The Intergenerational Effects of Head Start on Infant Health

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Abstract

This paper evaluates the effects of maternal access to the largest federal preschool program in the U.S., Head Start, on infant health at birth using Vital Statistics Natality data. Using variation in the year of Head Start's introduction in each county from 1966 to 1980, we find that mother's access to Head Start during childhood improves infant health. As a potential pathway, we show that Head Start availability reduces the likelihood of drinking during pregnancy. These findings indicate that the benefits of early childhood programs have enduring effects that carry over to the next generation, indicating that the returns on these types of public investments may be significantly underestimated.

JEL classification codes: H53, I38, J13

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I Introduction

Children who grow up in poverty have, on average, worse socio-economic outcomes in adulthood relative to their more advantaged peers. These differences are observed for a variety of health, educational, labor market, and social outcomes. This creates a cycle of disadvantage, where poverty in one generation perpetuates itself into the future. Many public programs in the United States are focused on improving the lives of children, in order to "not only relieve the symptom of poverty, but to cure it and [...] prevent it", as President Lyndon B. Johnson described his vision for the War on Poverty (1965). The goal of these policies is to improve not only the lives of children in poverty but also the lives of their future children as well (Aizer and Currie, 2014; Currie and Rossin-Slater, 2015; Almond et al., 2018). A growing literature shows that early childhood interventions can have transformative effects for life.[1] However, evidence on whether these gains transfer to future generations is more limited (Rossin-Slater and Wüst, 2020; Barr and Gibbs, 2022; East et al., 2023). We contribute to this limited literature on the intergenerational benefits of early childhood interventions by investigating the effects of the Head Start program on infant health for the second generation. As the largest federal early childhood program in the United States, Head Start provides education, health, and other social services to economically disadvantaged children with the goal of promoting school readiness. Initially started as a summer program in 1965, the program spread to over 2500 counties in the next decade. Head Start has been shown to improve long-run education, health, and labor market outcomes of participants (e.g., Garces et al., 2002; Ludwig and Miller, 2007; Deming, 2009; Thompson, 2018; Bailey et al., 2021; Kose, 2023; Carneiro and Ginja, 2014). These benefits could lead to better second-generation outcomes, including infant health.

This study examines birth weight as the primary outcome, as low birth weight (defined as less than 2500 grams) is associated with adverse cognitive outcomes later in life and poses an increased risk of mortality (Currie, 2011; Currie and Moretti, 2003). Additionally, low birth weight has been associated with lifelong deficits (Corman et al., 1987; Almond et al., 2005), while children who are healthier at birth have better human capital outcomes as adults (Black et al., 2007). Head Start exposure of mothers may affect infant health by improving their economic well-being or by reducing engagement in risky behaviors. For example, Currie and Moretti (2003) argue that maternal education directly increases investments in children and the likelihood of having access to health insurance through higher labor market earnings.

[1] There are a number of papers that provide convincing evidence on the long-term effects of early childhood programs (Heckman et al., 2010; Cunha and Heckman, 2007; Anderson, 2008; García et al., 2020; Almond et al., 2018; Almond and Currie, 2011; Currie, 2001).

Beyond formal education, Head Start-induced improvements in first-generation non-cognitive skills, economic self-sufficiency, and health status could also improve investments in offspring infant health (Deming, 2009; Carneiro and Ginja, 2014; Thompson, 2018).

To estimate the intergenerational link between maternal exposure to Head Start and infant health at birth, we leverage variation from the rollout of the program across counties and over time between 1966 and 1980 for cohorts born 1955 through 1975. The main empirical strategy compares the outcomes of infants whose mothers were slightly too old to be eligible for Head Start when the program was introduced in their county with the outcomes of infants whose mothers were eligible at the time of the program's introduction. To ensure the results are robust if treatment effects are not homogeneous across counties and cohorts (Goodman-Bacon, 2021), we use the method from Sun and Abraham (2021) instead of the more traditional two-way fixed effects (TWFE) method to estimate our specifications. Head Start placement is not random: counties that receive early Head Start funding are different from counties that do not. To address this, we calculate the probability of each county receiving a Head Start grant based on pre-program characteristics. The results of this regression are then used to create propensity score weights to ensure that our control counties are as similar as possible to the treated counties.

We find that maternal exposure to Head Start improves infant health in the next generation. Our results show that the average baby born to a mother exposed to Head Start is 8-10 grams heavier. Impacts are larger at the bottom of the birth weight distribution: children born to mothers exposed to Head Start were more than 2.5% less likely to have a birth weight below 2,500 grams and around 5% less likely to be born below 1,500 grams. Moreover, our event study analyses suggest that the second-generation effects are strongest for those mothers fully exposed to the program. In contrast, for mothers who were too old to benefit from Head Start, the effects are around zero and statistically insignificant, which serves as evidence of the parallel trend assumption. Turning to potential mechanisms, maternal exposure to Head Start is associated with a reduction in drinking during pregnancy, which is considered a risk factor for intrauterine growth restrictions and low birth weight (De Bernabé et al., 2004).

This paper adds to the evidence on the effects of maternal access to social programs on infant health. However, this literature mostly focuses on in-utero exposure to social programs. Evidence from the nutrition programs (Food Stamps and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)) indicate that access to nutrition aid during pregnancy improves infant health (e.g., Almond et al., 2011; Bitler and Seifoddini,

2019; Hoynes et al., 2011; Rossin-Slater, 2013). Hoynes et al. (2015) provide evidence that access to the Earned Income Tax Credit (EITC) during pregnancy leads to better health outcomes for infants, while Currie and Gruber (1996) find that prenatal access to health insurance reduces the incidence of low birth weight and infant mortality. In addition, Kose et al. (2024) show that access to Community Health Centers also improves birth outcomes. In this paper, we focus on an early childhood program and its intergenerational effects.

This study also contributes to the limited literature on the intergenerational effects of parental experiences in early childhood (Rossin-Slater and Wüst, 2020; Barr and Gibbs, 2022; East et al., 2023). Notably, Barr and Gibbs (2022) show that Head Start access leads to substantial improvements in the long-term human capital outcomes of the second generation. We find that Head Start generates intergenerational health improvements for infants, a potential pathway to explain longer-term human capital improvements in the second generation.

II Data

Our primary data source is the Vital Statistics Natality data from the National Center for Health Statistics from 1970 to 2019. Natality data contain information on infant health outcomes and mothers' age, race, ethnicity, marital status, and education.[2] The estimation sample includes mothers who were born between 1955 and 1975. Importantly, mothers' state of birth, state of residence, and county of residence are reported in the Vital Statistics data, but mothers' county of birth is not reported. To improve the accuracy of the treatment assignment, we restrict the sample to mothers who were born and gave birth in the same state. Conditional on this, we use county of residence and year of birth to assign Head Start presence between the ages of three and six.[3]

Our outcome measures are the average birth weight (in grams), low birth weight (less than 2,500 grams), very low birth weight (less than 1,500 grams), small for gestational age (below the tenth percentile of birth weight for gestational age), prenatal care, mothers' risky health behaviors, mothers' medical risk factors (diabetes, hypertension, and eclampsia), mothers' socio-demographic characteristics, and the number of births.

Treatment exposure is defined by the year that each county obtained any Head Start

[2]Some states did not report the education and ethnicity of mothers prior to 1993. Hoynes et al. (2015) describe the missing information in greater detail.

[3]To test the validity of the treatment assignment, we will investigate the relationship between Head Start exposure and migration using the NSLY79.

funding from 1966 to 1980 and the age of the mother when Head Start arrived in her county. The rollout data come from Bailey et al. (2021) who rely on multiple sources including the National Archives and Records Administration (NARA) Community Action Program Files (CAP), and Federal Outlay System Files (FOSF).

Variables measuring 1960 county variables are obtained from Almond et al. (2011) and include the percentage of the 1960 population living in an urban area, is black, is less than 5 years old, is 65 or older, has income less than $3,000, the percentage of land that is farmland, and the county population. Other cash transfers, other social programs, and medical spending data are from the Regional Economic Information Systems (REIS) and CAP files obtained from Bailey and Goodman-Bacon (2015). These data include public assistance per capita, total active medical doctors per capita, and all-age mortality rate.

III Research Design

This paper leverages variation in Head Start funding availability across counties and over time from 1966 to 1980. To estimate the causal effect of maternal access to Head Start on infant health, we use a difference-in-differences design and a flexible event-study framework to estimate static and dynamic treatment effects. For computational ease, we collapse the Vital Statistics data into the child's birth-year, birth-county, [4] mother's birth-year and race cells. Our static specification is the following:

$$Y_{ctbr} = \tau HeadStart_{cb} + \mathbf{M}'_{ctbr}\boldsymbol{\alpha} + \theta_c + \delta_b + \eta_t + \mu_{sb} + \varepsilon_{ctbr} \tag{1}$$

where Y_{cbtr} is the birth outcome of infants born in county c and year t, and whose mother was born in year b and belongs to race r.

$HeadStart_{cb}$ captures maternal exposure to Head Start based on the mother's birth year and the mother's county of residence at child's birth.[5] $HeadStart_{cb}$ is equal to 1 if Head Start was available in county c and the mother's age at the rollout was six or younger. Unfortunately, we do not observe the mother's exact date of birth. Therefore, to calculate the mother's year of birth and her corresponding age at the Head Start rollout, we use the mother's age at the time of birth and the child's year of birth. This approach may misassign

[4] County of birth corresponds to the mother's county of residence at the time of the birth.

[5] Ideally, we would have the mother's county of birth or county of residence when she was three to six years old, the ages targeted by the introduction of Head Start. However, Vital Statistics only report the mother's state of birth, but not the county of birth. As a proxy, we use the mother's county of residence when the birth occurred for the sample of mothers who were born and gave birth in the same state.

treatment exposure for mothers who were near the eligibility cutoff age when Head Start funding arrived in their county. Therefore, we define exposure as age six and younger so that we do not accidentally assign treated mothers to the untreated group.[6]

$\mathbf{M}_{ctbr}$ contains a constant and cell-level characteristics, including maternal race and the fraction of male births. θ_c represent county fixed effects that absorb time-invariant characteristics that could be correlated with the Head Start rollout. δ_b and η_t represent the mother's and child's birth-year fixed effects, respectively, that capture time-varying national changes in the outcomes. We also include state-by-mother birth year fixed effects (μ_{sb}) to account for changes in state policies that could differentially affect mothers' birth cohorts (Bailey et al., 2021).[7] Standard errors are clustered at the county level. As counties that introduced Head Start before 1980 could be different than those that adopted it later or never adopted it, our preferred estimates use inverse propensity score weights to make control counties comparable to treated ones according to pre-program introduction characteristics, as discussed further below.

To examine dynamic treatment effects, we estimate the following event-study specification:

$$\begin{aligned} Y_{ctbr} = \lambda D_c 1\left(AgeTreat_{cb} \leq -6\right) + \sum_{a=-5}^{15} \tau_a D_c 1\left(AgeTreat_{cb} = a\right) \\ + \nu D_c 1\left(AgeTreat_{cb} \geq 16\right) + \mathbf{M}'_{ctbr}\boldsymbol{\alpha} + \theta_c + \delta_b + \mu_{sb} + \varepsilon_{ctbr} \end{aligned} \tag{2}$$

where D_c is a binary treatment variable equal to one if the county ever receives a Head Start grant. Indicator variables ($1AgeTreat_{cb} = a$) capture the mother's age in the year that Head Start became available in county c for observations in counties that ever received the program and are zero in all periods for counties without a Head Start program by 1980. The identifying variation for the coefficients τ_a is driven by differential exposure to Head Start's introduction within cohorts across counties and within counties across cohorts. Head Start's target age group at the time of its introduction was three to five, although children up to age six did attend some programs (Levitan, 1969). Therefore, ages older than six are not treated, and the evolution of the coefficients for those ages directly tests the identification

[6]Head Start eligibility depends on age, income, and the state age cutoff for first grade, none of which we can directly observe in our data.

[7]We present our results excluding and including state-by-mother birth year characteristics in the results section.

assumption that the county's Head Start adoption is not correlated with pre-program trends in infant health.

Observations corresponding to maternal ages older than 15 years are captured by the dummy $1(AgeTreat_{cb} >= 16)$. Similarly, observations corresponding to maternal ages less than age -6 are captured by the dummy $1(AgeTreat_{cb} <= -6)$.[8] The omitted category is age seven. This corresponds to a conservative cutoff. As previously noted, maternal age at Head Start rollout is proxied based on the mother's reported age in years at the birth of the child, and omitting age seven accurately captures mothers who were definitely not exposed to the program. In the dynamic model, we also add the control variables described in the static specification.

To address the potential biases from treatment effect heterogeneity that are likely to contaminate staggered designs as ours (Goodman-Bacon, 2021), we use the method from Sun and Abraham (2021). Our control group is the set of counties that have not adopted Head Start by 1980.

A Determinants of Head Start Adoption

The validity of our research design relies on the assumption that the timing of Head Start roll-out across counties is uncorrelated with other county, time-varying determinants of our outcomes of interest. To provide evidence on this assumption, we test whether 1960 county characteristics (pre-program introduction) predict the timing of a county's Head Start program implementation in Table 1, following a similar approach to that used by other WOP studies (e.g., Hoynes and Schanzenbach, 2009; Bailey, 2012; Bailey et al., 2023). The dependent variable is the year of Head Start's rollout, expressed as an index equal to one beginning in 1966. The results, presented in Table 1, include models with and without state fixed effects, as well as unweighted and weighted by county population of children ages three to six in 1960. We find that urbanicity and public transfers per capita, among other factors, are associated with the earlier introduction of Head Start. However, the quantitative importance of these predictors is very small, and most of the variation remains unexplained (as illustrated by the low R-squared, which ranges from 0.04 and 0.17). This benefits our identification strategy as there is no strong evidence of endogeneity in the timing of Head Start.

[8] Following similar recent studies (e.g., East et al., 2023; Bailey et al., 2023), these binned endpoints are not reported in the figures because they are estimated using an imbalanced sample of counties (we observe only a small subset of counties with Head Start funding in the endpoints).

To mitigate potential unobservable differences between the treatment and control groups, our preferred strategy uses an alternative approach similar to Sant'Anna and Zhao (2020)'s doubly robust approach. In all our estimates, we reweight untreated counties using an estimate of the propensity of county Head Start adoption by 1980 based on pre-program county characteristics measured in 1960.[9] After estimating the predicted probability of adopting Head Start by 1980, $\hat{p}$, we compute the following cell weights. For control counties (no Head Start by 1980), we weight the cells by the number of births in the cell times $\hat{p}/(1-\hat{p})$. For treated countries, the weight is left as the number of births in the cell.

IV Results

A Infant Outcomes

Table 2 presents the static effects of maternal Head Start exposure on birth weight (in grams), the fraction of low birth weight (less than 2,500 grams), the fraction of very low birth weight (less than 1,500 grams), and small for gestational age. Column 1 shows our baseline specification, which includes county, birth year for mothers and children, and mother's race fixed effects. For each outcome, Column 2 adds state-by-mother birth year fixed effects. The results indicate that maternal exposure to Head Start led to an increase in offspring birth weight between 8-10 grams (≈0.3% of the control mean). Looking at the left tail of the distribution, we find that children born to mothers exposed to Head Start were around 3% less likely to have a low birth weight (<2500 grams) and 4-5% less likely to have a very low birth weight birth (<1500 grams). As birth weight is partly determined by gestational age (Kramer, 1987a,b), we also examine the probability of an infant being small for their gestational age. Our static effects indicate a reduction in this measure, though it is not statistically significant.

Turning to the dynamic effects, Figure 2 plots the event study coefficients of maternal exposure to Head Start by maternal age at Head Start's rollout. Given the nature of the Head Start program and the staggered implementation, we consider mothers aged 4 to 6 to be partially exposed (only a few preschool years) and expect any program effects to be smaller for those cohorts. Mothers aged three or younger, when their county received a Head Start grant, would be fully exposed for all their preschool years and potentially benefit the most from the program. In contrast, mothers older than six at Head Start rollout would not

[9] In particular, the logit model includes the share of the population under poverty, the share of the population under 5 years old and 65 and older, the share of the adult population with various levels of education, the share non-white, the share urban/rural, welfare programs transfers per capita, median family income and the number of doctors per capita in 1960.

benefit from the program as they were too old to participate. Thus, the coefficients for older cohorts can test for differential pre-trends and should be close to zero.

Visually, the point estimates reflect this pattern. For mothers fully exposed, Panel (a) of Figure 2 shows that the impacts of Head Start exposure on their children's birth weight are around 8-10 grams, while the effects for partially exposed mothers are around 2-3 grams. The effects are close to zero and statistically insignificant for mothers not exposed to the program. For low birth weight (Panel b), very low birth weight (Panel c), and small for gestational age (Panel d), we find significant reductions, particularly among fully exposed mothers. In contrast, for non-exposed mothers, the point estimates are smaller or in the opposite direction and not statistically significant. We formally test this identification assumption with an F-test of the joint significance of the point estimates for the older cohorts. The p-value of this test is printed on the subfigure for each respective outcome in Figure 2. In each case, there is no evidence of significant differential pre-trends in infant outcomes.

We further investigate the effects of mothers' childhood access to Head Start across the distribution of birth weight in Figure 3. Each point represents the change in the probability that a baby is born at or below the birth weight threshold on the horizontal axis relative to the control group mean. Consistent with the results from Table 2, maternal program access improves birth weight across the distribution, but the largest effects are for the smallest infants. In contrast, for infants that are already in the healthy part of the weight distribution, the effects are more modest or close to zero.

Lastly, we explore treatment effect heterogeneity by race. Black mothers are, on average, more economically and socially disadvantaged in childhood, and these disparities persist through adulthood (Currie, 2011). Table 3 shows that both Black and White infants benefit from maternal access to Head Start but the effects are felt for different parts of the distribution. Panel (a) shows that the overall effect is stronger for babies born to Black mothers. Relative to their respective means, the percent effect is 70% larger for Black infants. However, these gains are not happening at the most vulnerable parts of the distribution. The reductions in the incidences of low birth weight and very low birth weight births are only occurring for White mothers. Focusing on human capital, Bailey et al. (2021) also find similar overall effects for White and non-White adults that are driven by different parts of the education distribution. Given the different life experiences of White and Black women in the US, it's not surprising that Head Start may affect their birth outcomes in different ways.

Our main results indicate that the benefits of Head Start extend the next generation's

health. East et al. (2023) examined the intergenerational effects of Medicaid, finding that changes in first-generation prenatal Medicaid eligibility improved offspring birth weight by 3-5 grams and reduced the likelihood of low birth weight by 0.1-0.2 percentage points. Our ITT effects of maternal exposure to Head Start on offspring infant health, as described above, are comparable to their findings, suggesting that both prenatal health insurance and early childhood interventions can generate intergenerational spillover effects.

B Maternal Behaviors and Medical Risk factors

To understand how maternal exposure to Head Start improves infant health, we examine health behaviors during pregnancy. Through Head Start's increases in adult human capital, we could expect mothers to make better health decisions (e.g., Grossman, 2017; Kenkel, 1991). The effects on prenatal care take-up, maternal risky behaviors during pregnancy, and medical risk factors are presented in Table 4 and Figure 4. We find small and noisy impacts on starting prenatal care during the first trimester. This suggests that the timing of prenatal care is not driving the improvements in infant health shown above, although there may be an unobservable effect on quality. Higher human capital is associated with higher quality doctor-patient interactions, which may lead to better compliance with medical guidelines and ultimately a positive effect on infant health (Verlinde et al., 2012).

Turning to risky behaviors, maternal access to Head Start is associated with reductions in the likelihood of the mother reporting smoking or drinking during her pregnancy. The point estimate from our preferred specification for tobacco use suggests a decrease of 3.5% relative to the control mean (Table 4); however, in the dynamic specification, the effects are noisier and not statistically significant. This reduction in smoking during pregnancy is consistent with the improvements in health at birth as smoking is considered one of the largest risk factors for intrauterine growth restrictions and low birth weight (Kabir et al., 2013; Reeves and Bernstein, 2008; Kramer, 1987a).

The decline in alcohol use is even stronger: an almost 10% reduction in drinking during pregnancy relative to the control mean in our preferred specification.[10] Additionally, the dynamic specification shows effects that are larger for fully exposed mothers. This evidence provides an additional mechanism to explain the observed improvements in birth weight (De Bernabé et al., 2004). Beyond the observable effect on birth weight, alcohol use during pregnancy has serious negative consequences for brain development. Individuals whose mothers drank during pregnancy show greater levels of impulsiveness as children and ado-

[10]It is worth noting that alcohol use is only available in the data through 2006.

lescents (Lees et al., 2020). Reductions in prenatal alcohol use in the first generation due to Head Start access should lead to a decrease in impulsive behavior in the second generation, a likely mechanism for the longer-run effects documented in Barr and Gibbs (2022).

Previous work has found Head Start access leads to health improvements in adulthood (Thompson, 2018), and better pre-conception health could explain the infant health improvements discussed above (Stephenson et al., 2018). To investigate this, we construct an indicator for maternal medical risk factors, which includes diabetes, chronic hypertension, pregnancy-related hypertension, and eclampsia. We find small and statistically insignificant reductions both in the static and dynamic specifications. If pre-conception health is playing a role here, it's not operating through the conditions reliably tracked on the birth certificate.

C Maternal Outcomes

Considering Head Start's broad impacts on long-run outcomes (Bailey et al., 2021), maternal access during childhood could affect who gives birth and their circumstances when they do. To understand whether Head Start is changing the composition of mothers, we estimate equation 1 with information on mothers at the time of their first birth as the dependent variable. We focus on first births for these outcomes because they are defined at the level of the mother, not the birth. Table 5 shows that access to Head Start did not change the probability of having their first birth while still in their teens, being married at the time of the birth, or having the father on the birth certificate. There is also no observable change in the educational attainment of mothers exposed to Head Start.

V Conclusion

This paper investigates the ability of the Head Start program to improve outcomes for future generations. Using a difference-in-differences research design, we find that maternal exposure to Head Start leads to larger overall birth weight and a lower incidence of low birth weight births. A reduction in risky maternal behaviors during pregnancy is an important mechanism for this effect: we find significant reductions in reported use of both alcohol and tobacco for mothers who were exposed to the Head Start program as children.

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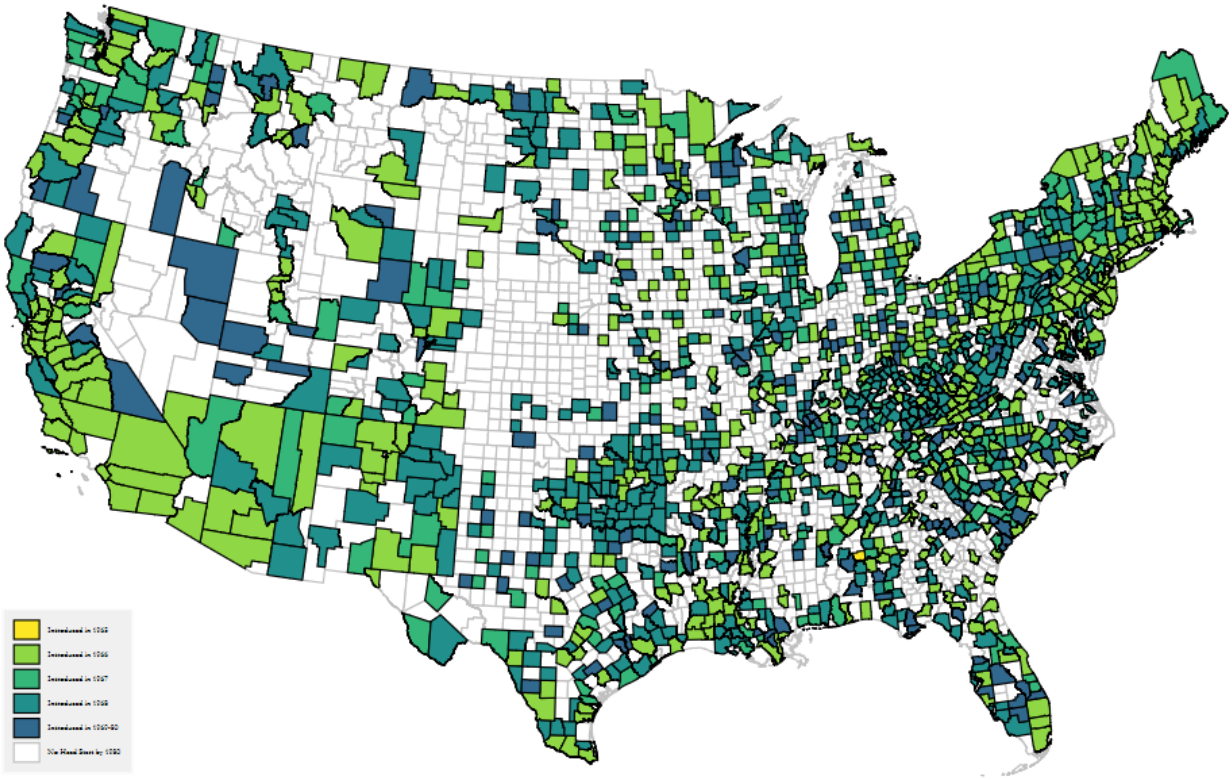
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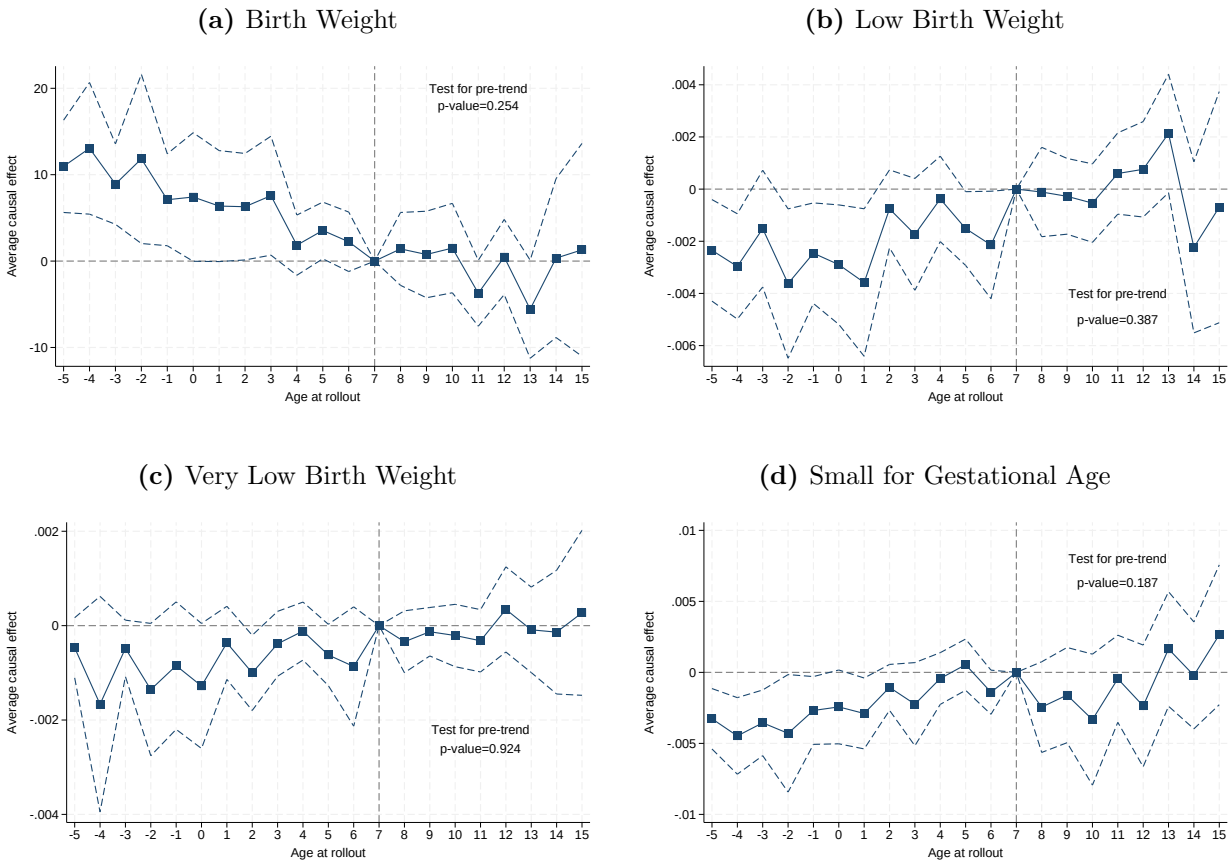
VI Figures

Figure 1: Timing of Head Start Adoption and Geographic Variation



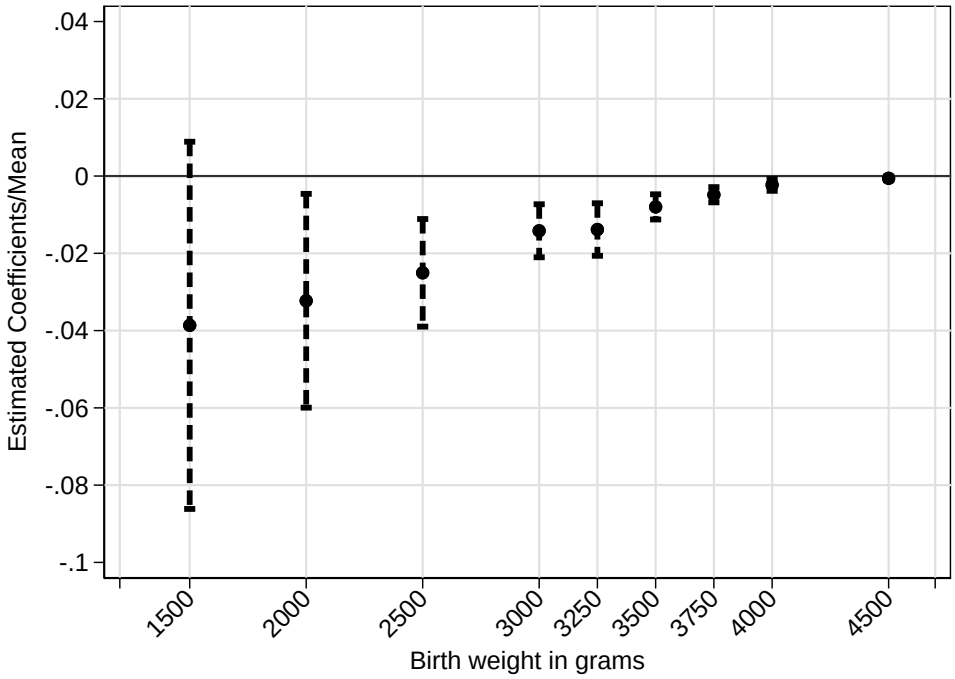
Notes: Data is compiled by Bailey et al. (2021) using National Archives and Records Administration, Community Action Programs and Federal Outlay System Files.

Figure 2: The Effect of Mother's Exposure to Head Start on Birth Outcomes



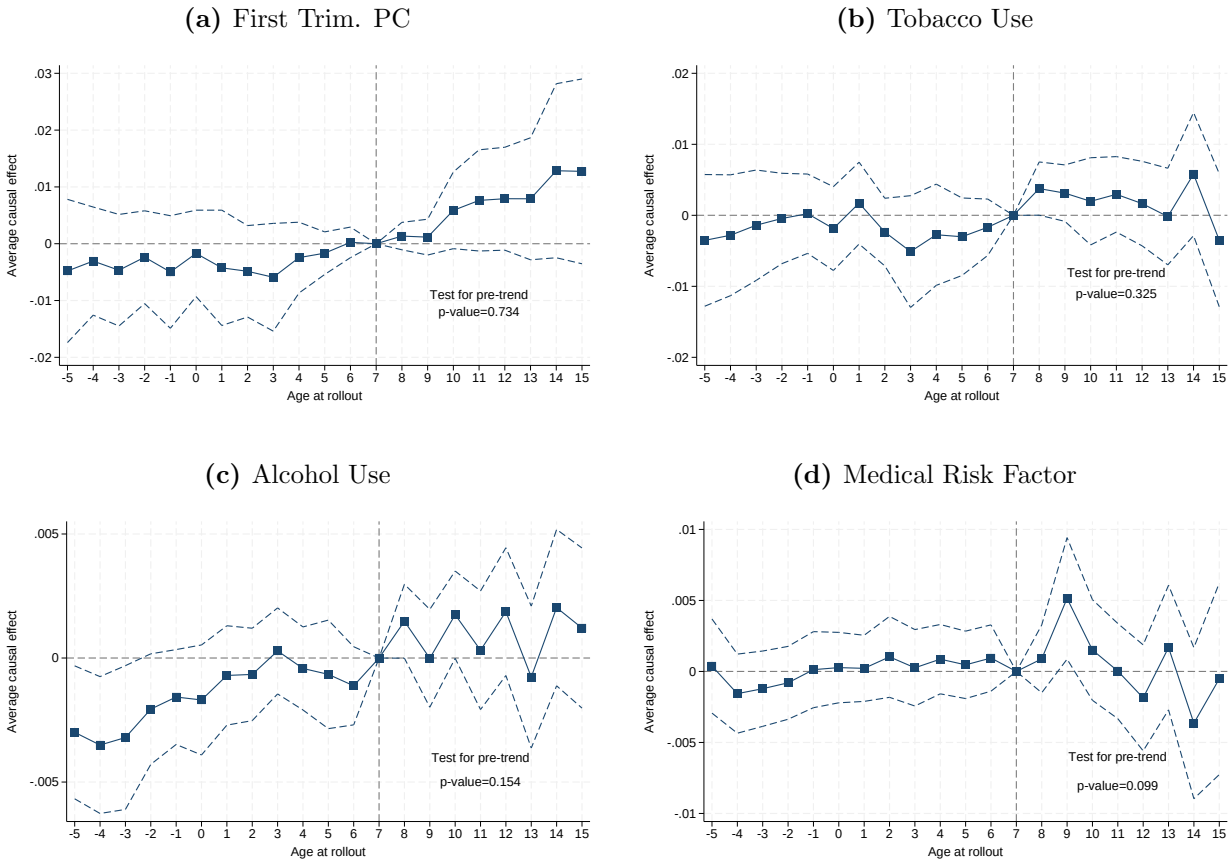
Notes: Sample corresponds to children from mothers born between 1955 and 1975. This figure plots the estimated coefficients and 95% confidence intervals obtained from event study specification using the methods introduced in Sun and Abraham (2021). The X-axis corresponds to maternal age at Head Start rollout. All regressions include the child's county of birth, birth year, race, and state-by-mother's birth year fixed effects. Standard errors are clustered at the county level. Our preferred specification re-weights untreated counties using an estimate of the propensity of Head Start adoption based on pre-program characteristics. For more details about the estimation, see Equation 2 in Section III.

Figure 3: The Effect of Mother's Exposure to Head Start in the Distributional of Birth Weight
Percent Impacts



Notes: We estimate the effect of Head Start on the probability that birth weight is below a given gram threshold using individual level data: 1,500; 2,000; 2,500; 3,000; 3,250; 3,500; 3,750; 4,000; or, for 4,500 grams, the probability that it is above that number (Almond et al., 2011). We plot the "% Impacts [coef./mean]" and the 95% confidence intervals separately using Sun and Abraham (2021) method and our preferred specification which re-weights untreated counties using an estimate of the propensity of Head Start adoption based on pre-program characteristics. All regressions include the child's county of birth, birth year, race, and state-by-mother's birth year fixed effects. Standard errors are clustered at the county level. For more details about the estimation, see Equation 2 in Section III.

Figure 4: The Effect of Mother's Exposure to Head Start on Pregnancy Behaviors and Medical Risk



Notes: Sample corresponds to children from mothers born between 1955 and 1975. This figure plots the estimated coefficients and 95% confidence intervals obtained from event study specification using the methods introduced in Sun and Abraham (2021). The X-axis corresponds to maternal age at Head Start rollout. All regressions include the child's county of birth, birth year, race, and state-by-mother's birth year fixed effects. Standard errors are clustered at the county level. Our preferred specification re-weights untreated counties using an estimate of the propensity of Head Start adoption based on pre-program characteristics. For more details about the estimation, see Equation 2 in Section III.

VII Tables

Table 1: Determinants of Head Start Rollout

	1966-1980		
	(1)	(2)	(3)
Median family income- 1959	0.3872	0.2801	0.1977
	(0.4924)	(0.4677)	(0.5082)
w/ family income 3000 - 1959	-0.0087	0.2489	-0.1079
	(0.3700)	(0.3745)	(0.4369)
w/ family income 10000+ - 1959	-0.2922*	-0.1683	-0.0560
	(0.1684)	(0.1532)	(0.1599)
population aged 0-4 -1960	0.0336	0.1175	0.0325
	(0.0877)	(0.0807)	(0.0960)
population aged 65+ - 1960	0.1165	0.1562*	0.2070**
	(0.0999)	(0.0907)	(0.1050)
persons 25+ w/ 4 yrs sch. - 1960	0.0422	-0.1442	0.0511
	(0.1027)	(0.0916)	(0.1149)
persons 25+ w/ 12+ yrs sch. - 1960	0.0178***	0.0155***	0.0096
	(0.0052)	(0.0035)	(0.0139)
nonwhite - 1960	0.0687	0.1118	0.0263
	(0.0769)	(0.0794)	(0.0948)
urban - 1960	-0.2946***	-0.3931***	-0.6280***
	(0.0770)	(0.0997)	(0.1203)
rural farm - 1960	0.1301*	0.2518*	0.1708
	(0.0782)	(0.1413)	(0.1279)
Total Active MDs per pop 1960	-0.1071**	-0.0504	-0.0311
	(0.0415)	(0.0430)	(0.0448)
AMR - All Ages - 1960	-0.0583	-0.1700**	-0.0153
	(0.0659)	(0.0844)	(0.0853)
Public Assist Transfers PC 1960	-0.0690	-0.1988***	-0.2719***
	(0.0517)	(0.0620)	(0.1003)
R-Squared	0.039	0.148	0.173
Obs	1542	1539	1539
Weighted	N	Y	Y
State FE			Y

Notes: The data are at the county level and the dependent variable is the year (normalized to one in 1966) that Head Start was introduced in the county. The covariates come from the City and County Data Book for 1960. Columns 2-3 are weighted by 1960 county population. Heteroskedasticity-robust standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.

Table 2: The Effect of Mother's Exposure to Head Start on Birth Outcomes

	Birth Weight		Low Birth Weight		Very Low Birth Weight		Small for Gestational Age	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Maternal HS exposure	9.6043***	7.6836***	-0.0021**	-0.0020***	-0.0008*	-0.0006	-0.0012	-0.0006
	(3.5939)	(1.6628)	(0.0009)	(0.0006)	(0.0004)	(0.0003)	(0.0012)	(0.0010)
Control Y-mean	3321.9553	3321.9553	0.0763	0.0763	0.0135	0.0135	0.0974	0.0974
% Effect (Coef/Control Mean)	0.289	0.231	-2.689	-2.579	-5.628	-4.106	-1.245	-0.632
Adj. R-Squared	0.350	0.353	0.126	0.128	0.042	0.043	0.128	0.131
Obs	2216920	2216920	2216920	2216920	2216920	2216920	2159146	2159146
State-by-Mother's BirthYear		Y		Y		Y		Y

Notes: Sample includes mothers who were born between 1955 and 1975. Maternal exposure to Head Start is calculated based on county program availability when the mother was in preschool age. All regressions include mother's birth year, and race fixed effects, and child's county of birth and birth year fixed effects. Our preferred specification re-weights untreated counties using an estimate of the propensity of Head Start adoption based on pre-program characteristics. Standard errors are clustered at the county level. $*$ p<0.10, $**$ p<0.05, $***$ p<0.01.

Table 3: Heterogeneity by Race: The Effect of Mother's Exposure to Head Start

	All		White		Black	
	(1)	(2)	(1)	(2)	(1)	(2)
A: Birth Weight						
Maternal HS exposure	9.6043***	7.6836***	8.8020**	6.9163***	11.9075	10.8831***
	(3.5939)	(1.6628)	(4.0666)	(1.8155)	(7.3743)	(3.2908)
Y-mean	3321.9553	3321.9553	3379.1073	3379.1073	3070.8514	3070.8504
% Effect (Coef/Control Mean)	0.289	0.231	0.260	0.205	0.388	0.354
Adj. R-Squared	0.350	0.353	0.155	0.161	0.055	0.063
Obs	2216920	2216920	1461479	1461479	587260	587222
B: Low Birth Weight						
Maternal HS exposure	-0.0021**	-0.0020***	-0.0020**	-0.0020***	-0.0012	-0.0007
	(0.0009)	(0.0006)	(0.0010)	(0.0006)	(0.0025)	(0.0013)
Y-mean	0.0763	0.0763	0.0617	0.0617	0.1405	0.1405
% Effect (Coef/Control Mean)	-2.689	-2.579	-3.268	-3.294	-0.886	-0.509
Adj. R-Squared	0.126	0.128	0.029	0.032	0.026	0.032
Obs	2216920	2216920	1461479	1461479	587260	587222
C: Very Low Birth Weight						
Maternal HS exposure	-0.0008*	-0.0006	-0.0010**	-0.0008**	0.0001	0.0004
	(0.0004)	(0.0003)	(0.0005)	(0.0004)	(0.0008)	(0.0005)
Y-mean	0.0135	0.0135	0.0100	0.0100	0.0286	0.0286
% Effect (Coef/Control Mean)	-5.628	-4.106	-9.808	-7.875	0.409	1.338
Adj. R-Squared	0.042	0.043	0.007	0.009	0.013	0.015
Obs	2216920	2216920	1461479	1461479	587260	587222
D: Small for Gestational Age						
Maternal HS exposure	-0.0012	-0.0006	-0.0008	-0.0004	-0.0020	-0.0010
	(0.0012)	(0.0010)	(0.0013)	(0.0011)	(0.0026)	(0.0015)
Y-mean	0.0974	0.0974	0.0833	0.0833	0.1604	0.1604
% Effect (Coef/Control Mean)	-1.245	-0.632	-0.939	-0.465	-1.268	-0.596
Adj. R-Squared	0.128	0.131	0.057	0.061	0.021	0.025
Obs	2159146	2159146	1431415	1431415	565331	565289
State-by-Mother's BirthYear		Y		Y		Y

Notes: Sample includes mothers who were born between 1955 and 1975. Maternal exposure to Head Start is calculated based on county program availability when the mother was in preschool age. All regressions include mother's birth year, and race fixed effects, and child's county of birth and birth year fixed effects. Our preferred specification re-weights untreated counties using an estimate of the propensity of Head Start adoption based on pre-program characteristics. Standard errors are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: The Effect of Mother's Exposure to Head Start on Pregnancy Behaviors and Medical Risk

	First Trim. PC		Tobacco Use		Alcohol Use		Medical risk factor	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Maternal HS exposure	-0.0153*	-0.0067	-0.0049	-0.0056**	-0.0060***	-0.0027***	-0.0004	-0.0012
	(0.0088)	(0.0059)	(0.0070)	(0.0027)	(0.0019)	(0.0010)	(0.0014)	(0.0010)
Control Y-mean	0.7844	0.7844	0.1596	0.1596	0.0279	0.0279	0.0809	0.0809
% Effect (Coef/Control Mean)	-1.955	-0.858	-3.049	-3.479	-21.424	-9.659	-0.529	-1.438
Adj. R-Squared	0.533	0.538	0.289	0.299	0.138	0.151	0.166	0.169
Obs	2168105	2168105	1316525	1316525	1127260	1127259	1442354	1442354
State-by-Mother's BirthYear		Y		Y		Y		Y

Notes: Sample includes births to mothers who were born between 1955 and 1975. Maternal exposure to Head Start is calculated based on county program availability when the mother was in preschool age. All regressions include mother's birth year, and race fixed effects, and child's county of birth and birth year fixed effects. Our preferred specification re-weights untreated counties using an estimate of the propensity of Head Start adoption based on pre-program characteristics. Standard errors are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: The Effect of Mother's Exposure to Head Start on Maternal Composition

	Teen		Married		Dad missing	
	(1)	(2)	(1)	(2)	(1)	(2)
A: Demographic characteristics						
Maternal HS exposure	-0.0054	0.0012	0.0033	0.0034	0.0108	0.0061
	(0.0055)	(0.0035)	(0.0077)	(0.0045)	(0.0086)	(0.0042)
Control Y-mean	0.300	0.300	0.713	0.715	0.149	0.148
Percent Effect	-1.793	0.398	0.468	0.479	7.293	4.100
Adj. R-Squared	0.772	0.831	0.844	0.878	0.803	0.868
Obs	60438	60417	41673	41652	60438	60417
	HS grad		Any College		College grad	
	(1)	(2)	(1)	(2)	(1)	(2)
B: Educational attainment						
Maternal HS exposure	0.0039	-0.0011	0.0044	-0.0020	0.0044	0.0063
	(0.0051)	(0.0042)	(0.0103)	(0.0065)	(0.0128)	(0.0046)
Control Y-mean	0.777	0.777	0.349	0.349	0.153	0.153
Percent Effect	0.505	-0.143	1.263	-0.579	2.899	4.131
Adj. R-Squared	0.779	0.835	0.818	0.876	0.754	0.833
Obs	41545	41545	41545	41545	41545	41545
State-by-Year		Y		Y		Y

Notes: Sample includes first births to mothers who were born between 1955 and 1975. Maternal exposure to Head Start is calculated based on county program availability when the mother was in preschool age. Not all states reliably reported educational attainment or marital status in birth data for our sample years. We follow Kearney and Levine (2007) and only include states that reliably report the relevant information for the marital status and educational attainment regressions. All regressions include mother's birth year, and race fixed effects, and child's county of birth and birth year fixed effects. Our preferred specification re-weights untreated counties using an estimate of the propensity of Head Start adoption based on pre-program characteristics. Standard errors are clustered at the county level. * p<0.10, ** p<0.05, *** p<0.01.