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The Effect of Unconditional Cash Transfers on Maternal Assessments of Children's Early Language and Socioemotional Development: Experimental Evidence From U.S. Families Residing in Poverty

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Economic disadvantage has often been associated with poorer performance on measures of early childhood development. However, the causal impacts of income on child development remain unclear. The present study uses data from the Baby's First Years randomized control trial to identify the causal impact of unconditional cash transfers on maternal reports of early childhood development. One thousand racially and ethnically diverse mothers residing in poverty were recruited from four U.S. metropolitan areas shortly after giving birth. Mothers were randomized to receive either a $333/month or $20/month unconditional cash transfer for the first several years of their child's life. Maternal reports of language and socioemotional development, concerns for developmental delay, and enrollment in early intervention services were collected annually at the time of the child's first, second, and third birthdays. In this registered report, we document no statistically detectable impacts of the high-cash gift on maternal reports of child development. We discuss the significance and implications of these findings.

Public Significance Statement

This study investigates the impact of the first 3 years of an ongoing, 6-year monthly unconditional cash transfer intervention for families of newborns with low income. Findings show no effect on maternal reports of children's early language and socioemotional development. We discuss a range of plausible interpretations for these findings and directions for future research.

Keywords: unconditional cash transfers, poverty, randomized controlled trial, early language development, early socioemotional development

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The study's design and hypotheses were preregistered (see https://clinicaltrials.gov; identifier: NCT03595546). Anonymized data and materials for the study are publicly available on the Inter-University Consortium for Political and Social Research (ICPSR) at https://www.icpsr.umich.edu/web/DSDR/studies/37871/versions/V7 (Magnuson, Noble, et al., 2024). The analytic code for the current analyses can be found on openICPSR and can be accessed at https://doi.org/10.3886/E159422V3.

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continued

Poverty and discriminatory systems that give rise to poverty shape the experiences of individuals, families, and communities, which in turn affect children's early environments, experiences, and development (Bronfenbrenner, 1992; García Coll et al., 1996; Iruka et al., 2022). Children residing in families with very low income often demonstrate lower performance on measures of child development when compared with their more economically advantaged peers (National Academies of Sciences, Engineering, and Medicine, 2019). These patterns are present early in childhood and tend to widen across the first several years of life (DiPrete & Eirich, 2006; Duncan et al., 1994; Noble et al., 2015). Indeed, income-related disparities in development observed across children's formal schooling years are generally present at school entry (Reardon, 2011).

Although environments and experiences influence human development throughout the life course, early childhood is thought to be a particularly important period during which interventions may be effective at preventing the emergence of income-related disparities. The developing brain is particularly sensitive and malleable to experience during early childhood (Farah, 2017; Gunnar & Quevedo, 2007; Noble & Giebler, 2020). The brain's sensitivity in early childhood has been proposed as one explanation for why higher household income during early childhood is more strongly associated with later adult outcomes, compared with income in other developmental periods (Duncan et al., 1998, 2010; Votruba-Drzal, 2006). Thus, preventing and reducing the impacts of poverty during the earliest years of childhood may have benefits for immediate- and long-term development (Aizer et al., 2022; Almond et al., 2018; Heckman, 2006).

Intervening to directly increase family income is one straightforward and feasible preventative policy strategy. Some experimental and quasi-experimental evaluations of earned-income supplements and tax credits in the United States (e.g., Dahl & Lochner, 2005; Duncan et al., 2011) and experimental evaluations of cash transfer programs in global contexts (e.g., Fernald et al., 2008) have shown small positive impacts on early child development, though other experimental evaluations have found statistically nonsignificant effects (e.g., Huston et al., 2003; Macours et al., 2008). However, in these studies, it is difficult to identify whether program impacts are attributable to income per se, because many of these programs bundle income supports with additional treatments or condition them on specific behaviors such as employment. Most studies do not include the period of early childhood—when socioeconomic disparities first emerge—or consider key developmental domains, such as language and socioemotional development.

Correlational research using observational and longitudinal data that charts how poverty is associated with language and socioemotional development during the first few years of life faces a different set of limitations. First, these studies have frequently investigated the sensitivity of language and socioemotional development to variations in composite measures of family socioeconomic status (SES), of which income is just one component. Such studies have generally found evidence that lower family SES is associated with lower performance on language and socioemotional measures in childhood (e.g., Pace et al., 2017; Piotrowska et al., 2015), but have not examined the role of poverty or income specifically (see Duncan & Magnuson, 2012). Studies that have specifically examined associations with income have often (e.g., Dearing et al., 2001, 2006), but not always (Brito et al., 2020; Melvin et al., 2017; Noble et al., 2015), found evidence that lower family income is associated with lower performance on measures of language and socioemotional skills in early childhood. However, confidently parsing the independent effects of income from other correlates of income, such as parental education, is not possible in these studies, given the limitations of correlational design (Duncan et al., 2004).

Taken together, the previous studies have not clearly determined whether directly targeting income, without targeting other aspects of socioeconomic disadvantage (e.g., educational access, employment opportunities, other systemic inequities), will have positive effects on children's early development. The aim of the present study was to shed new light on this question by identifying the effect of a monthly unconditional cash transfer, provided to mothers, on their assessments of children's development in early childhood. The study employs a randomized control trial design to yield causal impacts of income, as distinct from its correlates, on measures of children's development.

Family Income and Children's Early Development

Family economic conditions are posited to affect early childhood development via two mechanisms: (a) enabling parents to spend

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time and money in ways that support children and (b) reducing parents' psychological distress that may interfere with their engagement in developmentally supportive caregiving behaviors (see Duncan et al., 2017).

Correlational research on this topic has often operationalized income as one component of larger SES composites and has indirectly examined associations between income and child outcomes in socioeconomically diverse samples. SES composites typically consist of some combination of family income, parental educational attainment, and parental occupational prestige (Duncan & Magnuson, 2014). Higher SES has generally been associated with higher scores on measures of language (e.g., Pace et al., 2017) and socioemotional skills (e.g., Piotrowska et al., 2015). A smaller collection of studies, reviewed below, has focused on income specifically. These studies provide estimates that are more relevant to informing expectations about the probable causal impacts of an income supplementation program than estimates from studies using socioeconomic composites.

Compared with peers with higher household incomes, children from families with lower incomes tend to perform worse on direct assessments of language development in the first 3 years of life, though these associations may differ by age. For example, some studies have found associations between income and both expressive and receptive language development by the time children are 3 years of age (Romeo et al., 2022), while others have found no statistically significant links between family income and receptive or expressive language skills in infancy (Brito et al., 2020; Melvin et al., 2017) or toddlerhood (Noble et al., 2015). Dearing et al. (2001) found that increases in family income over the first 3 years of life were associated with higher scores on measures of expressive and receptive language skills at age 3 among children in very low-income families, but not among children in families with higher incomes.

Examinations of socioemotional outcomes have found higher levels of maternally reported externalizing, or behavioral, problems (e.g., acting out, aggression, inattention; McConnell et al., 2011; A. B. Miller et al., 2021) among children with lower household income than among children with higher household income. Using temporal variation within families, Dearing et al. (2006) found that mothers reported that their child had fewer behavior problems when their family income was relatively high, compared with when their income was relatively low. Another study found that trajectories of increased household income across the first 3 years of life for children residing in poverty were associated with significantly more positive social behaviors at age 3 and fewer behavioral problems at age 3, though the latter was not statistically significant (Dearing et al., 2001).

Studies examining associations between family income and clinically defined developmental delays (typically conceptualized as low performance across language, socioemotional, and/or motor domains) and receipt of early intervention services have generally found that lower income has been associated with greater likelihood of meeting criteria for delay on developmental screeners (Çelikkiran et al., 2015; Demirci & Kartal, 2018; Ozkan et al., 2012). However, children with developmental delay raised in families with lower incomes are less likely to receive early intervention services, compared with children with developmental delay raised in families with higher incomes (Clements et al., 2008; McManus et al., 2020).

Collectively, this body of findings suggests mixed evidence for the theory that intervening to increase family income among families with low income will improve children's language and socioemotional development. Importantly, this correlational work is limited in several ways. First, associations between income and child development may be biased by a host of omitted variables. It is impossible to conclude that observed associations between income and child development are caused by income per se; instead, they may reflect a range of other factors such as important family (e.g., parental education) and community (e.g., school and housing quality) characteristics.

Second, it is unclear how much can be inferred about children in families with very low income from associations derived from samples that span the full income distribution. Correlations between income and child outcomes among families with low income may be more relevant for generating expectations about the potential impacts of poverty reduction interventions than estimates from samples with higher income families. The handful of studies that *have* focused on income variability at the low end of the income distribution suggest some evidence of associations between income and child development. For example, Dearing et al. (2001) found that increases in income were only associated with better language and socioemotional development at the lower versus higher end of the income distribution (also see Duncan et al., 1994, 2011, for similar findings among older children).

Antipoverty Programs and Children's Development

Randomized control trials of welfare reform programs in the 1990s offered an opportunity to test the effect of multipronged, employment-based antipoverty programs on the development of preschool- to middle-school-aged children. These reforms combined employment- and work-related requirements, child care supports, and earnings supplements to encourage work among single parents, predominantly single mothers. Evaluations of such multipronged programs have examined impacts on measures of cognitive development, consisting of a mix of reading and math test scores and parent- and teacher-reported achievement. These evaluations showed small but positive effects on cognitive development a few years after randomization among children who were preschool-aged at the time of enrollment (Duncan et al., 2011; Morris et al., 2001, 2005). However, the programs had statistically nonsignificant, small, and mixed-direction impacts on prosocial behaviors and externalizing behaviors among young children (Gennetian & Miller, 2002; Morris et al., 2001). Impacts on child development from birth to age 3 were not available because of participant inclusion criteria. Findings from the evaluation of a similar study, called New Hope, which offered parents with very low income a bundle of child care, health, and earned-income subsidies, found statistically nonsignificant and small impacts on reading and socioemotional development for children who were 1–3 years old at study entry, 5 years after randomization (C. Miller et al., 2008).

A related body of quasi-experimental work has estimated the effects of the Earned Income Tax Credit (EITC)—tax refunds targeted to working families with low income—on child outcomes. Increases in EITC generosity have been associated with benefits in reading among 5- to 9-year-old children (Dahl & Lochner, 2005). Receipt of the EITC in the year following the birth of an infant,

determined by a birth date cutoff, has also been linked to long-run benefits on an index of reading and math performance among children when they reached the third grade (Barr et al., 2022). While promising, EITC studies were not designed to estimate impacts on early development, with the notable exception being the estimation of impacts on infant birth weight (Hoynes et al., 2015).

Together, these findings suggest some promise that income enhancement may yield impacts on preschool- to school-aged children's cognitive development. However, as was made clear by Duncan et al.'s (2011) efforts to isolate the effects of income increases from other program components involved in the 1990s' welfare randomized control trials, the bundling of program components and conditioning of income enhancement on parental employment limit the extent to which the observed effects can be causally attributed to changes in income alone. Additionally, it is not possible to ascertain impacts on child development from birth to age 3 from these studies.

Evaluations of cash transfer programs (particularly conditional cash transfers) in low- and middle-income countries have examined impacts on preschoolers' language and related cognitive outcomes (see de Walque et al., 2017). Evaluations of the Oportunidades conditional cash transfer program in Mexico found positive impacts on language development among 3- to 6-year-olds (Fernald et al., 2008). Another conditional cash transfer randomized control trial in Nicaragua found that, for the youngest children in the sample (newborn through age 3), the program had small, statistically nonsignificant impacts on language development and socioemotional skills (Macours et al., 2008). As with the welfare reform studies, uncoupling the impact of income enhancement from other changes (e.g., parental employment, children's school enrollment, and receipt of health care) is difficult. One exception is a study of a monthly unconditional cash transfer to mothers with very low income in Ecuador, which found nonsignificant main effects on child language and behavioral outcomes for children 1–3 years old at the time of assessment (Fernald & Hidrobo, 2011).

Income Supplementation and Early Development Among Families With Low Income

In summary, existing empirical work leaves open many questions about the direct impact of income on development in early childhood, when socioeconomic disparities first emerge. Developmental theory suggests that increases in family income should have positive effects on early development. Correlational work on the empirical associations between income and early development also suggests the possibility that direct income support may have positive effects on development for young children residing in poverty. However, it is not possible to conclude from this body of research whether associations are due to the causal impact of income specifically. Conclusions from evaluations of poverty-reducing interventions help fill this gap but also have limitations, as they have typically tested a bundle of policies that simultaneously affect other types of behaviors (e.g., parental earnings) which can have independent effects on children's development. Even the most promising studies of income transfers cannot speak to impacts on early child development in the U.S. context and in the first 3 years of life. The Baby's First Years (BFY) randomized control trial of unconditional cash transfers is uniquely positioned to evaluate the causal effects of income supplementation on early development for families with low income.

The Present Study

In BFY, mothers were randomized to receive either a large ($333) or small ($20) unconditional cash transfer—referred to from here on as the high-cash and low-cash gift groups—every month for the first several years of their child's life.[1] The present study examined the differential impacts of receipt of the high-cash gift versus the low-cash gift on maternal assessments of children's language and socioemotional development over the course of the first 3 years of life. We hypothesized that mothers randomized to the high-cash gift group would report more favorable child language and socioemotional development and less concern about children's language, socioemotional, and general development, compared with mothers in the low-cash gift group, in the first 3 years of life (see https://clinicaltrials.gov, identifier: NCT03593356, for BFY preregistration). Given correlational evidence that socioeconomic disparities emerge and widen (DiPrete & Eirich, 2006) with increased duration of exposure to socioeconomic disadvantage (Duncan et al., 1994; Noble et al., 2015), we predicted that the effects of the high-cash gift would become progressively larger as children aged (i.e., as they experienced more months of the high-cash gift).

The present study also examined the impact of the BFY monthly unconditional cash gift on receipt of early intervention services, which families may access to address clinically significant delays in their child's language and socioemotional skills. It is not clear how a monthly unconditional cash gift might affect receipt of early intervention services. On the one hand, we might expect children in the high-cash gift group to be *less* likely to receive early intervention services compared with those in the low-cash gift group, reflecting our prediction that the cash gift would facilitate early development. On the other hand, children in the high-cash gift group may be *more* likely to receive services, if the cash gifts increase mothers' attention to developmental patterns and their bandwidth both to access services where professionals identify developmental issues in young children and to successfully enroll in these services upon referral.

Finally, the present study explored whether impacts of the high-cash gift on measures of child development were moderated by the following participant characteristics: child sex assigned at birth, maternal educational attainment, and depth of family poverty at study entry. Each of these characteristics could shape the extent to which the cash gifts affected the mechanisms hypothesized to impact child development (increased investments and reduced stress; see Gennetian et al., 2024; Magnuson, Duncan, et al., 2024). Income-boosting programs often result in heterogeneous impacts by child sex, with some evidence that boys benefit more than girls from increases in income (see Huston et al., 2005) and other work suggesting the opposite (see Tanner et al., 2015). In the case of maternal educational completion and family income, the BFY high-cash gift could have larger impacts among mothers with lower educational completion or family income, who may stand to gain the most from income enhancement (see Magnuson et al., 2009, which found that improvements in maternal education were most beneficial for child development among families with the lowest levels of educational attainment). Alternatively, the high-cash gift could be more effective among families who have relatively higher

[1] The gender of the person who gave birth was not collected at the time of enrollment. For ease of exposition, the term "mother" is used throughout the article in referring to this parent.

educational attainment or income and an existing level of baseline resources.

Findings from the larger BFY study have shown that the cash gifts led to an approximately 20% increase in household income for the high-cash gift group over baseline incomes (Gennetian et al., 2024). Despite this, most families remained economically disadvantaged, with 94% of families in both treatment groups reporting annual incomes at or below 200% of the federal poverty line by age 3 (Gennetian et al., 2024). To date, the study team has found evidence that the high-cash gifts increased parental investments (e.g., more child-focused expenditures on books and toys; more time spent reading and telling stories; greater consumption of fresh produce; Gennetian et al., 2024; Sperber et al., 2023). However, the high-cash gift group did not experience decreases in material hardship or improved maternal well-being (e.g., stress, mental health, happiness) relative to the low-cash gift group (Magnuson, Duncan, et al., 2024).

Method

BFY Randomized Control Trial

Data for this analysis came from the BFY study (Magnuson, Duncan, et al., 2024). The institutional review boards of Teachers College (Protocol 18-210) and the New York State Psychiatric Institute (Protocol 7606) approved the study. BFY is a randomized control trial in which mothers experiencing poverty were randomized to receive a monthly unconditional cash gift of either $333/month or $20/month (a difference of $313/month) for the first several years of their child's life. The $313/month difference is similar in magnitude to income increases in evaluations of welfare expansions and the EITC (e.g., Dahl & Lochner, 2012; Duncan et al., 2011). Additional detailed description of the BFY study design can be found in Noble et al. (2021) and in the BFY preregistration on https://clinicaltrials.gov (identifier: NCT03593356).

Between May 2018 and June 2019, 1,000 mothers with incomes below the U.S. federal poverty threshold were enrolled in the study shortly after giving birth in 12 hospitals across four metropolitan areas (New York City, the greater New Orleans metropolitan area, the greater Omaha metropolitan area, and Minnesota's Twin Cities of Minneapolis/St. Paul). To be eligible for participation, mothers had to be of legal age for informed consent (either 18 or 19 years old depending on site), speak either English or Spanish (due to availability of validated child outcome measures in those languages), reside in the state of recruitment and not report being highly likely to move to a different state or country within 12 months, have newborns that did not require intensive care, and have newborns who would be discharged to their custody. Shortly after birth, 13,482 mothers were approached and offered the opportunity to participate in a study of child development. Of these, 1,051 were eligible for participation, agreed to participate, and completed a baseline survey in the hospital. Following completion of the survey, mothers were offered the opportunity to receive a monthly unconditional cash gift for the first several years of their child's life, which they were free to spend however they wished.[2] A total of 1,003 mothers agreed to receive the cash gift, at which point they were randomized to the high-cash ($333 per month; treatment group) or low-cash ($20 per month; control group) gift group. Three mothers notified the research team within 3 days of completing the baseline survey that they wanted to withdraw from the study prior to having spent any of the money, resulting in a final sample of 1,000 mothers. Randomization procedures were specified so that 40% of mothers were assigned to the high-cash gift group ($n = 400$) and 60% to the low-cash gift group ($n = 600$). Mothers were debriefed on the randomization process (i.e., interviewers informed them of the two cash gift groups).

After randomization, and before mothers left the postpartum ward, the interviewer activated a debit card—a Mastercard with a "4MyBaby" logo—that was preloaded with the first cash gift. Monthly cash gifts are automatically disbursed on the card on the day of the child's birth date, accompanied by a text message. Mothers are free to use the cash gifts loaded on the debit card as they wish and are provided access to a 4MyBaby customer service hotline, in addition to the Mastercard customer service line, to call for support for a lost, stolen, or malfunctioning card (Gennetian et al., 2023). The debit card mechanism for cash gift disbursement has been successful. Of the 900 families who consented for their debit card transactions to be analyzed for research purposes, only five mothers (all from the low-cash gift group) had never used the card by the Age 3 assessment (Gennetian et al., 2024).

Participants

Table 1 details the baseline characteristics of the full BFY sample. Overall, randomization proved successful; the high-cash and low-cash gift groups were balanced across a host of demographic characteristics. About half of the children were female. On average, mothers were approximately 27 years old and had completed less than a high school degree at the time of enrollment (around 11 years of school). Approximately 41% of mothers were Hispanic, predominately originally from the Dominican Republic (34%), Mexico (16%), or the United States (33%). Among those who identified as non-Hispanic, about 2% identified as American Indian or Alaskan Native, 1% identified as Asian or Pacific Islander, 42% identified as Black, 4% identified as having multiple races, 2% reported their race as "other," and 10% identified as White.

Figure 1 details retention across the Age 1, Age 2, and Age 3 data collection follow-up waves. At the Age 1 assessment, 93% of BFY families took part in data collection (high-cash gift group: 96%; low-cash gift group: 91%). At Age 2 assessment, approximately 92% of the original sample provided data (high-cash gift group: 95%; low-cash gift group: 91%). At the Age 3 assessment, about 92% of the original sample provided data (high-cash gift group: 95%; low-cash gift group: 90%). Supplemental Tables S1–S3 present baseline demographics for participants who completed Age 1, Age 2, and Age 3 assessments, respectively. The null hypothesis of no group differences at each assessment could not be rejected in joint tests, suggesting that among those that completed the surveys, the cash-gift groups were well-balanced. Supplemental Table S4 presents the baseline characteristics for responders and nonresponders at each

[2] At the time of recruitment, the distribution of the cash gifts was planned for the first 40 months of the children's lives. This was subsequently extended twice. First, in response to the need to postpone in-person data collection due to the COVID-19 pandemic, the cash gifts were extended for an additional year, through the first 52 months of children's lives. More recently, motivated by evidence that the likelihood of unfavorable outcomes increases with the number of years a child spends in poverty, additional philanthropic funding enabled the extension of the monthly cash gifts for two more years, through 76 months of age.

Table 1
BFY Baseline Characteristics and Equivalence Tests of the High- and Low-Cash Gift Groups

Baseline characteristic	Low-cash gift		High-cash gift		Standardized mean difference		
	M (*SD*)	*n*	*M* (*SD*)	*n*	Hedges' *g*	Cox's index	*p*
Child is female	0.50	600	0.48	400		−0.06	.46
Child weight at birth (pounds)	7.13 (1.08)	599	7.09 (1.01)	399	−0.04		.57
Child gestational age (weeks)	39.09 (1.25)	596	39.04 (1.24)	399	−0.04		.51
Mother age at birth (years)	26.80 (5.82)	600	27.38 (5.86)	400	0.10		.11
Mother education (years)	11.88 (2.83)	593	11.88 (2.96)	398	−0.00		.98
Mother race/ethnicity: White, non-Hispanic	0.11	600	0.09	400		−0.17	.13
Mother race/ethnicity: Black, non-Hispanic	0.40	600	0.44	400		0.11†	.09
Mother race/ethnicity: multiple, non-Hispanic	0.04	600	0.03	400		−0.18	.37
Mother race/ethnicity: other or unknown	0.05	600	0.03	400		−0.37†	.07
Mother race/ethnicity: Hispanic	0.41	600	0.41	400		0.01	.59
Mother marital status: never married	0.42	600	0.49	400		0.18*	.02
Mother marital status: single, living with partner	0.26	600	0.22	400		−0.14	.12
Mother marital status: married	0.21	600	0.21	400		0.02	.79
Mother marital status: divorced/separated	0.05	600	0.03	400		−0.37†	.06
Mother marital status: other or unknown	0.06	600	0.04	400		−0.18	.40
Mother health is good or better	0.88	600	0.92	400		0.25*	.04
Mother depression (CESD)	0.68 (0.45)	600	0.69 (0.46)	400	0.02		.81
Cigarettes per week during pregnancy	5.05 (21.17)	595	3.45 (11.76)	397	−0.09		.11
Alcohol drinks per week during pregnancy	0.17 (1.63)	598	0.03 (0.39)	399	−0.11†		.05
Number of children born to mother	2.40 (1.38)	600	2.53 (1.41)	400	0.09		.15
Number of adults in household	2.12 (1.00)	600	2.03 (0.96)	400	−0.09		.16
Biological father lives in household	0.40	600	0.35	400		−0.12	.15
Household combined income	22,466 (21,360)	562	20,918 (16,146)	370	−0.00		.22
Household income unknown	0.06	600	0.07	400		0.14	.48
Household net worth	−1,981 (28,640)	531	−3,308 (20,323)	358	−0.00		.42
Household net worth unknown	0.12	600	0.10	400		−0.09	.64

Note. Joint test: $\chi^2(30) = 33.98$, $p = .24$, $n = 1{,}000$. *p* values were derived from a series of ordinary least squares bivariate regressions in which each respective baseline characteristic was regressed on the treatment status indicator using robust standard errors and site-level fixed effects. The bivariate regressions were also run without site-level fixed effects, and the *p* values differed on average by .011. The *p* values without fixed effects do not appear in the table. The joint test of orthogonality was conducted using a probit model with robust standard errors and site-level fixed effects. Standardized mean differences were calculated using Hedges' *g* for continuous variables and Cox's index for dichotomous variables. For the purposes of presenting baseline balance in missingness rates, a dummy variable indicating missingness is presented if the baseline covariate had more than 10 missing cases. For the purposes of the joint test, mean imputation was used to account for baseline covariates missingness for all covariates with missingness, and a dummy variable indicator for missingness was included in the joint test model. Chi-square tests of independence were conducted for the two categorical variables: mother race/ethnicity and mother marital status. For both tests, $p > .05$. BFY = Baby's First Years; CESD = Center for Epidemiological Studies Depression Scale.
* $p < .05$. † $p < .10$.

assessment. In two of the three waves, joint tests could reject the null hypothesis of no difference, with nonrespondents reporting worse physical and mental health in the baseline interview. These baseline characteristics are included as control variables in our regression models that produce treatment impact estimates.

Procedure

Following study enrollment, data were subsequently collected around the time of the children's first, second, and third birthdays. On average, children were 13 months old at the time of the Age 1 assessment, 24.7 months old at the Age 2 assessment, and 36.9 months old at the Age 3 assessment. The Age 1 assessment began as an in-home assessment. However, due to the emergence of the COVID-19 pandemic, home visits were stopped on March 13, 2020, and data collection pivoted to phone-based survey administration. Likewise, planned in-person assessments could not be administered at the Age 2 and Age 3 waves in adherence with COVID-19 precautions, and data collection was again limited to phone surveys. At each wave, a comprehensive survey was administered in the mother's preferred language (either English or Spanish). As previously described, the baseline questionnaire was administered prior to randomization. To support trust and familiarity with the study, interviewers who recruited families were retained to maintain contact with families and to collect data at subsequent waves, when possible. However, interviewers were not informed (for new interviewers) or reminded (for continuing interviewers) of participants' treatment status.

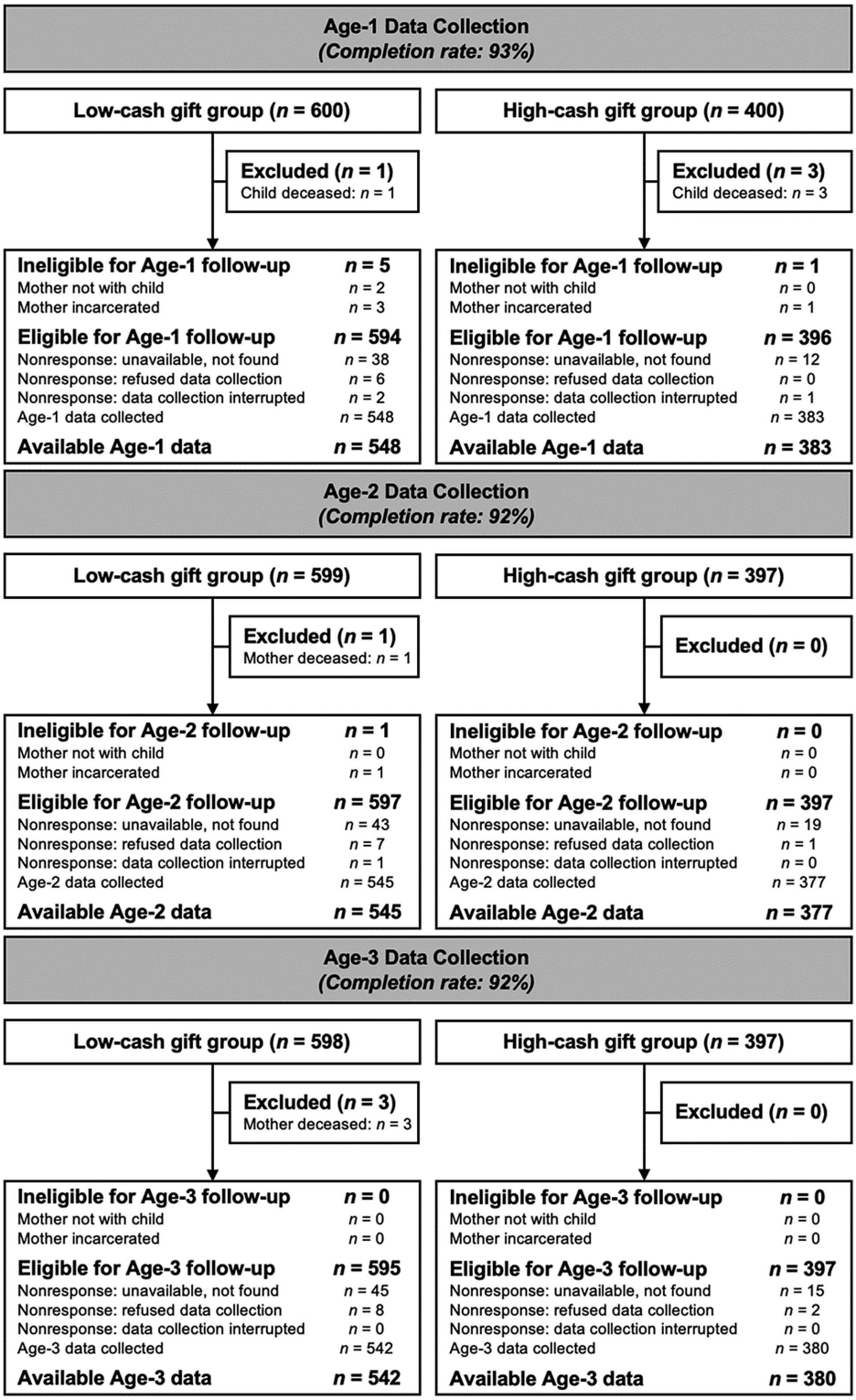
Measures

Language Development

Ages and Stages Questionnaire. During the Age 1 data collection wave, infant language milestones were captured using the Communication subscale of the Ages and Stages Questionnaire, Third Edition (ASQ-3; Squires et al., 2009). Items on the ASQ-3

Figure 1

Consolidated Standards of Reporting Trials Diagram



Note. The Consolidated Standards of Reporting Trials diagram depicts the number of participants who completed at least one developmental assessment at the Age 1, Age 2, and Age 3 assessments. At the Age 2 assessment wave, two mothers completed the MacArthur–Bates Child Development Inventory assessment but no other parts of the Age 2 assessment. These mothers are included as respondents with available Age 2 data in this diagram. At the Age 3 assessment wave, two mothers completed some portion of the survey but did not complete at least one assessment of child development and, as such, were marked as not responding due to data collection interruption.

varied based on developmental stage, with norming occurring in 2-month segments (e.g., 10- to 12-month version, 12- to 14-month version). The ASQ has been validated for use with children ages 2–60 months (Squires et al., 2009). Thus, either the 12-, 14-, 16-, or 18-month version of the ASQ-3 was administered based on the child's age at assessment. The Communication subscale included six items capturing children's achievement of developmentally appropriate expressive and receptive language milestones (e.g., "Does your baby make two similar sounds, such as 'ba-ba', 'da-da', or 'ga-ga'?"). For each item, mothers reported the frequency with which their child demonstrated the language skill, if at all (0 = *not at all*, 1 = *sometimes*, 2 = *regularly*). Item scores were summed to calculate total raw scores and then were transformed into *z* scores for analysis given that different versions of the scale were administered based on child age. Higher scores indicated more frequent demonstration of developmentally relevant language milestones. Participants had to have no more than three missing items to receive a Communication subscale score. One additional developmental concern variable was created based on the summative scores. This differentiated scores into three categories of risk for developmental delay: further professional assessment may be needed due to potential developmental delay (2), the child would benefit from monitoring and additional learning activities (1), or the child's development appeared to be on schedule (0). Participants had to answer all items to receive a developmental concern score.

MacArthur–Bates Child Development Inventory. During the Age 2 wave of data collection, child expressive vocabulary was measured using the MacArthur–Bates Child Development Inventory short form (MCDI; Fenson et al., 2000), which has been validated in low-income English- and Spanish-speaking samples (Song et al., 2012). The MCDI utilized maternal report to measure child vocabulary production. Level II, Form B, which has been validated for use among children ages 16 months to 30 months old, was utilized (Fenson et al., 2000). The assessment included a checklist of 100 developmentally appropriate words (e.g., "yum yum," "kitty," "cloud," "no," "run," "today"). For each word, mothers indicated whether they had heard their child say the word (including a childlike pronunciation of the word) or not. Thus, higher scores indicated that the child had said more words.

Prior to administration of the MCDI, mothers were asked what language(s) their child heard most at home. If they selected either English or Spanish but not both, then only the English or Spanish version of the MCDI was administered, respectively. If they selected that their child heard both English and Spanish, then both versions were administered. Of note, due to timing constraints, mothers were sent the MCDI to complete online after the phone-based survey data collection. We requested that mothers complete the assessment right away. However, many mothers did not complete the assessment. Thus, there was a lower response rate for this measure than for other assessments (73% completion in the low-cash gift group and 76% completion in the high-cash gift group).

The English and Spanish versions shared 26 items in common. Since the English and Spanish versions of the assessment were normed independently, performance on these common 26 items was totaled to form a "conceptual score," which we use in our primary analyses. For participants who completed either the English version or Spanish version only, the respective English or Spanish score was used. For those who completed both the English and Spanish versions of these 26 items, the higher score between the two was utilized in analyses. Additional sensitivity checks were performed utilizing the full 100 items and associated percentile scores (see Supplemental Tables for details).

Parents' Evaluation of Developmental Status. During the Age 3 wave of data collection, maternal concern for child language development was measured using two items from the Parents' Evaluation of Developmental Status (PEDS; see below for more details). Mothers answered two questions about whether they had concerns about their child's expressive language (i.e., "Do you have any concerns about how your child talks and makes speech sounds?") and receptive language (i.e., "Do you have any concerns about how your child understands what you say?"). For each question, mothers reported either *no* (0), or *yes*, or *a little* (1). Item scores were summed to create a total score with a minimum of 0 and a maximum of 2. Higher scores indicated more maternal concern for child language development. Participants had to answer both questions to be assigned a score.

Socioemotional Development

Brief Infant–Toddler Social–Emotional Assessment. During the Age 1 and Age 2 waves of data collection, children's socioemotional development was measured using the Brief Infant–Toddler Social–Emotional Assessment (BITSEA; Briggs-Gowan et al., 2004). The BITSEA measures children's socioemotional issues and delays, as well as their socioemotional competencies, and has been validated for use with children ages 12–36 months (Briggs-Gowan et al., 2004). The survey includes 42 maternal report items, 31 of which comprise the Behavioral Problems subscale and 11 of which comprise the Competencies subscale. During the Age 1 wave of data collection, only the Behavioral Problems subscale was administered, whereas during the Age 2 data collection wave, both subscales were administered. For each of the items on the Behavioral Problems subscale (e.g., "Is restless and can't sit still" and "Hits, bites, kicks you or other parent") and the Competencies subscale (e.g., "Tries to help when someone is hurt, e.g., gives a toy"), mothers reported the frequency with which their child demonstrated the behavior (0 = *not true/rarely*, 1 = *somewhat true/often*, or 2 = *very true or happens often*). Total subscale scores were created by summing the subscale items. Higher scores on the Behavioral Problems subscale indicated more behavioral problems, and higher scores on the Competencies subscale indicated greater socioemotional competence. Participants had to have no more than five missing items to receive a behavioral problems score and no more than two missing items to receive a competencies score.

Of note, at the Age 1 assessment, eight of the 31 item responses were incorrectly administered to include four response options (i.e., 1 = *not at all worried*, 2 = *a little worried*, 3 = *worried*, 4 = *very worried*). This error was consistent across the full sample. To correct for this error, these eight items were recoded with scores from 0 to 2, with the third and the fourth items scored as 2.

Child Behavior Checklist. During the Age 3 wave of data collection, socioemotional development was measured using a short version of the Child Behavior Checklist (CBCL; Achenbach & Rescorla, 2000). The CBCL measures children's behavioral problems across various domains and has been validated for use among children from 18 months to 5 years (Achenbach & Rescorla, 2000). The Anxiety/Depression (eight items), Aggressive Behavior (19 items), Attention Problems (five items), and Emotionally

Reactive (nine items) subscales of the CBCL were administered, which together comprised 41 items measuring children's behavioral problems. For each item, mothers reported the extent to which each behavior was a problem for their child in the preceding 2 months (0 = *not true*, 1 = *somewhat or sometimes true*, 2 = *very true*). Items from each subscale were summed and converted into four standardized *T* scores according to the CBCL conversion table. Additionally, items were summed across the subscales to create a total behavioral problems score. Higher *T* scores and total scores indicated more behavioral problems. Participants had to answer at least 21 items (50% of items) for a total score to be calculated. They had to answer all of the subscale items for a subscale *T* score to be calculated.

Parents' Evaluation of Developmental Status. During the Age 3 wave of data collection, maternal concern for child socioemotional development was measured using the Parents' Evaluation of Developmental Status (PEDS; see below for more details). Mothers answered two questions about whether they had concerns for their child's socioemotional development: "Do you have any concerns about how your child behaves?" and "Do you have any concerns about how your child gets along with others?" For each question, mothers reported either *no* (0) or *yes* or *a little* (1). Item scores were summed, and the resulting measures had a minimum score of 0 and a maximum of 2. Higher scores indicated higher maternal concern for socioemotional development. Participants had to answer both questions for a total score to be calculated.

General Parental Concerns About Developmental Delays

The PEDS questionnaire was gathered during the Age 3 data collection. The PEDS captured mothers' concern for their child's development and has been used as a clinical screener for developmental delays (Glascoe, 1997). The PEDS has been validated for use among children from birth through age 8 (Glascoe, 2003). The survey included eight questions addressing maternal concern for the development of expressive and receptive language, fine and gross language skills, behavioral problems, social emotional development, independence, and learning. For each item, mothers reported whether they had any concern for each behavior: *no* (0) or *yes* or *a little* (1). Mothers were also asked two open-ended questions: "Please list any concerns about your child's learning, development, and behavior," and "Please list any other concerns." These responses were reviewed and coded. When mothers indicated a concern about any of eight aforementioned areas in their open-ended responses, this item was scored 1 if the mother had not already indicated concern for this item when responding to the respective question. Responses to these questions were also coded for fit within the categories of "global cognition" and "other/health."

Two scores were calculated from the PEDS questions. First, a total concerns score was calculated by summing responses to the eight questions and the two additional items (global cognition and other/health), with a minimum score of 0 and a maximum score of 10. Higher scores indicated higher maternal concern for developmental delay. Participants had to answer at least five items for a score to be calculated. Second, a total predictive concerns score was calculated by summing the number of developmental concerns mothers endorsed that the PEDS has deemed predictive of developmental delay at age 3. These areas included concern surrounding global/cognitive, expressive language, receptive language, gross motor, and other/health. The predictive concerns scores ranged from 0 to 5, with higher scores indicating greater maternal concern for development in areas considered predictive of developmental delay. Participants had to provide a response for at least two of the predictive concern items for a score to be calculated.

Receipt of Early Intervention Services

During the Age 2 and Age 3 waves of data collection, mothers were asked whether their child had ever received any early intervention services, including speech therapy, physical therapy, or occupational therapy. Mothers' responses were coded as 1 (*yes*) or 0 (*no*).

Omnibus Measures of Child Development

Two omnibus measures of child development (one for socioemotional development and one for language development) were created by standardizing and averaging all of the available measures (e.g., ASQ, MCDI, and PEDS language questions for the language domain; BITSEA, CBCL, and PEDS socioemotional questions for the socioemotional domain) collected across all assessment waves.[3] Negatively valenced scores (e.g., BITSEA problem scores) were reverse scored prior to standardization and averaging such that higher composite scores indicated more desirable outcomes (e.g., fewer behavioral problems, greater language skills). Participants had to have all measures nonmissing to be assigned composite scores.

Principal component analysis (PCA) was conducted to ensure that the measures loaded together sufficiently well. For the Language Composite, the PCA produced one factor with an eigenvalue greater than 1 (eigenvalue = 1.69) with loadings ranging from .51 to .64. For the Socioemotional Composite, the PCA also produced one factor (eigenvalue = 2.38) with loadings for four of the five measures ranging from .42 to .54. The loading for the Age 2 BITSEA Competencies measure was lower than acceptable (loading = .27). Thus, we estimated the treatment impact on a composite comprising all measures (see primary results) and a composite that did not include the BITSEA Competencies measure (see Supplemental Material; results did not substantively change).

Analytic Plan

Following preregistration protocols (see https://clinicaltrials.gov; identifier: NCT03593356), regression models were used to estimate intent-to-treat impacts on each of the measures of early development. These preregistered analyses included site-level fixed effects, a host of covariates collected at study entry, and standard error adjustments (i.e., robust variance estimation) for any further correlation in variance to increase the precision of the treatment impact estimates (Cameron et al., 2008). Additional analyses were used to test the sensitivity of the findings to alternate analytic and measurement decisions. Impact estimates were reported in the original valence of the measure and in standard deviation units of the low-cash gift group.

Models included covariates measured at study entry, with the goal of improving impact estimation precision. These included the mother's age, mother's years of completed schooling, household

[3] Of note, to avoid duplication, the developmental concerns score generated using the ASQ at the Age 1 wave was excluded from the creation of the composites from the outset, because the variable was created based on the ASQ total score, which was already represented in the composite.

income, net worth, general health, depressive symptoms, race and ethnicity, marital status, number of adults in the household, number of other children born to the mother, whether the mother drank alcohol during pregnancy, whether the mother smoked during pregnancy, biological father living with the mother, child's sex assigned at birth, birth weight, and gestational age at birth. Additionally, some non-preregistered postrandomization covariates were included: (a) whether data were collected over the phone or in person for Age 1 analyses (given that the COVID-19 pandemic interrupted in-person data collection),[4] (b) questionnaire administration language (English or Spanish), and (c) child age in months at the time of assessment. The Supplemental Material details how missing data for covariates were handled.

To avoid making Type II errors, Westfall–Young familywise adjustments for multiple comparisons were made when more than one assessment of language or socioemotional development was collected for a single data collection wave (Westfall & Young, 1993). "Family" groupings used for these adjustments are indicated in Table 2. The study was originally designed to allow for the detection of an effect of .207 *SD*s for a given measure based on an anticipated 20% attrition rate.

Exploratory Analyses

We also conducted several exploratory analyses. First, we estimated intent-to-treat impacts on an omnibus composite of child language and socioemotional development across assessment waves, created by averaging together all of the language and socioemotional measures, respectively. Second, we examined moderation of intent-to-treat impacts by three theoretically motivated family, maternal, or child characteristics collected at baseline: (a) child sex assigned at birth, (b) whether maternal educational attainment was less than the sample median (12 years), and (c) whether family income was less than 50% of the official poverty level for the respective family size (i.e., "deep poverty"; Nguyen et al., 2020).

Transparency and Openness

Anonymized data and materials for the study are publicly available on the Inter-University Consortium for Political and Social Research at https://www.icpsr.umich.edu/web/DSDR/studies/37871. The analytic code used in the current analyses are publicly available on openICPSR at https://doi.org/10.3886/E159422V3.

Results

Descriptives

For all measures, children in the BFY sample scored close to national averages and/or close to averages reported in measure validation studies. The first two columns of Table 2 present the means and standard deviations for each of the measures for both the high-cash and low-cash gift groups. For the ASQ measure of language milestones collected at the Age 1 assessment, average scores among infants in the BFY sample were slightly higher than those reported in the socioeconomically diverse validation sample (Squires et al., 2009).[5] For the BITSEA Behavioral Problems measure collected at the Age 1 and Age 2 assessments and BITSEA Competencies collected at the Age 2 assessment, scores among children in the BFY sample were similar to those reported in the BITSEA validation article, which collected data on a socioeconomically diverse sample (Briggs-Gowan et al., 2004). For the MCDI measure of language development collected at the Age 2 assessment, average scores were near the 50th percentile for children in the BFY sample who completed the English assessment and near the 45th percentile for children who completed the Spanish assessment.

The majority of mothers in the BFY sample reported no or few concerns about their child's development as captured by the PEDS. At the Age 3 wave, nearly 70% of mothers reported no concerns predictive of developmental delay, 20% reported one predictive concern, and 10% reported three or more concerns. Across all developmental domains, more generally, 53% of mothers had no concerns for development, about 21% reported one concern, 21% reported between two and three concerns, and 5% reported four to nine concerns. Approximately 7% of mothers reported accessing early intervention services at the Age 2 wave. At the Age 3 wave, 12% reported accessing such services, lower than the rate observed by Clements et al. (2008) for families with low SES and comparable to the sample-wide estimate of 13.7% across socioeconomic levels.

Impacts of the High-Cash Gift on Maternal Assessments of Early Child Language and Socioemotional Development

Overall, there were no statistically significant differences in maternal reports of child development between children in the high-cash versus low-cash gift groups. Table 2 presents impacts of the high-cash gift on each maternal assessment of child development and the exploratory Language Composite and Socioemotional Composite. For language outcomes, high-cash gift treatment impacts ranged from −0.07 *SD* to 0.08 *SD* ($SE = 0.06$–0.08, $p = .24$–$.91$). The impact on the Language Composite score was 0.04 *SD* ($SE = 0.09$, $p = .64$).

For socioemotional outcomes, impacts of the high-cash gift ranged from −0.01 to 0.06 *SD* and were not statistically significantly different from 0 ($SE = 0.07$, $p = .38$–$.95$). The impact on the Socioemotional Composite score was −0.04 *SD* and not statistically significant ($SE = 0.07$, $p = .61$).

There were no statistically significant impacts of the high-cash gift on maternal concerns of their child's development at the Age 3 wave (0.06 *SD*; $SE = .07$, $p = .41$) or on maternal concerns predictive of developmental delay (0.04 *SD*; $SE = 0.07$, $p = .53$). Likewise, there were no differences in maternal endorsement of having accessed early intervention services between the high-cash and low-cash gift groups at the Age 2 wave (−0.01 *SD*; $SE = 0.07$, $p = .93$) or Age 3 wave (0.00 *SD*; $SE = 0.07$, $p = .96$).

[4] See the Supplemental Material for additional consideration of the extent to which differences in data collection modality impacted our results.

[5] Recall that different versions of the ASQ were administered depending on the child's age at the time of the assessment. While 76% of the sample were administered the 12-month version, 16% were administered the 14-month version, 6% were administered the 16-month version, and 2% were administered the 18-month version (reflective of the majority of assessments occurring near children's first birthday). In comparison to the validation sample, averages for those who completed the 12- and 14-month assessments (92% of the sample) were slightly higher, and averages for the small subset of the sample who completed the 16- and 18-month assessments were slightly lower. Additional descriptive information on ASQ scores by version type can be found in the Supplemental Material.

Table 2
Descriptive Statistics and Treatment Effects for Maternal Assessments of Children's Development

Maternal report measure of child development	High-cash gift *M* (*SD*)	Low-cash gift *M* (*SD*)	Glass's Δ	ES (*SE*)	*p*	*p*, adjusted	High-cash gift *n*	Low-cash gift *n*
Language								
Age 1 (ASQ)	0.29 (0.85)	0.20 (0.89)	0.11	0.08 (0.07)	.24	$.38_a$	377	524
Age 1 (ASQ–Concerns) –	0.09 (0.33)	0.13 (0.38)	−0.09	−0.07 (0.06)	.30	$.38_a$	377	524
Age 2 (MCDI)	16.57 (7.21)	16.64 (7.33)	−0.01	0.01 (0.08)	.91	—	289	393
Age 3 (PEDS) –	0.36 (0.63)	0.38 (0.65)	−0.03	−0.01 (0.07)	.85	—	377	542
Language Composite	0.03 (0.76)	−0.01 (0.73)	0.06	0.04 (0.09)	.64	—	274	351
Socioemotional skills								
Age 1 (BITSEA–Problems) –	8.78 (6.60)	8.44 (6.22)	0.05	0.06 (0.07)	.38	—	382	547
Age 2 (BITSEA–Problems) –	11.73 (7.18)	11.57 (7.31)	0.02	−0.01 (0.07)	.88	$.99_b$	376	543
Age 2 (BITSEA–Comp.)	17.55 (3.23)	17.61 (3.44)	−0.02	0.00 (0.07)	.95	$.99_b$	374	541
Age 3 (CBCL) –	18.48 (12.98)	18.51 (13.26)	−0.00	0.02 (0.07)	.81	$.87_c$	377	542
Age 3 (PEDS) –	0.41 (0.69)	0.39 (0.67)	0.02	0.03 (0.07)	.66	$.87_c$	378	542
Socioemotional Composite	−0.02 (0.70)	0.01 (0.66)	−0.05	−0.04 (0.07)	.61	—	358	490
Developmental concerns								
Age 3 (PEDS total score) –	1.14 (1.85)	1.08 (1.72)	0.03	0.06 (0.07)	.41	$.52_d$	378	542
Age 3 (PEDS predictive concerns) –	0.48 (0.87)	0.47 (0.82)	0.02	0.04 (0.07)	.53	$.52_d$	378	542
Early intervention services								
Age 2	0.06 (0.24)	0.07 (0.26)	−0.04	−0.01 (0.07)	.93	—	374	543
Age 3	0.12 (0.32)	0.12 (0.33)	−0.02	0.00 (0.07)	.96	—	378	542

Note. Means and standard deviations are in raw units. Glass's Δ presents the raw differences in nonrounded means divided by the nonrounded control-group standard deviation. (Nonrounded means and standard deviations were used. As such, reported effect sizes do not exactly match what would be calculated using the rounded means and standard deviations reported in this table.) Effect size coefficients, standard errors, and associated *p* values are from regression analyses with robust variance estimation techniques, site-level fixed effects, and covariates (see below). Impacts are in control-group standard deviation units. Adjusted *p* values reflect Westfall–Young adjustments for multiple comparisons when there were multiple assessments within a wave and domain. Subscripts on the adjusted *p* values indicate the family groupings used for Westfall and Young *p* value adjustments (e.g., "Family 1" comprised both ASQ measures collected at the Age 1 assessment), and "—" indicates that there was one comparison within the wave/domain and adjusted *p* values were not estimated. Language and Socioemotional Composites were formed by standardizing and averaging all of the Language or Socioemotional scales available across all waves (with negatively valenced scales reverse scored), respectively, such that higher scores indicates higher language or socioemotional performance. For all other measures, effects are presented in the original valence of each measure; "–" next to the measure name indicates a negative valence (i.e., more language concerns), and no symbol indicates a positive valence (i.e., higher language performance). To approximate intervention impacts in percentage points (a more interpretable statistic) for the two dichotomous outcomes (early intervention services at ages 2 and 3), one may multiply the reported covariate-adjusted effect size by the control-group standard deviation. Covariates were mother's age, mother's years of completed schooling, household income, net worth, general health, depressive symptoms, race and ethnicity, marital status, number of adults in the household, number of other children born to the mother, whether the mother drank during pregnancy, whether the mother smoked during pregnancy, biological father living with the mother, child's sex assigned at birth, birth weight, gestational age at birth, child age, questionnaire administration language, and whether data were collected over the phone or in person (for Age 1 outcomes only). ES = effect size; *SE* = standard error; ASQ = Ages and Stages Questionnaire; ASQ Concerns = one item from the ASQ that indicates whether the child's ASQ score provides reason for concern about development; MCDI = MacArthur–Bates Child Development Inventory; PEDS = Parents' Evaluation of Developmental Status; BITSEA = Brief Infant–Toddler Social–Emotional Assessment; BITSEA–Problems = BITSEA Problem Behaviors subscale; BITSEA–Comp. = BITSEA Competencies subscale; CBCL = Child Behavior Checklist.

Sensitivity Checks

We examined the consistency and sensitivity of the main findings to alternative analytic decisions, attrition considerations, and measurement choices. First, we tested the robustness of estimates to a model in which we dropped baseline covariates (see Supplemental Table S5). The estimates were in some cases slightly larger in magnitude than our primary estimates (0–0.03 *SD* larger), though still not statistically different than 0.

Second, we examined whether attrition affected our findings, and we found that it had no substantive impact on the main findings. Supplemental Table S4 shows differences in baseline characteristics for the small number of nonrespondents versus the respondents with at least one child development assessment at each data collection wave. Overall, a joint test of differences in baseline characteristics suggested significant differences between respondents ($n_{\text{Age 1}} = 931$, $n_{\text{Age 2}} = 924$, $n_{\text{Age 3}} = 920$) and nonrespondents ($n_{\text{Age 1}} = 69$, $n_{\text{Age 2}} = 76$, $n_{\text{Age 3}} = 80$) at the Age 1 and Age 2 waves but not at the Age 3 wave (see Supplemental Table S4 for details). At the Age 3 wave, there were statistically significant differences in four of the 26 baseline characteristics (at $p < .05$). Impacts of the high-cash gift were substantively similar when nonresponse weights were applied (see Supplemental Table S5; differences ranging from 0 *SD* to 0.01 *SD*). We constructed the weights using baseline characteristics and the Toolkit for Weighting and Analysis of Nonequivalent Groups (Griffin et al., 2014).

Next, we examined whether the main findings held across alternative measurement decisions and found that they did. Across alternative versions of the outcomes, we could not reject the null hypothesis of no difference between the high-cash and low-cash gift groups. Impacts of the high-cash gift on each CBCL subscale were comparable with impacts on the nontraditional CBCL total score used in this study (see Supplemental Table S6). Impacts of the high-cash gift on alternative MCDI percentile scores (instead of the conceptual score, which we used because some participants completed the Spanish assessment and others completed the English assessment,

which were not conormed) did not differ from the main findings (see Supplemental Table S7). An alternative Socioemotional Composite, in which we dropped the BITSEA Competencies measure due to its low factor loading, produced an impact similar to the full composite impact (see Supplemental Table S8). For full transparency, Supplemental Table S9 presents the complete regression output for the primary models.

Exploratory Analyses

Across 44 of the 45 models tested, we found no statistically significant interaction between receipt of the high-cash gift and child sex, maternal educational attainment, or deep poverty at birth (see Supplemental Table S10).

Discussion

This preregistered study sought to understand the causal impacts of unconditional cash transfers on maternal assessments of development during early childhood. We examined the impact of a monthly unconditional cash gift on maternal reports of child development through the first 3 years of the child's life. The study had sufficient statistical power to detect effects of approximately 0.20 *SD* or greater. Across maternal assessments collected when children were 1, 2, and 3 years old, we could not reject the null hypothesis that there were no differences between the high- and low-cash gift mothers' reports of children's language or socioemotional development, general developmental concerns, or receipt of early intervention services. Effects did not differ by child sex or by maternal education completion or depth of family poverty at the time of birth. Findings were robust across alternate analytic approaches and adjustments for differential attrition.

It is important to interpret these findings in the context of the broader BFY cash gift intervention and impacts on other economic and family outcomes. Uptake of the cash gift was very high. BFY families received the monthly cash gift as intended, per automatic delivery of the cash gift via a debit card, and nearly all of the cash gift money was spent or withdrawn at ATMs. Families that received the high-cash gift had higher family income before taxes by approximately 20% across waves (Gennetian et al., 2024); however, the vast majority of BFY families were still considered to have low income—defined as having household income below 200% of the official federal poverty line—at the Age 3 wave of data collection (Gennetian et al., 2024). The high-cash gift did not reduce material hardship or improve wealth or maternal well-being (Gennetian et al., 2024; Magnuson, Duncan, et al., 2024).

Other BFY research to date has shown mixed effects of the high-cash gift on mechanisms hypothesized to affect child development. On the one hand, families that received the high-cash gift demonstrated significantly higher levels of investments in the child, in terms of both goods and time. The mothers in the high-cash gift group have consistently reported spending about $70 more in the month prior to the assessments on child-focused expenditures, such as books and toys, than mothers in the low-cash gift group (Gennetian et al., 2024), as well as more time engaged in developmentally supportive activities, such as reading and storytelling (Gennetian et al., 2024). Mothers receiving the high-cash gift also reported that their children consumed more fresh produce by age 2 (Sperber et al., 2023). These effects of the high-cash gift ranged from about 0.10–0.30 *SD* in magnitude.

However, over the first 3 years, the receipt of the high-cash gift did not lead to improvements in maternal stress or mental health, nor has it led to improved self-reports of maternal happiness (Gennetian et al., 2024; Magnuson, Duncan, et al., 2024). Additionally, the high-cash gift did not generate significant impacts on observed parent–infant interaction quality, maternal use of harsh disciplinary behaviors, or engagement in infant-directed speech (Egan-Dailey et al., in press; Magnuson, Duncan, et al., 2024). Moreover, no statistically significant impacts have been observed on maternal reports of child health or sleep in the first 3 years of life (Sperber et al., 2023). Resulting cash gift impact estimates for these outcomes were all small in absolute magnitude (below 0.15 *SD*), with some effects in the hypothesized direction and others in the opposite direction.

The only direct assessment of child development in the BFY study to date was infant brain activity, which was measured after the first year of monthly unconditional cash gift receipt, in the subsample of children who were able to be assessed in-person prior to the onset of the COVID-19 pandemic. The BFY high-cash gift was associated with suggestive evidence of changes in infant brain activity, in patterns that have been associated with subsequent learning and development (Troller-Renfree et al., 2022).

In interpreting failure to reject the null hypotheses in the present report, we consider several possibilities. First, we consider potential methodological limitations that may have precluded observation of detectable treatment effects, such as lack of intervention fidelity, lack of adequate statistical power, or not having direct performance-based measures of child development. In terms of intervention fidelity, uptake of the cash gifts was nearly universal, with almost all mothers spending the funds each month (Gennetian et al., 2023). We therefore consider this explanation unlikely. In terms of statistical power, the BFY study was originally designed to detect an impact of 0.21 *SD*, an effect size that was determined based on past correlational and quasi-experimental work estimating the effects of income on children's achievement and early-adult outcomes (Dahl & Lochner, 2005; Duncan et al., 1998, 2011).[6] In the present study, the standard errors estimated in association with treatment impacts indicated that we had statistical power to detect effects between 0.17 and 0.22 *SD*, depending on the measure (mode = 0.20 *SD*; Bloom, 1995). Thus, while estimates lead us to reject the hypothesis that cash gifts of $333/month had impacts of approximately 0.20 *SD* or greater on maternal assessments of child development, the study was not statistically powered to determine whether the cash gifts may have generated smaller impacts on child developmental outcomes. That observed effect sizes across outcomes were consistently much smaller than 0.20 *SD* suggests that if there were statistically undetectable impacts on child outcomes, they were likely relatively small in magnitude.

In terms of the extent to which the measures objectively captured children's early development, the use of maternal reports of child development is a limitation. While we are confident that the well-validated measures captured treatment impacts on maternal perceptions of child development, such maternal reports can be biased by

[6] Note that we were limited to using past work that has estimated the effects of income on children's achievement and early-adult outcomes because there is little to no research on the effects of income on *early childhood* development in samples with low income.

mothers' perceptions, beliefs, and knowledge of early development (Madsen et al., 2020; Najman et al., 2001). It is also possible that mothers' responses, and the range of experiences and behaviors of families in BFY, could have been biased by social desirability effects given that mothers were aware that the study was about child development and knew that participants were randomized to different cash-gift amounts. With this said, the maternal assessments of child development reported by BFY mothers were similar to average levels documented in other studies. Moreover, direct assessments of child-directed speech around children's first birthday were correlated with subsequent maternal reports of language development (Egan-Dailey et al., in press), supporting the construct validity of the BFY language measures. Nonetheless, direct assessments of child outcomes at ages 4 and 6 will provide additional insight on impacts of the BFY cash gift on children's development.

Moving past methodological concerns that could have influenced the findings, we next consider several features of the BFY cash gift intervention related to the structure and format of the cash gift, as well as the broader context of the study. These include the size and duration of direct income support, the BFY cash delivery format, and the broader economic context, which are all important considerations when drawing broader conclusions about the effects of income on early development.

One possibility is that the magnitude of the monthly cash gift was not large enough to bring about sufficient changes across multiple dimensions of family life and parenting. The cash gift may not have done enough to meet families' needs. Additionally, the fact that the benefit was communicated as temporary may have also changed the way in which families planned their finances and spent the money.

It is also worth noting that alternate cash delivery modalities could shape family processes and child outcomes in very different ways. Whereas the BFY intervention involved a monthly cash gift, it is possible that a lump-sum cash transfer could support family savings or expenditures on different types of goods in ways that would produce different impacts on children's development than monthly cash gifts (Dahl & Lochner, 2005; Parolin et al., 2023).

The high-cash gift of $333/month was not adjusted for changing circumstances due to family size or cost of living. In contrast, other public benefits, such as the 2021 expanded Child Tax Credit, for example, adjusted income amounts based on the number and ages of children, resulting in a much larger infusion of direct income support to families with multiple children (see Parolin et al., 2023). The average BFY mother had two children; thus, it may be that a larger amount of money was needed. However, arguing against this point, we do not find larger impacts among children who do not have siblings.

Another possibility is that impacts on family life and parenting may take longer to emerge, and that 3 years of a predictable monthly BFY-sized cash transfer is not enough time to produce meaningful effects on child development. Arguing against this possibility is the suggestive evidence of positive impacts on brain development that were observed when the children were 12 months old, among the subsample of families who were reached prior to the onset of the pandemic (Troller-Renfree et al., 2022). Family and child assessments to be collected in the future will be informative regarding the impacts of intervention duration, as the present registered report focused only on findings from the first 3 years of a planned 6-year monthly cash gift intervention.

Additionally, broader external and economic conditions interact with the impact and interpretation of any intervention, and the BFY cash gifts co-occurred with a variety of broader economic and public health shifts. The onset of the COVID-19 pandemic had widespread effects on health, employment, child care, and schooling, in ways that could have uniquely interacted with the impacts of the cash gifts for families. That said, Premo et al. (2023) showed that subjective well-being and mental health did not appreciably decline during the height of the pandemic for the mothers in the BFY low-cash or high-cash gift groups. Likewise, Gennetian et al., 2024 showed that there were no major changes in the impact of parental investments across each of the Age 1 to Age 3 assessments, including periods in which the severity of the pandemic dramatically declined. However, many BFY families were beneficiaries of pandemic-related expansions of income support, including the 2021 expansion of the Child Tax Credit: At the Age 2 assessment, the majority (59%) of BFY mothers reported receiving direct deposits or check payments from the government. These shifts in context do not threaten the internal validity of the randomized study design and resulting impact estimates; however, the use and impact of the BFY monthly cash under these circumstances may differ from what would have been observed under circumstances of higher employment, lower public health risk, and fewer government benefits.

Finally, these findings raise the possibility that, in a contemporary context, additional direct income support for families with low income may not affect early development. This study was designed to test whether expectations informed by evidence from quasi-experimental literature on welfare and tax credit expansions in the 1990s (e.g., Duncan et al., 2011) would be replicated among infants and toddlers. Yet, these earlier studies were both conducted at a time when there were fewer supports, particularly in the realm of health care coverage and tax credits for low-income families (Whitaker et al., 2023), and in some cases, could not disentangle income increases from employment increases. It is also noteworthy that experimental studies of cash transfers in some developing countries have not found consistent impacts on similar measures of young children's outcomes (e.g., Fernald & Hidrobo, 2011; Macours et al., 2008). This raises the possibility that poverty reduction and income increases of the magnitude in the present study may not play a strong causal role in children's early developmental processes in current contexts. Nevertheless, small, undetectable impacts of the high-cash gift on various family processes and domains of early child development could still produce detectable effects on later outcomes (see Hart et al., in press, for discussion). Data from future assessment waves—which include direct assessments of child development and impacts of a full 6 years of monthly unconditional cash gifts—will shed light on this possibility.

Conclusion

Findings in this registered report show that monthly unconditional cash transfers—which increased net income by approximately 20% for families with low income—did not have detectable impacts on maternal reports of child language or socioemotional development in the first 3 years of life. These findings raise many potential avenues for future research. First, future work can investigate whether larger transfers, longer transfers, or transfers under other economic and public health conditions produce different impacts.

Future research can also investigate whether transfers of the magnitude in this study generate small impacts that may be statistically detectable with larger samples or whether coupling cash with additional direct services may be most effective in supporting early child development. Open questions remain about the effects of combining direct income support with other caregiver supports and about how impacts of direct income support may differ across contexts with varying social safety net supports, labor markets, and related economic contexts. Research in these areas will be crucial for advancing understandings of early development and estimating the extent to which antipoverty policy impacts early development.

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