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Household Income and Child Development in the First Years of Life (Baby's First Years) Statistical Analysis Plans for Phase 1 and Phase 2

Our Analysis Plan for Phase 1 (dated December 13, 2022) covers hypotheses for data collected in our age 4 follow-up. (Analytic plans for hypotheses for data collected in our ages 1, 2 and 3 follow-ups are available through the "Record History" link on the study's clinicaltrials,gov page.
Our Phase 2 Analysis Plan (dated June 29, 2024) covers hypotheses for data proposed to be collected in our ages 6 and 8 follow-ups.
This document begins with the December 13, 2022 Phase 1 Plan, followed by the June 29, 2024 Phase 2 Plan. An appendix to this document includes summary charts of all hypotheses and their associated measures for data collected at ages 1, 2, 3, 4, 6, and 8. Child-related hypotheses and measures are listed in Table 9; maternal- and family-related hypotheses and measures are listed in Table 10.

We made the following two edits on January 24, 2025: Added a link to our supplemental preregistration plan for age-4 DNA analysis, public on the Open Science Forum OSF, and noted that we dropped one measure at age 6 due to floor effects.

Baby's First Years Phase 1: Summary, Pre-registered Hypotheses, Analysis Strategies

December 13, 2022

Project Summary

In the Baby's First Years (BFY) study, one thousand infants born to mothers with incomes falling below the federal poverty threshold in four metropolitan areas in the United States were assigned at random within each of the metropolitan areas to one of two cash gift conditions. The sites are: New York City, the greater New Orleans metropolitan area, the greater Omaha metropolitan area, and the Twin Cities. IRB and recruiting issues led to a distribution of the 1,000 mothers across sites of 121 in one site (the Twin Cities), 295 in two of the other sites (New Orleans and Omaha) and 289 in New York. (We have also randomly sampled 80 of the participating families in the Twin Cities and New Orleans to participate in an in-depth qualitative study, but do not elaborate on those plans in this document.)

Mothers were recruited in maternity wards of the 12 participating hospitals shortly after giving birth and, after consenting, were administered a 30-minute baseline interview. They then were asked to consent to the cash gifts. The "high-cash gift" treatment group mothers (40% of all mothers) are receiving unconditioned cash payments of $333 per month ($4,000 per year) via debit care for 52 months. Mothers in the "low-cash gift" comparator group (60% of all mothers) are receiving a nominal payment – $20 per month, delivered in the same way and also for 52 months. The 40/60 randomization assignment is stratified by site, but not by hospitals, within each of the four sites.

BFY was originally formulated to study the effects of monthly unconditional cash transfers on child development for the first three years of life, with the cash gifts set to be distributed for 40 months (3 years, 4 months). In response to the COVID-19 pandemic and the need to postpone in-person research activities, the cash transfers were extended for an additional year, through 52 months (4 years, 4 months), enabling us to postpone in-person direct child assessments to age 4. Interviews conducted at child ages 1, 2 and 3 are providing information about family functioning as well as several maternal reports of developmentally-appropriate measures of children's cognitive and behavioral development. The current analysis plan includes lab-based assessments at child age 4.

Conditional on participants' consent and our success in securing agreements with state and county agencies, we are also collecting state and local administrative data regarding parental employment, utilization of public benefits such as Medicaid and Supplemental Nutrition Assistance Programs (SNAP), and any involvement in child protective services. (We have worked with state and local officials to ensure to the extent feasible that our cash gifts are not considered countable income for the purposes of determining benefit levels from social assistance programs.)

The compensation difference between families in the high and low cash gift groups will boost family incomes by $3,760 per year, an amount shown in the economics and developmental psychology literatures to be associated with socially significant and policy relevant improvements in children's school achievement. After accounting for likely attrition, our total sample size of 800 at age 4 years, divided 40/60 between high and low payment groups, provides sufficient statistical power to detect meaningful differences in cognitive, emotional and brain functioning, and key dimensions of family context (see below).

At the age 4 lab visit we will administer validated, reliable and developmentally sensitive measures of language, executive functioning and socioemotional skills. We will also collect direct EEG- and ERP-based measures of young children's brain development at age 4. Measures and preregistered hypotheses about them as well as family-based measures are shown in the two tables at the end of this document. Child-focused preregistered hypotheses are presented in Appendix Table 7 and maternal and family focused preregistered hypotheses are presented in Appendix Table 8.

The family process measures that we will gather are based on two theories of change surrounding the income supplements: that increased investment and reduced stress will facilitate children's healthy development. We are obtaining measures of both of these pathways annually. *Investment pathway*: Additional resources enable parents to buy goods and services for their families and children that support cognitive development. These include higher quality housing, nutrition and non-parental child care; more cognitively stimulating home environments and learning opportunities outside of the home; and, by reducing or restructuring work hours, more parental time spent with children. *Stress pathway:* A second pathway is that additional economic resources may reduce parents' own stress and improve their mental health. This may allow parents to devote more positive attention to their children, thus providing a more predictable family life, less conflicted relationships, and warmer and more responsive interactions.

Analysis Plan

Pre-registered Hypotheses. We preregistered hypotheses with clinicaltrials.gov within a month after recruitment began (May, 2018) and in September, 2018, preregistered hypotheses with the Registry of Effectiveness Studies and the AEA RCT Registry. Appendix Tables 1 and 2 detail our original hypothesized impacts. Appendix Tables 3 and 4 incorporate minor changes (mostly made to data collection at age 2, with a few changes to age 3 data collection and no changes to Age 1) to the tables that were originally posted in our pre-registrations. Appendix Tables 5 and 6 incorporate minor changes to reflect the COVID-19 disruptions that impacted data collection at age 2, and altered data collection plans at age 3 and ages 48 months. Appendix Tables 7 and 8 reflect updated hypothesized impacts at ages 48 months.

Hypothesis Testing and Power Analysis. Our key aims are to evaluate the impacts of income supplementation on validated, reliable, and developmentally-sensitive measures of cognitive, language, self-regulation, and socio-emotional functioning at child ages 1 (a small subset of these measures), 2 and 3 (a larger subset), and age 4 (almost all) – this is Aim 1 in our original NICHD application; developmentally-sensitive electroencephalographic-based measures of brain functioning at child ages 1 and 4 (Aim 2); and family expenditures, food insecurity, housing and neighborhood quality, parent stress and parenting practices, and child care arrangements gathered at child ages 1, 2, 3, and 4 (Aim 3).

All of our pre-registered hypotheses focus on full-sample impacts, although we will also estimate in exploratory analyses moderation of impacts by gender, race/ethnicity (African American, Latino, White), family structure at birth and depth of poverty at birth (income to needs $\leq .5$ or not). Before conducting these main analyses, all measures will be examined for psychometric equivalence across race/ethnicity and whether Spanish or English is a primary language spoken at home and we will compare high and low cash gift groups within site on all baseline characteristics to confirm successful implementation of random assignment.

Our basic empirical approach will use the survey and neuroscience data to compare the pooled cross-city $333/month and $20/month groups on a wide range of family process and child outcome measures. Because of random assignment, the low cash gift group average outcomes enable us to identify the average outcomes corresponding to the counterfactual state that would have occurred for individuals in the high cash gift group if they had not been offered the additional $313/month income supplement. Therefore, differences in outcomes for the high compared with the low group (after random assignment) can be interpreted as estimates of causal treatment effects of the $313/month higher income (regardless of whether treatment-group participants actually expend all of the funds.) These are commonly known as intent-to-treat effects.

Estimation strategy. We illustrate our approach to estimation in a simple regression framework. The “Intent-To-Treat effect” (ITT) is captured by the estimate of the coefficient π_1 in a regression of some child or family process outcome (Y) on a dichotomous indicator for assignment (Z) to the high payment group as in (1).

$$(1)\ Y = Z\pi_1 + X\beta_1 + \varepsilon_1$$

We have experienced extremely low rates of “non-compliance” with the offer of cash gifts paid via the debit cards, with less than 10 of the 1,000 participants never having charged anything on their debit cards. We will adjust standard errors using robust variance estimation techniques (Cameron et al. 2008). We will estimate (1) without and then with baseline demographic child and family characteristics (X) to improve the precision of our estimates by accounting for residual variation. These baseline measures, all gathered prior to random assignment, have been checked for adequate variation and sufficient independence from other baseline measures. They include: dummy variables for three of the four sites; mother’s age, completed schooling, household income, net worth, general health, mental health, race and Hispanic ethnicity, marital status, number of adults in the mother’s household, number of other children born to the mother, whether the mother smoked or drank alcohol during pregnancy and whether the father is currently living with the mother; and child's sex, birth weight, gestational age at birth and birth order.

We will apply our regression estimation strategy to the assessment-based measures of cognitive, language, self-regulation, and socio-emotional functioning and EEG measures of brain activity as outlined in Appendix Tables 7. Further information on the EEG hypotheses and analysis plan is described in the section titled *Age-4 Resting EEG Hypotheses and Analysis Plan* below. To investigate family process impacts, we will apply our estimation strategy to maternal and family measures gathered at child ages 1, 2, 3, and 4 as shown in Appendix Table 8.

Attrition. The greatest threat to internal validity is potential bias from sample attrition overall, within site, and differential attrition rates by treatment status overall and within site. We will carefully track response rates by site, by treatment status across sites, and then treatment status within site. Response rates have been very high at ages 1. Of the original 1,000 recruited participants, we secured interviews with 931 at age 1, 922 at age 2, and 922 again at age 3. We expect at least 800 completed cases in our age-4 lab visit.

We will also conduct sensitivity checks to evaluate whether missing data might be biasing estimates. Most sample attrition that is systematically related to our outcomes of interest (Y) would presumably also be related to the distribution of baseline characteristics (X), and so bias due to sample attrition would be evident if our estimates are sensitive to conditioning on baseline characteristics. Some attrition may be due to time-varying (or unobserved) characteristics, and we can approach this problem in two ways. First, we will examine the sensitivity of our results to worst-case bounds, which enable us to bracket the true effects of our treatment without imposing any assumptions about the unobserved outcomes of participants (Manski, 1989; Manski, 1990; Manski, 1995). A second approach to addressing the problem of missing data will be to use multiple imputation strategies with all available data, (including all survey and administrative data on outcomes and predictor variables). Multiple imputation is an appropriate method if, conditional on all observed information, data are missing at random. Finally, because we have permission to collect administrative data from over 75% of mothers, we will be able to compare survey respondents and survey non-respondents on formal earnings and receipt of income from social programs.

Interpretation of parameters. The coefficients obtained in our regression models will be used to quantify the causal effects of the $313/month difference in income supplementation on age-1 and 4 child brain circuitry, cognitive development and socioemotional functioning. We will use the same methods to generate causal impact estimates for the family processes in each of the conceptual pathways. Examining the possible explanatory mechanisms in this way uses a series of separate regression equations to estimate program effects on possible treatment mediators, rather than estimating a structural-equation mediation model, and has been effectively used to infer possible mediation in comparable studies. This approach is preferred because it preserves the experimental variation in income generated by random assignment. The underlying insight is that randomization occurred with respect to receipt of the cash gifts and not on the basis of the proposed pathway mediators. With the potential for multiple mediators, a causal interpretation cannot be given to mediational models without very strong, often implausible, assumptions that there are no unobserved confounds of the association between the mediator and outcome. Still, the pattern of impacts can yield important insight as to which processes are likely to be present and absent and set the stage for future analyses.

Statistical power. The compensation difference between families in the high- and low-cash gift groups amounts to $313 per month and $16,276 over the course of the 52 months. This amount is in the range of income increases associated with child impacts of around .20 sd in studies of welfare experiments and the EITC (Duncan, Morris & Rodrigues, 2011; Morris, Duncan, Clark-Kauffman, 2005; Dahl & Lochner, 2012). After accounting for likely 20% attrition in the age-4 lab visit, and in the absence of adjustments for sample clustering within hospitals or increased precision owing to the inclusion of baseline covariates in our impact estimates, the sample size of 800 at age 4, divided 40%/60% between high and low payment groups, provides 80% statistical power to detect a .219 sd impact at $p < .05$ in a two-tailed test on cognitive functioning and family processes. The use of baseline covariates in estimation models will improve this power, while the use of bootstrap standard errors will decrease it. Based on exploratory analyses of age-3 cognitive outcomes in the Fragile Families study, we expect that these two offsetting factors will have little net impact on the size of our estimated standard errors.

Multiple comparisons. One strength of our study is the collection of survey, neuroscience lab and administrative data on a wide range of outcomes and explanatory pathways. However, the probability of rejecting a true null hypothesis for at least one outcome is greater than the significance level used for each test. We will address the possibility of false positives while minimizing the reduction in statistical power to detect meaningful effects. Best-practice methods differ across disciplines so we will draw from multiple approaches with the goal of ensuring that results from one approach are consistent with results from others (Romano & Wolfe, 2005; Porter, 2018; Benjamini, 2010; Holm, 1979, Westfall & Young, 1993; Schochet, 2008). Where possible we have aggregated measures used to test our pre-registered hypotheses into indexes. In the case of related measures that cannot be aggregated into a single index, we will estimate the statistical significance of the entire family ("familywise error rate") using stepdown resampling methods in Westfall and Young (1993; Westfall, Tobias, Wolfinger, 2011). Pre-registered clusters of measures are identified with grey bars in appendix tables.

Data release. We are releasing data and documentation from our study to the research community approximately 18 months following the end of each data collection wave to enable independent researchers to pursue replication, mediation, moderation as well as other related analytic questions.

Age-4 Resting EEG Hypotheses and Analysis Plan.

Following our publication of Age-1 resting EEG treatment impacts (Troller-Renfree et al., 2022), we amended our Age-4 resting EEG analysis plan to include primary and secondary hypotheses. The original preregistration of Age-1 EEG data included hypotheses across multiple frequency bands. However, due to participant refusal of EEG, the rejection of artifact-laden EEG files, and the high correlation between EEG bands as well as the expected effect size and consistency of the hypothesized effects, we were left with inadequate statistical power for multiple hypothesis testing across bands. As we have uniform, directional hypotheses for all three mid- to high-frequency bands, we have updated this analysis plan to instead include an index of mid- to high-frequency power (described below; primary hypothesis) as well as more traditional neuroscientific investigation of power within bands (described below; secondary hypothesis). Please see the history of preregistrations, including analysis plans, to see a history of how Age-1 EEG findings altered our preregistered analyses.

For our primary hypothesis, we will test whether the high-cash gift group has more mid- to high-frequency power than the low cash gift group, we will create a single a single composite measure that aggregates across the portion of the spectrum defined by the three mid-to-high-frequency bands (alpha, beta, and gamma power), from 7-45 Hz. Because this approach is focused on estimating intent-to-treat differences in a single index score, there is no need for multiple-testing adjustments. Covariates will include all preregistered covariates as well as the number of artifact-free epochs contributed by each participant. Models will be examined with and without preregistered baseline covariates as above, and we will conduct sensitivity checks to evaluate whether missing data might be biasing estimates, as described above.

As to secondary hypotheses, consistent with the methods used by another prominent RCT examining an early-life intervention on EEG activity (Debnath, Tang, Zeanah, Nelson, & Fox, 2020; Marshall, Fox, & BEIP Core Group, 2004; Vanderwert, Marshall, Nelson, Zeanah, & Fox, 2010; Vanderwert, Zeanah, Fox, Nelson, & III, 2016), we will explore band-specific and regional effects using mixed-design analyses of variance (mixed-ANOVA). Our secondary hypothesis is that there will be an intervention effect on frontal gamma spectral power between the low-cash gift group and high-cash gift group. Covariates will include all preregistered covariates as well as the number of artifact-free epochs contributed by each participant.

In addition, to explore all regional-frequency effects, we will perform separate mixed-ANOVAs for each frequency band of absolute and relative power with region (frontal, central, parietal, occipital) as a within-subject factor, and group (low-cash, high-cash) as the between-subjects factor. Greenhouse–Geisser correction will be applied for violations of sphericity. Post hoc comparisons will be performed for significant main effects of group. Any main and interaction effects not involving group will not be followed up. Multiple-adjustment corrections will be applied for all post hoc comparisons. Covariates will include all preregistered covariates as well as the number of artifact-free epochs contributed by each participant.

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