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Talking about Words Boosts Preschool-Age Children's Vocabulary: Evidence from a Parent Intervention

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Talking about Words Boosts Preschool-Age Children's Vocabulary: Evidence from a Parent Intervention

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Abstract: Correlational research shows that children whose parents talk with them more often have stronger vocabulary and language skills than children whose parents talk to them less frequently. To test whether this relationship is causal and to test the impact of a light-touch parent intervention, we designed and implemented a program ("Chat2Learn") to support parents in talking to their preschool-age children about words. Chat2Learn sent three text-based prompts per week for six months to a sample of nearly 600 low-income parents in the United States. Chat2Learn tested two different approaches. In approach one (the "definition approach") parents were prompted to talk with their child about what a word means. In approach two (the "conversation approach") parents were prompted not only to define the word but also to have a conversation using the word. Both approaches significantly increased children's vocabulary (effect sizes of .37 and .23, SDs respectively) for words contained in our intervention – or treatment words. Quantile regressions further showed that the definition approach significantly boosted vocabulary for non-treatment words for children at the upper end of the vocabulary distribution. The definition approach also reduced by .17 SD parents' beliefs that the child's intelligence is fixed and cannot be changed. Neither approach changed parents' feelings of stress, fatigue, or enjoyment of learning activities.

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I. Introduction

Understanding the mechanisms that foster children's language development has scientific and policy implications for human capital formation. Psychologists hypothesize that language development is driven by exposure to child-directed speech, such that children who receive more language input from parents and caregivers acquire larger vocabularies (Golinkoff et al., 2019; Rowe, 2008). Empirical evidence highlights the importance of back-and-forth interactions, rather than passive exposure to overheard speech, in supporting linguistic growth and boosting cognitive, social, and emotional development (Golinkoff et al., 2019). Early vocabulary acquisition is associated with long-term learning outcomes and noncognitive skills such as self-control and executive function thought to be critical for success later in life (Cunha & Heckman, 2007; Golinkoff et al., 2019). Though a few studies in psychology suggest positive impacts of parenting interventions to boost children's vocabulary, it is difficult to isolate the causal effect of parent talk because the studies bundle multiple treatments, rely on small sample sizes, or do not estimate treatment impacts from experimental designs (Reese et al., 2010).

Observational studies also show that parents with low levels of education and income talk less frequently with their children compared to more economically advantaged parents (Golinkoff et al., 2019; Kalil & Ryan, 2020). Parents with lower education and income are also less likely to engage their children in back-and-forth conversation as opposed to using one-way language directives to manage children's behavior (Golinkoff et al., 2019; Rowe, 2008). Descriptively, children from less economically advantaged homes have smaller vocabularies and slower vocabulary growth in early childhood, and as many as 65% of children from these families exhibit clinically significant language impairment (Roberts et al., 2019). The amount that parents and caregivers talk to their children may be a key driver of inequality in children's

human capital development.[1] Boosting interactive caregiver-child language interaction is thus key to narrowing gaps in children's school readiness.

This project is also interested in the development of children's curiosity. Language development is more than learning a string of words; it is also the expansion in thinking and the acquisition of new ideas and new concepts that come with learning new words. We posit that the conversation approach might increase children's curiosity via parental modeling, as suggested by social learning theory (Bandura, 1977). Specifically, in the conversation approach, if children observe their parents engaging in curiosity-driven behaviors—such as asking open-ended questions or showing interest in novel words and concepts —this might encourage the children to imitate and adopt similar behaviors.

It is also possible that the conversation approach not only boosts children's curiosity but also has an even greater impact on children's vocabulary growth than the definition approach. We base this hypothesis on studies in psychology that examine the relationship between children's curiosity and language acquisition, which suggests that early language learning is driven by children's curiosity. This is because curiosity in young children motivates them to extract linguistic stimuli from their environments (Twomey & Westermann, 2019). The conversation approach is designed specifically to pique young children's interest in new ideas and new concepts. If that approach succeeds in boosting children's curiosity that might further boost their vocabulary.

This article presents evaluation results from Chat2Learn, a program we designed to boost the amount that parents talk to their children about words in subjects of interest to the child to

[1] The World Health Organization has identified language as one of the domains of development that is associated with not only early learning and academic success but also long-run economic participation and health.

increase children's vocabulary.[2] Chat2Learn sent 600 parents of preschool-aged children of low-income backgrounds three-word prompts per week via text-message for six months. Motivated by recent empirical work in psychology and economics that highlights the importance of different skills (Alan & Mumcu, 2024; Jenkins et al., 2018; Nesbitt & Farran, 2021), we randomized parents into one of two treatment groups and we examined treatment impacts on children's vocabulary and curiosity. In the definition approach parents were prompted to define a word for their child and ask their child to repeat what the word means. The prompt provides the definition, using age-appropriate language, for the parent to use. In the conversation approach, parents were prompted not only to define the word but also to have an open-ended conversation using the word. The prompt also provides the script for this exercise. In each of these approaches, the prompt was accompanied by a photo that was illustrative of the word's definition.

These two treatment arms capture two different learning modalities. One provides a word definition with no further prompt to use the word in context - it relies on memorization of the definition. The other provides this prompt - which may help to solidify an applied understanding of the word's definition. We expect different effects of the two modalities not only on the number of words the child learned among the words that parents defined but also on the number of words not defined by the parent that the child learned. Furthermore, we expected the different treatments to have different effects on children's curiosity. We expect that as parents engage in longer, deeper, open-ended conversations guided by the child's interest, these conversations will spark the child's interest in discovery, increasing measured curiosity (Loewenstein, 1994).

[2] The program on which we report here is part of a larger initiative designed to boost children's skills by increasing the quantity and quality of the language environment in children's homes and classrooms. That initiative is called "Chat2Learn"; we use this name to describe the intervention whose results we report in this article.

Descriptive research shows that children in disadvantaged homes (and pre-K settings) are much less likely to be asked open-ended questions and engage in back-and-forth conversations (Nesbitt & Farran, 2021).

We find that vocabulary increased for the children in both treatment groups by 0.37 (definition) and 0.23 (conversation) standard deviations compared to children in the control group. These effects are statistically indistinguishable between the two groups. In both groups, children's vocabulary increased for the words included in the treatments. In addition, quantile regressions further showed that the definition approach significantly boosted vocabulary for non-treatment words for children at the upper end of the vocabulary distribution. Neither treatment increased our measure of curiosity. One possibility is the conversation approach worked less well than we expected because it took the focus away from the target word to a different conversation. Another theory is that the conversation approach was too complex in that it contained two directives instead of just one for the parent.

We contribute to the literature on the effects of caregivers' talking with children on children's vocabulary by providing causal evidence of this relationship. Some work in economics suggests that in-person structured parent training programs can help improve children's vocabulary (List, Pernaudet, & Suskind, 2021). York, Loeb, and Doss (2019) demonstrated that a light-touch motivational/instructional text-message program boosted parents' engagement in children's learning at home and school and increased low-income preschool-age children's letter-sound awareness (York, Loeb, & Doss, 2019). Yet, causal evidence in large samples is lacking on how parents' talking to children about words influences a child's vocabulary and curiosity (Greenwood et al., 2020). We demonstrate that a text-message program increases vocabulary. We do so in a large field experiment with low-income parents; a highly

relevant population given these caregivers' lower likelihood of talking to their children in a way that supports children's vocabulary growth.

Chat2Learn also fills critical gaps in the design and evaluation of programs containing the features necessary to build the capacity for large-scale intervention and prevention of income-based inequalities in child skill development. Greenwood et al. (2020) identify three features of early childhood language interventions central to what they deem "next-generation prevention science standards" in efforts to close such gaps. These three features are (a) ecological validity, (b) rigor, and (c) readiness for scale-up. Chat2Learn advances the field in all three dimensions. First, concerning ecological validity, Chat2Learn recognizes parents in the home as the natural social agents in economically disadvantaged communities who are the implementers of language-promoting interventions. Greenwood et al. (2020) argue that a weakness in many studies was having the research staff as implementers, which, in their opinion, makes the findings preliminary and not ecologically valid. In contrast, Chat2Learn affords the parents complete agency and autonomy to use the program. Chat2Learn does not rely on practitioners or paraprofessionals to deliver the program, and it does not "instruct" or "train" parents on how to use it. Testing an ecologically valid program with these design features provides better insight into how Chat2Learn might work in the real world.

Second, concerning rigor, Greenwood et al. (2020) emphasize the gold standard of randomized design and fidelity of implementation. Our randomized evaluation of Chat2Learn has strong evidence of proper implementation, low attrition, high engagement, and acceptability for the target population (i.e., social validity). Regarding rigor, we would also emphasize having a large sample that is powered to detect even small treatment impacts and the adoption of pre-registered hypotheses in a published pre-analysis plan. These features characterize Chat2Learn.

Next, Greenwood et al. report that most intervention durations in the 140 child language intervention studies they reviewed were only 2-8 weeks long. It is possible, though unlikely, that treatment impacts on child language skills could be achieved in so short a time. Our evaluation of Chat2Learn lasted six months.

Finally, Greenwood et al. (2020) did not deem any of the studies they reviewed ready for scale, and they referred to this problem as a particular weakness in existing work in need of more research and development (see also List, 2024). Digital technologies, arguably a key feature to support program scale-up, were infrequently used in the 140 studies they reviewed and typically only in the role of data collection. Instead, most programs rely on costly program delivery methods, including coaching, home visiting, modeling, written manuals, and group training (but see Rowe, Turco, & Blatt; 2021; and Blumfield, Balsa, Cid, and Oreopoulos, 2025 for some interesting technology-forward exceptions). In their review, Greenwood et al. reported that only 4% of studies rely on technology for program delivery to adults. Chat2Learn relies on technology for program delivery.

We also contribute to a nascent experimental literature on children's curiosity. Research in psychology (Gottfried et al., 2016; Shah et al., 2018) and economics (Alan & Mumcu, 2024) suggest curiosity is important for later cognitive development. An observational study (McCormick et al., 2021) found that children of parents who engage their children in curiosity-focused, open-ended activities learn more preliteracy and literacy skills than children of parents who engage their children using a more typical academic approach that stresses closed-ended, rote learning (McCormick et al., 2020). We know of no large-scale study that measures curiosity in young (age 3–5) children or that implements an intervention to increase curiosity through parental behavior other than through schooling (see Alan & Mumcu, 2024). We develop a new

technology-based assessment of curiosity based on Jirout and Klahr (2012) to measure curiosity in children ages 3 to 5.

Finally, we test the limits of a light-touch intervention. Chat2Learn is delivered by text message, does not interact with parents in person, does not have a structured academic curriculum, and does not provide parents with information, suggestions, or motivation regarding the importance of talking to children. Nevertheless, although the intervention did not follow a curriculum, it was structured. Words and definitions were repeated multiple times, and at the end of the program, intervention words were combined for use in context. Chat2Learn tests the possibility of supporting parents to talk with their children to boost children's vocabulary using simple, inexpensive tools.

The rest of this article is organized as follows. Section II provides experimental details. The estimates of the main impacts are presented in Section III, alongside robustness checks and heterogeneity analysis. In Section IV, we discuss the interpretation of our estimates on children's vocabulary, curiosity, and set of preregistered secondary outcomes. Section V concludes.

II. Experimental Details

2.1) Design of Treatment Arms

Parents were randomized into three different groups: the definition approach; the conversation approach; and a control group. Every parent, regardless of the group they were assigned to, received three Chat2Learn text messages per week for six months. In every text message, parents in the two treatment groups received the same word to share with their children. Parents in the definition approach received a message with a word and its definition to share with their child. A sample message is, "*APPLAUDING is when you clap your hands to show that you like something. What does APPLAUDING mean?*" Parents in the conversation approach received that

same content but in addition, the message in the text prompted them to help engage their child in conversation and imaginative thinking. For example: "*APPLAUDING is when you clap your hands to show that you like something. Can you sing a song? I'll APPLAUD when you're done!*" Messages in the control group provided the parent only with information unrelated to children's literacy skills and provided no specific prompt for talking. For example: "*Food isn't just for nutrition. Food is also a link to culture! [child name] will grow up with fond memories of food from your family's culture*."

Each treatment includes prompts for 32 words, all of which were included in the vocabulary assessment (see section 2.3 below). Each word was sent independently to parents twice and used in context with other words at least once during the intervention. All prompts for all groups can be found in Appendix Table A1.

2.2) Sample and Recruitment

Eligibility for participation required parents to have a preschool-aged child with no learning disability, reside in the United States, and be under Chicago's 2022 low-income limit.[3] The sample was recruited from various sources described below in two separate phases.

In phase one, recruitment began in October 2022 by distributing an online interest form through housing authorities, libraries, and health clinics in Chicago. Starting in March 2023, participants who filled out the interest form and who met the eligibility requirements were sent a baseline survey and asked to schedule an appointment for the child to take the baseline assessment. By the end of April 2023, we had recruited 169 participants who were randomized into the three groups. The treatment was implemented between May and October 2023, and the

[3] The eligibility criterion based on income limits that used in this project can be found here: https://www.chicago.gov/content/dam/city/depts/doh/general/2022_Income_and_Rent_Limits.pdf. This requirement was added only for the second phase of recruitment.

end-line surveys and assessments were collected during the next two months after the intervention.

The second phase of recruitment started in June 2023. In this phase we recruited mainly through social media advertising like Facebook.[4] The advertisement targeted parents of preschool-aged children and directed interested parents to the baseline survey. Participants who completed the baseline survey were asked to schedule an assessment appointment. By the beginning of August, we had recruited 561 participants who completed the baseline survey and assessment and were randomized into the three groups. Participants in both phases were compensated for their participation.[5] The treatment and control prompts were sent between August 2023 and January 2024.

Table 1 compares the baseline characteristics of the samples recruited in each phase. Participants from phase one have lower household incomes and education. Parents' race also differs between phases, with more Black participants from phase one and more Hispanic and White participants from phase two. Children from phase two are older and more likely to be enrolled in preschool. Finally, whereas the vocabulary scores are similar in both subsamples, phase two children have higher curiosity scores.

	Phase 1 (N = 121)	Phase 2 (N = 473)	Mean difference
Child Variables			
Male	0.48	0.48	-0.00
	(0.05)	(0.02)	
Speaks English at home	0.95	0.90	0.05
	(0.02)	(0.01)	

[4] See the online appendix for details about potential fraud using Facebook as a recruitment tool.
[5] Participants in both phases received a $60 Amazon gift card within 10 business days after completing the baseline survey and assessment, and another $60 gift card after completing the endline survey and assessment. To increase retention, we also offered a raffle for which one randomly selected participant received an additional $120 Amazon gift card. The retention rate was high with over 80% of our initial sample completing the endline survey and assessment.

Speaks Spanish at home	0.04	0.08	-0.04
	(0.02)	(0.01)	
Age	3.99	4.25	-0.26**
	(0.04)	(0.04)	
Preschool enrollment	0.49	0.69	-0.20**
	(0.05)	(0.02)	
Novelty score at baseline	4.93	5.63	-0.69*
	(0.28)	(0.15)	
Very curious score at baseline	2.24	3.03	-0.79**
	(0.21)	(0.14)	
Vocabulary score at baseline	23.58	24.19	-0.61
	(0.85)	(0.41)	
Parent and Household Variables			
Hispanic	0.12	0.23	-0.10*
	(0.03)	(0.02)	
White	0.22	0.47	-0.25**
	(0.04)	(0.02)	
Black	0.60	0.20	0.39**
	(0.04)	(0.02)	
Employment	0.64	0.48	0.16**
	(0.04)	(0.02)	
BA	0.20	0.60	-0.40**
	(0.04)	(0.02)	
Learning activities with child	100.29	99.95	0.34
	(5.27)	(2.52)	
Household income	27430.34	48612.68	-21182.34**
	(2036.40)	(1142.01)	

Notes: *p<0.05; **p<0.01. The number of observations in each row may vary due to missing values. Columns 1 and 2 show the mean for each recruitment phase and the standard errors in parenthesis, only for the final analytical sample. The novelty, very curious, and vocabulary scores are defined in section 2.3.

2.3) Measures

We asked parents by text message to complete an online baseline and end-line survey. The survey recorded parents' beliefs about their child's skills and interests, along with other parent attitudes, beliefs, and perceptions. The end-line survey repeated the questions about

parents' beliefs and additionally asked for their opinions about Chat2Learn. The end-line surveys and assessments were collected within two months of the conclusion of the intervention.

The baseline and end-line assessments evaluated children's skills in two areas: vocabulary and curiosity. Assessments were completed over Zoom by assessors in our research team.[6] Parents were required to be present in the Zoom call to assist in case of any technological difficulties. The assessors began by asking the parent to confirm the child's date of birth to make sure that it matched the one in our records. They also asked the parents to refrain from intervening during the assessment.

The vocabulary assessment was based on the Peabody Picture Vocabulary Test (PPVT). It evaluates the child by showing them four pictures and asking them to point to the image that depicts a certain word (for example, showing four pictures of different animals, and asking the child to point at the kangaroo). The PPVT measures receptive vocabulary for Standard American English and yields an estimate of verbal ability (Dunn & Dunn, 1981). This test is frequently used in economics studies of child skill formation (e.g., Cunha et al., 2010; Cunha & Heckman, 2008; Francesconi & Heckman, 2016) and is considered among the best-established tests of verbal intelligence and scholastic aptitude across childhood. It is also used in national surveys such as the Bureau of Labor Statistics' National Longitudinal Surveys.

Piloting the PPVT assessment revealed that implementing an assessment of its length in a virtual environment was not viable given the young age of the children. Our pilot also revealed that in a virtual setting on Zoom, children were easily distracted and unable to maintain their attention on the PPVT assessment long enough to reach their true ceiling, and the assessment took more than double the time it takes to administer to children in-person. Therefore, we created

[6] Members of the research team who conducted these assessments were blind to the participant's treatment status.

a truncated version of the PPVT assessment that followed the sequence of difficulty in vocabulary on the full assessment but took most children less than 25 minutes to complete by reaching their true ceiling. To ease children into the assessment, we included two words that over 90% of children correctly answered in the pilot. We verified that our shortened assessment followed the standard assessment's difficulty sequence and that there was sufficient variation in PPVT scores with our pilot data on the number of children who knew the meaning of each PPVT word. We ended up with 62 words, from which 32 were also words included in the intervention messages and were interspersed throughout the assessment. Since the assessment was conducted on Zoom, we adapted the assessment online to present each stimulus page as four pictures, each framed by a different color. We used software that allowed co-browsing so children could hover with the pointer over their picture selection or if preferred, say the frame color of their picture selection. The final vocabulary score is constructed based on the child's performance on this assessment, representing the number of questions they answered correctly.

We also developed and tested an assessment of curiosity or novelty preference. This assessment was based on work in Jirout and Klahr (2012) that studied children's scientific curiosity. The assessment that we developed had 10 trials. In each round, children are shown two objects: the silhouette of a familiar animal that a preschool aged child would likely know (for example, a lion), and a box that concealed a mystery animal.[7] An example from the assessment can be found in the Appendix Figure A1. First, children were asked to choose whether they wanted to see the animal that was in silhouette or the animal in the box. The assessor would then show the child either the animal in silhouette or the animal in the box. The animals behind the

[7] On average, children claimed to know the animal behind the silhouette 9 out of 10 times and were correct about their guess 8 out of 10 times.

boxes are less familiar than the animals in silhouette. After the animal was revealed, the child was told the name of the animal and asked whether they would like to know more about it.

We use the choice of the box as our indicator of novelty preference. The novelty score ranges between 0 and 10, and it represents the number of times that the child chose the box. This is related to what Berlyne (1954) defines as "perceptual curiosity," which is the stimulation caused by being exposed to novelty. We also construct a variable that equals one if they both choose the unknown object and said that they would like to know more about this animal. We call this measure the "very curious score," and it also ranges between 0 and 10. This measure is related not only to perceptual curiosity but also to "epistemic curiosity," which is defined as a desire for knowledge (Alan & Mumcu, 2024). We hypothesized that the conversation approach would boost these two central dimensions of children's curiosity.[8]

III. Results

3.1 Data and Descriptive Results

Including both rounds, 730 participants were randomized into one of the three groups. We include parents in the analytical sample if their child was assessed in vocabulary and curiosity both at baseline and endline. Out of the 730 participants we randomized, 16 parents dropped out during the intervention, 8 children missed either the vocabulary or curiosity assessments at baseline, and 112 children missed one or both assessments at endline. Therefore,

[8] Neither of our curiosity outcomes were defined in our pre-analysis plan; they were developed after we received the experimental data. This raises two concerns. First, there is the possibility that we defined our curiosity measures in a way that artificially creates an effect of our intervention on curiosity. However, as we demonstrate in Section 3.2, this is not the case. Second, because we did not establish a fully formed and well-defined measure of curiosity before the study, we cannot claim to make definitive contributions to the literature on interventions that influence curiosity in children. That said, we have approached our curiosity data in a highly agnostic and transparent manner when constructing our outcomes. Both the novelty preference and very curious measures are intuitive and do not involve questionable data manipulations. For these reasons, we believe that these measures remain unbiased and are not influenced by our prior exposure to the data.

our analytical sample consists of 594 participants. If a child started an assessment but did not complete it, we use the score they earned in the questions they answered as their assessment score. To address concerns about whether participant's assessment completion was nonrandom, we conduct a robustness check where we exclude participants who did not reach the final question of the assessments from the analysis. We find results consistent with our main analysis.[9]

To study attrition, we first create an indicator variable that equals one if the participant was randomized into the treatment groups but is not part of our final analytical sample. We regress this outcome on the treatment variables. Table 2 shows that there was no differential attrition between the treatment and control groups. These results are robust to incorporating baseline characteristics as control variables to the regression.

Table 2: Differential attrition between control and treatment groups

	Attrition
Definition	0.042
	(0.04)
Conversation	0.025
	(0.03)
Constant	0.164**
	(0.02)
Observations	730

Note: *p<0.05; **p<0.01. The omitted group is the control group.

Column 1 of Table 3 presents the baseline characteristics of our analytical sample. Just over half the children are girls, they are mostly English speakers, and 65% of them were

[9] Our intervention lasted 24 weeks, leaving open the possibility that participants left the study (i.e., did not sign up for a follow-up assessment) nonrandomly, potentially impacting estimates of our treatment effects. To avoid this possibility, our research team sent home several text messages to prevent attrition. Attrition-reduction messages were sent to all groups periodically throughout the intervention and increased in frequency as the intervention went on (total = 6). Attrition messages included reminders of incentives *("We're counting on you for the final survey + zoom session! Don't miss out on a raffle entry for an additional $120 gift card!")* and appeals to helping behavior *("We hope you're enjoying Learning Curiosity – THANK YOU for your support of research to help kids and families")*.

attending preschool at the time of the baseline survey. Average scores in the curiosity measures are 5.48 for the novelty score and 2.87 (both out of 10) for the very curious score. Their average vocabulary score is 24 out of 60, which means that children initially knew around 40% of the words in the assessment. Half of the parents report having a BA degree and half of report being employed. Parents report doing learning activities with their children 100 minutes per week, on average.[10] Household income is $44K per year on average, placing them in the 30th percentile of the US household income distribution based on data available from the US census.[11] We next test whether assignment to treatment was balanced across characteristics that we measured. Columns 2-5 of Table 3 present the results from 15 regressions in which we regress each baseline covariate on the two treatment groups. Here, we test whether the means for each characteristic are equal across the three groups. In these tests, we find a marginally significant difference between the treatment groups on one variable only (learning activities with child). Provided we conducted 15 different tests, this result is less frequent than what we would expect to see purely by chance. Therefore, we conclude that our randomization process was successfully implemented.

Table 3: Descriptive Statistics and Balance Table

	All analytical sample (N=594)	Control group (N = 204)	Definition (N = 193)	Conversation (N = 197)	F-stat Prob > F
Child Variables					
Male	0.48	0.50	0.46	0.48	0.30
	(0.50)	(0.04)	(0.04)	(0.04)	0.74
Speaks English at home	0.91	0.93	0.92	0.89	0.82
	(0.28)	(0.02)	(0.02)	(0.02)	0.44

10 This number comports well with other estimates of learning activities in the household. Using data from the American Time Use Survey, we find mothers spend on average 16 minutes per day doing learning activities with their 3-to-5-year-old children, including reading, doing homework, and homeschooling, which corresponds to 96 minutes per week.

11 Data used here can be found at the following link: https://bit.ly/46n5c7N.

Speaks Spanish at home	0.07	0.06	0.07	0.09	0.60
	(0.26)	(0.02)	(0.02)	(0.02)	0.55
Age	4.20	4.19	4.23	4.18	0.25
	(0.72)	(0.05)	(0.05)	(0.05)	0.78
Preschool enrollment	0.65	0.63	0.64	0.68	0.47
	(0.48)	(0.03)	(0.03)	(0.03)	0.62
Novelty score at baseline	5.48	5.46	5.31	5.69	0.67
	(3.23)	(0.22)	(0.23)	(0.23)	0.51
Very curious score at baseline	2.87	3.09	2.69	2.82	1.00
	(2.85)	(0.20)	(0.20)	(0.20)	0.37
Vocabulary score at baseline	24.07	24.68	23.77	23.72	0.72
	(9.07)	(0.65)	(0.68)	(0.61)	0.49
Parent and Household Variables					
Hispanic	0.21	0.21	0.20	0.21	0.02
	(0.41)	(0.03)	(0.03)	(0.03)	0.98
White	0.42	0.45	0.43	0.39	0.66
	(0.49)	(0.03)	(0.04)	(0.03)	0.52
Black	0.28	0.24	0.28	0.32	1.78
	(0.45)	(0.03)	(0.03)	(0.03)	0.17
Employment	0.52	0.56	0.51	0.48	1.37
	(0.50)	(0.03)	(0.04)	(0.04)	0.26
BA	0.52	0.54	0.51	0.51	0.30
	(0.50)	(0.04)	(0.04)	(0.04)	0.74
Learning activities with child	100.02	103.38	103.88	92.75	2.56
	(55.36)	(3.75)	(4.13)	(3.90)	0.08
Household income	44354.7	45583.49	44490.21	42948.72	0.52
	(25756.3)	(1802.00)	(1937.43)	(1765.36)	0.59

Notes: *p<0.05; **p<0.01. The number of observations in each row may vary due to missing values. Column 1 shows the mean of each variable and the standard deviation in parenthesis for the final analytical sample. Columns 2-4 show the mean for each group and the standard errors in parenthesis, only for the final analytical sample. The f-stat p-value column represents the p-value on a test with the null hypothesis of equal means across groups.

3.2 Experimental Results

To study treatment impacts on children's vocabulary and curiosity, we estimate the following statistical model using ordinary least square (OLS):

$$Y_{i,t} = \beta_0 + \beta_1 T_{1i} + \beta_2 T_{2i} + \alpha Y_{i,t-1} + \varepsilon_i,$$

where $Y_{i,t}$ is our outcome of interest measured at endline for participant i (e.g., vocabulary score), and T_{1i} and T_{2i} indicate participant i's assignment to the word definition and conversation

prompts, respectively. The omitted group is the control group. $Y_{i,t-1}$represents the outcome variable measured at baseline.

Table 4: Treatment Effects on Vocabulary Score

	(1) Vocabulary score	(2) Treat words from assessment	(3) Non-treat words from assessment
Definition	3.63**	3.13**	0.43
	(0.78)	(0.45)	(0.43)
	[0.00]	[0.00]	[0.33]
Conversation	2.30**	2.01**	0.23
	(0.74)	(0.44)	(0.41)
	[0.00]	[0.00]	[0.29]
Control mean	27.52	13.20	14.32
Control SD	9.77	4.74	5.56
Observations	594.00	594.00	594.00
R2	0.46	0.34	0.44

Notes: *p<0.05; **p<0.01. The omitted group is the control group. All regressions control for the baseline measure of the outcome. The table shows the estimated coefficients, the standard errors in parenthesis, and the empirical p-values in brackets.

Table 4 presents the estimated treatment impacts on the children's vocabulary scores. Column 1 shows that both approaches had significant positive effects on children's vocabulary knowledge compared to the control group. On average, the definition approach increased children's vocabulary score by 3.63 points (0.37 baseline control standard deviations), and the conversation approach increased children's vocabulary score by 2.30 points (0.24 baseline control standard deviations). These results are robust to including the unbalanced baseline variable, other demographic characteristics (e.g., parent race, BA, etc.) as control variables, and to excluding children who did not complete the assessments from the analysis (see Appendix Table A2 Panel A). The treatment effects for the two approaches are not statistically different from one another.

Columns 2 and 3 separate the outcome variable into the score obtained using only the words that were included in the treatment messages (score between 0 and 32) and only words

that were not included in the treatment messages (score between 0 and 30). Results presented in Table 4 reveal that the treatment effects are driven by an increase in the knowledge of the words that were part of the messages.[12]

Next, we study how the treatments affected children's novelty preference and a measure for "very curious." Our hypothesis was that parent-child pairs in the conversation group would experience larger treatment effects on curiosity than those in the word definition group because conversation messages were designed to elicit parent-child interactions that could spur creative thinking and behavior. Table 5 shows that our hypothesis was not correct: we fail to reject the null hypothesis of a zero effect for the conversation group. We find marginally significant positive effects for those in the definition group (0.17 baseline control standard deviations) on the preference for novelty score. However, this finding is not robust to controlling for baseline variables or to excluding incomplete assessments from the sample (see Appendix Table A2 Panel B). Neither treatment had a significant impact on our very curious measure.[13]

Table 5: Treatment Effects on Curiosity

	(1) Very curious score	(2) Novelty score
Definition	0.05	0.51
	(0.27)	(0.29)
	[0.86]	[0.08]
Conversation	0.23	0.10
	(0.28)	(0.29)
	[0.41]	[0.71]

[12] The results on treated words suggest a hypothesis that the observed increases in vocabulary might be crowding out other words, effectively replacing one set of words with another rather than expanding overall vocabulary. However, our null results on non-treated words suggest this is unlikely. If increases in treated word vocabulary were indeed crowding out other words, we would expect to see a decrease in knowledge of untreated words, as the gains in treated words would reduce the likelihood of the child knowing other, untreated words. Instead, although statistically insignificant, our estimates for untreated words are positive, indicating no evidence of such crowding out in our sample.

[13] Motivated by work applying machine learning techniques to analyze experiments, we implemented two approaches to improve the precision of our estimates: lasso regression and a random forest procedure as discussed in List, Muir and Sun (2024). This analysis was not pre-registered. We did not observe any changes in statistical significance across our outcomes using either technique. Results are available upon request.

Control mean	3.09	5.50
Control SD	2.84	3.06
Observations	594.00	594.00
R2	0.08	0.07

Notes: *p<0.05; **p<0.01. The omitted group is the control group. All regressions control for the baseline measure of the outcome.

3.3 Heterogeneous Treatment Impacts

We next explore whether the treatment effects were heterogeneous based on five preregistered characteristics: the child's sex, baseline vocabulary skills, both baseline curiosity scores, preschool enrollment status, and parental education. To study these heterogeneous effects, we estimate the following statistical model:

$$Y_{i,t} = \beta_0 + \beta_1 T_{1i} + \beta_2 T_{2i} + \theta_1 T_{1i} Z_i + \theta_2 T_{2i} Z_i + \alpha X_i + \varepsilon_i,$$

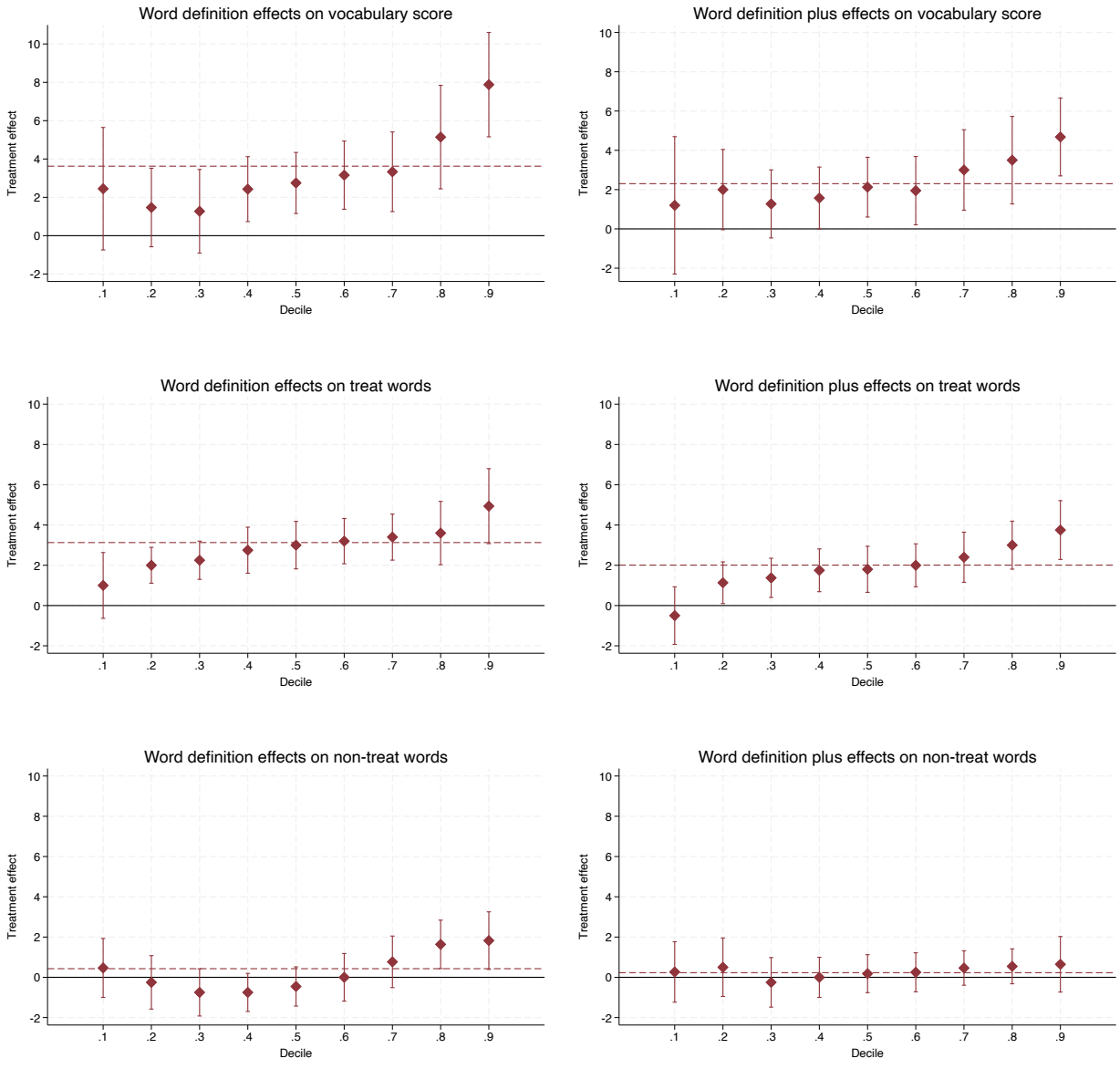
where Z_i is the baseline characteristic; the coefficients θ_1 and θ_2 show the difference in treatment effects by the baseline characteristic; X_i is a vector of controls including the indicated baseline characteristic, and the unbalanced variables. The results show that neither approach displays significant heterogeneity across any of our five parent and child characteristics (see Appendix Table A3).

3.4 Quantile regressions

Figure 1 displays results from quantile regressions for the vocabulary and curiosity outcomes. The quantile treatment effects represent the differences in the outcome distribution between the treatment and control groups at each decile. For comparison, the OLS estimates of the treatment effects are included in the respective figures. Both treatments exhibit non-linearities in the overall vocabulary score, with lower deciles showing no significant effects, while higher deciles—specifically deciles four through nine—experience increases in vocabulary. Results for treated words appear relatively uniform across the distribution. For non-

treated words, approach one demonstrates positive effects in the top two deciles, suggesting spillover effects on general vocabulary. In contrast, approach two does not display a similar pattern. Figure 2 presents results on novelty preferences and our very curious measure. These results reveal no discernible differences across the outcome distribution.[14]

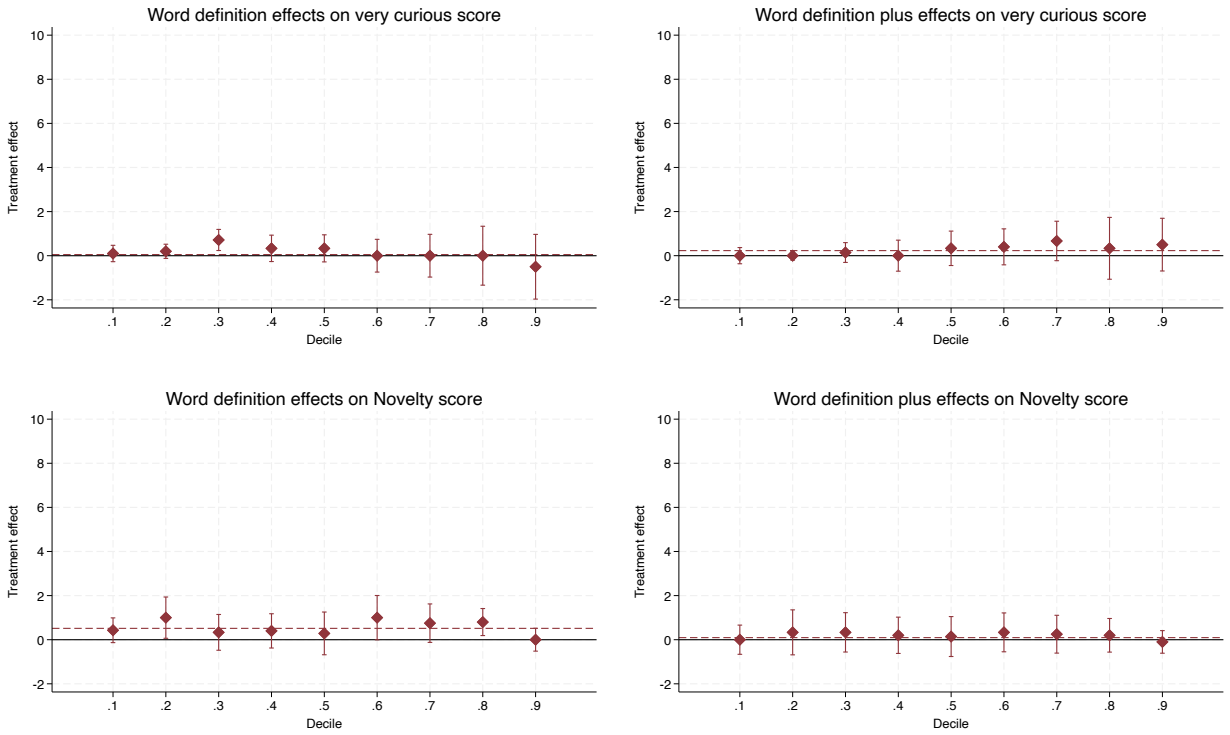
Figure 1: Quantile Regressions for Vocabulary Outcomes



[14] The one exception is the coefficient corresponding to the 80th percentile when studying the quantile effects on novelty score.

Note: The figures plot the coefficients and their 95% confidence intervals of quantile regressions for each decile of the distribution. The figures on the left represent the treatment effects of the word definition treatment, and the ones on the right represent the treatment effects of the conversation treatment. The dashed line represents the OLS regression estimates.

Figure 2: Quantile Regressions for Curiosity Outcomes



Note: The figures plot the coefficients and their 95% confidence intervals of quantile regressions for each decile of the distribution. The figures on the left represent the treatment effects of the word definition treatment, and the ones on the right represent the treatment effects of the conversation treatment. The dashed line represents the OLS regression estimates.

3.5 Effect on Parent Beliefs and Psychological Characteristics

Table 6 presents results of the effect of the treatments on a set of pre-registered measures of parents' beliefs and psychological characteristics. Results in Panel A show that treated parents reported sharing the parent talk prompts with their children more often than control group parents who received the nutrition prompts. Parents reported that their children enjoyed both types of Chat2Learn prompts and that their children enjoyed the conversation prompts

significantly more than the word definition prompts. Treated parents were no more likely to report enjoying the treatment messages compared to the control group parents, but parents in the definition approach (but not the conversation approach) reported that they were more willing to pay for the program (0.19 of the control group standard deviations, $p<0.10$).

Results in Panel B show that the treatment did not shift parents' feelings of stress, fatigue, or enjoyment of learning activities. We interpret this as evidence that our program places a low cognitive burden on parents and does not displace other activities that parents might do to maintain their psychological well-being or help their children learn. There was also no change in parents' reported beliefs about child skills. However, the word definition messages significantly reduced parents' adherence to a fixed mindset; i.e., the belief that intelligence and abilities are fixed traits that cannot change (Dweck, 2006) (reducing by five percentage points from a baseline average of 18%).

Table 6: Treatment Effects on Secondary Outcomes

Panel A: Perceptions of the intervention messages

	(1) Days shared messages	(2) Child enjoyment	(3) Parent enjoyment	(4) Willingness to pay	(5) Likely to recommend
Definition	0.44**	0.75**	-0.16	2.53	-0.09
	(0.15)	(0.28)	(0.25)	(1.39)	(0.26)
Conversation	0.54**	1.37**	0.05	1.09	0.10
	(0.15)	(0.25)	(0.24)	(1.32)	(0.25)
Control mean	2.51	5.98	7.56	8.89	7.85
Control SD	1.54	2.86	2.42	13.16	2.50
Observations	587.00	587.00	587.00	587.00	587.00
R2	0.03	0.05	0.00	0.01	0.00

Notes: *p<0.05; **p<0.01. The omitted group is the control group. All outcomes are self-reported. Outcome in column (1) ranges between 0 and 5 and represents how many days the parent shared the intervention messages with the child. Outcomes in columns (2) and (3) range between 0 and 10 and answer the question of how much the child/parent enjoyed the

intervention messages. Outcome in column (4) ranges between 0 and 50 and answers how much the parent would be willing to pay to continue receiving the intervention messages for one more year. Outcome in column (5) ranges between 0 and 10 and answers how likely the parent is to recommend the intervention messages to friends or family with a young child.

Panel B: Parent's feelings and preferences

	(1) Stress	(2) Parent is tired	(3) Enjoy learning activities	(4) Curiosity is very important	(5) Parent's fixed mindset
Definition	-0.10	-0.02	-0.14	0.06	-0.05*
	(0.21)	(0.04)	(0.15)	(0.04)	(0.03)
Conversation	-0.13	-0.02	-0.05	0.06	-0.03
	(0.22)	(0.04)	(0.16)	(0.04)	(0.03)
Control mean	5.87	0.54	8.60	0.76	0.18
Control SD	2.57	0.50	1.68	0.43	0.33
Observations	587.00	587.00	588.00	588.00	586.00

Notes: *p<0.05; **p<0.01. The omitted group is the control group. All regressions control for the baseline measure of the outcome. All outcomes are self-reported. Outcome in column (1) ranges between 0 and 10 and answers how stressful the parent finds life these days. Outcome in column (2) is a binary variable indicating if the parent "often" or "almost always" feels tired, worn out, or exhausted from raising a family. Outcome in column (3) ranges between 0 and 10 and answers how much the parent enjoys doing learning activities with the child. Outcome in column (4) is a binary variable indicating if the parent thinks it is "very important" for a child to be curious in kindergarten. Outcome in column (5) ranges between 0 and 1 and is an average of three binary variables; each binary variable indicates if the parent "somewhat" or "strongly" agrees with a statement, where all three statements indicate that intelligence is fixed and it can't be changed.

IV. Interpretation and Discussion

4.1 Effects on Curiosity

We hypothesized the conversation approach would boost children's curiosity. This hypothesis was not supported in our data. It is unclear to us why this would be so, especially because parents reported that children enjoyed the conversation prompts more (as we expected them to). It is possible that parents themselves were not as effective at implementing the

conversation messages in a way that boosted children's curiosity or that parents were not in the habit of making the gestures, vocalizations, or pretend play elicited by the conversation messages. Parents such as the ones in our study might need extra scaffolding to conduct conversations using open-ended questions.

Nonetheless, our novel measure of curiosity demonstrates high levels of test-retest reliability and is relatively easy to implement. To test how well our curiosity measure correlates with children's skills, we also find that children's vocabulary scores are significantly correlated with their measure of curiosity ($p<.01$) and that children of college-educated parents and households with higher income score significantly higher on curiosity ($p<.01$). These correlations controlled for other observable characteristics. This suggests that young children's curiosity behaves similarly to other measures of skills, for which gaps by family background appear early in life (Moullin et al., 2018). We therefore see this as an area ripe for additional research.

4.2 Effects on Parents' Beliefs

Some related interventions attempt to shift parents' beliefs and preferences (e.g., List et al., 2021), for example by trying to instill a "growth mindset" by teaching or training parents (and classroom teachers) that children's intelligence is malleable and can improve with parental investment and/or child effort (Dweck, 2007). In this approach, shifting parents from a "fixed" to a "growth" mindset is seen as a precursor to increasing parental investment (Doyle, 2020; Wagner et al., 2002). Our results suggest that changes in beliefs can arise from experience and the feedback this experience provides (Bernacer & Murillo, 2014). The identification of this potential feedback loop suggests that a useful approach for an intervention might simply be to help parents form self-reinforcing habits.

V. Discussion and Conclusion

To test whether the positive relationship between parent-child talk and children's vocabulary skills is causal, we evaluate Chat2Learn, a six-month intervention that uses text messages to boost the amount that parents talk about words with their children. We tested two approaches to differentiate the effect on vocabulary between prompting parents to provide a simple word definition to their child from prompting parents to engage in more complex conversation with their child. Both approaches increased children's vocabulary scores by 0.37 standard deviations (definition) and 0.24 standard deviations (conversation).

Despite the magnitude of the treatment impacts, most children only learned the words that were part of the intervention, though quantile regressions revealed that treated children near the top end of the distribution (80^{th} percentile) also learned more non-intervention words. Nonetheless, parents can in principle teach their children many new words. The approach we tested can have a substantial impact on vocabulary as a large and diverse collection of words can be articulated in sentences, stories, and ideas. Our 6-month intervention was too short and the children in this sample were too young to observe these plausible longer-run impacts. Chat2Learn had a high retention rate (98%) and parents rated their children's reception to Chat2Learn enthusiastically. Testing a longer intervention to see if such long-run impacts arise is an important next step for future research.

These results also expose the limits of parent training programs delivered by static text messages. Although we designed the conversation approach to induce language-rich, back-and-forth conversation we do not know if that goal was achieved. The treatment impacts on program words were the same across the two treatment arms. It is also possible that parents who are less familiar with how to sustain deeper, open-ended conversations with children will benefit less

from this approach. It is possible that a static text message, even with an illustration and a script for parents to follow for parents to help engage the child in imaginative thinking, was not enough to make a conversation occur, or at least not a conversation that introduced new words. Parents might need to be prompted explicitly to use new words, such as synonyms or antonyms to the target word. New advances in technology allow us to build supports and prompts for such dynamic and expansive oral language interactions (see Bloomfield et al., 2025 for an example). AI integrations can personalize prompts to curate customized conversation prompts based on children's interests. We are currently developing and testing such AI-enhanced versions of Chat2Learn.

Toward that end, we also developed a new test for novelty preference because our conversation treatment was hypothesized to elicit the formation of curiosity among treated children. We find that the definition approach modestly boosted children's novelty preference, an indicator of curiosity (effect size .17; $p<.10$) whereas the conversation approach did not. Curiosity is increasingly recognized as an important noncognitive skill (Alan & Mumcu, 2024). We find suggestive proof of concept that curiosity is malleable in early childhood with a light-touch program, but much more research is needed. Our measure adds to the child assessment tool kit and could easily be used in classrooms or other settings.

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