# Final Report: The Evaluation of the WINGS After-School Social-Emotional Program for At-Risk Urban Children[1]

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[1]This report presents the final results of a research project directed to evaluating and improving the WINGS after-school social-emotional program. The research was supported by the Institute of Education Sciences, U.S. Department of Education, through Grant R305A110703; the National Science Foundation under Grant REAL-1252463; and by the Edna McConnell Clark Foundation (EMCF) and Social Innovation Fund (SIF) through a grant to the WINGS program. The SIF is an important federal initiative that is designed to promote the scaling and replication of promising programs to improve the economic opportunities and development of individuals and communities. The Corporation for National Community Service (CNCS) administers the fund and EMCF is one of the intermediaries responsible for identifying and selecting promising interventions and providing matching funds to help the programs scale and replicate their models. WINGS was one of 12 evidence-based programs selected by EMCF to be part of the SIF and receive funding and technical assistance to scale and replicate its model in various locations across the United States. As part of the SIF, the University of Virginia was asked to extend and expand an ongoing evaluation study of the WINGS programs. This report describes the overall results from that study. The opinions expressed are those of the authors and do not represent views of the U.S. Department of Education, the National Science Foundation, EMCF, or CNCS. Please direct all correspondence to David Grissmer (dwg7u@virginia.edu).

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# The Evaluation of the WINGS After-School Social-Emotional Program for At-Risk Urban Children

## Executive Summary

This report summarizes the impacts of the WINGS program after one and two years of participation using outcome measures provided by parents, teachers, and individual child testing. The report summarizes the final impacts for all three cohorts of children as well as separate estimates for cohorts 1 and 2 and for cohort 3. This is the final report for the project submitted to the Edna McConnell Clark Foundation (EMCF) and to the Social Innovation Fund.

The WINGS program and its evaluation occupy a unique niche in the extensive literature that evaluates interventions to improve social-emotional skills. WINGS is a unique after-school social emotional learning (SEL) program for urban, at-risk K-5 children that was developed by a nonprofit whose research-based design evolved over 10 years prior to evaluation. The evaluation design incorporated randomized controlled trial methods with a three-cohort design that followed kindergarten children for two years through first grade, and had sufficient statistical power (N = 354) to measure results. The evaluation also included perhaps the most extensive data collection for an SEL program evaluation ever undertaken from parents, teachers, and child testing that measured more than 35 developmental, behavioral, and academic outcome measures longitudinally before and after one and two years of participation.

Intention-to-treat (ITT) results for cohorts 1 and 2 showed a pattern of strong ($p < .05$) or marginally strong ($p < .10$) effects (effect size from 0.23 to 0.40) from two years of participation for cohorts 1 and 2 on 12 of 16 measures of teacher-rated classroom behaviors and skills: decision-making ($p < .10$), relationship skills ($p < .10$), self-awareness ($p < .05$), self-management ($p < .10$), social skills composite ($p < .10$), less bullying ($p < .05$), less externalizing ($p < .05$), less hyperactivity ($p < .05$), less problem behaviors ($p < .05$), self-regulation ($p < .05$), closeness to teacher ($p < .10$) , less conflict with teacher ($p < .10$), and a measure of executive function ($p < .05$), and two measures of reading (naming vocabulary [$p < .05$] and letter-word ID [$p < .05$]).

Treatment-on-the-treated (TOT) estimates in cohorts 1 and 2 that are effects for children actually attending WINGS have similar levels of statistical significance to ITT results, but effect sizes are approximately 2.5 times larger (0.6 to 1.0) compared with ITT effects (0.23 to 0.40). The TOT results for cohorts 1 and 2 suggest that children attending two years of WINGS would raise their social-emotional, executive function, and reading and vocabulary skills by 24 to 34 percentile points. These effects would significantly narrow deficits in these skills for at-risk children.

In contrast to cohorts 1 and 2, cohort 3 registered null results for teacher and child testing measures that were predictable. The divergent results for cohort 3 would be predicted by three factors that differ between cohorts 1 and 2 and cohort 3. Cohort 3 had substantially lower compliance rates, higher overall and differential attrition rates, and impaired program quality compared with cohorts 1 and 2. Cohorts 1 and 2 had compliance rates of 39 percent compared with 15 percent for cohort 3. This lower compliance rate in cohort 3 was caused by the WINGS program being closed for one cohort 3 school and by a district-mandated additional after-school program implemented at two schools that led to transfers and lower compliance for WINGS. In addition, the new district-mandated program caused substantial disruptions in access to facilities at one school, which substantially impaired the quality of the program. In addition, the higher overall and differential attrition rates for cohort 3 measures did not meet the What Works Clearinghouse (WWC) 3 liberal standards for teacher and parent data (see Table 9). The failure to meet WWC liberal standards suggest substantial risk for bias.

These factors would predict results for cohort 3 that would approach null results and have a much higher threat for bias than for cohorts 1 and 2 measures. In some ways, cohort 3 simulated a natural experiment that tested whether the evaluation design and methodology would change in response to substantial changes in compliance, program quality, and attrition. The results suggest that the evaluation design and methodology registered these impacts, and that the results from cohorts 1 and 2 represent the effects of WINGS when compliance and program quality is much higher and attrition is much lower. If cohort 3 results had remained at cohorts 1 and 2 levels rather than falling to null results, overall results would have been problematic. The large number of outcome measures combined with the pattern of their effects across these measures, and the pattern of effects across cohorts, suggest an evaluation with strong internal validity.

The results also show that gains in cohorts 1 and 2 occurred after only two years of participation, and results after only one year of participation showed a pattern of null results. The size and significance of the two-year effects over a large set of outcome measures suggest that previous research may be underestimating the potential impact of social-emotional interventions due to their limited dosage of a year or less. The results also suggest that research-based after-school programs that focus on social-emotional skills may be equally or more effective than in-school programs for at-risk children.

Parent-rated measures of home behavior and social-emotional skills showed no impacts from WINGS after one or two years of participation. The lack of home effects might reflect that changed skills and behavior might be easier to transfer to the classroom than the home. A measure of overall parental stress included on the parent survey shows statistically significant higher ($p < .05$) levels of stress for treatment group parents. This increased stress may result from the challenges of having a child attending WINGS and

the associated challenges of late home arrival of a tired child. Parents of WINGS children, other things being equal, are more stressed, and our results also suggest that stressed parents—other things being equal—rate children's behavior lower.

Parent ratings may also be less objective than teacher ratings due to the lack of a peer control group for comparisons. Classroom behavior during the day for an entire school year provides an environment where a child's behavior can be more objectively compared with peers. Finally, teachers have much higher education levels (typically a college degree) than the parents in our sample (typically a high school degree or less). The survey outcome measures could be cognitively challenging in terms of the length of the survey and understanding the developmental language and measures, and teachers may be able to provide more reliable assessments.

The current evaluation using K-1 children may underestimate the impact of the K-5 WINGS program since it suggests that effects may increase for older children and children receiving more dosage. Higher dosage would occur if the WINGS program were implemented in all schools within an urban school district, so that children whose families frequently move between schools could receive more continuous years of participation. Such children receiving 3 to 6 years of participation could be expected to have much larger effects than K-1 children in this study.

Prior to the evaluation, the *level of evidence for the effectiveness of WINGS was preliminary* and came from two sources. The first source was a series of master's theses and unpublished studies that suggest that WINGS participants have better grades, state test scores, school attendance, classroom behavior, self-esteem, and higher high school graduation rates compared with students not in WINGS. The second source was based on the WINGS design that relies on extensive research about the characteristics of high-impact (SEL) programs (e.g., Durlak, Weissberg, et al., 2010) and after-school programs (Durlak, Mahoney, et al., 2010; McComb & Scott-Little, 2003; Kane, 2004).

*This evaluation of WINGS* was designed to provide a *moderate level of evidence on the impact of the WINGS program* with funding from the Institute for Education Science and the Social Innovation Fund. This moderate level was projected based on the design of the evaluation with randomization at entry, large sample size, and the extensive longitudinal data collection through two years of participation. This rigorous experimental study design could provide strong evidence about the causal impacts of providing access to WINGS on children's outcomes. The study participants are at high risk for poor academic and behavioral outcomes, and study results can be generalized to populations with similar characteristics.

Our judgment is that the results of the evaluation provide a moderate-to-strong level of evidence on the effectiveness of WINGS. The pattern of impacts across cohorts, with strong effects for cohorts 1 and 2 together with predictable null effects for cohort 3,

suggest strong internal validity of the results. The contrasting results for one and two years of evaluation also suggest strong internal validity. These patterns suggest that WINGS produces a pattern of strong, significant gains in teacher-rated classroom behavior, social-emotional skills, and child tests of executive function and reading and vocabulary skills after two years of participation. The large number of measures that show significant ($p < .05$) or marginally significant ($p < .10$) effects is unique to this study.

The general threat to internal validity for randomized controlled trials comes from overall and differential attrition and non-compliance that introduces a potential for bias. However, our overall and differential attrition levels for cohorts 1 and 2 for our teacher and child testing nearly all met WWC conservative standards. Non-compliance levels were higher due to unexpected high levels of family relocation to schools not having the WINGS program, but our analysis suggests that characteristics of families relocating were similar for treatment and control groups. We have also used imputation techniques for missing data and estimated both ITT and TOT effects that help to clarify, interpret, and take account of attrition and non-compliance. These results suggest that the levels of attrition and non-compliance were not major factors in biasing effects and their statistical significance. However, our recommendations include expanding WINGS to all schools in a district that would lower both attrition and non-compliance and could support an even more rigorous evaluation leading to an unambiguous strong evidence rating.

## Background

### *The WINGS Program and its Evaluation*

WINGS for Kids is a structured after-school SEL program for children attending low-performing schools in high-risk neighborhoods in Charleston County School District, South Carolina. The schools and communities in North Charleston that are served by WINGS have high levels of social, economic, and academic risk. More than 90 percent of students are black and more than 90 percent are eligible for free or reduced-price lunch. The median family income in 2008 was $39,653, compared to $63,211 for the nation, placing the majority of North Charleston's residents below 200 percent of the poverty level (U.S. Census Bureau, 2008). Fifty-two percent of North Charleston births in 2008 were to single mothers. Given the incidence of crime relative to the population, North Charleston has been ranked the seventh-most dangerous city in the United States (Paras, 2007). Across the schools served by WINGS, the majority of students (42 percent for reading, 52 percent for writing, 50 percent for math, 65 percent for science) do not meet statewide proficiency standards. The graduation rate for the high school attended by students in WINGS schools was 34.3 percent in 2007–2008, compared to 73.2 percent for the nation as a whole (Cataldi, Laird, & Kewal Ramani, 2009; McGinley, Rose, & Donnelly, 2009).

WINGS was designed based on research suggesting that effective SEL programs incorporated components that included (1) high participation rates, (2) a multi-year program, (3) a focus on both academic and social-emotional skills, (4) four "SAFE" characteristics (*sequenced, active, focused, and explicit*), and (5) a focus on five key SEL competencies: *self-awareness, self-management, responsible decision-making, social awareness,* and *relationship skills* (Zins et al., 2004; Payton et al., 2008; Lauer et al., 2006; Greenberg et al., 2003).

The multi-year program allows participation from kindergarten to fifth grade. During our study, the WINGS program served approximately 24 children in each grade at each participating school. The study randomly assigned children in three consecutive cohorts into WINGS at kindergarten entrance or a control group, and followed the children through kindergarten and first grade for up to two years. Control group children spent after-school time usually with parents or caregivers or in other after-school programs.

WINGS afforded opportunities for children to develop SEL skills using a curriculum that was implemented throughout the program's daily activities that included choice time, free play, academic center time, and meals or snacks. WINGS was implemented for three hours per day, five days per week during the school year. At each school, the programs are organized in groups or "nests" of 12 students, with two nests per grade. Each nest is assigned a WINGS Leader who serves as their mentor and teacher for the entire year. The five competencies are addressed across 30 learning objectives. Each week a new learning objective is emphasized and previously taught objectives are reinforced. Teaching is initially direct, with follow-up modeling, opportunity to practice skills, and coaching applied to real life lessons, also known as "teachable moments." Learning objectives are intentionally embedded into every program activity. Through these activities, the WINGS staff model each learning objective and reinforce SEL competencies. **The WINGS program *framework* states that at least two years of participation would be required to see significant shifts in SEL competency.**

The evaluation theory of action predicts that changes in SEL skills will transfer to more positive and less negative relationships and behaviors particularly in the school classroom, but also at home, and will have positive long-term impacts on children's academic outcomes. Three major data collection efforts included a parent survey, a teacher survey, and direct child assessments, which provided confirmatory and exploratory outcome measures. Direct child measures and parent surveys were collected in the summer/fall at kindergarten entry (pre-test), one year later in summer/fall of first grade and two years later at summer/fall of second grade. Teacher surveys were collected in the fall and spring of kindergarten and first grade. The study also collected an exploratory set of "building block" measures of early cognitive and emotional skills to better understand the underlying developmental mechanisms leading to the outcomes and to help interpret the pattern of outcomes.

### *Research Literature*

The evolution of the WINGS program from its inception in the early 2000s and the design and implementation of its evaluation through 2016 was guided by a rich body of literature spanning more than two decades of research on social-emotional skills and programs to improve these skills. SEL broadly refers to the process by which cognitive, affective, and behavioral skills are acquired that help children effectively establish and maintain positive, healthy relationships, successfully carry out various social tasks, and meet daily challenges (CASEL, 2016; Denham et al., 2012; Nickerson & Fishman, 2009). In young children, being socially and emotionally competent means they are able to inhibit impulsive behavioral responses, take into account others' perspectives, make good decisions, express healthy emotions, recognize problems and provide feasible solutions, and adjust and integrate emotions, behaviors, and actions, in order to work well socially with others, act responsibly and respectfully, and display developmentally appropriate prosocial behaviors (Denham et al., 2012; Durlak et al., 2011; Weissberg, Caplan, & Sivo, 1989; Zins, Elias, Greenberg, & Weissberg, 2000).

Children from low-income families, in particular, face many challenges and risks related to their social-emotional development that can have negative consequences later on in life (Duncan & Magnuson, 2005). Gaps in social-emotional development between low-income children and their more affluent peers are observed before entering formal schooling, and these gaps persist and increase during the elementary school years and beyond (Alexander, Entwisle, & Kabbani, 2001; Brooks-Gunn, Duncan, & Aber, 1997; Hamre & Pianta, 2001). Without early intervention in social-emotional and behavioral skills, young children are at greater risk for future academic problems, dropping out of school, peer rejection, and antisocial behaviors (Durlak, Weissberg, Dymnicki, Taylor, & Schellinger, 2011; Durlak & Weissberg, 2011; Greenberg et al., 2003).

Strengthening young children's social-emotional competence may serve as an important protective factor for school and life success, especially if they are exposed to multiple life stressors (Jones, Greenberg, & Crowley, 2015; Webster-Stratton, Reid, & Hammond, 2004). As such, researchers have begun investigating promising approaches and intervention programs, ranging from in-school curricula to teacher and parent training programs that target the promotion of social-emotional competence in children (e.g., Jones & Bouffard, 2012; Morris et al., 2013; Morris et al., 2014; Webster-Stratton,Reid, & Hammond, 2004). There is less work, though, on the impact of social-emotional competence interventions in after-school settings, despite the fact that these competencies can be taught in various ways across many different settings and contexts (CASEL, 2016).

Unfortunately, it is more difficult to conduct research on the effects of social-emotional and other non-cognitive skills than it is to study the direct improvement of academic outcomes during schooling. The complexity associated with measuring social-emotional

skills and measuring the effects of social-emotional programs arises from several sources: the lower reliability of measures, the lack of routine data collection on social-emotional skills, the limited number of programs developing these skills, the challenges of collecting data within school, after-school and family settings, the methodological challenges associated with non-experimental, quasi-experimental, and experimental data, including accounting for non-compliance and non-response, and the lack of studies that provide dosage for more than a single year. These limitations have resulted in a complex body of research literature (Durlak, Weissberg, & Pachan, 2010; Lauer et al., 2006).

Research that spans both experimental and non-experimental results suggests that SEL programming promotes positive youth development across a wide developmental span; in school-based, after-school, and community settings; with students who do and do not have presenting problems; in urban, suburban, and rural areas; among racially and ethnically diverse student bodies; and as implemented by professionals as well as school staff (Payton et al., 2008). Studies have suggested students benefit from SEL across a wide range of outcomes, including having higher-quality relationships with peers and adults, having fewer problem behaviors at school, using drugs and alcohol less, engaging in risky sexual behavior less, and behaving violently less (Greenberg et al., 2003). Students also have better attitudes about themselves, others, and school, and earn higher grades and test scores (Durlak, Weissberg, et al., 2011; Payton et al., 2008).

In a meta-analysis of 317 studies of SEL programs, Payton et al. (2008) suggested that SEL programming was associated with students' gaining an average of 11 to 17 percentile points on achievement tests. Among the 180 studies of programs considered "universal" (not targeted), the authors found a mean effect on academic performance of 0.28. Effects on other outcomes such as attitudes toward self and others, positive social behavior, conduct problems, and emotional distress were similarly in the 0.20 range (Payton et al., 2008). Similarly, in another meta-analysis of 213 studies involving more than 270,000 students, Durlak and colleagues (2011) found that, overall, SEL programs both in and out of school were significantly effective (grand study-level mean = 0.30). Specifically, students who participated in evidence-based SEL programs demonstrated enhanced SEL skills, better attitudes about themselves, others, and school, and increased prosocial behaviors, compared with students who did not participate in these programs. Students participating in the SEL programs also had fewer conduct and internalizing problems, lower levels of emotional distress, increased ability to manage stress and depression, as well as significant gains of 11 percentile points in academic achievement compared with students in the control group. Follow-up data (at least six months later) showed sustained effects in all areas listed above, with effect sizes ranging from 0.11 to 0.32.

### *Characteristics of Successful SEL Programs*

Few SEL programs have been evaluated using experimental methods with long-term follow-up. However, enough empirical evidence exists to suggest that certain types of programs are more effective in improving student outcomes than others. Greenberg et al. (2003) described the most effective programs as those that use structured manuals and curricula to create consistency in program delivery; address a range of SEL constructs; are long-term, with multi-year programs being best; and provide a developmentally appropriate progression of opportunities for skill-building, spanning ideally from pre-kindergarten to adolescence. Zins et al. (2004) noted effective programs tend to be theory- and research-based, with the most rigorous programs undertaking continuous self-improvement through outcome evaluation.

The Collaborative for Academic, Social, and Emotional Learning (CASEL), a group at the forefront of SEL research and theory development, asserts that a combination of social competency instruction and positive learning environments (e.g., a safe and supportive school climate, active partnership between family and school) contributes to children's short- and long-term success (https://casel.org/impact/); Zins et al., 2004). A number of SEL programs based on this theory, including WINGS, use a model of instruction built around a framework of five key person-centered SEL competencies: self-awareness, social awareness, responsible decision-making, self-management, and relationship management (Payton et al., 2008; Zins et al., 2004). Improvements in these skill areas, in conjunction with positive environments, are hypothesized to lead to less risky behavior, greater attachment to school, better academic performance, and more success in life (https://casel.org/impact/).

Results from a recent series of meta-analyses of SEL program effects further suggest that theory-based programs that go on to employ evidence-based skill-training approaches in social competency instruction are the most effective (Durlak & Weissberg, 2007; Durlak, Weissberg, et al., 2010; Payton et al., 2008). ***More specifically, SEL programs that provide training that is sequenced, active, focused, and explicit (given the acronym SAFE) have greater effects on student outcomes across a number of domains.*** Notably, in these meta-analyses, when effect sizes were calculated separately for programs that met SAFE criteria and programs that did not, in many domains where there had previously been a significant overall effect, the effects of non-SAFE programs fell to non-significance while the effects of SAFE programs remained (Durlak & Weissberg, 2007; Durlak, Weissberg, et al., 2011; Payton et al., 2008). For example, in their review of 180 studies of universal SEL programs, Payton et al. (2008) found an overall effect size on students' positive social behavior of 0.24. However, when differentiated, the mean effect size of SAFE programs was 0.28, while the mean effect size of non-SAFE programs was 0.02.

Durlak and Weissberg (2007) and a follow-on meta-analysis by Durlak, Weissberg et al. (2011) found SAFE SEL programs had significant positive effects on a range of student outcomes, including child self-perceptions (self-confidence and self-esteem), school bonding, positive social behaviors, problem behaviors, drug use, achievement test scores, school grades, and attendance. Average effect sizes across these outcomes ranged from 0.14 (school attendance) to 0.37 (child self-perceptions) (Durlak & Weissberg, 2007; Durlak, Weissberg et al., 2010). **Taken together, these results suggest that SEL programs that meet SAFE criteria have particular promise as an intervention promoting positive youth development.**

Five core components of SEL have been identified (CASEL, 2016) and are specifically highlighted within this report: self-awareness, self-management, responsible decision-making, social awareness, and relationships skills.

*Self-awareness* captures the ability to accurately recognize one's feelings, attributes, and values, and understand how those feelings influence behavior (Denham & Brown, 2010). For young children, learning new words to label how they feel and describe what led to those feelings, and developing a sense of self, including knowing what they like and dislike and identifying strengths and weaknesses, are important for developing self-awareness (CASEL, 2016).

*Self-management* describes the ability to successfully regulate one's emotions, thoughts, and behaviors, and appropriately express them in multiple contexts, as well as the ability to manage stress, control impulses, and set goals and persist in meeting those goals (CASEL, 2016). Although some children may be able to describe how they are feeling, most children transitioning to formal school are still learning how to express and react to their feelings and match them to the expectations of the different situations and contexts they encounter.

*Responsible decision-making* entails learning how to make constructive and respectful choices about personal behavior and social interactions, analyzing and solving problems, being ethically responsible, and considering the well-being of oneself and others (CASEL, 2016; Denham et al., 2010). With the help of adults, children are learning how to make choices based on personal opinions, social norms, and rules, and contemplating the consequences of their actions.

*Social awareness* is the ability to understand what behaviors are socially and ethically acceptable in different situations and contexts, as well as the ability to take another's perspective, and show empathy toward others, including those from diverse backgrounds (CASEL, 2016; Denham et al., 2010). Through interacting with peers and adults, children from a young age are learning how to interpret others' emotions and understanding that how they feel may not necessarily be how others are feeling.

*Relationship skills* refer to the ability to establish and maintain healthy relationships across a diverse range of individuals, as well as to use skills, such as cooperation, listening, negotiating, seeking and offering help when needed, and turn taking, to build and sustain these relationships (CASEL, 2016; Denham & Brown, 2010). Young children are beginning to learn what it means to be a good friend, ask for and offer help, communicate effectively, cooperate, negotiate conflicts, and share.

Each of these components, though distinct, is highly interrelated. Both individually and together, they predict a range of positive outcomes (CASEL, 2016; Denham et al., 2012). For instance, children's social-emotional competence has been linked to positive relationship skills and behaviors, both in the classroom and at home, and increases in children's long-term academic skills.

Many interventions and programs targeting the promotion of social-emotional competence also aim to promote the building blocks that set a strong foundation for social-emotional development in young children (e.g., Morris et al., 2014). Executive function, in particular, has received much attention given its critical role in the development of social-emotional competence (e.g., Bierman, Nix, Greenberg, Blair, & Domitrovich, 2008; Blair & Raver, 2015; Riggs, Jahromi, Razza, Dillworth-Bart, & Mueller, 2006).

Executive function is a multi-faceted construct that can broadly be defined as the processes of cognitive flexibility, working memory, and inhibitory control that are necessary for purposeful, goal-directed behavior. Studies show that there are persistent and growing poverty-related gaps, not only in achievement (Reardon, 2011), but also in the regulation of attention, emotion, stress response, and executive function (Cicchetti, 2002; Evans, 2003). Evidence from neuroscientific studies suggest that focusing on executive function can enhance children's learning and development and can establish positive academic trajectories, particularly for children from low-income families (Blair & Raver, 2015; Evans & Schamberg, 2009; Raver, Blair, & Willoughby, 2013).

Specific to children's social-emotional competence, executive function has been directly implicated in the concurrent and longitudinal development of social-emotional skills (e.g., Riggs, Blair, & Greenberg, 2003). This is not surprising given the many overlaps between the subskills that underlie both executive function and social-emotional development. Studies shows that difficulties in executive function lead to difficulties in multiple components of social-emotional functioning, including impulsivity, delay of gratification, emotion regulation, problems with attention, behavioral issues, and problem solving (e.g., Cole, Usher, & Cargo, 1993; Hughes, 2002; Jahromi & Stifter, 2008; Kim et al., 2016; Pennington, 2002; Séguin, Boulerice, Harden, Tremblay, & Pihl, 1999). Moreover, the executive function components related to planning, inhibiting response, and controlling one's attention may be particularly useful for resisting

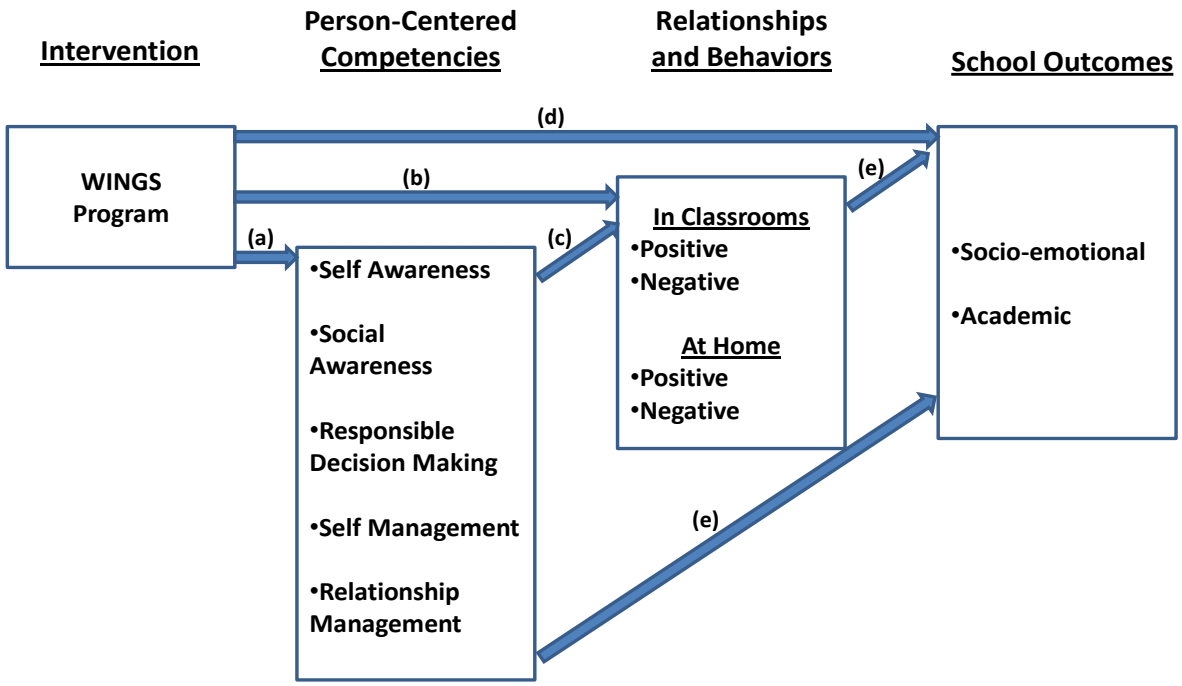
temptation, regulating frustration and stress (Mischel et al., 1989), and behaving according to social norms (Peake, Hebl, & Mischel, 2002).

## Evaluation of WINGS

### *Theory of Action*

Figure 1 depicts the WINGS program's theory of change. The WINGS program had been in existence for about 10 years prior to the start of the evaluation. During that time, the program used the research cited in the previous section and structured a program that met the SAFE criteria and focused on the five competencies described earlier. The stated objectives of the program are to enhance children's person-centered competencies (self-awareness, social awareness, responsible decision-making, self-management, and relationship management), and the theory of change follows that (a) children assigned to WINGS will develop greater person-centered competencies than children who are not assigned to WINGS.

*Figure 1. Theory of Change*




The theory of change also proposes that the positive impacts of assignment to WINGS will transfer to the classroom and home setting. Specifically, we hypothesize that compared with children not assigned to WINGS, children assigned to WINGS will develop and display more positive and less negative behavior and relationships with their teachers and classmates in school and at home. The (b) increased positive and

decreased negative relationships and behaviors are hypothesized to be (c) mediated through the improvements in children's enhanced person-centered competencies. Finally, assignment to WINGS is proposed to have (d) longer-term positive impacts on children's academic school outcomes and social-emotional behavior. We also collected a broader set of measures for exploratory analysis that focused on measures of early emotional and cognitive skills including executive function that can contribute to identifying possible causative mechanisms that underlie the impacts as well as interpret the pattern of results across outcome measures.

### *Research Questions*

The following research questions are addressed in this study:

Question 1. Does assignment to WINGS have a positive impact on children's person-centered competencies after one year (kindergarten) and two years of WINGS (kindergarten and first grade) participation?

Question 2. Does assignment to WINGS have a positive impact on children's relationships and behaviors in the classroom and at home after one year and two years of WINGS participation?

Question 3. Does assignment to WINGS have a positive impact on measures of children's short-term academic skills after one year and two years of WINGS participation?

Question 4. Does the impact of WINGS on children's person-centered competencies, and relationships and behaviors at school and home after one year and two years vary for children with different characteristics?

Question 5. Does the impact of WINGS on children's person-centered competencies, and relationships and behaviors at school and home change across cohorts?

Question 6. Does the impact of WINGS on children's person-centered competencies, relationships and behaviors, and school outcomes vary by the level of initial skills?

### *Exploratory and Confirmatory Outcome Measures*

Assessment tools used in this study included direct child assessments, and measures from teacher and parent surveys on classroom and home behavior and relationships.

Person-Centered Competencies. Direct assessments were completed in areas that align closely with the constructs of self-awareness, social awareness, responsible decision-making, self-management, and relationship management identified in the theory of change. Parents and teachers reported on the five SEL skills (self-awareness, social awareness, responsible decision-making, self-management, and relationship

management) via the *Devereux Student Strengths Assessment* (DESSA; Lebuffe, Shapiro, & Naglieri, 2009).

Teacher-reported measures of children's relationships and classroom behaviors included the *Student-Teacher Relationship Scale* (STRS; Pianta, 2001), which measures the quality of the teacher's relationship with individual children, and the *Social Skills Improvement System* (SSIS; Gresham & Elliott, 2008), which is a measure of an individual child's relationships and social behaviors in the classroom.

Parent-reported measures of children's relationships and behaviors at home were assessed during parent/caregiver interviews using parent versions of the *Social Skills Improvement System* (SSIS; Gresham & Elliott, 2008) and the *Child-Parent Relationship Scale* (Pianta, 1992). We also used the Holmes-Rahe Life Stress Inventory for occurrence of stressful life events. Our measure is a weighted score based on weights developed by the measure's authors in order to adjust for severity of each event (e.g., a death of the caregiver's spouse is weighted 100 points, whereas a major change in eating habits is weighted only 15 points).

School outcomes. Direct assessments of academic outcomes were completed using the *Woodcock-Johnson-III Tests of Achievement* (WJ-III; Woodcock, McGrew, & Mather, 2001), which evaluates reading skills (Sound Awareness and Letter-Word Identification subtests) and mathematics skills (Applied Problems and Quantitative Concepts subtests).

Building block skills included measures of executive function: *Head-Toes-Knees-Shoulders Task* (HTKS; Ponitz, McClelland, et al., 2008), *Emotion Matching Task* (EMT; Morgan, Izard, & King, 2010), *Assessment of Children's Knowledge Task* (ACES; Mavroveli et al., 2009), and *Theory of Mind* (NEPSY II; Korkman, Kirk & Kemp, 2007).

### *Research Design*

Three cohorts of children entering kindergarten whose parents applied for the WINGS program and consented to be in the study were randomly assigned to a treatment group eligible for WINGS participation or a control group not eligible for participation. Cohort 1 included four WINGS schools and cohort 2 and 3 had only three schools due to the discontinuation of the program at one school (James Simons Elementary). James Simons transitioned to a Montessori magnet school that changed its demographic characteristics, and the WINGS program was discontinued. Specifically, child-level random assignment to WINGS or control was determined within four schools in cohort 1 and three schools in cohorts 2 and 3. Because the program serves 12 girls and 12 boys who enter kindergarten each year and conducts SEL activities separately within each gender "nest," gender will also serve as a randomization block to ensure equal numbers of girls and boys are enrolled in the program. Thus, there will be 20 randomization blocks for the three-cohort study.

Pre-test data was collected in the summer/fall of kindergarten and post-test data for one year of potential WINGS participation was collected in the spring of kindergarten and the summer/fall following kindergarten. Post-test data for two potential years of WINGS participation was collected in the spring of first grade and in the summer/fall following first grade.

Table 1 shows that children in the three-cohort study were randomized within 20 randomization blocks. Overall, 209 children were assigned to treatment and 145 to the control group. Table 1 also provides the sample sizes for each randomization block, as well as the probabilities for being assigned to treatment or control within each block. Overall, about 59 percent of participants were assigned to treatment and 41 percent to the control group. We assigned more to treatment to compensate for expected non-compliance.

Table 2 provides descriptive statistics for sample demographic characteristics and life circumstances collected from a parent survey at the beginning of the study. These factors are important because such background characteristics can affect whether and how much children will benefit from a particular intervention. As expected, children in this sample would be characterized as living in high-risk circumstances, with a majority (96 percent) qualifying for free or reduced-price lunch and a majority having racial minority status (91 percent black). Furthermore, 80 percent of families received some form of public assistance, unemployment was 35 percent for caregivers, and the overall education level of mothers was low, with more than a quarter (29 percent of respondent caregivers) having less than a high school degree. The average number of children in the home (2.8) together with the average age of the mothers (29.4) suggest that a significant proportion were teen mothers.

The Holmes-Rahe Life Stress Index (249.5) was quite high as measured over the previous two years. Caregivers reported major changes, with more than 49 percent reporting moving; more than one in six (17 percent) moved more than once during that time. About one-half of caregivers reported a major change in employment or work-related responsibilities, hours, or conditions; and more than a third (36 percent) reported someone in the home starting or stopping either work or education. These characteristics suggest that these low-income families frequently encountered a major change in life circumstances that could affect their level of stress, the behavior and development of their children, and participation and attendance at WINGS.

*Table 1. Randomization Blocks: Frequencies and Probabilities*

| | | | Random Assignment: Frequencies (n) | | Random Assignment: Probabilities (%) | |
|---|---|---|---|---|---|---|
| Cohort | School | Gender | Control | Treatment | Control | Treatment |
| 1 | Chicora | Female | 11 | 16 | 40.7 | 59.3 |
| | | Male | 5 | 9 | 35.7 | 64.3 |
| | Memminger | Female | 6 | 9 | 40.0 | 60.0 |
| | | Male | 9 | 13 | 40.9 | 59.1 |
| | North Charleston Elementary School | Female | 9 | 13 | 40.9 | 59.1 |
| | | Male | 8 | 11 | 42.1 | 57.9 |
| | James Simons | Female | 6 | 7 | 46.2 | 53.8 |
| | | Male | 4 | 4 | 50.0 | 50.0 |
| 2 | Chicora | Female | 8 | 12 | 40.0 | 60.0 |
| | | Male | 6 | 8 | 42.9 | 57.1 |
| | Memminger | Female | 7 | 10 | 41.2 | 58.8 |
| | | Male | 6 | 7 | 46.2 | 53.8 |
| | North Charleston Elementary School | Female | 7 | 11 | 38.9 | 61.1 |
| | | Male | 9 | 11 | 45.0 | 55.0 |
| 3 | Chicora | Female | 8 | 13 | 38.1 | 61.9 |
| | | Male | 10 | 17 | 37.0 | 63.0 |
| | North Charleston Elementary School | Female | 8 | 12 | 40.0 | 60.0 |
| | | Male | 5 | 7 | 41.7 | 58.3 |
| | NCES | Female | 6 | 9 | 40.0 | 60.0 |
| | | Male | 7 | 10 | 41.2 | 58.8 |
| **TOTAL** | | | **145** | **209** | **41.0** | **59.0** |

*Table 2. Characteristics of the Sample*

| Baseline Characteristic or Experience | Mean (SD) | % |
|---|---|---|
| Child Gender (% Male) | | 46.9 |
| Adult Race (% Black) | | 91.0 |
| Child Race (% Black) | | 87.9 |
| Receiving Free/Reduced Lunch | | 96.0 |
| Receiving Other Forms of Public Assistance | | 80.4 |
| Parent Employed (or Student) | | 65.1 |
| Attended Preschool | | 90.0 |
| Mother's Education | | |
| Less than High School | | 29.2 |
| High School/Equivalent | | 36.1 |
| More than High School | | 34.8 |
| Age (years, on first day of school) | 5.5 (0.3) | |
| Mother's Age (years, at time of first survey) | 29.4 (5.2) | |
| Number of Children in Home | 2.8 (1.4) | |
| Holmes-Rahe Life Stress Inventory Weighted Score | 249.5 (167.2) | |

Table 3 shows the demographic characteristics of parents who responded to the baseline survey, and the differences between the treatment and control groups. Overall 2 of 15 family characteristics showed significant differences at baseline: number of children in the home and Holmes-Rahe Life Stress weighted scores. Parents in the treatment group reported higher numbers of children in the home and higher initial stress levels than control group parents. The remaining 13 family characteristics showed no significant differences at baseline, suggesting that random assignment was reasonably balanced on demographic characteristics.

Table 4 shows the differences in baseline outcome measures reported by parents, teachers, and individual testing of children, and the significance of the differences. The parent- and teacher-reported pre-test measures showed only one significant difference (internalizing) among the 18 outcome measures. However, the direct child measures showed a distinct pattern of control children having somewhat higher developmental

skills on four building block measures: theory of mind ($p < .01$), verbal comprehension ($p < .10$), emotional regulation ($p < .10$), and executive function ($p < .15$).

*Table 3. Sample Demographic Characteristics From Parent Survey by Treatment and Control (Characteristics ordered from most different to least different between treatment and control)*

| Baseline Characteristic or Experience | Treatment | | Control | | Comparison | |
|---|---|---|---|---|---|---|
| | Mean (SD) | % | Mean (SD) | % | Diff | *p* value |
| Number of Children in Home | 2.9 (1.4) | | 2.5 (1.3) | | 0.4 | .02 |
| Holmes-Rahe Life Stress Inventory Weighted Score | 249.5 (167.2) | | 193.6 (133.7) | | 54.9 | .04 |
| Attended Preschool | | 88.5 | | 92.4 | -3.9 | .33 |
| Perceived Financial Strain (possible range: 1-5) | 2.0 (0.9) | | 2.0 (0.9) | | 0 | .40 |
| Adult Race (% Black) | | 92.2 | | 89.0 | 3.2 | .41 |
| Number of Moves in 2 Years Prior to Kindergarten | 0.8 (1.0) | | 0.7 (0.8) | | 0.2 | .42 |
| Mother's Education | | | | | | .60 |
| Less than High School | | 27.1 | | 32.5 | -5.4 | |
| High School/Equivalent | | 37.2 | | 34.2 | 3.0 | |
| More than High School | | 35.6 | | 33.3 | -2.3 | |
| Age (Years) | 5.5 (0.3) | | 5.5 (0.3) | | 0 | .70 |
| Receiving Free/Reduced Lunch | | 95.7 | | 96.6 | 0.9 | .77 |
| Child Gender (% Male) | | 46.8 | | 48.2 | 1.4 | .83 |
| Perceived General Stress (possible range: 1-5) | 2.6 (0.6) | | 2.6 (0.6) | | 0 | .85 |
| Receiving Other Forms of Public Assistance | | 80.8 | | 79.8 | 1.0 | .88 |
| Number (#) of Adults in Home | 1.5 (0.7) | | 1.5 (0.6) | | 0 | .92 |
| Parent-Child Relationship Stress (possible range: 1-5) | 1.5 (0.4) | | 1.5 (0.4) | | 0 | .95 |
| Mother's Age (years, at start of study) | 29.0 | | 29.1 | | 0.1 | .96 |

*Table 4. Sample Baseline Outcome Measures by Treatment and Control*
*(Outcomes ordered by Source, Measure, then from most different to least different between treatment and control)*

| Baseline | | | Treatment | | Control | | Difference | |
|---|---|---|---|---|---|---|---|---|
| Source | Measure | Outcome | Mean (SD) | % | Mean (SD) | % | Diff | *p* value |
| **Child Testing** | **# respondents** | | **201** | **96%** | **137** | **94%** | **2%** | **.62** |
| | NEPSY | Theory of Mind | 10.2 (3.9) | | 11.5 (4.2) | | -1.3 | .003 |
| | DAS | Verbal Comprehension | 118.0 (15.0) | | 120.9 (13.0) | | -2.9 | .08 |
| | EMT-ACES | Emotion Recognition | 40.4 (8.9) | | 41.8 (9.2) | | -1.5 | .08 |
| | HTKS | Executive Function | 14.5 (16.8) | | 17.7 (17.5) | | -3.1 | .14 |
| | DAS | Naming Vocabulary | 118.3 (16.0) | | 118.4 (17.1) | | -0.1 | .99 |
| **Teacher Reports** | **# respondents** | | **182** | **87%** | **131** | **90%** | **-3%** | **.44** |
| | DESSA | Self-Management | 3.6 (0.8) | | 3.6 (0.8) | | 0 | .40 |
| | | Self-Awareness | 3.4 (0.9) | | 3.4 (0.8) | | 0 | .46 |
| | | Social Awareness | 3.7 (0.8) | | 3.7 (0.8) | | 0 | .60 |
| | | Relationship Skills | 3.8 (0.8) | | 3.7 (0.7) | | 0.1 | .68 |
| | | Decision-Making | 3.6 (0.8) | | 3.6 (0.7) | | 0 | .78 |
| | STRS | Closeness | 4.1 (0.7) | | 4.2 (0.7) | | -0.1 | .65 |
| | | Conflict | 1.8 (0.9) | | 1.7 (0.8) | | 0.1 | .68 |
| **Parent Reports** | **# respondents** | | **193** | **92%** | **119** | **82%** | **10%** | **.004** |
| | SSIS | Internalizing | 1.5 (0.4) | | 1.4 (0.4) | | 0.1 | .04 |
| | | Bullying | 1.2 (0.4) | | 1.2 (0.4) | | 0 | .59 |
| | | Externalizing | 1.7 (0.5) | | 1.7 (0.5) | | 0 | .76 |
| | | Hyperactivity | 2 (0.6) | | 2 (0.6) | | 0 | .99 |
| | DESSA | Self-Management | 3.8 (0.6) | | 3.8 (0.7) | | 0 | .42 |
| | | Social Awareness | 4.1 (0.6) | | 4.1 (0.6) | | 0 | .63 |
| | | Decision-Making | 4.2 (0.7) | | 4.2 (0.6) | | 0 | .66 |
| | | Relationship Skills | 4.5 (0.6) | | 4.5 (0.5) | | 0 | .70 |
| | | Self-Awareness | 4.3 (0.6) | | 4.3 (0.6) | | 0 | .85 |
| | STRS | Closeness | 4.8 (0.3) | | 4.8 (0.2) | | 0 | .40 |
| | | Conflict | 1.9 (0.8) | | 1.9 (0.8) | | 0 | .94 |

Overall, the pre-test family characteristics and the parent and teacher outcome measures suggests an overall balance due to randomization. However, the direction of the pre-test measures that showed significant or nearly significant differences were in the same direction. These measures suggested that control children were in smaller families that had lower levels of Holmes-Rahe life stress and higher levels of child-tested developmental measures than treatment children. To correct for these differences in estimations, we included covariates for number of children in the home, theory of mind, verbal comprehension, emotional regulation, and executive function. We did not collect Holmes-Rahe measures for all three cohorts, and could not include this measure as a covariate.

### *Overall and Differential Attrition*

Some levels of non-response or attrition is an inevitable part of evaluations, particularly for research that involves programs like WINGS outside the regular school day and where pre-test and outcome data are collected longitudinally from teachers, parents, and children. While the level of overall non-response can pose selectivity issues that can bias effects, a particular issue in experimental studies is the presence of differential attrition between treatment and control groups. The What Works Clearinghouse (WWC) has attempted to quantify the risk associated with various levels of overall and differential attrition (WWC, 2017). We will assess our levels of non-response and use the WWC guidelines to assess bias risk.

Table 5 shows the response rates for each type of assessment at each time point by cohort. There are several patterns in the response data that are important. The response rates in each cohort tended to be the highest at the first data collection in summer/fall of kindergarten, but response rates predictably declined at the summer/fall of first grade and in the summer/fall of second grade. This pattern is typical for longitudinal data collection and a major determinant of declining response was due to the higher than expected mobility of study families relocating—usually to schools in the same school district, but some moved out of the city and state. We followed and collected data from many of these residents who relocated to more than 50 schools within the school district, but response rates for those who moved was lower.

The second pattern in the response data was a decline in response rates from cohort 1 to cohort 3. This pattern is likely due to two factors. The first factor was the increasing workload on study personnel as the number of data collection points peaked in later time periods when later data collection in cohort 1 overlapped with initial data collection in cohort 3. That is, the data collection for each cohort could span more than two years, so initial data collection for cohort 3 could overlap with final data collection for cohort 1 and intermediate data collection for cohort 2. The second factor was the much higher than anticipated migration of families to more distant schools, so that in each cohort,

later data collection meant traveling, contacting and seeking permission from new schools and teachers, and more difficulty in contacting parents for survey data. As the unexpected workload grew, response rates declined particularly for cohort 3.

*Table 5. Percentage of Data Collected (Response Rates) by Time Point, Measure Type, and Cohort*

| Time Point | Type of Measure | Cohort 1 | Cohort 2 | Cohort 3 |
|---|---|---|---|---|
| Summer/Fall of Kindergarten | Child Testing Measures | 99% | 98% | 90% |
| | Parent Reports | 92% | 97% | 75% |
| | Teacher Reports | 93% | 96% | 76% |
| Spring of Kindergarten | Teacher Reports | 96% | 97% | 75% |
| Summer/Fall of First Grade | Child Testing Measures | 95% | 84% | 70% |
| | Parent Reports | 89% | 75% | 63% |
| | Teacher Reports | 86% | 53% | 42% |
| Spring of First Grade | Teacher Reports | 73% | 60% | 47% |
| Summer/Fall of Second Grade | Child Testing Measures | 77% | 63% | 69% |
| | Parent Reports | 73% | 63% | 70% |

The third pattern in the data is that child testing response rates were generally higher than teacher and parent response rates partly due to having easier access to children for testing in schools. Teacher response rates were usually lower than parent rates for later time points because for students who relocated to new schools, permission from schools and teachers was needed for cooperation with the study. Teacher response rates were particularly low for cohort 3, with less than a 50 percent response rate for teacher surveys that measured two years of participation.

Response rates for low-income, urban families are a particular challenge due to their frequent relocation of households and changing schools for their children. Table 6 shows the percentage of children who were enrolled in a non-study school by the summer of 2015, approximately three years after the start of the study for cohort 1 and two years after the start of the study for cohort 2. This data suggests an annual migration of 20 percent of children relocating to non-study schools during the study. Study children who originally attended four study schools are currently dispersed across at least 52 different schools, only 10 of which are outside of South Carolina. Part of the cause of the relocation can be changes in jobs or income that demand a move. The higher relocation rates at Memminger and NCES suggest that some relocation may have been to better housing and/or jobs because parents had higher education levels than at Chicora.

*Table 6. Percentage of Children Relocated by Cohort and School*

| School attended at start of study | Percentage relocated to non-study schools as of summer 2015 | |
|---|---|---|
| | Cohort 1 | Cohort 2 |
| Chicora | 39% | 29% |
| Memminger | 68% | 43% |
| NCES | 68% | 34% |
| JSE | 43% | N/A |
| Total | 56% | 35% |

Table 7a provides response rates for all three cohorts by type of measure (parent, teacher, and child testing) and by treatment and control group for the one-year participation sample and the two-year participation sample. The table also shows the differential attrition level and the tests for the statistical significance of the differences between treatment and control groups. Only 1 of 10 comparisons between treatment and control groups show a significant difference. The initial response rate in the first parent survey was 82 percent compared to 92 percent for the treatment group.

Table 7b is similar to Table 7a, but it shows data for cohorts 1 and 2 only. This data shows no significant differences for the 10 comparisons between treatment and control groups. However, Table 7c provides similar data for cohort 3 only and it shows significant differences for 2 of the 10 comparisons and much greater differential attrition levels than in cohorts 1 and 2.

Tables 8 and 9 show the WWC status of each data collection point in the one-year evaluation and two-year evaluation, respectively. For the one-year evaluation including data from all three cohorts, Table 8 shows that 5 of the 6 data collection points used at pre-test and post-test met conservative WWC criteria, while the parent data at pre-test met liberal WWC standards. Similarly, Table 8 shows for cohorts 1 and 2 combined that 5 of 6 data collection points met conservative WWC standards, and the post-test parent surveys met liberal WWC standards. In contrast, the data for cohort 3 shows that 4 of 6 data collection points (all for parents and teachers) failed to meet liberal WWC standards, while two direct child measures data collection points met WWC conservative standards. This data suggests that the direct child measures from all three cohorts can be used for evaluation of one-year effects, but data from cohorts 1 and 2 will provide more reliable estimates for teacher and parent measures.

*Table 7a. Overall and Differential Response Rates for Each Type of Measure and Time Point for One- and Two-Year Participation Samples, All Three Cohorts*

| Time Point | Type of Measure | Percentage Collected | | Difference |
|---|---|---|---|---|
| **One Year of Participation Sample** | | | | |
| | | Treatment (n = 209) | Control (n = 145) | % Diff (*p* value) |
| Summer/Fall of Kindergarten | Child Testing | 96% | 94% | 2% (*p* = .62) |
| | Parent Reports | 92% | 82% | 10% (*p* < .01 ) |
| | Teacher Reports | 87% | 90% | -3% (*p* = .44) |
| Spring of Kindergarten | Teacher Reports | 88% | 94% | -6% (*p* = .28) |
| Summer/Fall of First Grade Entry | Child Testing | 81% | 85% | -4% (*p* = .35) |
| | Parent Reports | 79% | 74% | 5% (*p* = .32) |
| **Two Years of Participation Sample** | | | | |
| | | Treatment (n=209) | Control (n=145) | % Diff (*p* value) |
| Fall of First Grade | Teacher Reports | 60% | 66% | -6% (*p* = .31) |
| Spring of First Grade | Teacher Reports | 62% | 59% | 3% (*p* = .66) |
| Summer/Fall of Second Grade | Child Testing | 71% | 70% | 1% (*p* = .83) |
| | Parent Reports | 71% | 66% | 5% (*p* = .42) |

Table 9 shows the data using WWC criteria to measure two-year impacts. When all cohorts are included, 4 of the 6 data collection points (all direct child measures and teacher data) met WWC conservative standards, with only the two parent reports meeting liberal standards. When only cohorts 1 and 2 are included, all 6 data collection points met WWC standards. However, the data for cohort 3 only shows that 4 of 6 data collection points (all teacher and parent data) did not meet even liberal standards, while the direct child measures met conservative standards at pre-test and liberal standards at post-test. This suggests that data for cohorts 1 and 2 have little risk for bias when evaluating two-year effects, but cohort 3 carries considerable risk if used to evaluate two-year effects.

*Table 7b. Overall and Differential Response Rates for Each Type of Measure and Time Point for One- and Two-Year Participation Samples, Cohorts 1 and 2*

| Time Point | Type of Measure | Percentage Collected | | Difference |
|---|---|---|---|---|
| **One Year of Participation Sample** | | | | |
| | | Treatment (n = 141) | Control (n = 101) | % Diff (*p* value) |
| Summer/Fall of Kindergarten | Child Testing | 99% | 96% | 3% (*p* = .20) |
| | Parent Reports | 96% | 91% | 5% (*p* = .14) |
| | Teacher Reports | 95% | 93% | 2% (*p* = .71) |
| Spring of Kindergarten | Teacher Reports | 96% | 97% | -1% (*p* = 1.00) |
| Summer/Fall of First Grade entry | Child Testing | 87% | 91% | -4% (*p* = .37) |
| | Parent Reports | 77% | 84% | -7% (*p* = .25) |
| **Two Years of Participation Sample** | | | | |
| | | Treatment (n = 141) | Control (n = 101) | % Diff (*p* value) |
| Fall of First Grade | Teacher Reports | 70% | 76% | -7% (*p* = .31) |
| Spring of First Grade | Teacher Reports | 67% | 68% | -2% (*p* = .90) |
| Summer/Fall of Second Grade | Child Testing | 70% | 71% | -1% (*p* = .97) |
| | Parent Reports | 68% | 68% | 0% (*p* = 1.00) |

*Table 7c. Overall and Differential Response Rates for Each Type of Measure and Time Point for One- and Two-Year Participation Sample, Cohort 3*

| Time Point | Type of Measure | Percentage Collected | | Difference |
|---|---|---|---|---|
| **One Year of Participation Sample** | | | | |
| | | Treatment (n = 68) | Control (n = 44) | % Diff (*p* value) |
| Summer/Fall of Kindergarten | Child Testing | 91% | 89% | 3% (*p* = .20) |
| | Parent Reports | 84% | 61% | 22% (*p* = .01) |
| | Teacher Reports | 71% | 84% | 14% (*p* = .16) |
| Spring of Kindergarten | Teacher Reports | 71% | 82% | -11% (*p* = .26) |
| Summer/Fall of First Grade Entry | Child Testing | 66% | 68% | -2% (*p* = .99) |
| | Parent Reports | 74% | 48% | 25% (*p* = .01) |
| **Two Years of Participation Sample** | | | | |
| | | Treatment (n = 68) | Control (n = 44) | % Diff (*p* value) |
| Fall of First Grade | Teacher Reports | 41% | 43% | -2% (*p* = .99) |
| Spring of First Grade | Teacher Reports | 53% | 39% | 14% (*p* = .20) |
| Summer/Fall of Second Grade | Child Testing | 74% | 66% | 8% (*p* = .51) |
| | Parent Reports | 76% | 59% | 17% (*p* = .08) |

*Table 8. What Works Clearinghouse Status of Data Used in One-Year Participation Evaluation*

| Data Used for One-Year Participation Evaluation | | | |
|---|---|---|---|
| **Time Point** | **Type of Measure** | **Cohorts** | **WWC Status** |
| Summer/Fall of Kindergarten Pre-tests | Child Testing Measures | 1 and 2 | Conservative |
| | | 3 | Conservative |
| | | 1-3 | Conservative |
| | Parent Reports | 1 and 2 | Conservative |
| | | 3 | Neither |
| | | 1-3 | Liberal |
| | Teacher Reports | 1 and 2 | Conservative |
| | | 3 | Neither |
| | | 1-3 | Conservative |
| Spring of Kindergarten | Teacher reports | 1 and 2 | Conservative |
| | | 3 | Neither |
| | | 1-3 | Conservative |
| Summer/Fall of First Grade Entry | Child Testing Measures | 1 and 2 | Conservative |
| | | 3 | Conservative |
| | | 1-3 | Conservative |
| | Parent Reports | 1 and 2 | Liberal |
| | | 3 | Neither |
| | | 1-3 | Conservative |

*Table 9. What Works Clearinghouse Status of Data Used in Two-Year Participation Evaluation*

| Data Used for Two-Year Participation Evaluation | | | |
|---|---|---|---|
| **Time Point** | **Type of Measure** | **Cohort** | **WWC Status** |
| Summer/Fall of Kindergarten Entry (Pre-tests) | Child Testing Measures | 1 and 2 | Conservative |
| | | 3 | Conservative |
| | | 1-3 | Conservative |
| | Parent Reports | 1 and 2 | Conservative |
| | | 3 | Neither |
| | | 1-3 | Liberal |
| | Teacher Reports | 1 and 2 | Conservative |
| | | 3 | Neither |
| | | 1-3 | Conservative |
| Spring of First Grade | Teacher reports | 1 and 2 | Conservative |
| | | 3 | Neither |
| | | 1-3 | Conservative |
| Summer/Fall of Second Grade Entry | Child Testing Measures | 1 and 2 | Conservative |
| | | 3 | Liberal |
| | | 1-3 | Conservative |
| | Parent Reports | 1 and 2 | Conservative |
| | | 3 | Neither |
| | | 1-3 | Liberal |

### *Non-Compliance and its Significance*

Sample Non-Compliance In contrast to the term "study attrition," which refers to individual children or families not being available to provide data for the *study*, the term non-compliance refers to whether the participants complied with their treatment and control assignment. Participants who won the lottery were non-compliers if they did not meet the standards for completing either one or two years of WINGS (no-shows). Participants who lost the lottery were non-compliers if they actually attended the WINGS program and met the standards for one and two years of attendance. These participants are termed "crossovers." Non-compliance can introduce bias if the non-compliers' characteristics are not similar to compliers for both treatment and control groups. Non-

compliance does not directly affect the estimation of ITT effects, but it can make their interpretation more complex through markedly affecting TOT results.

When non-compliance is present, decisions and rules are needed to determine which children received "treatment." A common assumption in many interventions is to designate children receiving any dosage as "receiving treatment." However, according to the WINGS logic model, attending WINGS for two years is thought to be necessary before seeing positive impacts. In conjunction with WINGS personnel, we also established minimum attendance criteria for each year based on actual attendance data. We set the criteria of at least 100 days of attendance in kindergarten and first grade to qualify as having "received treatment." Based on these criteria, Tables 10, 11, and 12 provide the consort data for the levels of compliance and non-compliance by cohort.

Table 10 shows that 30 of 82 (37 percent) participants assigned to treatment in cohort 1 met the attendance criteria in both kindergarten and first grade, while the compliance rate for treatment in cohort 2 (see Table 11) was 42 percent and the compliance rate in cohort 3 (see Table 12) was 15 percent. The compliance rates for one year of attendance were much higher, with 68 percent (cohort 1), 61 percent (cohort 2), and 46 percent (cohort 3) receiving treatment. The compliance rates for control children were very high, with 98 percent (cohort 1), 98 percent (cohort 2), and 100 percent (cohort 3) for one-year participation, and 86 percent (cohort 1), 91 percent (cohort 2), and 100 percent for children not receiving treatment.

*Table 10. Cohort 1 Two-Year Consort Data*

| Randomized Children: | 140 | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Treatment Condition:** | **Treatment (n = 82)** | | | | **Control (n = 58)** | | | |
| Attended at least 100 days in kindergarten? | Yes | | No | | Yes | | No | |
| | 56 | | 26 | | 1 | | 57 | |
| % of condition group | 68% | | 32% | | 2% | | 98% | |
| Attended at least 100 days in first grade? | Yes | No | Yes | No | Yes | No | Yes | No |
| | 30 | 26 | 8 | 18 | 0 | 1 | 7 | 50 |
| % of condition group | 37% | 32% | 10% | 22% | 0% | 2% | 12% | 86% |

The low compliance rate for treatment children in cohort 3–less than one-half of the cohorts 1 and 2 rates–can be attributed to two factors: (1) The WINGS program was closed at one of the three WINGS schools in cohort 3 so that treatment children could not attend WINGS in their second year of participation. (2) The school district mandated that each school initiate a district-sponsored after-school program, which had two

impacts. At one school the WINGS program had restricted access to facilities, which significantly compromised the effectiveness of the program. Also at both schools with a WINGS program, children could transfer to the alternate program that provided less restrictive attendance and earlier transportation. The net impact of these changes was that cohort 3 children had much lower compliance rates and exposure to a less effective program. Our earlier attrition data also suggested that cohort 3 teacher response data often failed to meet WWC liberal standards, increasing the risk for bias. The net effect of lower compliance and failure to meet WWC standards suggests that cohort 3 results might be different than cohorts 1 and 2 results, and estimation methodology should test whether these differences are present.

*Table 11. Cohort 2 Two-Year Consort Data*

| Randomized Children: | 102 | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Treatment Condition:** | **Treatment (n = 59)** | | | | **Control (n = 43)** | | | |
| Attended at least 100 days in kindergarten? | Yes | | No | | Yes | | No | |
| | 36 | | 23 | | 1 | | 42 | |
| % of condition group | 61% | | 39% | | 2% | | 98% | |
| Attended at least 100 days in first grade? | Yes | No | Yes | No | Yes | No | Yes | No |
| | 25 | 11 | 5 | 18 | 1 | 0 | 3 | 39 |
| % of condition group | 42% | 19% | 8% | 31% | 2% | 0% | 7% | 91% |

*Table 12. Cohort 3 Two-Year Consort Data*

| Randomized Children: | 112 | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Treatment Condition:** | **Treatment (n = 68)** | | | | **Control (n = 44)** | | | |
| Attended at least 100 days in kindergarten? | Yes | | No | | Yes | | No | |
| | 31 | | 37 | | 0 | | 44 | |
| % of condition group | 46% | | 54% | | 0% | | 100% | |
| Attended at least 100 days in first grade? | Yes | No | Yes | No | Yes | No | Yes | No |
| | 10 | 21 | 0 | 37 | 0 | 0 | 0 | 44 |
| % of condition group | 15% | 31% | 0% | 54% | 0% | 0% | 0% | 100% |

The study maintained records for each child that drew from parent conversations and WINGS personnel and recorded the reasons for treatment children withdrawing from the program. This data shows the reasons for non-compliance. About 60 percent of non-

compliance for treatment children was due to a relocation and attendance at another school without the WINGS program. The WINGS program was available only in the four study schools, so almost all relocation was to district schools without WINGS. The second most important reason for non-compliance (23 percent) was removal by the parent without relocation. Removal by the parent could occur for a wide number of reasons and might reflect the increased stress on the child and parent from the longer day at school, and the parent preference to have the child return home after school. Parents responded very positively when asked about the WINGS program, and there is little evidence that dissatisfaction with WINGS was a significant cause of removal.

## Estimation Methodology

We estimate the intent-to-treat (ITT) and treatment-on-the-treated (TOT) impacts of WINGS on each of the more than 35 parent, teacher, and child testing measures after both one year and two years of potential participation. The ITT analysis provides estimates of the impact on all children assigned to WINGS, regardless of whether they attended. If all students winning the lottery accepted the offer to attend, and no lottery losers gained access to WINGS (i.e., perfect compliance), then ITT effects would reflect the impact of ***actually attending*** WINGS. Since less than one-half of treatment children attended two years of WINGS, ITT estimates do not reflect impacts for students who actually attended WINGS. TOT provides estimates for those who actually attended WINGS. The TOT effects show much larger effects than ITT effects.

We report three levels of "statistical significance" at the $p < .05$, $p < .10$, and $p < .20$ levels because lower levels of significance are important for providing guidance on improving the program, for interpreting patterns of results across measures, and possibly foreshadowing future effects with larger samples. We evaluated the magnitude of effect sizes by the conventional scale of small effect (~0.25 standard deviation [SD]), moderate effect (~0.50 SD), and large effect (~0.75 SD).

Intention-to-Treat (ITT) Analysis. The ITT estimates are calculated from a multiple regression model:

$$Y_i = \beta_1 T_i + \sum_{q=1}^{N} \delta_q X_{qi} + \varepsilon_i$$

where $Y_i$ represents an achievement outcome $Y$ for student $i$, $T_i$ is a dichotomous variable indicating the lottery outcome for student $i$ (0 = lottery loser-control; 1 = lottery winner-treatment), and the coefficient $\beta_1$ is the WINGS treatment effect of interest. $X_{qi}$ is a vector of N covariates and $\varepsilon_i$ is the random error term. The covariates include several sets of variables. The pre-test variable is included. Lottery-specific fixed effects are dummy variables that account for the 20 lotteries (by school, cohort, and gender) and

account for lottery-level heterogeneity of the types of students and other factors that differ across lotteries and also correct the standard errors for the loss of degree of freedom due to blocking. Covariates to adjust for differences caused by randomization between test and control groups are also included. These covariates include those characteristics shown in Tables 3 and 4 that showed significant differences between test and control groups (number of children in the home, theory of mind, verbal comprehension, emotion recognition, and executive function). In addition, we included demographic characteristics that include mother's education, age of mother at child's birth, and child's age. Finally, for child-administered and parent measures we included only the variables that reflected the difference in timing and setting for child testing and parent survey administration. These included a dummy for testing done during the summer or school year and a variable for the days between pre-test and post-test. These adjustments were not necessary with teacher reports, as all teacher reports were collected on roughly the same date; in contrast, the parent reports and direct assessments had wide windows for administration (from summer through fall).

Treatment-on-Treated (TOT) Analysis. The ITT estimates present an internally valid estimate of the effect of being offered a position in WINGS. However, not all students who were assigned to the treatment group chose to enroll in WINGS to receive the required dosage of 100 days in kindergarten for the one-year impact analysis, or 100 days in both kindergarten and first grade for the two-year impact analysis. In addition, a few children who lost the lottery (crossovers) attended WINGS and received the required dosage. Therefore, the ITT estimates do not accurately measure the impact of actually *attending WINGS and receiving the required dosage*.

We estimated the effect of attending WINGS using an instrumental variables (IV) approach to account for no-shows or those who chose not to attend after being offered admission, and crossovers or those who attend WINGS after not being offered admission (Bloom, H. S.,1984). The first-stage equation, relating treatment assignment to treatment receipt, was as follows:

$$\hat{T}_i = \gamma_0 + \gamma_1 Z_i + \Sigma \gamma_q X_{qi} \quad (2)$$

where $\hat{T}_i$ represents the expected treatment receipt status for child $i$ (0 = did not receive WINGS treatment; 1 = received WINGS treatment), based on the child's treatment assignment and consideration of covariates, $\gamma_1$ is the effect of treatment assignment ($Z_i$; 0 = control; 1 = treatment) on treatment receipt ( $T_i$), and $X_{qi}$ is a vector of covariates with their accompanying regression coefficients, $\gamma_q$, representing each covariate's "effect" on the child's likelihood to receive treatment.

The second-stage equation, which models the association between predicted treatment receipt and child outcomes, is:

$$Y_i = \delta_0 + \delta_1 \hat{T}_i + \sum \delta_{qi} X_{qi} + e_i \quad , (3)$$

where $\delta_1$ represents the association between *predicted* treatment receipt ($\hat{T}_i$, as in equation 2) and child outcome $Y_i$, and $X_{qi}$ is our vector of covariates with their accompanying regression coefficients, $\delta_{qi}$, representing each covariate's "effect" on the outcome. Because $\hat{T}_i$ is the predicted treatment receipt based on random assignment and covariates, the magnitude of $\delta_1$ can be interpreted as the difference in child outcomes between children who do and do not receive WINGS treatment due to treatment and assignment and covariates. A reduced single-equation form of these analyses can be devised by substituting the right side of formula 2 in for $\hat{T}_i$ in formula 3. After some simplification, this reduced form can be written as:

$$Y_i = \beta_0 + \beta_1 Z_i + \sum \beta_{qi} X_{qi} + \varepsilon_i \quad , (4)$$

where $\beta_1 / \delta_1$ represents the estimate for our treatment effect.

We also estimated ITT and TOT interaction effects by cohort (cohorts 1 and 2 versus cohort 3), gender, and level of initial skills. For instance, the three cohort results were estimated using the ITT estimation described earlier with full imputation, while the results for cohorts 1 and 2 and cohort 3 were estimated using an interaction term for either cohorts 1 and 2 or cohort 3 with full imputation. Similar interaction estimates were done using gender or initial skill level as the interaction term.

### *Imputation of Missing Data*

Missing data can be problematic for randomized controlled trials in two ways. The first concern is selectivity bias if the characteristics of participants with data differ from those without data. A particular concern is when there are different levels of missing data and potential differential selectivity between test and control groups. In our sample, there is little evidence of selectivity bias from differences in overall attrition or differential attrition between treatment and control groups in cohorts 1 and 2, but cohort 3 is more problematic (see Tables 5, 7a, 7b, 7c, 8, and 9). The second concern is that missing data can reduce the statistical power of the sample to detect effects if only observations with complete data are used in estimation.

Imputation of missing data does not always result in increasing the power of the sample (and significance of results) because imputation also introduces additional uncertainty into standard errors. The magnitude of this uncertainty depends on the amount and quality of non-missing data and whether imputations using this non-missing data can reliably predict missing data. Imputation can therefore change effect coefficients in either direction (indicating selectivity effects between missing and non-missing data) as well as their statistical significance in either direction. If attrition is random, then effect estimates should not change and the net effect on standard errors of added sample size and uncertainty from imputation would be to reduce standard errors and increase the statistical significance of effects.

The inclusion of missing data into both the ITT and TOT estimates posed a challenge because the most frequently used procedure for incorporating missing data is full information maximum likelihood (FIML) estimates. However, while FIML can be used for ITT estimates, we could not find any literature or software that allowed TOT estimates to incorporate FIML. TOT estimates are two-stage estimates, and FIML is currently only used for one-stage estimates.

Our use of multiple imputation (MI), rather than FIML, was based on the fact that TOT estimates are a critical component in interpreting the impacts of WINGS and were attainable by using an MI approach. The MI approach begins by filling in, that is imputing, probable values in place of missing data points. This process makes use of information from predictor variables as well as auxiliary variables, and also explicitly preserves uncertainty of imputed estimates by creating multiple, separate imputed data sets. Thus, each missing observation to be imputed is replaced with not just one imputed value, but several, and can therefore be thought of as having its own distribution of plausible values. Analyses are then run and pooled across imputed data sets to provide reliable parameter estimates, with standard error calculation accounting for the uncertainty retained by the multiple estimates for each observation.

MI is a general technique that can be carried out using any one of a variety of algorithms, procedures, and software tools. We used the MICE (multivariate imputation by chained equations-R version 3.2.5) (Van Buuren & Groothuis-Oudshoorn, 2011), alternatively called fully conditional specification. In a chained equations approach, the imputation model is defined variable by variable to allow for customization by analysts according to the unique features of each variable (e.g., the reasons why a particular variable has missing data). Then, each equation is run successively in an order defined by the analyst (in our case, predictors were imputed before outcomes) and the process is repeated over several iterations (in our case, 40 iterations) to improve precision.

We imputed data separately by construct (or "bucket") represented in the WINGS logic model. Thus, a set of imputed data was generated for teacher Devereux Student Strengths Assessment (DESSA) outcomes, along with all predictors in the teacher DESSA models, for example. We chose to impute 40 data sets for each "bucket," and all subsequent analyses was performed on each of the 40 data sets before being pooled to produce final estimates. Auxiliary variables included child outcomes from "middle" time points, which were not included in the models (e.g., for "building blocks" outcomes in our two-year analyses, the kindergarten assessments were used as predictors, the second-grade assessments as outcomes, and the first-grade assessments as auxiliary variables).

## Results

The results differed markedly by source of the outcome data. Teacher- and child-administered outcome measures show a different pattern of results than parent-reported measures. We discuss these sets of results separately.

### *Teacher- and Child-Administered Outcome Measures*

The pattern of effects sizes and level of significance of the evaluation results for WINGS suggest five distinct patterns of results across our teacher and child outcome measures. These five distinctive patterns of results are:

- Table 13 shows distinctly different results for cohort 3 versus cohorts 1 and 2. Cohort 3 has a predictable pattern of null ITT effects for two years of participation with about equal numbers of coefficients that are positive or negative, with no statistically significant coefficients at $p < .05$ or $p < .10$. In contrast, cohorts 1 and 2 results show a pattern of all but one coefficient being positive and 15 of 23 statistically significant coefficients at $p < .05$ or $p < .10$.
- Table 13 shows a pattern of significant ($p < .05$) or marginally significant ($p < .10$) ITT effects (effect sizes from 0.23 to 0.40) for cohorts 1 and 2 for two years of participation for 15 of 23 teacher-reported or child-administered measures: decision-making ($p < .10$), relationship skills ($p < .10$), self-awareness ($p < .05$), self-management ($p < .10$), a social skills composite ($p < .10$), less bullying ($p < .05$), less externalizing ($p < .05$), less hyperactivity ($p < .05$), less problem behaviors ($p < .05$), self-regulation ($p < .05$), closeness to teacher ($p < .10$) , less conflict with teacher ($p < .10$) , executive function ($p < .05$), naming vocabulary ($p < .05$) and letter-word ID ($p < .05$).
- Table 14 compares ITT and TOT results for two years of participation for cohorts 1 and 2 that show TOT estimates for children actually attending WINGS that have similar levels of statistical significance to ITT results, but TOT effect sizes were approximately 2.5 times larger (0.6 to 1.0) than ITT effects (0.23 to 0.40).

- Table 15 shows that the pattern of strong effects for two years of participation contrasts with very weak to null effects after one year of participation. The results for the sample at the end of the first year show null effects for teacher-rated measures, and small statistically significant or marginally significant for two child testing outcome measures: verbal comprehension and executive function. These results suggest the program may have been less effective for kindergarten students and/or that the large effects of WINGS require two years of participation and/or that effects are delayed. In the latter case, the early effects of executive function and verbal comprehension for one year of participation may have contributed to the large-scale effects after two years of participation.
- Finally, Table 16 shows ITT impacts after one year for three samples: all three cohorts, cohorts 1 and 2 only, and cohort 3. These effects are estimated similarly to the effects in Table 13. However, unlike in Table 13, the effects after one year do not show the distinctive pattern across cohorts that the effects after two years show. Effects after one year show consistent and predominantly null effects across all three cohorts. These results suggest that the distinctive and different pattern of effects shown for cohorts 1 and 2 versus cohort 3 for two-year effects are not likely caused by differences in characteristics of children across cohorts or in differences in program effects at kindergarten. Rather, these results suggest that differences at two years between cohorts 1 and 2 and cohort 3 might be due to: (1) changes in program effectiveness during the second year for cohort 3, (2) changes in the quality of measurements during the second year for cohort 3, or (3) changes in participation or compliance in the second year for cohort 3.

*Table 13. Comparing ITT Effects for Two Years of Participation for Different Cohorts*

| | | | All Three Cohorts | | Cohorts 1 and 2 | | Cohort 3 | |
|---|---|---|---|---|---|---|---|---|
| Source | Measure | Outcome | Effect | Stat-Sig | Effect | Stat-Sig | Effect | Stat-Sig |
| Teacher | DESSA | Decision-Making | 0.17 | + | 0.24 | * | 0.010 | |
| Teacher | DESSA | Relationship Skills | 0.20 | + | 0.23 | * | 0.123 | |
| Teacher | DESSA | Self-Awareness | 0.21 | * | 0.30 | ** | 0.029 | |
| Teacher | DESSA | Self-Management | 0.17 | + | 0.26 | * | -0.018 | |
| Teacher | DESSA | Social Awareness | 0.04 | | 0.15 | | -0.205 | |
| Teacher | DESSA | Social Skills Comp. | 0.17 | + | 0.25 | * | -0.017 | |
| Teacher | SSIS | Less Bullying | 0.12 | | 0.31 | ** | -0.305 | |
| Teacher | SSIS | Less Externalizing | 0.13 | | 0.30 | ** | -0.266 | |
| Teacher | SSIS | Less Hyperactivity | 0.24 | * | 0.40 | ** | -0.124 | |
| Teacher | SSIS | Less Internalizing | 0.09 | | 0.22 | | -0.230 | |
| Teacher | SSIS | Less Problem Bhav | 0.17 | + | 0.36 | ** | -0.259 | |
| Teacher | SSIS | Self-Control | 0.10 | | 0.16 | | -0.043 | |
| Teacher | SSIS | Engagement | 0.16 | | 0.15 | | 0.180 | |
| Teacher | SSIS-CBRS | Self-Regulation | 0.26 | * | 0.33 | ** | 0.087 | |
| Teacher | STRS | Closeness | 0.15 | | 0.27 | * | -0.120 | |
| Teacher | STRS | Less Conflict | 0.16 | + | 0.24 | * | 0.019 | |
| | | | | | | | | |
| Child | DAS | Naming Vocab | 0.12 | | 0.27 | ** | -0.249 | |
| Child | DAS | Verbal Comp | 0.15 | | 0.04 | | 0.412 | |
| Child | EMT-ACES | Emotion Reg | 0.02 | | 0.02 | | 0.036 | |
| Child | HTKS | Executive Function | 0.25 | ** | 0.32 | ** | 0.070 | |
| Child | NEPSY | Theory of Mind | -0.10 | | 0.03 | | -0.404 | * |
| Child | WJ | Applied Problems | 0.08 | | -0.12 | | 0.027 | |
| Child | WJ | Letter-Word ID | 0.25 | ** | 0.26 | ** | 0.247 | |
| | | | | | | | | |
| ** $p < .05$ | | | | | Wrong Sign | | | |
| * $p < .10$ | | | | | | | | |
| + $p < .20$ | | | | | | | | |

*Table 14. Comparing ITT and TOT for Two Years of Participation, Cohorts 1 and 2*

| | | | TOT Results | | ITT Results | |
|---|---|---|---|---|---|---|
| **Source** | **Measure** | **Outcome** | **Coefficient** | **Significance** | **Coefficient** | **Significance** |
| Teacher | DESSA | Decision-Making | 0.62 | * | 0.24 | * |
| Teacher | DESSA | Relationship Skills | 0.59 | + | 0.23 | * |
| Teacher | DESSA | Self-Awareness | 0.76 | ** | 0.30 | ** |
| Teacher | DESSA | Self-Management | 0.65 | * | 0.26 | * |
| Teacher | DESSA | Social Awareness | 0.36 | | 0.15 | |
| Teacher | DESSA | Social Skills Composite | 0.64 | * | 0.25 | * |
| Teacher | SSIS | Less Bullying | 0.78 | * | 0.31 | ** |
| Teacher | SSIS | Less Externalizing | 0.76 | * | 0.30 | ** |
| Teacher | SSIS | Less Hyperactivity | 1.02 | ** | 0.40 | ** |
| Teacher | SSIS | Less Internalizing | 0.57 | + | 0.22 | + |
| Teacher | SSIS | Less Problem Behaviors | 0.92 | ** | 0.36 | ** |
| Teacher | SSIS | Self-Control | 0.42 | | 0.16 | |
| Teacher | SSIS | Engagement | 0.40 | | 0.15 | |
| Teacher | SSIS- | Self-Regulation | 0.85 | ** | 0.33 | ** |
| Teacher | STRS | Closeness | 0.69 | + | 0.27 | * |
| Teacher | STRS | Less Conflict | 0.639 | * | 0.24 | * |
| | | | | | | |
| Child | DAS | Naming Vocabulary | 0.58 | * | 0.27 | ** |
| Child | DAS | Verbal Comprehension | 0.10 | | 0.04 | |
| Child | EMT- | Emotion Recognition | 0.04 | | 0.02 | |
| Child | HTKS | Executive Function | 0.73 | ** | 0.32 | ** |
| Child | NEPSY | Theory of Mind | 0.06 | | 0.03 | |
| Child | WJ | Applied Problems | -0.28 | | -0.12 | |
| Child | WJ | Letter-Word ID | 0.62 | * | 0.26 | ** |
| | | | | | | |
| ** $p < .05$ | | | Wrong Sign | | | |
| * $p < .10$ | | | | | | |
| + $p < .20$ | | | | | | |

*Table 15. Comparing ITT Results for One and Two Years of Participation*

| | | | One Year | | Two Years | |
|---|---|---|---|---|---|---|
| **Source** | **Measure** | **Outcome** | **Coefficient** | **Significance** | **Coefficient** | **Significance** |
| Teacher | DESSA | Decision-Making | -0.01 | | 0.24 | * |
| Teacher | DESSA | Relationship Skills | 0.07 | | 0.23 | * |
| Teacher | DESSA | Self-Awareness | 0.01 | | 0.30 | ** |
| Teacher | DESSA | Self-Management | 0.09 | | 0.26 | * |
| Teacher | DESSA | Social Awareness | 0.04 | | 0.15 | |
| Teacher | DESSA | Social Skills Composite | 0.04 | | 0.25 | * |
| Teacher | SSIS | Less Bullying | 0.01 | | 0.31 | ** |
| Teacher | SSIS | Less Externalizing | 0.02 | | 0.30 | ** |
| Teacher | SSIS | Less Hyperactivity | 0.03 | | 0.40 | ** |
| Teacher | SSIS | Less Internalizing | 0.06 | | 0.22 | + |
| Teacher | SSIS | Less Problem Behaviors | 0.03 | | 0.36 | ** |
| Teacher | SSIS | Self-Control | -0.05 | | 0.16 | |
| Teacher | SSIS | Engagement | 0.05 | | 0.15 | |
| Teacher | SSIS-CBRS | Self-Regulation | 0.09 | | 0.33 | ** |
| Teacher | STRS | Closeness | -0.15 | + | 0.27 | * |
| Teacher | STRS | Less Conflict | -0.03 | | 0.24 | * |
| | | | | | | |
| Child | DAS | Naming Vocabulary | 0.04 | | 0.27 | ** |
| Child | DAS | Verbal Comprehension | 0.27 | ** | 0.04 | |
| Child | EMT-ACES | Emotion Recognition | 0.06 | | 0.02 | |
| Child | HTKS | Executive Function | 0.18 | * | 0.32 | ** |
| Child | NEPSY | Theory of Mind | -0.11 | | 0.03 | |
| Child | WJ | Applied Problems | -0.11 | | -0.12 | |
| Child | WJ | Letter-Word ID | 0.12 | + | 0.26 | ** |
| | | | | | | |
| ** $p < .05$ | | | Wrong | | | |
| * $p < .10$ | | | | | | |
| + $p < .20$ | | | | | | |

*Table 16. Comparing ITT Effects for One Year of Participation for Different Cohorts*

| | | | All Three Cohorts | | Cohorts 1 and 2 | | Cohort 3 | |
|---|---|---|---|---|---|---|---|---|
| Source | Measure | Outcome | Effect | Stat-Sig | Effect | Stat-Sig | Effect | Stat-Sig |
| Teacher | DESSA | Decision-Making | -0.01 | | -0.05 | | 0.09 | |
| Teacher | DESSA | Relationship Skills | 0.07 | | -0.04 | | 0.32 | * |
| Teacher | DESSA | Self-Awareness | 0.01 | | -0.01 | | 0.07 | |
| Teacher | DESSA | Self-Management | 0.09 | | 0.07 | | 0.14 | |
| Teacher | DESSA | Social Awareness | 0.04 | | -0.05 | | 0.23 | + |
| Teacher | DESSA | Social Skills Comp. | 0.04 | | -0.02 | | 0.18 | |
| Teacher | SSIS | Less Bullying | 0.01 | | 0.00 | | 0.03 | |
| Teacher | SSIS | Less Externalizing | 0.02 | | 0.02 | | 0.02 | |
| Teacher | SSIS | Less Hyperactivity | 0.03 | | 0.06 | | -0.02 | |
| Teacher | SSIS | Less Internalizing | 0.06 | | 0.01 | | 0.18 | |
| Teacher | SSIS | Less Problem Bhav | 0.03 | | 0.02 | | 0.05 | |
| Teacher | SSIS | Self-Control | -0.05 | | -0.08 | | 0.02 | |
| Teacher | SSIS | Engagement | 0.05 | | -0.01 | | 0.17 | |
| Teacher | SSIS-CBRS | Self-Regulation | 0.09 | | 0.17 | + | -0.07 | |
| Teacher | STRS | Closeness | -0.15 | + | -0.12 | | -0.21 | |
| Teacher | STRS | Less Conflict | -0.03 | | 0.02 | | -0.14 | |
| | | | | | | | | |
| Child | DAS | Naming Vocab | 0.04 | | 0.06 | | -0.02 | |
| Child | DAS | Verbal Comp | 0.27 | ** | 0.28 | * | 0.24 | + |
| Child | EMT-ACES | Emotion Recog | 0.06 | | 0.05 | | 0.10 | |
| Child | HTKS | Executive Function | 0.18 | * | 0.16 | + | 0.21 | |
| Child | NEPSY | Theory of Mind | -0.11 | | -0.18 | + | 0.07 | |
| Child | WJ | Applied Problems | -0.11 | | -0.13 | + | -0.06 | |
| Child | WJ | Letter-Word ID | 0.12 | + | 0.14 | + | 0.08 | |
| | | | | | | | | |
| ** $p < .05$ | | | Wrong Sign | | | | | |
| * $p < .10$ | | | | | | | | |
| + $p < .20$ | | | | | | | | |

### *Results for Parent Outcome Measures*

The parent measures included similar scales assessing children's behavior and social-emotional skills that were reported by teachers. These parent measures for both one- and two-year impacts showed a pattern of null ITT effects across all measures. That is, parents with children who were offered admission to WINGS did not report a changing pattern of behavioral or social-emotional skills compared with parents whose children did not receive offers of admission to WINGS. Therefore, the reports of classroom behavior and social-emotional skills by teachers show a pattern across measures of significant WINGS impacts, while similar parent reports show no WINGS impacts.

However, parents were also asked to provide measures of overall stress, financial stress, and child-parent stress. These measures showed that overall stress levels were higher for treatment parents than control parents. The overall stress level showed statistically significance differences ($p < .05$). The levels of financial stress and child-parent stress showed higher stress for WINGS eligible parents, but no statistically significant differences.

## Discussion

### *Interpretation of Main Results*

The divergent results for cohort 3 would be predicted by three factors that differ between cohorts 1 and 2 and cohort 3. Cohort 3 had substantially lower compliance rates, higher attrition and differential rates, and impaired program quality compared with cohorts 1 and 2. Cohorts 1 and 2 had compliance rates of 39 percent compared to 15 percent for cohort 3. This lower compliance rate in cohort 3 was caused by the WINGS program being closed for one cohort 3 school and to a district-mandated additional after-school program implemented at two schools that led to transfers and lower compliance for WINGS. In addition, the new district-mandated program caused significant disruptions in access to facilities at one school that significantly impaired the quality of the program, and the higher overall and differential attrition rates for cohort 3 measures did not meet WWC 3 liberal standards for teacher and parent data (see Table 9). The failure to meet WWC liberal standards suggest substantial risk for bias.

These factors would predict results for cohort 3 that would approach null results and have a much higher threat for bias than for cohorts 1 and 2 measures. In some ways, cohort 3 simulated a natural experiment that tested whether the evaluation design and methodology would change in response to significant changes in compliance, program quality, and attrition. The results suggest that the evaluation design and methodology registered these impacts, and that the results from cohorts 1 and 2 represent the effects of WINGS when compliance and program quality is much higher and attrition is much lower.

TOT estimates for children actually attending WINGS show effects for two years of participation between 0.6 to 1.0, indicating that typical gains in classroom behavior, social-emotional skills, executive function, and reading measures would be 25 to 35 percentile points. This size of effects is sufficient to substantially narrow gaps in executive function, social-emotional skills, and reading skills for disadvantaged children.

The dramatic gains that occurred after two years of dosage versus one year of dosage suggests that building social-emotional skills may require higher levels of dosage than is commonly used in research to measure their effects. That is, previous research may underestimate the potential effectiveness of programs and the power of social-emotional skills to affect later outcomes because of limitations in dosage. Future research should focus on interventions that have at least 2 years of intervention.

Our evidence would suggest that WINGS effects may grow with more dosage, for older children, and may have delayed effects. Thus, the current WINGS evaluation that includes only K-1 children with up to two years of dosage may underestimate the full impact of the WINGS K-5 program. For children who remain in schools that have the WINGS program, it is possible to attend up to six years and effects might be expected to grow much larger.

Our data suggests two factors that make it difficult for children to receive higher levels of dosage. These factors are the high rate of migration of low-income families within school districts to schools not having a WINGS program, and the turbulence and stress present in these families from frequent changes in jobs, income, health, and relationships that prevent regular WINGS attendance. A district-wide WINGS program could achieve much higher dosage and effects for at-risk children.

The transformative impacts for those attending the after-school program would also likely increase achievement in classrooms for all students—even those not attending WINGS—due to the improved classroom behavior of WINGS children.

Such notable impacts arising from an after-school program alone, while surprising at one level, may simply replicate some of the expanded and more diverse opportunities and learning environments that are present in advantaged families and their schools that create the social-emotional skill differences for at-risk children (McCombs, et al, 2017).

### *Interpretation of Parent Results*

The absence of similar effects from ratings of behavior and social-emotional skills from parents and teachers suggests four hypotheses. The first hypothesis is that the child's behavior and social-emotional skills have improved in the classroom but not at home. Our data shows that more than 65 percent of control children who arrived home after school were supervised by a caregiver and others attended alternative after-school care programs. Home behavior may be different for children attending WINGS who arrive

home much later and are tired after a long day at school compared with children who arrive home after school and spend time with caregivers. A measure of overall stress included on the parent survey shows statistically significant higher levels of stress for treatment group parents. This increased stress may result from the challenges of having a child attending WINGS and the associated challenges of late home arrival of a tired child. Parents of WINGS children, other things being equal, are more stressed, and our results suggest that stressed parents–other things being equal–rate children's behavior lower.

The second hypothesis about parent results is that parent ratings are less objective than teacher ratings and can be biased by the lack of a peer control group for comparisons that teachers have, but parents do not. Classroom behavior during the day for an entire school year provides an environment where a child's behavior can be more objectively compared with peers.

The third hypothesis is that the classroom environment is also much more similar to and places similar demands on children as the WINGS environment, so new learned behavior during WINGS may be easier to transfer to the classroom than to the home environment. Finally, teachers have much higher education levels (typically a college degree) than the parents in our sample (typically a high school degree or less). The survey outcome measures could be cognitively challenging in terms of length of the survey as well as an understanding of the developmental language and measures, and teachers may be able to provide more reliable assessments.

### *Interpretation of Differences in Results for One Year and Two Years of Participation*

The results also suggest that attending WINGS only in kindergarten does not produce these positive impacts found after two years. This pattern of impacts has three possible interpretations. One interpretation is that WINGS effects occur only if children attend for two years; a single year of attendance provides insufficient dosage for significant impacts. A second interpretation is that the WINGS program is not effective for kindergarten students, but the program is more effective for first grade students. A third interpretation is that the current two-year results partly reflect both longer-term, delayed effects from kindergarten and short-term impacts from first grade. If effects are delayed, then effects will increase in the longer term and current estimates will underestimate long-term effects.

Each of these interpretations would suggest that WINGS impacts would increase with more dosage, and/or with older children and/or if long-term impacts are measured. The WINGS program serves children through fifth grade, and the current evaluation does not include older children or children who receive more than two years of dosage or measure long-term results. Thus, the current evaluation that includes only younger

children with limited dosage and measures short-term impacts may underestimate the full impact of the WINGS K-5 program.

WINGS is currently configured with only a few schools in a school district offering WINGS. For children who remain in schools that have WINGS, it is possible to attend up to six years since the program serves children through fifth grade. However, our data suggests two factors that make it difficult for children to receive higher levels of dosage. These factors are the high rate of migration of low-income families within school districts to schools not having a WINGS program, and the turbulence and stress present in these families from more frequent changes in jobs, income, health, and relationships that prevent regular attendance at WINGS. For instance, only four in ten children who were given the opportunity to start WINGS in kindergarten attained two years of treatment. Parents of WINGS children express strong approval and support for the program, but regular attendance requires remaining near schools offering WINGS and a level of family stability and commitment that some families find difficult to attain.

One direction for increasing the number of children who can have the opportunity to receive two or more years of WINGS from K-5 is to undertake a district-level demonstration project that makes WINGS available in all schools in an urban school district. Doing so might substantially increase the opportunity for children to receive more than two years of dosage, and increase dosage for older students. Such a project would allow students who move within a school district to maintain WINGS attendance, and allow students who drop out of WINGS for a year or more to return and receive additional dosage.

There are school districts throughout the nation who are experimenting with extended-day programs, but they often use the extended time for more direct instruction. However, there is strong experimental evidence that more direct instruction in reading and math in extended-day programs does not lift achievement (Black, et al, 2009; McCombs et al, 2017). However, the current evaluation results for WINGS appear strong enough to support a larger demonstration project in a school district that would allow experimental measurements of the long-term impacts for older children and for children who receive more than two years of dosage.

### *Comparison of Results to Previous Research*

This evaluation occupies a fairly unique niche in the literature on social-emotional programs. The WINGS social-emotional program was an after-school program for low-income, urban children that developed and evolved with research input over more than 10 years, and it was implemented, managed, and well-funded by a nonprofit organization. WINGS incorporated components that included (1) high participation rates, (2) a multi-year program, (3) a focus on social/emotional skills, (4) four “SAFE” characteristics (*sequenced, active, focused, and explicit*), and (5) a focus on five key

SEL competencies: *self-awareness, self-management, responsible decision-making, social awareness,* and *relationship skills.* The WINGS program developed an excellent reputation in the Charleston, SC community and was awarded a $4 million, highly competitive Social Innovation Fund grant (4 programs for children were awarded grants out of 83 applying) to expand to other locations.

Most social-emotional programs that are evaluated are developed by researchers and implemented for up to a year, but they do not have the opportunity to evolve and improve over longer time periods. Evaluating existing programs presents unique challenges and opportunities in contrast to researcher-designed programs. Perhaps the main limitation of researcher-designed programs is that the evaluation is done in a protective environment. Even if such evaluations show impacts, the issue of scaling up and becoming viable in real-world environments is often problematic. Existing programs are fully implemented and evolved and become viable in response to the needs of their clients. Such programs are often more easily scalable.

Comparing our results to results from previous research is not straightforward for several reasons. Almost all previous research on social-emotional programs either during the school day or in after-school programs were for programs with dosage less than a year, for older children, for researcher-designed programs, or for a demographically more diverse population. Our comprehensive evaluation also incorporated substantially more outcomes measures than previous studies and incorporated measures from three sources: teachers, child testing, and parents.

Another complicating factor for comparing results is that previous evaluations of social-emotional programs implemented either during the school day or in after-school programs generally report only ITT results. ITT results can be compared across programs only when compliance rates and attrition rates are equivalent across studies. However, school-based programs completed in a single school year will generally have much lower levels of attrition and non-compliance compared with out-of-school programs. When compliance and attrition rates differ across evaluations, comparing TOT estimates provides a better comparison since TOT estimates attempt to estimate effects on children who actually receive treatment.

Meta-analysis of ITT effects for social-emotional programs, whether in school or out of school including primarily older children for programs of less than a year, tend to fall in the small-to-moderate range. For our sample of kindergarten children, a single year of dosage showed only null to weak effects perhaps reflecting that one year of dosage is insufficient or that after-school programs that extend the school day by three hours does not work well for very young kindergarten children. Our results suggest that effects were much stronger for first grade students who received two years of dosage, and our comparable TOT effects were in the moderate to large range (0.6 to 1.0 SD) compared

with small-to-moderate ITT effects for previous studies. These results might suggest that previous evaluations would have gotten stronger results if continued for two years.

A study by Blair and Raver (2014) provides an interesting comparison because of its focus on kindergarten children that included a low-income sample and was focused on improving executive function, self-regulation, and academic skills. It differed from WINGS in being for a single year and an in-school intervention, but included some similar child testing measures. The two interventions also shared a somewhat common approach to boosting children's skills.

Blair and Raver (2014) designed an intervention that was a curriculum and pedagogical-based intervention called Tools of the Mind. The intervention has a "coherent focus on executive function as a primary mechanism through which children make progress and develops teachers to learn how to organize and manage instruction so that children build self-regulation skills through purposeful interactions with classmates." Teachers also "engage in dynamic assessment of children's development in core areas and provide individualized, differentiated scaffolding depending on how a child performs a specific skill."

In many ways the underlying approach used in Tools of the Mind and WINGS is similar in the extended focus throughout either the school day or the three-hour after-school period on the child's activities and providing individualized feedback and learning opportunities in interaction with other children to improve executive function and social-emotional skills. In Tools of the Mind, the focus incorporates academic learning activities rather than typical after-school activities. But both interventions attempt to use many activities that encourage social peer interactions and use play activities as opportunities for learning. Both interventions rely on a strategy of using teachers or WINGS leaders to infuse an activity-planned play or academic environment with opportunities for learning and social interactions that can be used to provide individualized feedback to children on behavior directed toward improving their executive functions and social-emotional skills. WINGS might be seen as a "Tools of the Mind" type of program for an after-school intervention.

Blair and Raver (2014) found after one year of intervention significant ($p < .05$) or marginally significant ($p < .10$) ITT effects from three of six measures of executive function and self-regulation in the low-income sample ranging from 0.3 to 0.6, and for math and two of three reading measures with effects between 0.13 and 0.5. Although the intervention did not continue for a second year, follow-up measures at two years for the academic measures generally showed continued or somewhat smaller impacts.

In contrast, WINGS effects at the end of one year showed TOT estimated comparable effects of 0.25 ($p < .10$) for a measure of executive function and 0.37 ($p < .05$) for verbal comprehension, but no effects on reading or math academic measures or teacher-

reported classroom behavior measures. However, after two years of WINGS, executive function showed TOT effects of 0.73 ($p < .05$) and two measures of reading showed TOT effects of 0.58 to 0.73 ($p < .10$), and 16 of 23 teacher-reported classroom measures related to executive function, self-regulation, and social-emotional skills showed significant or marginally significant comparable TOT effects between 0.6 to 1.0. WINGS did not show any math results.

The positive effects measured in both studies suggest that the common elements and approach—the theory of action—that underlie these interactions might be working in both an in-school and an after-school program. Our results suggest that Blair and Raver (2014) might have obtained much stronger results had the intervention lasted for two years.

### *Limitations*

It is important for researchers and policy-makers to recognize the limitations of this research as well as its strengths. Limitations are factors that could affect the internal or external validity of the reported results. The factors that can limit the internal validity of our study include: (1) selectivity and low reliability in reported measures, (2) unlucky randomization, (3) sample attrition and missing data, (4) non-compliance, (5) adjustments for multiple outcome measures, (6) poor fidelity of implementation, and (7) flawed analysis. The external validity of the study is limited by the homogeneity of the sample to low-income, urban, at-risk children and to the uncertainty of scaling the program to other schools, school districts, and urban areas.

### *Selectivity and Reliability in Reported Measures*

The design of our study accounted for the high level of funding and tried to design a randomized controlled trial with mixed methods that incorporated a wider variety of measures from parents, teachers, and child administration than needed for evaluation, but could also be used to provide information on causative factors that might explain results (Grissmer, 2016). Also implicit in this evaluation was the realization that an unusual opportunity was present to better understand the factors that shaped the development and outcomes of at-risk urban children from kindergarten entry to end of first grade. This strategy led us to incorporate extensive data collection from parents, teachers, and child-administered tests and include a number of developmental measures that might explain changes in social-emotional skills (building block) skills. We also incorporated “experimental” measures that were in earlier stages of development. Finally, the skill levels of these children significantly lagged behind the skills of the nationally representative children that formed the basis of the appropriate age ranges for many measures. A few of the measures chosen simply did not work well as administered or displayed significant floor effects that resulted in their elimination from the analysis.

We included 38 measures from teachers, child testing, or parents in our final analysis. We eliminated the following measures from the analysis for the listed reasons: Differential Ability Scales-II (DAS) working memory (very high level of floor effects), WALLY (difficulty administering and failed to show significant correlations between time 1 and 2), an experimental executive function measure (measure in development with some issues in administration that we included only in final two cohorts, positive insignificant results), Woodcock-Johnson general knowledge (not part of original theory of action, showed positive, insignificant results), and delay of gratification (complex question with six choices that has negative correlation with other behavioral and building block measures and negative, but insignificant outcome results).

Finally, measures can be unreliable if they are not collected at similar points in time and in similar administrative conditions. Our child testing data and parent data at each data collection point could have been collected at different times either in the summer preceding kindergarten, first- or second-grade entry, or during the first two to three months of the school year. We incorporate covariates to control for the days between tests as well as whether the tests were administered in summertime or during the school year. A sensitivity analysis shows little systematic impact on results from including or eliminating these variables for outcomes associated with child testing or parent measures. It is only our child testing and parent variables that include the timing covariates. Teacher-reported outcomes that show similarly strong impacts as child testing outcomes are collected at the beginning and end of each school year and do not have this source of variance.

### *Unlucky Randomization*

The possibility of unlucky randomization impacting results is generally tested through statistical tests of pre-test variables collected close to randomization. Such tests are stronger as the number of pre-test variables increases. This study has an unusually large number of pre-test variables from child testing, parents, and teachers to test whether the overall sample had significant differences in test and control groups on each of these variables. The statistical tests for differences between treatment and control children show only a very small proportion that show significant differences. The primary exception is four developmental variables (theory of mind, verbal comprehension, executive function, and emotional recognition), a demographic variable (number of children in the home) that all show differences in the same direction that are significant at between the $p < .01$ to $p < .20$ level. That is, all are in the direction of either increased parental stress or lower developmental characteristics for treatment children. We have also incorporated these variables as covariates (along with parent education, child age, and mother's age at child's birth) to adjust for randomization differences. Inclusion of these variables as covariates predictably strengthens somewhat the effect size and significance of the results.

### *Sample Attrition and Missing Data*

Study attrition can threaten the internal validity of results if those who stop providing data have different characteristics than those who supply data, and these characteristics are correlated with outcome measures. Particularly problematic is attrition levels that are different between test and control groups (differential attrition). Our data does not show significant levels of overall and differential attrition for cohorts 1 and 2, but does show unacceptable levels for cohort 3. The What Works Clearinghouse (WWC), which serves to assess the internal validity of studies, publishes guidelines for assessing the levels of overall attrition and differential attrition. Our levels of attrition across almost all measures in cohorts 1 and 2 show overall and differential attrition that meets the conservative WWC guidelines. However, cohort 3 attrition characteristics for teacher and parent data do not meet even liberal WWC criteria for overall and differential attrition.

We also did not find statistically significant differences in early pre-test characteristics of those who stay in the study and leave the study. A primary reason for leaving the study was that participants moved out of the WINGS schools to other schools in the school district. Although we tracked many of these people and continued collecting data, many who moved stopped supplying data to the study. However, there were no significant differences in relocation between test and control group characteristics. Overall results suggest that attrition due to relocation was not correlated with winning or losing the lottery.

The quality of missing data imputation for each individual depends on the amount and quality of data available for that individual and for all individuals in the sample. We collected initial pre-test data from about 88 percent of parents, 89 percent of teachers, and 95 percent of children were given initial batteries of tests. Only 2 to 3 percent of children were missing data entirely. The remaining missing data came mainly from missing a parent or teacher survey or child testing at the end of year 1 or year 2. But in general, there is a fairly rich set of data for the individual and/or for similar students that can be used to impute missing data. A comparison of results using observations with complete data to results using imputed data showed the expected patterns with similar or slightly weaker effect coefficients, but stronger statistical significance for imputed results.

### *Non-Compliance*

Non-compliance occurs when children randomized into treatment groups do not attend WINGS at all or do not meet participation criteria for WINGS (no-shows) and/or children randomized into control groups actually do attend WINGS and meet participation criteria (crossovers). In our study, non-compliance from no-shows was about 32 percent of the one-year treatment sample and 61 percent of the two-year sample. The primary reason for the high level of no-shows was parents relocating out of the schools with the WINGS program to other schools in the same district. Our data suggests that the level of relocation was not different among treatment and control children, nor would one expect

a relocation decision—a common occurrence for low-income parents in urban areas—to be linked to winning or losing the WINGS lottery.

Incorporating the effects of non-compliance into randomized controlled trial estimation is done by changing the interpretation of ITT results and making additional estimates of TOT results. The ITT results are estimated in the same way regardless of the level of non-compliance, and these estimates measure effect differences between all children in treatment versus control groups, regardless of whether children complied. However, the impacts carry a caveat that the results measure effects across both children who attended as well as those not attending, and, if effects are positive, presumably the effects would have been larger if all treatment children had attended the program. The TOT estimates account for non-compliance (of both types) and make estimates for impacts for those actually attending the program. However, these estimates cannot eliminate the potential for bias that would occur if the characteristics of those who attended are different from those not attending, and these differences are correlated to outcome variables. In our case, we found no differences in characteristics between attenders and non-attenders.

### *Adjustments for Multiple Outcome Measures*

No consensus exists about statistical adjustments for multiple comparisons when virtually all outcome measures are correlated and for large sets of outcome measures. Any consideration of adjustments should recognize that while such adjustments decrease the chances of a false positive (an ineffective program will be labeled as effective), they increase the chances of a false negative (an effective program will be labeled ineffective). In the evolutionary research stages of developing improved social-emotional measures and programs, searching for patterns linked to effective programs may be more important than guarding against misallocating future resources to an ineffective program. In our case, we do not recommend widespread expansion of WINGS that would require significant resources and consideration of adjusting for multiple outcome measures. However, the current WINGS results, in our view, are strong enough to suggest it should be expanded to all schools at a school district level. This recommendation would allow measurements for older children and children receiving more than two years of dosage that our results would suggest would have stronger effects. It would also allow for a replication of the current results. Thus, the current results are certainly strong enough to implement the program in an environment that reaches more children, especially older children, who will receive more dosage and likely produce stronger effects.

### *Fidelity of Implementation*

Fidelity of implementation measures can be useful when there are a large number of separate locations with sufficient sample at each location that allow results to be

statistically compared across locations. In our study, we did not have the sample size of number of locations that allow such an analysis. In this situation, fidelity data can be useful in interpreting results and identifying factors that might explain differences across locations. In our study, there were dramatic differences in fidelity between cohorts 1 and 2 and cohort 3 due to the closure of the program at one cohort 3 school and the limited access to facilities at another school due to a second after-school program that effectively impaired the normal activities of the WINGS program and lowered compliance. The dramatic differences between strong results in cohorts 1 and 2 and null results for cohort 3 suggest that the normally operating WINGS programs in cohort 1 in four schools and cohort 2 in three schools produced strong effects that declined to null effects when 2 of 3 schools in cohort 3 were not able to operate.

We collected several different measures of implementation fidelity primarily for cohorts 1 and 2, in order to assess whether programs were implemented with fidelity across schools and in cohorts 1 and 2. While the programs at different schools in cohorts 1 and 2 encountered some difficult and challenging circumstances, we did not find marked differences in measures across schools or between cohorts 1 and 2. We did run interaction terms by school and cohort and found no consistent differences in effects across schools or between cohorts 1 and 2. The results would suggest that fidelity of implementation was strong enough across schools and cohorts 1 and 2 to produce the strong pattern of impacts found for cohorts 1 and 2, but they were not useful in better understanding differences for one- and two-year participants partly due to the limited sample sizes and small number of schools and cohorts involved.

### *Flawed Analysis*

Flawed analysis often is involved if results cannot be replicated and can occur either due to programming errors or from studies that selectively choose publishing results not directly linked to confirmatory hypotheses or changing and searching among model specifications for positive results. There is no substitute for replication. But studies are arguably more vulnerable to these flaws if the number of outcomes measures is few and data comes from a single set of participants with data collection at a single time period and with smaller samples that lack adequate power to detect larger effects. Our complex data set from parents, teachers, and child testing with 38 outcome measures shows distinctive patterns of results that would be hard to “game.” While programming errors are possible, these errors tend to be identified during the analysis process when results dramatically shift (or do not run at all). Subtle programming errors are rare.

### *Lessons Learned*

One of the major problems when evaluating existing programs for children that raise public and private funding to support their program is the tension that exists between researchers’ “objectivity” and the threat that poor results from the evaluation pose to

program funding and existence. Failure to recognize and incorporate this tension into the evaluation process can easily result in premature termination of any evaluation.

Evaluations involving nonprofit programs dependent on nongovernmental funding require development of a strong researcher-clinician partnership. Directors of nonprofit programs for children are in a challenging position because their outside funding from foundations and philanthropists often depends on successful evaluations. These sources of funding often do not have the sophistication required to understand the complexity of evaluating these programs and seek simple "black-white" answers (e.g., Did the evaluation show your program was effective at a $p < .05$ level?). Directors also often do not develop the requisite understanding of research to communicate effectively more nuanced results. But researchers also often have a "black-white" approach to evaluation and often use imprecise language in communicating results.

It is essential in these projects to develop the researcher-clinician partnership that allows both researchers to better understand the programs being evaluated and the concerns of program managers and clinicians to better understand research and interpretation of results. Researchers have a lot to learn from clinicians, and vice-versa. We came into the project with the traditional "hands-off" approach (i.e., let's do an "objective" evaluation and not get involved in the program) to evaluation.

We have determined partly through an adversarial process with WINGS and their able set of research advisors, that this "hands-off" approach was counter-productive and would not have allowed the project to proceed, nor would a fair evaluation have been possible. We recognized that evaluations must broaden their scope to include better understanding the complexity of the lives of program participants, the complexity of managing and improving out-of-school programs to improve children's outcomes, the difficulty of measuring these early developing skills, and the complexity of the analysis required to obtain and interpret results.

We learned that results can be communicated in a more accurate way that better reflects the probabilistic outcomes of randomized controlled trials that program managers can also understand. We learned that evaluations even under ideal conditions can seldom label programs as ineffective unless negative effects are statistically significant. Even null effects have an even chance of program effects being positive. At best, evaluations can measure positive effects, if present, but such effects usually come with important caveats. We also learned that program managers are very interested in results that reflect $p < .20$ and $p < .10$ because these results can point to ways of improving the programs, and may also be harbingers of future effects as measures strengthen and the power of samples increase. We learned that the mixed methods data collected can be used in many ways to suggest improvements in the program.

We also learned that, in our case, program managers can develop a pretty sophisticated understanding of research methods and offer suggestions and interpretations that strengthen the research. In particular, annual meetings of presentation of results allowed WINGS program managers and staff to suggest new, different, and sometimes better interpretations of the results than supplied by researchers. This ongoing sharing and interpretation of results at annual meetings was critical to building the partnership.

### *Future Research*

Research on children's short- and long-term outcomes and the experimental evaluation of school-based programs and out-of-school programs to improve outcomes are undergoing rapid evolution and development. Historically, most of this research and evaluation was focused on the impact of schools and school-based interventions that have reading and/or math achievement as primary outcomes. School-based interventions using achievement as outcomes provide an advantage in evaluation because the measures are highly reliable and replicable, non-compliance and missing data are usually not problems, statistical power is high from large samples, and outcomes measures are few. In this evaluation environment, the standard ITT and TOT methods of evaluation using statistical significance levels of $p < .05$ are warranted. The best studies also incorporate multiple methods so that causative mechanisms can potentially be identified, and assessments can be done to determine whether some children benefit more than others and whether there are ways of improving the intervention through improved fidelity.

However, research is increasingly suggesting that children's long-term outcomes are predicted as much or more by developmental skills learned outside of direct school instruction than by skills learned in direct school instruction (McCombs et al., 2017). These skills include self-regulation, executive function, social-emotional, visuo-spatial, and early comprehension (Grissmer et al., 2010). These early developmental skills have less reliable measures, require a larger set of outcome measures to capture their effects, and need more complex interventions for their improvement because they are learned largely outside schools. These interventions focused on activities outside of schools make large samples less accessible and have much higher levels of non-compliance and missing data that make adequate statistical power more difficult.

The measurement of these early developmental skills is a work in progress, and the quality and reliability of these measurements have not approached those of the most commonly used measures associated with achievement. Part of the problem is that any measure, achievement or otherwise, made with younger children have less quality and reproducibility. We should not expect at this stage of development to have the same kinds of results that would be produced using achievement measures because achievement measures are narrower and better developed—whereas these skills are displayed in a much wider set of behaviors that are more difficult to measure and less

reproducible. These measures will improve over time allowing better measures, but at this stage of development, the criteria should not be the most stringent levels of statistical significance, that is, $p < .05$. In the long run when measures and interventions improve and samples have more power, imposing a statistical significance standard seems reasonable, but the purpose of an evaluation during the evolutionary period of improving measures and programs with weaker samples should incorporate a different set of objectives.

Evaluation methodology during the evolving period when measures and interventions are improving should incorporate the following elements:

- The standard randomized controlled trial ITT and TOT analysis should be used, which includes methods of incorporating missing data with particular emphasis on the TOT effect sizes.
- Lower levels of statistical significance should be reported on measures.
- Assessing ways of improving measures should be an important objective by including a wider range of exploratory outcome measures than is typical in randomized controlled trials.
- Assessing ways of improving the effectiveness of the intervention is also an important objective to undertake in addition to standard evaluation analysis.
- Interpretations should focus on the internal consistency and predictability from the theory of action of the broad patterns of results across measures rather than entirely on consideration of statistics across individual measures.

Perhaps most importantly, the collection of mixed methods data seems critical for research involving the building of these early developmental skills in the out-of-school context. Randomized controlled trials involving the building of early developing skills learned largely outside school involving low-income children are particularly challenging during the early stages of research and program development. Investment in the collection of mixed methods data may be critical for accomplishing the broader set of objectives outlined above.

Much of the long-term value of this project may still be ahead of us and lie not only with the “evaluation results,” but in this future research with the data collected. Most of our mixed methods data has yet to be fully incorporated into the analysis and interpretation of results, and stronger recommendations for improving future research and the effectiveness of the WINGS program will emerge from continuing research with this data. The unique data collected in this project on the lives of low-income urban families, the development of their children in the K-1 period, and the impact of schools and programs on their lives can support years of research that can: (1) assess the sensitivity of current results to alternate assumptions, (2) assess the relationships among

measures of early developing cognitive skills and academic outcomes, (3) improve current measures of early developing skills, (4) improve the WINGS program, and (5) identify social and educational policies that can improve outcomes for low-income children.

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