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Do Effects of Social-Emotional Learning Programs Vary by Level of Parent Participation? Evidence From the Randomized Trial of INSIGHTS

Meghan P. McCormick[a], Elise Cappella[b], Erin O'Connor[b], Jennifer L. Hill[b], and Sandee McClowry[b]

ABSTRACT
Social-Emotional Learning (SEL) programs aim to improve students' social-emotional competencies in order to enhance their achievement. Although SEL programs typically implement classroom curricula, some programs also include a component for parents. Yet, little is known about the types of parents likely to participate in services, and whether parent participation moderates program effects on student outcomes in low-income urban schools. This article aims to fill these gaps in the literature using data from the randomized trial of the SEL program INSIGHTS into Children's Temperament ($N = 435$ parent/child dyads), which was conducted in 22 low-income urban elementary schools during children's kindergarten and first-grade year. Descriptive findings revealed that children at lower risk for poor achievement had parents who were more likely to participate in program services. In addition, findings from inverse probability of treatment-weighting models demonstrated larger effects of INSIGHTS on academic, attentional, and behavioral outcomes for children whose parents participated at lower rates. Implications for prevention science and SEL program implementation and scale-up are discussed.

KEYWORDS
social-emotional learning
parent programs
parent involvement
elementary school
achievement
behaviors
attention

A wide and rich body of literature has identified the family as the key context influencing children's academic development (Kreppner & Lerner, 2013). Given the critical role of parents in children's academic development (e.g., Fan & Chen, 2001; Jeynes, 2005), school districts and policymakers have sought to engage parents in children's learning, particularly low-income families (Booth & Dunn, 2013). Increasing parental engagement has been one of the focal points of both President Bush's No Child Left Behind Act and President Obama's Race to the Top policies (Robinson & Harris, 2014). Meta-analyses conclude that efforts to engage low-income parents do improve students' academic achievement (Jeynes, 2012, 2015). Such research has prompted developers of some school-based preventive interventions to integrate programming components targeted at students' parents.

Social-Emotional Learning (SEL) programs are one such type of school-based preventive intervention. SEL programs aim to improve children's social-emotional competencies

CONTACT Meghan P. McCormick meghan.mccormick@mdrc.org MDRC, Families & Children Policy Area, 16 East 34th St. 19th Floor, New York, NY 10016, USA.
[a]MDRC, New York, New York, USA
[b]New York University, New York, New York, USA
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(behavioral regulation, attentional skills, problem-solving, social skills), in order to support their academic development. Typically implemented in school settings, SEL programs explicitly target teacher and student individual-level skill development and the quality of classroom contexts (e.g., Brown, Jones, LaRusso, & Aber, 2010; Rivers, Brackett, Reyes, Elbertson, & Salovey, 2013). Some interventions also use a comprehensive prevention approach that engages parents in services (e.g., Kumpfer, Alvarado, Smith, & Bellamy, 2002). Such models theorize that parents can be effective in enhancing social-emotional skill development at home if they are exposed to the content that their children learn at school.

Yet, given few quantitative studies on parent program take-up rates, little is known about the factors that are predictive of parents' participation in SEL programs (McMahon et al., 2001; Wilson, 2012). Moreover, although ecological theory suggests that two-generation service models should enhance the efficacy of SEL interventions (Bronfenbrenner & Morris, 1998; Conduct Problems Prevention Research Group, 2002), there is little empirical research testing this hypothesis. In fact, no previous studies have used causal modeling approaches to determine whether SEL program effects differ by varied levels of parent engagement in services. Given that parent programs can be resource intensive, it is critical to determine whether they do in fact enhance the benefits of SEL programs for students. Knowledge of parent program efficacy can inform future SEL program development, implementation, and scale-up in urban elementary schools.

This article specifically examines the parenting component of INSIGHTS into Children's Temperament, an SEL program that includes a manualized curriculum for teachers, students, and parents. Results from a randomized trial revealed that INSIGHTS improved students' achievement and sustained attention, and reduced their disruptive behaviors (O'Connor, Cappella, McCormick, & McClowry, 2014). The current study extends this work and tests whether program impacts on low-income urban kindergarten and first-grade students' academic, social-emotional, and behavioral outcomes differed by levels of parent participation. The study uses modeling techniques that weaken the assumptions necessary for drawing causal inferences from the findings.

Social-Emotional Learning Programs in Urban Elementary Schools

Children who develop core social-emotional competencies (e.g., emotional and behavioral regulation, attention skills) in early schooling are more likely to successfully navigate the transition to elementary school (Fantuzzo et al., 2007; Rimm-Kaufman, Fan, Chiu, & You, 2007). Low-income racial/ethnic minority children are at heightened risk for emotional and social difficulties in elementary school (Brooks-Gunn, Duncan, & Aber, 1997; Li-Grining, 2007). Given this evidence, and political and public pressure to close socioeconomic and racial achievement gaps, a growing movement seeks to improve the social-emotional skills of children attending low-income urban elementary schools (Collaborative for Social-Emotional and Academic Learning, 2014; Kahn, 2013; Tough, 2011). SEL programs aim to accomplish this goal by targeting an interrelated set of cognitive, emotional, and behavioral skills regarded as foundational for academic performance. SEL programs emphasize that responsive learning environments support student–teacher and peer relationships, family involvement, improved classroom management and instructional practices, and school community-building activities (Hawkins, Smith, & Catalano, 2004). Many SEL programs involve classroom curricula (Durlak, Weissberg, Dymnicki, Taylor, and Schellinger, 2011). Less

frequently implemented are interventions that supplement classroom programming with family services (Greenberg et al., 2003).

SEL programs have generally been successful in improving the social-emotional skills of low-income racially and ethnically diverse students in early school settings (Jones, Brown, & Aber, 2011; Morris et al., 2014; Webster-Stratton, Reid, & Stoolmiller, 2008). Additional evidence suggests benefits of SEL programs for students' academic development (Jones et al., 2011; O'Connor et al., 2014; Raver et al., 2011). A meta-analysis by Durlak et al. (2011; $N =$ 213 studies) identified a positive and significant impact of SEL programs on children's academic achievement of about half a standard deviation. This effect size translates into an 11 percentile point gain on achievement tests.

Although these findings have helped prompt school districts and states to adopt SEL interventions as part of their elementary school curricula, such wide-scale implementation may be premature (CASEL, 2014; Kahn, 2013; Tuttle et al., 2013). Indeed, a recent large-scale random assignment evaluation of three SEL curricula, implemented in Head Start settings, found no effects of any curricula on low-income children's pre-academic skills or kindergarten academic outcomes (Morris et al., 2014). As argued by Blair and Raver (2014), however, it may be that preschool SEL programs are less effective for enhancing academic skills than SEL programs implemented in kindergarten and first-grade settings. In addition, most SEL programs have typically only been tested in small-scale efficacy trials where researchers use an "intent-to-treat" research design to compare the outcomes of a group randomly assigned to the program (i.e., the treatment group) with a group randomly assigned to a control group (Durlak et al., 2011). This strategy produces unbiased impact estimates of program efficacy. Yet, few studies of SEL programs have sought to identify the "active ingredient" important for supporting student outcomes (Abry, Hulleman, & Rimm-Kaufmann, 2014).

Understanding SEL program parent components, across a variety of settings, can have implications for future program implementation and scale-up. It is possible that two-generation comprehensive program models may not be feasible when implemented at scale and outside the direct supervision of the program developer and research team (Elliott & Mihalic, 2004). Parent programs delivered in low-income urban school settings can be difficult to implement because they are typically voluntary and because it is logistically difficult to align services with many of the daily demands faced by low-income parents (Levin, 2013).

The Role of Parents in SEL Programs

There is debate about whether it is important to include services that engage parents in school-based interventions. Some research has found that integrative and comprehensive programs involving schools *and* families are most effective for supporting students' development (Catalano, Berglund, Ryan, Lonczak, & Hawkins, 2004; Flay, Graumlich, Segawa, Bruns, & Holliday, 2004). Theoretically, parent participation in programs would reinforce positive practices from school when children are at home. In line with Bandura's social learning theory, parental modeling of the SEL practices taught in sessions would be hypothesized to be a powerful agent for changing children's behaviors (Greenberg et al., 2003; Grusec, 1992). There is empirical evidence for this theory. In a recent meta-analysis, Grindal et al. (2015) found larger impacts (of .25 *SD*s) on children's cognitive skills for early childhood programs that included a parenting component.

Other evaluations have not shown that engaging parents in services increases the size of program effects on students (Marcon, 1999). Head Start offers parents a variety of services in addition to programming for children (Sydnor et al., 2007). The National Head Start evaluation, however, found no impacts on parenting outcomes theorized to enhance child outcomes (Puma et al., 2010). Moreover, early childhood programs are different from traditional K–12 settings because they presuppose a baseline level of parent engagement that does not necessarily translate into later grades (Durlak, 2010). More work is thus needed to understand the effect of parent components embedded in elementary school SEL programs.

An additional constraint is that nontraditional work schedules, limited free time, and stressors related to health, residential mobility, parental education, and transportation can negatively affect low-income parent program participation (McDermott & Rothenberg, 2000; Pena, 2000; Wilson, 2012). Another body of research argues that parents' likelihood of engaging in school-based activities is partly dependent on psychosocial or cultural factors such as parents' role beliefs, invitations from children and teachers to participate (Green, Walker, Hoover-Dempsey, & Sandler, 2007), trust of school settings (McDermott & Rothenberg, 2000), shared language, welcoming attitudes of school staff, and cultural influences (Hoover-Dempsey et al., 2005; Pena, 2000). Few researchers, however, have identified the empirical factors likely to predict low-income parent participation in voluntary school-based programs. In general, voluntary parent programs typically attract and retain a small percentage of eligible parents (~5%), who are likely to be higher in SES (Durlak & DuPre, 2008).

One can use a dosage analysis to test whether parent components of school-based interventions, such as SEL programs, are critical for supporting outcomes. Although no studies of SEL parent programs have done so, researchers of some school-based parenting interventions have considered dosage effects. For example, Gross and colleagues (2009) found that parents who participated in at least 50% of Chicago Parenting Program (CPP) sessions reported greater improvements in parenting self-efficacy, more consistent discipline, greater warmth, and a greater decline in behavior problems when compared to controls. In addition, Brotman and colleagues (2011) found that when parents attended more sessions of the ParentCorps program, impacts on parenting skills and improved behaviors were larger. Findings suggest that including a high-dosage parent component within the context of an SEL intervention should help to build on and reinforce the skills that children are learning at school when at home. In turn, children's social-emotional, behavioral, and possibly academic outcomes will be better supported.

However, relative to established interventions such as CPP and ParentCorps that specifically target parenting practices, dosage of program content delivered to parents in SEL programs may be more variable. For example, in a randomized trial of the preschool version of the SEL program the Incredible Years, researchers found that about one third of parents assigned to the treatment group never attended a session, and from 12% to more than 50% of parents attended less than half of the sessions (Webster-Stratton, Reid, & Hammond, 2001). Given these trends, it is important to explicitly examine effects of parent programs within the context of an SEL program. Doing so in early elementary school is particularly important because this experience is universal for school-aged children, and the start to formal schooling is a key period for engaging parents in children's schooling (Hill & Taylor, 2004).

In addition, there are potential methodological limitations in previous dosage studies of parenting programs, SEL programs, and school-based preventive interventions. There are a

number of studies where dosage, a posttreatment variable, has been used as a moderator to determine differential effects based on program participation. In doing so, past studies have compared the full control group to the members of the treatment group who did take up the treatment. This approach is likely to bias dosage impact estimates, as a comparison is made between high-dose treatment participants and the full control group (Zhai et al., 2010). In the examples discussed previously (CPP, ParentCorps, Incredible Years), all dosage effects were examined by interacting dosage and treatment in the regression models predicting outcomes.

Recent studies of education and social policy interventions have used a variety of weighting and matching methods to address potential biases (Hill, Brooks-Gunn, & Waldfogel, 2003; Zhai et al., 2010). In this framework, researchers predict the likelihood that control group members would have taken up the treatment at high levels, based on a comprehensive set of baseline covariates. Those likelihoods are used to compare high-dosage participants to the members of the control group predicted to be high-dosage participants. Such approaches can address some of the methodological limitations inherent in traditional dosage analyses.

INSIGHTS Into Children's Temperament (INSIGHTS)

The current article examines the role of the parent program in one particular SEL program—INSIGHTS into Children's Temperament. INSIGHTS is a comprehensive preventive intervention with teacher, parent, and classroom programs. In brief, INSIGHTS provides teachers and parents with a temperament framework for supporting the individual differences of children. Temperament is defined as the consistent reaction style that an individual exhibits in situations that involve stress or change (Rothbart, Ahadi, & Evans, 2000). Temperament is biologically based, multidimensional, relatively stable through childhood, and not malleable to intervention (Rothbart & Bates, 2006; Chess & Thomas, 1984). Key to temperament theory is the importance of goodness of fit, or ensuring an appropriate match between the demands, expectations, and opportunities of the environment and the child's temperament (Chess & Thomas, 1984).

INSIGHTS helps parents and teachers recognize a child's temperament and respond with warmth and discipline strategies that support adaptive social-emotional and behavioral outcomes (O'Connor et al., 2014). The teacher and parent programs aim to enhance goodness of fit by assisting caregivers in determining the ideal combination of warmth and discipline strategies for a particular child. Primary grade students also participate in classroom curricula designed to enhance empathy for individuals with different temperaments and to use problem-solving techniques when confronted with daily dilemmas. Program facilitators help children to identify their own temperament and then use strategies for dealing with behavioral challenges based on their temperament and consistent reaction style.

A recently published study identified positive intent-to-treat effects of INSIGHTS on low-income urban kindergarten and first graders' math and reading achievement, sustained attention, and disruptive behaviors (O'Connor et al., 2014). However, we have yet to determine whether program impacts varied by levels of parent participation in the program. Participation in the child and teacher components of the program was uniformly high across schools assigned to INSIGHTS. However, there was significant heterogeneity in program take-up among study participants' parents. Given these factors, this study aims to answer three research questions:

1. Did treatment group parents who had high levels of participation in INSIGHTS differ from treatment group parents who had low participation?
2. Did treatment children whose parents had high levels of parent participation in INSIGHTS experience greater gains in sustained attention and lower disruptive behaviors, compared to treatment children whose parents participated in INSIGHTS at lower levels?;
3. Did treatment children whose parents had high levels of parent participation in INSIGHTS experience bigger treatment impacts on math and reading achievement, relative to treatment children whose parents participated in INSIGHTS at lower levels?

Analyses will aim to maintain the integrity of the original randomized study design in order to infer causality from the dosage findings, requiring key assumptions. Results will help inform future SEL program development, refinement, and scale-up.

Method

Data come from a school-randomized trial of INSIGHTS in kindergarten and first-grade classrooms. The duration of the study was four years (2008–2012; see O'Connor et al., 2014). Three cohorts of urban elementary schools entered the study over three consecutive years. Each cohort participated for two years, when students were in kindergarten and first grade. All parents, teachers, and children assigned to receive the INSIGHTS program were offered 10 total INSIGHTS sessions, held during the school year. Intervention was delivered separately to the child, teacher, and parent groups.

Sample

Participants included 435 children and parents as well as 120 kindergarten and first-grade teachers from 22 schools. Specific procedures for enrolling participants in the study are outlined below. Children ranged from four to seven years of age at baseline ($M = 5.38$ $SD = 0.61$). Half (52%) of the children were male. Eighty-seven percent of children qualified for free or reduced-price lunch. Seventy-five percent of children were Black, non-Hispanic, 16% were Hispanic, non-Black, and the remaining children were biracial. A majority of parents were the children's biological mothers (84%); others included fathers (8%), kinship guardians (7%), and a category designated "other" (1%). Approximately 28% of adult respondents had education levels less than a high school degree; 26% had at least a high school degree or GED diploma; 24% had at least some college experience; and the remaining 22% had graduated from a two- or four-year college. Sixty kindergarten and 62 first-grade teachers participated (96% female). Teachers reported their race/ethnicity as African American/non-Hispanic (61%), Hispanic/non-Black (10%), White (23%), and Asian (6%). Ninety-six percent of teachers had a master's degree.

Children enrolled in the study were similar to the students at the schools who chose not to participate in the study. Participating schools had high percentages of students who were racial/ethnic minorities (Black, $M = .77$, $SD = .13$; Hispanic, $M = .40$, $SD = .27$) and eligible for free or reduced-price lunch ($M = .80$, $SD = .16$). Schools had an average attendance rate of 86.26% ($SD = .19$) and size of 465 total students ($SD = 158.46$).

Measures

In this study, confounding covariates were measured in the late fall or early winter in kindergarten (Time 1; T1). Data on parent program participation were collected throughout the duration of the 10-week INSIGHTS intervention in kindergarten or first grade. Parents were allowed to choose the year when they participated in the parent intervention. Student outcomes were assessed in the spring of kindergarten (Time 2; T2) and again in the fall of first grade (Time 3; T3). Another 10-week round of intervention took place in the fall/winter of first grade for all students and teachers and for parents who had not participated in kindergarten. Student outcome data were subsequently collected in the late winter (Time 4; T4) and spring (Time 5; T5) of first grade. In the following sections, we provide details about the outcome, treatment, and dosage variables, as well as confounding covariates measured at baseline (T1). Field researchers collected data from teachers, parents, and students at each time point through surveys and direct assessments.

Outcomes

Child sustained attention was measured with the Attention Sustained subtest from the *Leiter International Performance Scale–R* (Roid & Miller, 1997). Children were shown a page with pictures of a variety of objects scattered throughout and a target object at the top. They were asked to cross out as many of the objects matching the target as possible without accidentally crossing out any other objects. Children were given a limited amount of time to perform four trials (30 seconds for the first three trials and 60 seconds for the fourth) but were not scored on speed. Performance across trials was averaged to yield two attention scores. A total score was calculated by subtracting incorrect cross-outs from the total number of correct cross-outs. This task has demonstrated reliability and validity (Roid & Miller, 1997). In this study, scores at T2–T5 will be used as outcomes, and T1 levels will be used as confounding covariates in analyses.

Child disruptive behaviors were measured with the 36-item Sutter–Eyberg Student Behavior Inventory (Eyberg & Pincus, 1999). On a frequency scale ranging from 1–7 (1 = *never* to 7 = *always*), teachers reported on the frequency with which each consented child engaged in a range of problematic behaviors. A mean score was calculated from the survey items. Across study time points, the average Cronbach's alpha was .97. In addition, Querido and Eyberg (2003) have shown evidence of validity for the scale. Disruptive behavior scores at T2–T5 will be used as outcomes, and T1 levels will be used as confounding covariates in analyses.

Reading and math achievement were assessed using raw scores from the Letter–Word Identification and Applied Problems subtests of the *Woodcock–Johnson III Tests of Achievement, Form B* (*WJ–III*; Woodcock, McGrew, & Mather, 2001). The *Letter–Word ID* subtest assesses letter naming and word decoding skills by asking children to identify a series of letters and words presented in isolation. The *Applied Problems* subtest assesses children's counting skills and the ability to analyze and solve mathematical word problems presented orally. Possible scores range from 0 to 76 on the *Letter–Word ID* test and from 0 to 64 on the *Applied Problems* test. The *WJ–III* is a nationally normed and widely used achievement test with demonstrated internal consistencies. Achievement scores at T2–T5 will be used as outcomes, and T1 levels will be used as confounding covariates in analyses.

Treatment Variable

Treatment is operationalized with a dummy variable representing assignment to the treatment or comparison condition (INSIGHTS = 1; comparison = 0).

INSIGHTS Parent Program Participation. *Dosage* was assessed using facilitators' reports of number of parent sessions attended. All teachers, students, and parents assigned to a treatment school were invited to attend 10 INSIGHTS sessions. There was no variation in the amount of intervention offered to INSIGHTS participants. In addition, dosage for teachers and children was high across schools. Out of 10 INSIGHTS sessions, teachers attended 8.99 ($SD = 1.28$) sessions on average. The average number of classroom sessions attended by participating children was 8.30 ($SD = 2.25$). However, parental attendance was lower. Although both program facilitators and program staff (recruiters and field managers) made all possible efforts to engage parents in INSIGHTS sessions, the average number of sessions parents attended was 5.93 ($SD = 4.15$). In cases where parents began participating in sessions and later stopped coming, program staff made all possible efforts (phone calls, text, e-mails) to reengage them. Twenty-five percent of the parents were present for all sessions and 38% were present for eight sessions or more.

We defined high parent program participation as attending eight or more INSIGHTS parent sessions (1 = eight or more sessions; 0 = less than eight sessions). This decision was made for three reasons: (a) The distribution in parent participation was bimodal with about half of parents participating at high rates and half participating in few if any sessions; (b) Eight or more sessions is considered adequate for experiencing the full program dose; and (c) 80% of program participation is considered high across multiple types of social interventions (Gaubert et al., 2010; Hsueh et al., 2012). There were 84 students assigned to INSIGHTS whose parents participated at high levels. The remaining 141 students in the INSIGHTS group participated in less than eight sessions. Notably, 83 of the low-dosage parents did not participate in services at all. These students are compared to 210 participants assigned to the comparison condition.

Parents could only participate in the INSIGHTS intervention for one ten-week session—during either kindergarten or first grade. Sixty-eight percent of parents who did participate at high levels did so when their children were in kindergarten. In addition, there are certainly parents in the study who provided data and enrolled in the study, but never intended to participate in the parent program. However, all parents in schools assigned to INSIGHTS were given the offer of participating in the parent program. Because of the randomized study design we can assume that there are similar numbers of parents who never intended to participate in the program in the schools assigned to INSIGHTS and the schools in the comparison condition.

Confounding Covariates

Confounding covariates represent the variables that predict parent participation and any of the student outcomes.

Demographic Characteristics. Parents reported on demographic characteristics. Child-level covariates included child ethnicity (Hispanic or Black; White is the referent group), child's gender (male = 1, female = 0), child age (days from birth to Time 1 assessment), and child free lunch eligibility (eligible = 1, not eligible = 0). Parent-level confounding covariates

included parent age (years), parent ethnicity (Hispanic or Black; White is the referent group), parent education (years), whether the parent is single or married/cohabitating with a partner (single = 1, not single = 0), and parental work status (1 = works full time; 0 = works part time or not at all).

Teacher–Child Relationship Quality. The 15-item teacher-reported Student–Teacher Relationship Scale (STRS; Pianta, 2001) was used to assess teacher perceptions of the quality of the teacher–child relationship at T1. Using a 5-point Likert scale that ranged from 1 (definitely does not apply) to 5 (definitely applies), teachers rated how applicable statements were to their current relationship with a particular child. This scale contains two subdimensions: closeness and conflict. The closeness subscale consists of eight items and measures the amount of warmth and communication present in the relationship. The conflict subscale consists of seven items and measures the extent to which the relationship is marked by disharmonious interactions. The mean of each scale was taken to calculate a dimension score. Cronbach's αs in this study were $\alpha = .92$ for closeness at T1, $\alpha = .88$ for conflict at T1.

Teacher Perceptions of Academic Competence. Two subscales of the Academic Competency Evaluation Scale (ACES; DiPerna & Elliott, 2000) measured teacher perceptions of children's achievement-related behaviors in reading and mathematics. Teachers rated students' academic skills in comparison with the grade-level expectations at their school (1 = far below; 3 = grade level; 5 = far above). Consisting of 11 items, the Reading/Writing subscale includes items about the skills necessary for generating and understanding written language. Examples include "reading comprehension," "spelling," and "grammar." The eight-item Mathematics subscale reflects skills related to use and application of numbers, including computation, and problem solving. Examples include "pattern analysis," "mental math," and "problem solving." The mean of each scale was taken to calculate an average score. Internal consistencies were high in this study ($\alpha = .96$ – T1 reading; $\alpha = .97$ – T1 math).

Parent involvement in elementary school was assessed with the parent-reported Family Involvement Questionnaire for Elementary School (FIQ-E). Consisting of 44 items, the FIQ-E was developed for lower-income urban families. The FIQ-E asks parents to report on the frequency with which they engage in behaviors related to their child's schooling on a scale from 1 (*never*) to 4 (*always*). Example items include "I talk with my child's teacher on the telephone," "I read with my child," and "I attend parent workshops or training offered by my child's school." A mean score was calculated from the scale items, and possible scores thus range from 1–4. The average alpha was $\alpha = .96$ at T1.

Child temperament was measured with the School-Aged Temperament Inventory (SATI; McClowry, 1995, 2002). The SATI is a 38-item 5-point Likert-type scale (ranging from 1 = *never* to 5 = *always*) that was standardized with a racially/ethnically and socioeconomically diverse sample of 883 parents reporting on their children. The instrument has four dimensions derived from principal factor analysis: negative reactivity (12 items; intensity/frequency of negative affect), task persistence (11 items; self-direction in fulfilling task responsibilities), withdrawal (9 items; child's initial response to new situations), and activity (6 items; large motor activity). For each subscale, the mean of the items was taken to calculate an average score. Cronbach's alphas for the SATI (completed at enrollment) were activity: $\alpha = 0.77$; withdrawal: $\alpha = .81$; task persistence: $\alpha = .85$; negative reactivity: $\alpha = .87$.

Procedure and Random Assignment of Schools

Principals at 23 elementary schools made a two-year commitment to participate in the study. Prior to randomization, however, one school withdrew during a principal transition. Each of the cohorts began with recruitment of the kindergarten teachers in September. First-grade teachers were recruited from the same schools. In all, 96% of the kindergarten and first-grade teachers consented to participate; there was no teacher attrition. Teachers reported on student behaviors, academic competencies, and relationships for each participating student and received $50 in gift cards for classroom supplies to thank them for their time.

Parents from the participating kindergarten teachers' classrooms were recruited at school in September and October. Recruiters first made contact with parents in person at participating schools. Then, they followed up with the parents over the phone to schedule an initial meeting to sign a consent form and complete baseline data collection. Recruiters made all possible efforts to enroll parents during both the initial school meetings and follow-up telephone calls. In addition, all parents were told they would be invited to participate in either a 10-week intervention focused on behavior (if assigned to INSIGHTS) or a two-week program focused on students' reading skills (if assigned to the comparison group, discussed in more detail later). Parents were explicitly told they could participate in the study but choose not to attend parent sessions. Parents reported on demographic characteristics, child temperament, and family involvement. Each time that parents provided information on themselves and their child, they received a $20 gift card to thank them for their time in completing data collection activities. Child assent was acquired after the informed consent procedure was completed. Trained data collectors, blind to study condition and procedures, conducted child assessments at each of the five data time points. Data collectors were trained by an outside consultant on the *Woodcock–Johnson* (Woodcock et al., 2001) and the *Leiter–R* (Roid & Miller, 1997) during a one-day training session in the fall of each study year. A graduate assistant conducted a mock assessment in the lab and observed all data collectors conduct multiple assessments in the field before impact data were collected. Weekly check-in meetings were subsequently conducted.

Significant efforts were made to recruit a representative group of students for the study within each participating classroom. Recruitment efforts took place over six to eight weeks in each year of the study. The number of students in each class who enrolled ranged from four to ten. Although some parents did consent to participate early in the recruitment period, all possible efforts were made to recruit additional parents until data collection was scheduled to begin. As noted above, children enrolled in the study were similar to the students at the schools who chose not to participate in the study. Most children (79%) were enrolled in the study at the beginning of kindergarten. The remaining 21% enrolled in first grade.

Random Assignment

Schools were the unit of random assignment (Shadish, Cook, & Campbell, 2002). After baseline data were collected in kindergarten, a random numbers table was used to randomly assign schools to INSIGHTS or a supplemental reading program, referred to throughout this study as the comparison condition. Eleven schools were randomized to INSIGHTS (N = 225 students; N = 57 teachers); the remaining eleven schools hosted the comparison condition (N = 210 student participants; N = 65 teachers). Independent samples t tests showed a

statistically significant difference in baseline reading achievement between children participating in INSIGHTS and those in the comparison group ($t(433) = 3.12, p < .01$). Chi-square analyses also revealed that there were more Hispanic children enrolled in INSIGHTS compared to the comparison condition. Statistical modeling will be used to address these pretreatment differences.

INSIGHTS Intervention Procedures

As described earlier, INSIGHTS included: (a) teacher sessions, (b) parent sessions, and (c) universal classroom sessions (see O'Connor et al., 2014). Teacher and classroom sessions were implemented during the regular school day in teachers' and students' classrooms. In contrast, parent sessions were held after the end of the normal school day at the school. All teachers and parents were invited to attend 10 weekly two-hour facilitated sessions based on a structured curriculum that included didactic content and professionally produced vignettes as well as handouts and group activities. Parents in INSIGHTS schools received $20 for each session they attended (exclusive of financial incentives for data collection). Teachers received professional development credit and $40 gift cards for each session attended. During the same 10 weeks, the classroom program was delivered in 45-minute lessons to all students in participating classrooms. The curriculum materials included puppets, workbooks, flash cards, and videotaped vignettes, and aimed to help students resolve challenging dilemmas at home and school.

More specifically, the curriculum for the parent and teacher programs has three parts. In Part I, "The *3 Rs* of Child Management: Recognize, Reframe, and Respond," participants are taught to *recognize* the unique qualities that children exhibit as an expression of their temperament. Intentionality, the belief that a child consciously misbehaves, is reduced when participants recognize that many reactions to specific situations are related to one's temperament. Participants are encouraged to *reframe* their perceptions with the understanding that every temperament has strengths and challenges. They also learn that although temperament is not amenable to change, parent and teacher *responses* are and can, in turn, influence the behavior of children. Recognition and acceptance of a child's temperament, however, does not imply permissiveness. In Part II, "Gaining Compliance," temperament-based management strategies are implemented to improve children's behavior. Parents and teachers are assisted in replacing negative patterns of interaction with child management strategies that are matched to specific types of temperaments. Finally, Part III focuses on strategies that support children in becoming more socially competent, particularly when encountering situations that are temperamentally challenging. Pedagogical techniques involved primarily group-based and didactic activities, and included direct instruction and individual assignments.

Facilitator Training

Facilitators were selected based on their relevant skills prior to training. The eight facilitators were graduate students in psychology, education, and educational theater from varied racial/ethnic backgrounds. Intervention facilitators attended a graduate-level course to learn the theory and research underlying the intervention. New facilitators attended a training where they learned to use the intervention materials. Each facilitator conducted the full intervention (teacher, parent, and child) in the schools to which s/he was assigned.

Intervention Fidelity

Facilitators followed scripts, used material checklists, documented sessions, and received ongoing training and supervision. Deviations were discussed weekly in meetings with the program developer. Supervision focused on challenges related to conducting sessions, implementation logistics, and participant concerns. Parent and teacher sessions were videotaped and reviewed for coverage of content and effective facilitation. Fidelity coding, conducted by an experienced clinician, revealed that 94% of the curriculum was adequately covered in the teacher sessions; 92% was covered in parent sessions.

Comparison Condition

Schools not assigned to INSIGHTS participated in a supplemental reading program after school called Read Aloud for children whose parents consented. Children were offered 10 weeks of the reading program, while teachers and parents were offered two 2-hour workshops in which reading coaches provided materials and presented strategies to enhance literacy. Parents received $20 per workshop, and teachers received professional development credit and $40 for resources for each workshop. Twenty-four percent of children enrolled participated in the full 10 sessions; an additional 19% took part in eight or nine sessions. Thirty percent of parents and 83% of teachers attended both sessions. Reading program facilitators had weekly meetings with the project director to ensure that all components were implemented. Curriculum fidelity was high; 95%–100% of topics were covered across the 10-week program.

The comparison condition was implemented in this study so that any effects of INSIGHTS could be attributed to intervention on social-emotional and behavioral skills, and not simply any attention or intervention, regardless of content. Comparing INSIGHTS schools to comparison schools in this way provides a more conservative test of the effect of the treatment. We argue that we can be more confident in any observed effects of INSIGHTS because the comparison intervention does not attempt to change the pattern of norms and social interactions within the classroom context and also takes place after school. Moreover, attendance at Read Aloud sessions was fairly low for comparison group parents (30% attending two sessions). Although there is a valid concern about comparing INSIGHTS parents with Read Aloud parents, the INSIGHTS intervention is theorized to be a more powerful lever for changing student outcomes.

Analytic Approach

Examination of Posttreatment Variables in the Context of Randomized Trials

This article aims to determine whether parent program dosage moderates the effects of INSIGHTS on children's achievement, behaviors, and sustained attention. This analysis is more challenging than a typical subgroup analysis, however, because program dosage was measured posttreatment. Moreover, dosage was not randomly assigned in this experiment. As such, it is quite possible, and expected, that certain types of study participants, perhaps those with higher levels of education and self-efficacy, were more likely to take up the treatment.

Given these issues posed by selection, examining dosage as a standard moderator is likely to bias subgroup impact estimates because a comparison is made between the subgroup of treatment participants who participated at high levels and the full control (or comparison)

group, who would have likely participated at varying levels (Zhai et al., 2010). To address this limitation, recent studies of education and social interventions have used variations of propensity score matching to calculate an unbiased estimate of the treatment on the treated effect (see Lara, Mizala, & Repetto, 2011; Zhai et al., 2010). Inverse probability of treatment weighting (IPTW) is one technique that addresses possible selection bias into being a high-dose participant (Austin, 2008; Imbens, 2004).

In IPTW analyses, propensity scores are used to re-weight the comparison group participants to mirror the treatment group participants (assigned to INSIGHTS), based on covariates, to calculate the effect of the treatment on the high-dose treated (as well as the effect of the treatment on the low-dose treated). Propensity score techniques have advantages over using OLS regression to control for confounds as they do not require as strict assumptions about the functional form of the relationship (Gelman & Hill, 2007) between the outcome and the confounding covariates. Although regression models can make predictions outside of the available data, IPTW approaches can better focus predictions to the area of the covariate space where there are extant data.

In this article we use IPTW to compare the outcomes for the students assigned to INSIGHTS whose parents participated at high levels to the outcomes for the students assigned to the comparison group whose parents are predicted to participate at high levels. Then, we conduct a similar analysis comparing the students assigned to INSIGHTS whose parents participated at low levels (or not at all) to the outcomes for the students assigned to the comparison group whose parents are predicted to participate at lower levels. These analyses, which identify a counterfactual group for each dosage level based on confounding covariates, will thus help address the issue posed by examining dosage effects in the context of a randomized trial. The extent to which the IPTW estimates should be considered causal relies on the plausibility of key assumptions, which will be discussed in more detail in the Results section (Gelman & Hill, 2007).

Missing Data Analyses

There were no missing dosage data in this study for the group assigned to INSIGHTS. Child-level demographic information collected at baseline was generally complete for all participants. However, given some attrition (7% of students by Time 5; $N = 31$) and student absences during data collection periods, coupled with parents' decision not to disclose information on income and education, child-level variables had missingness rates ranging from 0 to 20% across time.[1] We first compared students who were missing and not missing individual data points on a series of baseline characteristics, specifically, school, teacher, cohort, child ethnicity (e.g., Hispanic or Black), child's gender, child age, child free-lunch eligibility, behavior problems, sustained attention, math achievement, reading achievement, parent gender, parent age, parent ethnicity, parent education, parent marital status, and parent work status. Although we did not find substantial differences in rates of missingness between students by treatment status or student outcomes of interest, missingness patterns between baseline variables were not random. Students with lower levels of parental education, parents who were not married, and those with more behavior problems were most likely to be missing outcome data equally across the treatment and comparison groups. As such, the assumptions

[1] Missingness (meaning a child was missing at least one variable from the analysis) was 5% for T1, 7% for T2, 8% for T3, 14% for T4, and 20% for T5.

for complete case analysis were not met (Hill, Waldfogel, Brooks-Gunn, & Han, 2005; Little & Rubin, 2002).

A multiple imputation method (MI) was thus employed, and 20 separate data sets were imputed by chained equations, using STATA MICE in STATA version 12 (Enders, 2013; Schafer & Graham, 2002). Multiple imputation assumes that data are Missing at Random (MAR), meaning that the probability that a variable is missing for a unit is dependent only on available information. MI replaces missing values with predictions based on all other information observed in the study. Unlike single imputation methods, MI accounts for uncertainty about missing data by imputing several values for each missing value, generating multiple data sets. For this article, STATA ran each set of analyses 20 times and aggregated the findings across the data sets.

Research Question 1

Descriptive statistics on continuous study variables were compared across the high-dosage parent INSIGHTS group, the low parent dosage INSIGHTS group, and the comparison group. Independent samples *t* tests were used to test for significant group differences (between the high-dosage parent group and the low-dosage parent group) between study variables. The findings from this analysis will help answer research question 1 and provide insight into the factors that are associated with high parent program participation.

Research Questions 2 and 3

Throughout the current section and the results section, we refer to groups of interest as the "high participation group" and the "low participation group." We must conduct two separate sets of analyses to examine impacts for both the high and low participation groups. For the sake of parsimony we formally present the approach for estimating the impacts for the high participation group. However, following conclusion of those models, we did repeat this procedure to estimate impacts for the low participation group.

To begin the IPTW modeling, we used a logistic regression with school fixed effects (given variation in parent program take-up across schools) to estimate the likelihood of high parent program participation from a set of confounding covariates.[2] The Step 1 equation is:

$$\text{Logit } (Dij) \;=\; \beta_0 + \beta_1 C_{ij} + \alpha_j,$$

C_{ij} is a vector representing pretreatment characteristics for student i in school j that influence the child's likelihood of having a high participation parent. β_1 is a vector representing predicted probabilities for those confounding covariates. α_i represents school fixed effects that account for differences across schools in the likelihood of having a parent who participates at high levels.

We then used the coefficients for the INSIGHTS group and applied them to covariate data for the group of students originally randomly assigned to the comparison group. Next, we used IPTW to weight the comparison group so that it looked like the high participation group in terms of all confounding covariates. Assuming that all covariates were included, the weighted comparison group should provide an appropriate counterfactual group with distributions of the

[2] A simple examination of the decrease in the variance explained in parent participation after including the school fixed effect for the treatment schools revealed that school membership explained about 22% of variation in parent participation.

pretreatment variables similar to those of the treatment participants with high levels of parent dosage. Using the weighted comparison group, one can estimate the effect of high parent participation in INSIGHTS on posttreatment outcomes, relative to the outcomes for the students in the comparison group whose parents would have participated at high levels if given the opportunity. The *estimand* in this analysis is the effect of the treatment on those who participate at high levels. In contrast, when we examine these models for the low participation group, the *estimand* will be the effect of the treatment on those who participated at low levels. Importantly, at this point we also examined balance statistics (discussed in the assumptions section below) to determine how well the weighted high participation comparison group mirrored the high participation treatment group in terms of the means and standard deviations of these covariates. By establishing appropriate balance, the IPTW principal score (Hill et al., 2003) approach follows similar procedures as the more traditional propensity score matching approach. In determining which factors are most prudent and conservative for assessing balance, we build on work by Hill (2008) and Austin (2008), demonstrating that *t* statistics or other sample-size-dependent tests are not necessarily valid for determining whether the treatment group is statistically similar to the weighted comparison group. Rather than only considering mean differences, Hill (2008) argues that researchers should examine higher order sample statistics such as standardized differences and variances (or standard deviations), and consider differences in the magnitude of these measures across the treatment and weighted comparison groups.

Finally, using a sample composed of the high participation group and weighted comparison group, we ran a series of individual growth models similar to those examined in the previous intent-to-treat study (see O'Connor et al., 2014), but applying the appropriate weights. Data on repeated measures from T2 to T5 were utilized in order to generate sufficient power to detect dosage impacts. T1 measures were used as covariates in all models. Time was centered on the last time point so that the intercept would represent the treatment difference at T5. Because repeated measures were nested in students, a random effect was included at Level 2 in all models, allowing the intercept to vary at the student level. Based on comparisons of model iterations, we did allow slopes to vary randomly across students, and permitted the random intercept and random slope to covary, as displayed below:

$$\text{Outcome}_{ti} = \gamma_{00} + \gamma_{10}(\textit{Assessmentpoint} - 4)_{ti} + u_{0i} + u_{ti} + \alpha_{0i} + \varepsilon_{ti}$$

As shown in this equation, each student's outcome score at the intercept is modeled as a grand mean outcome score (γ_{00}), as well as a residual term that demonstrates deviations in outcome scores about the grand mean (u_{0i}). Each student's rate of change across time is modeled as a grand mean rate of change in the outcome of interest (γ_{10}) centered at the final data collection time point. In addition, there is a random slope for students' outcome scores (u_{ti}), and the provision that the student level random intercept (u_{0i}) and slope (u_{ti}) are permitted to covary ($\text{Corr}(u_{0ij}, u_{ti}) = \rho_{u_0 u_t}$).

In the next iteration of the model, we included a coefficient for Treatment (γ_{01}), an interaction for Time × Treatment (γ_{11}), and a series of student-level covariates (γ_{02}).

$$\text{Outcome}_{ti} = \gamma_{00} + \gamma_{10}(\text{Assessmentpoint} - 4)_{ti} + \gamma_{01}(\text{Treatment})_i + \gamma_{11}(\text{Assessmentpoint} - 4)_{ti} * (\text{Treatment})_i + \gamma_{02}(\text{StudentCovars})_i + u_{0i} + u_{ti} + \alpha_{0i} + \varepsilon_{ti}$$

After applying the weights from the IPTW procedure, the coefficient for Treatment represents the impact of high participation on the outcome relative to what would have happened in the absence of INSIGHTS.[3] The coefficient for Treatment × Time represents the impact of high participation on growth in the outcomes, relative to what would have happened in the absence of INSIGHTS. Causal interpretations are correct if the appropriate assumptions for IPTW are met.

As we discussed at the beginning of this section, we then repeated this entire procedure, beginning with the Step 1 equation, to estimate low parent participation treatment impacts relative to the comparison group members whose parents would have participated at low levels if given the opportunity. Analyses examining research question 2 (outcomes: sustained and disruptive behaviors) were examined first, following by models examining research question 3 (outcomes: reading and math achievement). Findings are reported for the impacts within the high-dosage group first, followed by the impacts for the low-dosage group.

Results

Research Question 1: Factors Related to Parent Program Participation

Descriptive statistics for all pretreatment study variables are presented in Table 1. There were differences between the high- and low-dosage parent participation groups at baseline. Specifically, high-dosage parents were older, were more likely to be married, and were more likely to be working full time. Importantly, high-dosage parents had about a year more education than low-dosage parents. High-dosage parents had children with higher levels of math and reading achievement than children of low-dosage parents. Children of high-dosage parents were also less likely to be withdrawn. Results from a series of independent samples *t* tests demonstrated that all these descriptive differences were statistically significant.

Descriptive statistics across the five study time points are presented in Table 2. Across all groups, sustained attention, reading achievement, and math achievement improved across time. Disruptive behaviors appear to be relatively stable but did increase a bit. Notably, however, the low-dosage group appears to have made greater gains in sustained attention, math, and reading achievement, as well as greater decreases in disruptive behaviors than the high-dosage group. Still, both groups appear to have outpaced the comparison group in terms of these outcomes.

Research Question 2: Dosage Effects on Disruptive Behavior and Sustained Attention

All results for research question 2 are displayed in the top section of Table 3. As illustrated, there were no average treatment impacts for high-dosage participants in INSIGHTS, relative to their counterfactual condition of no treatment receipt. Neither were there main impacts on sustained attention and disruptive behaviors for treatment children whose parents participated at low levels, relative to what would have happened in the absence of assignment to INSIGHTS. These findings were expected, given that there were no main effects of

[3] We conducted a sensitivity check by also included a covariate to measure whether parents participated during children's kindergarten or first-grade year. Results were nearly identical after including this covariate. This finding was expected given that student outcomes did not differ for children whose parents participated in kindergarten and first grade.

Table 1. Descriptives for all study variables at baseline by treatment dosage and condition.

	Tx high dosage $N = 84$		Tx low dosage $N = 131$		Control $N = 220$	
Variable	Mean	*SD*	Mean	*SD*	Mean	*SD*
Demographic characteristics						
Child age	5.57	0.72	5.56	0.69	5.52	0.59
Child Black	0.75	0.44	0.77	0.42	0.73	0.44
Child Hispanic	0.21	0.41	0.20	0.40	0.16	0.37
Child male	0.54	0.50	0.52	0.50	0.52	0.50
Parent age	36.35	9.41	33.83	7.26	35.95	8.76
Parent years education	12.73	2.18	12.32	2.45	13.37	2.97
Parent unmarried	0.62	0.49	0.61	0.49	0.50	0.50
Parent Black	0.75	0.44	0.81	0.40	0.76	0.44
Parent works full time	0.14	0.35	0.12	0.33	0.16	0.37
Child characteristics at baseline						
Sustained attention	47.49	12.25	45.62	12.84	45.47	12.79
Motor activity (1–5)	2.91	0.91	2.87	0.95	2.79	1.02
Negative reactivity (1–5)	2.87	0.84	2.86	0.86	2.93	0.90
Task persistence (1–5)	3.80	0.78	3.81	0.75	3.78	0.82
Withdrawal (1–5)	2.34	0.85	2.47	0.86	2.38	0.97
Reading achievement (0–72)	17.11	8.29	16.00	7.45	18.49	7.58
Math achievement (0–67)	15.21	4.61	14.31	4.87	14.47	5.15
Behavior problems (1–7)	2.37	1.23	2.27	1.18	2.20	1.09
Reading skills (1–5)	2.62	0.83	2.54	0.86	2.75	0.80
Math skills (1–5)	2.68	0.71	2.62	0.68	2.76	0.67
Critical thinking skills (1–5)	2.63	0.74	2.61	0.69	2.68	0.64
Student–teacher conflict (1–5)	1.92	0.94	1.89	1.06	1.83	0.96
Student–teacher closeness (1–5)	4.13	0.69	4.18	0.74	4.11	0.80
Parent involvement (1–4)	2.69	0.52	2.72	0.53	2.74	0.55
Parent–child conflict (1–5)	2.12	0.72	2.05	0.73	2.09	0.83
Parent–child closeness 1–5)	4.57	0.31	4.53	0.42	4.57	0.36
Behavioral engagement (%)	0.65	0.20	0.69	0.19	0.70	0.20
Off-task behaviors (%)	0.16	0.09	0.15	0.09	0.14	0.09

$N = 435$ children; $N = 22$ schools

Note. One way ANOVA tests revealed statistically significant group differences for reading achievement ($F(2, 435) = 4.96$, $p < .01$). Treatment/control differences for baseline reading achievement have been previously documented.

INSIGHTS on sustained attention and disruptive behaviors in the intent-to-treat analysis (O'Connor et al., 2014).

There were, however, significant dosage effects on the growth in outcomes across time. The coefficient for Treatment × Time on sustained attention was statistically significant for the high-dosage parent group ($B = 2.80$, $SE = 1.03$, $p = .01$; see Table 3). Thus, growth in sustained attention was faster for INSIGHTS participants with high parent participation, relative to comparison participants with predicted high levels of parent participation.

In the low parent participation group there were also significant effects on growth. Growth in sustained attention was faster for INSIGHTS participants with low parent participation, relative to the comparison group ($B = 4.03$, $SE = .69$, $p < .01$; see Table 3). Reductions in disruptive behaviors were faster for INSIGHTS participants with low parent participation, relation to the comparison group ($B = -.16$, $SE = .07$, $p = .03$; see Table 3). As illustrated in the last column of Table 3, a *Wald* test suggests that the impacts for the low-dosage group are bigger for sustained attention ($\chi^2 = 7.53$, $p < .01$). The treatment impact on disruptive behaviors is also larger for the group with low-parent dosage ($\chi^2 = 5.16$, $p < .01$).

Table 2. Descriptives for all study outcomes at follow-up points by treatment dosage and condition.

	Time 2						Time 3						Time 4						Time 5					
	Tx high dosage $N = 84$		Tx low dosage $N = 131$		Control $N = 220$		Tx high dosage $N = 84$		Tx low dosage $N = 131$		Control $N = 220$		Tx high dosage $N = 84$		Tx low dosage $N = 131$		Control $N = 220$		Tx high dosage $N = 84$		Tx low dosage $N = 131$		Control $N = 220$	
Variable	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*
Sustained attention	52.24	12.27	50.25	12.92	53.67	10.76	59.49	8.57	56.67	10.06	56.40	9.26	59.58	9.07	59.69	8.37	59.14	9.07	61.60	9.51	61.36	7.79	60.54	9.45
Reading achievement (0–72)	20.06	8.14	19.92	7.73	23.75	7.97	22.86	11.34	23.10	10.38	26.37	10.21	30.95	9.89	30.91	8.72	32.49	8.54	31.29	9.82	32.50	9.13	30.57	11.41
Math achievement (0–67)	17.05	4.87	17.08	4.91	17.71	5.04	18.60	5.63	17.91	5.03	18.69	4.25	22.45	4.39	22.42	4.00	22.60	3.84	23.24	4.85	23.44	4.98	23.17	4.19
Behavior problems (1–7)	2.60	1.62	2.47	1.40	2.22	1.12	2.31	1.30	2.13	1.15	2.32	1.15	2.53	1.60	2.19	1.22	2.43	1.38	2.49	1.53	2.17	1.26	2.83	1.42
Reading skills (1–5)	2.82	0.78	2.68	0.80	2.86	0.79	2.42	0.87	2.43	0.86	2.51	0.91	2.65	0.93	2.76	0.92	2.87	0.99	2.75	1.10	2.89	1.04	2.94	0.97
Math skills (1–5)	2.81	0.74	2.71	0.69	2.87	0.66	2.47	0.77	2.50	0.72	2.62	0.73	2.69	0.85	2.69	0.84	2.83	0.87	2.83	0.76	2.84	0.82	2.88	0.83
Critical thinking skills (1–5)	2.76	0.68	2.85	0.71	2.99	0.62	2.67	0.64	2.61	0.63	2.71	0.74	2.71	0.90	2.76	0.85	2.96	0.91	3.05	0.81	2.95	0.76	3.03	0.86
Student-teacher conflict (1–5)	1.93	1.10	1.90	1.03	1.81	0.97	1.78	1.10	1.65	0.82	0.71	0.86	1.96	1.08	1.75	0.91	1.83	1.04	1.98	1.21	1.92	1.09	1.90	1.09
Student-teacher closeness (1–5)	4.27	0.73	4.24	0.83	4.10	0.77	3.79	0.81	3.79	0.81	4.03	0.74	3.91	0.75	4.07	0.73	4.11	0.68	4.02	0.84	4.02	0.84	4.15	0.75
Parent involvement (1–4)	2.74	0.51	2.66	0.55	2.70	0.55	2.97	0.57	2.79	0.47	2.73	0.41	2.84	0.41	2.72	0.46	2.64	0.50	2.78	0.42	2.71	0.44	2.59	0.49
Parent-child closeness (1–5)	4.59	0.43	4.50	0.45	4.58	0.40	4.57	0.37	4.62	0.33	4.59	0.38	4.57	0.37	4.59	0.37	4.62	0.35	4.54	0.39	4.58	0.42	4.57	0.42
Parent-child conflict (1–5)	2.17	0.80	2.04	0.98	2.03	0.84	1.99	0.84	2.00	0.84	2.02	0.82	1.96	0.78	1.97	0.86	1.95	0.82	1.98	0.76	1.88	0.75	1.96	0.90
Behavioral engagement (%)	0.64	0.27	0.69	0.25	0.66	0.24	0.70	0.15	0.69	0.16	0.71	0.16	0.69	0.16	0.72	0.18	0.70	0.15	0.71	0.19	0.74	0.20	0.75	0.24
Off-task behaviors (%)	0.16	0.12	0.16	0.12	0.17	0.12	0.19	0.09	0.18	0.08	0.17	0.08	0.13	0.08	0.13	0.07	0.14	0.07	0.14	0.08	0.13	0.09	0.14	0.09

Table 3. Estimates from inverse probability of treatment weighting procedure.

Outcomes	High parent dosage $N = 84$		Low parent dosage $N = 131$		Sig. difference, high and low dosage
	B	*SE*	*B*	*SE*	
Average impact					
Math achievement	0.56	0.42	1.17**	0.37**	**
Reading achievement	1.59	1.27	3.26**	1.24**	**
Sustained attention	1.80	1.35	0.96	1.45	
Behavior problems	−0.12	0.17	−0.25	0.17	
Treatment × time impact					
Math achievement	2.23**	0.18	2.34**	0.18	
Reading achievement	4.14**	0.30	4.56**	0.34*	*
Sustained attention	2.80**	1.03	4.03**	0.69**	**
Behavior problems	−0.06	0.09	−0.16*	0.07**	**

**$p < .01$ *$p < .05$

Note. The final column ("sig. difference, high and low dosage") describes whether the magnitude of the coefficient for the high parent dosage group is statistically significantly different from the magnitude of the coefficient for the low parent dosage group.

Research Question 3: Dosage Effects on Math and Reading Achievement

All results for research question 3 are displayed in the bottom section of Table 3. Findings from the dosage analyses predicting math and reading achievement revealed no average treatment impacts for high-dosage participants in INSIGHTS, relative to their counterfactual condition of no treatment receipt. However, there were main treatment effects of INSIGHTS for low-dosage participants for both math ($B = 1.17$, $SE = .37$, $p < .01$; see Table 3) and reading achievement ($B = 3.26$, $SE = 1.24$, $p < .01$; see Table 3), relative to the comparison group. At the final time point, low-dosage participants in INSIGHTS had higher math and reading achievement relative to their comparison condition. See Figures 1 and 2 for illustrations of these effects.

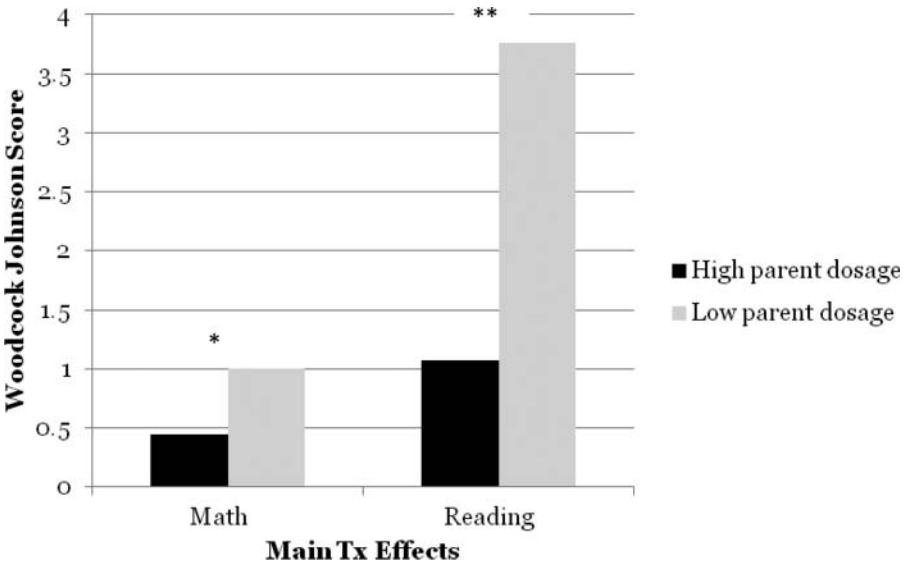

Figure 1. Differential impact by dosage on math and reading achievement. The columns in the table represent change scores in the WJ scores for math and reading.

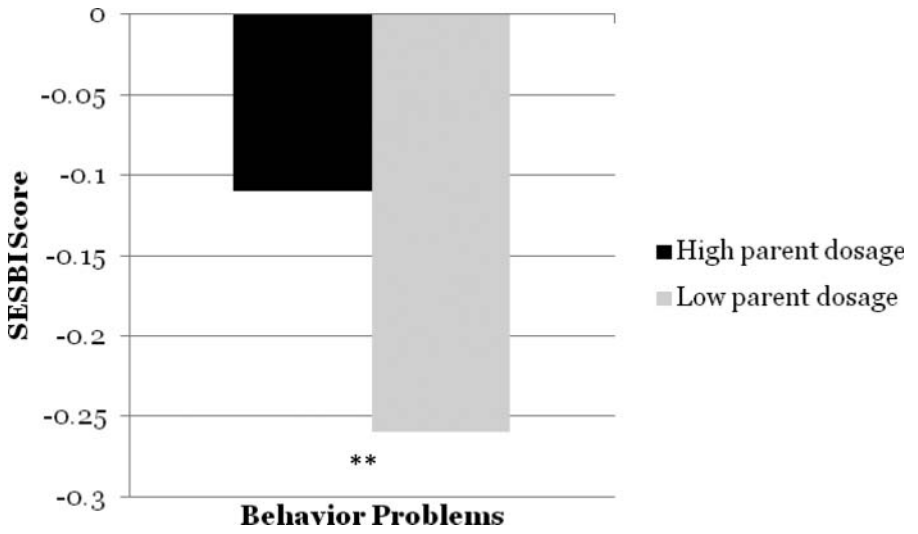

Figure 2. Differential impact by dosage on disruptive behaviors. The columns in the table represent change scores in SESBI behavior problems score.

There were also differences in growth in the outcomes over time. The coefficients for Treatment × Time were statistically significant in the models for high parent participation predicting math ($B = 2.23$, SE $= .18$, $p < .01$; see Table 3) and reading achievement ($B = 4.14$, SE $= .30$, $p = .01$; see Table 3). Growth in math and reading achievement was faster for students in INSIGHTS with high levels of parent participation, relative to their counterfactual condition. There were similar findings, however, for the low participation group. Growth in math ($B = 2.34$, SE $= .18$, $p < .01$; see Table 3) and reading achievement ($B = 4.56$, SE $= .34$, $p < .01$; see Table 3) was faster for students in INSIGHTS with lower levels of parent participation, relative to the comparison condition. All illustrated in the final column of Table 3, *Wald* tests suggests that these differences for the low-dosage group were larger for both math ($\chi^2 = 7.06$, $p < .01$) and reading ($\chi^2 = 9.18$, $p < .01$) achievement.

Tenability of Assumptions

In order to interpret findings as causal impacts, one must make a number of strong assumptions. First, we must assume ignorability. In the context of this study, ignorability means that all factors differentiating the high-dosage and low-dosage parent participation groups, which then predict subsequent outcomes of interest, have been accounted for in the analysis. We drew on a wealth of multimethod data sources for covariates. In addition, the sample is largely homogenous in terms of general demographic characteristics and schools were specifically recruited to be similar to one another. However, ignorability is still a strong assumption. For instance, we cannot control for parental motivation to participate in school-based programs. It is possible that there are selection characteristics that have not been accounted for in this analysis. The second assumption is that there is substantial overlap between the high-dosage treatment group and the predicted high-dosage comparison group (as well as the low-dosage treatment and low-dosage predicted comparison group). Because of the randomized study design we assume similar numbers of parents participating at high and low

levels in the schools assigned to INSIGHTS than there would be in schools assigned to the comparison condition, had they had the opportunity to participate in INSIGHTS. Thus, overlap is tenable.

Third, one must assume the stable unit treatment value assumption, or that the treatment assignment for one person does not affect the outcomes for another person. Given that the data were drawn from schools, there is a possibility of a clustering effect. However, as discussed earlier, there was little between-school nesting of the outcomes. Moreover, we accounted for school fixed effects in the prediction models to adjust for school-level differences in average levels of participation. Finally, one must assume appropriate balance between the treatment group and reweighted comparison group. As alluded to earlier, we built on work by Hill (Hill et al., 2003, Hill et al., 2008) to determine acceptable balance statistics. A conservative rule of thumb is for the standardized mean difference to be no greater than 0.1 and the ratio of standard deviations to fall between 0.9 and 1.1 (see Hill et al., 2003; McCormick, O'Connor, Cappella, & McClowry, 2013). Although balance was not perfect, the balance statistics calculated for this analysis (see Tables 4 and 5) suggest that the average

Table 4. Balance statistics for inverse probability of treatment weighting predicting high parent participation in INSIGHTS program.

	Treatment		Unweighted comparison		Weighted comparison			
Variable	Mean	*SD*	Mean	*SD*	Mean	*SD*	STD difference	Ratio of *SD*s
Demographic characteristics								
Child age	5.54	0.72	5.36	0.68	5.47	0.70	0.10	1.03
Child Black	0.75	0.44	0.81	0.39	0.76	0.43	−0.02	1.02
Child Hispanic	0.22	0.41	0.18	0.39	0.20	0.42	0.05	0.98
Child female	0.46	0.50	0.49	0.50	0.49	0.50	−0.06	1.00
Parent age	36.76	9.42	33.41	7.35	36.12	8.73	0.07	1.08
Parent years education	12.74	2.18	12.52	2.25	12.61	2.22	0.06	0.98
Parent unmarried	0.63	0.48	0.62	0.49	0.62	0.48	0.02	1.00
Parent Black	0.75	0.44	0.81	0.39	0.76	0.42	−0.02	1.05
Parent works full time	0.15	0.35	0.13	0.33	0.14	0.35	0.03	1.00
Child characteristics at baseline								
Sustained attention	47.23	12.04	45.56	13.01	46.11	12.37	0.09	0.97
Motor activity (1–5)	3.01	0.93	2.81	0.86	2.95	0.89	0.06	1.04
Negative reactivity (1–5)	2.92	0.79	2.77	0.72	2.85	0.77	0.09	1.03
Task persistence (1–5)	3.76	0.81	3.81	0.65	3.72	0.73	0.05	1.11
Withdrawal (1–5)	2.39	0.83	2.48	0.71	2.47	0.74	−0.10	1.12
Reading achievement (0–72)	16.98	8.55	16.05	7.47	16.39	7.91	0.07	1.08
Math achievement (0–67)	15.22	4.68	14.72	4.72	14.74	4.77	0.10	0.98
Behavior problems (1–7)	2.36	1.26	2.20	1.17	2.25	1.19	0.09	1.06
Reading skills (1–5)	2.65	0.79	2.53	0.84	2.61	0.81	0.05	0.98
Math skills (1–5)	2.70	0.70	2.63	0.71	2.64	0.70	0.09	1.00
Critical thinking skills (1–5)	2.64	0.73	2.62	0.69	2.66	0.77	−0.03	0.95
Student–teacher conflict (1–5)	1.91	0.94	1.89	1.03	1.87	1.01	0.04	0.93
Student–teacher closeness (1–5)	4.14	0.69	4.19	0.72	4.11	0.73	0.04	0.95
Parent involvement (1–4)	2.68	0.50	2.69	0.47	2.64	0.47	0.08	1.06
Parent–child conflict (1–5)	2.12	0.72	2.07	0.72	2.10	0.70	0.03	1.03
Parent–child closeness 1–5)	4.58	0.28	4.54	0.42	4.60	0.32	−0.07	0.88
Behavioral engagement (%)	0.65	0.20	0.68	0.20	0.67	0.20	−0.10	1.00
Off-task behaviors (%)	0.16	0.09	0.14	0.08	0.15	0.08	0.08	1.13

Note. "Good balance" is represented by a situation where the absolute value of the standardized difference is no greater than .1 and the ratio of standard deviations is between .9 and 1.1.

Table 5. Balance statistics for inverse probability of treatment weighting predicting low parent participation in INSIGHTS program.

	INSIGHTS low dosage		Unweighted comparison		Weighted comparison			
Variable	Mean	*SD*	Mean	*SD*	Mean	*SD*	STD difference	Ratio of *SD*s
Demographic characteristics								
Child age	5.56	0.69	5.36	0.68	5.50	0.68	0.09	1.01
Child Black	0.77	0.42	0.81	0.39	0.77	0.44	0.00	0.95
Child Hispanic	0.20	0.40	0.18	0.39	0.22	0.40	−0.05	1.00
Child female	0.52	0.50	0.49	0.50	0.52	0.50	0.00	1.00
Parent age	33.83	7.26	33.41	7.35	34.14	7.75	−0.04	0.94
Parent years education	12.32	2.45	12.52	2.25	12.34	2.46	−0.01	1.00
Parent unmarried	0.61	0.49	0.62	0.49	0.63	0.49	−0.04	1.00
Parent Black	0.81	0.40	0.81	0.39	0.79	0.42	0.05	0.95
Parent works full time	0.12	0.33	0.13	0.33	0.13	0.30	−0.03	1.10
Child characteristics at baseline								
Sustained attention	50.25	12.92	45.56	13.01	49.14	13.10	0.09	0.99
Motor activity (1–5)	2.87	0.95	2.81	0.86	2.95	0.91	−0.08	1.04
Negative reactivity (1–5)	2.86	0.86	2.77	0.72	2.83	0.84	0.03	1.02
Task persistence (1–5)	3.81	0.75	3.81	0.65	3.77	0.76	0.05	0.99
Withdrawal (1–5)	2.47	0.86	2.48	0.71	2.49	0.84	−0.02	1.02
Reading achievement (0–72)	20.06	8.14	16.05	7.47	20.14	8.07	−0.01	1.01
Math achievement (0–67)	17.05	4.87	14.72	4.72	16.89	4.91	0.03	0.99
Behavior problems (1–7)	2.60	1.62	2.20	1.17	2.55	1.60	0.03	1.01
Reading skills (1–5)	2.82	0.78	2.53	0.84	2.79	0.81	0.04	0.96
Math skills (1–5)	2.81	0.74	2.63	0.71	2.86	0.70	−0.07	1.06
Critical thinking skills (1–5)	2.76	0.68	2.62	0.69	2.81	0.73	−0.07	0.93
Student–teacher conflict (1–5)	1.93	1.10	1.89	1.03	1.95	1.04	−0.02	1.06
Student–teacher closeness (1–5)	4.27	0.73	4.19	0.72	4.22	0.70	0.07	1.04
Parent involvement (1–4)	2.74	0.51	2.69	0.47	2.70	0.50	0.08	1.02
Parent–child conflict (1–5)	4.59	0.43	2.07	0.72	5.63	0.42	−2.42	1.02
Parent–child closeness 1–5)	2.17	0.80	4.54	0.42	2.16	0.79	0.01	1.01
Behavioral engagement (%)	0.64	0.27	0.68	0.20	0.67	0.30	−0.11	0.90
Off-task behaviors (%)	0.16	0.12	0.14	0.08	0.15	0.11	0.06	1.09

Note. "Good balance" is represented by a situation where the absolute value of the standardized difference is no greater than .1 and the ratio of standard deviations is between .9 and 1.1.

standardized difference between confounding covariates in the INSIGHTS high-dosage and comparison weighted high-dosage group was 0.03 (Range: −.10–.10). The average ratio of standard deviations was 1.02 (Range: .88–1.13). The balance between the INSIGHTS low-dosage and comparison weighted low-dosage group was similarly acceptable. Standardized differences across covariates ranged from −0.11 to 0.09 (average STD difference = .00). The average ratio of standard deviations was 1.02 (Range: .90–1.09). The balance is sufficiently good to suggest that this assumption is not untenable.

Sensitivity Analyses

One potential concern related to dosage analysis is that 8 out of 10 sessions is too high of a standard to represent "high dosage." As such, we repeated the analyses using a 5 out of 10 session threshold as "high dosage." Results were consistent with the findings, favoring impacts for the low parent dosage, rather than the high-dosage group.

Discussion

Previous research on school-based preventive interventions has typically found that more program dosage—at multiple levels—is associated with larger gains for students (Brotman et al., 2011; Gross et al., 2009; Lochman, Boxmeyer, Powell, Roth, & Windle, 2006; Reyes, Brackett, Rivers, White, & Salovey, 2012). Yet, the results of the current article suggest that the dosage story in the INSIGHTS evaluation may be more nuanced than has been previously understood in literature on school-based interventions. Broadly, there were program impacts for children whose parents participated at *high* and *low* levels. However, the magnitude of the program effects on math and reading achievement and more adaptive behaviors was actually larger for children whose parents participated at lower levels. After considering selection into being a high-dosage versus low-dosage parent, however, evidence suggests that children of low-dosage parents were more likely to be at risk for poor achievement, behaviors, and attention. Such selection differences may help explain why gains in achievement and sustained attention, and reductions in disruptive behaviors, were larger for the group of students whose parents participated at lower levels.

Taken together with descriptive findings on the types of parents who participated in INSIGHTS at high rates, it appears that the children of high-dosage parents were already performing better in school than children of low-dosage parents on average, prior to the implementation of INSIGHTS. In turn, the children whose parents participated less frequently or not at all in the intervention appeared to be at higher risk for poor outcomes at the beginning of the study. This finding aligns with previous evaluations of SEL programs in high-poverty samples (e.g., Bierman et al., 2010; Domitrovich, Cortes, & Greenberg, 2007; Jones et al., 2011; Morris et al., 2014). One immediate takeaway from results is that it may not be important to implement a parenting component as part of a school-based SEL program, if the main goal is to improve children's academic and behavioral outcomes. If higher risk children are more likely to benefit from an intervention such as INSIGHTS, it may also be prudent to identify strategies to differentiate the intervention based on both child temperament and level of academic and behavioral skills at baseline. In this way, the intervention may be better designed to support the development of children who have both lower and higher skill levels. On a broader level, however, we also argue that these results can provide insight into designing school-based programs in high-poverty urban schools, and identifying the types of parents who are likely to participate in them.

For example, one key descriptive lesson from this study is that there is great variation in the types of students and families who attend low-income urban schools (Lareau, 2011; Wilson, 2012). In the current sample, drawn exclusively from high-poverty urban elementary schools, there was variation in the extent to which parents were educated, married, employed, and had children who arrived at kindergarten school-ready. In designing interventions in urban schools with parent components it is important to consider that parents may take up programs at different rates, depending on their demographic characteristics (Gibson & Weisner, 2002; Power, Willmot, & Davidson, 2011; Rosier & Corsaro, 1993). Children with parents who are employed and better educated, and have lower behavioral problems to start may be less likely than children with fewer parent supports to benefit from intervention (Bradley & Corwyn, 2002; Conduct Problems Prevention Research Group, 2011; Hart & Risley, 1995; Leventhal & Brooks-Gunn, 2000; Van Lier, Muthén, van der Sar, and Crijnen, 2004). When targeting parents within urban schools, it is then critical to identify the highest-risk students

and allocate sufficient resources to recruiting and retaining their parents (Axford, Lehtonen, Kaoukji, Tobin, & Berry, 2012; Heinrichs, Bertram, Kuschel, & Hahlweg, 2005). Given the considerable burden such efforts put on staff, it may also be vital to hire or identify staff to deal exclusively with program recruitment (Axford et al., 2012).

In this vein, a second major lesson from these findings is that SEL programs that aim to integrate parents likely need to focus a great deal of resources toward efforts to recruit and retain parents. The larger INSIGHTS study was an efficacy trial with a random assignment design. It was explicitly designed to test the effects of the INSIGHTS program, not to find and recruit the hardest-to-reach parents. The broader lessons, however, corroborate past research demonstrating that parents of high-risk children are less likely to take up program services (Durlak & DuPre, 2008). Such information is important because it can help identify possible barriers to school-based program participants. For example, it may be important to address transportation and childcare as potential barriers (Hornby and Lafaele, 2011; Williams & Sánchez, 2011). Psychosocial and cultural barriers, such as parents' role beliefs, invitations from children and teachers to participate in activities, and trust of school settings can pose additional challenges. Schools may need to offer programs in the late evening, early morning, or weekends when urban parents are available to participate. For example, in their study assessing parent engagement in the SEL program the Incredible Years, Axford and colleagues (2012) recommended crucial components of successful programs for parents, including: (a) clear recruitment processes; (b) good communication with stakeholders, (c) incentives for recruitment/retention; (d) active and creative outreach; (e) investment in relationship-building; and (f) accessibility. Integrating these lessons into scale-up efforts is likely critical.

In weighing one's options for implementing SEL programs, developers and practitioners should also have realistic expectations about the considerable cost associated with creating services for parents and investing in their recruitment and retention (Axford et al., 2012). As evidenced in this study, if directly improving students' achievement and social-emotional skills within the school year of intervention is the main purpose of implementing an SEL program, then it may be prudent for schools to focus less strongly or not at all on providing services for parents. In line with findings, the revised INSIGHTS theory of change may argue that children's skills will be best supported through implementation of the teacher and child components of the program. However, if schools' overall goal is to use an SEL program as an opportunity for family and community engagement then they may consider allocating increased time, human capital, and money to implement a parent program. Work should then be done to determine whether hard-to-reach parents are taking part in services and changing practices at home as a result of the program. It is important to note that even in cases where program operators are able to recruit the parents of more disadvantaged children, it is still unclear whether their participation would result in larger program effects. Future work should consider such a hypothesis.

Given current policy debates related to children's academic and behavioral development, we wanted to focus on these child outcomes for this article. However, in discussing these findings, it is important to note that the parent program may have benefited parent and student outcomes we did not consider. For example, in a previous trial of a similar low-income urban population, investigators of INSIGHTS found that the program benefited parenting efficacy, an attitudinal outcome that was not examined in this study (O'Connor, Rodriguez, Cappella, Morris, & McClowry, 2012). Although it is unclear whether parenting efficacy would lead to improvements in children's outcomes, improving parents' attitudes about

their ability to productively rear their children would generally be positive for overall family functioning and parent well-being. It is also possible that the follow-up data collection in this study may have been too short for impacts on parents to influence their children. Past evaluations of interventions directed at parents have shown impacts on children in the longer term but not necessarily immediately (Grindal et al., 2015; Sandler, Schoenfelder, Wolchik, & MacKinnon, 2011). Future studies should collect additional follow-up data to test if effects become realized across time. Parent programs may be more likely than teacher interventions to have gains in the long term because parents are consistently connected to their children, and teachers' relationships with children are more ephemeral (Sandler et al., 2011).

Limitations and Directions for Future Research

The broader INSIGHTS study was not designed to test the relative effects of parent participation on student outcomes. However, limitations in this study can help inform the development of a future project focused on maximizing internal validity. Such an effort would explicitly randomize parents to different levels of participation over and above the teacher and child programs. This would enable the generation of unbiased estimates of the incremental effects of parent dosage on children's outcomes. Such work could truly help identify the added value of the parenting program, relative to the child and teacher programs.

Even given the opportunity to engage in such work in the future, it is important to consider a number of limitations of the current study. First, balance between the treatment and weighted comparison groups was adequate but imperfect. The assumption of balance is somewhat strong in this study. Similar to interpreting the effects as causal, it is possible that the assumption of ignorability was not met given potential unobserved characteristics associated with both parent take-up of services and child outcomes. Second, although temporal precedence was established for the majority of parents who participated when students were in kindergarten, there were also students whose parents participated in first grade. Thus, these parents were not blind to study condition. Additionally, the outcomes measured at T1, T2, and T3 do not reflect parent program participation for 32% of the sample. Third, only outcomes from the published intent-to-treat analysis were examined in this study. It is possible that additional outcomes were improved by high parent participation, which could undermine the finding that impacts were driven by the low parent participation group. Next, the comparison group in this study participated in a supplemental reading program. It may be important for future work to consider a business-as-usual control group when comparing treatment effects by dosage. In addition, given recruitment limitations, it is possible that parents enrolled in the study are not representative of all parents in the school. Finally, this within-group sample has limited generalizability to non-urban, middle to upper income samples.

Implications for Policy and Practice

A growing body of research suggests that SEL programs have the potential to reduce the socioeconomic and racial achievement gaps in early elementary school that become increasingly difficult to close as children age (Durlak et al., 2011). Additional work suggests the importance of integrating parent programs into school-based interventions in order to improve parent engagement and target curricula at multiple settings in which children develop (Bronfrenbrenner & Morris, 1998; Brooks-Gunn & Markman, 2005; Conduct Problems Prevention

Research Group, 2011; Grindal et al., 2015). Yet this article shows that effectively implementing an SEL program that incorporates parent programming is no small task. The parents whose children are already at an advantage may be the ones most likely to participate in programming. Although on average all children may benefit from an SEL intervention, the children whose parents do not participate may experience the biggest impact because they have a greater need for school-based services than children whose parents participate at lower rates.

Policymakers focused on increasing parent engagement should also note the extremely large challenge posed by integrating parents into services. Rather than allocating all funding to service provision, it is also important to provide supports that help schools recruit and retain the hardest-to-reach parents. We do not want to end this article with the reader believing that parent program components make no difference for student outcomes in the context of school-based preventive interventions. The children whose parents participated at the highest rates did experience some benefit of the program, relative to the comparison-condition children predicted to have high-dosage parents. Rather, the main conclusion of this article is that, in cases where the participating parents have children who already have higher academic and behavioral skills at baseline, it may be unlikely for those models to produce larger effects for the high dosage parents. If efforts will not be taken to recruit parents of the children who stand to benefit the most from intervention, parent intervention components may not be effective. Thus, directing the majority of resources toward classroom programming may be the more prudent decision.

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