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Causal Effects of Social Capital on Child Outcomes

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Abstract

Social capital has been widely touted as a source of many social benefits, but empirical research has been constrained by conceptual and causal ambiguity. Sifting through the literature, we define social capital as relations of trust, mutual expectations, and shared values embedded in a social network. We conceive of social capital as a property of groups rather than of individuals, acknowledge that it may have negative as well as positive effects, and attend to the quality as well as the structure of social relationship among group members. To address causal questions, we propose an intervention approach, in which social capital is exogenously manipulated through a social-capital-building intervention. We demonstrate our approach with data from over 3,000 first-graders and their families in 52 schools in Phoenix, AZ, and San Antonio, TX. Half the schools were randomly assigned to an intervention called Families and Schools Together (FAST) that strengthens relations within and among families and between families and schools. The other schools served as controls. Multilevel models reveal strong positive effects of treatment assignment on parent social capital and more modest but statistically significant effects on reducing children's behavioral problems. Complier average causal effect (CACE) models show that the strongest effects on parent social capital occurred for families that participated fully in the intervention, whereas the CACE models were less consequential for child outcomes. Instrumental variables models suggest that the social capital effects may be regarded as causal, and causal mediation models suggest that the intervention effects on child outcomes are mediated by social capital.

Introduction

Social capital, broadly defined, has been linked to a wide range of social benefits, including improved educational outcomes (e.g., grades, test scores, course selection, high school completion, and college enrollment), decreased crime and delinquency, enhanced labor market outcomes (e.g., job searches and job performance), and improved physical and mental health outcomes (Carbonaro 1998; Carmeli, Ben-Hador, Waldman, and Rupp 2009; Ceballo and McLoyd 2002; Coleman 1988; Dika and Singh 2002; Kunitz 2004; Matthews, Pendakur, and Young 2009; Morgan and Sorensen 1999; Stanton-Salazar 1997). A declining level of social capital has been offered as an explanation for many social ills (most famously by Putnam 2000), so increasing social capital may be a promising strategy to address social problems. Moreover if access to social capital is unequal (e.g., Coleman and Hoffer 1987; Lareau 1987), then increasing social capital for disadvantaged groups may help reduce inequality in valued outcomes (Gamoran et al., 2012).

Despite the promising rhetoric surrounding social capital, evidence of its causal role is limited for three reasons. First, research on social capital suffers from conceptual ambiguity. Second, reflecting the conceptual ambiguity, even a brief review of the literature unearths a wide range of indicators used to measure social capital that, taken together, suggest the literature also suffers from operational ambiguity. Third, most empirical assessments of social capital have been limited to correlational analyses, which limit the causal interpretation of the effects of social capital. In particular, it is unclear whether social capital is a reflection of unobserved variables, a matter of selection (individuals who are alike tend to associate with one another), or a matter of influence (social capital and behavioral outcomes are causally related), and if the latter, which way the causal pathway runs (Mouw, 2006).

A concept with such potential utility warrants more rigorous research to assess its causal effects. To that end, the purpose of this study is to estimate the causal effects of social capital on children's educational outcomes using a randomized field experiment. Before describing the experiment, we address the conceptual and operational ambiguities that have encumbered social capital research until now.

The Concept of Social Capital

Researchers disagree about the conceptualization and operationalization of social capital. In a review of 35 studies of social capital and educational outcomes between 1992 and 2001, Dika and Singh (2002) reported that social capital was conceptualized in a wide variety of ways, including as networks, network resources, trust, information channels, and norms and sanctions; and they reported that social capital was operationalized through an even wider range of measures, including family structure and size, parents' and friends' expectations and aspirations, residential mobility, youth activities, church activities, parent involvement, racial identity, reading and writing abilities, family cohesion, intergenerational closure, neighborhood attributes, social support networks, socioeconomic status, language proficiency and use, and whether parents have a say in school policy. This list, which is not exhaustive, illustrates a clear need for a more focused definition of social capital and a more precise specification of its indicators.

Conceptual problems with social capital can be characterized in four ways. First, how do we distinguish the sources of social capital from the benefits derived from them? Second, is social capital a property of groups or individuals? Third, are the effects of social capital always positive, or can there be negative effects of social capital? Fourth, should social capital be conceptualized as the structure of a social network, the quality of that network, or some combination of the two? Addressing these important conceptual disagreements and ambiguities

will result in a more precise definition as well as more accurate measures of what is now a vague but widely used concept with limited utility (Durlauf and Fafchamps, 2005).

Relations versus Resources

At the heart of the social capital debate is whether social capital refers to the relations among persons or to the resources acquired through those relations. Bourdieu was among the first sociologists to use the term social capital, which he described as “made up of social obligations (‘connections’), which is convertible, in certain conditions, into economic capital and may be institutionalized in the form of a title of nobility” (1986, p. 243). A few pages later, he also described social capital as “the aggregate of the actual or potential resources which are linked to possession of a durable network...” (p. 248). Although the first quote suggests that he conceptualized social capital as the relations or connections among persons, the second excerpt, which is quoted more frequently, suggests that he conceptualized it as the resources acquired through those relations. It is likely that this ambiguity led to variation in the conceptualization of social capital in the research that followed.

The distinction between social capital as a property of relations within networks and social capital as the resources accessed through those networks marks a necessary step for the development of social capital theory (Coleman 1988; Lin 2000; Portes 1998; Putnam 1995). This distinction is important for two reasons. First, we agree with Portes’ warning that “equating social capital with the resources acquired through it can easily lead to tautological statements persuades us... [and] is tantamount to saying that the successful succeed” (1998, p. 5). In other words, it is necessary to separate resources from the social relations that allow resources to be shared. Second, defining social capital as the resources acquired through social relations confounds social capital with other forms of capital, such as human capital or cultural capital.

For example, if a person acquires job search information from relatives, some researchers consider the information to be social capital. However, this resource – information – is indistinguishable from human capital, which commonly refers to knowledge, skills, and information. We separate the information from the social relations that allow this information to be shared. Similarly, access to a network of elite status partners (social capital) may provide individuals with a sense of taste and a facility with high-status cultural expressions (cultural capital). Here, we distinguish between cultural sensibilities and the relationships through which those sensibilities are absorbed. If social capital is to become a useful concept for social scientists, it must be distinct from other forms of capital, which makes a strong case for defining it as social relations, not as the resources acquired through those relations.

Individual versus Group

Following Coleman (1988) and Putnam (1995), we conceptualize social capital as a property of groups rather than of individuals. Coleman (1988) underscored that, “unlike other forms of capital, social capital inheres in the structure of relations between actors and among actors. It is not lodged either in the actors themselves or in physical implements of production” (p. S98). In a similar vein, Putnam (1995) consistently used the concept of social capital as a property of groups such as communities and nations. We find Coleman and Putnam’s perspective persuasive: because social capital resides in social relationships, it is most useful to define social capital as a property of groups rather than of individuals. An individual cannot possess social capital alone, as social capital cannot exist without social relationships between at least two people. Furthermore, individual group members can benefit from the social relations of others even if their own relations are limited (known as spillover effects). For example, a young child is unlikely to have many social relationships with adults, but the child may benefit if his or

her parents have many strong connections with other trustworthy adults. Should this child with limited social relations but with full access to the parents' social relations be classified as having low social capital? We assert that the answer is no. This is why it is important to conceptualize social capital as a property of groups, not individuals.

In contrast, Portes (1998), who is perhaps the strongest proponent of the individualist perspective, argued that if social capital were a property of groups, it would simultaneously be a cause and an effect. Portes was particularly concerned with the use of aggregate-level characteristics, such as low crime, as both indicators of social capital and outcomes of social capital. We agree that using the same factors as both indicators and outcomes of social capital is problematic, but the problem stems from the use of the same factor, not the level of aggregation. If Putnam, whose work Portes uses to illustrate this problem, had used as an indicator of social capital the average number of neighbors known by name at time one and crime rate at time two as an outcome, then he may have been able to make a stronger claim about the relation between social capital and crime. Although problems remain in this example, the unit of analysis is not one of them.

Conceiving of social capital as a property of groups rather than individuals further distinguishes social capital from other forms of capital. Whereas individuals may possess money, education, and cultural sensibilities, social capital resides in the group, and individuals may draw on the resources generated by social capital such as information and normative guidance. However, while this perspective enriches the conceptual utility of social capital, it also deepens its operational complexity, which we will discuss later.

Positive versus Negative Effects of Social Capital¹

Social networks typically have boundaries that make them exclusive, and different members of the network may have varying levels of access to other members of the network (Bourdieu 1986; Coleman 1988). We propose that exclusion is a natural by-product of social capital. From marriages to citizenship, exclusive networks are everywhere, and their restrictions, whether implicit or explicit, are a necessary attribute of an effective network. In contrast to Portes and Landolt (1996), who argued that social capital can have negative consequences because “the same strong ties that help members of a group often enable it to exclude others” (p. 19), we argue that exclusivity is not a negative feature of social capital, but rather a feature inherent to belonging to a group. However, social capital can lead to negative outcomes. For example, if belonging to a gang that promotes criminal activity leads to imprisonment, then indeed these social relations have negative effects. But the gang’s exclusivity per se is not a negative effect of social capital. As Coleman (1988) and many others have argued, social capital also can have positive effects. When social relations of trust and shared expectations lead parents and teachers to enforce similar norms about schoolwork, and to circulate information that helps parents guide their children’s educational choices, children may obtain better educational results. Thus, the consequences of social capital can be positive or negative, and one role of the social scientist is to sort out the circumstances under which each occurs.

Structure versus Quality

Finally, to further develop the concept of social capital, it is important to consider both the structure and the quality of the social network (Torche and Valenzuela, 2011). The social capital literature often focuses on one but not the other, resulting in an incomplete picture of

¹ Unfortunately, the label used by Portes and Landolt, “negative social capital,” is sometimes misunderstood to refer to a negative *form* of social capital (as in negative values) rather than negative *effects* of social capital. We use the term to refer to negative effects of social capital.

social relations. For example, Carbonaro (1998) was among the first to test empirically a particular type of social relation, labeled by Coleman as intergenerational closure, which refers to whether parents know the parents of their children's friends. Carbonaro's study used a remarkably precise operational definition of social capital, measuring it as the number of parents of children's friends (up to five friends) known by parents. Although this is a useful measure of the structure of the social network, it does not capture the quality of that network. For example, do parents know the other parents well enough to call them if they need to know where their children are? Do they know them well enough to ask for a ride to a school event? Or are the parents simply acquaintances that see each other occasionally but do not have each other's contact information?

Stanton-Salazar and Dornbusch (1995) also focused on the structure of social networks, and they included innovative measures that simultaneously captured both structure and quality, such as the number of high-status adults named as likely sources of information, the number of nonfamily weak ties, and the number of school-based weak ties. Although these measures are all quantitative, they also capture some aspects of the quality of the resources (e.g., high-status adults) as well as the quality of the relations (e.g., weak ties). Using a more qualitative approach, Horvat, Weininger, and Lareau (2003) used ethnographic data to capture quality differences in the social networks of middle-class, working-class, and poor families (e.g., whether their social ties included professionals as well as the types of professionals in their networks). Horvat, Weininger, and Lareau also reported some quantitative aspects of the structure of social relations, such as the number of peers whose parents were known by name, but these were based on a very small sample size. As is common in ethnographic studies, the sample size is small, but the depth and richness of the qualitative measures go far beyond what is normally captured in

quantitative studies. Optimally, a study of social capital would incorporate both approaches, including qualitative and quantitative measures of both the structure and the quality of social relations.

For social relations to serve as a source of valuable resources, they must be relations of trust, mutual expectations, and shared values. These relations of trust do not necessarily have to be close or intimate. Sometimes unfamiliar people are trustworthy because of their affiliation with a trusted group or organization. For example, a parent may be perfectly willing to entrust her children to someone whose name she does not know simply because that person is providing childcare at a church she knows and trusts. Torche and Valenzuela (2011) argue that social capital in personal relations is likely to be characterized by reciprocity, whereas trust is a distinctive marker of social capital in relations with strangers. Such indicators of the quality of relationships are an essential complement to structural measures.

Summary: Defining Social Capital for an Empirical Assessment

We depart from Coleman's (1988) assertion that "social capital is defined by its function" (p. S98), as this leads to circular logic and prevents a test of causality (Portes and Landolt 1996). We define social capital as relations of trust, mutual expectations, and shared values; we conceptualize social capital as a property of groups rather than of individuals; and we assert that measures of social capital should capture both the structure and quality of social relations. In this study, we focus on relations between parents and other parents in school communities. These social relations play an integral role in connecting individuals affiliated with a school, and we hypothesize that schools with more social capital produce better academic and behavioral outcomes for children.

The Causal Role of Social Capital

A variety of studies have tested the relations between social capital and child outcomes, and although most have relied on longitudinal data, the causal direction in these studies is ambiguous. Does social capital foster school success, or do better social ties emerge in more successful schools? Even Putnam (2000) acknowledged that the causal direction of social capital can be difficult to discern, and Mouw (2006) warned that empirical estimates of social capital might be biased because people choose their friends, group memberships, neighborhoods, and schools, and they tend to associate with similar people, a practice known as social homophily. As a result, what is attributed to social capital may actually be due to a host of other factors that explain the selection of social relations, not the social relations themselves.

Experimental manipulation offers the best method for testing the causal effects of social capital. By randomly assigning a set of schools to an intervention that produces social capital, we can observe the causal effects of improved social capital compared to an untreated set of schools. This experimental design reduces problems of ambiguous causality and omitted variables that have created uncertainties in prior studies. It renders the causal direction clear, and it eliminates the selectivity processes that lead some families to have more, and qualitatively different, social capital than others.

An Intervention Approach to Assessing the Causal Impact of Social Capital

The intervention we use as an exogenous stimulus to social capital is Families and Schools Together (FAST), a program designed to develop relations of trust and shared expectations among parents, school staff, and children (McDonald 2002; McDonald and Frey 1999). FAST has been identified as an exemplary evidence-based model by the U.S. Department of Education and the Office of Juvenile Justice and Delinquency Prevention, and it is listed in the National Registry of Effective Prevention Programs of the U.S. Substance Abuse and Mental

Health Services Administration (Office of Juvenile Justice and Delinquency Prevention 2006; US Department of Education Office of Educational Research and Improvement 1998). FAST is a multi-family after school program that is typically implemented in three stages: (1) active outreach to recruit parents, (2) an eight-week session of weekly group meetings, and (3) two years of follow-up monthly parent-led meetings (FASTWORKS). Before this study, FAST was usually implemented as a single group (hub) of five to ten families of at-risk children, but in this study, FAST was implemented as multiple groups (multi-hub) of universally recruited (as opposed to targeted at-risk) children.

A trained team of parents and professionals led the FAST sessions, which begin with activities designed primarily to strengthen relationships within the family by reinforcing the boundaries and the hierarchy of the family unit (Minuchin 1974).² When families arrive, their hub greets them with a “FAST Hello,” and they sing a song together. Families sit at their own tables for one hour of parent-led family activities. They share a meal, with parents directing their children to serve the food and clean up after eating. Parents direct their children (including siblings) in their native language in games and activities that encourage turn-taking, listening, sharing feelings, and delaying gratification. The children observe that their parents know what to do at the school, regardless of parents’ language proficiency or literacy.

These first activities of the evening, designed to improve social relations between parents and their children, also contribute to trust, shared expectations, and shared values across families and between families and school staff. For example, the family meal develops reciprocity among the families. Each week, one family wins a gift card to a grocery store to purchase and prepare the meal for their hub of families. Additionally, these activities occur within the school context,

² Descriptions of FAST sessions are laid out in FAST materials such as McDonald and Frey (1999) and McDonald (2002), and were verified by on-site observations in this study.

with the participation of school staff, which helps to reduce anxiety about school for children and their parents.

After the family-focused time, the FAST sessions turn to developing relations across families. The children and adults are separated, with the children going to a supervised area to play and complete homework, while the parents begin “Buddy Time,” a fifteen-minute period in which two parents partner together to take turns, each speaking for seven uninterrupted minutes about his or her day. The parents are directed to listen without judging or giving advice. This activity develops friendships across families. Next, the parents come together for “Parent Group Time,” in which the parents sit in a circle to talk together. A FAST team member, who is instructed to participate as little as possible, explains that the group conversation is confidential and that their goal is “to help each other help their child succeed in school.” School staff are not permitted to join this meeting. The Parent Group Time is intended to build a peer support system and facilitate the growth of a social network among the parents.

Parent Group Time is followed by 15 minutes of one-to-one parent-child time called “Special Play,” during which the child takes the lead in playing. The goal is for each parent to pay full attention to the child without criticism or interruption, following four guidelines: “(1) Don’t boss. (2) Don’t teach. (3) Don’t judge. (4) Follow the child’s lead.” Parents are assigned “homework” in which they are to repeat special play at home.

Finally, the hub reunites to further develop relationships across families and between families and school staff. Each week, one family wins a door prize, including the gift card for purchasing and preparing the meal for the next FAST session. Every family wins one time during the 8-week program, ensuring that all families are included. After the prizes are distributed, members can make announcements about school events, community events, and

birthdays, and the entire group comes together in a circle to create a “rain storm” through snapping, clapping, and foot-stomping. These closing activities are designed to reinforce togetherness through routine and a positive group experience.

One of the distinctive features of FAST is that the eight-week intervention is followed by two years of parent-led monthly meetings, where parents can further develop and reinforce the relationships established through FAST. The follow-up meetings, known as FASTWORKS, provide a supportive structure for increased parent involvement, which should become self-sustaining as it maintains the newly formed relationships. Especially among economically disadvantaged minorities, such a structure may be a useful vehicle for navigating the educational system for their children’s success.

Data and Methods

Having settled on the FAST program as our tool for manipulating social capital, we then selected research sites, developed measures of social capital, and designed an analytic strategy to test for causal effects.

Sample and Design

To implement FAST, we turned to elementary schools in San Antonio, Texas, and Phoenix, Arizona. We selected these communities for three reasons. First, they have effective social service agencies with successful experience implementing FAST in the past. Second, many schools in these communities had high concentrations of low-income, Latino families, a prime target for increasing social capital (Gamoran et al. 2012). Third, we found school districts in these communities that were willing to participate in the study, including randomization of participating schools to FAST and control groups. One large district in San Antonio participated in the study, as did three medium-sized districts in Phoenix, for a total of 52 schools.

Randomization of schools occurred within districts and we include district as a fixed effect (block) in our analyses. To ensure representation of the most disadvantaged schools in our San Antonio district, we created two blocks of schools based on percentage of students on free or reduced-priced lunch, and randomized students within blocks. This blocking is also indicated by a fixed effect in our analyses.

Following a cluster-randomized design, we randomly assigned 26 schools (half in San Antonio and half in Phoenix) to participate in FAST and the other 26 schools to continue business as usual without FAST. Because FAST implementation is labor intensive, we staggered implementation across two years, and we randomly assigned schools to two cohorts, where Cohort 1 consisted of 12 FAST schools and 12 control schools during the 2008-09 academic year, and Cohort 2 consisted of 14 FAST schools and 14 control schools during the 2009-10 academic year. A total of 3,049 students participated in the study, all of whom were first graders at the start of the study. Students and their families were followed through third grade (2010-11 for Cohort 1, 2011-12 for Cohort 2). The data for this paper are restricted to first grade.

FAST and control schools were compared to determine whether the randomization produced comparable groups, and t-tests demonstrated that there were no statistically significant differences in terms of the characteristics normally used to compare schools, including size, racial/ethnic composition, and the proportions of students who are eligible for the national school lunch program, who are Limited English Proficient, who receive special education services, and who are proficient in reading and math state assessments (Gamoran et al. 2012).

We aimed to recruit universally all first graders in participating schools, which on average had 96 first graders per school. The research team, along with staff from the local social service agencies that implemented FAST, recruited families to the study at FAST and control

schools through family dinner events, parent-teacher conferences, and home visits. Potential participants learned about the study, chose whether to consent to the study, and received a \$10 gift card as compensation for filling out a short 3-page pre-test questionnaire. Parents in the FAST schools also learned about FAST and chose whether to consent and participate in FAST.³ Teachers also learned about the study, chose whether to participate, and were compensated \$150 for filling out questionnaires for all of their participating students about two weeks after the FAST intervention concluded.

Parents' Social Capital

We measured social capital among parents within the same school both prior to the treatment period and afterward in ways that capture both the structure and quality of social relations. First, parents were asked to characterize their relationships with other parents at the school in terms of the how many parents of their child's friends at the school that they know. This is the widely used measure of intergenerational closure (e.g., Morgan and Sorensen, 1999). Second, we measured the quality of relations within parents' networks as shared expectations and reciprocity among parents. We asked parents to report how much they thought other parents at the school shared their expectations for their child. Parents could say: "not at all," "a little," "some," or "a lot." (We also asked parents a series of questions about how much they interacted with other parents at the school in activities such as babysitting, shopping, discussing problems, and socializing at meals or parties, with response categories are the same as those for the shared expectations measure described above, but these measures are not used in this paper.)

³ In FAST schools, families were invited to participate in the study even if they declined to attend FAST, and they were welcome at FAST even if they declined to participate in FAST. In practice, however, virtually everyone in the FAST schools who consented to FAST also consented to the study, and vice versa. Not all who consented to FAST actually attended, however.

Children's Socio-Emotional Behavior

Teachers reported on student behaviors and social skills through a series of questions from the Strengths and Difficulties Questionnaire (SDQ) (Goodman, 1997). This widely used instrument for assessing social adjustment and behavior problems taps five dimensions of psychological functioning: emotional symptoms, behavior problems, hyperactivity/inattention, peer relationship problems, and prosocial behavior.

We report on each of the five dimensions separately, and we also combined emotional symptoms, conduct problems, hyperactivity, and peer problems into a "total difficulties" measure. Indices for each of these constructs exhibit moderate levels of internal consistency, with alpha coefficients that range from 0.70 to 0.90, with the exception of the peer problems scale, which has an alpha of 0.60. Further details about the dependent variables were reported by Gamoran et al. (2012).

Analytic Plan

We employ multiple analytic strategies to uncover the effects of social capital, each of which has a unique set of assumptions with unique consequences for the hypothesized set of relations we are able to test. The purpose of this paper is to explore whether or not, when taken together, our analytic approaches provide sufficient evidence to test whether there is a positive, causal effect of social capital on child outcomes in our data.

FAST as an Indicator of Social Capital

Because the treatment was randomized at the level of the school, multilevel models are the appropriate technique for estimating treatment effects (Raudenbush 1997). We estimated multilevel models of students nested within schools, with FAST included as a predictor at the school level. These models also included pre-treatment indicators of parents' shared

expectations, reciprocity, and intergenerational closure. We assess the impact of FAST on social capital measures, and of FAST on children's socio-emotional behavior.

From one perspective, FAST effects on child outcomes may be viewed as evidence of causal effects of social capital. Indeed, FAST may be a better proxy for social capital than many other proxies that have been used in past studies, such as family structure, educational expectations, discussions with children about academic issues, PTA involvement, and residential mobility (e.g., Kao and Rutherford 2007; Kim and Schneider 2006; Ream and Palardy 2008; Teachman, Paasch, and Carver 1997). However, we will also endeavor to assess social capital effects directly, as explained below.

In assessing FAST effects, we are interested in both "intent to treat" (ITT) effects and "treatment on the treated" (TOT) effects. ITT effects assess the impact on parent or child outcomes of attending a school that was assigned to FAST implementation. The ITT analysis provides important policy information: it answers the question of which benefits one might expect to see if school districts like the ones we studied were to provide FAST in their schools. The TOT effect measures the impact of FAST on those who actually "graduated" from FAST, meaning that they attended at least 6 of the 8 weekly FAST sessions. This estimate may be of greater interest to our study if social capital is elevated mainly for those who graduate from FAST and not for others in the school.

We estimate the ITT effect with a straightforward multilevel model. We estimate the TOT effect with a complier average causal effect (CACE) model that identifies individuals in the control group who would have been likely to attend FAST had it been offered to them (Angrist, Imbens and Rubin 1996; Bloom 1984; Borman and Dowling 2006; Jo et al. 2008; Muthen and Muthen 1998-2010). The complier average causal effect is the difference in outcomes between

FAST participants who graduated from FAST and control group members who would have been likely to graduate had they had the chance. We modeled compliance in the comparison schools as a function of pretreatment measures of social capital—intergenerational closure, shared expectations, and reciprocity, which are strong predictors of participation in FAST within the treatment group. Thus, compliance in the CACE model is treated as a latent variable. The final class counts and proportions for the latent class patterns are based on estimated posterior probabilities. The prediction model indicated that the pretreatment measures of social capital effectively discriminated between the latent compliance classes.⁴

FAST as an Instrument for Social Capital

For a more direct assessment of social capital effects on child outcomes, we adopt an instrumental variables approach in which assignment to FAST serves as an instrument for the causal effect of social capital. Holland (1988) demonstrated that if there is an unmediated effect of an intervention, the effect of the mediator on the outcome is equal to the instrumental variable (IV) estimand (the effect of Z on Y divided by the effect of Z on M). Sobel (2008) further demonstrated conditions under which the IV estimand identifies the mediated effects under a weaker assumption, the “exclusion restriction.” The exclusion restriction assumes that the treatment effect operates only through the mediator; if that is true, variation in the mediator induced by the treatment assignment can be interpreted as a causal effect on the outcome. As a result these models are often underpowered (Morgan and Winship 2007). In our case, if the effects of FAST operate *only* through social capital, then the IV estimate reflects the causal effects of social capital on child outcomes.

⁴ Across individuals assigned to the complier class, the average predicted posterior probability was about 0.98.

While the exclusion restriction may hold in principle, it seems unlikely that our measures capture everything that is important about social capital, and in the way we implement the IV approach in this paper, with a single measure of social capital, the exclusion restriction certainly does not hold. Consequently we explored another approach to assessing causal effects of social capital in which the exclusion restriction is not required.

Social Capital as a Mediator of FAST Effects

As a third approach to identifying causal effects of social capital, we estimate a structural equation model (SEM) that recovers the average causal mediation effect (ACME) of FAST through explicit measures of social capital (Imai et al. 2010a). In typical experimental studies, treatment effects are assessed using recursive linear structural equation models (e.g., Baron and Kenny, 1986; Judd and Kenny, 1981a; 1981b) to decompose the total effects of an intervention into direct effects and indirect effects through treatment receipt, such that the total effect is equal to the sum of the indirect and direct effect. However, as Holland (1988) argued, structural parameters for direct and indirect effects are not necessarily equivalent to corresponding parameters from a causal model, and applying SEM to experimental data may lead to causal inferences about both the direct and indirect effect that are not valid. We draw from the rapidly developing statistical literature on causal mediation developed in response to these critiques (Imai, Keele, and Tingley, 2010a; Imai, Keele, and Yamamoto, 2010b).

The goal of causal mediation models is to identify the mediators through which a program or intervention may operate.⁵ Imai et al. (2010) provided a single framework for the

⁵ The goal of our study is slightly different. Rather than estimating the effect of FAST that works through increases in social capital, we are interested in the effect of social capital. Despite this challenge, we pursue the experimental mediation approach for two reasons. First, our study is the first experimental study of social capital and provides a jumping off point for future studies in sociology that are interested social processes. Second, one reason for pursuing social capital research is the hope that under-served populations can gain social capital through interventions. Future comparisons of the causal mediation effect of FAST with that of other interventions designed to improve social

formal definition, identification, and estimation of causal mediation following the counterfactual framework of causal inference.⁶ Although this approach is not subject to the exclusion restriction, it requires a different assumption: sequential ignorability (SI). According to this assumption, there are no unobserved confounders that causally affect both the mediator and the outcome after conditioning on the observed treatment and pretreatment covariates. (i.e., omitted exogenous variables are ignorable). In experimental studies, it is reasonable to assume no unobserved confounders of the treatment, in that the treatment was randomly assigned, and departures from random assignment can be corrected by including pretreatment covariates. Assuming no unobserved confounders of the relation between the mediator and the outcome is more problematic, and Imai and his colleagues provided sensitivity analyses to help judge whether the mediation findings are robust to violations of the sequential ignorability assumption (Imai, Keele, and Tingley, 2010a; Imai, Keele, and Yamamoto, 2010b).

Results

We present results in three sections: first the multilevel models of ITT and TOT effects on both parents' social capital and children's socio-emotional behavior; then the IV results that assess the casual impact of parents' social capital on child outcomes under the exclusion restriction; and finally the causal mediation results that examine the extent to which social capital is causally implicated in the effects of FAST on child outcomes.

FAST as an Indicator of Social Capital

capital, along with cost-effectiveness studies, might reveal real-world ways that we can improve children's outcomes.

⁶ We used the R package -mediation- which is available from the free Comprehensive R Archive Network (Imai, Keele, Tingley, & Yamamoto, 2010; R Development Core Team, 2009). <http://cran.r-project.org/web/packages/mediation>

Table 1 displays estimates of FAST effects on two measures of parent social capital: intergenerational closure and shared expectations. The results indicate that FAST generates structural aspects and quality of parent social capital. In the intent-to-treat (ITT) analysis, parents in FAST schools were more likely to know other parents in the school than parents in comparison schools (effect size (ES) = 0.13). Parents in FAST schools were also more likely to share expectations with other parents, as they had 33% higher odds of sharing expectations with other parents than parents in control schools. These effects are much stronger when parents who graduated from FAST were compared with parents who would have graduated if their children had the chance to attend a FAST school: in the treatment-on-treated (TOT) results, the effect size for intergenerational closure is 0.35, and parents had 97% higher odds of sharing expectations with other parents. All of these effects are statistically significant. It is clear from these estimates that FAST sets the foundation for the generation of social capital within schools.

Table 2 examines ITT and TOT effects on children's socio-emotional behaviors. The intent-to-treat (ITT) estimates suggest that children in FAST schools have lower levels of total problem behaviors (ES= -0.13, p-value= 0.04). This effect is driven primarily by the peer problems behaviors subscale (ES= -0.19, p-value= 0.03). For some outcomes, the effects of FAST are larger if we compare "apples to apples," as we do in the TOT models. For example, the effect size for conduct problems is nearly twice as large in the TOT model (-0.16 versus -0.07) and the effect for hyperactivity is also more pronounced (-0.16 versus -0.10). The effect on total behavior problems, however, is only slightly larger in the TOT compared to the ITT results (-0.14 versus -0.13). Effects on prosocial behavior – which is not included in the total behavior problems scale – are very small and non-significant in both cases.

Overall, to the extent that FAST is an indicator of social capital, these findings provide evidence of causal effects of social capital on classroom behavior problems, where an increase in social capital causes an improvement in behavior problems. The finding that the ITT effects on total problem behaviors are nearly as large as the TOT effects suggests that some spillover may occur: students whose families did not graduate from FAST benefitted almost as much as those whose families did graduate, perhaps because FAST participation has resulted in a better classroom climate for all students.

FAST as an Instrument for Social Capital

The instrumental variable (IV) model results can be found in Table 3. These models use assignment to FAST as an instrument for social capital effects on children's outcomes. We estimate two models, one in which the number of parents known is instrumented by FAST and the other in which shared expectations is instrumented by FAST. We treat these estimates as exploratory because the exclusion restriction is clearly violated. The IV models, like our initial results, provide further evidence that social capital reduces behaviors that are problematic in classrooms. However, these results are not statistically significant due to the greater power demands of the IV models.

Social Capital as a Mediator of FAST Effects

Tables 4 and 5 provide results of the causal mediation models. For brevity, we focus on two social capital indicators – intergenerational closure and parents' shared expectations – and two child outcomes – peer problem behavior and total behavior problems. For each analysis, we estimate three parameters: the total effect, which represents the overall effect of FAST on the child outcome; the mediation effect, which reflects the indirect effect of FAST on the behavioral

outcome via intergenerational closure; and the direct effect of FAST on the behavioral outcome that is not mediated by social capital.

The results again implicate social capital causally. Table 4 displays results for intergenerational closure as the mediator. In the analysis of total behavior problems, the mediation effect of -0.11 accounts for about 31% of the total effect of FAST. In the analysis of peer problems all three of the parameters are statistically significant and the causal mediator accounts for about 17% of the total effect. Results for mediation via parents' shared expectations are similar: Table 5 indicates that parents' shared expectations mediates 21% of the causal effect of FAST on total problem behaviors, and 11% of the causal effect of FAST on peer problems. Mediation effects are statistically significant in both cases.

Although these results, building on the earlier analyses, seem to add up to persuasive evidence for the causal role of social capital, sensitivity analyses inject some caution into our conclusion. Figures 1 (intergenerational closure) and Figure 2 (parents' shared expectations) reveal how large an unobserved confounder would need to be to reduce the mediation effect to zero. In these figures, the vertical axis represents the average causal mediation effect (ACME) and the horizontal axis represents the impact of a potential confounder (sensitivity parameter). Focusing on mediation effects for total problem behaviors, in the case of both mediators, a confounder with an impact of just -0.10 would be enough to change our results. Results are similar for peer problem behavior (not shown). Our conclusion of causal effects must be tempered by this uncertainty.

Discussion and Conclusions

In this paper we constructed a more focused conception of social capital, with more precise measures, that facilitated a causal analysis of the effects of social capital on children's

outcomes. Taken together, the three analytical approaches in this study – multilevel models of ITT and TOT effects, IV models, and causal mediation models – provide stronger evidence than previous studies that social capital improves children’s outcomes and that these improvements are not simply a result of other factors that explain the selection of social relations but rather that these improvements result from the social relations themselves.

This study has methodological and substantive implications. Methodologically, it is clear that even with a randomized control trial, which is considered by many researchers as the “gold standard” for providing causal evidence, it is difficult to arrive at a definitive statement as to whether a causal relation exists. Each method requires different assumptions, and the strongest claims result from using not a single approach but rather a combination of approaches that describe carefully the assumptions and limitations of each.

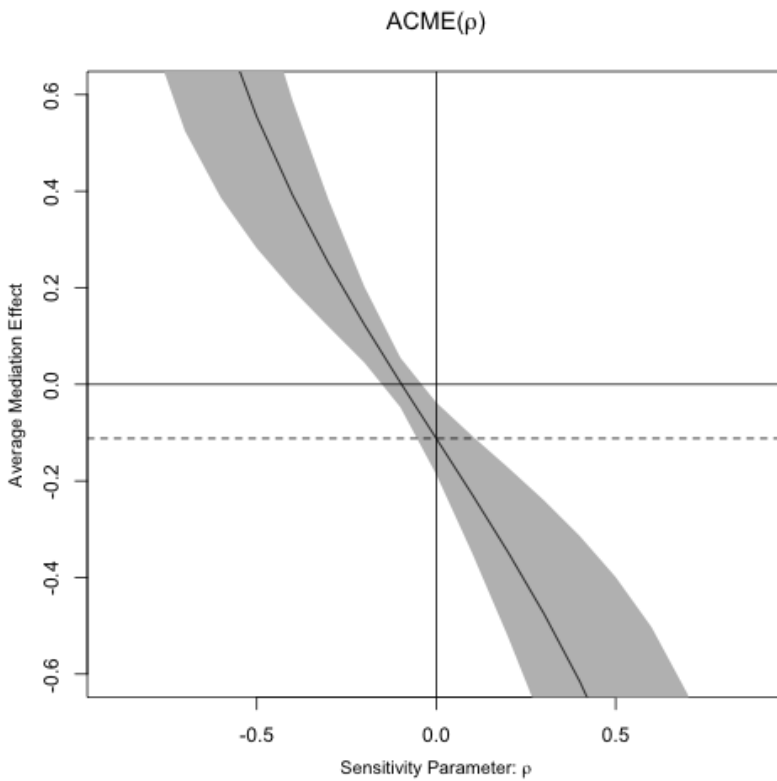
Substantively, the causal evidence presented in this study suggests that investing in improving the quantity and quality of social relationships among parents at schools is worthwhile. When parents know the parents of their children’s friends and they share expectations for their children, their children behave better in school. Behavioral improvements, in turn, are linked to performance improvements. Parents and schools can undertake many steps to improve social relationships, including after school programs such as FAST and creating opportunities for parents to get to know each other better formally and informally.

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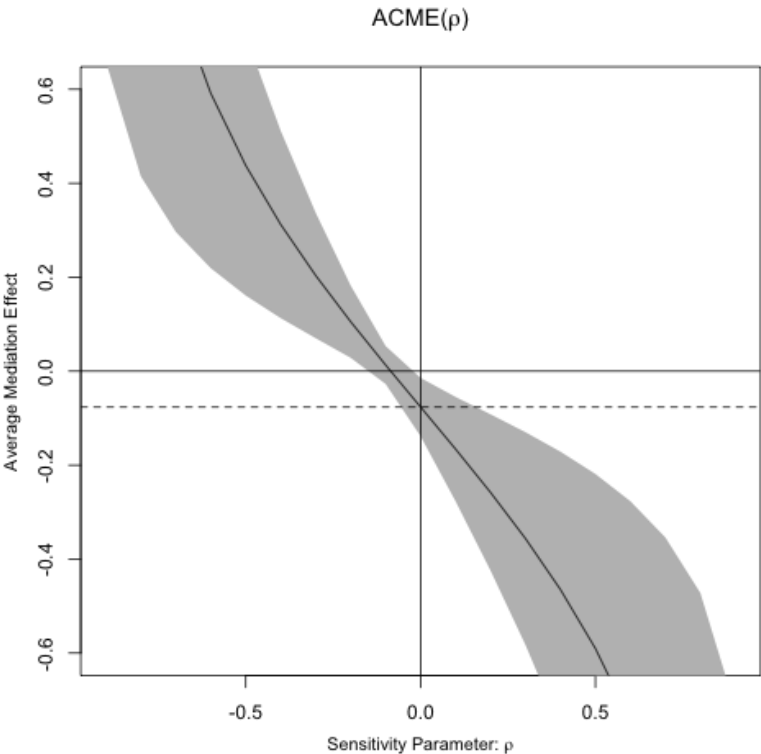
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Figure 1. Sensitivity analysis for Average Causal Mediation Effect of Intergenerational Closure on Total Problem Behaviors



Note: This figure shows that an unobserved confounder (sensitivity parameter) of -0.10 would move the mediation effect from -0.11 to 0.

Figure 2. Sensitivity Analysis for Average Causal Mediation Effect of Parents' Shared Expectations on Total Problem Behaviors



Note: This figure shows that an unobserved confounder (sensitivity parameter) of -0.10 would move the mediation effect from -0.08 to 0.

Table 1. Effect of FAST on Parents' Social Capital

	Number of Parents Known			Shared Expectations		
	Effect Size	Est/SE	P-Value	Effect Size	Est/SE	P-Value
ITT	0.13	3.02	0.003	0.33	3.28	0.001
TOT	0.35	2.83	0.005	0.97	2.93	0.003

N = 1,951 Students in 52 Schools

Models adjusted for pre-treatment measures of social capital and design effects

Table 2. Effect of FAST on Emotional and Behavioral Problems

	Emotional Symptoms			Conduct Problems		
	Effect Size	Est/SE	P-Value	Effect Size	Est/SE	P-Value
ITT	-0.04	-0.66	0.51	-0.07	-1.29	0.20
TOT	0.02	0.20	0.84	-0.16	-1.96	0.05
	Hyperactivity			Peer Problem Behaviors		
	Effect Size	Est/SE	P-Value	Effect Size	Est/SE	P-Value
ITT	-0.10	-1.86	0.06	-0.18	-2.23	0.03
TOT	-0.16	-1.53	0.13	-0.18	-0.53	0.60
	Total Behavioral Problems			Prosocial Behaviors		
	Effect Size	Est/SE	P-Value	Effect Size	Est/SE	P-Value
ITT	-0.13	-2.05	0.04	0.01	0.22	0.83
TOT	-0.14	-1.70	0.09	0.02	0.18	0.86

N = 1,951 Students in 52 Schools

Models adjusted for pre-treatment measures of social capital and design effects

Table 3. Instrumented Effect of Social Capital on Emotional or Problem Behaviors

	Number of Parents Known		
	Effect Size	Est/SE	P-Value
Emotional Symptoms	-0.08	-0.35	0.73
Conduct Problems	-0.04	-0.20	0.84
Hyperactivity	-0.11	-0.60	0.55
Peer Problem Behaviors	-0.09	-1.29	0.20
Total Problem Behaviors	-0.18	-0.76	0.45
Prosocial Behaviors	-0.08	-0.38	0.71
Shared Expectations			
	Effect Size	Est/SE	P-Value
Emotional Symptoms	-0.18	-0.36	0.72
Conduct Problems	-0.09	-0.21	0.84
Hyperactivity	-0.44	-1.33	0.19
Peer Problem Behaviors	-0.81	-1.33	0.19
Total Problem Behaviors	-0.41	-0.79	0.44
Prosocial Behaviors	-0.18	-0.37	0.71

N = 1,951 Students in 52 Schools

Models adjusted for pre-treatment measures of social capital and design effects

Table 4. Causal Mediation Effects of Number of Parents Known for Two Behavioral Outcomes

	Total Problem Behaviors			
	Estimate	95% CI Lower	95% CI Higher	P-Value
Mediation Effect	-0.11	-0.20	-0.05	<0.01
Direct Effect	-0.24	-0.80	0.30	0.38
Total Effect	-0.31	-0.91	0.20	0.21
Proportion via Mediation	0.31	-3.35	3.04	0.21
	Peer Problems			
	Estimate	95% CI Lower	95% CI Higher	P-Value
Mediation Effect	-0.03	-0.05	-0.01	<0.01
Direct Effect	-0.15	-0.30	<0.01	0.05
Total Effect	-0.18	-0.32	-0.04	0.01
Proportion via Mediation	0.17	0.05	0.77	0.01

N = 1,951 Students in 52 Schools

Models adjusted for pre-treatment measures of social capital and design effects

Notes: “Total Effect” is effect of FAST on the behavioral outcome; “Mediation Effect” is the indirect effect of FAST on the behavioral outcome via intergenerational closure (Number of Parents Known); “Total Effect” is the sum of the direct and mediation effects; and “Proportion via Mediation” is the proportion of the total effect of FAST that is mediated by intergenerational closure.

Table 5. Causal Mediation Effects of Parents' Shared Expectations for Two Behavioral Outcomes

	Total Problem Behaviors			
	Estimate	95% CI Lower	95% CI Higher	P-Value
Mediation Effect	-0.08	-0.15	-0.02	<0.01
Direct Effect	-0.28	-0.81	0.29	0.32
Total Effect	-0.36	-0.88	0.23	0.23
Proportion via Mediation	0.21	-2.03	1.47	0.23
	Peer Problems			
	Estimate	95% CI Lower	95% CI Higher	P-Value
Mediation Effect	-0.02	-0.04	-0.01	<0.01
Direct Effect	-0.16	-0.31	-0.02	0.02
Total Effect	-0.18	-0.33	-0.04	0.01
Proportion via Mediation	0.11	0.03	0.44	0.01

N = 1,951 Students in 52 Schools

Models adjusted for pre-treatment measures of social capital and design effects

Notes: "Total Effect" is effect of FAST on the behavioral outcome; "Mediation Effect" is the indirect effect of FAST on the behavioral outcome via parents' shared expectations; "Total Effect" is the sum of the direct and mediation effects; and "Proportion via Mediation" is the proportion of the total effect of FAST that is mediated by parents' shared expectations.