Demography (2025) 62(2):405–417
DOI 10.1215/00703370-11872728 Published online: 28 March 2025

A Research Note on Unconditional Cash Transfers and Fertility in the United States: New Causal Evidence

Molly A. Costanzo, Katherine A. Magnuson, Greg J. Duncan, Nathan Fox, Lisa A. Gennetian, Sarah Halpern-Meekin, Kimberly G. Noble, and Hirokazu Yoshikawa

ABSTRACT As cash transfer policies have gained traction in recent years, interest in how financial resources could impact fertility has also grown. Increasing an individual's purchasing power with additional economic resources, such as those provided in unconditional cash transfers, might better enable parents to meet their fertility and reproductive goals, whether those goals are to become pregnant and give birth or to avoid or terminate pregnancies. In this research note, we provide new experimental evidence of the causal impact of a monthly unconditional cash transfer on fertility-related outcomes for U.S. families with at least one young child and low incomes. We find trends of increased pregnancy after three years but no corresponding impacts on births, miscarriages, or terminations. Our findings might indicate that modest cash transfers to mothers with low incomes in the United States are unlikely to have substantial impacts on fertility.

KEYWORDS Cash transfers • Fertility • Poverty

Introduction

As cash transfer policies have gained traction in recent years, policymakers' and demographers' long-standing interest in the association between fertility and economic resources has also grown. The role of family income in shaping fertility outcomes has been the subject of significant demographic research (e.g., Borg 1989; Freedman and Thornton 1982; Jones et al. 2008). Yet, the extent to which cash transfers to families with low incomes would change fertility outcomes in the United States is unclear. Additional economic resources might better enable parents to achieve their fertility and reproductive goals (Ross and Solinger 2017), perhaps leading some women to have more children and others to avoid or terminate pregnancies.

Theoretically, income-related fertility changes in high-income countries can be understood as a trade-off between women's earnings and caregiving responsibilities (Doepke et al. 2023). Policies that mitigate the trade-off between earnings and caregiving, such as unconditional cash transfers, might also affect fertility. Indeed, economic theory suggests that an increase in nonwage income should

ELECTRONIC SUPPLEMENTARY MATERIAL The online version of this article (https://doi.org/10.1215/00703370-11872728) contains supplementary material.

positively affect fertility (Becker 1960) because such an increase in household economic resources will not reduce the time parents spend caregiving or completing other household production tasks. Yet, theoretically, whether the impact of income would be the same for first births versus later births is unclear. Some fixed costs of caregiving remain the same regardless of the number of children and can lead to economies of scale, which might increase fertility. However, with higher order births, some mothers might have already reached their desired family size (Cowan and Douds 2022).

Evidence about the impact of higher nonwage income for families with low incomes is very limited. In the United States, studies of the causal impact of nonwage income on the fertility of women with low incomes have yielded mixed findings. The multisite negative income tax (NIT) experiments conducted in the 1970s had differing findings across sites, with results from one site suggesting a negative impact on fertility, results from another indicating no impacts, and still another indicating mixed results (Forget 2011; Keeley 1980; Kehrer and Wolin 1979). Evidence from the Alaska Permanent Fund found that increases in annual cash payments were associated with increases in fertility rates in the first two years following the higher payment, with larger impacts for lower income populations (Cowan and Douds 2022). A recent study of the impact of casino-based cash transfers in North Carolina also found an increase in conceptions and births for transfer recipients (Singh et al. 2023). Additionally, both Cowan and Douds (2022) and Singh and colleagues (2023) found evidence that effects on fertility were largest for first births, suggesting that the impact of income on higher order births might be negligible. However, the NIT and the Alaska Permanent Fund vary in the amount, timing, regularity, and predictability of nonwage income, leaving little evidence about the role of a regular, unconditional cash transfer.

Related evidence also comes from two policy reforms that changed families' access to nonwage cash income. Evidence from a welfare reform that affected low-income families' access to nonwage income had little to no effect on fertility (Blank 2002). The earned income tax credit (EITC) expansions in the 1990s, which expanded nonwage income to low-income families, also yielded inconsistent results, with some evidence that EITC receipt increased first births for some subpopulations (Baughman and Dickert-Conlin 2003). Considering higher order births, Meckel (2015) found that the EITC decreased the time between births but not overall fertility. Again, though, these results do not clearly generalize to what might be expected from unconditional cash transfers because they are conditioned on employment and, in the case of welfare reform, are part of a bundle of work-related policy changes.

In this research note, we provide new evidence of the impact of a monthly unconditional cash transfer on higher order fertility outcomes for women with low incomes in the United States. Compared with much of the research in this area, it provides evidence of whether an increase in predictable monthly nonwage income affects the fertility of women with low incomes who have at least one previous birth. Given the increased interest in cash transfers and the limited prior evidence of the impact of nonwage income, this research note offers important evidence for demographers and policymakers.

Methods

Sample

We used data from the Baby's First Years (BFY) study, a randomized-controlled study designed to identify the impact of an unconditional monthly cash transfer to families with low incomes in four sites in the United States.[1] From June 2018 to May 2019, the study enrolled 1,000 women[2] from 12 hospital postpartum wards where they had given birth to a focal child in the previous one or two days. Women who spoke English or Spanish, had family incomes below the poverty level, and had babies admitted to the well-child nursery were eligible to enroll. Of those enrolled, 40% ($n = 400$) were randomized into the "high-cash group" (or treatment group) to receive an unconditional cash transfer, or gift, of $333 each month for the first several years of the child's life ($3,996 annually, amounting to an average 18% increase in income). The remaining 60% received a nominal $20 per month for the same length of time.[3] The present study includes data from surveys administered to mothers around the time of the focal child's birth as well as first, second, and third birthdays. Detailed measures of pregnancies, births, miscarriages, and terminations were collected in the second survey wave (two-year survey). Randomization was successful, with family and related characteristics of mothers and children in the high-cash gift group similar to those in the low-cash gift group. See Noble et al. (2021) for more details of the study methods and data collection. Sample retention was quite high, at more than 94% for each survey wave (see Figure A1, online appendix).

This analytic sample consists of 957 mothers who completed at least one of the two-year and three-year surveys, a sample size that gives us 80% power to detect differences of 7.5–8.5 percentage points in pregnancies and births between the two groups at $p < .05$ with a two-tailed test. At the time of enrollment in the study (coinciding with the birth of the focal child), mothers' household income was, on average, just over $23,000, which is below the 2018 poverty threshold for a family of four (Table 1). On average, households had two adults, and participants had given birth to 2.5 children, including the focal child. Most mothers were women of color; slightly more than 40% identified as Hispanic (of any racial background), another 40% identified as non-Hispanic Black, and less than 10% identified as non-Hispanic White. About 4% of the sample identified as Asian, Pacific Islander, Native American, or Alaska Native, and another 4% identified as multiracial. At enrollment, roughly 45% of mothers had never married and were not cohabiting, 24% were cohabiting with a partner, and 22% were married. The proportion living with a spouse or partner remained similar in later waves (45% at the two-year and three-year interviews). The average age of participants at enrollment was roughly 27. Note that prior research

[1] Participants were recruited from New York City, greater New Orleans, Minneapolis–Saint Paul, and the Omaha metropolitan area.

[2] The sample consists of birthgiving individuals who were recruited at the time of their child's birth. Throughout this note, we use the terms *woman* and *mother* to reflect how most of the sample identifies, but we acknowledge that some sample individuals might not identify with these terms.

[3] Mothers were initially informed that the monthly cash transfer would last for 40 months. The transfers were subsequently extended twice, first to 52 months and then to 76 months.

Table 1 Characteristics of mothers who completed the two-year and three-year surveys ($N = 957$)

	Sample Mean	Low-Cash Group Mean	High-Cash Group Mean	p Value of Difference
Participant Age at Focal Birth (years)	27.1	26.8	27.4	.14
		(5.8)	(5.8)	
Education (years)	11.9	12.0	11.9	.82
		(2.8)	(3.0)	
Race and Ethnicity (%)				
Hispanic, any race	41.2	40.4	42.3	.38
Black, non-Hispanic	41.6	39.9	44.1	.15
White, non-Hispanic	9.8	10.7	8.5	.20
Asian/Pacific Islander/Native American/ Alaska Native, non-Hispanic	3.6	4.7	2.3	.03
Multiple races, non-Hispanic	3.7	4.2	2.8	.23
Marital Status at Baseline (%)				
Never married	45.2	42.0	50.0	.01
Single, living with partner	23.9	26.0	20.9	.07
Married	21.5	21.4	21.6	.92
Divorced/separated	4.0	4.7	2.8	.12
Other/unknown	5.3	5.8	4.6	.40
Partner Status (%)				
Has a romantic partner or spouse at the two-year interview	59.1	59.3	58.9	.91
Coresides with partner/spouse at the two-year interview	45.4	46.5	43.8	.41
Coresides with partner/spouse at the three-year interview	44.6	46.3	42.2	.23
Household Measures at Baseline				
Number of children born to mother	2.5	2.4	2.5	.16
		(1.4)	(1.4)	
Age of youngest child (excluding focal child)[a]	4.0	3.9	4.0	.70
		(3.2)	(3.1)	
Number of adults in household	2.1	2.1	2.0	.28
		(1.0)	(1.0)	
Household income ($)	23,151	24,179	21,644	.22
		(45,002)	(16,835)	
n		569	388	

Notes: Standard deviations are shown in parentheses. Data are from the authors' calculations.

[a] The sample for this variable is limited to mothers with a birth before baseline ($n = 675$).

found that fertility rates are highest among women aged 25–35 who have had a previous birth in the last three years (Osterman et al. 2023)—characteristics that describe most of the BFY sample.

Measures

We drew our primary measures from the two-year survey, which contained the most detailed information about pregnancy and pregnancy outcomes of all survey waves.

We asked mothers whether they were currently pregnant at the time of the survey and how many pregnancies they had experienced since the birth of the focal child (approximately two years prior). Mothers were then asked about the outcomes of each pregnancy, including whether they had given birth, experienced a miscarriage, or terminated a pregnancy.

We supplemented our analysis with data from the survey conducted around the focal child's third birthday (three-year survey). In that survey, mothers reported whether they were currently pregnant, but no additional information about past pregnancies or pregnancy outcomes was gathered. For this reason, we used data from detailed questions about the membership of the mother's household. These data identified new biological children in the household and their birth dates. The two-year survey also includes these measures. Because some mothers did not provide complete information about their household composition, we are missing responses from approximately 1% of participants for this measure.

Thirty-five mothers completed the three-year survey but not the two-year survey, and 36 completed the two-year survey but not the three-year survey. We used multiple imputation with chained equations to impute missing data for all measures, which enabled us to include the full sample of mothers ($N = 957$) who completed either the two-year or the three-year survey. Combining the measures from both surveys allowed us to identify with certainty mothers who had ever been pregnant by the two-year interview or at the time of the three-year interview. Using the household membership roster at both the two-year and three-year interviews also allowed us to identify women who gave birth to and lived with a child by the three-year interview, providing a more comprehensive measure of pregnancy. However, this approach means that we do not know whether mothers had pregnancies that did not result in a live birth after the two-year interview or whether they gave birth to a child whom they did not have custody of after the birth.

We also examined birth spacing between the focal child and the next birth following the focal child using the detailed household roster that mothers provided during the annual survey. This analysis is limited to those mothers who had a subsequent birth by the time of the three-year interview and provided birth months for those births ($n = 204$). The roster data included household members' relationship to the participant and their birth dates. Because some mothers did not provide birth dates, we are missing these measures for approximately 1% of participants.

Analytic Approach

The BFY experimental research design allowed us to use regression-based intent-to-treat analyses to estimate the causal impact of the monthly unconditional cash gift on fertility.[4] Because previous work indicated potential differences in the impact of nonwage income by parity and the mother's relationship status (Baughman and Dickert-Conlin 2003; Cowan and Douds 2022), we also conducted subgroup analyses.

[4] We do not have measures of pretrends for our high- or low-cash groups. We leverage the successful implementation of our random assignment design, which resulted in equivalent groups at enrollment, allowing us to compare outcome measures in a regression framework to estimate intent-to-treat differences.

We compared mothers for whom the focal child was a first birth with mothers who had previous births, and we compared mothers who were living with a partner at the time of the baseline survey with those who were not. Given the size of the subgroups of interest, we were powered at 80% to detect differences from 10 percentage points (for the largest subgroup) to 15 percentage points (for the smallest subgroup) between the two groups at the $p < .05$ level. Finally, given the substantial right-censoring of subsequent births, we also estimated a Cox proportional hazard model to estimate differences in the likelihood and timing of a subsequent birth by treatment status.

Regressions were adjusted by a series of preregistered demographic characteristics of the mother at the time of study recruitment (or the focal child's birth) and site fixed effects.[5] These characteristics were balanced across the treatment and comparison groups because the joint test of significance was not statistically significant. Nevertheless, there were several small differences in baseline characteristics across cash gift groups. Specifically, mothers in the high-cash gift group were slightly less likely to be cohabiting with a partner at the time of the baseline survey, although these differences did not exist in later survey waves. In addition, there is no impact of the cash gift on the likelihood of cohabiting with any romantic partner in later survey waves, and the increased likelihood of marriage at the one-year survey does not persist in later waves (see Table A1, online appendix) (Shah et al. 2024). We included covariates to correct for any differences in these baseline characteristics and improve the precision of the impact estimates. Estimates are reported as both point estimates and standard deviation effect sizes, which are calculated as the ratio of the point estimate to the standard deviation of the low-cash gift group.

We conducted sensitivity analyses to ensure that our results were robust to alternative covariate specifications. First, we excluded all covariates, and our results were similar. Next, we estimated our analyses using inverse probability weights (using TWANG; Ridgeway et al. 2022) to make the two groups more similar on observed baseline characteristics. Again, results were robust to this modeling approach.

Results

Descriptively, we found some differences in the likelihood and timing of subsequent births for mothers in the high- and low-cash gift groups. Kaplan–Meier estimates indicated that high-cash gift mothers had a higher likelihood of a subsequent birth 12–24 months following the birth of the focal child than mothers in the low-cash group (Figure 1). However, there were no apparent differences by Month 36.

Turning to regression-adjusted estimates of the impacts of the high-cash gift on mothers' fertility, we did not find evidence that 24 months of an unconditional cash transfer impacted the likelihood that mothers had experienced a pregnancy (Table 2). At the time of the two-year interview, approximately two years after the birth of the focal child, 24.6% of mothers in the low-cash gift group had experienced a pregnancy, including 8.4% who were pregnant at the time of the interview. Roughly 29.3% of

[5] The primary measures in this analysis are not preregistered. Full preregistration information for the study is available at clinicaltrials.gov (ID: NCT03593356).

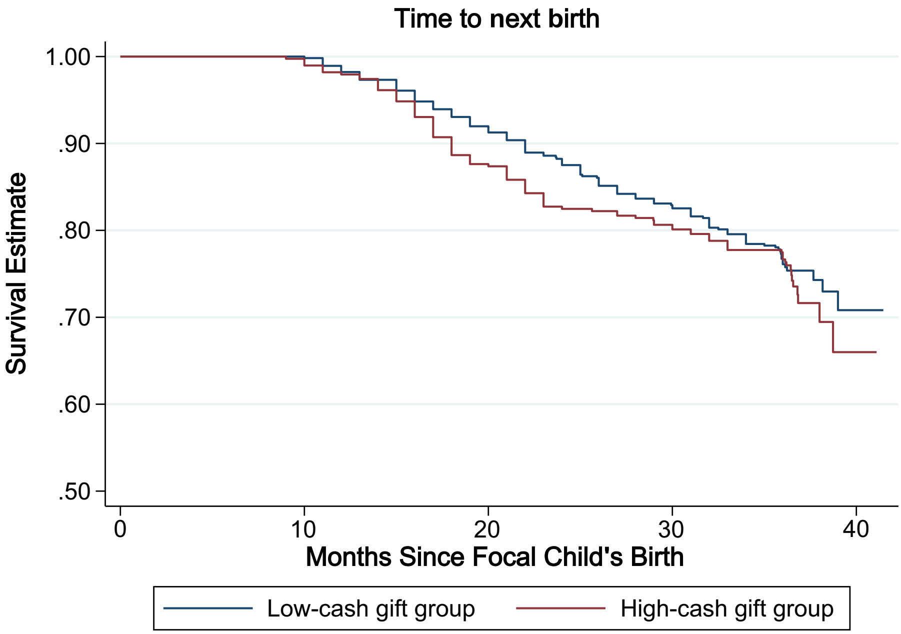

Fig. 1 Kaplan–Meier survival estimates for time to first birth after focal child by treatment status. Data are from the authors' calculations.

mothers who received the high-cash gift had been pregnant, including 7.1% who were pregnant at the time of the interview; the difference was marginally statistically significant when we controlled for baseline differences.

When the focal child was 3 years old, marginal statistically significant differences in the likelihood of pregnancy continued, suggesting that the high-cash gift group might have been slightly more likely to be pregnant (Table 2). At the three-year interview, 33.8% of women in the high-cash gift group had experienced a pregnancy, compared with 28.0% of women in the low-cash gift group. This difference remained marginally statistically significant when we controlled for baseline covariates, suggesting a trend of pregnancies among more mothers in the high-cash gift group compared with mothers in the low-cash gift group. When we included in this measure women who had likely given birth between the two-year and three-year surveys, we found a similar pattern of results, although the difference was not statistically significant. We obtained similar results when using unimputed data (i.e., excluding mothers who did not complete the two-year or three-year survey).

Although there were some small and marginally significant differences in pregnancy rates across the high-cash and low-cash gift groups, we did not detect any differences in pregnancy outcomes (Table 2). That is, although more mothers in the high-cash gift group had had a subsequent birth by the two-year survey (16.4% vs. 13.0%) and the three-year survey (25.8% vs. 23.7%), this difference was not statistically significant at either time point. Mothers who received the high-cash gift had a subsequent birth, on average, approximately 1.5 months earlier than mothers in the

Table 2 Impact of cash transfer on fertility measures at two-year and three-year surveys

	Low-Cash Group Mean	High-Cash Group Mean	Unadjusted Mean Difference	Point Estimate	Effect Size	*p* Value	*N*
Pregnancies							
Two-year survey							
Pregnancy since focal child was born (%)	24.6	29.3	4.7	0.050 (0.030)	0.12	.10†	957
Multiple pregnancies since focal child was born (%)	3.9	3.0	–0.9	–0.001 (0.013)	–0.01	.92	957
Three-year survey							
Currently pregnant at interview (%)	7.0	6.7	–0.3	0.006 (0.017)	0.02	.72	957
Pregnant before focal child is age 2 or currently pregnant (%)	28.0	33.8	5.8	0.058 (0.031)	0.13	.07†	957
Has given birth, has been pregnant, or is currently pregnant (%)	33.5	38.5	5.0	0.048 (0.032)	0.10	.14	957
Live Births, Terminations, Miscarriages							
Two-year survey							
Experienced pregnancy loss (%)	4.5	6.7	2.2	0.021 (0.017)	0.09	.21	957
Terminated a pregnancy (%)	2.0	1.3	–0.7	–0.004 (0.010)	–0.04	.70	957
Has given birth since focal child was born (%)	13.0	16.4	3.4	0.037 (0.024)	0.11	.12	957
Number of births since focal child was born	0.13	0.16	0.03	0.032 (0.024)	0.09	.18	957
Three-year survey							
Has given birth since focal child was born (%)	23.7	25.8	2.1	0.017 (0.029)	0.04	.55	957
Birth spacing in months (child after focal child)	22.3	20.5	–1.8	–1.470 (1.100)	–0.20	.18	204
Time to next birth (Cox proportional hazard)				0.118 (0.140)		.40	957

Notes: Standard errors are shown in parentheses. Unless noted, the results are taken from an ordinary least-squares model that includes baseline covariates and study site fixed effects. Covariates drawn from the BFY baseline survey include mother's age, education completed, household income, household net worth, mother's health, mother's mental health, mother's race and ethnicity, mother's relationship status, number of adults in the household, number of other children born to the mother, smoking during pregnancy, alcohol use during pregnancy, father and mother cohabiting, and child's sex, birth weight, and gestational age at birth. Child's age at interview (in months) is also included. The effect size is derived from the standard deviation of the low-cash group. Time to next birth is drawn from a Cox proportional hazard model.

Source: Data are from authors' calculations.

† $p < .10$

Table 3 Impact of BFY cash gift on fertility outcomes by mother's romantic relationship status and parity

	Romantic Relationship Status at Baseline							
	Married or Cohabiting				Not Married or Cohabiting			
	Point Estimate	Effect Size	*p* Value	*n*	Point Estimate	Effect Size	*p* Value	*n*
Two-Year Survey								
Pregnancy	0.077† (0.044)	0.18	.081	435	0.020 (0.044)	0.05	.644	522
Given birth	0.072* (0.032)	0.21	.025	435	0.004 (0.035)	0.01	.904	522
Pregnancy loss	0.030 (0.027)	0.14	.265	435	0.009 (0.022)	0.04	.691	522
Termination	0.016 (0.009)	0.11	.105	435	–0.023 (0.014)	–0.16	.115	522
Three-Year Survey								
Pregnancy	0.086† (0.045)	0.19	.058	435	0.038 (0.043)	0.08	.381	522
Given birth	0.073† (0.041)	0.17	.076	435	–0.020 (0.041)	–0.05	.627	522
Birth spacing	–2.677 (2.807)	–0.37	.346	79	–1.264 (1.509)	–0.17	.405	124
	Parity							
	Focal Child Is First				Focal Child Is Not First			
	Point Estimate	Effect Size	*p* Value	*n*	Point Estimate	Effect Size	*p* Value	*n*
Two-Year Survey								
Pregnancy	0.026 (0.060)	0.06	.662	282	0.054 (0.036)	0.12	.134	675
Given birth	0.022 (0.046)	0.06	.638	282	0.045 (0.029)	0.13	.126	675
Pregnancy loss	0.019 (0.034)	0.09	.569	282	0.016 (0.018)	0.08	.367	675
Termination	0.017 (0.023)	0.12	.441	282	–0.011 (0.009)	–0.08	.220	675
Three-Year Survey								
Pregnancy	0.016 (0.062)	0.04	.799	282	0.074* (0.037)	0.16	.046	675
Given birth	–0.026 (0.056)	–0.06	.640	282	0.033 (0.035)	0.08	.346	675
Birth spacing	–2.639 (2.863)	–0.36	.366	64	–2.207 (1.499)	–0.30	.144	139

Notes: Standard errors are shown in parentheses. Unless noted, the results are taken from an ordinary least-squares model that includes baseline covariates and study site fixed effects. Covariates drawn from the BFY baseline survey include mother's age, education completed, household income, household net worth, mother's health, mother's mental health, mother's race and ethnicity, mother's relationship status, number of adults in the household, number of other children born to the mother, smoking during pregnancy, alcohol use during pregnancy, father and mother cohabiting, and child's sex, birth weight, and gestational age at birth. Child's age at interview (in months) is also included. The effect size is derived from the standard deviation of the full sample low-cash group. Romantic relationship status was determined by the presence of a spouse or partner who coresided with the participant at the time of the baseline survey. Parity was determined by whether the participant gave birth before the focal child's birth.

Source: Data are from authors' calculations.

†$p<.10$; *$p<.05$

low-cash group. The Cox proportional hazard estimates also show no difference in the likelihood of having a second birth.

We found evidence that the impact of a cash transfer on fertility outcomes varied by household structure and, particularly, the mother's relationship status (Table 3). Notably, the cash gift increased pregnancies for women who were cohabiting with a partner or who were married at baseline ($n = 435$) but had no statistically significant impact on women not living with a spouse or partner ($n = 522$). We found limited evidence that the focal child's birth order (i.e., whether mothers had at least one previous biological child at the time of the focal child's birth) resulted in significant differences in the high-cash gift impact. Among mothers who had a birth before the focal child ($n = 675$), the cash gift had a statistically significant impact on having been pregnant at the three-year interview but had no impact on mothers for whom the focal child was their first ($n = 282$), although the estimates did not differ statistically.

Discussion

Our findings offer new estimates of the causal impact of a predictable monthly unconditional cash transfer on fertility outcomes for families with low incomes and young children. Women of childbearing age with limited income are a population that has been found to be particularly responsive to a cash transfer, although previous research indicated a stronger impact on first births (e.g., Baughman and Dickert-Conlin 2003; Cowan and Douds 2022). The women in this sample all had at least one previous birth, and we found no statistically significant impacts of the cash gift on fertility estimates for our full sample. However, we found important heterogeneity in estimates, particularly by whether mothers were married or cohabiting and, to a lesser extent, by parity. We note, too, that we were limited in our ability to detect statistically significant differences between groups with small sample sizes.

The BFY cash gift increased the likelihood of a subsequent pregnancy within three years of a focal child's birth for mothers who were married or cohabiting at the time of enrollment in the survey but not for mothers without a cohabiting partner. Parity might also matter. We found the cash gift did not impact the likelihood of pregnancy or subsequent birth three years after the birth of a focal child for mothers with only one prior birth (i.e., the focal child was their first) but did increase the likelihood of pregnancy for mothers with multiple previous births, although the estimates did not differ statistically. These findings underscore and build on previous work indicating the importance of contextual factors, and parity in particular, in understanding the role of cash transfers in fertility outcomes.

The importance of disentangling measures of pregnancy from measures of pregnancy outcomes is also clear. Although we found increased pregnancies, we did not find a corresponding impact on births. Given differences in pregnancy likelihood at the time of the three-year survey, this divergence in findings might be an artifact of timing, and later measures of births might result in similar findings for births. That we found increased pregnancies but no corresponding impact on births might also reflect underreporting of terminations or miscarriages at the two-year survey (although underreporting of these outcomes would be somewhat surprising, given that these pregnancies were reported). Our data suggest that some of this disconnect could be the result of not including measures of terminations or miscarriages at the three-year survey; a small number of mothers reported a pregnancy at the two-year interview but did not report living with a new child at the three-year interview. By the time of the two-year interview, more than 5% of mothers had either terminated a pregnancy or experienced a pregnancy loss. Although we found no impact of the cash gift on these outcomes, the proportion of mothers who experienced either of these outcomes combined with the number of mothers who reported a pregnancy but did not have a child living with them one year later indicates the importance of measuring different aspects of fertility and reproductive health. Note that in previous work we found the cash gift did not impact overall contraceptive use at the time of the two-year survey, although it was associated with trends toward higher use of multiple contraceptive methods and short-term hormonal contraception (Costanzo et al. 2024). Given the rise in policies limiting access to abortion and other reproductive health care, attending to multiple facets of reproductive health and fertility outcomes might become increasingly salient in the coming years, particularly as economic resources continue to shape access to prenatal and abortion care.

Our findings should be considered in light of some limitations. First, although our study used a random assignment design, which should result in groups with similar pretrends in fertility before enrollment, we could not measure pretrends. Nonetheless, the combination of balance between the groups with respect to their prior number of children and our use of random assignment suggests that our estimates are unlikely to be biased by differential fertility trends. The study was also limited by its sample size, which is not large enough to detect small impacts. We are also limited in understanding the full range of fertility outcomes between the two-year and three-year surveys, given that we had measures of only current pregnancy and reported new children in the household at the three-year interview. Therefore, we could not observe pregnancy losses or terminations between waves, which might contribute to the disconnect between estimates of pregnancy likelihood and birth outcomes. Future research should continue to account for the full range of fertility outcomes, including disentangling pregnancy measures from births. Further, the mothers in this study are still of reproductive age, and our findings offer evidence only through the first three years following the focal child's birth; continuing to follow the fertility of the mothers in our study will be instructive. Finally, these data are taken from the years when families were impacted by the COVID-19 pandemic and thus might not be generalizable to other periods. Continuing to follow fertility outcomes will be instructive, and additional studies of unconditional cash transfers will be important to replicate these findings.

As policymakers consider the impact of cash transfers for families with young children and low incomes in the United States, evidence of how these policies may impact fertility outcomes takes on increasing importance. Here, we offer evidence of the limited impact of cash transfers on fertility outcomes for women with low incomes and young children in four sites across the United States. Our findings, which should be considered in the context of the noted limitations, suggest that an unconditional cash transfer did not have broad impacts on fertility among mothers with children. However, they provide some evidence of increased pregnancies for some women, particularly those with multiple children and cohabiting partners. ■

Acknowledgments We thank the University of Michigan Survey Research Center, our partners in recruitment, data collection, and participant location and retention. We also thank participants at the 2023 APPAM Fall Research Conference for helpful feedback on early versions of this manuscript, particularly Tara Watson and Lindsey Bullinger. Research reported in this publication was supported by the Eunice

Kennedy Shriver National Institute of Child Health and Human Development of the National Institutes of Health under Award Numbers R01HD087384 and 2R01HD087384. The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institutes of Health. This research was additionally supported by the U.S. Department of Health and Human Services, Administration for Children and Families, Office of Planning, Research and Evaluation; National Institute of Mental Health; Office of Behavioral and Social Sciences Research–Office of the Director, National Institutes of Health; Andrew and Julie Klingenstein Family Fund; Annie E. Casey Foundation; Arnold Ventures; Arrow Impact; BCBS of Louisiana Foundation; Bezos Family Foundation; Bill & Melinda Gates Foundation; Bill Hammack and Janice Parmelee; Brady Education Fund; Chan Zuckerberg Initiative (Silicon Valley Community Foundation); Charles and Lynn Schusterman Family Philanthropies; Child Welfare Fund; Esther A. and Joseph Klingenstein Fund; Ford Foundation; Greater New Orleans Foundation; Heising-Simons Foundation; Holland Foundation; Jacobs Foundation; JPB Foundation; J-PAL North America; Lozier Foundation; New York City Mayor's Office for Economic Opportunity; Perigee Fund; Robert Wood Johnson Foundation; Robin Hood; Sherwood Foundation; Valhalla Foundation; Weitz Family Foundation; W.K. Kellogg Foundation; and three anonymous donors. Other than Costanzo and Magnuson, the authors are listed in alphabetical order.

References

Baughman, R., & Dickert-Conlin, S. (2003). Did expanding the EITC promote motherhood? *American Economic Review: Papers & Proceedings, 93,* 247–251.

Becker, G. S. (1960). An economic analysis of fertility. In National Bureau of Economic Research (Ed.), *Demographic and economic change in developed countries* (pp. 209–240). New York, NY: Columbia University Press.

Blank, R. M. (2002). Evaluating welfare reform in the United States. *Journal of Economic Literature, 40,* 1105–1166.

Borg, M. O. M. (1989). The income–fertility relationship: Effect of the net price of a child. *Demography, 26,* 301–310.

Costanzo, M. A., Magnuson, K. A., Gennetian, L. A., Halpern-Meekin, S., Noble, K. G., & Yoshikawa, H. (2024). Contraception use and satisfaction among mothers with low income: Evidence from the Baby's First Years study. *Contraception, 129,* 110297. https://doi.org/10.1016/j.contraception.2023.110297

Cowan, S. K., & Douds, K. W. (2022). The effects of income on birth rates: The case of a universal cash transfer. *Social Forces, 101,* 1003–1030.

Doepke, M., Hannusch, A., Kindermann, F., & Tertilt, M. (2023). The economics of fertility: A new era. In S. Lundberg & A. Voena (Eds.), *Handbook of the economics of the family* (Vol. 1, pp. 151–254). Amsterdam, the Netherlands: North-Holland.

Forget, E. L. (2011). The town with no poverty: The health effects of a Canadian guaranteed annual income field experiment. *Canadian Public Policy, 37,* 283–305.

Freedman, D. S., & Thornton, A. (1982). Income and fertility: The elusive relationship. *Demography, 19,* 65–78.

Jones, L. E., Schoonbroodt, A., & Tertilt, M. (2008). *Fertility theories: Can they explain the negative fertility–income relationship?* (NBER Working Paper 14266). Cambridge, MA: National Bureau of Economic Research.

Keeley, M. C. (1980). The effects of negative income tax programs on fertility. *Journal of Human Resources, 15,* 675–694.

Kehrer, B. H., & Wolin, C. M. (1979). Impact of income maintenance on low birth weight: Evidence from the Gary Experiment. *Journal of Human Resources, 14,* 434–462.

Meckel, K. (2015). *Does the EITC reduce birth spacing?* (Working paper). New York, NY: Columbia University, Department of Economics.

Noble, K. G., Magnuson, K., Gennetian, L. A., Duncan, G. J., Yoshikawa, H., Fox, N. A., & Halpern-Meekin, S. (2021). Baby's First Years: Design of a randomized controlled trial of poverty reduction in the United States. *Pediatrics, 148,* e2020049702. https://doi.org/10.1542/peds.2020-049702

Osterman, M. J. K., Hamilton, B. E., Martin, J. A., Driscoll, A. K., & Valenzuela, C. P. (2023). *Births: Final data for 2021* (National Vital Statistics Report, Vol. 72 No 1). Hyattsville, MD: National Center for Health Statistics.

Ridgeway, G., McCaffrey, D. F., Morral, A. R., Cefalu, M., Burgette, L. F., Pane, J. D., & Griffin, B. A. (2022). *Toolkit for weighting and analysis of nonequivalent groups: A tutorial for the R TWANG package* (Report). Santa Monica, CA: Rand Corporation.

Ross, L. J., & Solinger, R. (2017). *Reproductive justice: An introduction* (Vol. 1). Oakland: University of California Press.

Shah, H., Gennetian, L. A., Magnuson, K., Halpern-Meekin, S., & Noble, K. G. (2024). *Impacts of unconditional cash transfers on single parenthood* [Unpublished manuscript]. Department of Economics, Duke University, Durham, NC.

Singh, P., Gemmill, A., & Bruckner, T.-A. (2023). Casino-based cash transfers and fertility among the Eastern Band of Cherokee Indians in North Carolina: A time-series analysis. *Economics & Human Biology, 51,* 101315. https://doi.org/10.1016/j.ehb.2023.101315

Molly A. Costanzo (corresponding author)
macostanzo@wisc.edu

Costanzo • Institute for Research on Poverty, University of Wisconsin–Madison, Madison, WI, USA; https://orcid.org/0000-0002-5082-8725

Magnuson • Institute for Research on Poverty and Sandra Rosenbaum School of Social Work, University of Wisconsin–Madison, Madison, WI, USA; https://orcid.org/0000-0002-1140-1200

Duncan • School of Education, University of California, Irvine, Irvine, CA, USA; https://orcid.org/0000-0002-9869-6311

Fox • Department of Human Development and Quantitative Methodology, University of Maryland, College Park, MD, USA

Gennetian • Sanford School of Public Policy, Duke University, Durham, NC, USA; https://orcid.org/0000-0002-4639-7547

Halpern-Meekin • School of Human Ecology and LaFollette School of Public Affairs, University of Wisconsin–Madison, Madison, WI, USA; https://orcid.org/0000-0002-2142-5097

Noble • Department of Biobehavioral Sciences and Department of Human Development, Teachers College, Columbia University, New York, NY, USA; https://orcid.org/0000-0003-1496-5113

Yoshikawa • Department of Applied Psychology, New York University, New York, NY, USA; https://orcid.org/0000-0001-5607-8526