

# Natural Window of Opportunity? Low-Income Parents' Responses to Their Children's Impending Kindergarten Entry

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*Parental investments in the activities and materials that drive learning are central to young children's school readiness and life success. Little is known, however, about how parents adjust these investments in response to outside pressures, including their children's impending entry into kindergarten. In the present study, we employ two analytical strategies (multilevel residualized change and regression discontinuity) within national data from the Head Start Impact Study to examine whether parents of children facing an impending entry to kindergarten invest more time and materials in their children's language and literacy skill development compared with parents of otherwise similar children who are not yet facing formal school entry. Results suggest that low-income parents react to the impending kindergarten transition by increasing their provision of parent-child language and literacy activities ( $d = .15$ ) but not related materials. We discuss the implications of our findings for the timing of parenting interventions.*

**Keywords:** *Head Start, kindergarten transition, language and literacy, parenting*

PARENTAL investments in preschoolers' opportunities for learning—including their provision of cognitively stimulating activities and material resources—are important predictors of young children's school readiness. Previous research has extensively documented that these early investments are consequential for children's later success in elementary school and beyond (Chatterji, 2006; Miedel & Reynolds, 2000). At the same time, additional work suggests that despite considerable variation in investments within social class (Rodriguez & Tamis-LeMonda, 2011), lower-income parents, on average, invest less in language and literacy learning activities and materials for young children, relative to their higher-income parents (Duncan & Murnane, 2011; Hart & Risley, 1995; Raikes et al., 2006), although these gaps shrank somewhat between 1998 and 2010 (Bassok, Finch, Lee, Reardon, & Waldfogel, 2016).

Parenting and parental investments, however, are not fixed. Rather, they change over time in response to children's developmental stages and temperament (Leyva, Tamis-LeMonda, Yoshikawa, & Jimenez-Robbins, 2015). In the

present study, we investigated whether parental investments also change in response to factors *outside* the child and family, a topic about which very little is known empirically, particularly using large samples and rigorous research designs. Specifically, we examined whether low-income parents of preschool-age children increased their language and learning activities and materials investments in the months leading up to a normative experience for most young U.S. children: the transition to kindergarten.

To do so, we employed two different analytical strategies (multilevel residualized change and regression discontinuity [RD]) within the national Head Start Impact Study (HSIS). Essentially, we explored whether parents within this low-income sample experienced a natural pressure to prepare their children for kindergarten and changed their provision of material and social resources accordingly. If so, we posited that leveraging this “natural window of opportunity” could lead to increases in low-income parents' investments in their children's early learning and to reduce income-based gaps in children's school readiness skills.



### Importance and Timing of Early Support for Learning in the Home

A large body of research has illuminated the importance of the different forms of parental investments in their children's early learning, for the purpose of building children's human capital. For example, parents' involvement in preschool-age children's home-based learning, such as making space and time for learning at home, has been shown to relate more strongly to child outcomes across domains (vocabulary, problem behaviors, and learning behaviors) than either school-based involvement (e.g., volunteering in the classroom) or home-school conferencing (e.g., communicating about progress; Fantuzzo, Perry, & McDermott, 2004). In the language and literacy domain in particular, increased parent-child time spent on book reading, letter sounds, letter knowledge, conversations, and other language and vocabulary *activities* are predictive of improvements in children's language and literacy skills in the early childhood period (Bus, van IJzendoorn, & Pellegrini, 2009; Lonigan & Whitehurst, 1998; Weigel, Martin, & Bennett, 2006; York & Loeb, 2014). Specifically, parent-child joint reading is predictive of young children's vocabulary and comprehension skill development, and parents' decoding-focused activities with their young children is predictive of their word reading in first grade (Sénéchal & LeFevre, 2002). Language and literacy learning *materials* matter as well, in that they provide opportunities for meaningful adult-child interaction and support the development of children's early literacy skills, intrinsic motivation, and positive attitudes toward learning, all of which are important predictors of later reading achievement (Gottfried, Fleming, & Gottfried, 1998). The richness of these home literacy resources may also interact positively with parents' activities with children, as previous work has shown home language materials to be positively associated with higher frequencies of mother-child and sibling-child literacy activities among low-income families (Farver, Xu, Lonigan, & Eppe, 2013).

Overall, research has found that parenting and family factors, such as parenting behaviors, poverty, and family structure, explain more variance in children's literacy skills both at kindergarten entry (Magnuson, Meyers, Ruhm, & Waldfogel, 2004) and at age 12 (Belsky et al., 2007) than do their formal educational experiences in preschool. At the same time, although substantial variation exists (Rodriguez & Tamis-LeMonda, 2011), young children from low-income families are exposed to lower levels of average cognitive stimulation in their homes than their more socioeconomically advantaged peers, including less natural language exposure, language complexity, and book reading (Hart & Risley, 1995; Raikes et al., 2006). Lower provision of language-rich home environments and early learning materials and opportunities have been linked with striking short- and long-term disparities in children's literacy and vocabulary

skills, particularly within low-income populations (Aikens & Barbarin, 2008; Christian, Morrison, & Bryant, 1998; Dickinson & DeTemple, 1998; Hart & Risley, 1995; Hill, 2001; Payne, Whitehurst, & Angel, 1994).

Given these disparities, an important question emerges: Are low-income parents' investments in their children's learning fixed, or do they show evidence of naturally occurring fluctuation that might be further leveraged through intervention? To date, several studies have found that parents' responsiveness, sensitivity, and harshness do indeed change over time as a function of children's developmental stage and temperament (see Leyva et al., 2015, for a review). Less is known, however, about whether and how the timing of factors *outside* the child affect parents' behavior and investments in their children. The work that does exist remains limited by descriptive methods and small, nonrepresentative samples and may be conflating parents' responses to children's developmental changes with external changes. For example, although several studies report increases in parents' daily book reading and library visits as children enter the preschool period (Dickinson & DeTemple, 1998; Raikes et al., 2006), it is unclear whether these changes can be attributed to children's increased engagement with and interest in books over time versus parents' explicit intention to prepare children for a more formal academic setting. Another study found that between preschool and kindergarten, parental involvement and provision of home learning materials remained stable, the variety of out-of-home experiences parents provided their children increased, and cognitive stimulation at home decreased (Powell, Son, File, & Froiland, 2012), a pattern that could be explained either by children's developmental changes or by parents' responses to child entry into preschool and/or impending transitions to kindergarten. Suggestive that outside factors may affect parents' behavior and investments in their children, one survey study found that more than 20% of low-income parents reported responding to their children's impending school transition through engaging in more literacy and language learning activities at home (Ramey, Lanzi, Phillips, & Ramey, 1998).

Beyond this suggestive empirical evidence, there are theory-driven reasons to expect time-varying responses in parents' investments in response to the outside pressure of impending kindergarten entry. From a motivational perspective, socioemotional selectivity theory posits that limits on time provide the framework within which individuals select and prioritize goals (Carstensen, 2006; Carstensen, Isaacowitz, & Charles, 1999). Applied to the present study, this theory suggests that the parents of a preschool-age child may make different choices when kindergarten entry is around the corner (i.e., when time is short) versus when it is years away, irrespective of children's developmental changes during this time period. Parents of young children may, for

instance, begin to invest more of their available time and money for promoting children's letter identification and early writing skills as kindergarten approaches. In the absence of an impending kindergarten transition, the same parents—particularly within a low-income setting—may instead have invested their resources in meeting more urgent family needs or in pursuing their own longer-term goals, such as searching for a better-paying job or furthering their own education.

Empirically, some work has demonstrated that despite school readiness gaps by family socioeconomic status, low-income parents in fact place *more* emphasis on their children's preacademic skills (e.g., knowing the letters of the alphabet) than higher-income parents (Stipek, Milburn, Clements, & Daniels, 1992). Parents of Head Start children tend to place even more emphasis on such skills than other low-income parents (Piotrkowski, 2004). Accordingly, timing may matter for low-income parents' investments in their children's language, both because of their particularly strong emphasis on preacademic aspects of school readiness and because of naturally occurring, externally imposed time limits to attain these school readiness skills.

### *The Present Study*

Despite a large and growing body of research suggesting the importance of parental investment in language and literacy learning opportunities for children's school readiness, very few methodologically rigorous studies have examined whether low-income parents change their investments to better prepare their children for kindergarten. In the present study, we add to the literature on parents' home support for early learning by examining whether low-income parents increase their investments in language- and literacy-related activities and materials in the home as their child approached kindergarten entry, independent of other confounding characteristics, such as children's age or enrollment in formal school readiness or transition programs. Specifically, we hypothesize that parents whose children are approaching kindergarten will increase their provision of learning-related activities and materials in the home over the course of the year significantly more than otherwise similar parents of children who are not immediately facing a transition to elementary school. In particular, we expect that parents will increase their provision of parent-child language and literacy activities to a greater extent than they will their provision of language and literacy materials, as the former are less subject to monetary constraints experienced (by definition) by most low-income families.

Exploring this research question is important for two reasons. First, understanding if, how, and why low-income parents' investments in their young children's early language and literacy skills change over time is critical for providing a more complete picture of the kindergarten transition—and

externally imposed pressures more generally—from the perspective of a particularly vulnerable and underrepresented group. Second, if low-income parents do show a natural response to impending kindergarten entry, this may suggest that parenting interventions may be more effective (and cost-efficient) when implemented during certain “windows” of time when caregivers are naturally open to and motivated for change.

In addition to these conceptual contributions, this study also attempts to address several methodological limitations of previous work on kindergarten transitions and parental involvement. In particular, we use data from the large-scale, national HSIS, which provides a more diverse and representative sample of low-income parents than has been used in most previous work. Analytically, we employ two complementary approaches—multilevel residualized change modeling and a quasiexperimental RD approach—and perform an extensive set of checks on the sensitivity of our findings to threats to internal validity. Because the RD approach compares children who are more or less the same age, we are better able than previous work to eliminate the competing hypothesis that any detected differences in investment are due to parents' response to children's development rather than the hypothesized externally imposed pressure of children's impending kindergarten entry.

## **Method**

### *Sample*

The present study sample represents a subset of families who participated in the national HSIS in 2002–2003. The HSIS randomized 4,667 eligible 3- and 4-year-old first-time Head Start applicants from a national sample of 378 over-subscribed Head Start centers (Puma, Bell, Cook, & Heid, 2010). These children were randomly assigned to Head Start (the study's treatment group) or to a control group whose members could not enroll in the Head Start center in which they were randomized. The HSIS restricted-use file, which is the basis for the present analysis, omits sample members from Puerto Rico, leaving 4,440 children from 351 Head Start centers in 22 states.

To be included in the present analytical sample, valid data on at least one parental investment outcome were required. Because we use an age-cutoff RD as one of our analytical strategies, valid child date of birth information was also required, and the parent and child had to reside in a state with a statewide birthday cutoff (see Figure 1 for a study sample selection tree and see online Appendix A, Table A1, for state cutoff descriptive information). After applying these criteria, our sample totaled 2,760 children and their parents in 16 states (62% of parents and 72% of states in the HSIS). On the basis of data available from the National Institute for Early Education Research and the U.S. Census (Barnett, Robin,

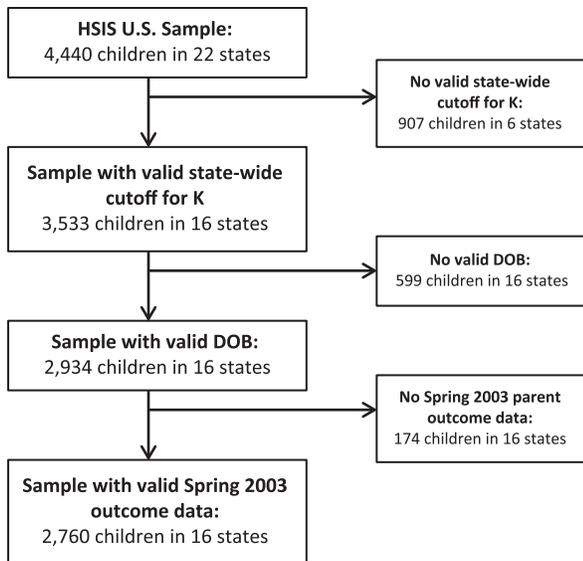


FIGURE 1. *Sample selection tree.*

Hustedt, & Schulman, 2003; U.S. Census Bureau, 2000), included states were similar to excluded states on nine different relevant characteristics (state has public pre-K, mean teacher salary, percentage of children age 3 or 4 in pre-K, percentage of teachers with a BA, percentage of English-speaking adults, percentage unemployed, median family income, percentage of children under 5 in poverty, and percentage of adults with at least a high school diploma; see online Appendix A, Table A2). Demographic characteristics of the final analytical sample are available in Table 1.

### *Procedures*

In the fall of 2002 and spring of 2003, a parent or primary caregiver living with and responsible for raising the focal child was interviewed in person in the home. Interviews were available in both English and Spanish, and English-Spanish bilingual speakers were hired to conduct interviews with Spanish-speaking parents (Puma et al., 2010). During home visits in the fall, parents provided all information relevant to the present study, including child and family sociodemographic characteristics, as well as baseline values for the two primary outcomes of interest: parent-child activities and materials related to language and literacy. Information on language and literacy activities and materials was also reported by parents at the follow-up visit conducted in the spring of 2003.

### *Measures*

*Impending kindergarten.* We formed our key analytical variable—impending kindergarten entry—by using the age cohort variable from the HSIS data set. Children in the HSIS

were assigned to the age 3 versus the age 4 cohort on the basis of their birth date relative to their state’s age cutoff for kindergarten. Specifically, the impending-kindergarten variable took on a value of 1 if the child was in the age 4 cohort (1 year away from kindergarten, or “impending K”) and 0 if the child was in the age 3 cohort (2 years away from kindergarten, or “non-impending K”).

In addition to the dichotomous impending-K variable taken from the original HSIS data set, we used state cutoff information (Education Commission of the States, 2004) to form a continuous predictor to measure how many days from the cutoff the child’s birthdate fell, centered around the cutoff within each state. This predictor was the “forcing variable” in our regression discontinuity analysis—the clear cut point that we treated as the exogenous determinant of whether kindergarten was impending or not for the children of parents in our sample (Lee & Lemieux, 2010). Positive integer values indicated the child was born before the state’s cutoff, and negative, after. A value of 0 indicated that the child was born on the cutoff date. An empirical plot demonstrating the overlap between the continuous forcing variable and the dichotomous impending-K status variable is shown in Figure 2.

To protect child and family privacy, children’s specific day of birth (DOB) is not available in the HSIS, although month and year of birth are available. In calculating our forcing variable, we assumed each child was born on the 15th of the month (i.e., the average day value for most months). This decision minimized the error with which DOB was measured in our study. We examined the sensitivity of our results to this decision, as explained in a later section.

*Outcomes.* Drawing from the parent interview data, we conceptualized two distinct dimensions of parental investment in early language and literacy. In doing so, we created two primary parenting measures: frequency of parent-child language and literacy activities, and provision of language and literacy materials. The first outcome variable, parent-child language and literacy activities, is a composite score defined by six parent-reported indicators of their dyadic, reciprocal interactions with children and joint attention on literacy. There were other parent-child language and literacy activity indicators that we chose not to include, either because (a) they were collected in spring only and therefore did not fit well with our residualized change approach (eight items) or (b) they focused on how often the *child* initiated the activity and did not fit with our conceptual focus on parents (two items). The included six indicators included how often a parent or someone in the family (a) read to child, (b) retold or made up stories, (c) worked on learning names of letters or words, (d) practiced writing the alphabet, (e) practiced writing and spelling name, and (f) practiced rhyming words. All indicators with the exception of (a) included a 6-point Likert-type ordinal response scale: *never* (= 0), *once a month or*

TABLE 1  
*Descriptive Statistics by Impending Kindergarten (K) Status*

Outcome	Non-impending K ( <i>n</i> = 1,472)	Impending K ( <i>n</i> = 1,288)	Difference statistically significant?	Percentage missing
	<i>M</i> ( <i>SD</i> )	<i>M</i> ( <i>SD</i> )		
Language and literacy activities	0.55 (0.23)	0.61 (0.22)	**	0
Language and literacy materials	0.73 (0.21)	0.72 (0.21)	<i>ns</i>	0.18
Baseline scores				
Language and literacy activities	0.52 (0.22)	0.56 (0.22)	**	0.04
Language and literacy materials	0.69 (0.22)	0.68 (0.22)	<i>ns</i>	0.04
Baseline characteristics and parent survey timing variables				
W-J LW score	293.18	306.46	****	9.46
Treatment assignment	0.63	0.62	<i>ns</i>	0
Distance from state age cutoff (in days)	162.73 (127.72)	-162.69 (116.92)	<i>ns</i>	0
Month of baseline survey	4.30 (0.76)	4.35 (0.79)	*	0
Days between surveys	164.42 (33.07)	164.86 (32.20)	<i>ns</i>	0.54
Child age (years) at time of testing	3.65 (0.49)	4.50 (0.51)	**	1.59
Child is male	0.47	0.50	<i>ns</i>	0
Child is Black	0.37	0.20	**	0
Child is Hispanic	0.35	0.46	**	0
English home language	0.74	0.62	**	0
Mother's education < HS	0.35	0.44	**	0.76
Mother's education = HS	0.32	0.29	<i>ns</i>	0.76
Mother is married	0.42	0.48	**	0.72
Mother was previously married	0.14	0.18	**	0.72
Child tested in English	0.77	0.64	**	1.59
Caregiver age	28.66 (7.42)	29.43 (6.98)	**	0.22
Biological father lives with child	0.47	0.52	**	0.14
Mother is a recent immigrant	0.15	0.23	**	0.14
Mother was a teenager at child's birth	0.16	0.17	<i>ns</i>	0
Neighborhood resources	67.38 (65.74)	80.62 (71.05)	**	0
Neighborhood poverty	26.33 (15.24)	23.45 (13.91)	**	0
Neighborhood population density	3,280.71 (5,165)	4,532.74 (6,335)	**	0

Note. W-J LW = Woodcock-Johnson Letter-Word Identification; HS = high school.  
 \**p* < .05. \*\**p* < .01. \*\*\**p* < .001. Statistical significance of differences based on *t* tests.

less (= 1), two to three times a month (= 2), once or twice a week (= 3), three to four times a week (= 4), and every day (= 5). For reading frequency (a), a 4-point Likert-type ordinal response scale was used: *not at all* (= 0), *once or twice per week* (= 1), *three or more times* (= 2), and *every day* (= 3). We used a linear transformation to place (a) on a 6-point scale. Internal consistency of this scale was acceptable, with a Cronbach's alpha of .79.

The second parent outcome, language and literacy materials, is a scale composed of eight survey indicators highlighting whether particular reading materials were available at home. Parents were asked on a response scale of yes (= 1) or no (= 0) which of the following items they had in their home: (a) books for children; (b) magazines for children; (c)

magazines for adults, like *Newsweek* or *People* or *Sports Illustrated*; (d) newspapers; (e) catalogs; (f) religious books, like the Bible or prayer book; (g) dictionaries or encyclopedias; and (h) other books, like novels or biographies or non-fiction. Parents were also asked about comic books, but this item had a lower loading than other items, and psychometrics of the construct were improved without it (results available upon request). The Cronbach's alpha of this measure was .60.

Confirmatory factor analysis was used to verify the unidimensionality of our primary outcome measures, as well as their configural invariance across Spanish- and English-speaking children, the HSIS treatment and control groups, and the 3- and 4-year-old cohorts. Fit statistics were good

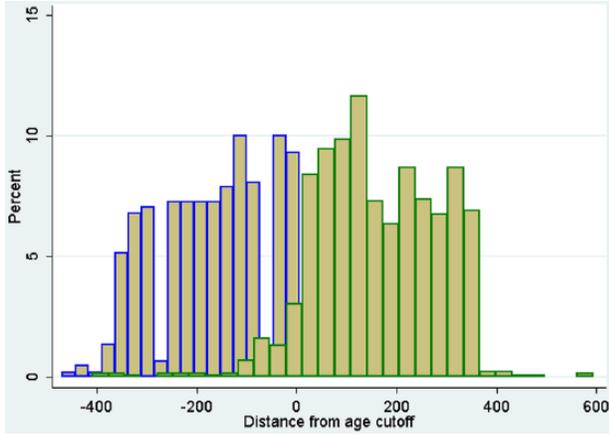


FIGURE 2. Sample histogram of the density of observations around the age cutoff on the forcing variable ( $N = 2,760$ ). Non-impending-kindergarten sample shown in blue and impending-kindergarten sample shown in green.

overall, although there was some suggestion of non-invariance for both outcomes by home language (for details, see online Appendix A, Table A4).

Indicators were unit weighted and averaged within each measure to create composite scores ranging from 0 to 1, where higher scores indicated greater frequency of language and literacy activities and materials. Descriptive details of all outcomes are available in Table 1.

*Covariates.* Covariates were drawn from the HSIS baseline parent survey. Most of our selected covariates matched those used in the original HSIS evaluation (Puma et al., 2010) and several subsequent studies using these data (Bloom & Weiland, 2015; McCoy, Connors, Morris, Yoshikawa, & Friedman-Krauss, 2015). This set included child gender, child race, home language, mother education, mother marital status, primary caregiver age, whether the child was tested in English at baseline, whether the child lived with both biological parents, whether the mother was a recent immigrant, and whether the mother was a teenager at the time of the child's birth. In the event that parents' responses were affected by survey timing, we also included controls for the time between the fall and spring parent interviews as well as the month of parent interview in spring. To account for differences in children's baseline skill levels, we also controlled for children's fall early reading score (as measured by the Woodcock-Johnson Letter-Word Identification subtest; see Puma et al., 2010). Next, we controlled for whether the mother was the respondent to both the fall and spring surveys. Finally, although we were not interested in the impacts of random assignment to Head Start on the parenting outcomes used in the present study, we recognized its potential to influence parent outcomes and, accordingly, controlled for it in our analyses.

### Analytical Approach

*Residualized change.* To answer our research question, we used two different but complementary analytical approaches: multilevel residualized change regression and RD. For the former, we fitted a multilevel regression model with random intercepts for the Head Start center at which parents and children originally sought care:

$$\begin{aligned} \text{Springparent}_{ij} = & \beta_0 + \beta_1 \text{ImpendK}_{ij} + \\ & \beta_2 \text{Fallparent}_{ij} + \gamma \text{State}_j + \delta Z_{ij} + \epsilon_{ij} + \mu_j, \end{aligned} \quad (1)$$

where  $i$  denotes parents and  $j$  denotes center, Springparent is a parent-level outcome (parent-child language and literacy activities or language and literacy materials), ImpendK is a dichotomous variable that denotes whether the child was expected to enter kindergarten in fall 2003, Fallparent is the relevant score on parents' language and literacy activities or materials, State is a vector of fixed effects for parents' state of residence,  $Z$  is a vector of covariates (child and family characteristics, time between fall and spring interviews, month of parent spring interview, child treatment status, child fall Woodcock-Johnson Letter-Word Identification subtest score, whether mom was the respondent in fall and spring interviews),  $\epsilon$  is a child-level error term, and  $\mu$  is a random intercept for center. The coefficient on ImpendK and its associated  $p$  value identified whether there was a statistically significant association between children's impending kindergarten entry and the change in parents' activities or materials for their children leading up to kindergarten entry.

*RD.* To estimate the effect of impending kindergarten on the key parent outcomes using an RD approach, we took advantage of study procedures in the HSIS regarding whether children were placed in the study's 3- or 4-year-old cohort, representing their impending entry into kindergarten. Within states with a statewide kindergarten cutoff, we assumed that assignment to impending kindergarten entry was exogenous around the cutoff. Provided this assumption was correct and that we met the requirements of RD, any differences in the parenting outcomes in spring 2003 between parents of children whose birth dates fell just to one side of the cutoff versus the other provided estimates of the effect of impending kindergarten on the language and literacy activities and materials parents offered their children. Using the standard RD design (Imbens & Lemieux, 2008; Shadish, Cook, & Campbell, 2002; Thistlethwaite & Campbell, 1960), data of children remote from the birthday cutoff were used to project back more precisely the value of the outcome on each side of the cutoff for parents of children whose birthdays fell in the immediate vicinity of the birthday cutoff for their state.

Analytically, to implement the RD approach, we simply added the forcing variable—the child's age from the state

kindergarten cutoff for their state, centered on zero—to Equation (1). The  $\text{ImpendK}$  coefficient and its associated  $p$  value was the parameter of interest, identifying the effect of children’s impending kindergarten entry on parent–child activities or materials related to language and literacy leading up to kindergarten. We fitted the model within different bandwidths from the cutoff to balance bias (i.e., influence from parents far from the cutoff) with precision (i.e., a larger window provides a larger sample). We used three bandwidths across all RD analyses (365, 180, and 90 days on either side of the cutoff), in addition to an outcome-specific “optimal” bandwidth determined using the cross-validation (CV) procedure (Imbens & Lemieux, 2008; Lee & Lemieux, 2010). The CV procedure balances precision and bias by identifying the bandwidth that minimized the mean squared error of prediction at the cutoff. We conducted a host of other robustness checks—some standard in the literature and others unique to the present study—to examine the validity of our identification strategy and sensitivity of our results.

In fitting all our regression models, we used multiple imputation (with 20 imputations) to account for missing data on independent variables, following Graham (2009). Table 1 presents data on the original levels of missingness in our analytical sample. As shown, missingness levels were low (0% to 9.5%).

*RD assumptions and complementarity of the two analytical approaches.* Importantly, one design feature of the HSIS presents challenges for our RD approach and deserves further elaboration: The HSIS included only children who were new to Head Start. Accordingly, assuming parents were perfectly aware of the state kindergarten cutoff rules, implications for their child’s kindergarten entry date, and Head Start policies regarding placement into the age 3 versus age 4 cohort, parents of children in the 4-year-old cohort were intentionally applying for 1 year of Head Start, whereas those in the 3-year-old cohort were intentionally applying for 1 or 2 years of Head Start. Parents of children in the 4-year-old cohort by definition were most likely eligible for Head Start the year before fall 2002 but were waitlisted or chose not to apply and/or to take it up. Accordingly, they could have been systematically different in terms of their beliefs about preschool, their child care needs, or many other factors than those who wanted Head Start at age 3 (the 3-year-old cohort). We found empirical hints of some substantial differences in the two cohorts, with statistically significant average differences on 17 out of 22 baseline characteristics (see Table 1) for the full sample of 3-year-olds versus 4-year-olds.

The internal validity of our RD estimates depends on whether, despite these average differences, parents of children *close to the cutoff* in either cohort were not precisely aware of kindergarten entry policies and their implications for the number of years their child would be eligible for

Head Start. Conceptually supporting this assumption, parents of children close to the cutoff for their state applied to the same center-based preschool program when their children were approximately the same age—that is, they viewed their child as “ready for preschool” around the same time. Empirically, there were also children in both age cohorts (5% of the sample) who ended up in the wrong kindergarten cohort, signaling that birth date was not always the sole determination of children’s entry into kindergarten (most state pre-K program likewise show similar wiggle room in kindergarten entry; Wong, Cook, Barnett, & Jung, 2008). Formal tests of balance of participant characteristics at the cutoff—a key criterion for internally valid estimates—suggest that our strong assumption that children on either side of the cutoff were equivalent in expectation is reasonable. We found no difference across the cutoff on any of the 24 parent and child characteristics tested using a bandwidth, or window, of 365 days; on 23 out of 24 characteristics for a bandwidth of 180 days; and on 22 out of 24 characteristics for a bandwidth of 90 days (see online Appendix B, Table B1).

Nonetheless, we fully acknowledge that there may have been differential entry into the impending-kindergarten cohorts that threatens the internal validity of our RD results. We accordingly consider our RD estimates not to be causal estimates but, rather, an alternative and likely more rigorous version of our residualized change approach. In our view, the RD estimates, because they are based on children who are the same age, better help to eliminate the confounding of parents’ reactions in response to impending kindergarten versus to children’s own increases in their interest in language and literacy materials and activities due to natural developmental changes. But the two approaches also yield estimates for different populations (i.e., the full population with residualized change approach versus those just at the cutoff with RD), making any conflicts in their results difficult to resolve. Ultimately, we viewed consistency of results across the two approaches as stronger evidence of the relation between impending kindergarten and parents’ investments than results from one approach alone, and we viewed both as suggestive—rather than definitive proof—of parents’ reactions to impending kindergarten.

## Results

As shown in Table 1, parents reported engaging in more frequent parent–child language and literacy activities and providing more language and literacy materials in the spring of 2003 than at baseline, regardless of impending-kindergarten status.

Results of our residualized change and RD analyses are shown in Table 2. We found that having a child for whom kindergarten was impending was positively and significantly associated with modest increases in parents’ language and literacy activities with their children and with

TABLE 2

*Relation Between Impending Kindergarten (K) Entry and Spring Parenting Outcomes*

Variable	RC	RD			
		BW = 365	BW = 180	BW = 90	BW = CV determined
Language and literacy activities					
Impending-K coefficient	0.032**	0.035**	0.043*	0.050*	0.043*
<i>SE</i>	(0.011)	(0.013)	(0.017)	(0.020)	(0.018)
Effect size	0.14**	0.15**	0.18*	0.21*	0.18*
<i>n</i> children	2,760	2,760	1,512	853	1,308
<i>n</i> centers	255	255	240	217	234
Language and literacy materials					
Impending-K coefficient	0.018*	0.014	0.019	0.008	0.007
<i>SE</i>	(0.009)	(0.011)	(0.014)	(0.017)	(0.015)
Effect size	0.09*	0.07	0.09	0.04	0.03
<i>n</i> children	2,755	2,755	1,509	852	1,305
<i>n</i> centers	255	255	240	217	234

*Note.* RC = residualized change; RD = regression discontinuity; BW = bandwidth in days; CV = cross-validation. In RD models, the functional form of the relation between child age and outcomes was specified as linear across bandwidths. For language and literacy materials BW = 180, in accordance with our functional form testing, we included an interaction between child age and impending kindergarten entry. All models included the following covariates: state fixed effects, time between fall and spring parent interviews, month of parent interview in spring, child fall Woodcock-Johnson Letter-Word Identification score, whether the mother was the respondent to the fall and spring surveys, Head Start treatment assignment, and the standard set of covariates in the Head Start Impact Study (mother age, child gender, child race, home language, mother's education, mother's marital status, whether the child was tested in English at baseline, mother's age, whether the child lived with both biological parents, whether the mother was a recent immigrant, and whether the mother was a teenager). All models included random intercepts for center of random assignment. Models also included controls for baseline scores on these same measures. CV-determined bandwidths were as follows: 149 for language and literacy activities and 157 for language and literacy materials.

\* $p < .05$ . \*\* $p < .01$ . \*\*\* $p < .001$ .

parents' provision of more language and literacy materials in spring ( $d = 0.14$  for activities;  $d = 0.09$  for materials). In the RD models, however, the impending-kindergarten boost remained statistically significant only for parents' engagement in language and literacy activities with their children. For this outcome, impending-kindergarten coefficients across four bandwidths were similar in magnitude to the residualized change coefficients (range in effect sizes of  $d = 0.15$  to  $0.21$ ). In the RD models, the impending-kindergarten coefficient was neither statistically significant nor stable in magnitude across bandwidths for language and literacy materials, with the largest impending-kindergarten effect size ( $d = 0.09$ ) being 3 times the magnitude of the smallest ( $d = 0.03$ ).

We extended our language and literacy activities analysis to examine relations between impending kindergarten entry and the six specific language and literacy practices that composed the overall construct (see Table 3; see online Appendix A, Table A3, for item-level descriptive statistics for these practices). These analyses were aimed at identifying if parents increased their home supports for language and literacy more in some areas than in others. We found that the uptick in activities was concentrated in parents' literacy-focused activities with their children: spelling/writing the child's name, writing the alphabet, learning letter

names, and rhyming, with spelling/writing the child's name as particularly salient relative to other activities. There was little evidence of an uptick in language-focused activities (e.g., reading books and telling stories).

#### *Robustness Checks*

We performed a host of robustness checks on our key findings to determine their sensitivity to alternative model specifications. These analyses targeted several standard, RD-specific threats to the internal validity of our results: (a) nonsmooth or discontinuous variation of observed and unobserved parent and child characteristics around the cutoff, (b) treatment misallocation at the cutoff, (c) discontinuities in the outcomes at points other than the cutoff, and (4) incorrect specification of the functional form of the relation between the outcome and the forcing variable. Our primary results and conclusions were robust to these threats. For parsimony, we discuss our analytical work on these threats and display our results in online Appendix B.

In addition to examining common threats to the RD design, we also examined whether associations between impending kindergarten and increased parent-child language and literacy activities might have been attributable not to internal psychological pressure felt by the parent, as we

TABLE 3

*Relation Between Impending Kindergarten (K) Entry and Specific Spring Parent Language and Literacy Activities*

Activity	RC	RD		
		BW = 365	BW = 180	BW = 90
<b>Read to child</b>				
Impending-K coefficient	-0.027	0.053	0.227*	0.199
<i>SE</i>	(0.073)	(0.091)	(0.115)	(0.139)
Effect size	-0.02	0.03	0.15*	0.13
<i>n</i> children	2,755	2,755	1,509	851
<i>n</i> centers	255	255	240	217
<b>Retold/made up stories</b>				
Impending-K coefficient	-0.099	-0.029	0.032	0.123
<i>SE</i>	(0.092)	(0.114)	(0.146)	(0.176)
Effect size	-0.05	-0.02	0.02	0.07
<i>n</i> children	2,749	2,749	1,508	850
<i>n</i> centers	255	255	240	217
<b>Worked on learning names of letters or words</b>				
Impending-K coefficient	0.104	0.188*	0.162	0.222
<i>SE</i>	(0.074)	(0.092)	(0.116)	(0.136)
Effect size	0.07	0.13*	0.11	0.15
<i>n</i> children	2,758	2,758	1,512	853
<i>n</i> centers	255	255	240	217
<b>Practiced writing the alphabet</b>				
Impending-K coefficient	0.248**	0.258**	0.242*	0.318**
<i>SE</i>	(0.078)	(0.096)	(0.123)	(0.150)
Effect size	0.15**	0.16*	0.15*	0.20*
<i>n</i> children	2,756	2,756	1,510	851
<i>n</i> centers	255	255	240	217
<b>Practice writing and spelling name</b>				
Impending-K coefficient	0.428***	0.348***	0.366**	0.474**
<i>SE</i>	(0.081)	(0.101)	(0.129)	(0.159)
Effect size	0.25***	0.20***	0.21**	0.27**
<i>n</i> children	2,745	2,745	1,506	849
<i>n</i> centers	255	255	240	216
<b>Practiced rhyming words</b>				
Impending-K coefficient	0.279**	0.187	0.258	0.264
<i>SE</i>	(0.087)	(0.108)	(0.137)	(0.161)
Effect size	0.16**	0.11	0.15	0.15
<i>n</i> children	2,758	2,758	1,512	853
<i>n</i> centers	255	255	240	217

*Note.* RC = residualized change; RD = regression discontinuity; BW = bandwidth in days; CV = cross-validation. In RD models, the functional form of the relation between child age and outcomes was specified as linear across bandwidths. For practice writing the alphabet and practice writing and spelling name, in accordance with our functional form testing, we included an interaction between child age and impending kindergarten entry for BW = 90. All models included the following covariates: state fixed effects, time between fall and spring parent interviews, month of parent interview in spring, child fall Woodcock-Johnson Letter-Word score, whether the mother was the respondent to the fall and spring surveys, Head Start treatment assignment, and the standard set of covariates in the Head Start Impact Study (mother age, child gender, child race, home language, mother's education, mother's marital status, whether the child was tested in English at baseline, mother's age, whether the child lived with both biological parents, whether the mother was a recent immigrant, and whether the mother was a teenager). All models included random intercepts for center of random assignment. Models also included controls for baseline scores on these same measures.

\* $p < .05$ . \*\* $p < .01$ . \*\*\* $p < .001$ .

had theorized, but to the availability of more formal care options with parenting components for the parents of 4-year-olds versus 3-year-olds. To examine this possibility, we controlled for child's primary care setting in 2002–2003 (i.e., Head Start, other center-based care, and family-based day care, with parent care as the reference category). Associations between impending kindergarten and parent–child language and literacy activities (and for materials) in spring when controlling for care type were nearly identical to our primary results in both magnitude and statistical significance (see online Appendix C, Table C1).

Another alternative explanation was that the differences in reported activities we detected were due not to naturally felt internal pressures but to differences in the *content* of parenting interventions available to the age 4 versus age 3 cohorts. Parents of 4-year-olds enrolled in center care, for example, might have received different messages about the urgency of language and literacy preparation than did the parents of 3-year-olds in similar care settings. To explore this possibility, we restricted our sample to those in parent care only—as they received no such messages from child care providers since their child did not attend formal programming—and fitted our primary analysis model within this sample. Statistical power was substantially reduced in this analysis, as the sample fell to 567 maximum and 169 minimum across models (versus a maximum of 2,760 and a minimum of 852 across bandwidths in the full sample). Nonetheless, associations between impending kindergarten and parent–child language and literacy activities in spring were fairly stable in magnitude in three out of five models (residualized change and the two largest RD bandwidths), although the association was statistically significant in the residualized change model only. Consistent with full sample findings, results for language and literacy materials were null with this approach (see online Appendix C, Table C2).

Because of the limited power in our parent-care-only models, we also examined the content of parenting interventions hypothesis by estimating relations between impending kindergarten and parent's language and literacy activities separately for children in formal care (center based and Head Start, with presumably stronger kindergarten readiness parenting content) versus in informal care (parent care, relative care, nonrelative home-based care, with presumably weaker kindergarten readiness parenting content). Effect sizes were larger for those in informal care on three out of five bandwidths and larger for those in formal care for two out of five bandwidths. However, none of these differences was statistically significant ( $p > .05$ ; see online Appendix C, Table C3). In sum, across two approaches, we found little evidence that the uptick in parent activities was due to the content of parenting interventions available to the age 4 versus age 3 cohorts.

In addition to differences in the types of care that children were receiving, we also examined whether the detected

effect of impending kindergarten on parent–child language and literacy activities was robust to the influence of state and neighborhood characteristics. We fitted additional models that controlled for levels of resources (e.g., commerce, social services, religious institutions), household poverty rates, and population density of the neighborhoods (i.e., census tracts) surrounding the Head Start center in which children were originally assigned using data from the 2000 Census (U.S. Census Bureau, 2000, 2002). We also fitted alternative models that controlled for the state characteristics listed above (e.g., whether the state provided public pre-K, average levels of child poverty) in place of using state fixed effects. These additional models were intended to test whether results were robust across diverse settings in which children faced varying levels of social disadvantage and had access to different levels of community resources. As shown in online Appendix C, Tables C4 and C5, these results showed relatively similar patterns of results to the main findings discussed above.

As is noted above, we also tested the robustness of findings to alternative definitions of children's DOB. As the HSIS did not provide information on the specific day on which children were born, we assumed that each child was born on the 15th of the month in our primary analyses. In online Appendix C, Table C6, we show the results of our main analyses using alternative definitions of children's DOB in generating our forcing variable. Specifically, we test results assuming that all children were born on the 1st day of the month and assuming that all children were born on the last day of the month. These results are consistent with those from our primary analyses.

We also performed a kind of falsification test to address concerns that results could be attributed to reporter biases (e.g., where parents of 4-year-olds felt increased pressure or social desirability to report more positive parenting behaviors over time). To do so, we explored an additional parenting outcome for which we expected parents would not change in response to impending kindergarten—family routines (five items total, tapping whether parents enforced consistent routines regarding eating, bedtimes, TV watching, and chores)—and examined associations between impending kindergarten and this outcome, using residualized change and RD models. As shown in online Appendix C, Table C7, the magnitude of this association was very near zero and not statistically significant across bandwidths, supporting the claim that our primary results were likely not driven solely by reporter biases.

As mentioned earlier, impending-kindergarten estimates were stable across four bandwidths for parents' engagement in language and literacy activities with their children. For states with small samples, however, bandwidth changes were dropping relatively few children. We examined whether estimates for parents' engagement in language and literacy activities with their children were stable dropping the six states with fewer than 100 children (388 dropped children

total, 14% of the analytic sample. Results were very stable to this change in magnitude (results available upon request).

Finally, we also tested whether the effect of impending kindergarten on parents' learning investments emerged earlier than the spring before kindergarten. We used the pre-tests—collected in the fall before kindergarten entry (~11 months prior)—as outcomes for these analyses and refit our primary RD models (there was no data collection before this time point, so residualized change models were not possible). There was no relation between impending kindergarten and reading materials (see online Appendix C, Table C8). The effect size for learning activities was slightly smaller in magnitude than its spring-before-kindergarten counterpart and statistically significant for one out of three bandwidths. We interpret these results as suggestive but ultimately inconclusive regarding an uptick in parents' learning investments as early as nearly a full year before kindergarten.

### Discussion

The present study examined whether low-income parents increased their parent-child learning activities and provided more language and literacy learning materials in the months leading up to their child's entry into kindergarten. Consistent with socioemotional selectivity theory, we found that parents seemed to respond to the natural pressure created by their children's impending entry into formal schooling, independent of children's enrollment in formal school readiness or transition programs. More specifically, parents reported increases in their efforts at home to help their children become ready for school via more frequent parent-child language and literacy activities. This result held across two different analytical approaches (residualized change models and RD) and across extensive sensitivity analyses. Aligned with prior work that found that low-income parents in fact place more particular emphasis on their children's preacademic skills in particular (e.g., knowing the letters of the alphabet; Stipek et al., 1992), we found that uptick in parents' language and literacy activities was concentrated in literacy activities—particularly name writing and spelling—and not in language-focused activities. Although there was some suggestion that parents also provided more language- and literacy-related learning materials to their children in anticipation of kindergarten entry, these results were not consistent across modeling approaches and did not hold up in sensitivity analyses.

Our findings contribute to the parenting literature in several ways. Conceptually, we show that low-income parents appear to adjust their activities, or time investments, with children in response to the external time pressure of kindergarten entry, which complements prior findings that parents adjust their behavior in response to children's internal developmental changes (Leyva et al., 2015). Empirically, our use

of more advanced predictive methods and a large, multistate sample provides improved internal and external validity relative to existing literature on school transitions that have primarily used more descriptive approaches and small samples. Despite these advances, as explained earlier, the assumptions necessary to infer causality are quite strong in the present study. We view our results as suggestive and as one early step toward a more robust understanding of low-income parents' investment in child learning in advance of the kindergarten transition.

To be clear, in absolute terms, adjustments in parents' investments were modest in our study. On average, parents in the non-impending-kindergarten group reported engaging in book reading about three times per week; in storytelling, letter naming, and name writing/spelling, once or twice a week; in alphabet writing, slightly less than once or twice a week; and in rhyming, two to three times per month. There was evidence of an increase across most of these activities for impending-kindergarten-entry families, but effect sizes did not translate into daily engagement in these activities. Even if higher-income families showed no such uptick (which is probably unlikely), the large gaps in investments between higher- and lower-income families in learning activities (Bassok et al., 2016) accordingly would be closed at most only slightly by the impending-kindergarten effect.

If replicated in studies with stronger research designs that can more definitively identify causality, our findings have implications for parent-focused interventions for preschoolers. An emerging body of evidence suggests that increased parental investment in early learning is an efficacious way to improve school readiness and to sustain the positive effects of classroom-based early childhood education programs (York & Loeb, 2014). At the same time, the specific targets and methods of these early parenting interventions appear to matter in determining their effectiveness—that is, they are not all equally effective (Grindal et al., 2016). Our results suggest that one method for improving the efficacy and cost-effectiveness of parenting interventions in the preschool period may be to adjust *when* they are implemented. In particular, it may be especially efficacious to target and/or intensify efforts to improve parent-child learning activities in the months leading up to kindergarten, when low-income parents may be naturally increasing their investments in their children's learning to ensure their children are ready for the language- and literacy-related demands of school.

Several well-known early childhood interventions already explicitly take advantage of naturally occurring transitions in the lives of young children and their parents. For example, the very successful Nurse-Family Partnership (Olds, 2002) intervention for low-income and at-risk pregnant women and new mothers was built on the theory that natural transitions matter for changing adult behaviors. Indeed, additional evidence from the Fragile Families Study highlights the degree

to which the birth of a child should be treated as a “magic moment” (McLanahan, Garfinkel, Mincy, & Donahue, 2010) and “window of opportunity” (Dubowitz, 2002) for increasing parent engagement and cooperative parenting. Just as the birth of a child is a vulnerable time when parents are more open to input and highly motivated, so too may be the months leading up to kindergarten. Previous research suggests all parents—including low-income parents—are committed to supporting their children’s learning during this transition (Holloway, Rambaud, Fuller, & Eggers-Pierola, 1995; Stipek et al., 1992), and our results suggest they are acting on this commitment by further engaging with literacy activities prior to school entry. Offering parents specific, evidence-based methods for boosting their children’s skills during this period may allow parents to harness their existing enthusiasm in even more effective ways. In particular, focusing on improving the quality of activities and interactions that are already taking place during this transition may provide a more sustainable solution than encouraging the purchase of additional learning-related materials in resource-strapped low-income samples.

We want to underscore, however, several important caveats to this “window” possibility. Parents increased their support for the development of literacy-focused (specifically, name and letterwriting) and not language-focused activities. Both are important to later reading achievement (Sénéchal & LeFevre, 2002). But if the “window” is open prior to kindergarten only for basic literacy skills, interventions timed during this period may not be worth investment because the vast majority of children will master these skills in kindergarten anyway (Paris, 2005)—for example, these skills may not be what Bailey and colleagues describe in their work on the persistence of intervention effects as “skills unlikely to develop in the counterfactual” (Bailey, Duncan, Odgers, & Yu, 2016). Also, an alternative interpretation of our findings is that the best time to intervene with parents may actually be when they are not already increasing their efforts. Supporting parents earlier in their children’s lives may, for example, help to them to interact with their children in ways that they would not otherwise be motivated to, therefore resulting in potentially larger payoffs. Ultimately, we view our results as suggestive that the timing of parenting interventions should be an area for future study, not as evidence against intervening earlier with parents.

We also want to address the possibility that, at face value, our results may have additional implications for studies that use age cutoffs to estimate the effects of preschool programs beyond those raised in a recent article on the limitations of the RD design in this context (Lipsey, Weiland, Yoshikawa, Wilson, & Hofer, 2015). Specifically, age-based RD studies make the assumption that the only difference between children in the treatment and control groups at the cutoff is assignment to a given preschool program—an assumption our findings suggest may not hold for parental

investments. For several reasons, we view our results as raising this possibility but not as adequate evidence of a universal assumption violation or of bias. The increases in language and literacy activities we observed were small in effect size and in practical terms, and our study does not establish whether these increases led to boosts in child school readiness. Further, some pre-K RD studies have found sizable positive impacts on language skills (Weiland & Yoshikawa, 2013; Wong et al., 2008), but we found no parental uptick in home language activities. Also, some pre-K cutoff studies are of non-income-targeted programs; our findings cannot speak to the investments of nonpoor families. And finally, once again, the internal validity of our results rests on very strong assumptions, and our results are in need of replication.

Despite the strengths of the present study, there are several limitations of this work that should be highlighted. First, although large and diverse, our sample consisted of Head Start seekers, who by definition may be more school-readiness conscious than low-income parents who do not seek out preschool programs for their children. Our results accordingly may not apply to this latter group. Second, our parenting measures come from self-reports by parents, not from observations of parents’ behaviors. Our language and literacy activities and materials outcomes were positively and significantly correlated with five different children’s spring language, literacy, and mathematics outcomes. Although the size of these associations was modest ( $r = .05-.27$ ), our parenting measures appear to capture parent inputs that are important to children’s development. Additionally, these positive associations further suggest that our parenting measures might serve as an overall proxy for the quality of early learning environment at home. Nonetheless, social desirability bias may have led parents to overstate their parenting inputs, and this bias may have been differential across the impending versus non-impending-kindergarten groups or across the multiple outcomes tested. Third, in addition to social desirability concerns, there are also measurement concerns with our materials outcomes, as the Cronbach’s alpha for this scale was a marginally acceptable .60. Also, only two out of the eight indicators were explicitly child focused. We found the similar null results in models that used only the two child-focused indicators instead of all eight indicators (results available upon request). But our largely null results for this outcome could have been due to measurement issues. Fourth, as we already emphasized, our RD strategy rests on strong assumptions that may not hold and that may threaten the internal validity of our RD results. Fifth, measures of parents’ socioemotional- and self-regulation-focused activities with their children were not available. Parents did report if the child did math activities during the day but not if he or she did these activities with a parent in an interactive manner. It is possible parents were also increasing the

counting, shape identification, emotion knowledge, and impulse control activities (for example, games like Simon Says or Red Light/Green Light) that they might have used to prepare their children for kindergarten. Understanding whether parents respond to impending school entry across all child developmental domains or only in certain domains would better pinpoint the best way to translate such findings into practice. Sixth, recent work has highlighted that the time and resource constraints low-income parents face should be taken into account in the design of parent-focused interventions (Niklas & Schneider, 2015). Our study does not provide insight into what specific design elements might be best for taking advantage of this natural window, if indeed there is one. Finally, the HSIS data set includes weights so that any produced estimates are nationally representative. Because we used a subsample of the full data set, we did not employ these weights, and our results accordingly are not nationally representative.

Notwithstanding these limitations, our study makes a contribution to the kindergarten transition and parenting literatures by highlighting the potential importance of timing of parental investments in low-income samples. Our results suggest that the months before formal school entry may be a natural opportunity for improving parenting practices related to student language and literacy, and one that is exploited by only a small number of school readiness programs. The Boston Public Schools, for example, offers a school-based, 5-week transition program for children without a formal preschool experience that features a component targeted at engaging parents and encouraging home-based learning activities just prior to children's school entry (BPS Department of Early Childhood, n.d.). Adjusting other programs and interventions to intentionally take advantage of this natural window could result in higher-quality parent-child interactions as well as children who are better prepared to succeed in school.

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