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EVIDENCE FROM THE EXPANDED CHILD TAX CREDIT
AND PANDEMIC STIMULUS CHECKS

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ABSTRACT

During the COVID-19 pandemic, the federal government issued stimulus checks and expanded the child tax credit. These pandemic payments varied by marital status and the number of children in the household and were substantial with some families receiving several thousand dollars. We exploit this plausibly exogenous variation in income to obtain estimates of the effect income on infant health. We measure the total amount of pandemic payments received during pregnancy, or the year before birth, and examine how this additional income affects birthweight, the incidence of low birth weight, gestational age and fetal growth. Data are from birth certificates and analyses are conducted separately by maternal marital status and education (less than high school or high school) to isolate only the variation in pandemic payments due to differences in the number of children (parity). Estimates indicate that these pandemic cash payments had no statistically significant, or clinically or economically meaningful effects on infant health. Overall, the findings suggest that income transfers during pregnancy will have little effect on socioeconomic disparities in infant health.

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1. Introduction

It is truism that more income is better than less. Increases in income expand our consumption possibilities and those with more income consume more than those with less income. Higher income also expands investment possibilities (i.e., future consumption) and those with higher income can invest more than those with lower income, particularly when borrowing to invest is infeasible. Investments in human capital are one important type of investment that is difficult to finance fully through borrowing (Ben-Porath, 1967). Therefore, low-income individuals and low-income families may be unable to invest efficiently in their own and their children's human capital. In the case of low-income children, shortfalls in human capital investments is likely to reduce their wellbeing throughout their life and perpetuate intergenerational poverty (Becker & Tomes, 1979).

The role of family income in financing child human capital investments has motivated a large empirical literature examining the relationship between family income and child development (Almond & Currie, 2011; Almond, Currie, & Duque, 2018; Cooper & Stewart, 2021; Risnes et al., 2011). One child development outcome that has received considerable attention is infant health. Infant health is a particularly important child developmental marker because it has been linked to a range of childhood and adult outcomes (Almond & Currie, 2011; Almond et al., 2018; Nepomnyaschy, 2009; Risnes et al., 2011). The magnitude of the empirical association between family income and infant health is also notable. For example, Nepomnyaschy (2009) reported that, for a nationally representative sample of infants born in the US in 2001, the odds of an infant from a family in the lowest quartile of income being low birth weight (birth weight <2500 grams), or small for gestational age (SGA), was more than twice that of an infant from a family in the highest quartile of income.[1] Similar results for the US were reported by Martinson and Reichman (2016), but these authors also documented an almost as strong relationship between relative family income and infant health in the United Kingdom (UK), Canada and Australia.

If causal, the positive association between income and infant health presents an opportunity to intervene to diminish income-related disparities in infant health, for example, by providing cash assistance to low-income families with children. In fact, based on this reasoning, several programs around the world provide money during pregnancy to improve infant health, for example, the Sure Start Maternity Grant in England and Baby and Pregnancy Payment in Scotland. However, as we describe below, while there are reasons to think that income is causally linked to infant health, there are also reasons to expect little or no effect. Moreover, quasi-experimental studies of the effect of income on infant health have not produced a consensus about the causal effect of income, although most studies reported a positive effect. Thus, it is unclear whether cash transfers during pregnancy is an effective (efficient) policy to address socioeconomic disparities in infant health. In terms of theory, the absence of a consistent finding of the effect of income on infant health limits our understanding of the causal pathway from family income to infant health that is mediated through the proximate causes (e.g., prenatal care) of infant health.

In this article, we add to the literature on the effects of income on infant health. We obtain estimates of the effect of a plausibly exogenous and large increase in income during the year prior to pregnancy on infant health. The variation in income we use stems from cash transfers associated with Coronavirus Aid, Relief, and Economic Security Act, Coronavirus Response and

[1] Nepomnyaschy (2009) also reported separate results by race and ethnicity and found a qualitatively similar gradient of infant health with respect to family income.

Relief Supplemental Appropriations Act , the American Rescue Plan Act of 2021, and the expansion of the Child Tax Credit in 2021 (Internal Revenue Service, 2024). Payments associated with these laws were substantial and vary by parity (family size), marital status and family income.

To answer this research question, we use data on infant health (e.g., birthweight, preterm birth) from vital statistics. For most analyses, we limit our analysis to births occurring between April 2020 and December 2021, but also report results for a longer period from 2018 through 2021.[2] We stratify the sample of births by marital status and education (less than high school and high school) because these factors are strongly related to family income, which is a determinant of the eligibility to receive pandemic payments. Therefore, the variation in cash payments that we use to obtain estimates of the effect of income on infant health is by parity within marital and education groups. Our analyses adjust for the effect of parity, time and other covariates, and we show that infant health of mothers of different parity follow statistically indistinguishable trends in periods prior to the pandemic-related cash payments.

Estimates of effects of $1000 increase in income during pregnancy or over a year before the infant's birth are small (e.g., <1% of mean) and the vast majority are not statistically significant. Estimates suggest that even larger cash amounts have small effects, suggesting that the pandemic cash transfer programs have had little effect on infant health. Overall, findings suggest that pandemic cash payments had no statistically significant, or clinically or economically meaningful effects on infant health.

2. Related Literature

[2] While there were some change in the fertility rate during this pandemic period, much of it was due to fewer immigrant births, which we drop from the analysis (Bailey, Currie, & Schwandt, 2023; Kearney & Levine, 2023).

There are a few existing studies that examined the effect of policies that are largely income transfers on infant health. Two studies examined the effect of variation in universal (not targeted) income payments from the Alaska permanent income fund on infant health (Chung, Ha, & Kim, 2016; Wyndham-Douds & Cowan, 2022). Chung et al. (2016) studied the period just before and after the first payments from the Alaska fund were distributed in 1982 and used a difference-in-differences design using other states as comparison group. They found that the increase in income from the fund was associated with a small increase in birthweight and small decrease in the probability of low birth weight. In contrast, Wyndham-Douds and Cowan (2022) exploited variation in payments by year and family size over the period from 1982 to 2010. They reported that the annual payments, which ranged from $625 to $16,832, had virtually no effect on infant health. Note that the timing of the income payments is not explicitly linked to pregnancy and are annual disbursements. Our study adds to these by providing evidence on the effect of relatively large cash transfers received during pregnancy for the near universe of births to low-educated (HS or less) mothers in the US in a recent period. As shown in Wyndham-Douds and Cowan (2022), there are significant differences between births in Alaska and the US that may limit the generalizability of these study findings.

There are also a series of studies of a cash transfer program in Manitoba, Canada: Healthy Baby Prenatal Benefit (HBPB). HBPB provided approximately Can$80 per month during the second and third trimesters of pregnancy for low-income women. Brownell et al. (2016) conducted a study of the program comparing infant health of mothers who received the payment to infant health of mothers who did not receive the payment but who were on welfare and low-income. After propensity-score adjustment, results indicated that the program was associated with approximately a 25% reduction in low birth weight and preterm birth. Studies by

Enns et al. (2021) and Struck et al. (2021) reported similar effects of the program for First Nations mothers and infants in Manitoba. The relatively large effects of a such a modest cash transfer (Can$500 during last two trimesters of pregnancy) in a setting in which there is universal healthcare is somewhat surprising and suggest that, perhaps, nutrition and stress may be important pathways. Again, our study of a recent large, national cohort and of a cash transfer that is substantially larger extends this research.

Reader (2023) studied the universal Health in Pregnancy Grant (HPG) implemented in the UK in 2009, which is now called the Sure Start Maternity Grant. The HPG provided approximately $300 (190GBP) in the third trimester to all pregnant women with the requirement that they attend antenatal meetings with a healthcare professional. Results from a regression discontinuity design that exploited the calendar date of eligibility for the HPG indicated that HPG was associated with a 10% reduction in preterm birth and a 3% reduction in low birth weight. Again, these effects seem to be sizeable for a universal (i.e., not income targeted), small cash grant in the last trimester of pregnancy, particularly because Reader (2023) reported that the conditional requirement to attend antenatal services had little impact, which is not surprising given the universal healthcare of the UK. Reader (2023) speculates that less tress may be the primary mechanism.

A study from Uruguay examined the effect of a cash grant and in-kind food benefit program (PANES) for poor households on infant health (Amarante, Manacorda, Miguel, & Vigorito, 2016). Eligibility was determined by family income and the analysis exploited the income eligibility cutoff to measure the effect of the program using a regression discontinuity design. Estimates indicated that the program, which increased income during pregnancy by approximately 25%, was associated with a 20% decrease in low birth weight and 10% increase in birthweight. The program had no effect on gestational age, preterm birth, and prenatal care.[3] The combination of findings is notable because it suggests that the cause of the reduced low birth weight rate was better nutrition (intrauterine growth).

The last study related to ours is González and Trommlerová (2022). This article investigated the effect of a universal child payment of €2500 in Spain in 2007 on infant health. A major difference between this study and the previous studies reviewed is that the child payments are not closely tied to pregnancy. The sample included births up to five years after receipt of the one-time child payment for the previous birth of the mother. Results of the study indicated that the one-time payment for the previous child decreased low- (<2000 grams) and very-low (<1500 grams) birth weight births by 20% to virtually 100% depending on sample (e.g., poor or not) and the threshold defining low-birth weight. These are huge effects for outcomes for which causes are largely unknown (IOM, 2007).

In sum, there are relatively few studies that directly assess the effect of an exogenous increase in income during a relevant preconception or prenatal period on infant health. Half were case studies of one state (Alaska) or province (Manitoba) and examined different types of cash transfers—one linked to pregnancy (Manitoba) and the other a general income support payment (Alaska). Three were country-specific studies that examined fundamentally different types of transfers: a small cash grant in third trimester of pregnancy (UK); a one-time cash payment to families for a previous birth (Spain); and a basic income support program to low-income families lasting up to two years in Uruguay. These studies also differed in the targeting of the payment. In Manitoba and Uruguay, the payments were targeted at low-income families while the other

[3] The program was associated with a decline in births attended by a physician and paid for with private insurance, and an increase in births in public hospitals. With respect to non-health outcomes, the program decreased mother's labor supply by 25% and decreased out-of-wedlock birth modestly.

programs were universal. Most studies reported a positive impact of the income transfers, although magnitudes were not related to the size and duration of the income transfers. The small, third trimester payment in the UK had about the same relative effect on infant health as the two-year income support program in Uruguay, and the one-time child payment in Spain that was up to five years prior to the birth of the focal child was associated with very large decreases in low-birth weight. Payments from the Alaska income fund, which were also large, had little to no effect on infant health. In short, it is hard to draw a conclusion about these results in terms of the effect of the timing, targeting and size of the income transfer on infant health and the likely mediating mechanisms.

Our study provides an analysis of a substantial cash transfer for a large portion of the US births. Notably, the variation income is mostly a pure income transfer.[4] This distinguishes our study from related literatures that examined the effect of social policies that provide nutritional subsidies to pregnant women (e.g., Special Supplemental Nutrition Program for Women, Infants, and Children) or alter income by changing incentives to work (e.g., Earned Income Tax Credit).

As just noted, our study is related to several strands of literature that examined the effect of policies and events that affect family income on infant health. There are several studies of the effect of the Earned Income Tax Credit (EITC) on infant health (Bruckner, Rehkopf, & Catalano, 2013; Dench & Joyce, 2019; Hamad & Rehkopf, 2015; B. Hill & Gurley-Calvez, 2019; Hoynes, Miller, & Simon, 2015; Markowitz, Komro, Livingston, Lenhart, & Wagenaar, 2017; Qian & Wehby, 2023; Strully, Rehkopf, & Xuan, 2010). The EITC is not a pure income transfer because

[4] The expansion of the CTC is limited to families under a specific income threshold. However, there was no phaseout of the expanded credit that affected the price of work (substitution effect). Thus, all families who were eligible received the lump sum (e.g., $300 per month). The expanded credit did not change the phaseout rate or range of the existing CTC. Similarly, the stimulus checks did not affect the price of work, although they too were limited to families under a specific income threshold. Almost all families in our sample received the maximum amount of stimulus checks.

it changes the opportunity cost of work, which affects both the quantity of work and earnings (income). Results of most of these studies generally find that EITC payments are associated with small improvements in infant health, although recent work comparing contiguous counties found no effects of refundable state EITC on birth weight and preterm birth (Qian & Wehby, 2023). Therefore, results from these studies are not strictly interpretable as the effect of income on child health.[5] A predecessor of the EITC studies was the auxiliary analysis of the sample included in the Gary Income Maintenance Experiment (Kehrer & Wolin, 1979). Like the EITC, the Gary experiment altered incentives to work (i.e., negative income tax), although the Gary experiment included a basic income guarantee that is not a feature of the EITC. Surprisingly, results of the analysis showed that for several groups within the treatment group, the experiment was associated with worse birth outcomes. One note of caution about the study is that only about 60% of births to those in the experiment were observed.

There are also several studies of the effect of the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) program on infant health (Bitler & Currie, 2005; Currie & Rajani, 2015; Figlio, Hamersma, & Roth, 2009; Foster, Jiang, & Gibson-Davis, 2010; Gai & Feng, 2012; Joyce, Racine, & Yunzal-Butler, 2008; Kowaleski-Jones & Duncan, 2002; Peet, Schultz, Lovejoy, & Tsui, 2023; Sonchak, 2016).[6] These studies are related because WIC provides food subsidies that may be (partly) fungible and similar to an increase in income, although WIC provides other services including health care referrals and nutritional education

[5] Strully et al. (2010), Markowitz et al. (2017), and Hill and Gurley-Calvez (2019) study whether the presence of a state EITC affected infant health, and all find that the state EITC was associated with small improvements in infant health. Hoynes et al. (2015), Hamad and Rehkopf (2015) and Bruckner et al. (2013) examined the federal EITC exploiting differences in payments by parity and find small improvements in infant health. Dench and Joyce (2020) provide evidence that national analyses of EITC may be confounded by omitted factors.

[6] Hoynes et al. (2011) examined the introduction of WIC in the 1970s and found that infant health improved modestly in areas where WIC was implemented.

that open up alternative pathways.[7] Results from these studies generally find that WIC decreased the probability of having a low birth weight infant with little effect on mean birthweight.

Other studies related to our study are those that examined events and policies that affect income and potentially infant health. For example, there are several studies of the effect of shale gas fracking, oil drilling and coal prices on infant health (E. L. Hill, 2018; Lim & Kim, 2016; Willis et al., 2021). While these studies examined events that potentially directly affect infant health, another mechanism are changes in income. Two studies that are perhaps most germane are Wehby, Dave, and Kaestner (2020) who examined the effect of changes in minimum wage on infant health and Lindo (2011) who examined the effect of job displacement on infant health. Both of these studies relied on changes in income as an important, if not the primary, mediating pathway, although labor supply responses are also a significant possibility. Wehby et al. (2020) reported a small positive association between a higher minimum wage and birth weight. Lindo (2011) found that job loss was associated with a small decrease in birth weight.

While the weight of the evidence suggests that income has a modest, beneficial effect on infant health, much of it comes from studies that examined policies and events that had alternative causal pathways, for example, time allocation and labor supply. Studies that directly examined the effect of income transfers are fewer and less consistent (e.g., Alaska studies and UK study). We add to this limited literature by examining the effect of a large income transfer for a national cohort of births.

3. Why income may or may not affect infant health

[7] Like WIC, the Food Stamp Program provides food subsidies that may be fungible as income. Almond et al. (2011) examined the effect of the introduction of Food Stamps Program in the 1960s and 1970s to examine its effect on infant health. Results suggest that the availability of Food Stamp Program was associated with a small increase in birth weight.

There are several plausible mechanisms (mediators) that can explain the empirical association between family income and infant health (Appendix Figure 1). First, infant health depends on the health of the mother, and higher family income facilitates better nutrition and a better living environment (e.g., less crime and pollution) for the mother that may improve maternal and infant health (American College of Obstetricians and Gynecologists, 2005; Fiore, 2002; Floyd et al., 2013; K. Johnson et al., 2006). Similarly, higher family income reduces financial stress, which has been linked to poor mental health and harmful health habits that can adversely affect the health of the mother and infant (Guan, Guariglia, Moore, Xu, & Al-Janabi, 2022; Ryu & Fan, 2023; Tsuchiya, Leung, Jones, & Caldwell, 2020; Weissman, Russell, & Mann, 2020). These two pathways are most pertinent to programs that provide longer duration income support such as the PANES program in Uruguay. Third, income is associated with health insurance coverage and access to preconception and prenatal care, although the scope for this to be the mediating pathway is limited in US.[8] Finally, unearned income from a cash transfer may affect labor supply and impact infant health through this pathway (Corinth, Meyer, Stadnicki, & Wu, 2021; Duncan & Le Menestrel, 2019).[9] However, labor supply effects are estimated to be quite small and are an unlikely explanation (McClelland & Mok, 2012).

There are also reasons to expect income to have little, or even a negative, effect on infant health. An increase in income may result in greater consumption of unhealthy foods (e.g., processed food) and drinks (e.g., alcohol and sugar sweetened beverages) that adversely affect maternal health (e.g., obesity) and infant health (Avcı et al., 2015; Meehan, Beck, Mair-Jenkins,

[8] Expanded insurance coverage in the US and relatively similar rates of prenatal care across income groups, as well as the presence of an income gradient in infant health in countries with universal health insurance coverage (UK and Canada) suggest that prenatal care utilization is unlikely to be a particularly important mechanism linking income to infant health in the US.
[9] See the debate over the effects of child tax credit on labor supply (Corinth et al., 2021; Duncan & Le Menestrel, 2019).

Leonardi-Bee, & Puleston, 2014; Ruager-Martin, Hyde, & Modi, 2010), although income elasticities of food and beverages are relatively small and unlikely to result in significant changes in maternal health, particularly over a short period. Second, for those relatively few women who are uninsured income may help with medical expenses, for example, for prenatal care that is relatively cheap and may be accessible with greater income. But as noted, few women are uninsured even during preconception period (Margerison, Kaestner, Chen, & MacCallum-Bridges, 2021). Finally, as noted, maternal health is a long-term outcome influenced by a lifetime of investments. Short-term changes in income, for example, during the pregnancy, may have relatively little effect on maternal health, and thus, infant health. It is arguably permanent income that matters for infant health, although the review above reported evidence that short term and longer-term income transfers had beneficial effects on infant health.

At a more conceptual level, for additional income to matter, there must be productive opportunities to invest in maternal and infant health that have not been undertaken. Consider maternal nutrition, which is a proximate cause of maternal and infant health. Do we expect additional income to significantly improve maternal nutrition? Evidence suggests that only 5% to 10% of additional income is spent on food among low-income households (Golan, Stewart, Kuchler, & Dong, 2008). Thus, the pathway from income to infant health through maternal nutrition seems unlikely to be significant except for perhaps the most destitute and malnourished mother. Similar types of logic apply to the other potential mediating pathways. How income sensitive are prenatal care visits? While not a direct proxy for income, our own calculations using vital statistics suggest that the number and timing of prenatal care visits differs relatively little by education (income). Additional income is unlikely to significantly affect infant health through this pathway, except, again, for the most disadvantaged. What other proximate causes of infant health are underinvested in because of limited liquidity? Neighborhood air pollution (Bekkar, Pacheco, Basu, & DeNicola, 2020)? Are low-income families under investing in residential location with respect to air pollution? Perhaps the answer is yes, but to be a meaningful pathway, residential location would need to be quite sensitive to additional income, particularly because air pollution has decreased significantly in all locations, but particularly in low-income areas (Currie, Voorheis, & Walker, 2023). The upshot is that, in the current US setting, the scope for income to affect infant health seems limited in the sense that there exist productive investments that have not been undertaken because of a lack of income.

Another way to view the problem is through the etiology of infant health, for example, preterm birth. There are few known causes of premature birth (IOM, 2007) and if the causes of the outcome are relatively unknown, then so must be the productivity of investments to prevent preterm birth. Therefore, it is unlikely that income can alleviate a liquidity constraint preventing productive investment.

To summarize, while there are potential mediating pathways through which income may affect infant health, most studies that examined the effect of income on infant health have not identified these pathways. In fact, studies such as Amarante et al. (2016), Brownell et al. (2016) and Reader (2023) show that prenatal care is not a mediating pathway. Other pathways are seldom studied. Here, we have briefly considered the likelihood of a couple of those alternative pathways and the conditions needed for them to be significant. In our view, the often-articulated pathways from income to infant health do not seem to be highly plausible, although this view is inconsistent with the preponderance of evidence reviewed earlier that income appears to be beneficial to infant health. That evidence, however, is itself difficult to reconcile when consideration is given to the timing and amount of income transfers vis-à-vis the reported effects.

4. Data

The primary data for the study come from the restricted version of the birth certificates from the U.S. Vital Statistics Natality Files, which includes the universe of all births occurring in the 50 states and Washington D.C. every year. For each birth, the data include detailed information on birth outcomes along with maternal demographic and socioeconomic characteristics such as age, race, education, marital status, and parity. The restricted version also includes county-level geocodes. We use data for years 2018 through 2021 that cover two years before the COVID-19 pandemic and two years during the pandemic when most of the federal pandemic related cash transfers that we study occurred. The primary sample is limited to women with a high-school degree or less between the ages of 18 to 39 (at time of pregnancy) years.

We consider alternate sample selection criteria. We initially exclude women giving birth to their first child (0 parity) because they would not have benefited from the CTC expansion and the child stimulus checks. Moreover, the parity 0 group may have experienced different infant health outcome trends during the pandemic than higher parity groups considering the disruptions to childcare and school activities during the pandemic. A second criterion to select the sample is the period of analysis. We start with the period from April 2020 to December 2021. Virtually everyone in this sample would have been eligible to receive cash transfers during the pandemic period (Figure 1). We also used an alternative time period from January 2018 through December 2021, which includes a long period of no cash transfers as part of the variation in deriving the cash transfer effects. We also limit the sample to women giving birth to their fifth child (parity 4) as the frequency of higher parity is extremely low (4.2%). Depending on the model and sample, the sample size for the study ranges between 146,910 and 1,166,689.

Infant health is measured several ways using information on birth weight and gestational age. We use a continuous measure of birth weight, and a binary indicator for low birth weight indicating birth weight less than 2500 grams. Gestational age is measured continuously in weeks, and alternately as a binary indicator for preterm birth (infant born before 37 weeks). We also calculate fetal growth (grams/week) defined as birth weight divided by gestational age as an additional outcome. The sample descriptive statistics are presented in Appendix Table 1.

5. Methods

Our empirical objective is to estimate the effect of income during pregnancy on infant health. The variation in income that we use to accomplish this objective is from COVID-19 pandemic payments.

5.1. COVID-19 Pandemic Cash Transfers

The income transfers during the COVID-19 pandemic began in March 2020 and ended by December of 2021. These transfers included three stimulus disbursements (Internal Revenue Service, 2024). The first was issued on March 27th, 2020, under the Coronavirus Aid, Relief, and Economic Security Act that provided $1200 per adult and $500 per qualifying child. The second was issued on December 27th, 2020, under the Coronavirus Response and Relief Supplemental Appropriations Act of 2020 that provided $600 per adult and $600 per qualifying child. The last transfer was issued on March 11th, 2021, under the American Rescue Plan Act of 2021 (ARPA) and provided $1,400 per adult and $1,400 per qualifying child. The ARPA also expanded the Child Tax Credit eligibility and credit amount and provided advanced monthly payments from July through December of 2021. Specifically, the ARPA expanded the maximum annual credit from $2000 to $3,600 per child for children aged 6 and younger and $3,000 per child for children aged 7 through 17. The whole credit was made refundable, and the earned income requirement was dropped; households could have no earnings and still qualify for the full credit per child. The credit diminished for incomes above $75,000 for single filers and $150,000 for joint filers. Families could receive half of total credit in advance via monthly checks ($250 for children aged 7-17 and $300 for children under 6) beginning in July through December 2021 unless they opted out from advance payment, and the other half can be claimed in the 2021 tax filing. The CTC expansion affected over 60 million (over 90%) of children (35 million households) nationally.

For each birth, we calculated the total amount of cash received by each mother throughout the entire conception period (i.e., 9 months prior to birth including birth month), and in some analyses the year (12 months including birth month) prior to birth. We used the maximum amount of income possibly received from the pandemic payments because, in our sample, over 90% of families have adjusted gross incomes that would make them eligible for the maximum payment.[10] Because the first and second stimulus checks were issued at the end of the month, we assign the following month for when individuals received those checks. An example helps to clarify the calculation. A single mother giving birth to her second child in August 2021 would have received the second stimulus check of $1200 in January 2021, the third stimulus check of $2800 in March 2021, and two months of advanced monthly child tax credit payments ($600 in total) for a total of $4600 over the 9-month pregnancy period (December 2020-August 2021). Appendix Table 2 reports the eligible cash transfers for each birth cohort.

As the above example illustrates, the amount of cash received varied by number of children and marital status. Accordingly, we conduct separate analyses for four education-by-marital status groups: non-married women with education less than high school (LTHS, Non-Married), married women with education less than high school (LTHS, Married), non-married

[10] We calculated income using the American Community survey for women of the same age and parity as the mothers in our sample.

with education at high school (HS, Non-Married), and married with education at high school (HS, Married). We focus on mothers with high school or less education because as noted above the majority (over 90%) are expected to qualify for the maximum payment. Moreover, we separate less than high school from high school because of differences in income – the same cash payment will represent a proportionally larger income change with less work income for those with less than high school and thus potentially different income effects by education.

5.2. Statistical Analysis

Using the variation in income received during pregnancy due to the pandemic payments, we estimate the effect of income on infant health using the following regression model:

$$y_{ict} = \alpha + \delta Cash_{it} + \beta Parity_{ict} + \boldsymbol{X_{ict}\Gamma} + \boldsymbol{\theta_c} + \boldsymbol{\omega_t} + (\boldsymbol{X_{ict}} \times \boldsymbol{\omega_t})\boldsymbol{\Phi} + \boldsymbol{\epsilon_{ict}} \quad (1)$$

In Equation (1), y_{ict} is a specific infant health outcome (e.g., preterm birth) for a given birth i in county c in month-year t. The key explanatory variable is $Cash_{it}$, which is the total amount of cash received by the mother during the pregnancy period (a 9-month period since birth, including the birth month or alternatively the 12-month period leading to the birth month). Other variables in the model include: $Parity_{ict}$ (the number of previous births); maternal characteristics ($\boldsymbol{X_{ict}}$) including age, race/ethnicity (non-Hispanic White, non-Hispanic Black, non-Hispanic other races, and Hispanic), and type of insurance (Medicaid, private coverage, self-pay, and unknown); county fixed effects ($\boldsymbol{\theta_c}$); and time (birth year-month) fixed effects ($\boldsymbol{\omega_t}$). We also control for the interactions between age and race/ethnicity, and time fixed effects (i.e., demographic-by-time fixed effects). We allow time effects to vary by these maternal characteristics because they are strongly correlated with parity and birth outcomes (see Appendix Table 1). We estimate Equation (1) using data from April 2020 to December 2021, but in alternative analyses use the period from January 2018 to December 2021.

5.3. Assessing the Validity of Research Design

The variation in income ($Cash_{it}$) used to identify the effect of income on infant health is by parity-by-time and, thus, the threat to internal validity is from differential trends in infant health by parity-by-time. We assessed if there were differential trends in two ways. First, we examined whether trends in infant health outcomes differed by parity-by-time in periods before the pandemic payments were made. Second, we conducted a placebo analysis in which we assume the same cash transfers occurred over the same months in 2018 and 2019 and estimated the model of equation (1) for this placebo cash transfer.

6. Results

6.1. Variations in Cash Transfers

Figure 1 shows the variation in pandemic payments received during pregnancy by birth month and parity (0, 1, 2 ,3-4) for non-married mothers (i.e., one adult household). Figure 1 illustrates that there is variation in payments by parity conditional on birth month as well as variation over time conditional on parity. The regression model controls for parity fixed effects and time fixed effects, so the identifying variation in payments is from parity-by-time. For the parity 3 group, payments range from $0 to $8600, while for parity 1 group, payments range between $0 and $2000; the payments for other parity groups are within this range. Payments begin for births in March 2020, and in terms of month-to-month increase, the largest increase from the previous birth month is for births in March 2021. Finally, the largest total payment is for births in September of 2021. Appendix Figure 1 shows the income increase for married mothers which shows a similar pattern with a larger increase from stimulus checks for an additional adult.

One concern is that the cash transfers received during the pandemic is replacing lost earnings. To evaluate this possibility, we used data from the monthly Current Population Surveys to measure monthly employment rates of the demographic groups used in our analysis. Appendix Figure 3 shows how employment evolves during our period of analysis. At most, we observe a modest decrease in employment around the second quarter of 2020 and around the time of the first pandemic payment. However, the decline in employment was temporary and affected a relatively small portion of our sample because many mothers did not work. Thus, the pandemic payments represent a substantial increase in family income with very little likely being a replacement of lost earnings.

6.2. Evidence of the Validity of Research Design

We begin with event study analyses to examine if there are divergent trends in infant health outcomes by parity before the pandemic. These event study analyses compare infant health outcomes over time and parity groups (2 or 3-4 children versus 1 child) from January 2018 through December 2021, with the first quarter of 2020 as the reference period. Estimates are shown in Figures 2 through 6. Estimates reveal no systematic divergence in pre-trends in outcomes across parity groups (within marital status and education). While there are a few statistically significant estimates, that would be expected by chance and there are overall no patterns consistent a violation of the parallel trend assumption. Similarly, there are no consistent differences in trends during the pandemic when the income transfers occur that would suggest an effect of income on infant health.

Next, we report results from a placebo analysis using “pseudo” income transfers. Specifically, we simulate pandemic payments in terms of calendar timing and family characteristics in the period prior to the pandemic from April 2018 through December 2019. For example, the income transfer for April 2020 is assigned to April 2018; that for December 2021 is assigned to December 2019; and so forth. Estimates from this analysis are shown in Table 1. For mothers with less than high school education, estimates are small and not statistically or clinically meaningful. For mothers with high school education, there are a few small, statistically significant estimates. For example, estimates indicate that a $1000 increase in income is associated with an increase in birth weight of 2 grams for non-married mothers and 4 grams for married mothers. While statistically significant, these estimates are very small and clinically insignificant. Overall, we view these results as additional evidence of a valid research design.

6.3. Estimates of the Effect of Income on Infant Health

Table 2 shows estimates of the effect of total pandemic payments on birth outcomes separately for four demographic groups classified by marital status (married, non-married) and education (LTHS, HS). Estimates are shown as effects of a $1,000 additional payment. Several estimates for each outcome and each group are shown and they differ by the following:

- whether a 9-month or 12-month period prior to birth is used to measure the amount of pandemic payments;
- whether women with parity 0 are included;
- and whether the sample period is 2020-2021 or 2018-2021.

For all of the demographic groups and different samples for each group (e.g., 12-month income, include parity 0), estimates of the effect of $1000 of income on all five measures of infant health (birth weight, low birth weight, gestational age, preterm birth, and fetal growth rate) are small and most are not statistically significant. There are small, statistically significant improvements in birth weight and other measures of infant health from models that use the extended time period from January 2018 to December 2021. However, even these estimates are quite small, and estimates are precise enough to rule effects large enough to be clinically meaningful. For birthweight, estimates are precisely enough estimated to rule out an effect of

$1000 of income of 5 grams or larger, which is small and arguably clinically unimportant given a mean of approximately 3200 grams. Similarly, for low birth weight, the precision of the estimates can rule out an effect of $1000 of income of 0.1 to 0.2 percentage points, depending on the sample, and again, these are quite small relative to the respective low birth weight rates. Even larger cash amounts have small effects; estimates suggest that a payment of $5000 would be associated with 10- to 15-gram increase in birth weight, which on average would be 0.3% to 0.5% increase in birth weight. Overall, the estimates provide consistent evidence that relatively large cash transfers from these programs have had little effect on infant health.

Next, we report estimates from a model that replaces the total cash payments measured as a continuous variable with binary indicators for ranges of payments. Results are shown in Figures 7-11. Across subgroups, estimates are generally small and almost all are statistically not significant. Also, there is no evidence of an income gradient with larger income transfers being associated with larger effects. Out of the 80 estimates in Figures 7 through 11, only three are statistically significant indicating a small increase in gestational age for married women of high school education who received income transfers up to $4,400 versus up to $2,400.

6.4. Estimates by Pregnancy Trimester of Payment

As discussed in the literature review, some studies have examined the effect of income received in the second and third, or just the third trimester, and have found significant results. These findings may reflect a critical period in which income may matter. To investigate this hypothesis, we estimated models that broke out the income transfers by the pregnancy trimester of receipt. Estimates form this model are in Appendix Table 3. Only three of 60 estimates in Appendix Table 4 are statistically significant and all estimates are small relative to the mean.

Among the statistically significant estimates, a $1000 increase during the 3rd trimester is associated with greater birth weight (+6 grams) and gestational age (+0.024 weeks) among married mothers with less than high school education. However, payments during the 2nd trimester are associated with declines in birth weight (-8 grams) and fetal growth (-0.2 grams/week). Overall, estimates of the effect of $1000 income by trimester of receipt are consistent with estimates reported earlier and suggest that this amount of cash transfer, and even larger amounts, regardless of when it is received, has little association with infant health.

6.5. Additional Analyses

To further assess the sensitivity of these estimates of the effects of cash transfer effects on infant health, we conducted two additional analyses. First, we estimated a model that excluded the interactions between race/ethnicity and age indicators with the time (birth year-month) indicators. These interactions were included because race/ethnicity and age are correlated with parity and are useful controls to bolster the identification strategy that relies on parity-by-time variation income. Second, we added county-by-time fixed effects (interactions between county and year-month dummy variables) to the model to control for time variation by geography that may be correlated with the evolution of the pandemic. Estimates from these additional analyses are shown in Appendix Table 4. A review of these results indicates that estimates from these models are very similar to those presented earlier. The similarity of estimates bolsters the support for the research design and validity of the estimates discussed earlier. For example, excluding the demographic-by-time fixed effects has virtually no effect on estimates, which suggests that the reliance on parity-by-time variation is likely valid.

6.6. Did Pandemic Payments Offset Adverse Impacts from Healthcare Disruptions

To this point, estimates have indicated that the large, pandemic cash payments had little effect on infant health. One possibility is that the payments did have a positive effect, but these positive effects were offset by contemporaneous circumstances that adversely affected infant health, for example, because of disruptions in prenatal care. We investigated this possibility by examining the effect of pandemic payments on prenatal care and pregnancy weight gain, which are two indicators of engagement with the healthcare system.[11] Estimates are presented in Appendix Table 5. All estimates are very small and statistically insignificant. There is no evidence that pandemic payments were correlated with prenatal care or weight gain.

7. Conclusion

There is a strong, positive association between family income and infant health. Infants from families in the bottom quintile of income are 2.4 times more likely to be born with low birthweight than infants from families in the top quintile of income (Martinson & Reichman, 2016). The long lasting, negative consequences of poor infant health make this socioeconomic disparity a significant problem and it is critical to understand to what extent income changes can reduce this disparity in infant health.

Whether income changes during pregnancy causally impact infant health among low-income families remains an open question including the magnitude of effects and which income-support policies are effective. Prior studies of this question have not provided a consensus answer, or an easily characterized set of facts, and there have been relatively few studies of policies that solely provide a cash transfer irrespective of employment and do not present conceptually or empirically direct effects on employment. In the US, the only studies of a pure cash transfer program are two analyses of the effect of income from the Alaska Permanent

[11] Weight gain may also reflect nutritional status that is independent of interaction with the healthcare system.

Income Fund on infant health. These two studies came to different conclusions, and generalizability of their findings to the whole US is not clear. Moreover, there have been no other recent national policies providing cash payments without direct links to or effects on employment than those during the COVID-19 pandemic. The pandemic cash transfer programs including the stimulus checks and the CTC expansions are notable considering that payments were relatively large especially for families with more children and essentially universal to low-income families. As such, our study contributes significantly to this literature by providing an analysis for the US as a whole, for a recent period, and for a large, plausibly exogenous change in income from cash payments not tied to employment.

Results from our analysis indicate that $1000 increase in income during pregnancy, or in the year before the infant's birth, had virtually no statistically significant, clinically important, or economically meaningful effect on infant health. Even if we extrapolate and calculate effects of larger income transfers, estimates still suggest small effects of questionable clinical relevance. To be clear, any improvement in infant health may be considered clinically important, so here we clarify exactly what we mean by the phrase and by the term economically meaningful (Currie, 2009; Ely & Driscoll, 2023; Smith et al., 2016). Consider infant mortality, Ely and Driscoll (2023) reported that low birthweight is the cause of death among infants 15% of the time. Thus, the 1% reduction in low birthweight from an additional $1000 of income during pregnancy that we find would reduce infant mortality by approximately 0.2%--one fifth of one percent. Low birth weight has also been associated with later life outcomes (Currie, 2009). Here too, estimates of the effect of income on low birth weight imply virtually no effect. Smith et al. (2016) reported that low birth weight was associated with a 0.4% (1.7% compared to 1.3%) increase in the hazard rate of experiencing any cardiovascular disease (CVD) event for a sample of approximately 64,000 women with an average age of 66 drawn from the Women's Health Initiative study. Our estimates suggest that the decline in low birth weight caused by an additional $1000 of income would have virtually no effect (-0.0004) on the hazard rate of CVD. Or consider the results in R. C. Johnson and Schoeni (2011) who reported that low birth weight was associated with 15% lower earnings at ages 30 to 40. Our estimates suggest that $1000 of income would reduce this negative impact by 0.2%--virtually nothing.

The finding that cash transfers during pregnancy during the COVID-19 pandemic have no meaningful effect on infant health has important policy implications. These results suggest that the socioeconomic disparities in infant health are unlikely to be diminished by income transfers during the pregnancy or year prior to birth. Thus, programs such as the UK Sure Start Maternity Grant are unlikely to be effective in the US.[12] Some studies of other economic support policies in the US including EITC, the minimum wage, or income-like transfers, for example, SNAP benefits, have found statistically significant positive effects on birth weight. Our estimates are generally not too different from these estimates, although somewhat smaller.[13] As noted previously however, programs like the EITC and minimum wage may also have employment and income effects and so estimates might not be directly comparable to those for the pandemic cash payments. More broadly however, these studies also point to small effects on

[12] We note that our results differ from Reader (2023) who studied the UK program. Reader (2023) reported that the small ($300) 3rd trimester grant was a associated with a 3% reduction in low birthweight, which suggests a larger effect than we find.

[13] For example, the estimates from Hoynes et al. (2015) suggest that a $1,000 income increase from the EITC are associated with a 2%-3% decline in low birth weight. Our estimates suggest 1% decline with $1,000 for married mothers. Similarly, the implied effect of a $1,000 income increase from the minimum wage on birth weight for married mothers is around 2% from Wehby et al. (2012) compared to 1% or less in our study. That study however finds little effect on low birth weight which is slightly smaller than what we find. Almond et al. (2011) reported that roll out of the original SNAP program (Food Stamps) was associated with small increase in birthweight and small decreases in low birthweight that is not too different from estimates reported above. The external validity of these results for current context is legitimately questionable given the vast changes in society that have occurred over the last 50 years. That said, estimates in Almond et al. (2011) are small and generally consistent with the arguments made above about the clinical importance of such effects.

infant health. Going forward, understanding how exposures to income support programs over a longer period than during pregnancy or shortly before that might affect infant health may offer new insights than focusing solely on the pregnancy period, especially in terms of potential effects on maternal preconception health.

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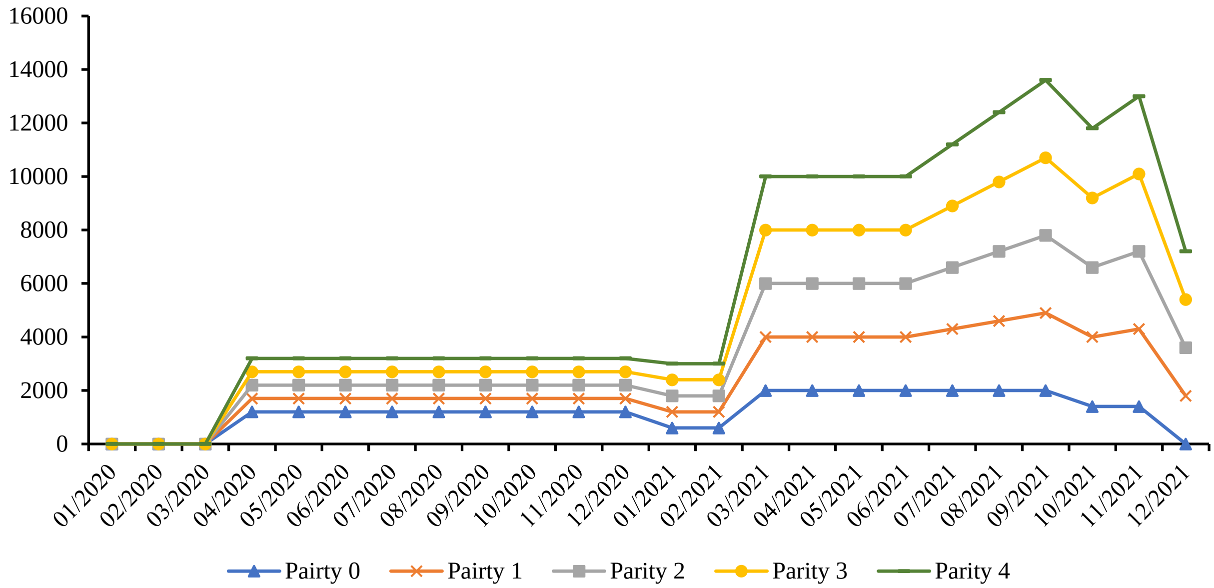
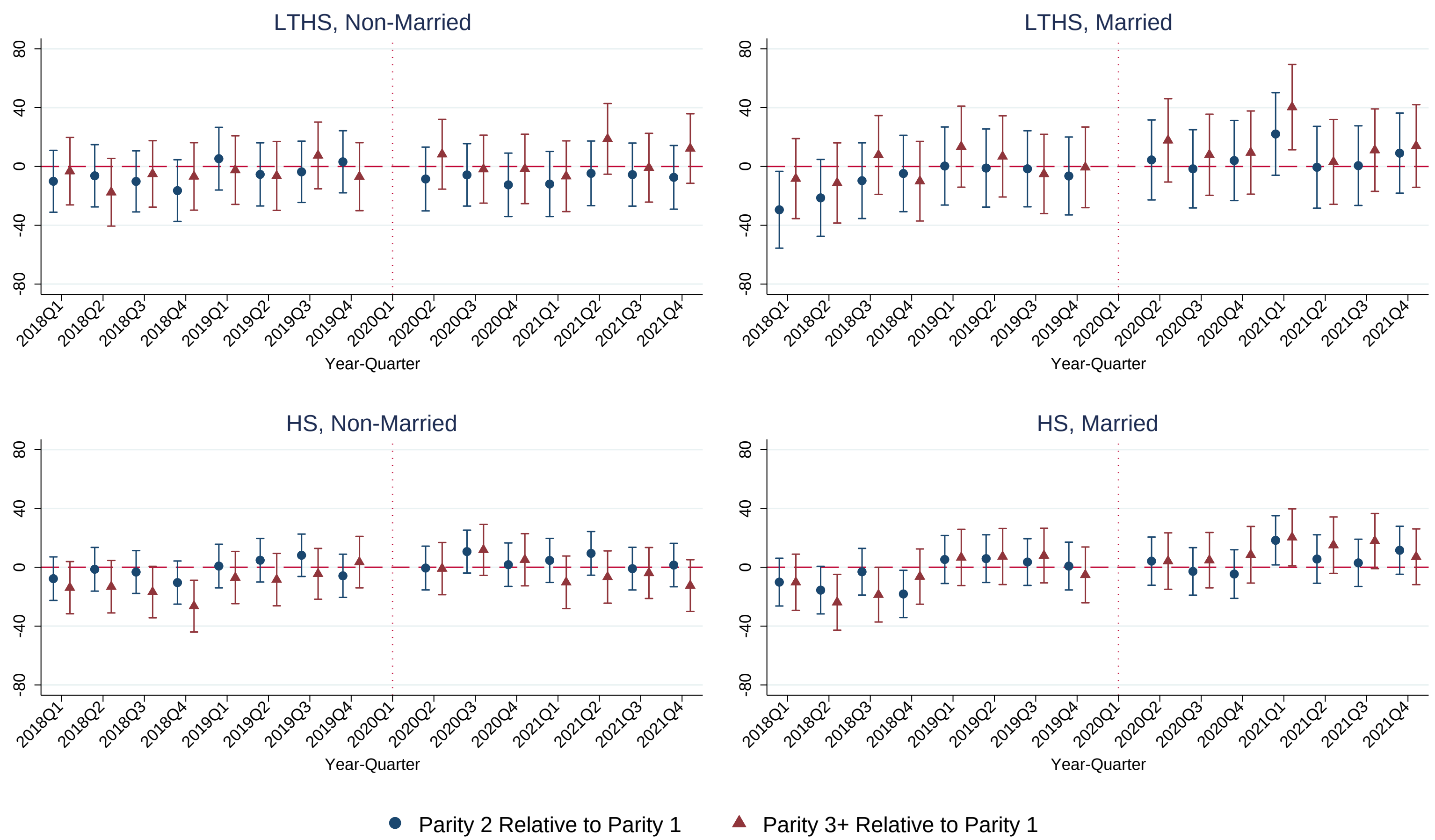
Figure 1. Total income received from stimulus checks and expanded child tax credit over the pregnancy period among non-married mothers by parity and year-month of birth



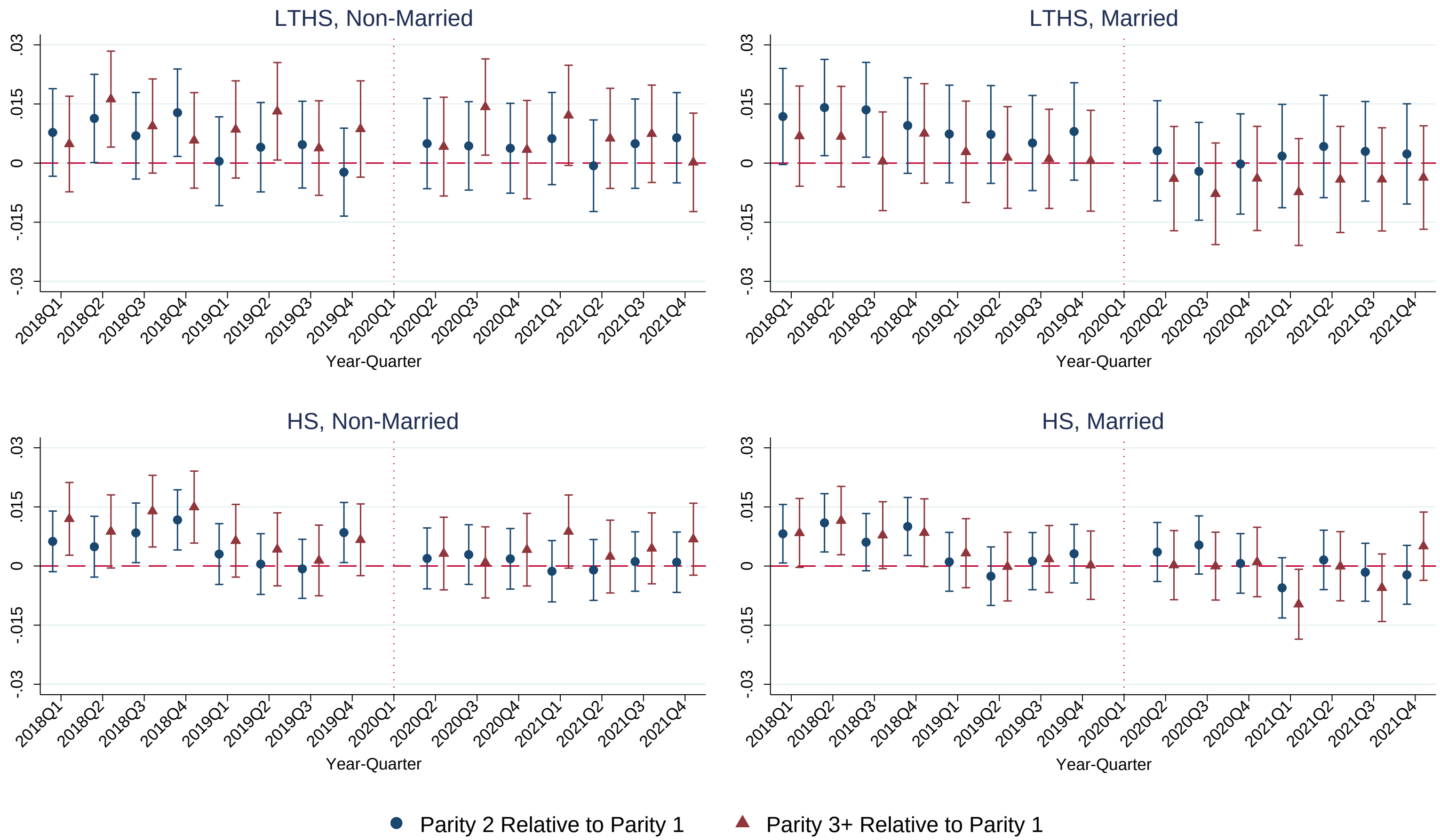
Notes: The expanded child tax credit is assiged at the maxiumu level of $300 per months from July 2021 to December 2021.

Figure 2. Event Study Estimates for Birth Weight in Grams



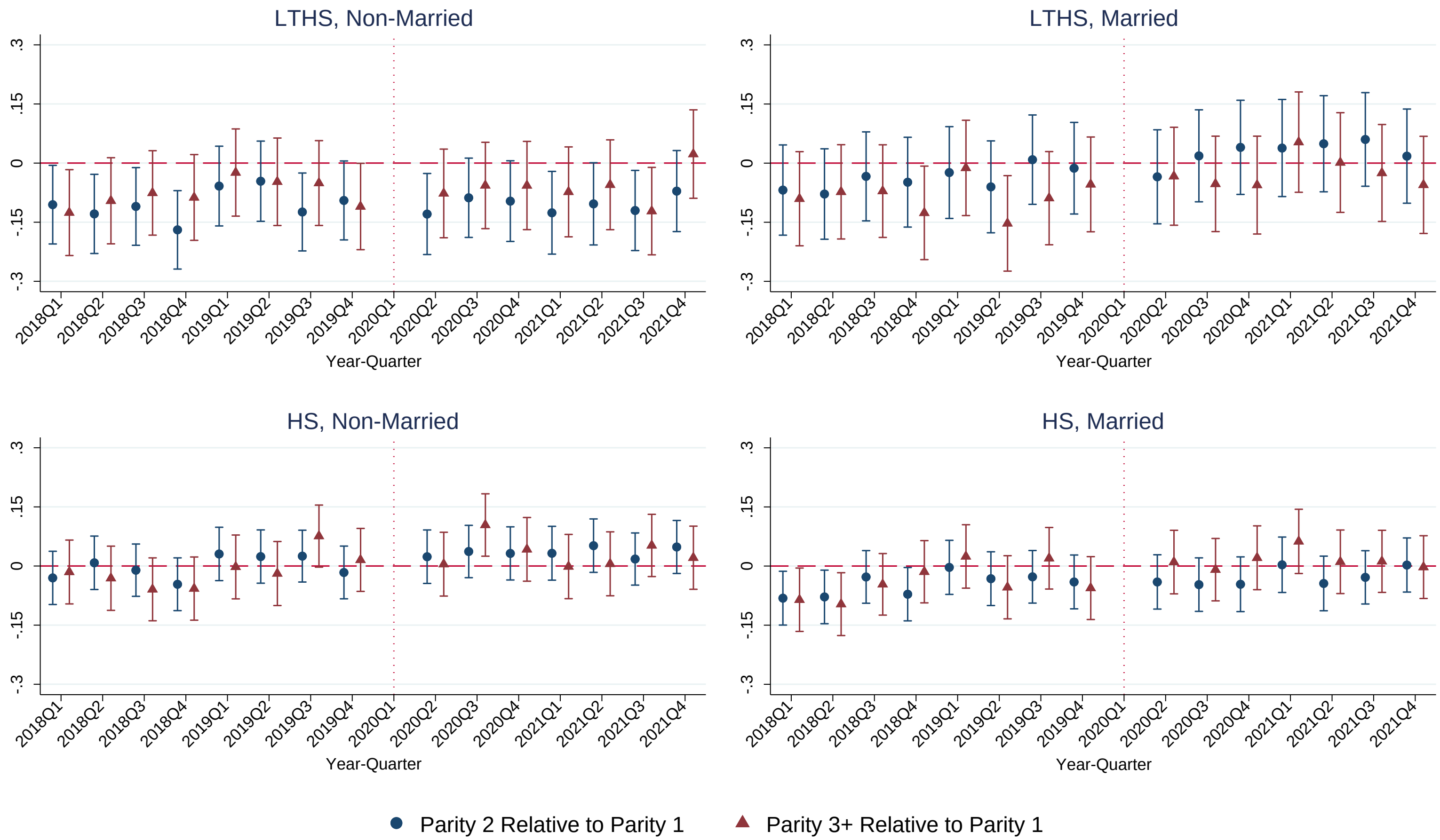
Notes: This graph shows event study estimates (dots) and the 95% confidence intervals (bars) for birth weight in grams. The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from January 2018 to December 2021. Estimates are obtained from an OLS regression of birth weight on a set of interaction terms between parity dummies and year-quarter dummies while controlling for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects and presented separately by four education-marital status groups. Depending on the sample, the number of observations varies from 365,918 to 1,166,689.

Figure 3. Event Study Estimates for Low Birth Weight



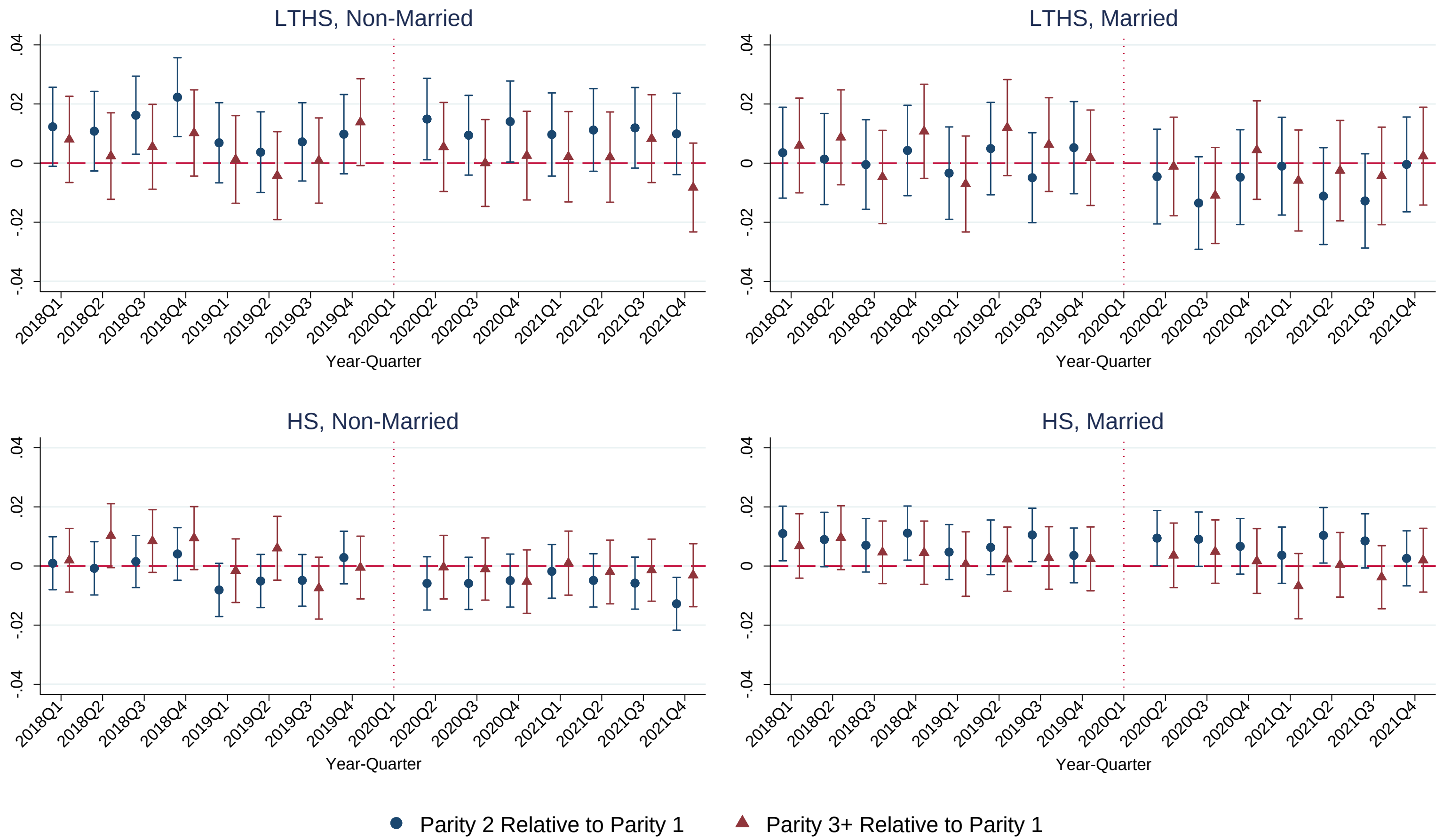
Notes: This graph shows event study estimates (dots) and the 95% confidence intervals (bars) for low birth weight (birth weight<2500 grams). The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from January 2018 to December 2021. Estimates are obtained from an OLS regression of birth weight on a set of interaction terms between parity dummies and year-quarter dummies while controlling for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects and presented separately by four education-marital status groups. Depending on the sample, the number of observations varies from 365,918 to 1,166,689.

Figure 4. Event Study Estimates for Gestational Age in Weeks



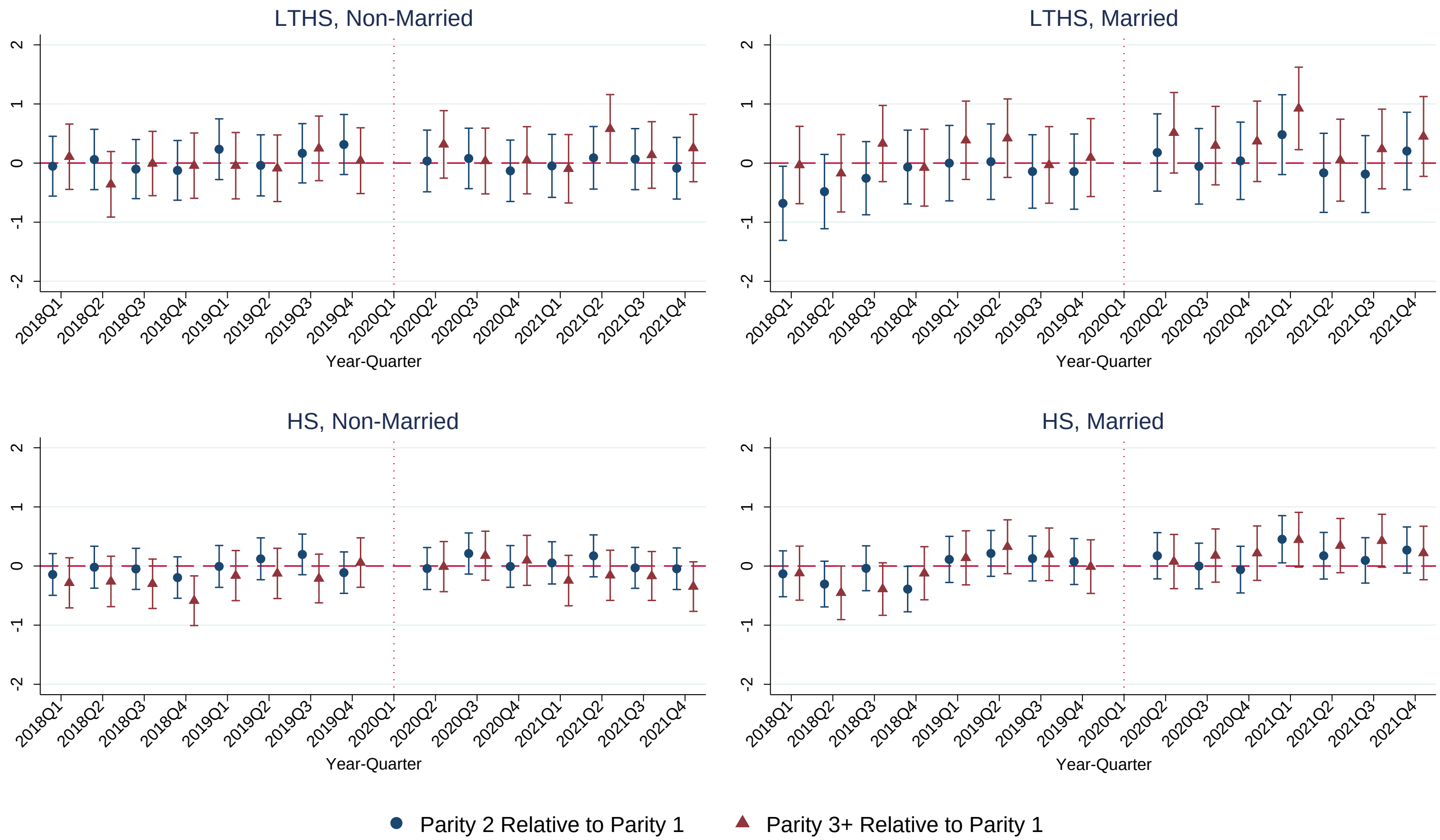
Notes: This graph shows event study estimates (dots) and the 95% confidence intervals (bars) for gestational age in weeks. The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from January 2018 to December 2021. Estimates are obtained from an OLS regression of birth weight on a set of interaction terms between parity dummies and year-quarter dummies while controlling for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects and presented separately by four education-marital status groups. Depending on the sample, the number of observations varies from 365,678 to 1,166,579.

Figure 5. Event Study Estimates on Preterm Birth



Notes: This graph shows event study estimates (dots) and the 95% confidence intervals (bars) for preterm birth. The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from January 2018 to December 2021. Estimates are obtained from an OLS regression of birth weight on a set of interaction terms between parity dummies and year-quarter dummies while controlling for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects and presented separately by four education-marital status groups. Depending on the sample, the number of observations varies from 365,678 to 1,166,579.

Figure 6. Event Study Estimates for Fetal Growth



Notes: This graph shows event study estimates (dots) and the 95% confidence intervals (bars) for fetal growth (grams/week). The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from January 2018 to December 2021. Estimates are obtained from an OLS regression of birth weight on a set of interaction terms between parity dummies and year-quarter dummies while controlling for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects and presented separately by four education-marital status groups. Depending on the sample, the number of observations varies from 365,361 to 1,165,848.

Table 1. Estimates of the Effect of $1000 of Income on Infant Health, Placebo Period April 2018-December 2019

	Non-Married		Married	
	Mean of Dep.	Estimates	Mean of Dep.	Estimates
Less than High School				
Birth Weight	3157.7	1.86	3268.9	0.81
		(1.36)		(1.59)
Low Birth Weight+	11.0	-0.053	7.7	-0.089
		(0.072)		(0.074)
Gestational Age	38.2	-0.00040	38.5	-0.0017
		(0.0064)		(0.0070)
Preterm Birth+	17.0	-0.013	13.0	0.064
		(0.087)		(0.093)
Fetal Growth	82.3	0.057	84.7	0.023
		(0.033)		(0.038)
High School				
Birth Weight	3166.5	2.47*	3291.1	3.79***
		(1.07)		(1.14)
Low Birth Weight+	11.0	-0.15**	7.4	-0.16**
		(0.055)		(0.052)
Gestational Age	38.2	0.015**	38.5	0.0055
		(0.0049)		(0.0048)
Preterm Birth+	16.0	-0.18**	12.0	-0.060
		(0.065)		(0.064)
Fetal Growth	82.5	0.041	85.2	0.094***
		(0.025)		(0.027)

Notes: The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from April 2018 to December 2019. Estimates are obtained from Equation (1) using OLS and representing the effects of $1000 from the cash transfers over the pregnancy period on infant health outcomes. The models control for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects. Estimates are presented separately by four education-marital status groups. Depending on the sample, the number of observations varies from 171,535 to 513,764.

*p<0.05, **p<0.01, ***p<0.001; + Mean, coefficients and standard errors are scaled by 100.

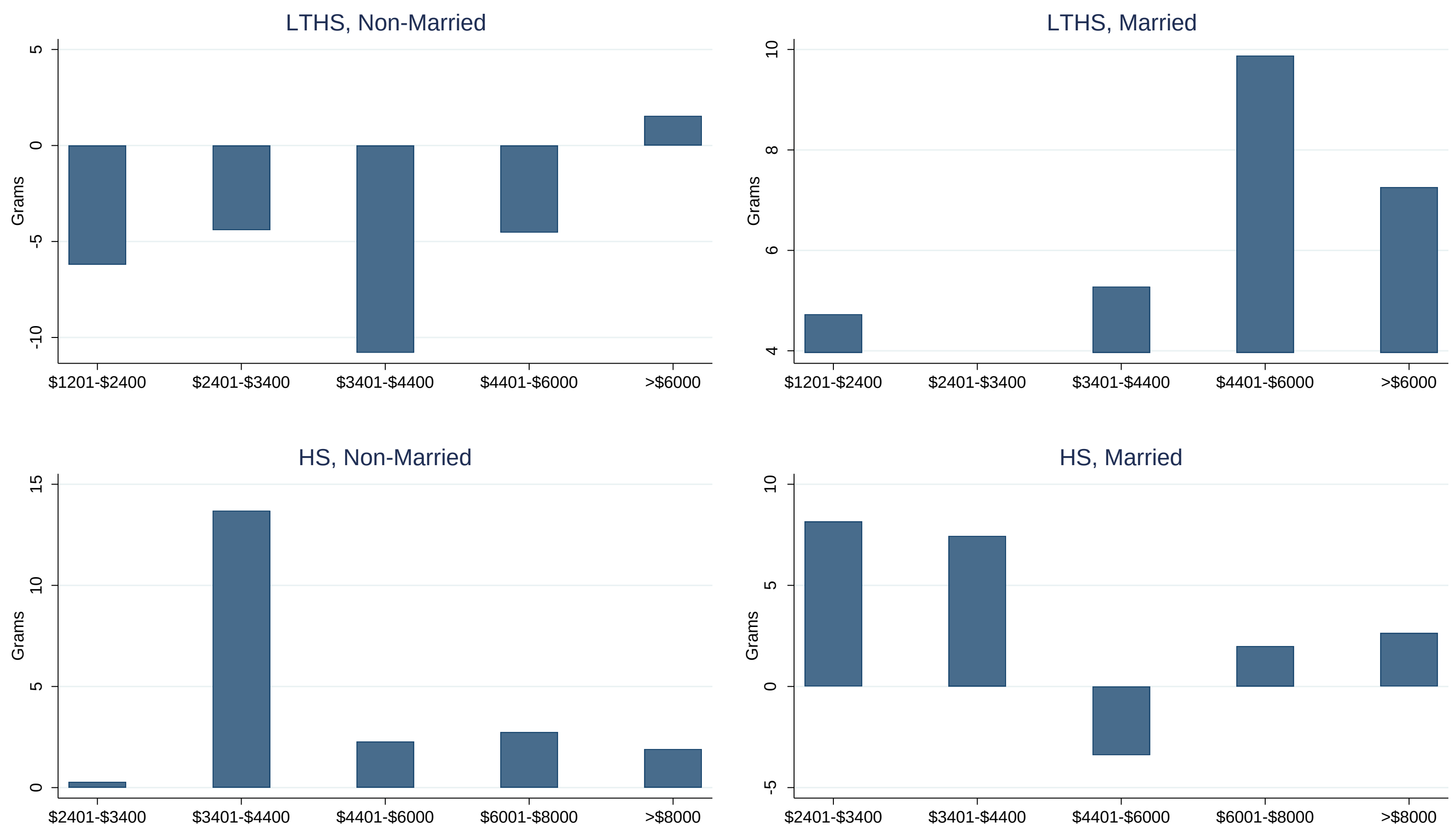
Table 2. Estimates of the Effect of $1000 of Income on Infant Health

	Non-Married					Married				
	Mean of Dep.	9-Months	12-Months	9-Months (0 parity)	9-Months (2018-21)	Mean of Dep.	9-Months	12-Months	9-Months (0 parity)	9-Months (2018-21)
Less than High School										
Birth Weight	3156	1.94	1.26	1.88	2.33*	3267	0.26	1.09	0.27	2.43*
		(1.45)	(1.20)	(1.05)	(0.96)		(1.70)	(1.39)	(1.30)	(1.12)
Low Birth Weight	11.0	-0.079	-0.072	-0.060	-0.055	7.6	0.0054	-0.017	-0.067	-0.14**
		(0.077)	(0.063)	(0.056)	(0.051)		(0.079)	(0.065)	(0.062)	(0.052)
Gestational Age	38.2	-0.0012	0.0042	0.0020	0.00095	38.5	0.0092	0.0063	0.013*	0.012*
		(0.0069)	(0.0057)	(0.0050)	(0.0046)		(0.0074)	(0.0061)	(0.0058)	(0.0049)
Preterm Birth+	17.0	-0.019	-0.077	-0.0073	-0.040	13.0	-0.071	-0.027	-0.12	-0.15*
		(0.092)	(0.076)	(0.066)	(0.061)		(0.10)	(0.082)	(0.076)	(0.066)
Fetal Growth	82.4	0.054	0.025	0.048	0.057*	84.6	-0.015	0.015	-0.019	0.036
		(0.035)	(0.029)	(0.025)	(0.023)		(0.041)	(0.033)	(0.031)	(0.027)
High School										
Birth Weight	3163	-1.85	-1.40	-0.35	0.56	3289	1.48	1.06	-0.21	3.41**
		(1.08)	(0.89)	(0.72)	(0.72)		(1.16)	(0.96)	(0.82)	(0.77)
Low Birth Weight+	11.2	0.023	0.018	0.012	-0.055	7.6	-0.039	-0.017	-0.014	-0.11**
		(0.056)	(0.046)	(0.037)	(0.037)		(0.053)	(0.044)	(0.038)	(0.035)
Gestational Age	38.2	-0.00084	-0.00054	0.0030	0.0079*	38.5	-0.0016	-0.000088	-0.0015	0.0076*
		(0.0049)	(0.0040)	(0.0033)	(0.0033)		(0.0049)	(0.0040)	(0.0035)	(0.0032)
Preterm Birth+	16.1	-0.016	-0.032	-0.055	-0.097*	12.5	-0.069	-0.063	-0.055	-0.093*
		(0.065)	(0.054)	(0.042)	(0.044)		(0.066)	(0.055)	(0.046)	(0.044)
Fetal Growth	82.5	-0.049	-0.038	-0.015	0.000085	85.2	0.039	0.027	-0.0043	0.073**
		(0.026)	(0.021)	(0.017)	(0.017)		(0.028)	(0.023)	(0.020)	(0.018)

Notes: The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from April 2020 to December 2021. Estimates are obtained using OLS from Equation (1) and representing the effects of $1000 from the cash transfers over the pregnancy period (labeled as "9-Month") or a year before birth (labeled as "12-Month") on infant health outcomes. The models control for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects. Estimates are presented separately by four education-marital status groups. Estimates are presented separately by four education-marital status groups. Depending on the sample, the number of observations ranges from 147,007 to 1,166,689.

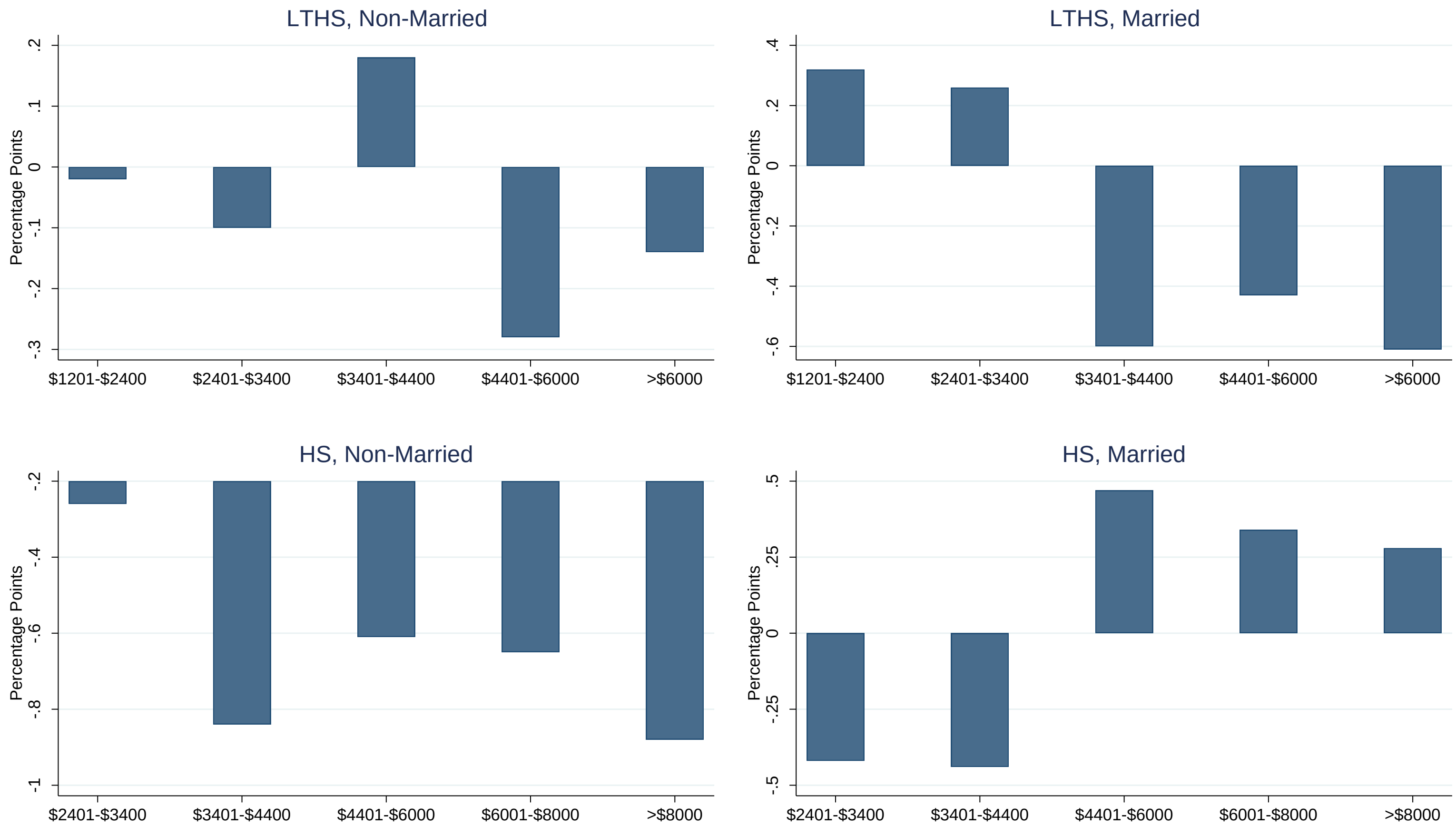
*p<0.05, **p<0.01, ***p<0.001; + Mean, coefficients and standard errors are scaled by 100.

Figure 7. Estimates of the Effect of Income Categories on Birth Weight in Grams



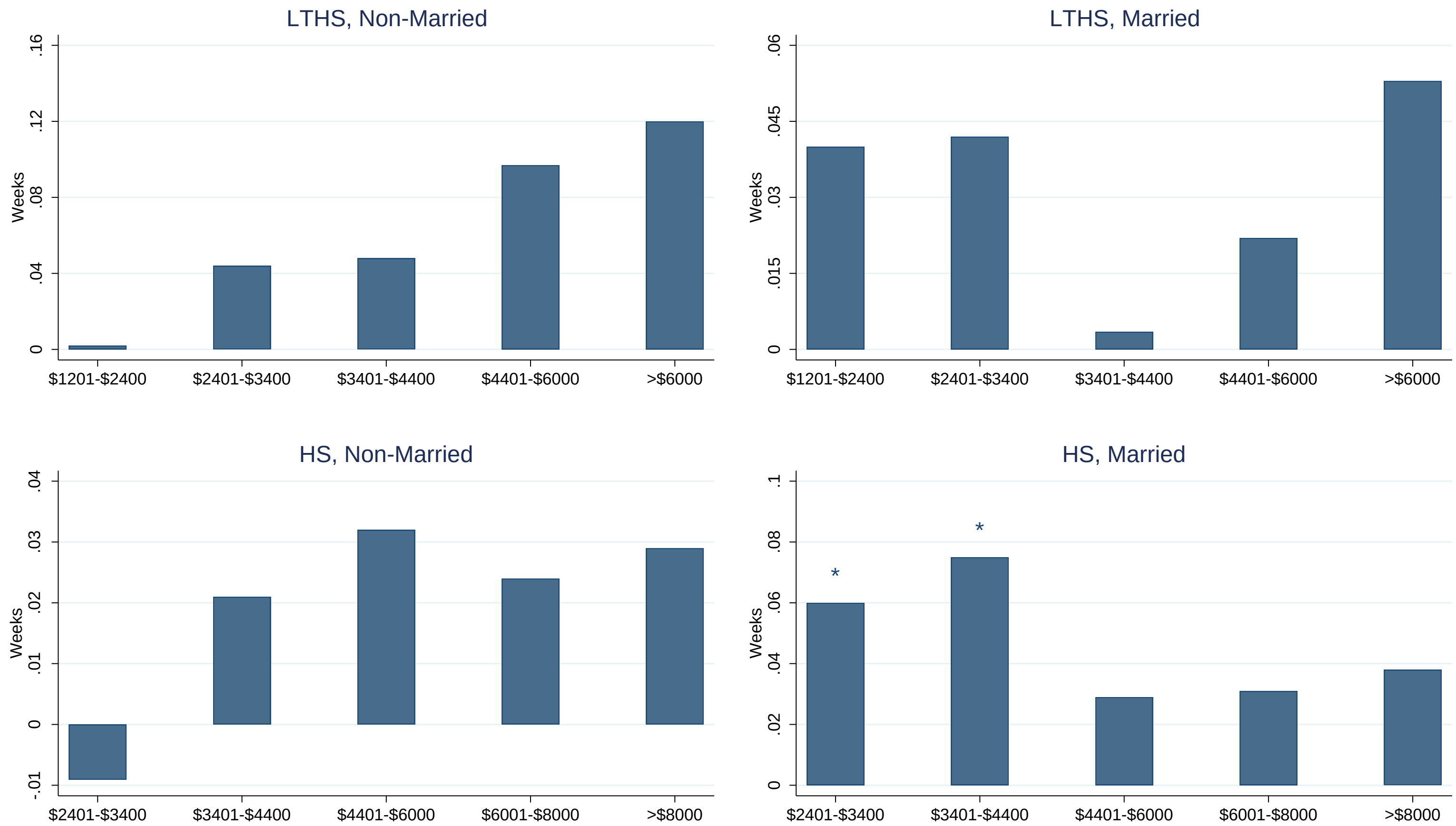
Notes: This graph shows the estimates (bars) of the effects of income categories on birth weight in grams. The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from April 2020 to December 2021. Estimates are obtained using OLS from Equation (1) with replacing continuous income measure with income categories. The models control for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects. Estimates are presented separately by four education-marital status groups. Depending on the sample, the number of observations ranges from 147,007 to 509,685.

Figure 8. Estimates of the Effect of Income Categories on Low Birth Weight



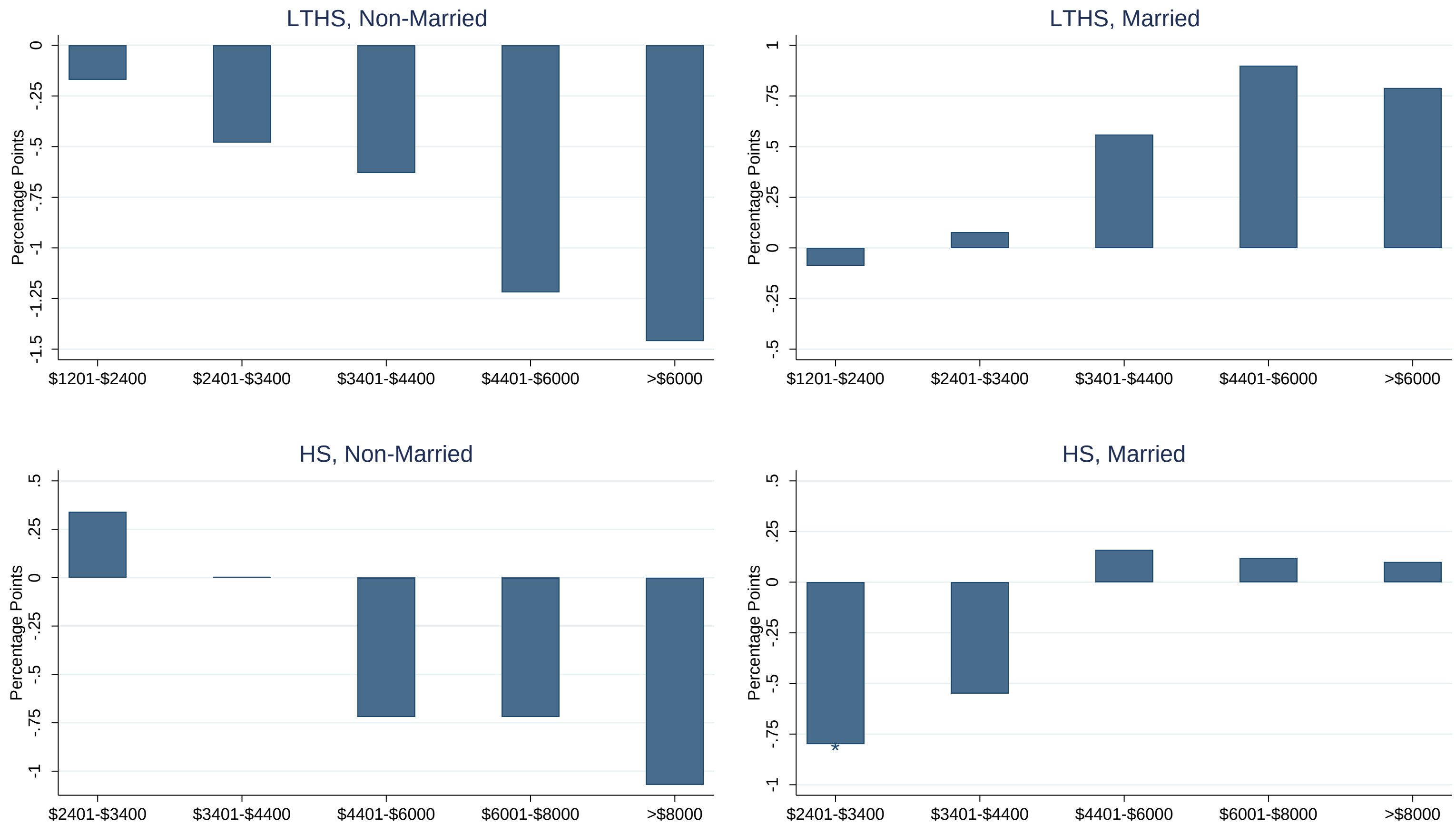
Notes: This graph shows the estimates (bars) of the effects of income categories on low birth weight. The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from April 2020 to December 2021. Estimates are obtained using OLS from Equation (1) with replacing continuous income measure with income categories. The models control for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects. Estimates are presented separately by four education-marital status groups. Depending on the sample, the number of observations ranges from 147,007 to 509,685.

Figure 9. Estimates of the Effect of Income Categories on Gestational Age



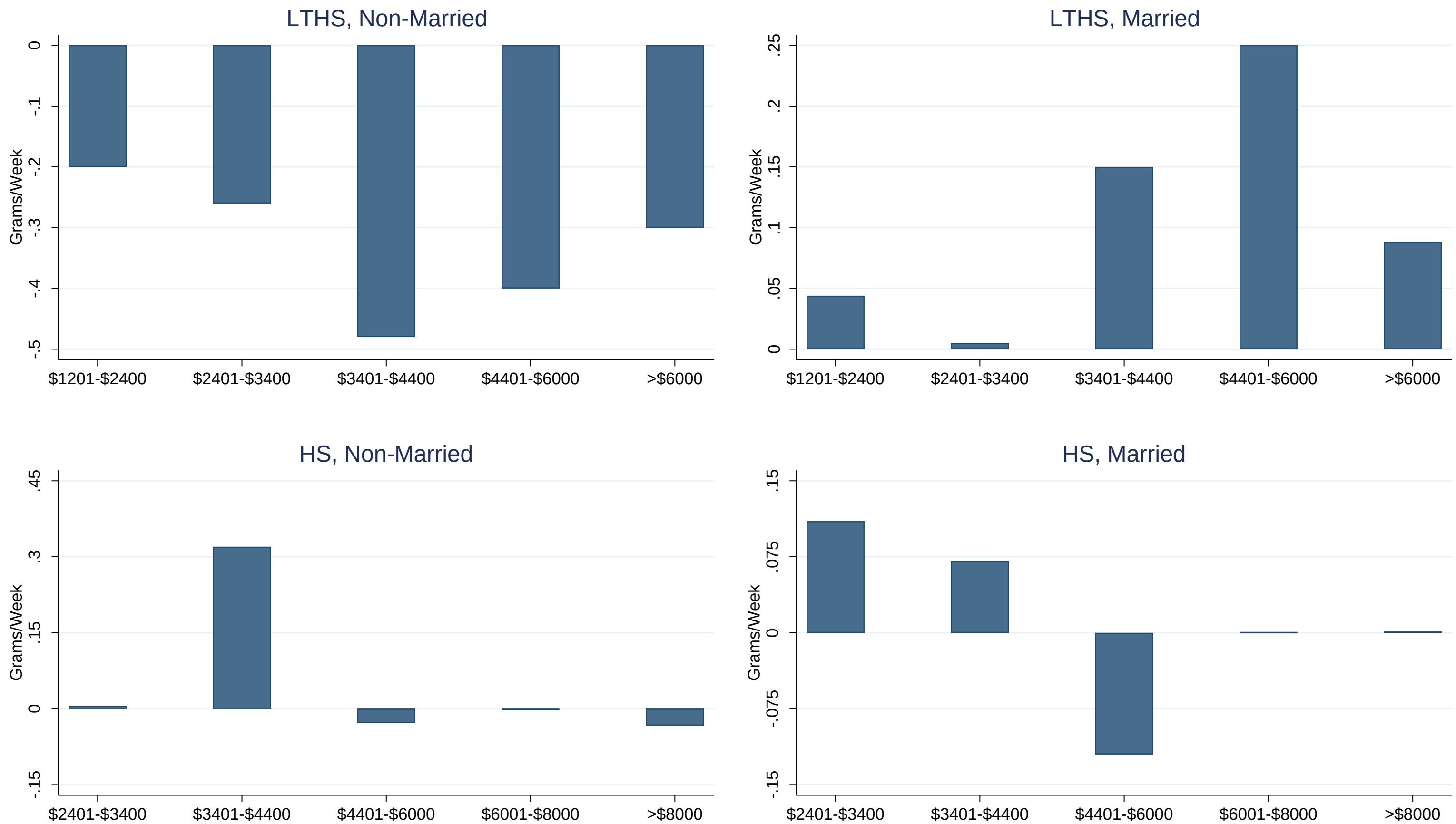
Notes: This graph shows the estimates (bars) of the effects of income categories on gestational age in weeks. The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from April 2020 to December 2021. Estimates are obtained using OLS from Equation (1) with replacing continuous income measure with income categories. The models control for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects. Estimates are presented separately by four education-marital status groups. Depending on the sample, the number of observations varies from 147,030 to 509,585. * indicates the coefficients are significant at $p<0.05$.

Figure 10. Estimates of the Effect of Income Categories on Preterm Birth



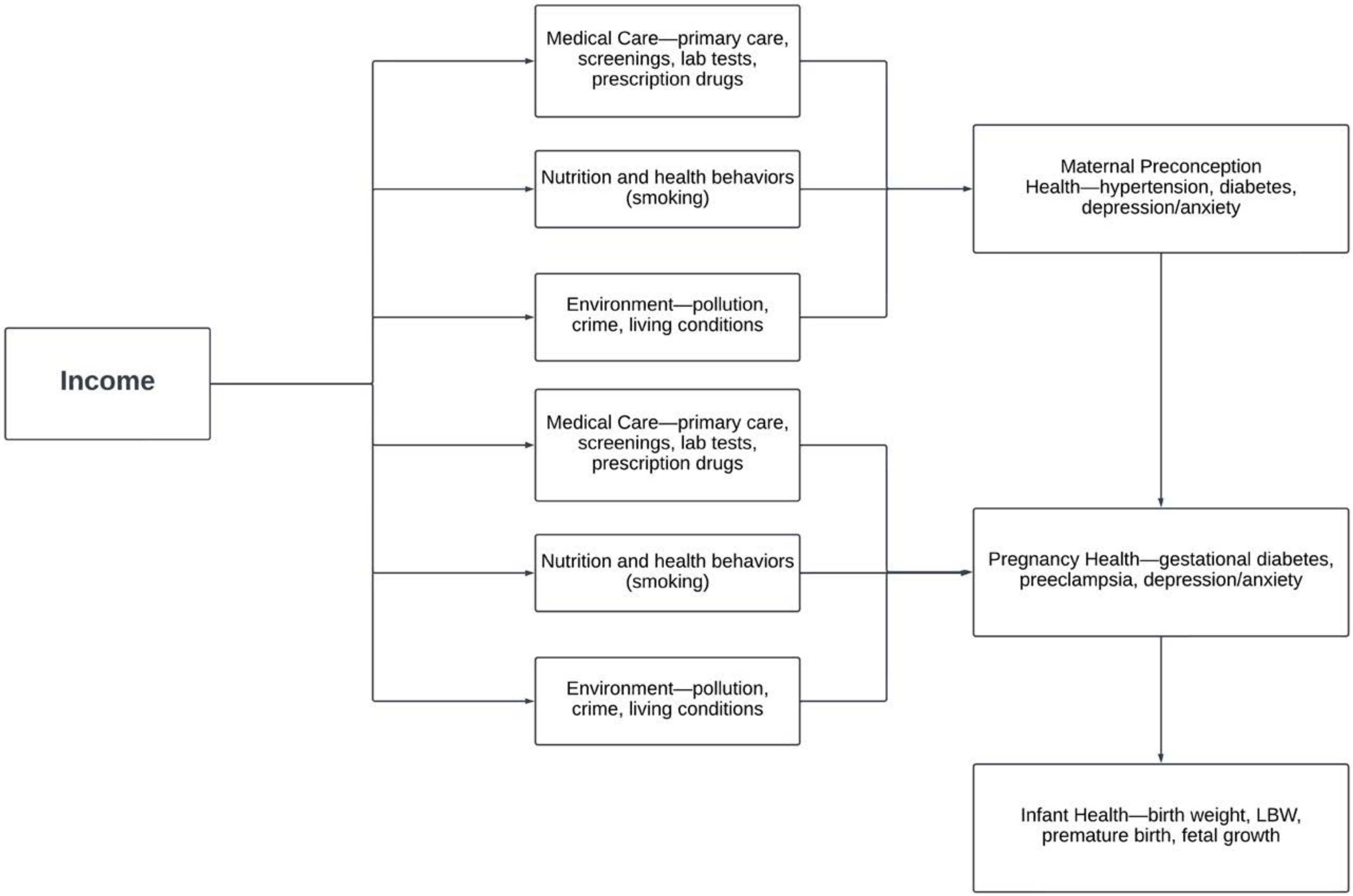
Notes: This graph shows the estimates (bars) of the effects of income categories on preterm birth. The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from April 2020 to December 2021. Estimates are obtained using OLS from Equation (1) with replacing continuous income measure with income categories. The models control for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects. Estimates are presented separately by four education-marital status groups. Depending on the sample, the number of observations varies from 147,030 to 509,585.

Figure 11. Estimates of the Effect of Income Categories on Fetal Growth



Notes: This graph shows the estimates (bars) of the effects of income categories on fetal growth (grams/week). The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from April 2020 to December 2021. Estimates are obtained using OLS from Equation (1) with replacing continuous income measure with income categories. The models control for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects. Estimates are presented separately by four education-marital status groups. Depending on the sample, the number of observations varies from 146,910 to 509,309.

Appendix Figure 1. Mechanisms linking family income and infant health



Appendix Table 1. Descriptive Statistics, April 2020-December 2021

	Less than High School				High School			
	Parity 0	Parity 1	Parity 2	Parity 3+	Parity 0	Parity 1	Parity 2	Parity 3+
NON-MARRIED SAMPLE								
Outcomes								
Birth Weight	3115.4	3159.0	3165.0	3142.8	3200.9	3247.3	3273.8	3277.8
	(1.77)	(1.87)	(2.18)	(2.45)	(3.15)	(2.59)	(2.54)	(2.60)
Low Birth Weight (%)	11.15	10.39	10.50	12.43	8.43	7.72	7.19	7.83
Gestational Age	38.6	38.3	38.2	38.0	38.8	38.6	38.6	38.4
	(0.0084)	(0.0087)	(0.010)	(0.011)	(0.014)	(0.011)	(0.011)	(0.011)
Preterm Birth (%)	11.15	10.39	10.50	12.43	8.43	7.72	7.19	7.83
Fetal Growth	80.5	82.2	82.6	82.4	82.3	83.9	84.8	85.1
	(0.042)	(0.045)	(0.053)	(0.059)	(0.075)	(0.062)	(0.061)	(0.062)
SC and CTC Income ($1000)	1.37	2.74	4.11	5.92	2.74	4.10	5.47	7.34
	(0.0017)	(0.0042)	(0.0082)	(0.014)	(0.0060)	(0.0081)	(0.012)	(0.017)
Maternal Age	21.7	24.6	27.4	29.7	25.1	27.0	29.3	31.2
	(0.013)	(0.016)	(0.019)	(0.019)	(0.029)	(0.023)	(0.022)	(0.020)
Race/Ethnicity (%)								
Non-Hispanic White	34.89	31.03	27.49	24.21	45.87	37.30	32.34	35.19
Non-Hispanic Black	17.16	17.09	17.84	21.77	6.27	6.02	6.53	8.72
Non-Hispanic Other Races	6.77	5.89	5.26	5.79	11.48	11.34	9.27	7.31
Hispanic	41.19	45.98	49.41	48.23	36.38	45.34	51.86	48.77
Health Insurance (%)								
Medicaid	79.64	81.32	81.11	82.02	53.42	62.42	65.88	65.87
Private Insurance	10.78	7.52	6.32	5.36	20.34	16.24	13.96	10.95
Self Pay	6.65	8.19	9.53	9.40	21.05	16.86	16.27	19.47
Other	2.28	2.31	2.40	2.51	4.41	3.66	3.14	3.00
Unknown	0.65	0.66	0.65	0.71	0.79	0.82	0.74	0.71
N	105743	98787	72574	64477	31827	47570	48512	51016
MARRIED SAMPLE								
Outcomes								
Birth Weight	3143.9	3177.6	3169.2	3116.2	3230.6	3291.6	3294.2	3273.8
	(0.97)	(1.18)	(1.58)	(2.03)	(1.50)	(1.39)	(1.69)	(2.13)
Low Birth Weight (%)	10.52	10.62	10.89	13.43	8.13	7.37	7.36	8.63
Gestation Weeks	38.6	38.3	38.2	37.9	38.8	38.6	38.5	38.4
	(0.0045)	(0.0053)	(0.0070)	(0.0093)	(0.0065)	(0.0058)	(0.0071)	(0.0089)
Preterm Birth (%)	12.40	14.71	16.06	19.92	10.52	11.36	12.64	14.82
Fetal Growth	81.1	82.6	82.7	81.8	83.1	85.1	85.4	85.1
	(0.023)	(0.028)	(0.038)	(0.048)	(0.035)	(0.033)	(0.040)	(0.051)
SC and CTC Income ($1000)	1.38	2.77	4.14	5.93	2.75	4.15	5.54	7.34
	(0.00089)	(0.0026)	(0.0058)	(0.011)	(0.0028)	(0.0043)	(0.0077)	(0.014)
Maternal Age	22.8	25.6	27.9	29.7	25.9	27.6	29.3	30.9
	(0.0068)	(0.0087)	(0.012)	(0.014)	(0.013)	(0.011)	(0.013)	(0.015)
Race/Ethnicity (%)								
Non-Hispanic White	40.74	39.33	36.57	32.02	58.70	56.31	51.96	50.97
Non-Hispanic Black	26.60	28.48	31.38	37.10	7.56	8.15	10.39	13.45
Non-Hispanic Other Races	5.76	5.60	5.55	6.28	7.82	7.68	6.61	6.20
Hispanic	26.90	26.60	26.51	24.61	25.92	27.86	31.04	29.38
Health Insurance (%)								
Medicaid	70.51	76.80	80.70	83.52	37.43	45.39	53.98	61.63
Private Insurance	23.96	17.94	13.82	10.52	49.72	44.06	35.78	28.09
Self Pay	2.84	2.99	3.19	3.49	4.93	4.86	5.55	6.26
Other	2.09	1.76	1.78	1.86	6.60	4.75	3.79	3.26
Unknown	0.60	0.53	0.51	0.61	1.31	0.94	0.89	0.75
N	370657	263754	147956	97659	147243	171315	116840	79662

Notes: Standard deviations in parentheses are for continuous variables.

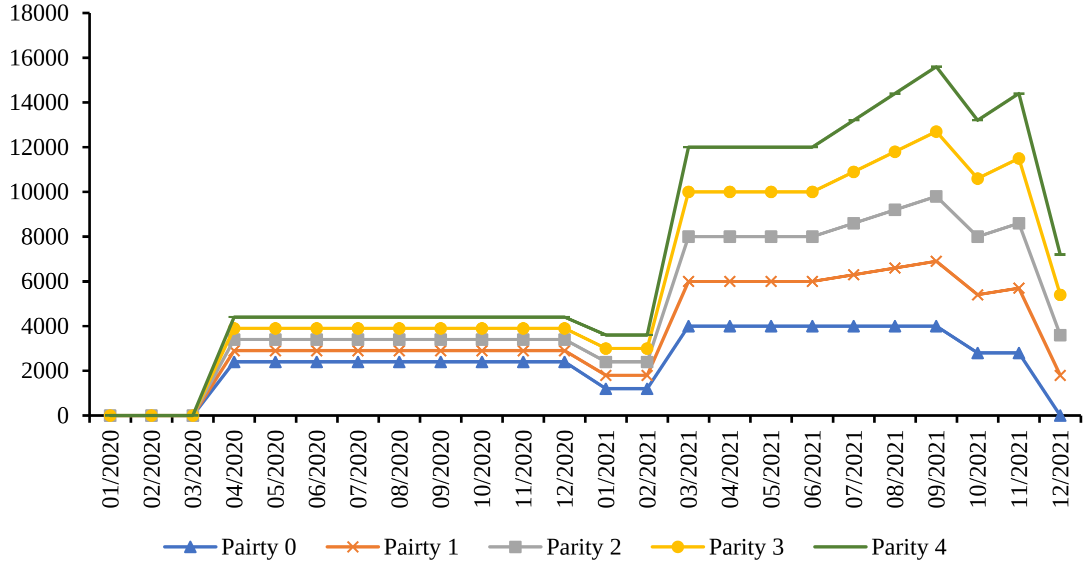
Appendix Table 2. Cash Transfers and Related Programs by Birth Cohort

	9-Months Period of Pregnancy								Total Cash Received over 9-Months	
Month/Year of Birth (T)	**T-1**	**T-2**	**T-3**	**T-4**	**T-5**	**T-6**	**T-7**	**T-8**	**Per Adult**	**Per Child**
04/2020	03/2020	02/2020	01/2020	12/2019	11/2019	10/2019	09/2019	08/2019	$1200	$500
05/2020	04/2020	03/2020	02/2020	01/2020	12/2019	11/2019	10/2019	09/2019	$1200	$500
06/2020	05/2020	04/2020	03/2020	02/2020	01/2020	12/2019	11/2019	10/2019	$1200	$500
07/2020	06/2020	05/2020	04/2020	03/2020	02/2020	01/2020	12/2019	11/2019	$1200	$500
08/2020	07/2020	06/2020	05/2020	04/2020	03/2020	02/2020	01/2020	12/2019	$1200	$500
09/2020	08/2020	07/2020	06/2020	05/2020	04/2020	03/2020	02/2020	01/2020	$1200	$500
10/2020	09/2020	08/2020	07/2020	06/2020	05/2020	04/2020	03/2020	02/2020	$1200	$500
11/2020	10/2020	09/2020	08/2020	07/2020	06/2020	05/2020	04/2020	03/2020	$1200	$500
12/2020	11/2020	10/2020	09/2020	08/2020	07/2020	06/2020	05/2020	04/2020	$1200	$500
01/2021	12/2020	11/2020	10/2020	09/2020	08/2020	07/2020	06/2020	05/2020	$600	$600
02/2021	01/2021	12/2020	11/2020	10/2020	09/2020	08/2020	07/2020	06/2020	$600	$600
03/2021	02/2021	01/2021	12/2020	11/2020	10/2020	09/2020	08/2020	07/2020	$2000	$2000
04/2021	03/2021	02/2021	01/2021	12/2020	11/2020	10/2020	09/2020	08/2020	$2000	$2000
05/2021	04/2021	03/2021	02/2021	01/2021	12/2020	11/2020	10/2020	09/2020	$2000	$2000
06/2021	05/2021	04/2021	03/2021	02/2021	01/2021	12/2020	11/2020	10/2020	$2000	$2000
07/2021	06/2021	05/2021	04/2021	03/2021	02/2021	01/2021	12/2020	11/2020	$2000	$2300
08/2021	07/2021	06/2021	05/2021	04/2021	03/2021	02/2021	01/2021	12/2020	$2000	$2600
09/2021	08/2021	07/2021	06/2021	05/2021	04/2021	03/2021	02/2021	01/2021	$2000	$2900
10/2021	09/2021	08/2021	07/2021	06/2021	05/2021	04/2021	03/2021	02/2021	$1400	$2600
11/2021	10/2021	09/2021	08/2021	07/2021	06/2021	05/2021	04/2021	03/2021	$1400	$2900
12/2021	11/2021	10/2021	09/2021	08/2021	07/2021	06/2021	05/2021	04/2021	$0	$1800

Notes: The shaded cells indicate the cash transfers a mother can receive over a 9-month period since her birth (including birth month).

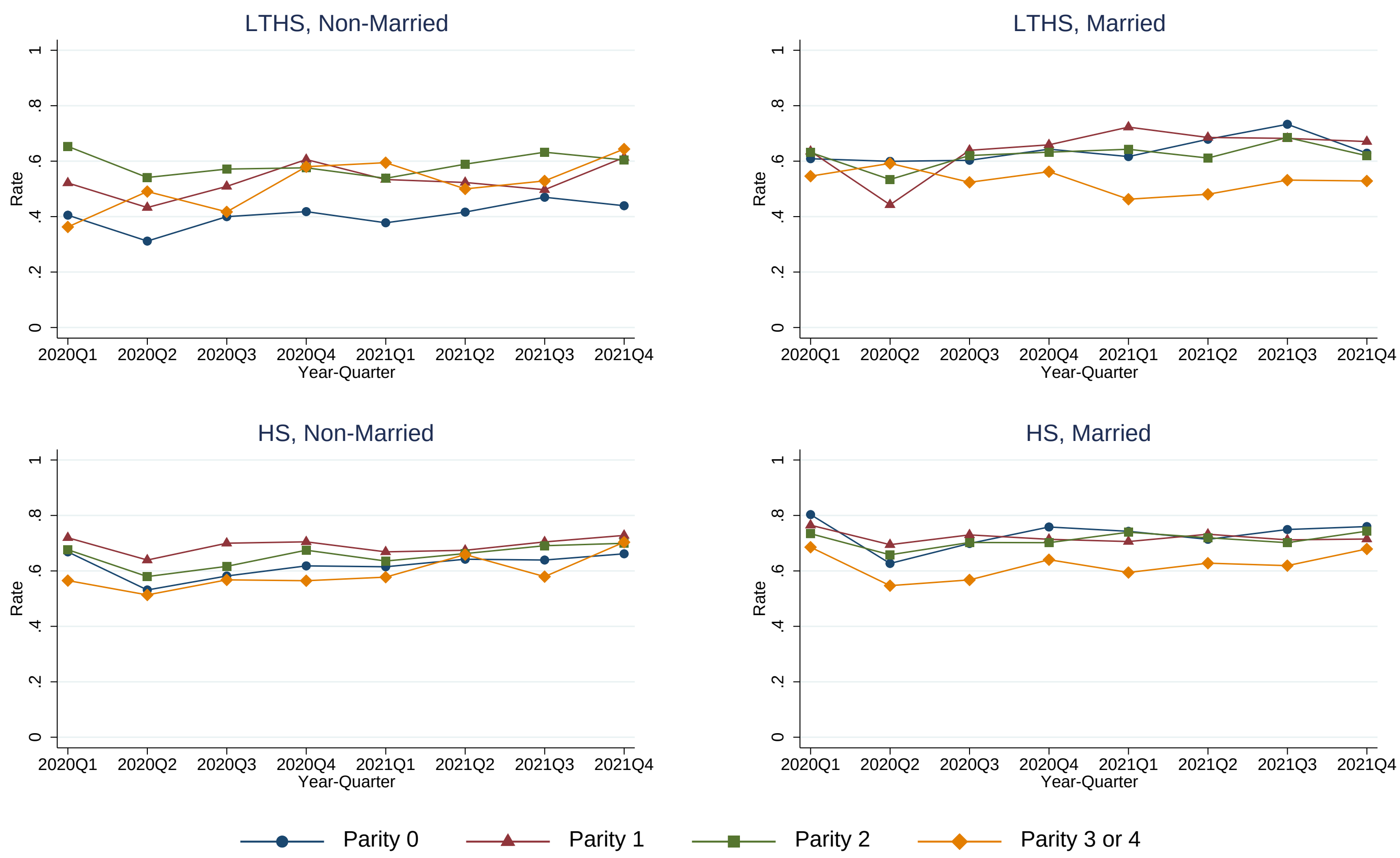
First stimulus check: issued on March 27th, 2020; $1200 per adult and $500 per child; assumed to be received in April 2020.
Second stimulus check: issued on December 27th, 2020; $600 per adult and $600 per child; assumed to be received in Jan 2021.
Third stimulus check: issued on March 11th, 2021; $1,400 per adult and $1,400 per child; assumed to be received in March 2021.
Expanded child tax credit: advanced monthly payment of maximum $300 per child from July 2021-December 2021.

Appendix Figure 2. Total Income Received from Stimulus Checks and Expanded Child Tax Credit Over the Pregnancy Period among Married Mothers by Parity and Year-Month of Birth



Notes: The expanded child tax credit is assiged at the maxiumu level of $300 per months from July 2021 to December 2021.

Appendix Figure 3. Employment Rate by Parity-Education-Marital Status Groups Between 2018 And 2021 Among Women Aged 18-39 Years, 2018-2021Current Population Survey



Notes: The quarterly employment rate by parity-education-martial status groups are obtained using the monthly data from Current Population Survey from 2018 to 2021.

Appendix Table 3. Estimates of the Effect of $1000 of Income on Infant Health based on Different Trimesters

	Non-Married				Married			
	Mean of Dep.	1st Trimester	2nd Trimester	3rd Trimester	Mean of Dep.	1st Trimester	2nd Trimester	3rd Trimester
Less than High School								
Birth Weight	3156.1	1.53	1.72	-0.25	3266.6	-2.40	-8.04**	6.04*
		(2.45)	(2.55)	(2.19)		(2.87)	(3.01)	(2.58)
Low Birth Weight+	11.0	-0.19	0.057	-0.0080	7.6	0.15	0.047	-0.12
		(0.13)	(0.13)	(0.12)		(0.13)	(0.14)	(0.12)
Gestational Age	38.2	-0.0037	-0.015	0.00029	38.5	-0.010	-0.0085	0.024*
		(0.012)	(0.012)	(0.010)		(0.013)	(0.013)	(0.011)
Preterm Birth+	17.0	0.12	0.24	-0.17	13.0	0.076	-0.047	-0.16
		(0.16)	(0.16)	(0.14)		(0.17)	(0.18)	(0.15)
Fetal Growth	82.4	0.050	0.076	-0.011	84.6	-0.048	-0.20**	0.11
		(0.059)	(0.061)	(0.053)		(0.069)	(0.072)	(0.062)
High School								
Birth Weight	3163.2	-0.84	-1.75	-2.17	3288.5	1.52	-0.100	1.29
		(1.82)	(1.89)	(1.62)		(1.98)	(2.04)	(1.76)
Low Birth Weight+	11.2	0.0036	0.017	0.082	7.6	-0.030	0.048	-0.12
		(0.095)	(0.098)	(0.084)		(0.091)	(0.094)	(0.081)
Gestational Age	38.2	0.0065	0.0075	-0.012	38.5	0.0037	-0.010	0.0012
		(0.0083)	(0.0086)	(0.0073)		(0.0083)	(0.0086)	(0.0074)
Preterm Birth+	16.1	-0.045	-0.056	0.062	12.5	-0.082	0.14	-0.16
		(0.11)	(0.11)	(0.098)		(0.11)	(0.12)	(0.10)
Fetal Growth	82.5	-0.028	-0.070	-0.039	85.2	0.035	0.014	0.027
		(0.043)	(0.045)	(0.039)		(0.047)	(0.049)	(0.042)

Notes: The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from April 2020 to December 2021. Estimates are obtained using OLS from Equation (1) with replacing continuous income measure with three separated variables for income received in each trimester and representing the effects of $1000 from the cash transfers on infant health outcomes. The models control for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects. Estimates are presented separately by four education-marital status groups. Estimates are presented separately by four education-marital status groups. Depending on the sample, the number of observations ranges from 146,910 to 509,585.

*p<0.05, **p<0.01, ***p<0.001; + Mean, coefficients and standard errors are scaled by 100.

Appendix Table 4. Estimates of the Effect of $1000 of Income on Infant Health using Alternative Specifications

	Non-Married				Married			
	Mean of Dep.	Main Estimates	Excluding demographic -by-time fixed effects	Adding county-by-time fixed effects	Mean of Dep.	Main Estimates	Excluding demographic -by-time fixed effects	Adding county-by-time fixed effects
Less than High School								
Birth Weight	3156	1.94	1.31	2.37	3267	0.26	-0.18	1.74
		(1.45)	(1.32)	(1.58)		(1.70)	(1.57)	(1.91)
Low Birth Weight	11.0	-0.079	-0.032	-0.11	7.6	0.0054	0.023	0.026
		(0.077)	(0.070)	(0.083)		(0.079)	(0.073)	(0.089)
Gestational Age	38.2	-0.0012	-0.00029	-0.0013	38.5	0.0092	0.0070	0.020*
		(0.0069)	(0.0062)	(0.0075)		(0.0074)	(0.0069)	(0.0084)
Preterm Birth+	17.0	-0.019	-0.055	-0.0047	13.0	-0.071	-0.033	-0.21
		(0.092)	(0.084)	(0.10)		(0.10)	(0.093)	(0.11)
Fetal Growth	82.4	0.054	0.033	0.066	84.6	-0.015	-0.023	0.0027
		(0.035)	(0.032)	(0.038)		(0.041)	(0.038)	(0.046)
High School								
Birth Weight	3163	-1.85	-1.35	-1.77	3289	1.48	1.32	0.92
		(1.08)	(1.00)	(1.13)		(1.16)	(1.11)	(1.25)
Low Birth Weight+	11.2	0.023	0.0085	0.058	7.6	-0.039	-0.042	-0.012
		(0.056)	(0.052)	(0.059)		(0.053)	(0.051)	(0.057)
Gestational Age	38.2	-0.00084	-0.0016	-0.0011	38.5	-0.0016	-0.0018	-0.0021
		(0.0049)	(0.0045)	(0.0051)		(0.0049)	(0.0047)	(0.0053)
Preterm Birth+	16.1	-0.016	-0.012	-0.0010	12.5	-0.069	-0.063	-0.035
		(0.065)	(0.061)	(0.069)		(0.066)	(0.063)	(0.071)
Fetal Growth	82.5	-0.049	-0.035	-0.047	85.2	0.039	0.038	0.022
		(0.026)	(0.024)	(0.027)		(0.028)	(0.027)	(0.030)

Notes: The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from April 2020 to December 2021. Estimates are obtained using OLS from Equation (1) and representing the effects of $1000 from the cash transfers over the pregnancy period on infant health outcomes on infant health outcomes. The models control for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects. Estimates are presented separately by four education-marital status groups. Estimates are presented separately by four education-marital status groups. Depending on the sample, the number of observations varies from 147,007 to 509,685.

*p<0.05, **p<0.01, ***p<0.001; + Mean, coefficients and standard errors are scaled by 100.

Appendix Table 5. Estimates of the Effect of $1000 of Income on Prenatal Care Visits and Weight Gain, April 2020-December 2021

	Non-Married			Married		
	Sample Size	Mean of Dep.	Estimates	Sample Size	Mean of Dep.	Estimates
Less than High School						
# of Prenatal Visits	230095	9.05	0.0061	143950	9.70	0.00077
			(0.010)			(0.012)
Weight Gain (pounds)	226804	26.3	0.065	141905	25.0	0.0038
			(0.037)			(0.040)
High School						
# of Prenatal Visits	498730	10.1	0.0080	361259	10.9	0.0090
			(0.0074)			(0.0077)
Weight Gain (pounds)	494788	27.8	-0.035	359268	27.2	0.014
			(0.028)			(0.029)

Notes: The sample includes low educated mothers (high school or less than high school) aged 18-39 years with parity one or greater for the period from April 2020 to December 2021. Estimates are obtained using OLS from Equation (1) and representing the effects of $1000 from the cash transfers over the pregnancy period on number of prenatal visits and weight gain in pounds. The models control for age, race/ethnicity, type of insurance, county fixed effect, time fixed effects, and the interactions between demographic characteristics and time fixed effects. Estimates are presented separately by four education-marital status groups. Estimates are presented separately by four education-marital status groups. Depending on the sample and outcome, the number of observations varies from 143,950 to 498,730.

*p<0.05, **p<0.01, ***p<0.001