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PUBLIC PROGRAMS AND THE WELL-BEING OF FAMILIES WITH CHILDREN‡

Head Start Access: No Evidence of Changes to Lifetime Fertility†

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Head Start has been shown to improve the long-run educational, health, and labor market outcomes for participants (e.g., Garces, Thomas, and Currie 2002; Ludwig and Miller 2007; Carneiro and Ginja 2014; Thompson 2018; Bailey, Sun, and Timpe 2021; Kose 2023). These effects could influence the fertility choices of women who were exposed to the program in childhood.

The effect of Head Start on total fertility is theoretically ambiguous and therefore requires empirical investigation. While improved labor market opportunities likely increase the opportunity cost of childbearing and may reduce fertility both theoretically (Becker and Lewis 1973) and empirically (Schaller 2016), more resources could lead to increased total fertility for these cohorts if children are a normal good (Black et al. 2013). Previous work has shown Head Start had no statistically significant effect on teen pregnancy (Deming 2009), but the effect of Head Start on total adult fertility has not been studied.

To examine the relationship between Head Start access and fertility, we use a robust difference-in-difference approach, leveraging variation from the rollout of Head Start across counties and over time between 1966 and 1980 for cohorts born 1955 through 1975. We find that Head Start access does not affect adult fertility on either the extensive or intensive margins. Our results suggest that there is no meaningful change in the share of the cohorts who have any children nor in the total number of children born to women before age 45.

This study contributes to the growing literature on the long-term and intergenerational effects of War on Poverty programs (e.g., Bailey et al. 2023; Bailey, Sun, and Timpe 2021; Barr and Gibbs 2022; Kose, O'Keefe, and Rosales-Rueda 2024) by examining later-in-life fertility responses as an indicator of adult socioeconomic well-being.

I. The Head Start Program

Head Start started in 1965 as a part of the War on Poverty initiative during Lyndon Johnson's presidency. It aims to close socioeconomic gaps in school readiness by providing education, nutrition, and health services for economically disadvantaged preschool-aged children.

Head Start rollout data are from Bailey, Sun, and Timpe (2021). Treatment exposure is defined by the year that each county obtained any Head Start funding from 1966 to 1980 and the age of the mother when Head Start arrived in her county. Figure 1 shows the geographic and time variation in the program rollout. Counties shaded in yellow and light green were early adopters, and the darkest blue represents later-adopting counties. Unshaded counties did not adopt Head Start until after 1980. By 1980, more than 1,500 counties had launched this program.

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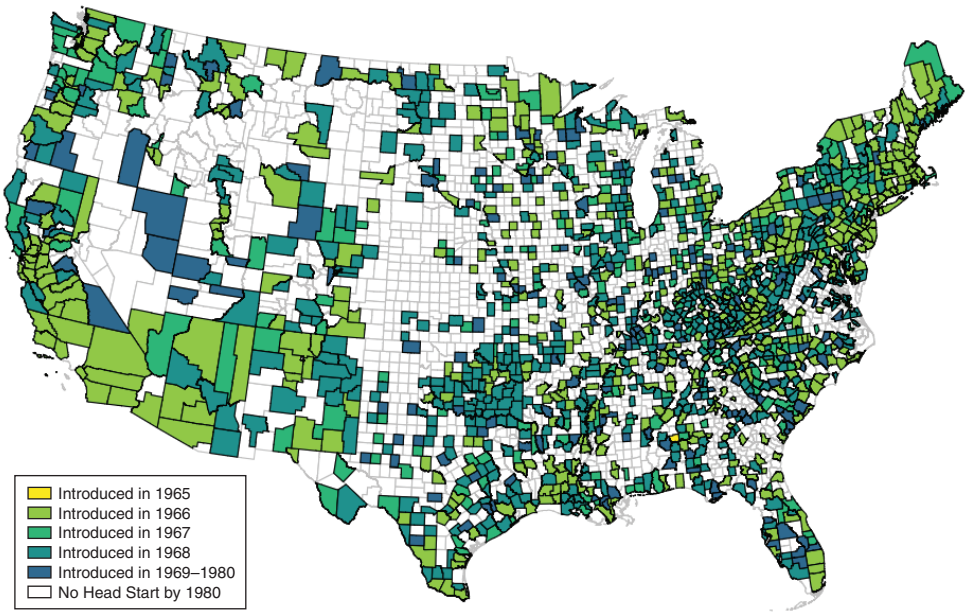

Figure 1. Timing of Head Start Adoption and Geographic Variation, 1966–1980

Source: Data are compiled by Bailey, Sun, and Timpe (2021) using National Archives and Records Administration, Community Action Programs, and Federal Outlay System Files.

II. Data

Our primary data source is the Vital Statistics Natality data from the National Center for Health Statistics from 1970 to 2019. 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 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 when giving birth to assign maternal exposure to Head Start.

We follow the procedure of Currie and Schwandt (2014) to create two measures of fertility that capture the extensive and intensive margins. To capture the extensive margin, we calculate the number of women in the county–birth year cohort with any children by totaling all the first births observed to these women. For the total fertility measure (intensive margin), we add all births observed to women in a cohort before age 45, as that is the latest age we observe all cohorts. Both of these measures are divided by the total number of women in the birth cohort to create the proportion of women with any children and the total fertility rate. Data on cohort size by county are from Bailey et al. (2016).

III. Methods

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 fertility, we use a difference-in-difference design and a flexible event study framework. For computational ease, we collapse the Vital Statistics data into county and mother's birth-year cells. Our static specification is the following:

$$Y_{ct} = \alpha + \beta HeadStart_{ct} + \theta_c + \mu_{st} + \varepsilon_{ct}, \tag{1}$$

where Y_{ct} is either the proportion of women with children or the total fertility rate for a cohort of women in county c and born in year t.

$HeadStart_{ct}$ captures maternal exposure to Head Start based on the mother's birth year and county of residence at child's birth. $HeadStart_{ct}$ is equal to 1 if Head Start was available in county c when the mother 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 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.[1]

θ_c represents county (of residence at child's birth) fixed effects that absorb time-invariant geographic characteristics that could be correlated with the Head Start rollout. We also include state-by-year fixed effects (μ_{st}) to account for changes in state policies that could differentially affect mothers' birth cohorts (Bailey, Sun, and Timpe 2021). Regressions are weighted by the number of women in the cohort, and standard errors are clustered at the county level.

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

$$Y_{ct} = \lambda D_c \mathbf{1}\{AgeTreat_{ct} \leq -6\} + \sum_{a=-5}^{12} \tau_a D_c \mathbf{1}\{AgeTreat_{ct} = a\} + \nu D_c \mathbf{1}\{AgeTreat_{ct} \geq 13\} + \theta_c + \mu_{st} + \varepsilon_{ct}, \tag{2}$$

where D_c is a binary treatment variable equal to 1 if the county ever receives a Head Start grant. Indicator variables, $\mathbf{1}\{AgeTreat_{ct} = a\}$, 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). As ages older than six are not treated, the evolution of the coefficients for those ages tests the parallel trends assumption that in the absence of the program, counties with and without Head Start would have had similar trends in fertility outcomes.

To address the potential biases from treatment effect heterogeneity that may contaminate staggered designs like ours (Goodman-Bacon 2021), we estimate these specifications using Sun and Abraham (2021). Our control group is the set of counties that have not adopted Head Start by 1980.

As counties that introduced Head Start before 1980 may be different than those that did not, our preferred estimates use inverse propensity score weights to make control counties more comparable to treated ones. In particular, we reweight untreated counties using an estimate of the propensity of Head Start adoption in the county by 1980 ($\hat{p}$) based on a logit model of preprogram characteristics.[2] For control counties (no Head Start by 1980), we weight the cells by the size of the cohort multiplied by $\hat{p}/(1-\hat{p})$. For treated countries, the weight is left as the size of the cohort.

IV. Effects on Fertility

Table 1 presents the static effects of Head Start exposure on the extensive margin (any children) and the intensive margin (the total number of children) of fertility. For each outcome, the first column

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

[2] 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 program transfers per capita, median family income, and the number of doctors per capita in 1960.

Table 1—The Effect of Mother's Exposure to Head Start on Fertility

	Any children		Total fertility rate	
	(1)	(2)	(1)	(2)
Maternal HS exposure	0.0094	0.0057	0.0391	0.0251
	(0.0177)	(0.0098)	(0.0388)	(0.0215)
Adj. R^2	0.832	0.858	0.853	0.876
Observations	60,841	60,841	60,841	60,841
State-by-birth-year		Y		Y

Notes: Sample includes mothers who were born between 1955 and 1975. All regressions include fixed effects for mother's birth year and county of residence at birth. Standard errors are clustered at the county level. HS is Head Start.

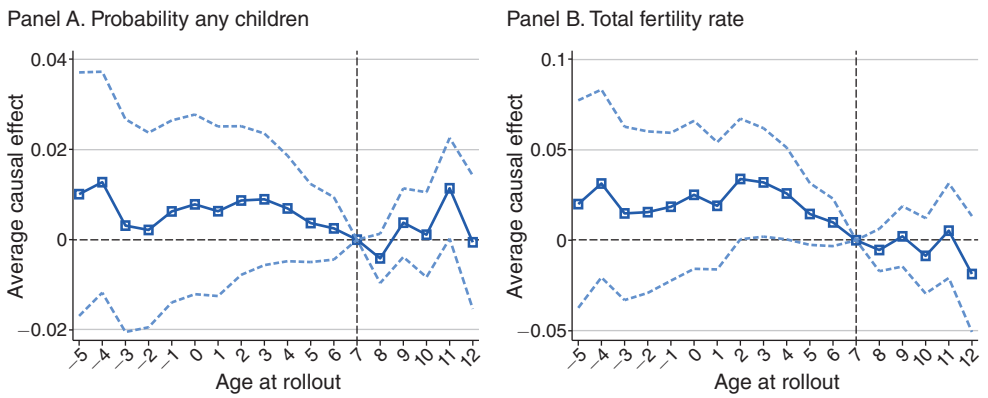

Figure 2. Dynamic Effects of Head Start on Completed Fertility

Notes: Sample corresponds to women born between 1955 and 1975. This figure plots the estimated coefficients and 95 percent confidence intervals obtained from equation (2). The *x*-axis corresponds to maternal age at Head Start rollout. The omitted category is age seven. We bin distant observations including maternal ages older than 12 years and maternal ages less than age 6 (not reported in the figure).

estimates a baseline specification that includes fixed effects for mother's birth year and mother's county of residence at child's birth. The second column presents our preferred specification that adds mother's birth-state-by-birth-year fixed effects. The results from both specifications indicate that early life exposure to Head Start does not affect later fertility for adult women.

Turning to the dynamic effects, Figure 2 plots the event study coefficients of maternal exposure to Head Start by age at rollout using our preferred specification. Given the nature of the Head Start program and the staggered implementation, mothers of ages four to six were partially exposed (only a few preschool years), while mothers who were of age three or younger when their county received a Head Start grant were fully exposed for all their preschool years. In contrast, mothers older than six at Head Start rollout would not 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.

Figure 2 shows that women partially or fully exposed to the program did not meaningfully change their fertility choices. Panel A shows small and statistically insignificant effects on the fraction of women in a cohort who have any children. Similarly, the effect on the total fertility rate by age 44 is relatively small and generally insignificant. There is also no evidence of significant differential pre-trends in fertility.

To place our results in context, East et al. (2023) examined the intergenerational effects of access to health care through Medicaid and found no effect of early life exposure to Medicaid coverage on later fertility. Becker and Lewis (1973) discuss the trade-offs between the "quantity" and "quality" of children, a framework often used to explain the negative relationship between women's education and fertility. However, the prior literature finds mixed results. For example, Currie and Moretti (2003) show that increases in women's education through access to university openings led to a reduction in expected fertility, while McCrary and Royer (2011) find increased schooling driven by variation in school entry policies has no effect on later fertility.

V. Conclusion

This study uses the rollout of the Head Start program for cohorts of women born between 1955 and 1975 to examine the effect of maternal early childhood education on adult fertility outcomes. Using both a static estimation and a flexible event study design, we find that the introduction of the Head Start program did not change intensive or extensive margin fertility patterns for women exposed to the program.

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