(0.18)	(0.37)	(0.27)
Pretreatment controls	No	Yes	No	Yes	No	Yes
N	1,712	1,695	1,712	1,695	1,712	1,695

Notes: Figures are regression coefficients, with standard errors in parentheses. Pretreatment controls are race/ethnicity, sex, free or reduced price lunch eligibility, English language learner status, special education status, language of consent, and pretreatment social capital. For the TOT models, FAST attendance and graduation are predicted in a prior model using FAST-by-randomization-block interactions; predicted values from the prior model are used in these models. Selection equation estimates (not shown) are available upon request. *p<0.05

Appendix:
Randomization Check and
Supplementary Analyses with Imputation of Missing Data

Table A-1. Student-level Descriptive Statistics, by Treatment (Main Analyses with Listwise Deletion)

	Met Proficiency Level in Math						
	Control			Treatment			Treatment – Control
	Mean	SD	N	Mean	SD	N	Mean diff.
Race/ethnicity							
Hispanic	0.79	-		0.76	-		0.03
Non-Hispanic White	0.12	-	1,072	0.13	-	1,060	-0.02
Black	0.07	-		0.08	-		-0.01
Asian, American Indian, or other race	0.01	-		0.01	-		0.00
Female	0.50	-	1,087	0.50	-	1,076	0.00
Eligible for free or reduced price lunch	0.77	-	1,078	0.79	-	1,062	-0.01
English language learner	0.27	-	1,080	0.26	-	1,065	0.01
Special education student	0.10	-	1,080	0.09	-	1,065	0.01
Parent completed baseline survey in Spanish	0.28	-	1,088	0.28	-	1,077	0.01
Intergenerational closure (linear)	3.21	(2.13)		2.94	(2.13)		0.27* (SE = 0.09)
Knew two or more parents of child's friends (dichotomous measure of intergenerational closure)	0.75	-	1,075	0.70	-	1,060	0.04*
Reciprocity scale (standardized)	0.06	1.00	1,085	-0.04	0.99	1,070	0.10* (SE = 0.04)
Shared expectations							
Not at all	0.31	-		0.33	-		-0.02
A little	0.22	-	1,065	0.24	-	1,053	-0.01
Some	0.26	-		0.27	-		-0.01
A lot	0.20	-		0.16	-		0.04*

Table A-2. Experimental Effects of FAST on Math and Reading Achievement, with Imputed Data

	Math Standardized Scores						Met Proficiency Level in Math					
	ITT		TOT Dosage		TOT Graduation		ITT		TOT Dosage		TOT Graduation	
FAST	-0.01	-0.02	-0.01	-0.04	0.02	-0.01	-0.01	-0.02	0.05	0.04	0.09	0.07
	(0.08)	(0.07)	(0.18)	(0.17)	(0.26)	(0.24)	(0.20)	(0.19)	(0.07)	(0.07)	(0.10)	(0.09)
Pretreatment controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	1,867	1,867	1,712	1,712	1,712	1,712	2,164	2,164	1,952	1,952	1,952	1,952
	Reading Standardized Scores						Met Proficiency Level in Reading					
	ITT		TOT Dosage		TOT Graduation		ITT		TOT Dosage		TOT Graduation	
FAST	0.05	0.03	0.14	0.08	0.19	0.10	0.09	0.10	0.06	0.05	0.08	0.08
	(0.08)	(0.07)	(0.17)	(0.14)	(0.24)	(0.20)	(0.21)	(0.20)	(0.07)	(0.06)	(0.09)	(0.09)
Pretreatment controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	1,854	1,854	1,700	1,700	1,700	1,700	2,164	2,164	1,952	1,952	1,952	1,952

Notes: Figures are regression coefficients, with standard errors in parentheses. All models include randomization controls. Pretreatment controls are sex, free or reduced price lunch eligibility, special education status, language of consent, pretreatment social capital, and a combined measure of race/ethnicity and English language learner status (1 = White, non-Hispanic; 2 = Black or Other, non-Hispanic; 3 = Hispanic, English language learner; 4 = Hispanic, non-English language learner); small Ns necessitated creating this combined measure to enable the imputation model to converge. The mi impute chained command in Stata was used to impute missing independent variables. ITT = intent-to-treat; TOT = treatment-on-treated.

Table A-3. Regression of Math and Reading Achievement on Survey Measures of Social Capital, with Imputed Data

	Math Standardized Score		Met Proficiency Level in Math		Reading Standardized Score		Met Proficiency Level in Reading	
Model 1: Social capital combined scale	0.09*	0.06*	0.04	0.00	0.06*	0.03	0.12	0.10
	(0.03)	(0.03)	(0.07)	(0.08)	(0.03)	(0.03)	(0.08)	(0.09)
N	1,233	1,233	1,417	1,417	1,223	1,223	1,417	1,417
Model 2: Reciprocity scale	0.08*	0.06*	0.05	0.01	0.05	0.03	0.11	0.09
	(0.03)	(0.03)	(0.07)	(0.07)	(0.03)	(0.03)	(0.07)	(0.07)
N	1,281	1,281	1,492	1,492	1,271	1,271	1,492	1,492
Model 3: Other parents share expectations								
A little	0.19*	0.19*	0.45*	0.49*	0.11	0.12	0.45*	0.53*
	(0.08)	(0.08)	(0.19)	(0.20)	(0.08)	(0.08)	(0.19)	(0.20)
Some	0.13	0.09	0.18	0.11	0.14	0.11	0.55*	0.53*
	(0.07)	(0.07)	(0.17)	(0.18)	(0.07)	(0.07)	(0.18)	(0.19)
A lot	0.25*	0.18*	0.38*	0.23	0.24*	0.18*	0.47*	0.41
	(0.08)	(0.08)	(0.19)	(0.20)	(0.08)	(0.08)	(0.19)	(0.21)
N	1,268	1,268	1,478	1,478	1,257	1,257	1,478	1,478
Model 4: Intergenerational closure (linear)	0.03*	0.03	0.05	0.05	0.01	0.01	0.07*	0.06
	(0.01)	(0.01)	(0.03)	(0.03)	(0.01)	(0.01)	(0.03)	(0.03)
N	1,281	1,281	1,492	1,492	1,271	1,271	1,492	1,492
Model 5: Intergenerational closure (dichotomous)	0.19*	0.16*	0.28	0.27	0.13	0.10	0.22	0.17
	(0.07)	(0.07)	(0.15)	(0.16)	(0.07)	(0.07)	(0.16)	(0.17)
N	1,281	1,281	1,492	1,492	1,271	1,271	1,492	1,492
Pretreatment controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Each survey-based measure of social capital was entered in a separate two-level, mixed-effects regression model. Figures are regression coefficients, with standard errors in parentheses. All models include randomization controls. Pretreatment controls are sex, free or reduced price lunch eligibility, special education status, language of consent, and a combined measure of race/ethnicity and English language learner status (1 = White, non-Hispanic; 2 = Black or Other, non-Hispanic; 3 = Hispanic, English language learner; 4 = Hispanic, non-English language learner); small Ns necessitated creating this combined measure to enable the imputation model to converge. The mi impute chained command in Stata was used to impute missing independent variables. *p<0.05