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CHILD DEVELOPMENT

EMPIRICAL ARTICLE

A strengths-based, culturally responsive family intervention improves Latino kindergarteners' vocabulary and approaches to learning

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Abstract
Food routines are an ecocultural asset of Latino families. This cluster-randomized trial with 248 children (M_{age} = 67 months; 50% girls; 13 schools) investigated the impact of a 4-week family program designed to capitalize on food routines in improving Latino kindergarteners' outcomes in the United States. There were moderate-to-large impacts on child vocabulary (especially food-related) at end-of-treatment and the 5-month follow-up, and suggestive evidence of moderate impacts on approaches to learning (ATL; including ATL math) and executive function at the 5-month follow-up (d = .38–.95). 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 the skills needed to succeed in school.

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, disempowering these families and eroding their social and cultural competence (Cabrera et al., 2012; Garcia-Coll et al., 1996; Melzi et al., 2019). To disrupt deficit-based approaches of racialized children (Kendi, 2019), researchers have called for strengths-based, culturally responsive approaches, adopting a resilience perspective and emphasizing the ecocultural assets that protect (reduce risk) and promote positive outcomes (Perez-Brena et al., 2018). Notably, there is a paucity of rigorous evaluations of strengths-based and culturally responsive interventions, particularly in Latino communities. It is critical to build this evidence base using rigorous designs like randomized controlled trials (RCTs) to understand the potential of this intervention approach to better support families and children and to develop additional such interventions.

In the present study, we experimentally test the effects of food for thought (henceforth FFT), a strengths-based, culturally responsive intervention that builds upon family food routines, a set of valued practices that are already established in the ecocultural context of the Latino family (i.e., grocery shopping, cooking, and eating together) to improve young Latino children's learning. As we detail below, FFT has shown promise in a feasibility study (Leyva & Skorb, 2017). Testing FFT via a small

Abbreviations: ASQ, Ages and Stages Questionnaire; ATL, approaches to learning; FFT, food for thought; ICC, intraclass correlation coefficient; IDELA, International Development and Early Learning Assessment; ITT, intent-to-treat; MDES, minimum detectable effect size; MI, multiple imputation; RCT, randomized controlled trial; TOT, treatment-on-the-treated; WM, Woodcock–Muñoz.

randomized trial represents the next step in its development and also contributes to the broader evidence base on innovative interventions that incorporate strengths-based and culturally responsive supports for families, particularly for those families living in poverty and experiencing marginalization (Cabrera et al., 2012; Melzi et al., 2019). 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.

Development of the FFT intervention

The lead author of the present study developed FFT to meet the needs of the growing and underserved Latino children population in the United States and in recognition of family food routines as a key Latino ecocultural asset.

Need to better support Latino students' learning in schools

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 (US Census Bureau, 2019). In the school district targeted by FFT, stakeholders (kindergarten teachers and principals) cited the provision of culturally sustaining supports to bridge home and school learning as a major area that needed improvement. Critically, early investments in this fast-growing segment of young U.S. children may translate into higher productivity, and lower health and education costs for the nation (Heckman, 2006).

Family food routines

A unique ecocultural asset of Latino families is the frequency and type of parent–child interactions during food routines. Latino families show the highest rates of shared mealtimes compared to any other ethnicity (6–7 times per week; Murphey et al., 2014) and high rates of home cooking, and high child involvement in kitchen chores (Eisenberg, 2002; Evans et al., 2011). Cultural beliefs and values sustain these practices. Latino parents use family food routines as a vital mechanism to preserve and transmit their culture (Evans et al., 2011). Through these routines, children develop their identity as Latinos and are socialized into enacting *familismo* (strong sense of identification and loyalty to family). The built-in benefits of Latino family food routines are not only the frequency, but also the type of parent–child interactions that these practices afford. Latino parents engage in more cognitively complex interactions with their children during food-related activities (e.g., baking cookies together) than non-food-related activities (e.g., book reading), due to parents' higher familiarity with the setting and sense of self-efficacy. For example, Latino parents ask more questions requiring active thinking (Tenenbaum & Leaper, 1997), provide more explanations and engage in counting (Eisenberg, 2002), and encourage children's independent responses and follow their interests more (Kermani & Janes, 1999) in food- than non-food related activities (Eisenberg, 2002; Kermani & Janes, 1999).

Hence, FFT is a 4-week school-based program that capitalizes on family food routines to help Latino parents foster their kindergarten children's learning. FFT incorporates Latino children's daily experiences (i.e., high participation in family food routines) and considers the racial/ethnic values that facilitate the development of their abilities in these contexts (e.g., *familismo*), and the social and structural factors that can hinder this development (e.g., poverty; Garcia-Coll et al., 1996). FFT focuses on kindergarten because the transition to elementary school is a time when Latino parents appear to be particularly eager to play an active role in their child's learning and logistically, are easier to reach because their children are part of the public education systems (Goldenberg et al., 2001). Based on best practices in adult education, FFT provides information (e.g., strategies supporting child learning), which increases parents' motivation to change. FFT then uses video clips, coaching, and onsite opportunities to practice these strategies which help transform this motivation into behavior change (Michie et al., 2009).

FFT curriculum

Food for thought promotes parent–child narratives, authentic writing and reading, and math talk, which are known to positively influence three high-value, high-priority learning outcomes that predict school achievement: language, literacy, and math.

Narratives

Latino communities place a strong emphasis on oral narratives for religious, moral, and personal reasons (Hammer & Sawyer, 2016); thus, narratives are a culturally appropriate way to foster child learning. Parent–child narratives (e.g., conversations about past or future events) provide children with opportunities to practice producing and comprehending language removed from the here and now (decontextualized talk) in the context of storytelling. This type of talk predicts academic achievement (Uccelli et al., 2019). RCTs have shown increases in parents' use of certain strategies during narratives (e.g., open-ended questions and following the child's lead), which translate into improvements in preschoolers' language (e.g., vocabulary) in Latino (Hammer & Sawyer, 2016; Reese et al., 2010) and non-Latino families (Leech et al., 2018; Peterson et al., 1999; Reese & Newcombe, 2007). Thus, FFT encourages parents to regularly engage in narratives with their children during food routines, and to use strategies such as asking many open-ended questions and following the child's lead.

Authentic writing and reading

FFT promotes authentic reading and writing (i.e., activities that serve the genuine purpose of communicating information to someone who needs it, such as writing and reading a grocery list to shop at a store; Gerde et al., 2012). There are documented benefits of using this type of activities in preschool and kindergarten classrooms (Gerde et al., 2012), and at least one RCT involving preschoolers from low-income households yielded larger effects on literacy (e.g., letter-word knowledge, emergent writing) when participating in such activities compared to book reading (Aram & Biron, 2004). Prior studies show that parents who use certain strategies while writing with their children (e.g., helping children discriminate sounds of spoken words and link them to letters) have preschoolers and kindergarteners with advanced literacy (letter-word knowledge and emergent writing) in ethnically diverse families, including Latinos (e.g., Bindman et al., 2014; Leyva & Skorb, 2017; Leyva et al., 2019). Thus, FFT encourages parents to regularly engage in authentic writing and reading with their children during food routines, and to use strategies such as helping children discriminate sounds of spoken words and linking sounds to letters.

Math talk

Parental math talk (e.g., talk about counting, comparing quantities, number recognition, adding, and subtracting) relates to child math skills in communities from diverse ethnic and income backgrounds, including Latino (Eason et al., 2020). Parental math talk helps children develop math vocabulary, which in turn facilitates children's math thinking (Eason et al., 2020). RCTs show that is possible to increase the frequency and type (i.e., use of strategies such as counting and comparing quantities) of parental math talk, which results in improvements in young children's math skills (e.g., Gibson et al., 2020) and at least one RCT improved parental math talk during cooking (Vandermaas-Peeler et al., 2012). Thus, FFT encourages parents to regularly engage in math talk with their children during food routines and to use strategies such as counting and comparing quantities.

FFT'S theory of change

Based on the evidence discussed above, we expected FFT to increase the frequency and type of parent–child narratives, authentic writing and reading, and math talk, which in turn would increase children's language, literacy, and math outcomes. Specifically, we expected increases in parent–child narratives to increase child vocabulary; increases in parent–child authentic writing and reading to increase child literacy (letter-word knowledge and emergent writing) and increases in parent–child math talk to increase child math.

We also explored whether FFT had effects on executive function and approaches to learning (henceforth, ATL), that is, children's motivation, persistence, and engagement in learning tasks (McDermott et al., 2014). We explored such effects because there is some evidence that the practices promoted by FFT may facilitate such outcomes. Specifically, authentic writing and reading enhance children's motivation, persistence, and engagement in writing tasks because such activities are enjoyable and meaningful and mobilize children's experience and expertise (Parsons & Ward, 2011). Thus, it was possible that such practices would facilitate ATL (engagement in learning tasks). Narratives (e.g., talking about past experiences) develop mind-mindedness (i.e., provide children with verbal tools to control their attention, emotion, and behavior), which is central to executive function (Bernier et al., 2010; Leyva & Nolivos, 2015). Thus, it was possible that such practices would facilitate executive function. In addition, executive function and ATL are "cognitive and social building blocks" of language, literacy, and math development (Best et al., 2011) and are ecocultural assets that Latino kindergarteners bring to school. Latino bilingual children have higher executive function skills than non-Latino monolingual children in 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).

Previous FFT evidence and the current study

We adopted an iterative intervention design to refine the program in preparation for program evaluation. First, we piloted FFT (N = 10, 1 school) in 2014 and as a result, we revised the curriculum (shortened it from 10 to 4 weeks). We then conducted a feasibility study to assess its implementation in 2015 (N = 68, 3 schools; Leyva & Skorb, 2017, Leyva et al., 2018), and further refined the curriculum (e.g., infused more strategies to help parents engage children in narratives during food routines). Program reach levels (recruitment rate = 34%; attendance rate = 58%) met or exceeded those reported by prior literature (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* = .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* = .46). Latino parents reported that FFT empowered them to support their children's learning and created a sense of community. Hence, results indicated that FFT was feasible to implement and yielded promising outcomes. However, by design, the feasibility study was small and correlational, no follow-up assessments were included, and no dosage levels were measured (i.e., the extent to which parents implemented FFT strategies at home; Durlak & DuPre, 2008).

In 2018, as the next stage of FFT's development, we launched a cluster-randomized trial to determine its effects on kindergarteners' outcomes. Following best practices (Gehlbach & Robinson, 2018), we pre-registered our hypotheses. Child language, literacy, and math were *confirmatory* outcomes and executive function and ATL were *exploratory* outcomes. Our specific research questions were: (1) What were the FFT program's reach and dosage levels? (2) Does FFT improve kindergarteners' language, literacy, math, executive function, and ATL skills at end-of-treatment post-test and the 5-month follow-up?

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) and explored the extent to which parents implemented FFT strategies at home during the 4-week intervention. We expected FFT to improve child language, literacy, and math, and hypothesized that such improvements would be apparent at end-of-treatment post-test and would persist through the 5-month follow-up, given the ecocultural nature of the activities promoted by FFT. That is, families may choose to keep implementing targeted practices and thus we anticipated either sustained or even larger effects beyond the immediate posttest. We explored FFT effects on executive function and ATL.

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. The cluster was schools; schools were randomly assigned to the FFT intervention condition or an active control condition. The 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). We anticipated collecting three cohorts of data; however, the COVID-19 pandemic prevented us from collecting data on our planned third cohort and from assessing the second cohort at the 5-month follow-up (planned for spring 2020).

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. Table S1 summarizes FFT topics, activities, and strategies per session. FFT sessions align with Latino assets; for example, because we know that children are highly involved in cooking and kitchen chores, session 1 and 2 are about going grocery shopping together and cooking, respectively. Because we know that families share mealtimes regularly, sessions 3 and 4 are about eating in and out, respectively. 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 3-h 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 min. During the first 60 min, parents watched and discussed video clips featuring Latino parents effectively using FFT strategies with their children. During the last 30 min, 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 5–8 min sharing their experiences practicing FFT strategies at home.

The active control condition entailed one 90-min session 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 onsite practice with children. Parents received a handout but no text reminders. We had two goals in using an "active" rather than "passive" (business as usual) control condition. The primary goal was to facilitate school and family recruitment. The second was to rule out that any kind of parenting session might have yielded the same results as our culturally responsive, strengths-based approach. This is a common tactic in RCT intervention studies. For example, in a

RCT of a language and literacy coaching intervention in Chile, the research team supplied 100 books to control group classrooms both to keep them engaged and to rule out the possibility that improvements detected in the treatment group could have been due simply to the provision of books (Yoshikawa et al., 2015).

School recruitment and randomization of schools

First, we identified 35 Title 1 elementary schools (i.e., schools serving a high percentage of students from low-income households) with 20% or higher percentage of Latino students in one of the largest school districts in the United States located in the Southeast. 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, four 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. Hence, the final number of participating schools was 13 (an additional school withdrew from the study in cohort 2, leaving 12 participating schools for the second year of the study). 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 Table S2.

Statistical power

We powered our study originally (17 schools, 3 cohorts) for a minimum detectable effect size (MDES) on the primary child outcomes of 0.38 (0.8 power, α-level .05 using a two-tailed test; see Table S3 for full assumptions). This effect size was consistent with the overall effect size of home-based interventions (mean d = .47, range = .39–.55) reported by meta-analytic work (Manz et al., 2010). Ultimately, due to attrition and to COVID-19 disruptions, our MDES on the primary child outcomes was 0.52 *SD* (which is still within the effect size range that meta-analytic work has found) for our final post-test sample (13 schools, 2 cohorts), and 0.68 *SD* for our follow-up sample (13 schools, 1 cohort). 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 (Institute of Educational Sciences, 2020) and an appropriate design for FFT's stage of development given that ours was the second empirical study of FFT and the first RCT. As part of such studies under the Institute of Educational Sciences 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. Table S3 compares results of power analyses for the original 17 schools and three cohorts of children and for our final post-test sample of 13 schools and two cohorts, and our follow-up sample of 13 schools and one cohort.

Study sample

We recruited 261 Latino families over a 2-year period. Year 1 (cohort 1) involved 129 families (54 in treatment, 41.86%); Year 2 (cohort 2) included 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. 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 United States. 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 5-month follow-up. Pre-test data were collected in September (beginning of the Kindergarten year), end-of treatment data were collected in November (1–2 weeks after program completion), and the 5-month follow-up data were collected in April. However, due to the COVID-19 pandemic, we were unable to collect data for the 5-month follow-up for cohort 2 in the spring of 2020.

Child data were collected in schools by a team of 20 bilingual trained assessors who were blinded to condition. Assessors went through a 3-h 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 min. 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 (WM) Language Survey Revised (Spanish and English Forms, Woodcock et al., 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 a 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. 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; Pisani et al., 2015; Save the Children, 2017), 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 5 s 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 of 20 possible points. Then, using the responses for foods and animals, we calculated a foods and animals score as the proportions of correct answers 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. Prior work reveals high levels of internal reliability in these items: Cronbach's $\alpha = .77$; test–retest reliability $r = .79$, and intraclass correlation coefficient (ICC) = .88 (Pisani et al., 2015); in our sample, Cronbach's α was .59. Construct validity for these items has been established using factor analysis (Wolf et al., 2017), and in relation to the Ages and Stages Questionnaire (ASQ), $r = .36$ (Pisani et al., 2015).

Child math

We used five tasks from the IDELA early 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 to 20); the addition and subtraction task involved three items (scores ranged from 0 to 3); the size/length comparison task involved four items (scores ranged from 0 to 4); and the sort and classification task involved two items (scores ranged from 0 to 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 Table S4. Prior work reveals high levels of internal reliability: Cronbach's $\alpha = .79$ and ICC = .87 (Pisani et al., 2015); in our sample, Cronbach's α was .68. Construct validity of these items has been established using factor analysis (Wolf et al., 2017), and in relation to the ASQ, $r = .48$ (Pisani et al., 2015).

Child executive function

We used the inhibitory control item taken from the IDELA, which is an adaptation of the Head–Shoulders–Knees–Toes Task (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. Prior work reveals high levels of internal reliability (see Wolf et al., 2017); in our sample, Cronbach's α was .88. Concurrent validity with other executive function tasks has been established; effect sizes ranged from .21 to .54 (Pisani et al., 2015).

Approaches to learning

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 to 2. A similar procedure was followed for executive function ATL. For the overall ATL, the assessor used a 4-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. Table S4 describes the items assessing overall ATL. High levels of internal reliability and construct validity have been established for these items in prior work (e.g., Cronbach's $\alpha = .94$ for overall ATL; Wolf et al., 2017); in our sample, Cronbach's α was .91 for overall ATL, .74 for math ATL, and .84 for executive function ATL.

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 (the *W* scale is a transformation of the Rasch ability scale, a score representing both the child's ability level and the task difficulty level). 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 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

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: We report raw scores as these scores were used in the primary specification. Combined sample size for children with assessment data reported in this table ranged from 202 to 244 at pre-test; 213 to 250 at end-of-treatment; and 115 to 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.

Abbreviations: ATL, approaches to learning; FFT, food for thought; IDELA, International Development and Early Learning Assessment; WM, Woodcock–Muñoz battery of tests.

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 Table S5 for a full list of dosage survey items.

Data analytic approach

RQ1: To answer the first 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.

RQ2: 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 Ordinary-Least-Square (OLS) regressions:

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

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 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 modeling 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:

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

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

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 vs. exclusion of covariates, multilevel modeling with random intercepts for classroom and school, raw vs. standardized scores), which are described in more detail below. Furthermore, 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). 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 (2008) 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.

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 S2 (note that child- and teacher-level characteristics were not available for the four attritor schools). We found no statistically significant differences in 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. For example, teachers in the treatment group had on average 2 fewer years of experience (9 years vs. 11 years, 0.26 *SD*) and schools in the treatment group had fewer students classified as economically disadvantaged (57% vs. 60%, 0.29 *SD*), fewer students in special education (8% vs. 9%, 0.73 *SD*), and fewer Hispanic students (37% vs. 47%, 0.46 *SD*). However, the overall F-test of baseline equivalence using all of the covariates in Table 2 was not statistically significant, $F(15,142) = 0.75$, $p = .73$, nor was the F-test in the Table S2, $F(11,1) = 0.21$, $p = .95$, 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 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%); aligning with What Works Clearinghouse conservative standards for low threat of bias.

RQ1: FFT program's reach and dosage

We found that the recruitment rate among eligible Latino families in treatment schools was 22%. Among

TABLE 2 Balance checks

	Treatment sample (n = 91)	Control sample (n = 157)		
	***M* (*SD*)**	***M* (*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 assessment 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: Overall F-test is $F(15, 142) = 0.75$, $p = .73$. Of the total 261 children randomized into the study, we did not have an 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 missing data. Standard deviations are only reported for continuous variables.

Abbreviations: ATL, approaches to learning; IDELA, International Development and Early Learning Assessment; WM, Woodcock–Muñoz battery of tests.

$^{+}p < .10$.

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 (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 treatment parents). As shown in Table 3, 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.

RQ2: FFT impacts

Language and literacy outcomes

In Table 4, we present both the ITT and TOT 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 and 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 4, 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 to 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, $p < .05$; effect size of 0.54–0.57 SD). We did not detect statistically significant differences between the treatment and control groups on the WM 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 ITT and TOT 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 WM subscores.

Math, executive function, and ATL

Table 5 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, $p < .05$

TABLE 3 Food for thought (FFT) dosage levels for treatment parents who attended at least one session (*n* = 57)

	N	*M* (*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.

for Model 2 ITT and TOT), executive function ATL (0.09–0.22 points), and overall ATL (0.03–0.41 points; $p < .05$ 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 5-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 (Table S6). Second, because of the change in sample 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 (Table S7). Third, we tested the robustness of our results to two different choices we made regarding the WM subscale measures. As described in the measures section, we calculated children's total vocabulary scores on the 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 (Table S8). 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, whereas 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.

DISCUSSION

We report results from the first RCTs of a strengths-based, culturally responsive approach to improving Latino kindergarteners' outcomes via family food routines. The frequency and type of parent–child interactions during food routines are a unique ecocultural asset of Latino families, which is 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 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 executive function. FFT had no impacts on children's math or literacy skills.

The positive impacts on a non-standardized test of language we found were aligned with FFT's content (IDELA; expressive vocabulary; assessed food vocabulary). The

TABLE 4 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 (0.03)*	0.32*	0.61***	0.12 (0.05)*	0.54*	0.06 (0.03)$^{+}$	0.26^{+}	0.56***	0.10 (0.06)$^{+}$	0.46^{+}
IDELA food vocabulary	0.08 (0.03)*	0.33*	0.61***	0.13 (0.06)*	0.54*	0.08 (0.04)$^{+}$	0.33^{+}	0.57***	0.14 (0.07)*	0.57*
IDELA animal vocabulary	0.05 (0.03)	0.20	0.62***	0.09 (0.05)	0.33	0.03 (0.04)	0.13	0.57***	0.06 (0.06)	0.22
WM Picture Vocabulary	0.15 (0.47)	0.03	0.62***	0.24 (0.75)	0.05	−0.32 (0.57)	−0.06	0.57***	−0.56 (0.96)	−0.11
WM Letter-Word Identification	0.62 (0.48)	0.13	0.61***	1.01 (0.75)	0.21	0.37 (0.52)	0.07	0.55***	0.67 (0.90)	0.14
WM Dictation	0.12 (0.32)	0.05	0.62***	0.18 (0.51)	0.07	0.04 (0.42)	0.02	0.57***	0.07 (0.71)	0.03
5-month follow up (cohort 1 only)										
IDELA vocabulary total	−0.02 (0.04)	−0.09	0.56***	−0.03 (0.07)	−0.16	0.05 (0.05)	0.23	0.62***	0.08 (0.07)	0.37
IDELA food vocabulary	0.01 (0.05)	0.03	0.55***	0.01 (0.09)	0.05	0.10 (0.07)	0.36	0.61***	0.16 (0.10)	0.59
IDELA animal vocabulary	−0.06 (0.04)	−0.21	0.58***	−0.10 (0.08)	−0.37	0.01 (0.06)	0.03	0.61***	0.01 (0.08)	0.04
WM Picture Vocabulary	0.17 (0.72)	0.03	0.56***	0.30 (1.23)	0.06	0.93 (0.93)	0.17	0.56***	1.66 (1.50)	0.31
WM Letter-Word Identification	−0.35 (1.31)	−0.04	0.53***	−0.66 (2.34)	−0.08	1.68 (1.56)	0.20	0.53***	3.18 (2.85)	0.37
WM Dictation	0.24 (0.58)	0.05	0.56***	0.42 (0.99)	0.10	0.26 (0.69)	0.06	0.53***	0.44 (1.06)	0.10

Note: 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 food for thought meeting. Sample sizes range from $N = 216$ to 229 on end-of-treatment outcomes and $N = 94$ to 102 on 5-month follow-up outcomes (cohort 1 only).

Abbreviations: IDELA, International Development and Early Learning Assessment; ITT, intent-to-treat; TOT, treatment-on-the-treated; WM, Woodcock–Muñoz battery of tests.

$+p < .10$

$*p < .05$; $***p < .001$.

TABLE 5 Impacts on child math, EF and 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 (0.02)	0.03	0.62***	0.01 (0.03)	0.04	0.02 (0.02)	0.10	0.57***	0.03 (0.04)	0.18
EF	0.03 (0.05)	0.08	0.60***	0.04 (0.09)	0.13	0.05 (0.06)	0.15	0.57***	0.09 (0.10)	0.27
Math-ATL	−0.08 (0.06)	−0.23	0.65***	−0.12 (0.09)	−0.35	−0.04 (0.07)	−0.11	0.60***	−0.07 (0.11)	−0.20
EF-ATL	0.04 (0.07)	0.09	0.61***	0.07 (0.11)	0.14	0.05 (0.09)	0.10	0.54***	0.10 (0.16)	0.20
Overall ATL	0.13 (0.08)	0.20	0.62***	0.22 (0.13)+	0.32+	0.22 (0.10)*	0.33*	0.58***	0.39 (0.17)*	0.58*
5-month Follow-up (cohort 1 only)										
Math	0.00 (0.02)	0.00	0.56***	0.00 (0.04)	0.00	−0.01 (0.03)	−0.05	0.59***	−0.01 (0.04)	−0.08
EF	0.12 (0.06)*	0.33*	0.57***	0.22 (0.10)*	0.58*	0.14 (0.09)	0.37	0.60***	0.23 (0.14)	0.61
Math-ATL	0.15 (0.08)+	0.30+	0.57***	0.27 (0.14)+	0.55+	0.26 (0.12)*	0.53*	0.61***	0.47 (0.20)*	0.95*
EF-ATL	0.09 (0.08)	0.16	0.55***	0.18 (0.15)	0.31	0.11 (0.15)	0.18	0.55***	0.22 (0.27)	0.38
Overall ATL	0.03 (0.09)	0.05	0.58***	0.06 (0.16)	0.09	0.23 (0.12)+	0.36+	0.62***	0.41 (0.18)*	0.63*

Note: 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 Woodcock–Muñoz outcomes. We defined compliers as parents who attended at least one food for thought meeting. Sample sizes range from $N = 170$ to 229 on end-of-treatment outcomes and $N = 94$ to 102 on 5-month follow-up outcomes (cohort 1 only).

Abbreviations: ATL, approaches to learning; EF, executive function; ITT, intent-to-treat; TOT, treatment-on-the-treated.

+$p < .10$.

*$p < .05$; ***$p < .001$.

effect sizes for the treatment on the treated effect were substantial ($d = .57$ at end-of-treatment) and are similar to those reported by meta-analytic work (Manz et al., 2010) on home-based interventions (mean $d = .47$, range = .39–.55) and markedly higher in magnitude than those targeting children from minority (mean $d = .16$, range = .07–.23) and low-income backgrounds (mean $d = .14$, range = .04–.24). We found some evidence these benefits persisted several months after intervention completion (5-month follow-up). These findings suggest that when improvements in children's competences are embedded in valued and existing ecocultural 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 RCT studies. In particular, our 5-month follow-up findings for cohort 1 for vocabulary showed some sensitivity to whether or not we included covariates. In an RCT, the purpose of covariates is to increase the precision of estimates; if random assignment produced balanced groups (as it appears to have in our study), covariates are not needed in impact models to obtain unbiased treatment estimates (Murnane & Willett, 2010). In such cases, treatment effects obtained with and without covariates are considered equally unbiased. But if their substantive interpretation differs—as in our case, with the 5-month follow-up findings for cohort 1 for vocabulary—those results should be interpreted more cautiously. Replication is needed in such cases to determine the treatment effect.

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 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).

The lack of impacts on literacy outcomes in our study might be surprising, given that dosage data indicated that parents implemented FFT authentic-writing-reading strategies as often as they implemented narrative 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. Furthermore, 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., WM 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 aligned with the intervention are more susceptible to improvement and there are potential problems with expecting program effects on standardized tests that children have not been previously exposed to (Hill et al., 2008).

Regarding the lack of math impacts, there are several potential explanations. The first one is low dosage; parents reported implementing FFT math-talk strategies at home less often (once per week) than narrative and authentic-writing-reading strategies (a few days per week). The second is variability in the complexity of parental math talk. FFT promoted math talk but did not explicitly instruct parents to challenge children by going slightly above their current level of skills (e.g., counting beyond 10 if the child already counted to 10). A recent RCT showed that the effectiveness of home math interventions depends on the complexity of parental math talk relative to children's current level of skills (Gibson et al., 2020). The third is variability in levels of parental math perceptions. A recent study found that engaging in math talk at home predicted math skills, but only for parents who had positive math perceptions (Cosso et al., 2021).

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 understand intervention effectiveness (or lack thereof), 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. Prior evidence suggests that the practices targeted by FFT might promote such outcomes and, these outcomes are ecocultural assets that Latino children bring to school, which support academic learning (Bustamante & Hindman, 2020; Galindo & Fuller, 2010). Children in the FFT condition showed larger overall ATL improvements compared to those in the control condition at end-of-treatment and the 5-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 5-month follow-up, though some of these findings were sensitive to covariate inclusion versus exclusion. The treatment on the treated effect sizes (d = .38–.95 in model 2) are somewhat larger in magnitude to cluster-randomized interventions targeting self-regulatory skills in preschoolers (d = .37–.43; e.g., Raver et al., 2011).

Food for thought promoted authentic reading and writing, enjoyable and meaningful activities that are known to increase children's motivation, persistence, and engagement in writing tasks (Parsons & Ward, 2011). It was possible that motivation and engagement in one "challenging" task (writing) transferred to other similarly challenging tasks (learning tasks). This may be why children in the FFT condition increased their overall motivation, persistence, and engagement in learning activities (i.e., ATL) compared to their peers. FFT promoted narratives, which are known to be opportunities to develop mind-mindedness (verbal tools to control attention and behavior; Bernier et al., 2010). Because the authentic writing and reading activities promoted by FFT (e.g., making and using a grocery list while shopping) required children to plan, follow directions, and control their behavior and attention, it is possible that these activities also contributed to facilitate children's executive function.

Future directions

In future work, it might be important to revise our theory of change to include executive function and ATL as confirmatory outcomes and revise our curriculum to more intentionally discuss with parents how the practices promoted by FFT can improve the "cognitive and social building blocks" of school success (executive function, ATL). A study found that kindergarten teachers in the United States consider executive function and ATL to be more critical for children's successful transitioning into school than skills such as knowing most of the alphabet and counting (Lin et al., 2003). Including strategies promoting science learning during food routines is a promising future avenue. Children's executive function predicts growth in science achievement (Anthony & Ogg, 2020), and food activities afford discussions about scientific concepts. It would be important to determine whether and how FFT's perceived value has changed. Data from the feasibility study indicated that parents perceived the intervention as doable (did not take much time or effort), enjoyable (did not feel like schoolwork), and closely aligned with their cultural values (i.e., akin to *consejos*, homilies with indirect teaching and nurturing advice; Leyva & Skorb, 2017).

Limitations

Our study has some limitations, the first and foremost of which is statistical power. Our study was designed to have reasonable power given FFT's stage of development but due to circumstances beyond our control (e.g., COVID-19 pandemic), power was lower than intended. Accordingly, we run the risk of both Type II errors as well as inflated effect sizes (Anderson et al., 2017). In light of this, we have taken pains to implement a number of robustness checks to assess the sensitivity of findings to different specifications (including MI). We are careful to caveat any signs of sensitivity of estimates to modeling decisions and we highlight repeatedly that findings need replication in larger-scale study appropriate for the next phase of FFT's development and testing. Second, this study involved Latino families from low-income households who recently immigrated to the United States from Mexico and Central America. Caution should be exercised when generalizing findings to the greater Latino community in the United States. Third, 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 languages might provide a more complete picture of the trajectories of growth in Latino kindergarteners' skills. Fourth, ATL was measured via assessor surveys; in future work, it would be important to incorporate parent/teachers as informants; although several well-cited rigorous studies in early childhood education have also relied on assessors' rather than parent/teachers' reports (e.g., Raver et al., 2011; Weiland & Yoshikawa, 2013). Fifth, due to resource limitations, we did not include pre- and post-test measures of parent–child narratives, authentic writing and reading, and math talk (the main mechanisms through which FFT influenced child outcomes). Future work should include such measures. Sixth, due to substantial missing data, we were not able to include covariates at the child level beyond age and gender or at the parent/family level (e.g., parent education). Previous early childhood RCTs have found that pretests—which we do include—explain far more residual variance in children's outcomes and thus add more precision to estimates than do child and family covariates (e.g., Bloom & Weiland, 2015). But including additional child and family covariates could have potentially increased our precision, as well as permitted additional balance check work. Finally, we did not measure whether parents continued using the intervention strategies between the end-of-treatment and 5-month follow-up. Future studies should collect this information.

CONCLUSION

Testing strengths-based, culturally responsive interventions rigorously, as we do here, is essential for delivering on the promise of this approach for the fastest growing child demographic in the United States. If validated in larger trials, FFT has potential implications for policy. Title I schools across the United States are aware of the importance of culturally sustaining supports to bridge home and school learning. At the same time, many schools feel under-prepared to build and maintain such supports (Mapp & Kuttner, 2013). By focusing on families' ecocultural assets, FFT has the potential to mold aspects of the Latino community's practices into durable, compounding improvements in critical outcomes that matter to school and life success while respecting and elevating the richness of Latino family life.

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