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A Strengths-Based, Culturally Responsive Family Intervention Improves Latino Kindergarteners' Vocabulary and Approaches to Learning

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Abstract

Food routines play a special role in Latino families. Using a cluster randomized trial with 248 children (*M* age = 67 months) from 13 schools, this study investigated the impact of a four-week family program designed to capitalize on food routines in improving Latino kindergarteners' outcomes in the U.S. There were moderate-to-large impacts on child vocabulary (especially food-related) at end-of-treatment and the five-month follow-up, and suggestive evidence of moderate impacts on approaches to learning (including approaches to learning math) and executive function at the five-month follow-up. There were no statistically significant impacts on children's math or literacy skills. A strengths-based, culturally responsive family intervention that is integrated into Latino family life can improve critical skills needed to succeed in school.

Keywords: Latino, intervention, strengths-based, family, culturally responsive

Word count: 120

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Socioeconomic (SES) and racial/ethnic disparities in academic achievement are evident even before children begin formal schooling and are exacerbated as children move through school (Reardon & Portilla, 2016). Variability in the home learning environment mediates the relation between SES/ethnicity and early academic development (e.g., Hoff, 2003). Accordingly, many preventive interventions aiming at reducing early academic disparities have targeted families (Manz et al., 2010). Often, such family interventions are developed from a deficit approach and focus on the knowledge, skills and resources families are "seemingly lacking" (Cabrera et al., 2012; Melzi et al., 2019). However, by taking this deficit approach, not only may we be disempowering these families and eroding their social and cultural competence, but we may also be missing the opportunity to build an accurate and more complete knowledge base of how children develop academic skills and the critical role that sociocultural ecologies play (Garcia-Coll et al., 1996).

In recognition of such limitations in existing interventions, a growing number of researchers have called for innovative interventions that incorporate strengths-based and culturally responsive supports to families, particularly for those families living in poverty and experiencing marginalization (Cabrera et al., 2012; Melzi et al., 2019). A strengths-based, culturally responsive approach adopts a resilience perspective and emphasizes the ecocultural assets that protect (reduce risk) and promote positive outcomes (Perez-Brena et al., 2018). From an anti-racist framework, grounding research-based interventions in ecocultural assets is fundamental to disrupting deficit-based approaches of racialized children (Kendi, 2019).

However, there is a paucity of rigorous evaluations of strengths-based and culturally responsive interventions, particularly in Latino[1] communities. We are aware of only one RCT of a culturally responsive family intervention specifically targeting Latinos – a pilot study with a small sample size of 73 families that aimed to promote preschoolers' language and literacy (Hammer & Sawyer, 2016). When designed and executed well, RCTs are the most rigorous impact design available and are known as the "gold standard" for evaluating the effectiveness of interventions (Hill et al., 2008). Accordingly, as interest grows in strengths-based and culturally responsive interventions, building this evidence base using rigorous designs like RCTs is critical to understanding the potential of this intervention approach to better support families and children and to developing additional such interventions.

In the present study, we help to build this evidence base by experimentally testing the effects of Food for Thought (henceforth FFT), a strengths-based, culturally responsive intervention that builds upon a set of valued practices that are already established in the ecocultural context of the Latino family (i.e., family food routines such as grocery shopping, cooking, and eating together) to improve young Latino children's academic skills (i.e., language, literacy, and math). FFT is a four-week program that capitalizes on family food routines to help Latino parents foster their kindergarten children's academic skills at home. The lead author of the present study developed it based on evidence of studies examining parent-child interactions in food-related activities and discussions with kindergarten teachers and principals in schools serving primarily Latino students and their families. It showed promise in a feasibility study in 2015 (Leyva & Skorb, 2017). In 2018, as the next stage of FFT's development, we launched a

[1] In this study, we use the term Latino because we want to honor the way that families in the study (all of whom immigrated from Latin America) preferred to be identified, the term that families relate to and understand. Although alternative terms have been proposed (e.g., Latinx, Latine, Latin*, Latin@), families told us they have not heard or used such terms, aligning with what others have found (Salinas, 2020; Pew Research Center, 2020).

pre-registered cluster-randomized trial to determine its effects on kindergarteners' vocabulary, literacy, and math skills (our confirmatory outcomes), as well as on approaches to learning and executive function (our exploratory outcomes).

Latino Children's Challenges and Strengths at School Entry

FTT was designed to meet the needs of the growing demographic of Latino children in the U.S., in recognition of both their challenges and strengths at school entry. One in every 4 children in the United States is of Latino heritage; Latino children represent 23% of school-age children, and the population of Latino preschool children is growing faster than any other racial/ethnic group (U.S. Census Bureau, 2019). Thus, it is critical to develop interventions to support Latino kindergarteners' academic skills to help them start school with the best possible chance of achieving and succeeding.

At school entry, Latino children lag behind their peers in domain-specific skills that are foundational for school success, particularly math, literacy, and English language skills, and these disparities persist during the elementary school years (e.g., Magnuson et al., 2016). For example, relative to their peers from other ethnicities, Latino children are 27% less likely to be able to count to twenty, 22% less likely to recognize all the letters of the alphabet, and 11% less likely to be able to write their name at school entry (Murphey et al., 2014). Latino children who speak a language other than English are home are one standard deviation below in English vocabulary scores than native speaking children at kindergarten entry (Hindman & Wasik, 2015).

At the same time, Latino kindergarten children show strengths in two domain-general skills that are foundational for school success, executive function and approaches to learning (henceforth ATL; a set of skills that help children to learn regardless of the academic content, including persistence, motivation, and engagement to learn; McDermott et al., 2014). Latino bilingual children have higher executive function skills (particularly, inhibitory control) than non-Latino monolingual children in preschool (White & Greenfield, 2017) and kindergarten (Carlson & Meltzoff, 2008). Latino kindergarteners have higher ATL than their African American peers and are no different from their White peers (Galindo & Fuller, 2010). High ATL scores in Latino children relate to larger gains in math scores in kindergarten (Galindo & Fuller, 2010) and ATL mediates the relation between being Latino and gains in academic skills during preschool (Bustamante & Hindman, 2020).

Because Latino children show both challenges and strengths at kindergarten entry, it is important to assess the effects of FFT on children's domain-specific (academic) skills (the focus of the program) and domain-general skills (i.e., executive function and ATL). FTT, as we explain below, was designed explicitly to take into account Latino children's opportunities for growth, as well as their strengths and ecocultural assets.

The Rationale for Embedding Supports for Latino Children in Family Food Routines

All families engage in food routines, but such routines play a special role in Latino communities. Food routines are woven into the very fabric of Latino family life because they promote the key values of close relationships and strong family ties. Compared to other ethnicities, Latinos are more likely to believe that a family meal is important and less likely to eat alone, and Latino children are more likely to eat dinner with their family 6-7 nights per week (Murphey et al., 2014). Furthermore, Latino parent-child interactions might be more cognitively complex and engaging in food-related vs. non-food-related activities. In studies of Mexican-descent communities: a) parents were more likely to ask questions requiring active thinking while playing with toy food with their preschoolers than while playing with toy cars (Tenenbaum & Leaper, 1997); b) parents discussed more complex concepts fostering cognitive learning (e.g., explanations, counting) while baking biscuits than while building block models (Eisenberg, 2002); c) parents encouraged their preschoolers' independent responses and follow their interests more while playing with homemade dough than while sharing a picture book (Kermani & Janes, 1999); and d) children exhibited higher levels of active engagement in food- than non-food related activities (Eisenberg, 2002; Kermani & Janes, 1999). A higher sense of efficacy and competence in familiar (food-related) vs. unfamiliar activities (e.g., book sharing) might explain these results. Hence, FFT embeds an intervention promoting academic skills within this existing feature of Latino family life.

Little is known about the effectiveness of interventions capitalizing on family food routines. One RCT study focused on family mealtimes and yielded increases in parents' and preschoolers' decontextualized talk (talk beyond the here and now; a predictor of later academic achievement (Uccelli et al., 2019)) during mealtimes; effects on literacy outcomes were not measured (Leech et al., 2018). Another RCT study focused on family cooking and yielded increases in parents' and preschoolers' math talk during a cooking task but had no effects on children's math outcomes (Vandermaas-Peeler et al., 2012). One RCT of an intervention capitalizing on family routines (e.g., grooming) yielded improvements in parents' engagement in home learning activities and preschoolers' literacy skills (York et al., 2019). This evidence, albeit limited, suggests that family food routines might be a promising venue to develop academic skills, but it leaves open the question of whether the benefits would extend to Latino kindergarteners and their families, given that prior evidence focused on preschool and two of these studies (Leech et al., 2018; Vandermaas-Peeler et al., 2012) targeted middle-income White families and included small sample sizes ($N = 36$, $N = 25$, respectively).

Development of the FFT Intervention

FFT is grounded in Garcia-Coll and colleagues' (1996) integrative model of developmental competencies in minority children. Following this model, FFT incorporates Latino children's *daily experiences* (i.e., participation in family food routines on a regular basis) and considers the racial/ethnic values that *facilitate* the development of their abilities in these contexts (i.e., sense of efficacy and competence in familiar, food-related contexts, which results in more cognitively complex and engaging parent-child interactions), and the social and structural factors that can *hinder* this development (e.g., poverty, racial discrimination, and lack familiarity with the U.S. educational system).

FFT focuses on kindergarten specifically for several reasons. Kindergarten entry traditionally is a noteworthy, rite-of-passage event for U.S. children and their families, one that sparks increased parental investments in children's learning (Weiland et al., 2017). Latino parents appear to be particularly eager to play an active role in their child's learning during the transition to elementary school, perhaps because preschool enrollment rates are lower for Latino children than that other racial/ethnic groups. Accordingly, U.S., kindergarten serves as the first formal educational context for Latino children more so than other groups (Goldenberg et al., 2001; Shapiro, Martin, Weiland, & Unterman, 2019). Hence, kindergarten entry may be particularly good time to reach Latino families – a time when logistically, they are part of the public education systems and when they are particularly motivated and eager to engage in supporting their children's learning.

FFT borrows several techniques identified as effective in changing adult behavior (Michie et al., 2009). It provides information (e.g., strategies fostering academic skills), which increases parents' motivation to change, and accompanies this information with the use of video clips, coaching, and onsite opportunities to practice (Grindal et al., 2016), which helps transform this motivation into behavior change (Michie et al., 2009). Our theory of change is that parents' increased engagement in "purposeful and contextualized activities" within food routines (e.g., making and using grocery lists and recipes) and increased use of strategies supporting language, literacy, and math during such activities (e.g., encouraging children to write and read on their own, asking questions in conversations about past events, counting, adding and subtracting) are the main mechanism through which FFT improves child academic outcomes (see Online Supplementary Material, Table A for a complete list of FFT activities and strategies). Domain-general skills (executive function and ATL) are ecocultural assets that Latino children bring to the school. Although not directly targeted, FFT may have effects on them as well. We also hypothesize that benefits may last beyond the intervention period given the ecological nature of these "purposeful and contextualized activities" (i.e., valued practices embedded in Latino family life). That is, families may choose to keep implementing targeted practices and thus we anticipate either sustained or even larger effects beyond the immediate posttest.

FFT promotes engagement in such 'purposeful' activities and the use of the aforementioned strategies for two reasons. First, parents of preschoolers and kindergarteners spontaneously used the aforementioned literacy-, language-, and math-support strategies and increased use of such parental strategies concurrently and longitudinally related to improvements in children's language, literacy, and math outcomes in Latino (e.g., Caspe, 2009; Leyva, Reese, & Wiser, 2012; Leyva & Skorb, 2017; Leyva, Davis, & Skorb, 2018) and non-Latino populations (e.g., Bindman et al., 2014; Bjorklund et al., 2004; Snow & Beals, 2006). Second, promotion of such activities and strategies has yielded positive impacts on children's outcomes. An RCT of a "purposeful writing/reading activities" intervention yielded larger effects on preschoolers' literacy skills compared to a book-reading intervention (Aram & Biron, 2004) and

RCTs of family-talk interventions yielded positive effects on preschoolers' vocabulary (Peterson et al., 1999).

Previous FTT Evidence and the Current Study

As mentioned, as part of FFT program's development and in preparation for program evaluation, we conducted a feasibility study to assess its implementation (Durlak & DuPre, 2008) in 3 schools in 2015 (*N* = 68; Leyva & Skorb, 2017; 2018). Program reach levels met or exceeded those reported by prior literature (recruitment rates between 20-30% and attendance rates of 50%; e.g., Heinrichs et al., 2005). Children whose parents attended more FFT sessions had larger gains in vocabulary, but not literacy, from pre-test to end-of-treatment post-test (*d* = 0.28). Children with low initial math skills whose parents attended more FFT sessions had larger gains in math skills from pre-test to end-of-treatment post-test (*d* = 0.46). Latino parents reported that

FFT empowered them to support their children's learning and created a sense of community. Dosage levels (i.e., how much the program is ultimately implemented) were not measured in the study but are critical to understanding program effects (Durlak & DuPre, 2008). In an RCT targeting Spanish-speaking children, low dosage (i.e., extent to which teachers used the intervention strategies in the classroom) helped explained the lack of intervention effects on literacy skills (Mendive et al., 2016). Taken together, results indicated that FFT was feasible to implement and yielded promising outcomes. However, by design, the feasibility study was small and correlational, no dosage levels were measured, and no follow-up assessments were included.

As the next phase of FFT's evaluation, we designed and conducted a pre-registered cluster randomized trial evaluating its effectiveness and filling the noted gaps in the feasibility study. Our specific research questions were:

1) Does FFT improve kindergarteners' academic (language, literacy, math) and domain-general (executive function, ATL) skills at end-of-treatment post-test and the 5-month follow-up?

2) What were the FFT program's reach and dosage levels?

We expected that improvements in children's academic skills would be apparent at end-of-treatment post-test and would persist through the 5-month follow-up. Following best practices (Gehlbach & Robinson, 2018), we pre-registered our hypotheses with AsPredicted, a preregistration platform managed by the Credibility Lab at the Wharton School at the University of Pennsylvania. Child academic skills (language, literacy, and math) were our *confirmatory* outcomes. We also investigated improvements in two domain-general *exploratory* outcomes (executive function and ATL). We considered these exploratory because there were not included in the prior FFT feasibility study and FFT did not directly target these skills in its curriculum. However, as stated before, these domain-general skills are foundational to academic skills and are ecocultural assets that Latino children bring to the school, which might facilitate academic development.

We expected levels of program's reach similar to those observed in the feasibility study (Leyva & Skorb, 2017) and prior literature (e.g., Heinrichs et al., 2005). In this study, we use the term 'dosage' to refer to the extent to which parents implemented FFT strategies at home (the "active ingredients" or hypothesized mechanisms of change) during the four-week intervention.

Method

Procedures

Research design. We estimated the impact of the FFT program on children's language, literacy, math, executive function, and ATL skills using a cluster-randomized design. Our cluster was schools; schools were randomly assigned to the FFT intervention condition or a much-reduced intervention (control condition). Our final sample size was 13 schools with 261 students across two kindergarten cohorts (*N* = 129 cohort 1 in 2018, *N* = 132 cohort 2 in 2019). In our original research design, we anticipated 17 schools and 3 cohorts of kindergarten students. However, four schools elected not to participate in the study after initially agreeing (two treatment, two control). The COVID-19 pandemic prevented us from recruiting a third cohort of families and their children and from assessing our second cohort at the 5-month follow-up. We describe the effects of these changes on our study below.

Program characteristics of the FFT and control conditions. The FFT program consisted of four group sessions (one per week) that took place in the fall of the kindergarten year in each treatment school. The Online Supplementary Material (Table A) summarizes FFT topics, activities, and strategies per session. Sessions were scheduled at convenient times for parents and school staff (typically, during school hours) and were delivered by a team of bilingual facilitators (15 in total; 2 Latina group leaders who had a master's degree and 12 bilingual research assistants, 3 of whom were Latinos). Facilitators were trained (i.e., participated in a three-hour training) and coached (i.e., were observed and received feedback during implementation) by a master trainer. At each session, there was one group leader and one to two research assistants present. FFT materials were available in Spanish and English. Sessions were delivered in the parents' preferred language (i.e., Spanish only or Spanish/English) with the majority of the sessions (95%) delivered in Spanish. Each session lasted 90 minutes. During the first 60 minutes, parents watched and discussed video clips featuring Latino parents effectively using FFT strategies with their children. During the last 30 minutes, parents practiced FFT strategies on-site with their children and were coached and received immediate feedback from facilitators. At the end of each session, parents received a hand-out summarizing the strategies. They also received a text reminder every week to practice the FFT strategies at home during the following week. At the beginning of sessions two through four, parents spent the first five to eight minutes sharing their experiences practicing FFT strategies at home.

The control condition was a much-reduced intervention entailing one 90-minute session taking place in the school and focused on encouraging parents to play simple games at home (e.g., puzzles, Legos®) to foster children's learning (inspired by activities used by Healey & Halperin, 2015). The session involved discussing games with parents and on-site practice with children. Parents received a handout but no text reminders. The goal of providing the control group with a much-reduced intervention was twofold: 1) to encourage control school and family participation in the study; and 2) to rule out the possibility that any detected intervention effects were due to an increase in overall parental engagement rather than engagement in academic support during family food routines.

School recruitment and randomization of schools. We drew a purposive sample of schools from one of the largest school districts in the U.S. located in the Southeast. First, we identified 35 Title 1 elementary schools in the school district (i.e., schools serving a high percentage of students from low-income households) with a significant percentage (20% or higher) of Latino students. We invited all 35 schools to participate in a 3-year study in the fall of 2017 and 17 schools accepted. In the spring of 2018, we randomized schools to the treatment and control conditions using a random number generator. To avoid potential spillover effects, the randomization occurred at the school level.

Of the 17 schools that initially agreed to participate in the study, 4 schools (2 in the treatment group and 2 in the control group) declined participation at the start of the study (fall of

2018), either because of a change in leadership staff (principal turn-over) or because they expressed feeling overwhelmed with other projects taking place at their school. An additional school withdrew from the study in cohort 2, leaving 12 participating schools for the second year of the study. Hence, the final number of participating schools across the two years of the study was 13 schools. We discuss balance checks for the students and teachers from the 13 schools at the beginning of the Results section and show balance checks for the 13 versus 17 schools in Online Supplementary Material (Table B).

Statistical power. We conducted a power analysis for the original 17 schools and 3 cohorts of students prior to data analysis. In addition, we conducted power analysis for our final post-test sample of 13 schools and two cohorts, and our follow-up sample of 13 schools and one cohort. We estimated an expected a minimum detectable effect size (MDES) on the primary child outcomes of 0.38 *SD* for our original sample (17 schools, 3 cohorts), 0.52 *SD* for our final post-test sample (13 schools, 2 cohorts), and 0.68 *SD* for our follow-up sample (13 schools, 1 cohort) at alpha-level 0.05 using a two-tailed test and power level of 0.80 (full power analysis details and results available upon request for parsimony). These MDES levels make ours an underpowered cluster randomized trial. We view our study as akin to an Institute of Education Science’s Development and Innovation study (IES, 2020), given that ours was the second empirical study of FTT and the first RCT. As part of such studies under the IES framework, researchers commonly conduct underpowered randomized studies, with the goal of evaluating whether the intervention merits larger-scale testing. We include power as a limitation in our Discussion section.

Study sample. We recruited participants over a two-year period. Year 1 (cohort 1) involved 129 families (54 in treatment, 41.86%); Year 2 (cohort 2) involved 132 families (41 in treatment, 31.06%). We recruited parents via flyers distributed during the school's open house and via invitation letters sent to parents in the child's backpack. Of the 261 Latino families recruited, we had pre-test score data for 248 children (on average, 10 children per school; *M* age = 67.18 months, *SD* = 4.13, 50% girls). Of those parents who completed at least some part of the demographic survey at pre-test (*n* = 152; 58%), 24% had a GED diploma or higher and about 90% of parents were born outside of the USA. Most families immigrated from Central America (47%) and Mexico (41%).

Data collection procedures. We collected child outcome data at three time points: pre-test, end-of-treatment, and five-month follow-up. Pre-test data were collected in September (beginning of the Kindergarten year), end-of treatment data were collected in November (one to two weeks after program completion), and the five-month follow-up data were collected in April. However, due to changes to in-person schooling in the wake of the COVID-19 pandemic in the spring of 2020, we were unable to collect data for the 5-month follow-up for cohort 2.

Child data were collected in schools by a team of 20 bilingual trained assessors who were blinded to condition. Assessors went through a three-hour training delivered by a master assessor. Children were individually assessed in a separate classroom or office in the school. The average time of this "pull-out" session was 20 minutes. We counterbalanced the order of presentation of child assessments within session. Assessments were administered in the child's dominant language, determined by triangulating parent, teacher, and child reports of language dominance at each time point. Assessors made sure children knew they were bilingual and that they could speak in either language with them. Although ideally, we would have conducted the assessments in both Spanish and English, we had time constraints per school staff requirements to conduct these assessments. We collected program's reach and dosage data during the implementation of FFT via parent surveys at each session.

Measures

Child language and literacy. We used the Woodcock-Muñoz Language Survey Revised (Spanish and English Forms, WMLS-R; Woodcock, Muñoz-Sandoval, Ruef, & Alvarado, 2005) to assess children's language and literacy skills. The Picture Vocabulary subtest assessed expressive and receptive vocabulary, the Letter-Word Identification assessed letter-word knowledge, and the Dictation subtest assessed emergent writing skills. Following best practices to assess vocabulary skills in language minority children, we used total vocabulary score (also known as conceptual score; e.g., Goodrich & Lonigan, 2018). Thus, children were given credit for a correct answer, regardless of the language they used to respond. For example, if the assessment was administered in Spanish and the child provided a correct answer in English, we scored it as correct. These subtests have high levels of internal reliability (Schrank et al., 2005) and have been used in previous RCTs involving Spanish-speaking children (e.g., Hammer & Sawyer, 2016; Yoshikawa et al., 2015).

In addition, we used the expressive vocabulary task from the IDELA (International Development and Early Learning Assessment; Save the Children, 2017; Pisani et al., 2018), which prompts children to list words in two familiar domains (food, animals). Because the IDELA was developed to be administered in low-resource settings, it is sensitive to the content knowledge and skills of children from low-income and ethnically diverse backgrounds, including Latinos. We selected these items because they focused on Latino children's proximal ecologies (Garcia-Coll et al., 1996) and embody a strengths-based, culturally responsive assessment of their competences. For the expressive vocabulary task, the child was first asked to name foods that can be bought from the supermarket and then asked to list the names of animals they knew. For each prompt (food, animal), the child was encouraged to name as many foods or animals as they could. If the child paused for five seconds or more, the assessor prompted the child (only once) by saying: “Can you think of any others?” If the child named more than 10 foods/animals, the child was asked to stop. Using the responses for the food and animal items, we calculated a total vocabulary score by calculating the proportion of correct answers for foods and animals out of 20 possible points; children were given credit for a correct answer regardless of the language used. Then, using the responses for foods and animals, we calculated a foods and animals score as the proportions of correct answers out of 10 possible points following the IDELA scoring manual (Save the Children, 2017). We calculated separate percentage correct scores for the food and animal items, and a composite vocabulary score from both items. Our rationale for separating food and animal vocabulary scores was that the former item was more closely aligned with FFT content; thus, effects might be seen in food but not animal scores. The IDELA has high interval consistency, high inter-rater and test-retest reliability, and high construct validity (see IDELA Technical Manual, Save the Children, 2017; Pisani et al., 2018). To our knowledge, the IDELA has not been used to assess intervention impacts on child outcomes in the U.S., although its use in international evaluations is more common (e.g., Borzekowski, 2018).

Child math. We used five tasks from the IDELA emergent numeracy domain (Save the Children, 2017): the one-to-one correspondence task involved three items (scores ranged from zero to three); the number identification task involved 20 items (scores ranged from 0-20); the addition and subtraction task involved 3 items (scores ranged from 0-3); the size/length comparison task involved 4 items (scores ranged from 0-4); and the sort and classification task involved 2 items (scores ranged from 0-2). The child received “1” for each correct answer to a question and “0” otherwise. To create an overall math score, we calculated the average of the percent of correct answers in each of five tasks. A description of each task can be found in the Online Supplementary Material (Table C).

Child executive function. We used the inhibitory control item taken from the IDELA, which is an adaptation of the Head-Shoulders-Knees-Toes Task (HTKS; Cameron-Ponitz et al., 2009). The task required three skills: inhibitory control, working memory, and attention but it is regarded mainly as an inhibitory control assessment (Cameron-Ponitz et al., 2009). In this task, the child was encouraged to play a game in which they did the opposite of what was said. First, the assessor administered two practice trials (e.g., What do you do if I say touch your head?). The child was given feedback if they responded incorrectly and instructions were repeated up to three times. Next, test trials were administered (e.g., Touch your toes); no feedback was provided. The child’s responses were coded as 0 (incorrect), 1 (self-correct) and 2 (correct response). We calculated the final score following the IDELA scoring manual, summing the raw scores from each of the trials and dividing the summary score by 12 possible points correct.

Approaches to learning (ATL). We assessed children’s persistence, motivation, and engagement in learning activities using items taken from IDELA at three time points: 1) right after the assessor administered the math items (henceforth, math ATL); 2) right after the assessor administered the executive function item (henceforth, executive function ATL); and 3) after the assessor administered all IDELA items (i.e., expressive vocabulary, math, and executive function items; henceforth, overall ATL). For the math ATL, the assessor answered whether the child was concentrated on the task and whether the child was motivated to complete the task. The child received a “1” if the answer was yes and “0” otherwise. Scores ranged from 0-2. A similar procedure was followed for executive function ATL. For the overall ATL, the assessor used a four-point Likert scale (from almost never to almost always) to answer seven questions about the child (e.g., whether the child paid attention to the instructions during the assessment). The overall ATL scores were an average of the responses of the seven items, ranging from zero to four. The Online Supplementary Material (Table C) provides more details about the overall ATL questions.

For our primary specification, consistent with prior studies (e.g., Yoshikawa et al.,, 2015), we used raw scores (controlling for age) for all outcomes. For standardized measures, we also used W-scores as part of the robustness checks. We present descriptive statistics for all child assessments by treatment status in Table 1. As shown, treatment children scored higher with each subsequent testing period, while control children did so on 3 out of 11 child assessments.

Covariates. We used two child covariates, gender and age, collected through the consent form process. We also used four school-level covariates taken from publicly available data school-level data from the North Carolina Department of Public Instruction. These covariates included the percent of students who were Latino, economically disadvantaged, participating in English language programs or special education programs, and the percent of students who were retained in third grade. Finally, we included two teacher experience measures (highest degree attained and years of experience) collected directly from the teachers at pre-test. Although we also collected parent demographic and home literacy data, the percentage of complete items was low, ranging from 33% to 58%; thus, these variables were not included as covariates.

FFT program's reach and dosage. To assess program reach, we kept records of recruitment (percentage of parents who signed the consent form out of the total number of eligible Latino parents of kindergarteners in the participating school) and attendance (percentage of meetings attended by parents who came to at least one meeting and average number of sessions attended by those parents who came to at least one meeting). To assess dosage, during FFT sessions two through four, parents completed a survey about the frequency with which they implemented FFT activities and strategies during the past week (e.g., made a grocery list with the child, used a grocery list at the supermarket) as well as how often they practiced different strategies that were taught during the family meetings (e.g., write with child; count, compare, or estimate objects with child). For the frequency items, we used a four-point scale (from not at all to every day). Given that parents had different opportunities to complete this dosage questionnaire (i.e., if they attended multiple sessions), we aggregated these data over any available surveys. To this end, we coded the dichotomous variables as "1" if the parent ever reported these activites. For the number of grocery lists question and the Likert-scale questions, we averaged across all available data. See Online Supplementary Material (Table D) for a full list of dosage survey items.

Data Analytic Approach

RQ1: To estimate the impact of FFT, we first estimated an intent-to-treat (ITT) effect of being assigned to participate in the FFT program using OLS regression:

$$Y_{ics} = \beta_0 + \gamma(Treat)_s + \upsilon(pretest)_{ics} + X'_{ics} + \theta'_{cs} + \tau'_s + \varepsilon_{ics} \qquad (1),$$

where *Y* is the child-level outcome of interest, *i* denotes child, *c* denotes classroom, and *s* denotes school. *Treat* is set to 1 if a given school randomly assigned to treatment and 0 otherwise. We also included the pretest score for child *i* on outcome *Y*, child-level covariates (*X'*; child gender, test language of pre- and post-test, and cohort), two characteristics of child *i*'s kindergarten teacher (θ'; highest degree attained of teacher and teacher's years of experience), and several aggregate school-level covariates (τ'; percent of students who are Hispanic, economically disadvantaged, participating in English language programs or special education programs, and percent of students retained in third grade). For the Approaches to Learning (ATL) outcomes, we also included a set of dummy variables for the test assessor to account for the greater susceptibility to rater bias in this more subjective measure. We adjusted for clustering in schools within the treatment and control conditions using robust cluster-corrected standard errors at the school level. As we detail in the robustness check section, findings are not sensitive to alternative error structure choices (i.e., random intercepts for classroom and school).

Second, we estimated a treatment-on-the-treated (TOT) effect of being assigned to FFT and participating in at least one FFT session using a two-stage least squares regression:

$$Attend_{ics} = \beta_0 + \gamma(Treat)_s + \upsilon(pretest)_{ics} + X'_{ics} + \theta'_{cs} + \tau'_s + \varepsilon_{ics} \quad (2)$$

$$Y_{ics} = \beta_0 + \gamma(\widehat{Attend})_{ics} + \upsilon(pretest)_{ics} + X'_{ics} + \theta'_{cs} + \tau'_s + \delta_{ics} \quad (3),$$

where assignment to FFT is used to predict attending at least one FFT session (equation 2) and then this predicted value of attendance is used to estimate the effect of FFT (equation 3). All other terms are defined as in equation 1. In all, in the full sample, about 63% of treatment group members attended at least one session, while 0% of control families did, for a compliance rate of 63%.

We also tested the robustness of our findings from these primary specifications to a number of analytic decisions (e.g., inclusion versus exclusion of covariates, multi-level modeling with random intercepts for classroom and school, raw versus standardized scores), which are described in more detail below. Further, with the exception of the IDELA executive fuction measure (23% missing), data were missing at relatively low rates at the student-level (< 10% at each time point; also see Table 1 and Table 2 notes). Thus, we used complete case analysis as our primary specification. However, we used multiple imputation to re-estimate the ITT models using Stata 16 (analysis available upon request). We imputed 100 data sets using multivariate normal regression where we imputed (a) both the outcome and predictors and (b) only the predictors. Finally, we followed the approach of Schochet (2009) regarding multiple comparisons adjustments. In this approach, adjustments are made within developmental domain, for statistically significant, confirmatory outcomes only. As we detail in the next section, we had statistically significant findings only for one confirmatory outcome in one domain (vocabulary) and thus adjustments were not needed.

RQ2. To answer the second research question – What were the FFT program's reach and dosage levels? –we calculated rates of recruitment and attendance (i.e., program's reach) and we calculated descriptive statistics for dosage.

Results

Baseline Balance

We tested for baseline differences in child- and teacher-level characteristics of those assigned to treatment and control to assess whether the randomization process appears to have generated groups that are equal in expectation. We did so by regressing each characteristic of interest on the treatment assignment variable, with a cluster correction for school where necessary. As shown in Table 2, we did not detect any statistically significant differences in child demographics, pretest assessment scores, or teacher characteristics.

For both our final sample of schools (*N*=13) and our original sample (*N*=17), we also show balance checks on school-level characteristics in Table B Online Supplementary Material (note that child- and teacher-level characteristics were not available for the four attritor schools). We found no statistically significant differences on these characteristics either, for either sample. In some cases, the magnitude of the estimated differences between the two groups (as measured in standard deviations from the control group mean) exceeded the threshold of 0.25 *SD,* the What

Works Clearinghouse standard for baseline equivalence (What Works Clearinghouse, 2017). For example, teachers in the treatment group had on average 2 fewer years of experience (9 years v. 11 years, 0.26 *SD)* and schools in the treatment group had fewer students classified as economically disadvantaged (57% v. 60%, 0.29 *SD*), fewer students in special education (8% v. 9%, 0.73 *SD*), and fewer Hispanic students (37% v. 47%, 0.46 *SD)*. However, the overall *F*-test of baseline equivalence using all of the covariates in Table 2 was not statistically significant (F (15,142) = 0.75, $p = 0.73$), nor was the *F*-test in the Table B Online Supplementary Material ($F(11,1) = 0.21$, $p = 0.95$), indicating overall balance by treatment status. As outlined in our analytic section, we include these covariates in our primary specification and also test the robustness of our results to their inclusion versus exclusion.

Attrition

At the school level, we had a total attrition rate of 24% ($N = 4$ schools out of 17), with zero differential attrition by treatment status. At the child-level, only about 5% of children (N = 13) who were assessed at pretest were not assessed again at either end-of-treatment or the 5-month follow-up and differential attrition by treatment status was very minimal (1.52%). This level of attrition meets What Works Clearinghouse conservative standards (2017) for low threat of bias.

RQ1: FFT Impacts

Language and Literacy Outcomes. In Table 3, we present both the intent-to-treat and treatment-on-the-treated estimates for two models for our language and literacy outcomes, one of which includes child covariates only (M1) and one of which adds school and teacher covariates (M2). Across outcomes, our results are generally stable across the two models, the second of which is our preferred specification (columns 7 & 10 for ITT and TOT respectively). We also show the results of the first-stage models predicting FFT attendance for the TOT models (Column 4).

As shown in Table 3, FFT had statistically significant positive impacts on one of our confirmatory language outcomes, children's vocabulary, as measured at end-of-treatment. Children in schools randomly assigned to the treatment group (i.e., ITT) had higher total vocabulary scores on the IDELA measure by 6-7 percentage points (C = 56% T = 62-63%, $p < .05$ in Model 1 and $p < .10$ in Model 2) relative to children in schools assigned to the control group. The effect size was 0.26-0.32 *SD* across the two ITT models. Children in the treatment group schools whose parents attended at least one FFT session (i.e., TOT) had total vocabulary scores that were 10-12 percentage points higher (C = 56% to T = 66-68%, $p < .05$ in Model 1 and $p < .10$ in Model 2) compared to children in the control group schools. TOT vocabulary effect sizes ranged from 0.46-0.54 *SD* across the two specifications. These increases were particularly pronounced in gains on the food subscore for those who attended at least one FFT session (13-14 percentage points TOT, $p < 0.05$; effect size of 0.54-0.57 *SD*). We did not detect statistically significant differences between the treatment and control groups on the Woodcock-Muñoz Picture Vocabulary, Letter-Word Identification, or Dictation subscores (ITT effect sizes between -0.06 and 0.13 *SD* and TOT effect sizes between -0.11 and 0.21*SD* across all three subscores, across models).

For cohort 1, we also estimated both intent-to-treat and treatment-on-the-treated effects at the 5-month follow-up. Although the study is considerably underpowered at follow-up due to the inability to collect data for cohort 2 as a result of the COVID-19 pandemic, we found some suggestive evidence that benefits might have persisted for vocabulary for treatment-group children whose parents attended at least one of FFT session (8 percentage points TOT; effect size of 0.37 *SD*). Consistent with the end-of-treatment results, this positive effect is particularly pronounced on the food subscore (16 percentage points; effect size of 0.59 *SD*). However, these findings were sensitive to the presence of covariates, with much smaller and even negative Model 1 findings (e.g., effect size of -0.16 for vocabulary and 0.05 for food vocabulary, both TOT). Again, we find no statistically significant effects on the Woodcock-Muñoz subscores.

Math, Executive Function, and ATL. Table 4 shows the results for math, executive function, and ATL. We found no effects on math outcomes (a confirmatory outcome) at either end-of-treatment or the 5-month follow-up. In contrast, we found some evidence of positive intervention effects on our exploratory executive function and ATL outcomes. At end of treatment, we find some evidence of positive impacts on overall ATL scores, though these effects are only statistically significant when estimated with the full covariate-adjusted model (Model 2). Children in schools assigned to the FFT condition (i.e., ITT) had higher scores on overall ATL at end-of-treatment (0.13-0.22 points, 0.20-0.33 *SD*, $p < .05$ in Model 2) relative to children in schools assigned to the control condition. The TOT effect for children in schools assigned to the treatment group whose parents attended at least one session was 0.22-0.39 points (0.32-0.58 *SD*, $p < .10$ in Model 1 and $p < .05$ in Model 2). There were no statistically significant effects at end-of-treatment on other ATL or EF measures.

At the 5-month follow-up, across the ITT and TOT models, children in treatment schools also had higher scores on executive function (0.12-0.23 points, $p < .05$ for Model 1 ITT and TOT), math ATL (0.15-0.47 points, $p < .10$ for Model 1 ITT and TOT, $p < .05$ for Model 2 ITT and TOT), executive function ATL (0.09-0.22 points), and overall ATL (0.03-0.41 points; $p < 0.05$ for Model 2 TOT) compared to children in the control condition schools. The effect sizes across all ATL-related constructs at 5-month follow-up ranged from 0.05 to 0.53 for those assigned to the intervention (i.e., ITT) and from 0.09 to 0.95 for those who attended at least one session (i.e., TOT). Notably, likely due to limited power at the five-month follow-up due to COVID-19, not all these findings for our exploratory outcome were statistically significant and some showed sensitivity to inclusion or exclusion of covariates. For example, the TOT estimate for math ATL in Model 1 was 0.55 ($p < .10$) and in Model 2, 0.95 ($p < .05$). Accordingly, we interpret them as suggestive of a pattern of lasting benefits on these outcomes only.

Robustness Checks. For all outcomes, we tested the robustness of our estimates to a number of our analytic decisions. First, rather than using a robust-cluster correction to account for nesting of students within schools, we used a hierarchical linear model with random intercepts for any non-zero ICCs for schools and classrooms (Online Supplementary Material, Tables E1 and E2). Second, because of the change insample of students at end-of-treatment (cohorts 1 and 2) versus the 5-month follow-up (cohort 1 only) due to COVID-19, we estimated the effect of FFT on cohort 1 separately at end-of-treatment (Online Supplementary Material, Table F1 and F2). Third, we tested the robustness of our results to two different choices we made regarding the Woodcock-Muñoz subscale measures. As described in the measures section, we calculated children's total vocabulary scores on the Woodcock-Muñoz (WM) assessment, which allows for bilingual children to toggle between languages when taking the assessment regardless of the language of the test form (e.g., Goodrich & Lonigan, 2018). However, we also calculated WM scores that only marked answers as correct if they were given in the language of the test form (Online Supplementary Material, Table G). We also tested the robustness of our WM results to using both the standardized scores (W-scores) and raw scores with age adjustment (e.g., Yoshikawa et al., 2015)(available upon request for parsimony). We found no evidence that our primary results are sensitive to these analytic decisions.

As for results based on the multiple imputation (MI) approach (available upon request), we found consistent results for the vocabulary findings across both MI specifications (i.e., imputing outcomes and predictors and only predictors). For the imputation of both the outcome and predictor the sample size was 261, wheras with the imputation for only the predictor the sample size was 239. We found some evidence of sensitivity of results for ATL and EF results to missing data adjustment choices, with magnitudes and statistical significance larger in some cases for complete case analysis and in other cases, for imputation models. These findings underscore caution in interpreting results for our exploratory outcomes as suggestive only.

RQ2: FFT Program's Reach and Dosage

To answer our second research question, we assessed the program's reach (recruitment, attendance, and retention rates) and dosage (frequency with which parents used FFT strategies). We found that the recruitment rate among eligible Latino families in treatment schools was 22%. Among Latino parents in treatment schools who consented to participate, the attendance rate was 63% (percentage of parents who attended at least 1 session). Of parents who attended at least one session, the average number of sessions attended was 2.67 (out of 4 possible).

Data on FFT's dosage were available for treatment parents who attended at least one session (N = 57 or 63%) and completed surveys at the session (N = 32-41 across items or 35-45% of treatement parents). As shown in Table 5, during the 4-week intervention, 92.5% of participating treatment parents who came to at least one FFT session made a grocery list with their child and 65.6% used the grocery list at the supermarket. Parents reported making 1.31 grocery lists with their child, indicating moderate levels of dosage of some of FFT activities. Parents reported higher levels of dosage of literacy- and language-support strategies than math-support strategies. While they implemented literacy- (i.e., write with your child, help learn letter names and sounds) and language-support strategies (i.e., talk with your child about past or future events or explanations at mealtime) a few days per week, parents implemented math-support strategies (i.e., counting comparing or estimating objects and adding and subtracting with the child) only about a day per week.

Discussion

We report results from the first RCT of a strengths-based, culturally responsive approach to improving Latino kindergarteners' academic skills via family food routines. These routines are a sociocultural practice that plays a special role in Latino life and is too rarely capitalized on in the context of preventive interventions but may hold significant promise in such contexts. Supportive of this promise, we found confirmatory evidence that the FFT program improved Latino children's vocabulary at end-of-treatment and some suggestive, exploratory evidence that FFT might have improved children's approaches to learning (ATL). Our 5-month follow-up evidence is particularly underpowered due to COVID-19 but is suggestive of lasting benefits on these outcomes, as well as on EF. FFT had no impacts on children's math or literacy skills.

The positive impacts on a non-standardized test of language we found was aligned with FFT's content (IDELA; expressive vocabulary; assessed food vocabulary). The effect sizes for the treatment on the treated effect were substantial (d = 0.57 at end-of-treatment) and are similar to those reported by meta-analytic work (Manz et al., 2010) on home-based interventions (mean d = 0.47, range = 0.39-0.55) and markedly higher in magnitude[2] than those targeting children from minority (mean d = 0.16, range = 0.07-0.23) and low-income backgrounds (mean d = 0.14, range = 0.04-0.24). We found some evidence these benefits persisted several months after

[2] Notably, variation (i.e., standard deviation of the mean) for the control group outcome measures was small in our study, contributing to the relatively large effect sizes we detect compared with those typically found in the education literature.

intervention completion (5-month follow-up), though this finding was sensitive to covariate decisions. These findings suggest that when improvements in children's competences are embedded in valued and existing sociocultural practices, they might be enduring and that changes in such practices may represent a sustaining environment (Garcia-Coll et al., 1996). However, these findings need replication in additional, better-powered rigorous studies.

Given calls to move beyond assessing program impacts by using assessments that are overly aligned with the intervention's content (Slavin, 2019), it is important to note that unlike many traditional vocabulary interventions, FFT did not target a specific set of words and that children were not assessed on whether they learned a specific set of words. Rather, FFT promoted children's general vocabulary knowledge *within* the food content area and children's vocabulary growth was assessed using a fairly open-ended and widely used assessment (and not a study-specific measure). This feature of FFT might have also contributed to our suggestive evidence of impact maintenance at the 5-month follow-up. However, one important question that needs to be addressed in future work is how transferable this competence is, that is, whether it is positively related to vocabulary in other content areas, other expressive language skills (e.g., narrative skills) or health behaviors (e.g., healthy dietary intake and health literacy).

The lack of impacts on literacy outcomes in our study might be surprising, given that dosage data indicated that parents implemented FFT literacy-support strategies as often as they implemented language-support strategies (a few days a week). However, the type of assessment used might explain these results. Unlike language assessments, none of the literacy assessments used in this study were specifically aligned with FFT content. Further, our results are in line with those reported by a prior RCT of a culturally responsive, strengths-based home-based intervention promoting Latino preschoolers' language and literacy (e.g., Hammer & Sawyer, 2016), wherein positive impacts on non-standardized, but not standardized, language tests were found. Their effect size was 0.27 on the non-standardized narrative task (slightly lower than ours) and they used similar standardized tests (i.e., Woodcock-Muñoz battery of tests). Findings are also in line with those reported in the FFT feasibility study, wherein associations with children's vocabulary, but not literacy, were found (Leyva & Skorb, 2017). As others have noted, outcomes that are more directly related to the intervention are more susceptible to improvement (Hill et al., 2008).

Regarding the lack of math impacts, low dosage is one potential explanation. Parents reported implementing FFT math-support strategies at home less often (once per week) than language- and literacy-support strategies (a few days per week). Parents' perceptions, beliefs, and anxiety about math might have deterred parents from implementing FFT math-support strategies more often (e.g., Sonnenschein et al., 2012). An RCT of a family program promoting first-graders' math during bedtime stories found positive effects but only for children whose parents had high levels of math anxiety (Berkowitz et al., 2015). In future work, it might be important to collect parent math anxiety data for subgroup analysis. Meta-analytic work indicates that the effectiveness of math interventions in elementary school depends on children's initial math skills (Burns et al., 2010). We were underpowered to conduct such moderation analysis, but future work should address this.

It is also important to be clear what the lack of effects on literacy and math outcomes are likely *not* due to, meaning FFT's recruitment and attendance (program's reach). The rates in this study (22% and 62% respectively) were similar to those observed in the feasibility study (34%, and 58% respectively; Leyva & Skorb, 2017) and are in alignment with those reported by others engaged in family interventions for young children (20-30% and 50% respectively; e.g., Heinrichs et al., 2005). Our study offers evidence of the importance of collecting implementation data to further understand intervention effectiveness, which is key to informing policy and practice (Durlak & DuPre, 2008).

We also found a pattern suggestive of substantial impacts of FFT on the exploratory domains of executive function and ATL. Although FFT did not directly promote these skills, they are ecocultural assets that Latino children bring to school, which might help propel their academic learning (Bustamante & Hindman, 2020; Galindo & Fuller, 2010; White & Greenfield, 2017). Children in the FFT condition showed larger overall ATL improvements compared to those in the control condition at end-of-treatment and the five-month follow-up. In addition, children in the FFT condition showed larger improvements in their executive function, math ATL, and executive function ATL compared to children in the control condition at the five-month follow-up, though some of these findings were sensitive to covariate inclusion versus exclusion. The treatment on the treated effect sizes (d = 0.38-0.95 in model 2) are somewhat larger in magnitude to cluster-randomized interventions targeting self-regulatory skills in preschoolers (d = 0.37-0.43; e.g., Raver et al., 2011). The mechanism by which FFT was intended to produce improvements in academic skills could also be supporting children's ATL and executive function. FFT promotes engagement in "purposeful and contextualized activities," where children learn and practice academic skills in a fun, meaningful, and intentional manner. Thus, it makes sense that children in the FFT condition increased their motivation and engagement in learning activities (i.e., approaches to learning) compared to their peers. Furthermore, given that these 'purposeful' activities (e.g., making and using a grocery list while shopping) required children to plan, follow directions, and control their behavior and attention, it makes sense that children in the FFT condition increased their executive function skills (i.e., inhibitory control) compared to their peers.

Given these findings, in future iterations of the FFT program, it might be important to revise our theory of change to include these domain-general skills as confirmatory outcomes and revise our curriculum to more intentionally show parents how engagement in purposeful activities and strategies during food routines can improve not only domain-specific skills (vocabulary) but also domain-general skills (executive function, ATL). In fact, a study found that kindergarten teachers in the U.S. consider these domain-general skills (e.g., following directions, taking turns) to be more critical for children's successful transitioning into school than domain-specific (academic) skills (e.g., knowing most of the alphabet, counting) (Lin et al., 2003).

Limitations

Our study has several important limitations. First, this study involved Latino families from low-income households who recently immigrated to the U.S. from Mexico and Central America. Caution should be exercised when generalizing findings to the greater Latino community in the U.S. Second, due to time and resource constraints, we assessed children's outcomes in the child's dominant language and calculated the child's total vocabulary score in either language (i.e., conceptual score; Goodrich & Lonigan, 2018). Assessing children in both both languages might provide a more complete picture of the trajectories of growth in Latino kindergarteners' skills. Third, ATL was measured via assessor surveys; in future work, it would be important to add direct assessments. Fourth, we did not measure changes in parents' engagement in 'purposeful' activities and use of literacy/math-promoting strategies, the main mechanism through which FFT influences child outcomes. Future work should include such measures. Fifth, due to the COVID-19 pandemic, the 5-month follow-up data on the second cohort of children were not collected (*N* = 132), which negatively impacted our ability to detect FFT effects. Sixth, ours was a small-scale RCT with limited statistical power, consistent with FFT's stage of development. Further testing in larger, better-powered studies is important for confirming its efficacy. Finally, future studies should collect information on parents' maintenance of intervention strategies after program completion and beyond the 5-month point.

Conclusion

Our study offers rigorous and ground-breaking evidence that supports calls for innovative interventions that incorporate strengths-based and culturally responsive supports to families (Cabrera et al., 2012; Melzi et al., 2019; Perez-Brena et al., 2018). However, interventions that use this approach with young Latinos in the U.S. are just emerging. Testing these interventions rigorously, as we do here, is essential for delivering on the promise of this approach for what is the fastest growing child demographic in the U.S.

If validated in larger trials, FFT may have potential implications for policy. School districts across the U.S. recognize that family engagement is an integral part of education but struggle to implement family engagement policies because they feel under-prepared to build and sustain such family-school partnerships (Mapp & Kuttner, 2013). Furthermore, under the Every Student Succeeds Act, Title I schools are required to reserve at least 1% of funds for family engagement activities (Every Student Succeeds Act, 2015). By focusing on Latino families' eco-cultural assets, FFT enacts an initiative that facilitates the conditions for Latino family engagement in schools, by building a relationship of trust and respect between home and schools and helping Latino families become equal partners in their children's education. FFT may have the potential to mold aspects of the Latino community's practices into durable, compounding improvements in critical outcomes (language, executive function, ATL) that matter to school and life success (McDermott et al., 2014). This approach can reduce racial/ethnic disparities in school success while respecting and elevating the richness of Latino family life.

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Table 1.

Means (SD) for Child Assessments by Treatment Status across the Three Time Points

	Treatment (FFT)			Control		
	Pre-test	End of Treatment (Post-test)	5-month Follow-up	Pre-test	End of Treatment (Post-test)	5-month Follow-up
WM – Picture Vocabulary	20.56 (5.41)	21.41 (4.87)	23.82 (4.85)	19.59 (5.40)	20.64 (5.16)	23.85 (5.36)
WM – Letter-word Identification	10.05 (5.04)	12.88 (4.93)	19.29 (4.14)	10.51 (4.74)	12.45 (4.92)	19.73 (8.52)
WM – Dictation	7.90 (2.09)	9.79 (2.93)	14.72 (4.25)	8.14 (2.15)	9.79 (2.56)	14.18 (4.30)
IDELA Total Vocabulary	0.54 (0.23)	0.60 (0.21)	0.68 (0.19)	0.57 (0.23)	0.55 (0.22)	0.72 (0.22)
IDELA Food Vocabulary	0.54 (0.29)	0.62 (0.26)	0.73 (0.22)	0.56 (0.28)	0.56 (0.24)	0.74 (0.27)
IDELA Animal Vocabulary	0.55 (0.29)	0.58 (0.27)	0.64 (0.23)	0.57 (0.27)	0.54 (0.27)	0.70 (0.27)
IDELA Math	0.55 (0.18)	0.65 (0.16)	0.79 (0.15)	0.56 (0.19)	0.64 (0.19)	0.81 (0.16)
IDELA Executive Function	0.40 (0.34)	0.55 (0.33)	0.62 (0.33)	0.49 (0.35)	0.58 (0.34)	0.55 (0.37)
IDELA Math - ATL	1.77 (0.52)	1.78 (0.54)	1.90 (0.31)	1.88 (0.40)	1.89 (0.33)	1.81 (0.50)
IDELA Executive Function - ATL	1.66 (0.69)	1.80 (0.48)	1.90 (0.31)	1.80 (0.53)	1.79 (0.48)	1.75 (0.58)
IDELA Overall ATL	3.29 (0.67)	3.49 (0.55)	3.51 (0.65)	3.35 (0.65)	3.35 (0.67)	3.46 (0.65)

Note. WM = Woodcock-Muñoz battery of tests. ATL = Approaches to Learning. We report raw scores as these scores were used in the primary specification. Combined sample size for children with assessement data reported in this table ranged from 202-244 at pre- test; 213-250 at end-of-treatment; and 115-118 at the 5-month follow-up. Note that at the 5-month follow-up, only cohort 1 data were available due to COVID-19.

Table 2.
Balance Checks

	Treatment sample (n = 91) Mean (SD)	Control sample (n = 157) Mean (SD)	Raw Difference	Effect Size
Child demographics (n = 248)				
Gender	0.49	0.51	-0.02	-0.04
Age at pre-test	67.04 (4.10)	67.30 (4.03)	-0.26	-0.06
Baseline child-level assessement data (n = 248)				
Language of pre-test is Spanish	0.56	0.69	-0.13	-0.28
WM – Picture Vocabulary	20.80 (5.42)	19.69 (5.26)	1.11	0.21^{+}
WM – Letter-word Identification	10.18 (5.09)	10.61 (4.70)	-0.43	-0.09
WM - Dictation	8.00 (2.08)	8.20 (2.15)	-0.20	-0.09
IDELA Vocabulary total	0.55 (0.23)	0.57 (0.22)	-0.02	-0.09
IDELA Food Vocabulary	0.55 (0.29)	0.56 (0.27)	-0.01	-0.04
IDELA Animal Vocabulary	0.55 (0.29)	0.57 (0.26)	-0.02	-0.08
IDELA Math	0.56 (0.18)	0.57 (0.19)	0.00	0.02
IDELA Executive Function	0.42 (0.34)	0.50 (0.35)	-0.09	-0.26
IDELA Math - ATL	1.80 (0.51)	1.89 (0.37)	-0.09	-0.24
IDELA Executive Function - ATL	1.71 (0.64)	1.80 (0.54)	-0.09	-0.26
IDELA Overall ATL	3.33 (0.64)	3.34 (0.65)	-0.01	-0.02
Teacher-level data (n = 71)				
Teacher experience	9.21 (5.97)	11.23 (7.69)	-2.02	-0.26
Teacher has a BA (vs. a Master)	0.65 (0.49)	0.64 (0.49)	0.02	0.04

Note. $^{+}p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$. ATL = Approaches to Learning. Overall F-test is $F(15, 142) = 0.75$, $p = .73$. Out of the total 261 children randomized into the study, we did not have a end-of-treatment outcome data for 13 children and they are excluded from this table. The raw difference column was obtained by regressing the characteristic of interest on intervention condition and clustering for school when applicable. Effect sizes were calculated by dividing the raw difference by the standard deviation of the control group. With the exception of IDELA Executive Function, which had missing data for 23% of students, missing data for all other student-level characteristics ranged from 0% to 9% (M = 6%; SD = 3%). At the teacher-level, 13% of teachers had missng data. Standard deviations are only reported for continous variables.

Table 3.
Impacts on Child Language and Literacy Skills

	Model 1					Model 2				
	ITT		1st stage	TOT		ITT		1st stage	TOT	
	Estimate	ES	Estimate	Estimate	ES	Estimate	ES	Estimate	Estimate	ES
End of treatment (Both cohorts)										
IDELA Vocabulary total	0.07*	0.32*	0.61***	0.12*	0.54*	0.06+	0.26+	0.56***	0.10+	0.46+
	(0.03)			(0.05)		(0.03)			(0.06)	
IDELA Food Vocabulary	0.08*	0.33*	0.61***	0.13*	0.54*	0.08+	0.33+	0.57***	0.14*	0.57*
	(0.03)			(0.06)		(0.04)			(0.07)	
IDELA Animal Vocabulary	0.05	0.20	0.62***	0.09	0.33	0.03	0.13	0.57***	0.06	0.22
	(0.03)			(0.05)		(0.04)			(0.06)	
WM Picture Vocabulary	0.15	0.03	0.62***	0.24	0.05	-0.32	-0.06	0.57***	-0.56	-0.11
	(0.47)			(0.75)		(0.57)			(0.96)	
WM Letter-word identification	0.62	0.13	0.61***	1.01	0.21	0.37	0.07	0.55***	0.67	0.14
	(0.48)			(0.75)		(0.52)			(0.90)	
WM Dictation	0.12	0.05	0.62***	0.18	0.07	0.04	0.02	0.57***	0.07	0.03
	(0.32)			(0.51)		(0.42)			(0.71)	
5-month Follow up (Cohort 1 only)										
IDELA Vocabulary total	-0.02	-0.09	0.56***	-0.03	-0.16	0.05	0.23	0.62***	0.08	0.37
	(0.04)			(0.07)		(0.05)			(0.07)	
IDELA Food Vocabulary	0.01	0.03	0.55***	0.01	0.05	0.10	0.36	0.61***	0.16	0.59
	(0.05)			(0.09)		(0.07)			(0.10)	
IDELA Animal Vocabulary	-0.06	-0.21	0.58***	-0.10	-0.37	0.01	0.03	0.61***	0.01	0.04
	(0.04)			(0.08)		(0.06)			(0.08)	
WM Picture Vocabulary	0.17	0.03	0.56***	0.30	0.06	0.93	0.17	0.56***	1.66	0.31
	(0.72)			(1.23)		(0.93)			(1.50)	
WM Letter-word identificaiton	-0.35	-0.04	0.53***	-0.66	-0.08	1.68	0.20	0.53***	3.18	0.37
	(1.31)			(2.34)		(1.56)			(2.85)	
WM Dictation	0.24	0.05	0.56***	0.42	0.10	0.26	0.06	0.53***	0.44	0.10
	(0.58)			(0.99)		(0.69)			(1.06)	

Note. +$p < .10$, *$p < .05$, **$p < .01$, ***$p < .001$. WM = Woodcock-Muñoz battery of tests. Standard errors in parentheses. Effect sizes are standardized using the standard deviation of the control group. Model 1 includes controls for pre-test language, post-test language, child age, child gender, and an indicator for cohort (for end-of-treatment outcomes only). Model 2 adds school-level covariates (% Hispanic, % Limited English Proficient, % special education, % economically disadvantaged) and teacher-level covariates (has master's degree, years of experience). We used raw scores with age adjustment for the WM outcomes. We defined compliers as parents who attended at least one FFT meeting. Sample sizes range from N = 216-229 on end-of-treatment outcomes and N = 94-102 on 5-month follow-up outcomes (cohort 1 only).

Table 4.
Impacts on Child Math, Executive Function (EF) and Approaches To Learning (ATL)

	Model 1					**Model 2**				
	ITT		1st stage	TOT		ITT		1st stage	TOT	
	Estimate	ES	Estimate	Estimate	ES	Estimate	ES	Estimate	Estimate	ES
End of treatment (Both cohorts)										
Math	0.01	0.03	0.62***	0.01	0.04	0.02	0.10	0.57***	0.03	0.18
	(0.02)			(0.03)		(0.02)			(0.04)	
EF	0.03	0.08	0.60***	0.04	0.13	0.05	0.15	0.57***	0.09	0.27
	(0.05)			(0.09)		(0.06)			(0.10)	
Math -ATL	-0.08	-0.23	0.65***	-0.12	-0.35	-0.04	-0.11	0.60***	-0.07	-0.20
	(0.06)			(0.09)		(0.07)			(0.11)	
EF -ATL	0.04	0.09	0.61***	0.07	0.14	0.05	0.10	0.54***	0.10	0.20
	(0.07)			(0.11)		(0.09)			(0.16)	
Overall ATL	0.13	0.20	0.62***	0.22+	0.32+	0.22*	0.33*	0.58***	0.39*	0.58*
	(0.08)			(0.13)		(0.10)			(0.17)	
5-month Follow-up (Cohort 1 only)										
Math	0.00	0.00	0.56***	0.00	0.00	-0.01	-0.05	0.59***	-0.01	-0.08
	(0.02)			(0.04)		(0.03)			(0.04)	
EF	0.12*	0.33*	0.57***	0.22*	0.58*	0.14	0.37	0.60***	0.23	0.61
	(0.06)			(0.10)		(0.09)			(0.14)	
Math -ATL	0.15+	0.30+	0.57***	0.27+	0.55+	0.26*	0.53*	0.61***	0.47*	0.95*
	(0.08)			(0.14)		(0.12)			(0.20)	
EF -ATL	0.09	0.16	0.55***	0.18	0.31	0.11	0.18	0.55***	0.22	0.38
	(0.08)			(0.15)		(0.15)			(0.27)	
Overall ATL	0.03	0.05	0.58***	0.06	0.09	0.23+	0.36+	0.62***	0.41*	0.63*
	(0.09)			(0.16)		(0.12)			(0.18)	

Note. $+p < .10$, $*p < .05$, $**p < .01$, $***p < .001$. EF = Executive Function. ATL = Approaches to Learning. Standard errors in parentheses. Effect sizes are standardized using the standard deviation of the control group. Model 1 includes controls for pre-test language, post-test language, child age, child gender, and an indicator for cohort (for end-of-treatement outcomes only). Model 2 adds school-level covariates (% Hispanic, % Limited English Proficient, % special education, % economically disadvantaged) and teacher-level covariates (has master's degree, years of experience). We used raw scores with age adjustment for the Woodcock-Muñoz outcomes. We defined compliers as parents who attended at least one FFT meeting. Sample sizes range from N =170-229 on end-of-treatment outcomes and N = 94-102 on 5-month follow-up outcomes (cohort 1 only).

Table 5.
Food For Thought (FFT) Dosage Levels for Treatment Parents Who Attended at Least One Session (n = 57)

	N	Mean (SD) or %	Range
Made a grocery list with your child this past week	40	92.50%	0-1
Used the grocery list you made with your child at the supermarket	39	65.63%	0-1
Number of grocery lists made with your child this past week	32	1.31 (0.62)	0-3
How often did you practice with your child this week to:			
a. Write with your child	41	2.07 (0.59)	0-3
b. Learn letter names and sounds	41	2.21 (0.66)	0-3
c. Talk with your child about past or future events or explanations at mealtime	41	2.24 (0.76)	0-3
d. Count, compare, or estimate objects or coins with your child	41	1.93 (0.74)	0-3
e. Add and subtract with your child	41	1.93 (0.68)	0-3

Note. "How often" question responses range from 0 (not at all) to 3 (everyday). We combined data across the three FFT sessions (sessions 2, 3, 4) where fidelity data were collected. For example, we coded the question about making or using a grocery list as "1" if a parent reported ever writing or using a grocery list based on available data.