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What causes the word gap? Financial concerns may systematically suppress child-directed speech

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Parents with fewer educational and economic resources (low socioeconomic-status, SES) tend to speak less to their children, with consequences for children's later life outcomes. Despite this well-established and highly popularized link, surprisingly little research addresses why the SES "word gap" exists. Moreover, existing research focuses on individual-level explanations with little attention to structural constraints with which parents must contend. In two pre-registered studies, we test whether experiencing financial scarcity itself can suppress caregivers' speech to their children. Study 1 suggests that caregivers who are prompted to reflect on scarcity—particularly those who reflect on financial scarcity—speak to their 3-year-olds less than a control group in a subsequent play session. Study 2 finds that caregivers speak less to their children at the end of the month—when they are more likely to be experiencing financial hardship—than the rest of the month. Thus, above and beyond the individual characteristics of parents, structural constraints may affect how much parents speak to their children.

word gap | scarcity | poverty | child-directed speech

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Introduction

A large body of work suggests that caregivers who have more educational and economic resources (high socioeconomic status, SES) speak more to their children than those who have less (e.g., 1–12). Popularized in 1995 as the "30 million word gap" (4), SES differences in both the quantity and quality of speech directed to children have been observed since the mid-1900s (1), and appear to have meaningful consequences for children's early language processing efficiency (13), vocabulary growth (5), brain development (9, 10, 12), and ultimately, their performance in school (4). With the popularization of the word gap came the emergence of a seemingly parsimonious explanation for the well-documented SES academic achievement gap: it seemed that differences in achievement began prior to school entry and were caused in part by differences in children's early interactions with their caregivers. As a result, many developmental scientists and policymakers have pushed for interventions that train caregivers—particularly those who are low in SES—to talk more with their children. Yet interventions targeting caregiver behaviors through training often invoke—and may even provoke—an assumption that the cause of the SES word gap is deficits in the knowledge or skills of the caregivers themselves (14). Here we explore an as yet untested hypothesis that the word gap may be in large part driven by stable, structural pressures associated with low SES that constrain caregivers' behavior and suppress their speech to their children.

In comparison to the overwhelming evidence of SES gradations in child-directed speech, there has been surprisingly little work focusing on why these differences exist. Several hypotheses have been posed and tested, most prominently (a) less parenting knowledge, (b) cultural deficits, and/or (c) deviant or inaccurate beliefs about parenting among lower SES caregivers. In one study, Rowe (6) found that scores on a test of parenting knowledge mediated the relation between SES and caregivers' speech to their children. However, at least one intervention that has successfully increased parenting knowledge long-term has not shown success at increasing child-directed speech over the same time period (15), raising questions about the causal role of parenting knowledge in the SES word gap. In another study, Hoff-Ginsberg (3) found that although mothers across SES levels believed that talking to their children was equally important and appropriate, mothers of lower SES levels spoke less both to their children and to adult researchers. Hoff-Ginsberg thus suggested a more general role of caregiver's speech style in explaining the SES word gap: perhaps lower SES individuals are simply less talkative overall (3). However, more recent studies using naturalistic measures of adult and child interactions have provided no evidence of a link between quantities of adult-directed and child-directed speech within lower SES communities (13), and have suggested that lower SES caregivers in some communities may even be more talkative than middle class caregivers (16). Thus, the root cause of observed SES differences in child-directed speech remains elusive.

More generally, the idea that stable individual differences exist between lower- and higher-SES caregivers—and that these differences drive the word gap—is undermined by the fact that the word gap persists across time and space. Despite dramatic demographic shifts in who has occupied higher- and lower-SES positions in the United States over the last 70 years, SES differences in child-directed speech have been documented since the 1950s (e.g., 1–3). These differences have also been observed beyond the United States (17, 18), in both urban and rural areas. And while caregivers within SES groups vary substantially in their child-directed speech (16), and the exact magnitude of the word gap has been debated, more recent estimates using non-intrusive record-

ing technology have confirmed group average differences in child-directed speech, suggesting the word gap may be closer to 4 million than 30 million words (11). Missing from the conversation is a careful assessment of the external forces that work to keep poor families poor, and whether poverty itself, or the constellation of experiences associated with it, might affect how parents interact with their children. Could structural factors affect families' speech above and beyond individual-level factors?

Lower-SES individuals face a variety of constraints that are likely to affect behavior in systematic ways. Below, we focus primarily on evidence from the United States, but the same or related constraints are found across the globe, in countries where there is income inequality, e.g., (19, 20). For example, lower SES renters—particularly those of color—are discriminated against in the housing market, and restricted to renting in neighborhoods where they must pay more for lower quality housing (21, 22). These neighborhoods are likely to be more dangerous, a factor that holds negative consequences for parents' distress levels and parenting style (23, 24), children's standardized test scores (25), and the caregiver-child relationship (26). Moreover, parents of young children are three times more likely to be evicted from their homes than otherwise matched adults (21), and evictions have been linked to deteriorating mental and physical health of caregivers as well as increased parenting stress (22). These constraints, and the many others faced by individuals in poverty, are intertwined with experiences of discrimination and other exploitative practices that keep the poor paying more for everyday commodities like food (27)—all of which make managing daily life, not to mention parenting, particularly taxing.

Financial hardship in particular brings with it both affective and cognitive challenges. Lacking financial resources is a defining feature of low SES, and its associated challenges are therefore salient across time and space, wherever there are individuals who are low in SES. Feeling financially restricted is not only stressful, but also means that one must plan each expenditure more carefully (28). And on top of their smaller incomes and additional expenses, the poor experience a large amount of income volatility, the unpredictability of which interferes with their ability to adequately plan for and pay for expenses (29). For example, the poor are particularly likely to work jobs in which the hours and pay are unpredictable. This makes it more difficult to cope with unexpected income shocks, such as expenses related to car repairs or medical problems. Moreover, the poor are more likely to lack insurance—or have insurance plans with high deductibles—meaning that they are more likely to incur financial shocks in these domains. Together, such factors prevent the poor from building up the liquid assets required to cope with financial shocks, let alone invest in endeavors like higher education that promote upward mobility (29).

Critically, besides taking caregivers' time and money away from their children, the factors discussed above are likely to take away their attention (28, 30). Said more simply: if you are worried about putting food on the table tonight, or

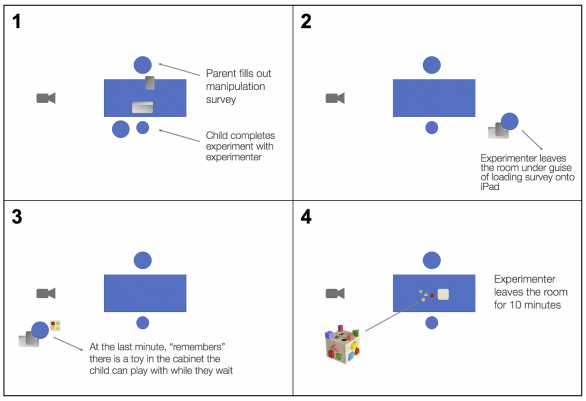

Fig. 1. Schematic of Study 1. Caregiver is seated across from child and experimenter at a table. After caregiver completes manipulation survey, the researcher leaves the dyad with a toy to play with, and a camera records caregiver-child interactions.

scraping together money for that medical bill, or figuring out where to enroll your child in school now that you have been evicted from your neighborhood—you may be less likely to narrate the color of the sky to your child as you ride together on the bus. Indeed, factors like work stress have been linked with more parenting withdrawal (31, 32), and there is even evidence that caregivers invest more in their children when structural constraints are eased (33). In this way, individual-level explanations for SES differences in child-directed speech, while not without merit, can be thought of as a small component of the phenomenon, embedded in a much larger social context (34).

In two pre-registered studies, we test whether financial concerns can meaningfully suppress parents' child-directed speech, above and beyond individual-level factors. We focus specifically on middle- and higher-SES caregivers, to ask whether experiences of financial scarcity suppress child-directed speech among caregivers who arguably possess any individual-level characteristics required to provide high levels of child-directed speech (like parenting knowledge). If financial scarcity has this effect on middle- and high-SES caregivers, it could more consistently suppress the child-directed speech of low-SES caregivers, who experience financial hardship more persistently.

Experimental Study

Our first study was inspired by a growing literature in behavioral economics suggesting that simple reminders of resource scarcity can systematically affect individuals' moment-to-moment attention and cognition (35). Thus, we asked whether reminding parents of their own experiences of scarce resources might also affect their subsequent speech to their children. Specifically, we asked whether higher-SES caregivers—who by all accounts possess the parenting knowledge that might be required for caregivers to provide high levels of child-directed speech (36)—would speak less to their children if they were first reminded of their recent experiences of resource scarcity.

Figure 1 provides a schematic of the procedure. Participants

Table 1. Demographics of participants in Experimental Study

	Condition				
	Scarcity*		Control*		*p*-value
Child age in years†	3.50	(0.29)	3.42	(0.27)	0.167
Income bins in thousands‡					0.669
Less than 50	0	(0.0)	1	(2.6)	
50–75	3	(7.7)	1	(2.6)	
75–100	2	(5.1)	4	(10.3)	
100–125	7	(17.9)	4	(10.3)	
125–150	6	(15.4)	4	(10.3)	
150–175	5	(12.8)	4	(10.3)	
175–200	5	(12.8)	5	(12.8)	
More than 200	11	(28.2)	16	(41.0)	
Caregiver education in years†	16.90	(1.53)	16.98	(1.76)	0.839
Child gender§	22	(52.4)	20	(47.6)	0.827
Caregiver gender§	8	(19.0)	6	(14.3)	0.77

* *n* = 42
† Mean (SD)
‡ *n* (%)
§ *n* (% male)

($n = 84$; see Table 1) were given a cover story about the purpose of the study (see *Methods*), and randomly assigned to the Scarcity ($n = 42$) or Control ($n = 42$) condition. In the Scarcity condition, caregivers were asked to reflect on 3–4 times in the last week when "resources were scarce" (see *Supplement*), and in the Control condition they were asked instead to reflect on 3–4 things they did in the last week (Table S1). The caregiver and child were seated across from one another at a table. The child completed an unrelated experiment with the researcher, while the caregiver completed the Scarcity or Control survey on an iPad. When the caregiver had completed the survey, the researcher left the room under the guise of loading a second survey onto the iPad, leaving the caregiver and child alone with a toy—which was introduced in an incidental way—to play with while they waited. A video camera and/or tape recorder recorded their interactions. The researcher returned after 10 minutes with the post-test survey, and debriefed families at the end of the study.

Below, we present results for all 10 minutes of the play session. Because some participants left the room early, we performed supplementary analyses to confirm that results were the same when limiting our analysis to the first two minutes; all were (see *Supplement*). Our primary outcome measures were 1) the quantity of caregivers' speech to their children, including the overall number of words spoken (word tokens) and the number of unique words (word types), and 2) the quality of their speech, including utterances that directed children's attention or behavior (directives) and those that expanded upon a child's topic of conversation (topic-continuing replies; see *Methods*).

Results.

Quantity of child-directed speech. Results are displayed in the left panel of Figure 2. For our primary pre-registered analyses, we found that on average, caregivers in the Control condition used 13 more word types, $t(80.49) = -1.44$, $p = 0.076$, $d = -0.32$, 95% CIs of d $[-0.75, 0.12]$, and 44 more word tokens, $t(79.39) = -1.2$, $p = 0.116$, $d = -0.26$ $[-0.70, 0.17]$ than those in the Scarcity condition, though this did not reach statistical significance.

Financial scarcity and quantity of child-directed speech. Results are displayed in the right panel of Figure 2. Ten out of 42 caregivers in the Scarcity condition wrote about some kind of financial scarcity (none in the Control condition did). This was unexpectedly low, as the vast majority of higher-income parents in our online pilot studies reflected on scarce financial resources (see *Supplement*). For example, one caregiver reflected on finding out that her husband hadn't been contributing to his retirement funds on top of the income he was bringing in, and explained, "What was lacking was enough disposable income for the lifestyle we were both accustomed to leading growing up." Caregivers who reflected on financial scarcity did not differ from the rest of their group in either average income, M (finances) = 168.06, M (no finances) = 147.08, $t(12.91) = -1.22$, $p = 0.245$, or years of education, M (finances) = 17.00, M (no finances) = 16.88, $t(16.78) = -0.25$, $p = 0.805$. After reflecting on finances, however, these caregivers spoke less to their children than caregivers who did not reflect on financial scarcity, reaching significance for word tokens, $t(22.56) = 2.46$, $p = 0.011$, $d = 0.73$ $[-0.02, 1.48]$, but not for word types, $t(15.58) = 1.59$, $p = 0.067$, $d = 0.56$ $[-0.18, 1.31]$. Caregivers who reflected on financial scarcity also spoke to their children significantly less than did caregivers in the control group, both for word types, $t(12.82) = 2.17$, $p = 0.025$, $d = 0.81$ $[0.08, 1.53]$, and for word tokens, $t(15.81) = 3.05$, $p = 0.004$, $d = 0.96$ $[0.23, 1.69]$. These findings suggest that caregivers may be less engaged with their children when they have thought about their own financial scarcity, as opposed to other kinds of scarcity (e.g., "Not enough time to prepare meals for family").

Income, scarcity, and quantity of child-directed speech. We found evidence for an interaction between caregiver income and the scarcity manipulation, which reached significance when predicting word types, $F(1, 74) = 6.38$, $p = 0.014$, but not word tokens, $F(1, 74) = 2.04$, $p = 0.158$. Interestingly, breaking this interaction down revealed that parents in the highest income bracket were most affected by the scarcity manipulation. While there was no difference between the Scarcity and Control participants among those in the two lower-income groups, $ps > 0.10$, there was a significant difference for those making $200,000 or more, types: $F(1, 25) = 10.24$, $p = 0.004$, $d = -1.25$ $[-2.13, -0.37]$; tokens: $F(1, 25) = 7.61$, $p = 0.011$, $d = -1.08$ $[-1.94, -0.22]$.

Quality of child-directed speech. There were no differences between conditions in the proportion of utterances that caregivers used to direct their children's behavior, $t(60.98) =$

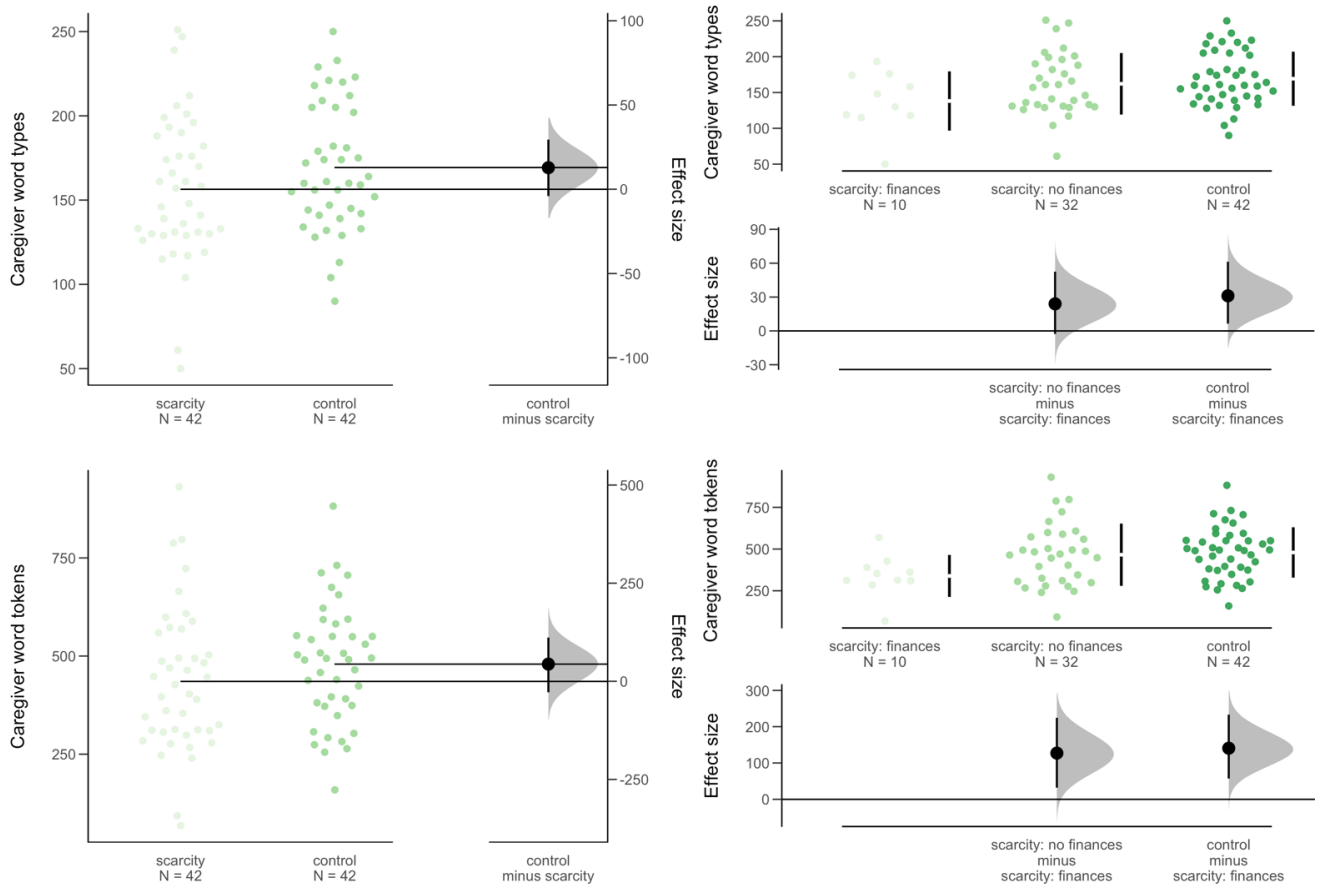

Fig. 2. Results of Study 1. Group differences in caregiver word types (top panel) and word tokens (bottom panel) displayed. Left panel shows pre-registered analyses comparing the Scarcity and Control groups. Right panel shows an exploratory comparison of the subset of the Scarcity participants who reflected on finances to Scarcity participants who did not reflect on finances, and to Control participants. Effect sizes and confidence intervals calculated using bootstrap resampling to test for a difference in means between groups (37).

0.66, $p = 0.257$, $d = 0.15$ $[-0.3, 0.61]$, nor in the proportion of utterances in which they replied to a child by continuing the child's topic of conversation, $t(68.4) = 1.35$, $p = 0.909$, $d = 0.31$ $[-0.15, 0.76]$.

Quantity of children's speech. There were no differences in children's quantity of speech production between the two conditions, either for word types, M (Scarcity) = 77.66, M (Control) = 78.03; $t(74.15) = -0.06$, $p = 0.523$, $d = -0.01$ $[-0.47, 0.44]$, or for word tokens, M (Scarcity) = 192.50, M (Control) = 193.79; $t(73.25) = -0.07$, $p = 0.527$, $d = -0.02$ $[-0.47, 0.44]$.

Post-testing. After the completion of the experiment, there were no significant differences between conditions in caregivers' self-reported positive or negative affect, nor their rated feelings of scarcity, $ps > 0.10$, paralleling our piloting results suggesting that the effect of the manipulation on perceived scarcity is brief (see *Supplement*).

Discussion.

This study presents preliminary evidence that caregivers speak less to their children when they have been reminded of their recent experiences of resource scarcity. Our strongest result comes from an exploratory analysis, suggesting that caregivers who reflected on having scarce financial resources spoke significantly less to their children than did caregivers who reflected on other kinds of scarcity and than caregivers who did not reflect on scarcity at all. We see this effect as a potential indicator of individuals who may have already been worried about financial resources in their lives, and who were given the opportunity to reflect on this concern during the study. Because these caregivers did not differ in SES from those who reflected on other kinds of scarcity, this presents initial evidence that simply having financial scarcity on one's mind—regardless of other personal characteristics—may lead caregivers to speak less with their children.

However, this result should be interpreted with caution, given its exploratory nature. Many of the participants in our Scarcity condition did not reflect on financial scarcity, and instead focused on issues that might not have the same gravity (e.g., "I ran out of fruit for my daughters which they eat at each meal"). We suggest that it may be difficult to induce feelings of resource scarcity experimentally; it is possible that our manipulation instead served to encourage those of our participants who were already genuinely grappling with financial scarcity in their lives to engage with those experi-

ences.

We also found that higher-income caregivers in the Scarcity condition showed more suppressed speech than lower-income caregivers. This is not altogether surprising, given that our manipulation was specifically designed to induce feelings of scarcity in more affluent participants, and piloting results suggested that it may have only been effective for higher-income individuals (see *Supplement*). One possibility is that higher-income individuals do not think about resource scarcity frequently, and thus are particularly affected when they are asked to think about such experiences, i.e., leading them to engage in thought patterns they do not normally have. Of course, future research is needed to replicate this result and confirm this hypothesis.

In sum, this study was limited by our ability to adequately induce feelings of resource scarcity in a laboratory setting among higher SES caregivers, and by the small number of participants who actually reflected on financial scarcity. In our next study, we sought to explore potential effects of financial scarcity on child-directed speech in a more naturalistic, observational setting.

Supplementary Note 1: Observational study

In our second study, we asked how caregivers' interactions with their children in their daily environments might vary as a function of the financial scarcity they are experiencing. We made use of a widely-studied phenomenon: Americans report experiencing more financial scarcity at the end of the month than during the rest of the month. Specifically, Americans of all income groups report being less financially secure and not having money in savings when surveyed at the end of the month (38). As a group, Americans also spend more in the first and third week of the month, around typical paycheck schedules, and less in the last week of the month (39). These observations motivated us to ask whether caregivers speak less to their children at the end of the month—when they are more likely to be experiencing more financial scarcity—than at the beginning of the month.

To test this hypothesis, we drew on three corpora of publicly available daylong language audio recordings. Children's language environments were recorded and quantified using LENA technology, and accessed from three corpora of publicly available LENA data hosted through Homebank (40–42), see Table 2. Each corpus included recordings spanning multiple time points for a given child, which varied randomly in their time of month. The median participant provided 38.5 hours of recording data (range: 8.1–394.5). Thus, we were able to use caregivers as their own controls and focus specifically on changes in their child-directed speech that might track with changes in their perceived financial scarcity over the course of a month. Our primary interest was in conversational turn count (CTC) — the number of back-and-forth adult-child vocalizations. Because adult speech (adult word count, AWC) includes a large amount of speech that is directed to other adults and children rather than to the target child, we identified conversational turns as the best proxy for the kind of contingent child-directed speech we were interested in. Notably, prior research also finds that conversational turn count is more strongly related to children's verbal scores and brain development than is overall adult word count (9, 12).

Results.

Conversational turns. We found that across all corpora, the rate of conversational turns was lower on average during the last week of the month than the rest of the month (see Figure 3). For the models without any covariates, this reached significance in the Bergelson corpus, $\chi^2(1) = 7.60$, $p = 0.006$, but not the others, Cougar: $\chi^2(1) = 1.28$, $p = 0.259$; Warlaumont: $\chi^2(1) = 0.82$, $p = 0.364$; all corpora together: $\chi^2(1) = 3.06$, $p = 0.080$. Effects held or became stronger when controlling for the overall number of adult words near the child, reaching significance in two out of the three corpora, Bergelson: $\chi^2(1) = 9.47$, $p = 0.002$; Cougar: $\chi^2(1) = 4.32$, $p = 0.038$; Warlaumont: $\chi^2(1) = 1.52$, $p = 0.217$, and when all data points were considered together, $\chi^2(1) = 7.34$, $p = 0.007$. Interestingly, however, these effects were eliminated when controlling for the overall number of child vocalizations, Bergelson: $\chi^2(1) = 0.61$, $p = 0.436$; Cougar: $\chi^2(1) = 0.34$, $p = 0.56$; Warlaumont: $\chi^2(1) = 0.39$, $p = 0.53$; all: $\chi^2(1) = 0.69$, $p = 0.407$, suggesting that the effects may be reflected particularly in child vocalizations (see Discussion).

Child vocalizations. We conducted several unplanned follow-up analyses to probe whether differences in conversational turns across the month were driven by differences in child vocalizations at the same times. We found that across all three corpora, children vocalized less on average during the last week of the month compared to the rest of the month, mirroring the effects seen for conversational turns (see Figure 4). The inclusion of time of month in a model predicting children's vocalizations yielded results that paralleled those with conversational turns, Bergelson: $\chi^2(1) = 6.89$, $p = 0.009$; Cougar: $\chi^2(1) = 3.63$, $p = 0.057$; Warlaumont: $\chi^2(1) = 2.13$, $p = 0.144$, reaching significance in all data points together, $\chi^2(1) = 7.26$, $p = 0.007$. Further, we used information quantified by the LENA software to probe who initiated the conversational turns, and found that children initiated fewer conversational turns during this last week of the month, with largely similar results, Bergelson: $\chi^2(1) = 8.97$, $p = 0.003$; Cougar: $\chi^2(1) = 1.73$, $p = 0.188$; Warlaumont: $\chi^2(1) = 1.69$, $p = 0.193$; all data points together: $\chi^2(1) = 3.89$, $p = 0.048$.

Adult word count. To probe whether all adult speech—and not just contingent speech with children—was suppressed at the end of the month, we also conducted an exploratory test of whether adult word count varied across the month. This was not the case for any of the corpora, Bergelson: $\chi^2(1) = 0.00$, $p = 0.949$; Cougar: $\chi^2(1) = 0.56$, $p = 0.454$; Warlaumont: $\chi^2(1) = 0.51$, $p = 0.475$; all datapoints: $\chi^2(1) = 0.76$, $p = 0.384$.

Table 2. Demographics of participants in Observational Study

	Corpora						
	Bergelson (m = 44)		Cougar (m = 92)		Warlaumont (m = 55)		p-value
n recordings	87		782		174		
Child gender*	24	(54.5)	44	(47.8)	32	(589.2)	0.452
Child age in years†	0.56	(0.04)	2.59	(1.15)	0.70	(0.44)	<0.001
week‡							0.239
1	27	(31.0)	202	(25.8)	52	(29.9)	
2	27	(31.0)	183	(23.4)	41	(23.6)	
3	17	(19.5)	203	(26.0)	48	(27.6)	
4	16	(18.4)	194	(24.8)	33	(19.0)	
Maternal education§‖							0.104
less than college	11	(26.2)	33	(35.9)	8	(34.8)	
college degree	9	(21.4)	28	(30.4)	10	(43.5)	
graduate degree	22	(52.4)	31	(33.7)	5	(21.7)	
Family income in thousands†‖	86.79	(29.78)	—		66.25	(33.15)	0.015
Recording duration per recording in hours†	14.32	(1.99)	11.43	(2.60)	11.58	(3.14)	<0.001
CTC rate/hour†	28.19	(12.88)	58.33	(30.75)	28.40	(16.33)	<0.001
AWC rate/hour†	1373.89	(602.99)	1395.10	(616.13)	1191.44	(570.77)	<0.001
CVC rate/hour†	96.04	(43.76)	221.66	(114.09)	109.19	(58.97)	<0.001

CTC = conversational turn count; AWC = adult word count; CVC = child vocalizations.
* m (% male)
† Mean (SD)
‡ n (%)
§ m (%)
‖ Not all participants provided education and income data; these numbers display only those for whom the information is available.

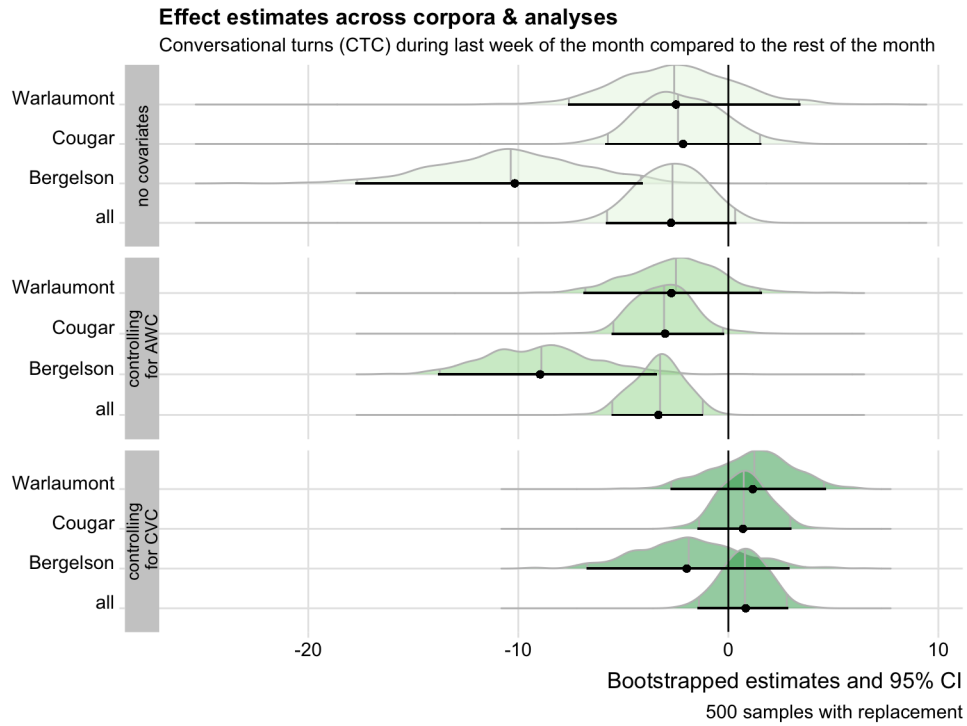

Fig. 3. Effect sizes in Study 2, across all corpora and analysis specifications. Top panel (lightest shade): models predicting conversational turn rate (CTC) from time of month alone. Middle panel: models including rate of adult words (AWC) as a covariate. Bottom panel (darkest shade): models including rate of child vocalizations (CVC) as a covariate. Values to the left of 0 indicate fewer conversational turns during the last week of the month. Effect sizes calculated using 500 resampled models; distribution of bootstrapped estimates and 95% confidence intervals displayed.

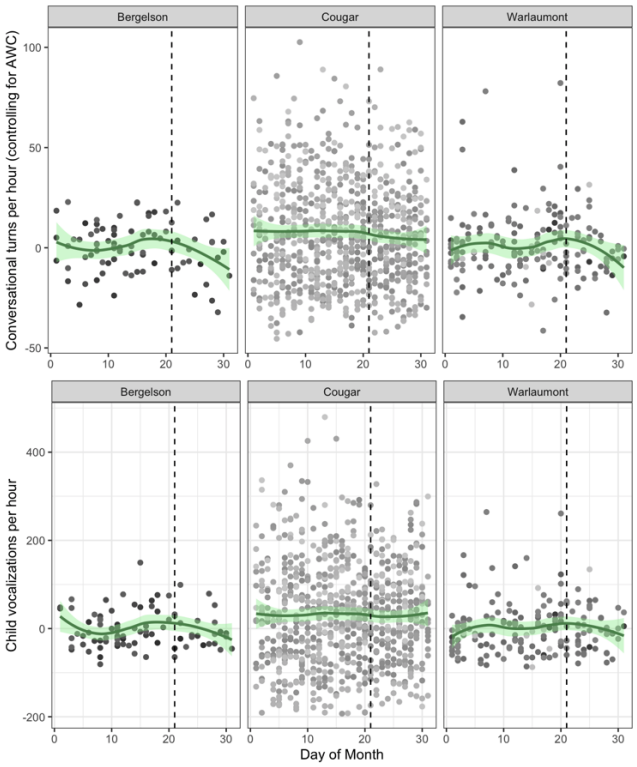

Fig. 4. Results of Experiment 2. Trends across days of a month in both conversational turns and child vocalizations. DVs centered around within-participant averages, which are further centered around the group average. Different shaded dots represent different participants.

Interactions with family income. There was a trend-level interaction of time-of-month with family income, treated as a continuous variable, $\chi^2(1) = 3.64$, $p = 0.056$. To further examine this, we split participants into a high- and low-income group based on a median split of income (high = above \$90K). We found that among higher-income participants, there was a significant time-of-month effect on conversational turns, $\chi^2(1) = 8.29$, $p = 0.004$; among lower-income participants there was no effect, $\chi^2(1) = 0.03$, $p = 0.852$.

Discussion.

We found that in at least one corpus, families reliably engaged in fewer conversational turns in the last week of the month, when they were likely to be experiencing the most financial scarcity. This pattern was evident across all three corpora analyzed, though it did not consistently reach significance. Patterns held or became more pronounced when controlling for the overall number of adult words near to the child, suggesting that it is contingent back-and-forth exchanges between caregivers and the target child in particular that decline at the end of the month. Interestingly, these effects appeared to be driven by differences in child vocalizations: children on average vocalized less during the last week of the month, and initiated fewer conversational exchanges. Extrapolating the estimates across all data points suggests that on average children engage in 2.73 fewer conversational turns per hour in the last week of the month, amounting to approximately 306 fewer conversational turns over the course of that week. These findings are consistent with the idea that caregivers may have experienced increased distress at the end of the month—due to increased financial worries—leading to less engagement with their children.

The finding that children vocalized less at the end of the month was striking and unanticipated. We speculate that this effect was still in some way driven by caregivers. Perhaps caregivers were not as physically close to their children during the last week of the month, or perhaps they initiated fewer exchanges non-verbally, for example, by making less eye contact with their children. It is also possible that caregivers were around their children less often during the last week of the month, but we find this unlikely given that overall adult word counts were not significantly lower in this last week.

It is also noteworthy that effects were strongest in one of the three corpora (40). There are several possible reasons for this finding. For one, this corpus had the most confined age group of any analyzed. All children were 6 months old at the first recording, and the second recording took place approximately one month later, reducing potential noise in the data. Moreover, this corpus had the longest recordings, which may have increased the reliability of our measures. Finally, there are also potential demographic explanations. The families in this corpus were highly educated and earned a higher income than families in the other corpora. Thus, they may have been more likely to have been paid on typical paycheck schedules, earning money on the first and third Fridays of each month (43).

Relatedly, we also found that effects of time of the month on conversational turns were in general stronger among higher-income families, mirroring the findings of Study 1. In addition to being more likely to be on a typical U.S. paycheck schedule (43), these families might show a stronger effect at the end of the month because lower-income families may feel more effects of financial strain throughout the month. Yet higher-income earners are not protected from financial strain altogether: a recent representative survey of workers found that over 75% of Americans live paycheck to paycheck, and 60% of people earning above \$100,000 report being in debt. Moreover, only 10% of all Americans save more than \$1,000 a month, suggesting that there is just a small margin of financial wiggle room even among the more affluent (44). We suggest that the scarcity that higher-income caregivers feel between paychecks is experienced in a more severe and enduring way by lower-income individuals, and that this may help to explain the persistence of the word gap.

Although we believe that financial strain at the end of the month suppressed caregivers' interactions with their children, the more proximal mechanism is unclear. Possible mechanisms include reduced conversational turns due to stress, less food consumed because of fewer funds, and the caregiver working more hours or less often being in the room with the child (though we believe that our data do not support this last explanation, as discussed above). Regardless of the more

proximal cause, our findings are important because changes in conversational turns over the course of a month within a single family must be in some way driven by changes in the external environment that act on caregivers and their children.

General Discussion

For decades, the word gap has been a topic of popular debate. In California, there are regular radio commercials encouraging caregivers to talk to their children (45). A slew of federally funded interventions and privately funded startups have mobilized parent training interventions for the same goal (46). Yet despite a long history of research on the consequences of the word gap, there has been much less systematic investigation of its cause. Across two studies, we present evidence that structural constraints—in particular concerns with financial resources—may play an important role in caregivers' speech patterns. Financial scarcity may shift caregivers' attention away from their children, thereby reducing child-directed speech, though future work is needed to address the exact mechanism through which scarcity influences parenting. This is the first study to our knowledge to suggest that such external factors could meaningfully influence parent-child interactions in children's typical, everyday environments, above and beyond the individual characteristics of caregivers.

While we focus here on financial constraints, we hope that our studies will stimulate future research on the effects of other structural constraints on child-directed speech and other parenting behaviors. For example, in addition to facing more financial insecurity, lower SES caregivers face a barrage of challenges to wellbeing, including food and housing insecurity, greater exposure to environmental toxins, unequal access to health and educational resources, over-policing, and more. Many of these are confounded with racial discrimination. In fact, for many years research on the word gap—explicitly or not—focused primarily on poor Black American families. Yet this is the very demographic most likely to face structural barriers to economic, political, and psychological wellbeing in the United States (47). Racism is frequently ignored in the modern conversation about the word gap, something we would encourage researchers to probe further. Future research should focus on how experiences of racial discrimination and other constraints associated with low SES might directly affect caregivers' interaction with their children.

Our findings hold important implications for interventions that seek to eliminate the word gap, and may help explain why the majority of existing interventions have not succeeded in altering caregivers' speech patterns long-term, (e.g., 15, 48, 49). If, as we suggest, structural constraints suppress caregivers' interactions with their children, then interventions that help alleviate those constraints, e.g., by providing families with more access to financial and other resources, may be needed to increase child-directed speech in a sustainable manner. Indeed, the most successful interventions to date appear to integrate parenting training with increasing access to other resources, such as pediatrician visits, (e.g., 50). Our suggestion that interventions should focus on easing structural constraints is not at odds with proposed explanations for the word gap like parenting knowledge; it simply locates an important source of variability in parenting knowledge at the structural level. For example, if lower SES families faced fewer obstacles in daily life, might they be more motivated to independently seek out parenting resources? There is ample opportunity for future research in this area. Natural experiments, such as the one described in our second study, may be a particularly promising avenue for investigation (51).

Finally, it is also important to understand what sources of language input children can learn from if they are not exposed to as much child-directed speech, and what learning strategies they might develop. This is particularly important given that children in many families, particularly outside of Western contexts, grow up in environments with low levels of child-directed speech (52). We note that while exposure to child-directed speech appears to have measurable consequences for children's early language development, children in cultures with much less child-directed speech still reach language milestones at similar ages (53), suggesting that there may be other avenues for learning. If, as our study suggests, caregivers' child-directed speech is suppressed by financial strain, it is important to identify potential pathways to success that are not reliant on direct caregiver input.

Taken together, these studies suggest that caregivers might speak less to their children when they are reminded of their experiences of financial scarcity (our experimental study), or when they are likely to be currently experiencing financial scarcity (our observational study). These findings indicate that the word gap could be partly explained by the cognitive or affective effects of low SES itself. We hope that these findings lead interventionists who aim to train parents to speak more to their kids to use caution. At their best, public campaigns about the word gap serve to let knowledge about child development out of the ivory tower so that it may be accessible to all caregivers. At worst, however, they reify the idea that the poor are poor because of their own shortcomings or bad decisions, and detract attention from the milieu of structural forces that any lower SES child—no matter how advanced their vocabulary—would have to face to perform well in school. The results of our studies suggest that structural-level interventions that provide lower SES families adequate access to resources—financial and otherwise—may be needed to create lasting changes in caregivers' child-directed speech.

Methods

Study 1.

Procedures and analyses were pre-registered on AsPredicted (http://aspredicted.org/blind.php?x=79ep26). All materials and analysis scripts, including the protocol to be used in the case of a future replication, are available on OSF (https://osf.io/xfreu/?view_

only=1cd220b4765f422d9dc61c5cff9e755f), and transcripts and videos are available with permissions on Databrary (https://nyu.databrary.org/volume/820). This study was approved by the Institutional Review Board at the University of California, Berkeley. Parents gave informed consent for both themselves and their child, and parents and children each received a small gift for their participation.

Participants. We recruited 100 3-year-olds and their caregivers to participate in our experimental study. This sample size was pre-registered and determined because it gave us 80% power to detect a medium effect size ($d = 0.5$). Participants were primary English-speakers and children had not been diagnosed with any language or learning delays. Of these participants, 16 had to be excluded due to pre-registered exclusion criteria: caregiver and child leaving the room less than five minutes into the play session (5); another adult or child being present or entering the room during the first five minutes (4); caregiver not following survey instructions (e.g., listing only one experience; 3); failure in video/audio recording (2); experimenter error (1); child too fussy to complete experiment (1). This left us with a final sample of 84 dyads, randomly assigned to either the Scarcity ($n = 42$) or Control ($n = 42$) condition. Dyads across conditions did not differ in age, child or caregiver sex, caregiver education, or family income ($ps > 0.10$; see Table 1).

Manipulation survey. Parents filled out an online survey that asked them to list 3 or 4 experiences from the last week and then write a brief reflection about 2 of those experiences. The only difference between surveys was that in the Scarcity condition, caregivers were specifically asked to list and reflect on times in the last week when they didn't have enough of something, or when resources were scarce. This manipulation was identical to a previous study (54), and was selected after extensive online piloting with higher-SES parents (see *Supplement*), who overwhelmingly reported on scarce financial resources.

Child experiment. Children completed an unrelated experiment with the researcher, in which they were taught the meaning of a new word ("daxy") and tested on their ability to extend this word to a different domain (e.g., learn that "daxy" means high in space; asked to extend this meaning to the domain of pitch (for more details, see 55).

Play session. The caregiver and child were left alone in the room with only a shape-sorter toy. This toy was chosen to be engaging and difficult for three-year-olds, and to open up the possibility for caregivers to use rich, scaffolding speech ("that is an octagon; it has eight sides") or to let the child play on their own.

Deviation from pre-registration. We deviated from our pre-registration in three ways. First, early in data collection, we decided not to exclude those participants who stayed in the room for at least the first 5 minutes of the 10-minute play session, given that so many families were leaving the room early. Second, upon reviewing prior literature in the creation of our qualitative coding scheme, we decided to code topic-continuing replies (following 3), as we deemed this a more age-appropriate and SES-sensitive measure than the elaborative and decontextualized utterances we had pre-registered. Third, as we describe in the results section, we performed two exploratory analyses, examining whether the efficacy of the manipulation depended on whether parents reported experiences of financial scarcity in the survey, and as a function of family income.

Analyses. All analyses were performed using R version 3.4.0. For tests in which we had a directional hypothesis, we used one-sided t-tests to compare groups. Where necessary, Cohen's kappa was assessed using the `psych` toolbox (56) and effect size for t-tests was assessed using the `cohen.d` function (57).

Language quantity. Play sessions were transcribed by trained research assistants who were blind to condition, and spot-checked by the first author. Transcribers segmented each two minutes of the play session, to allow for the examination of differences across time. Transcribers ended their transcription precisely 10 minutes after the start of the play session. Word types and word tokens were counted using the `tidytext` package in R (58).

Language quality. Two independent raters were trained on a qualitative coding scheme, adapted from (3). This coding scheme was chosen because previous studies have found SES-related differences in these measures (e.g., 3, 4). Directives were classified as any utterance which directed or instructed the child in any way, either directly or indirectly (e.g., "Go like this."). Topic-continuing replies were classified as any caregiver utterance which expanded or elaborated on the child utterance immediately preceding it (e.g., Child: "All three." Mother: "Yep so there are three of each color."). See *Supplement* for more information about the coding scheme. The raters came to agreement on twelve transcripts, resolving discrepancies after each round of coding. After this training period, reliability was assessed for every 10 transcripts to make sure that the coders did not drift in their responses. Reliability was high (directives: $k = 0.56$–0.93; total $k = 0.81$; topic-continuing replies $k = 0.76$–0.94; total $k = 0.83$).

Stability of measures across time. A linear mixed effects model predicting language measures from minutes elapsed (0–2, 2–4, 4–6, 6–8, 8–10), with subject as a repeated measure, showed that for caregivers, word types and tokens did not vary systematically as a function of time ($ps > 0.20$). Therefore, for the 7 caregivers who left the room between 5 and 10 minutes, we extrapolated their speech rate from the data available to estimate what it would have been for the full 10 minutes had it remained constant, and not been interrupted. However, analyses suggested that caregivers used relatively fewer directives and more topic-continuing replies over the course of the play session, and children used increasingly more word types and word tokens ($ps < 0.001$).

Thus, for measures in which there were no systematic differences across time we present analyses with extrapolated estimates; for measures in which there were systematic differences across time we present analyses with only those caregivers who stayed in the room all 10 minutes; and for all measures, we present analyses for the first 2 minutes of the play session, in which we have usable data for all 84 participants.

Financial scarcity. For each caregiver in the scarcity condition, two raters coded whether or not they wrote about financial concerns in any part of the survey (e.g., "I did our family budget for next year and felt very surprised how much we spent and wondered if we had enough money."). Agreement between raters was 98%. For the one case in which the raters disagreed, an agreement was reached prior to analyzing the data. In addition, the raters coded whether anyone in the control condition wrote about experiences of scarcity; none did, with 100% agreement between raters. As an exploratory analysis, we asked whether those caregivers who reflected on financial scarcity spoke less to their children than those who did not. We thought that financial scarcity in particular might affect caregivers' speech to their children, perhaps by capturing their attention (30) and/or reminding them of ongoing stressors.

Interactions with parental income. As another exploratory analysis, we asked whether the effect of the scarcity manipulation interacted with caregivers' family income. Approximately 1/3 of our sample had household income in the highest bracket, over $200,000 (32%), reflecting our recruitment strategy. We note that while this is high, the median income in the San Francisco Bay Area where this study was conducted was $101,000 in 2017 (59), classifying the majority of these families as middle- to upper-middle class (60). To account for this, we made the decision to analyze income in thirds, with the bottom third reflecting families making below $125,000, the middle third reflecting families making between $125,000 and $200,000, and the top third reflecting families making over $200,000. All results were similar when treating each income bracket as its own factor.

Post-testing questionnaires. After the completion of the play session, caregivers completed a survey asking about the extent to which they were feeling scarcity (54), their positive and negative affect (PANAS; 61), and their family income and years of education.

Study 2.

Procedures and analyses were pre-registered on AsPredicted (http://aspredicted.org/blind.php?x=gf7pd7), and all analysis scripts are available on OSF (https://osf.io/xfreu/?view_only=1cd220b4765f422d9dc61c5cff9e755f). At the time of writing our pre-registration, we had not looked at the data and did not consider that child age would vary as a function of corpus. Upon reviewing the data, we decided it necessary to consider potential effects of child age. Thus, for all analyses, we tested models without age against models with age as a random effect. When age added significant variance to the model, we retained it in subsequent analyses. All results are similar with or without including age as a covariate. Other than this, we did not deviate from our pre-registration.

Corpora. Children's language environments were recorded and quantified using LENA technology, and accessed from three corpora of publicly available LENA data hosted through Homebank (40–42), see Table 2. Briefly, LENA is a device that sits in children's front pockets and records and quantifies their natural language environment for up to 16 hours (62). This produces estimates of the adult-produced words near and clear to the child (adult word count, AWC), the number of child vocalizations (child vocalization count, CVC), and the number of back-and-forth adult-child vocalizations (conversational turn count, CTC).

We selected three corpora of publicly available LENA data hosted through Homebank (https://homebank.talkbank.org/). These corpora were selected based on the following criteria: 1) at least some families provided multiple recordings across different days, 2) caregivers were likely to be of the age and living in a country where they were receiving a paycheck and paying bills on a United States schedule, and 3) LENA data files—and not just audio—were available for download. This left us with three corpora to analyze: Bergelson (40), Cougar (41), and Warlaumont (42), as pre-registered.

Participants. Demographic information for each corpus is provided in Table 2. We planned to exclude participants who did not contribute at least 6 hours of usable data; every participant contributed at least this amount of data considering the repeated timepoints. This left us with 1,043 recordings across 191 participants.

Analyses. For all analyses, we performed linear mixed effects models using the `nlme` package in R, to account for the repeated measures in the data (63). These were fit using a maximum likelihood solution in order to allow for model comparison. Subject was included as a repeated measure (random intercept), and, where applicable, child age was included as a random slope nested within subjects. In contrast to the statistical tests in Study 1, which compare groups of subjects over a single timepoint, mixed models group each subject's data points around their average, making determining statistical significance less straightforward (64). Thus, to determine statistical significance, we compared models without the inclusion of the time of month to models with this variable included, and calculated whether time of month contributed significantly to model fit, using the `anova` function for likelihood ratio test model comparison. For language measures of interest, we calculated a rate for each recording by dividing the total count of the language measure by the recording duration. Time of month was considered as a categorical variable (beginning: days 1–23 vs. end: days 24+), as pre-registered, given that we expected to see effects concentrated in the last

week of the month. We performed each analysis separately in each corpus, and across all data points with corpus as a covariate.

Conversational turns. We were primarily interested in whether conversational turns—back and forth vocalizations between an adult and child—were reduced during the last week of the month compared to the rest of the month. We also planned to test the effect of time of month on conversational turns when controlling for the total number of adult words near to the child, to isolate the effects of speech that is likely to be child-directed versus adult-directed. In addition, we tested effects when controlling for child vocalizations. For these tests, we compared the additive power of time of month to the model, as described above, when either adult word count or child vocalizations was already included as a covariate.

Interactions with income. We also tested whether caregiver income moderated the effect of time of month on conversational turns. Only 66 participants had information about income, 44 in the Bergelson and 22 in the Warlaumont corpora. Because income brackets differed across study cites, we took the median of each income bracket and analyses were done with income median as a continuous variable, across all individuals who provided information about income with corpus as a covariate.

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Scarcity manipulation pilots

We piloted three different possible scarcity manipulations in order to select the manipulation for our experimental study. We refer to these as versions A, B, and C (see Table 3). Version A focused on the stock market; Version B focused on losing a large sum of money to a fire; and Version C focused on reflecting on recent experiences of scarcity. Only Version C had been previously used in the literature (54). Our primary test of efficacy of the manipulation was the sum of a 4-question survey of self-reported scarcity (54). Specifically, participants responded on a 7-point Likert scale rating the extent to which they agreed with the following statements: (1) My resources are scarce; (2) I don't have enough resources; (3) I need to protect the resources I have; (4) I need to acquire more resources. In all cases, pilots were completed on mTurk. Many more participants accepted the mTurk HIT than completed it; the vast majority of those who did not complete it closed out after the first screen, likely because it involved considerable writing.

Below, we present results for all four of our pilot studies, first separately and then combining across studies. Participants were randomly assigned to either the scarcity or control condition. When analyzing pilot studies with multiple manipulation versions, or when combining across pilot studies, we use linear regression and test for main effects of the manipulation (scarcity versus control) and interactions with manipulation and version (A, B, and C) or pilot study number (1, 2, 3, or 4). Significance of variables in linear regression is assessed using the `Anova` function from `car`, to determine which variables significantly contribute to model fit. Otherwise, we use two-tailed t-tests to test for a simple difference in means.

Pilot Study 1. In our first pilot study, we aimed to collect data from 150 participants across the three scarcity manipulations. 223 mTurkers began the study; of these, 152 completed it. Across our overall sample, there was no significant effect of the scarcity manipulation, $F(1,148) = 2.02$, $p = 0.157$, nor was there an interaction with the manipulation version, $F(1,148) = 0.38$, $p = 0.539$. However, given that we were ultimately interested in inducing scarcity in a higher-income population—and given that our manipulations were designed for this purpose—we performed an exploratory analysis, restricting our sample to the higher half of income earners. This left us with 91 participants who reported making $50,000 or more. Among these higher-income participants, there was a significant effect of the scarcity manipulation, $F(1,85) = 6.54$, $p = 0.012$, but again no interaction with the manipulation version, $F(2,85) = 0.59$, $p = 0.556$, suggesting that manipulations did not differ significantly in their effectiveness. However, visually inspecting the means of these groups suggested that participants who completed versions A, $M = 20.2$, and C, $M = 20.69$, had slightly higher scarcity ratings than those who completed version B, $M = 18.33$. The control groups, on the other hand, had similar ratings, A: $M = 17.38$, B: $M = 16.73$, C: $M = 16.33$. Thus, in our next pilot study, we focused on manipulations A and C. This time, we used mTurk screening to restrict our sample to parents whose household income was at least $100,000, in line with the demographic we anticipated in our lab study.

Pilot Study 2. In our second pilot study, we again aimed to collect data from 150 participants, this time for only versions A and C, and restricting our sample to mTurkers who reported being parents and making over $100,000. 227 mTurkers began the study; of these, 153 completed it, 93 in version A and 60 in version C. Mirroring our findings from our first pilot study among higher-income earners, we found a significant effect of the scarcity manipulation, $F(1,149) = 6.17$, $p = 0.014$, and no interaction with manipulation version, $F(1,149) = 0.83$, $p = 0.364$. At this point, we decided to focus on version C, given that it had been used by prior studies (54), and had what we believed to be a better-matched control condition.

Pilot Study 3. In our third pilot study, we aimed to collect data from 75 participants, again restricting the sample to mTurkers who reported being parents and making over $100,000. 138 mTurkers began the study; of these, 76 completed it. Given that we no longer needed to account for the version of the manipulation, we performed a simple t-test to test the difference in means between the scarcity and control group. In line with previous pilot studies, the scarcity group reported experiencing more scarcity, $M = 21.2$, than the control group, $M = 15.85$, $t(51.52) = 4.85$, $p < 0.001$.

Pilot Study 4. Finally, in an attempt to test how long effects of the scarcity manipulation may last, we administered participants a longer survey in which the manipulation check always came after participants had completed other measures. 281 participants began the survey and 150 completed it. After completing the scarcity manipulation, but before filling out the manipulation check, the participants completed a long version of the Stroop task (100 questions), the short-form PANAS (61), two questions about participants' locus of control (65), and four questions about their parenting locus of control; in counterbalanced order. We found that after completing this large number of filler tasks, participants across conditions did not significantly differ in their self-reported scarcity, $t(132.91) = 1.74$, $p = 0.083$, though there was a trend in the expected direction. Thus, to the extent that feelings of resource scarcity can be primed, they may not persist after performing interim activities.

All Pilot Studies. We combined all the pilot studies to test the effects of the scarcity manipulation in a higher-powered sample. Participants frequently reflected on scarce financial resources. For these analyses, we included only those participants from Pilot Study 1 who were higher-income, and only those participants from Pilot Studies 1 and 2 who completed version C of the manipulation, given the focus of our later pilot studies and our ultimate experimental study. This left us with a sample of

Table 3. Scarcity manipulation versions

Version	Scarcity	Control	Prompt
A	Imagine that the federal administration makes a decision that has catastrophic consequences for the economy, resulting in a stock market crash. As a result, values of assets diminish, the price of daily goods increases, and companies funded by venture capitalism suffer incredible losses.	Imagine that the federal administration makes the decision to increase the tax rate slightly. As a result, people in your income bracket will end up paying $100 more per year.	In the space below, please respond to the following questions: Would your family be directly affected by this? How would you deal with this? Would it cause you long-term hardship?
B	Imagine that there was a fire in your house. The fire spread extensively, destroying many valuable items. The cost of repair is estimated to be $150,000.	Imagine that there was a fire in your house. The fire was contained, destroying only a few items. The cost of repair is estimated to be $500.	In the space below, please respond to the following questions: Are there ways in which you may be able to come up with that amount of money on very short notice? How would you go about it? Would it cause you long-lasting financial hardship? Would it require you to make sacrifices that have long-term consequences? If so, what kind of sacrifices?
C	Please describe three or four times in the past week when you felt like you didn't have enough of something, or resources were scarce.	Please describe three or four things you did in the past week.	Now, please pick two of these times and describe in detail what was lacking and what you experienced.

Table 4. Summary of pilots (version C)

		Condition*			
Pilot	n	Scarcity		Control	
1	28	20.69	(4.48)	16.33	(6.67)
2	60	18.05	(7.76)	14.82	(3.28)
3	76	21.20	(5.11)	15.85	(4.00)
4	150	17.22	(5.45)	15.67	(5.24)

* Mean (SD). Includes only those participants who are higher income and completed version C.

314 participants. Across all pilot studies, there was a highly significant effect of manipulation condition, $F(1,306) = 25.89$, $p < 0.001$, a significant effect of pilot study number, $F(3,306) = 2.89$, $p = 0.036$, and a trend level interaction of manipulation with pilot study number, $F(3,306) = 2.40$, $p = 0.068$. Visualizing the means across conditions and pilots (see Table 4) suggested that while the means of the control condition were similar across pilot studies, the extent of self-reported scarcity in the fourth pilot was slightly reduced compared to the rest of the pilots, in line with the idea that the effects of the scarcity manipulation may not have been particularly durable.

In addition to measuring self-reported scarcity, we also administered a number of exploratory measures, including a Stroop task (66) (Pilot Studies 1, 3, and 4), positive and negative affect (61) (Pilot Studies 1 and 4), and Locus of Control (65) (full Locus of Control survey: Pilot Study 2; single questions: Pilot Study 3, 4; parenting questions: Pilot Study 4). Of these, only positive affect differed significantly as a function of manipulation condition (scarcity versus control), $F(1,174) = 5.74$, $p = 0.018$. Specifically, positive affect was slightly lower for participants completing the scarcity manipulation, $M = 27.95$, than the control manipulation, $M = 30.95$.

Taking all the pilot studies together, we determined that our manipulation was effective in inducing self-reported feelings of scarcity in a higher-income sample of parents. However, it was less clear whether it had any other downstream effects, whether it would persist through subsequent activities, and whether it would be effective for a lower-income sample.

Table 5. Topic-continuing utterances: Definitions and examples

Code	Types/Explanation	Examples
Topic_cont	Entity or event is referred to in prior child's utterance (includes non-verbal information)	CHI: All three. MOT: Yep so there are three of each color.
	Comments on objects referred to in prior utterance	CHI: Yeah it's pretty even. MOT: They all have equal colors.
	Answering a question (includes confirmation of something asked)	CHI: Can you play with this with me? MOT: Sure.
	Continued patterned speech	Reciting alphabets Counting Nursery rhymes
	Paraphrase or repetition of prior utterance	CHI: Fill her up. MOT: Put the gas in.
	Direct responses that propels topic forward	CHI: Let's do this. MOT: Okay.
	Acknowledgment of child (confirms or engages specifically with what the child has said, rather than simply acknowledging that they have spoken)	CHI: I want to be in that other play part. MOT: You want to be in that other side I know they have to get me one more survey I need to fill out.

CHI = Target child; MOT = Mother

Lab study: Qualitative coding scheme

Below, we present the definition, examples, and notes used for the purposes of qualitative coding of caregivers' utterances in Study 1. These were adapted based on (3).

Topic-continuing utterances. A topic-continuing utterance was defined as a caregiver utterance directly following a child utterance that referred to, commented on, paraphrased, responded to, or answered the child's previous utterance; continued patterned speech; or directly responded to the child's utterance in a way that propelled the topic of conversation forward. Examples of each of these kinds of topic-continuing utterances are listed in Table 5.

Additional notes from coders on tricky cases.

- Clarifying/asking for clarification is topic continuing ("What did you say?")
- Affirmation is topic continuing ("That's right")
- "No" with no additional information is not topic-continuing
- "Okay" is not topic continuing unless it involves direct acknowledgment of content of child's speech, like agreeing to an action (see Table 5)

Directives. Directives were defined as a caregiver utterance that directs or instructs the child's behavior or attention in the moment, either directly or indirectly. Examples of each of these kinds of directives are listed in Table 6.

Additional notes from coders on tricky cases.

- "No" is a directive when it is clearly prohibiting actions that the child is already doing/about to do (e.g., child reaches toward toy, mother says "no")

Table 6. Directive utterances: Definitions and examples

Code	Types/Explanation	Examples
Directive	Instructive directives manage the child's attention or behavior.	MOT: Go like this while you get your finger in there.
		MOT: Look at all these little sides.
	Prohibitive directives limit the child's behavior, attention, or task completion.	MOT: Off the table.
	Indirect directives imply a desired behavior or action without doing so in a commanding or prohibitive way	MOT: You might have to flip it around to another side to find the right one for that one.
		MOT: Should we put this away first?

CHI = Target child; MOT = Mother

- "Let's" statements are directives, except for "let's see" ("Let's put this here")
- "Let me…" is not directive, except for some cases of "let me see," depending on context and implication (e.g., if it is truly a request to see something)
- "Wait" is a directive, including "oh wait"

Lab study: Supplementary results

Given that some caregivers left the room before the full 10 minutes, we supplement our main analyses with analyses of the first two minutes of the play sessions below.

Quantity of child-directed speech. Caregivers in the Scarcity condition showed numerically suppressed speech compared to caregivers in the Control condition in the first two minutes of the play session, word types: $t(81.2) = -1.36$, $p = 0.089$, $d = -0.30[-0.73, 0.14]$; word tokens: $t(81.98) = -1.59$, $p = 0.058$, $d = -0.35$ $[-0.78, 0.09]$.

Quality of child-directed speech. There were no differences between conditions in the proportion of utterances that caregivers used to direct their children's behavior during the first two minutes, $t(78.14) = -0.28$, $p = 0.611$, $d = -0.06$ $[-0.5, 0.38]$, nor in the proportion of utterances in which they replied to a child by continuing the child's topic of conversation, $t(79.52) = 0.36$, $p = 0.639$, $d = 0.08$ $[-0.36, 0.52]$.

Quantity of children's speech. There were no differences in children's quantity of speech production between the two conditions in the first two minutes, either for word types, $t(81.99) = 0.27$, $p = 0.395$, $d = 0.06$ $[-0.38, 0.49]$, or for word tokens, $t(81.94) = 0.27$, $p = 0.393$, $d = 0.06$ $[-0.37, 0.49]$.