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MORE HOURS, MORE WORK: HEAD START EXPANSIONS BOOST MATERNAL EMPLOYMENT

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ABSTRACT

Women's employment remains highly sensitive to childcare constraints, making childcare availability a critical lever for supporting mothers' labor force attachment. We study the effects of expanded full-day programming in Head Start, using the 2016 federal funding initiative that targeted grantees with low full-day enrollment. Linking administrative program data, geo-coded center locations, and household data on employment, we estimate a difference-in-differences design by comparing mothers of young children in treated and untreated areas. The policy increased full-day enrollment by 19 percent and raised single mothers' employment (1.9%), hours (2.5%), and earnings (6.5%). Results show that extending program duration meaningfully improves maternal labor market outcomes.

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1 Introduction

Understanding the causes and consequences of women's labor force participation has been the focus of much economic inquiry, particularly given women's pronounced entry into the labor force over the last five decades. Specifically, there have been notable increases in participation among mothers of young children. A large body of research documents that women's career paths are more likely than men's to be interrupted by childbirth and caregiving responsibilities, making them particularly vulnerable to the disruptions of parenthood (Kleven, Landais and Søgaard, 2019; Kleven, 2025). However, evidence also indicates that policies supporting mothers' continued participation in or return to work after childbirth, and those helping them balance professional and domestic roles, can yield meaningful short- and long-term benefits (Kuka and Shenhav, 2024).

One prominent example of such policies is public investment in childcare availability and affordability. These programs serve a dual purpose: supporting children's development and facilitating parental labor force participation. The relationship between child care and parents' labor supply is grounded in the economic theory of intrahousehold decisions regarding market work and home production (Becker, 1965). Consistent with this framework, empirical evidence suggests that mothers do indeed respond to increased provision of, access to, and affordability of early childhood care and education (ECE) (Gelbach, 2002; Morrissey, 2017).

In recent years, ECE programs have increasingly expanded instructional time by lengthening program days and years through the growth of full-day preschool. Figure 1 illustrates trends in full- and part-day preschool participation over time, showing that the share of children enrolled in full-day pre-K began rising around mid-2010s, while part-day enrollment declined. This shift reflects a combination of federal, state, and local initiatives to extend program hours. In this paper, we focus on the federally funded Head Start program and examine how access to full-day Head Start affects maternal labor supply by exploiting a recent policy change that expanded the duration of Head Start programs. In 2016, the U.S. Department of Health and Human Services announced the availability of $294 million dollars in supplemental funds to extend the duration of Head Start services. Head Start grantees serving fewer than 40 percent of their center-based slots for a full school day and full school year were eligible to apply.

Our analysis leverages this eligibility rule by combining Head Start full-time enrollment data from the Program Information Reports (PIR) with geo-coded center locations from the Office of Head Start. We link these data to maternal employment information from

the 2008-2019 American Community Survey (ACS) to explore the impact on labor supply outcomes.

First, we identify Head Start grantees eligible for supplemental funding based on their share of enrollment in full-day programming in 2015 (before the policy change). Using their center locations, we then define geographic areas (public use microdata areas or PUMAs, in the ACS) as "treated" if they were served by one or more centers affiliated with eligible grantees, and as "untreated" otherwise.[1] We estimate the effects of the 2016 expansion using a difference-in-differences design by comparing changes in outcomes for single mothers with young children in PUMAs with Head Start centers operated by eligible grantees to those without, before and after the policy implementation. This approach controls for time-invariant local characteristics and common shocks, and we further assess its validity through event-study specifications that test for pre-treatment parallel trends. We focus on single mothers of young children, as they are both more likely to be eligible for Head Start and their labor supply is typically more sensitive to changes in childcare availability. Nationally representative surveys on household structures of Head Start children show that, in the fall of 2014, approximately 59 percent of Head Start children lived in households headed by the mother only or in which the parents were unmarried Aikens et al. (2017). By 2019, that figure had grown to 68 percent (Kopack Klein et al., 2021).[2]

We find that the 2016 duration expansion significantly increased full-time Head Start enrollment (≈19% of the pre-policy mean), resulting in approximately 100,000 slots shifting from half-day to full-day, which is consistent with the stated policy goal of "up to 135,000 children" (Rodriguez and Parrott, 2016).[3] In turn, single mothers of young children significantly increased their labor force participation by 1.5 percentage points (1.9% of the pre-policy mean) and their weekly work hours by 0.7 (2.5%) relative to comparable mothers in untreated PUMAs. Exposure to Head Start full-day expansions also resulted in higher average wage income (6.5%). Our event studies provide evidence of parallel pre-trends and show that the effects of full-day expansions emerge gradually over time, likely because Head

[1] Untreated PUMAs are those with Head Start centers but none operated by eligible grantees. Thus, mothers in the comparison group live in places served by Head Start but are unaffected by the policy change.

[2] In 2014, 34 percent of Head Start children lived in married-parent households while 3 percent lived in father-headed households and 4 percent lived with neither parent. In 2019, only 22 percent of Head Start participants were in married-parent households, 6 percent were with fathers only, and 4 percent were living with neither parent.

[3] Total Head Start enrollment was declining slightly in this time frame (Office of Head Start, 2014, 2020). When we apply our estimate of 19% growth in full-day slots in total Head Start enrollment, holding total slots fixed, to 2016 program enrollment as measured in aggregate program records or our analytic sample, we calculate that expansions led to approximately 100,000 slots transitioning from part- to full-day.

Start centers adjust their programming and families respond to the new availability of full-day slots.

We demonstrate that our results are very similar using a continuous measure of treatment exposure (i.e., the share of treated Head Start centers in a PUMA) and are largely robust to alternative specifications that control for time-varying measures of state pre-kindergarten programming, flexible trends in both 2015 state preschool enrollment and the 2015 unemployment rate, and PUMA-specific linear trends. Moreover, in placebo tests, when we estimate the impact of exposure to full-day expansions on mothers who are unlikely to be exposed to Head Start (i.e., mothers of older children, ages 5-6 and 7-11, who have a college education or who are married), we find no effects on maternal employment. Taken together, the estimated effects change little across alternative treatment measures, controls, and samples.

This study contributes to several strands of the existing literature, including a growing body of work on the effects of universal or near-universal ECE programs on parental employment, which generally finds mixed evidence. For example, Baker, Gruber and Milligan (2008) find substantial increases in maternal labor force participation following Quebec's introduction of universal, low-cost child care. In contrast, the U.S. studies of state preschool expansions often find limited average effects. Fitzpatrick (2010) shows minimal impacts on maternal labor supply from universal pre-K in Georgia and Oklahoma despite substantial increases in preschool enrollment. Subsequent studies suggest that maternal employment effects in these contexts are concentrated among lower-income families (Cascio and Schanzenbach, 2013) and single mothers whose focal child is the youngest in the household (Fitzpatrick, 2012). More recent research using a broader set of preschool programs across U.S. states documents effects among more advantaged mothers, suggesting expansions may affect families on different margins (Ilin, Shampine and Terry, 2022). Additionally, a recent study of universal preschool in New Haven, Connecticut, finds large and persistent effects on parental earnings, driven primarily by fathers, in a contemporary setting (Humphries et al., 2024).

Evidence on Head Start's impact on maternal employment is similarly mixed, likely reflecting both variation in programming and its historically limited hours of operation. Pihl (2022) finds negative and persistent effects on maternal employment in the long run using a regression discontinuity design based on the 1965 poverty thresholds used for grant writing assistance during the program's introduction. In contrast, more recently, Wikle and Wilson (2023) find evidence that 1990s Head Start enrollment expansions and exposure to Head Start via the 2002 randomized Head Start Impact Study increased employment and earnings of single mothers. Other studies using the Head Start Impact Study document heteroge-

neous effects on mothers' employment and own educational attainment, particularly by age of the child at Head Start enrollment (Sabol and Chase-Lansdale, 2015; Schiman, 2022). Importantly, no existing work explores the impact of the most recent, pronounced policy changes to Head Start programming, which supported the expansion of full-day enrollment at Head Start centers nationwide.

While the literature has established that childcare availability *can affect* maternal employment, several gaps remain. First, most studies focus on the introduction of new programs rather than expansions of existing ones, which may have different effects due to selection and baseline conditions. Furthermore, those treatment effects often average the impacts of different programs' characteristics and features (i.e., the average effect of full- and part-day programming when both structures exist within a program). Second, relatively few studies examine the dynamics of employment responses, particularly whether effects grow over time as programs scale up and families adjust to new childcare arrangements. Finally, policies that affect mothers in earlier career stages, i.e., when their children are younger, could be particularly impactful on their career trajectories. Head Start, for example, increasingly enrolls younger children, as states' public pre-kindergarten programs expand their coverage of four-year-olds (Office of Head Start, 2024). Therefore, recent policy changes provide new opportunities to study early childhood programs and parents' employment decisions in contemporary contexts. Our evidence highlights the role of program design, specifically, the length of the school day, in shaping parental labor supply responses.

2 Study Background

The economic theory underlying child care's role in maternal employment decisions builds on the standard labor supply model, in which mothers consider the costs and benefits of market work relative to home production. Childcare costs represent a significant barrier to employment, particularly for low-skilled mothers whose potential wages may barely exceed childcare expenses (Blau and Currie, 2006). Policies that lower these costs, including direct public provision or subsidies, reduce the effective cost of employment for mothers facing this tradeoff, which can shift their labor supply curve outward, increasing both employment likelihood (extensive margin) and hours worked (intensive margin). Such policies should have the largest effects on mothers who are on the margin of employment decisions; typically, single mothers, mothers with limited education, or those with low potential wages (Kimmel, 1998). Our study provides a direct test of the importance of childcare subsidization for low-income families in a recent policy context.

A substantial empirical literature has examined the impact of early childhood care and education programs on maternal employment, although results vary across countries, program structures, and historical contexts. Some of the influential evidence on the effects of child care on maternal employment comes from studies of universal childcare programs in economically developed countries. While researchers find evidence of substantial increases in maternal employment in the Quebec policy context (Baker, Gruber and Milligan, 2008), mothers' responses to subsidized child care in Norway and France's public preschool program were more muted, with some small effects for single and less-educated mothers in the latter case (Havnes and Mogstad, 2011; Goux and Maurin, 2010).

In a historical context, Herbst (2017) examines federally funded child care during World War II under the Lanham Act, which operated from 1943 to 1946, and finds substantial increases in maternal employment in states with higher childcare spending. Cascio (2009) examines the introduction of state-funded kindergarten programs across the U.S., primarily in the 1960s and 1970s, and finds concentrated effects among single mothers with no younger children. Perhaps consistent with the nature of historical programming and labor market conditions, work on the earliest iteration of the Head Start program documents some heterogeneity in employment responses in the short run, but finds negative effects on average in the long run (Pihl, 2022).

More recent evidence on both Head Start and universal preschool programs generally shows limited or null effects when looking at all women, and positive employment effects of ECE availability or access for marginal mothers, such as those who are not married, have low levels of education, and have no younger children in the home (Cascio and Schanzenbach, 2013; Fitzpatrick, 2010, 2012; Wikle and Wilson, 2023).[4] Two recent studies suggest the impact may be broader-based with effects of state public preschool programs and full-day kindergarten expansions affecting mothers' labor force participation (Gibbs, Wikle and Wilson, 2025; Ilin, Shampine and Terry, 2022). In their investigation of the impact of full-day kindergarten changes across states over three decades, Gibbs, Wikle and Wilson (2025) document effects on the employment of mothers with kindergarten-aged children, reinforcing the importance of program hours in mothers' employment decisions and providing a useful comparison for understanding (more targeted) Head Start duration expansions.

[4]One recent paper examines the effects of Early Head Start rollout on maternal employment and finds effects concentrated among low-educated and single mothers (Raza, 2025).

2.1 Policy Context

In the United States, the landscape of early childhood care and education is a patchwork of both public and private providers with programs operating in schools, centers, and homes. Major federal investments in ECE include childcare subsidies and support for state preschool programs, both operated through block grants to the states, as well as the Head Start program, established in 1965 as a cornerstone of the War on Poverty.

Head Start is the largest and only federally funded early childhood education program in the United States. It intends to prepare children from low-income families for formal schooling and to support families in facilitating their children's healthy development. The program provides comprehensive services that include early education, health, nutrition, and family support. Today, Head Start and its counterpart for younger children, Early Head Start, together serve approximately 800,000 children, reaching roughly 5–10 percent of all U.S. preschool-aged children in the last decade (Friedman-Krauss et al., 2025; Office of Head Start, 2024). The program primarily targets children from families with incomes below the federal poverty line, as well as children experiencing homelessness, those in foster care, and children with disabilities, who are eligible for a fixed number of slots regardless of income. The program has evolved over time, with greater focus on school readiness and academic preparedness, monitoring and evaluation, and availability of slots for working parents contingent with welfare reform in the 1990s.

In its earliest iteration, Head Start was a summer program in the months leading up to formal school entry, and it was fairly low dosage. It quickly pivoted from a summer program to a school-year program, with any remaining summer-only programs required by Congress to convert in 1982 (Office of Head Start, 2025). While there was discussion and encouragement of expanding program duration or intensity in 1986, 1997, and 1998, previous Head Start expansions focused on expanding the total number of children served (Office of Head Start, 2025). From 2008 (when detailed data became available) until 2016, Head Start programming was full-day (i.e., operated for the number of hours per day of a regular school day and every day of the school week) for approximately 55 percent of enrollees (Appendix Figure A.1).

On April 25, 2016, the Office of Head Start announced the availability of approximately $294 million in supplemental funding authorized under the Consolidated Appropriations Act of 2016 (U.S. Congress, 2015). The goal was to extend the duration of services and increase the share of children receiving a full school day and year, as research shows that longer program hours improve child outcomes (Office of Head Start, Administration for Children and Families, 2016). Importantly, eligibility for Head Start funds was restricted to programs

operating less than 40 percent of their center-based slots at the 1,020-hour standard (full-day, full-school-year). This cutoff excluded grantees already meeting or exceeding the threshold. Applications were voluntary and due by June 24, 2016, and successful applicants were required to implement extended service hours by the 2017–2018 program year. Subsequently, the new funding became part of the base grants for the awarded programs.

We use this variation in funding eligibility to estimate the impact of expanded Head Start programming duration on maternal employment outcomes. Using the best available information on which programs were likely eligible for funding, we match this information to maternal employment outcomes by geography, as discussed below. We next describe the data used in the analysis.

3 Data

This study utilizes two primary data sources. To measure impacts on Head Start enrollment, we use the Program Information Report (PIR) data from 2008 to 2019 (Office of Head Start, Administration for Children and Families, 2025). To examine the effects of the policy change on maternal employment, we combine the PIR data with the American Community Survey (ACS) for the same years (Ruggles et al., 2025).

The PIR is a publicly available administrative dataset collected annually by the Office of Head Start (OHS) for the program years labeled by the spring of the service year (e.g., 2017 refers to the 2016-2017 service year). It provides standardized, program-level information on Head Start services, including detailed data on children, families, staffing, and enrollment. All federally funded Head Start grantees and their delegate agencies are required to submit a PIR, making it a comprehensive source for program-level analysis. For this study, we restrict our sample to Head Start-only (HS) programs, excluding Early Head Start, American Indian and Alaska Native, and migrant/seasonal programs.[5]

Using the PIR data, we construct a measure of full-time enrollment for each grantee-program unit as the share of children enrolled full-time out of the total enrollment. Appendix Figure A.1 displays the average share of full-time enrollment in Head Start programs from 2008 to 2019. Over the study period, full-time enrollment remained relatively stable through

[5]We exclude Early Head Start (EHS) because it is a distinct program serving pregnant women and children under age 3, and its service mix includes home-based, family child care, and other program options that differ from Head Start preschool. In 2015-2016, less than 50% of EHS slots were center-based preschool, while for Head Start, 97% of services were (Office of Head Start, 2016). Head Start programs also serve infants and toddlers, and 42% of program grantees operate both Head Start and EHS, while one-quarter operate only EHS programs. We also exclude Alaska, Hawaii, and all US territories from our analysis due to differences in geographic definitions and data availability.

the early and mid-2010s, fluctuating around 55 percent. Beginning in 2017, however, the figure shows an upward trend, with the mean share of full-time enrollment increasing and reaching its highest point in 2019 (75 percent), consistent with the timing of the policy change.

Using these data, we identify grantee programs with less than 40 percent full-time enrollment in 2015 (the year before the policy was announced), the threshold used to determine eligibility for supplemental funding. We classify these grantee programs as *treated* (eligible to apply), and those with 40 percent or more full-time enrollment as *untreated* (ineligible to apply). Figure 2 shows trends in the mean share of full-time Head Start enrollment by treatment status. Untreated grantee programs, those with 40 percent or greater full-time enrollment in 2015, show consistently high levels of full-time enrollment throughout the period, with little variation around a mean of approximately 90 percent. In contrast, treated grantee programs, those with less than 40 percent full-time enrollment in 2015, show significantly lower baseline rates, averaging around 20 percent over the study period. For treated grantee programs, the share of full-time enrollment remained relatively flat from 2008 through 2016. Following the policy intervention marked by the April 2016 grant announcement and the June 2016 application deadline, an upward trend emerges in the 2016-2017 program year. By 2019, the share of full-time enrollment among treated grantees nearly doubled, reaching approximately 40 percent.[6] This figure also provides suggestive visual support for the parallel trends assumption underlying our difference-in-differences design. Prior to the 2016–2017 program year, both treated and untreated grantee programs exhibited relatively stable and non-converging trends in full-time enrollment rates, with treated programs consistently operating at lower levels of full-time provision.

A key feature of Head Start service provision is that some grantees operate across multiple counties; however, this cross-county coverage is not captured in the grantee-level PIR data. To address this limitation —and to better measure the geographic coverage of Head Start programs and determine geographic exposure to eligible grantee-operated programs —we supplement the PIR with center-level data, which provide detailed information on the physical locations of service delivery. For this, we use the center locations obtained from the Head Start Center Locator in 2015 (Office of Head Start, Administration for Children and Families, 2015).[7] We merge the geocoded center location data with the grantee-program

[6]We observe 80 percent of eligible grantees with an increase in the share of full-time enrollment in the post-period. Given that these funds were available as a voluntary supplement to existing grantees, not all eligible grantees applied.

[7]The Head Start Center Locator is an interactive tool provided by the Office of Head Start (OHS) that enables users to search for Head Start service locations by city, ZIP code, state, or grant number.

data using the grant number, program number, grantee name, program name, and program state information. Since the geographic level of the labor outcomes data in the ACS is at the PUMA level, we assign PUMA information to centers and grantees as follows: for centers, we use their longitude and latitude to find the corresponding PUMA; for grantee programs, we geocode their administrative addresses. For context, there are 1,519 grantee programs listed in the PIR that serve 13,706 centers in 2015. The resulting PUMA coverage highlights the value of the center-level data: while grantee addresses map to 1,058 PUMAs, the centers themselves are located in 2,081 unique PUMAs (out of 2,490 U.S. PUMAs in total), providing far greater geographic exposure and variation.

Using the PIR and center merged data, we construct two primary treatment variables. First, we define a binary indicator that equals one if any Head Start center in a given PUMA is affiliated with a grantee program eligible to apply for supplemental funding, and equals zero for PUMAs that contain Head Start centers but none affiliated with eligible grantees.[8] Second, we construct a continuous measure capturing the fraction of centers within a PUMA that are affiliated with eligible grantee programs. These variables capture both the presence and intensity of exposure to the policy intervention at the local level. Appendix Figure A.2 displays the PUMA-level variation in centers affiliated with eligible grantees. We merge these treatment variables, both defined based on 2015 program eligibility, with individual-level data from the ACS through IPUMS USA to analyze maternal employment outcomes.

The ACS is a nationally representative, continuous survey administered by the U.S. Census Bureau that collects detailed demographic, social, economic, and housing information from a large sample of households annually. Its scope and consistency make it well-suited for studying population-level trends over time. We use 12 years around the policy introduction, 2008–2019.[9] PUMAs are the most granular geographic unit available in the ACS public-use microdata, each containing at least 100,000 residents. Although PUMAs do not align perfectly with county boundaries, they offer sufficient spatial resolution to approximate local exposure to Head Start policies and full-day enrollment availability across communities. This approach enables us to link variation in local Head Start program offerings to labor market outcomes of mothers residing in those geographic areas.

Our sample of interest comprises single mothers with young children aged zero to four. We focus on single mothers for two reasons. First, as Head Start is a targeted program, single

[8] Results are robust to the inclusion of PUMAs with no Head Start centers.

[9] ACS respondents take the survey throughout the calendar year, so time of survey may correspond to two different academic years if taken in the beginning of the calendar year or the end of the calendar year (e.g., in the first post-policy year, 2017, respondents are providing data during the 2016-2017 school year if they respond in the first half of 2017 and the 2017-2018 school year if surveyed in the latter half of the year).

mothers are more likely to meet its income eligibility criteria than their married counterparts. Second, single mothers face greater childcare constraints. Consequently, their labor supply decisions are expected to be the most sensitive to the availability of full-time, subsidized care (e.g., Blau and Tekin, 2007; Gelbach, 2002).

We present summary statistics for outcomes in Appendix Table A.1. On average, compared to all mothers of young children (Column 2), single mothers (Column 3) are more likely to work, be non-White, and be Hispanic, and they report lower wages from the previous year. Additionally, 58% of this group are exposed to the Head Start expansions (live in a PUMA with at least one center affiliated with an eligible grantee).

4 Methods

We estimate the impact of the Head Start full-day expansion policy using a difference-in-differences (DD) design. Formally, we estimate the following specification:

$$y_{ipt} = \alpha + \delta Treatment_p \times Post_t + X_i + \mu_p + \gamma_t + \epsilon_{ipt} \tag{1}$$

where y_{ipt} denotes the outcome of interest (e.g., full-time Head Start enrollment or maternal employment) for grantee/individual i located in PUMA p in year t. The variable Treatment$_p$ is either (1) an indicator equal to one if the PUMA is exposed to at least one Head Start center affiliated with a grantee program that was eligible for the supplemental full-day funding in 2015 or (2) the share of centers within a PUMA that are affiliated with eligible grantee programs in 2015. Post$_t$ is an indicator for post-policy years, defined as post-2016, following the implementation of the funding initiative. X_i includes race and Hispanic indicators in models with maternal outcomes. The term μ_p denotes PUMA fixed effects to account for time-invariant local characteristics, and γ_t represents year fixed effects to control for common time shocks. The error term ϵ_{ipt} is clustered at the PUMA level to allow for correlation in outcomes within geographic areas over time. The coefficient δ captures the average treatment effect of exposure to the Head Start full-day expansion policy on the outcome of interest.

In the first stage, we use the PIR data to estimate the causal effect of the policy change on full-time Head Start enrollment collected at the grantee-program-service year level. Because the geographic unit in the outcomes data (ACS) is the PUMA level, we assign treatment at the PUMA level and map each grantee-program to a PUMA based on the locations of its affiliated centers in 2015, as described in the Data Section. As a robustness, we also report

first-stage results using treatment assigned directly at the grantee-program level.

We then examine the impact of the policy on maternal employment outcomes using individual-level data from the ACS. In addition to Equation 1, we also estimate an event-study version to examine dynamic responses in labor market outcomes as follows:

$$y_{ipt} = \alpha + \sum_{j=2008, j\neq 2015}^{2019} \delta_j (Treatment_p \times 1[t = j]) + X_i + \mu_p + \gamma_t + \epsilon_{ipt} \quad (2)$$

In this specification, we interact the Treatment_p indicator with year dummies. Each year dummy variable equals one when the time period matches that specific year and zero otherwise. The coefficients, δ_j, estimate the difference in outcomes between treated and untreated locations in year j relative to the base year, 2015. The coefficients for 2008-2014 represent the period before the policy was implemented. We expect the pre-treatment coefficients to be close to zero, indicating that treatment and comparison groups followed similar trends before the policy change. The coefficients for 2016-2019 capture the effects after the policy, showing how the policy's impact evolved over time.

5 Results

5.1 First Stage Results: Effect on Full-Time Head Start Enrollment

Table 1 presents the first-stage estimates of the effect of the Head Start full-day expansion policy on the share of full-time enrollment at the grantee-program level, using two alternative definitions of PUMA-level treatment. We interact the treatment indicators with a post-2016 dummy to capture the differential change in full-time enrollment following the policy implementation. All specifications include PUMA and year fixed effects, and results are shown both with and without population weighting, using the number of children ages zero through four.

Panel A uses a binary treatment indicator equal to one if the PUMA contains at least one Head Start center affiliated with a grantee program that was eligible for supplemental full-day funding. In the weighted specification (Column 1), exposure to the policy is associated with a 10.45 percentage point increase in full-time enrollment (≈19% increase relative to the baseline mean of ≈54% full-time enrollment). The unweighted estimate in Column 2 is slightly larger at 12.28 percentage points (≈26% increase relative to the mean of ≈47% full-time enrollment). Both of these estimates are statistically significant at the 1% level. Panel B reports results using a continuous treatment measure: the share of Head Start centers

in a PUMA affiliated with eligible grantee-programs. The estimated effects are 13.59 and 13.67 percentage points in the weighted and unweighted specifications, respectively, both statistically significant at the 1-percent level.

We test the sensitivity of the first-stage results to different model specifications in Appendix Table A.2. Column 1 reproduces the baseline estimates reported in Table 1. In Column 2, we account for contemporaneous policy changes and differential trends by controlling for time-varying state preschool enrollment. Column 3 includes flexible trends based on 2015 state preschool enrollment, and Column 4 adds flexible trends based on 2015 unemployment rates. While our first-stage effects are largely robust to these alternative specifications, the addition of PUMA-specific linear time trends in Column 5, a more demanding test, leads to smaller point estimates that are less precisely estimated.

As an alternative approach, we estimate the first-stage effect using grantee-program-level treatment status (a more granular measure of treatment exposure than the PUMA-level treatment), interacted with a post-2016 dummy. Specifically, we classify grantee programs as "treated" if they were eligible to apply for supplemental full-day funding, and "untreated" otherwise, based on PIR data. This analysis does not require geographic assignment to PUMAs or the construction of aggregate PUMA-level treatment variables (in contrast to the PUMA-level treatment used with the ACS). Instead, the grantee-level treatment variation provides a more direct assessment of the policy's effect by comparing changes in full-time enrollment between eligible and ineligible grantee programs before and after the policy change.[10] Appendix Table A.3 presents these results. Overall, using finer-grained treatment variation confirms that the policy led to a sizable increase in full-time Head Start enrollment, and the estimates remain robust across all specifications, further validating the first-stage relationship.

In sum, our first-stage results show that the policy change led to a sizable and meaningful increase in full-time Head Start enrollment. To better understand counterfactual enrollment choices among single mothers—since standard household surveys lack childcare enrollment data—we turn to the Early Childhood Program Participation (ECPP) component of the National Household Education Survey, which provides a limited snapshot. Among all young children in single-parent families, there was no overall change from 2016 to 2019 in the shares who had at least one regular, nonparental care arrangement in a typical week, or those who only experienced parental care. The proportion in weekly center-based care arrangements increased from 2016 to 2019 by three percentage points (from 36 percent to 39 percent), while

[10]Note that while this analysis does not rely on PUMA-level treatment assignment, we test the robustness of adding PUMA-specific linear using the existing grantee-PUMA mapping described in the 3 Section.

the share in non-relative care, including home-based child care, decreased from 15 percent to 11 percent (Corcoran and Steinley, 2019; Cui and Natzke, 2021).[11] The next subsection presents maternal labor market responses.

5.2 Maternal Outcomes

Table 2 examines the impact of Head Start expansions on labor market outcomes for single mothers with young children aged zero through four-years old, controlling for race/ethnicity, and PUMA and year fixed effects. We consider two measures of treatment exposure interacted with a post-2016 dummy: Panel A uses the presence of at least one treated Head Start center in a PUMA, while Panel B uses the share of treated centers within each PUMA.

Overall, we find that exposure to Head Start full-day expansions led to statistically significant and economically meaningful improvements in single mothers' labor supply and earnings. In Panel A, at the extensive margin, the policy increased employment by 1.5 percentage points, corresponding to approximately a 1.9 percent increase over the pre-policy mean of 77.6 percent. Based on ACS population estimates, this employment effect corresponds to roughly 20,000 additional mothers in the workforce as a result of these expansions.[12] We are limited in our ability to scale estimates to implied treatment-on-the-treated effects because we are unable to observe Head Start enrollment, including full- or part-day, in the ACS data, making it impossible to directly estimate a two-stage least squares effect.

At the intensive margin, the increase of 0.7 weekly work hours represents a 2.5 percent gain relative to the pre-policy mean of 27.7 hours. Similarly, the 1.4 percentage point increase in the probability of reporting positive wage income translates into a 1.8 percent gain over the baseline mean of 74.9 percent, while the $1,153 increase in annual wage income amounts to a 6.5 percent increase relative to the pre-policy average of $17,701. Panel B yields comparable relative magnitudes, with employment rising by 2.0 percent, hours worked by 2.8 percent, wage receipt by 1.8 percent, and annual earnings by 7.6 percent. In sum, the consistent effects across both panels underscore that the estimated impacts are not only statistically significant but also economically meaningful, indicating that Head Start expansions produced notable improvements in labor supply outcomes of single mothers. Separately, married

[11] Young children are those who are five-years old and younger and not yet enrolled in kindergarten. Children can be captured in more than one category if they experience multiple care arrangements during a typical week.

[12] There were just under 2.3 million single mothers of young children in the U.S. in 2015 (according to ACS population estimates), and 58% of single mothers were in treated PUMAs as displayed in Table A.1. We estimate a 1.5 percentage-point increase in employment among this group in Table 2, corresponding to approximately 20,000 more mothers in the labor force. As discussed, we also show that the policy resulted in approximately 100,000 additional full-day slots in Head Start programming.

mothers of young children may also be exposed to Head Start if income-eligible. Appendix Table A.4 Panels C and D show that Head Start full-day expansions did not improve labor market outcomes of married mothers, and negative effects may suggest that income effects (from subsidized childcare hours) dominate substitution effects in two-earner households. In addition, to remain eligible, married mothers may be discouraged from entering the labor force or work more.

Evidence on the labor supply effects of childcare and preschool expansions is mixed and varies by policy design and context. Large-scale reforms such as Quebec's universal childcare program have generated substantial employment gains. For example, Baker, Gruber and Milligan (2008) estimate a 7–8 percentage point increase in maternal employment, about a 15 percent rise relative to baseline, making it one of the largest documented effects. In contrast, Havnes and Mogstad (2011) find near-zero overall effects of Norway's universal expansion, pointing to crowd-out of informal care. Similarly, Goux and Maurin (2010) show that access to public preschool in France increased employment mainly among single mothers, with negligible effects for two-parent households. Specific to Head Start, experimental evidence from the Head Start Impact Study suggests no average effect on maternal labor supply (Gelber and Isen, 2013), though Schiman (2022) documents important heterogeneity, with mothers of three-year old entrants increasing full-time employment by about 5 percentage points (≈15 percent relative to baseline). Pihl (2022), studying the introduction of Head Start, finds declines in employment among single mothers, suggesting that Head Start operated more as a social service than a childcare substitute.

More recent quasi-experimental work aligns more closely with our results: Wikle and Wilson (2023) show that increased Head Start funding in the 1990s raised single mothers' employment by nearly 2 percentage points (≈2–3 percent relative gain) and boosted hours and earnings, while Humphries et al. (2024) find that universal pre-kindergarten in New Haven increased parental earnings, underscoring that modern expansions can meaningfully relax childcare constraints. Taken together, these studies suggest that the labor supply effects of early childhood programs are highly context-dependent, with more recent expansions, particularly those offering full-day coverage, yielding modest but consistent gains in maternal employment and earnings, in line with our estimates.

Figure 3 presents event study results for four different outcomes relative to 2015, the reference year. All panels display estimated coefficients along with their 95% confidence intervals. The red vertical dashed lines indicate the policy implementation period. Pre-treatment coefficients for all four outcomes are close to zero and mostly statistically insignificant, indicating no differential trends between treatment and comparison groups before policy implementa-

tion, which support the parallel trend assumption. In the post-treatment period, the effects emerge gradually following the implementation of the expansion. By 2019, we observe positive and statistically significant effects across all outcomes. Single mothers in treated areas were more likely to be employed, worked more hours, had higher probabilities of reporting positive wage income, and experienced higher overall earnings. These results suggest that the labor supply responses to Head Start expansions materialize over time, consistent with the view that families require time to adjust to increased childcare availability before translating those changes into employment and earnings gains.

In Table A.5, we examine whether the effects vary by mother's race. Overall, the results suggest that Head Start duration expansions improved labor-market outcomes for both racial groups, with a more consistent pattern of detectable gains among White mothers.

As a placebo check, we estimate the effects of Head Start expansions on the labor supply of mothers whose youngest child is aged 5–6 and 7-11 separately, the groups unlikely to be directly affected by access to early childhood care. Table A.6 presents the results for this placebo sample across two subgroups less likely to be eligible for Head Start: married mothers (Column 1/3) and highly educated mothers[13] (Column 2/4). The estimated coefficients are small in magnitude and statistically insignificant across all labor outcomes. These null results provide reassurance that the positive effects we document for single mothers with young children are not driven by broader labor market shocks or spurious correlations with Head Start expansions, but rather reflect causal impacts on the population most directly exposed to the policy.

We show that our results are largely robust to alternative specifications, which are presented in Table A.7, where Column 1 reproduces our baseline specification. One potential concern is that contemporaneous changes in state preschool programs may confound our analyses. To explore and address this, Column 2 controls for time-varying state preschool enrollment, while Column 3 controls for flexible trends based on 2015 state preschool enrollment. In Column 4, we additionally include flexible trends based on 2015 unemployment rates to control for potential differential evolution in labor markets. Finally, in Column 5, we include PUMA-specific linear trends to control for local differential trends. Our findings are similar across specifications for most outcomes, which provides evidence that our results are not driven by contemporaneous policy changes or spurious trends.[14]

[13]Highly educated mothers are defined as those with a college degree or more.

[14]Panel D reports the robustness results for wage income, where the point estimate falls from 1,153 to 865 when we include PUMA-specific linear trends, and it becomes statistically insignificant. While the point estimate remains positive and of a similar order of magnitude, standard errors increase in this more demanding specification.

6 Conclusion

This study provides new evidence that expansions in the duration of Head Start services can yield meaningful labor market benefits for single mothers of young children. Using a difference-in-differences model that exploits variation in grantee eligibility for supplemental funding, we show that the 2016 federal initiative to extend Head Start hours significantly increased the share of children enrolled full-time. In turn, these expansions led to higher employment rates, greater work hours, and increased earnings among single mothers with young children.

These findings highlight that even marginal expansions in publicly provided early childhood education can alleviate childcare constraints and promote maternal labor force participation. The results underscore the importance of program design, particularly the availability of full-day care, in shaping the economic outcomes of low-income families. As policymakers continue to evaluate investments in early childhood education, our findings suggest that extending service duration can be a powerful tool for supporting both parental employment and potentially children's development.

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Tables and Figures

Table 1: The Effect of the Head Start Expansions on Full-time Head Start Enrollment: First Stage using PUMA-level Treatment

	Weighted	Unweighted
	(1)	(2)
A: At least one treated center in a PUMA		
PUMA had at least one treated center*Post	10.445***	12.278***
	(1.860)	(1.226)
Preperiod Mean Y (Percent)	53.96	46.95
% Effect (100*Coef/Mean)	19.36	26.15
Obs	8884	8884
B: Share of treated centers in a PUMA		
PUMA share of treated centers*Post	13.586***	13.669***
	(2.360)	(1.271)
Preperiod Mean Y (Percent)	53.96	46.95
% Effect (100*Coef/Mean)	25.18	29.11
Obs	8884	8884
PUMA FE	Y	Y
Year FE	Y	Y

Notes: This table reports estimated coefficients and standard errors using a difference-in-difference (DD) model described in Equation 1. The outcome is grantee-program enrollment. The treatment is assigned based on (1) a binary indicator that equals one if any Head Start center located in a given PUMA is affiliated with a grantee program eligible to apply for supplemental funding in Panel A, and (2) the fraction of centers within a PUMA that are affiliated with eligible grantee-programs in Panel B. The post dummy is an indicator for post-policy years, defined as post-2016. Column 1 uses the county population of 0 to 4-year olds as weights in the regression, and Column 2 reports unweighted regression results. The outcome data come from the PIR data, 2008–2019. Treatment variables are determined based on Head Start center data merged with the PUMA crosswalk, which are from the Head Start center locator. Each regression uses PUMA and year fixed effects. Standard errors are clustered at the PUMA level. $*$ p<0.10, $**$ p<0.05, $***$ p<0.01.

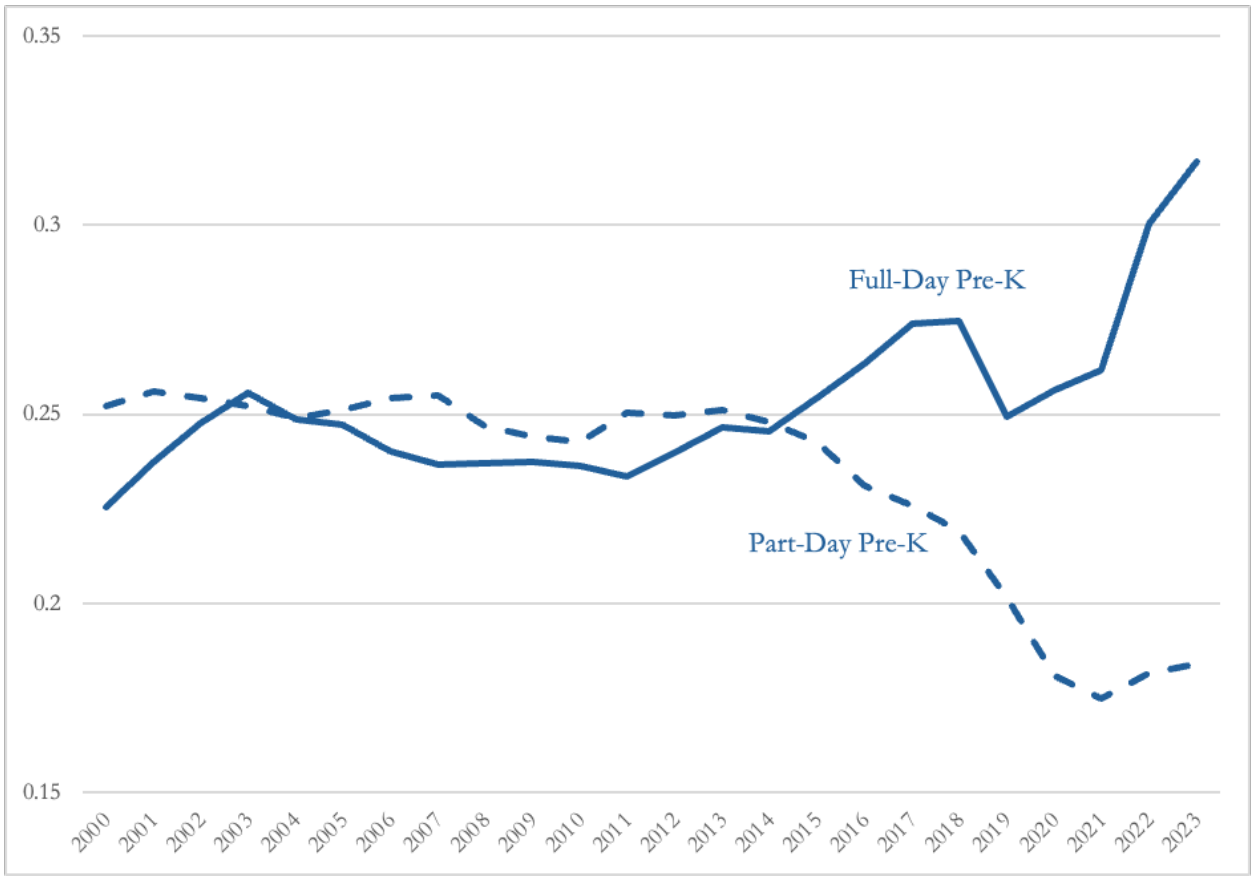
Table 2: The Effect of Head Start Expansions on

Labor Supply of Single Mothers with Young Children 0–4

	Past 12 months			
	Employed (1)	Usual hours worked per week (2)	Wage income > 0 (3)	Wage income (2015 USD) (4)
Panel A: At least one treated center in a PUMA				
PUMA had at least one treated center*Post	0.015***	0.705***	0.014**	1152.860***
	(0.006)	(0.247)	(0.006)	(323.548)
Mean Y (pre)	0.776	27.705	0.749	17701.395
Mean Y (post)	0.809	29.465	0.779	20609.184
Post-Pre Mean Y	0.033	1.760	0.030	2907.789
% Effect (100*Coef/Mean)	1.950	2.545	1.828	6.513
Mean X (Dummy)	0.586	0.586	0.586	0.586
R-Squared	0.044	0.047	0.043	0.109
Obs	182707	182688	182707	182707
Panel B: Share of treated centers in a PUMA				
PUMA share of treated centers*Post	0.015**	0.783***	0.014**	1339.719***
	(0.006)	(0.260)	(0.006)	(339.116)
Mean Y (pre)	0.776	27.705	0.749	17701.395
Mean Y (post)	0.809	29.465	0.779	20609.184
Post-Pre Mean Y	0.033	1.760	0.030	2907.789
% Effect (100*Coef/Mean)	1.966	2.826	1.837	7.568
Mean X (Share)	0.504	0.504	0.504	0.504
R-Squared	0.044	0.047	0.043	0.109
Obs	182707	182688	182707	182707
PUMA FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Notes: This table reports estimated coefficients and standard errors using a difference-in-difference (DD) model described in Equation 1, where treatment is assigned based on (1) a binary indicator that equals one if any Head Start center located in a given PUMA is affiliated with a grantee program eligible to apply for supplemental funding in Panel A, and (2) the fraction of centers within a PUMA that are affiliated with eligible grantee-programs in Panel B. The post dummy is an indicator for post-policy years, defined as post-2016. Each column reports the results from a separate regression using the labor market outcomes listed in the header. The outcome data come from the ACS data, 2008–2019. Treatment variables are determined based on Head Start center data merged with the PUMA crosswalk, which are from the Head Start center locator. Each regression uses PUMA and year fixed effects, and control variables for race and ethnicity. Standard errors are clustered at the PUMA level. $*$ p<0.10, $**$ p<0.05, $***$ p<0.01.

Figure 1: Share of 3- and 4-year olds in Full-day and Part-day Preschool in the U.S. Over Time

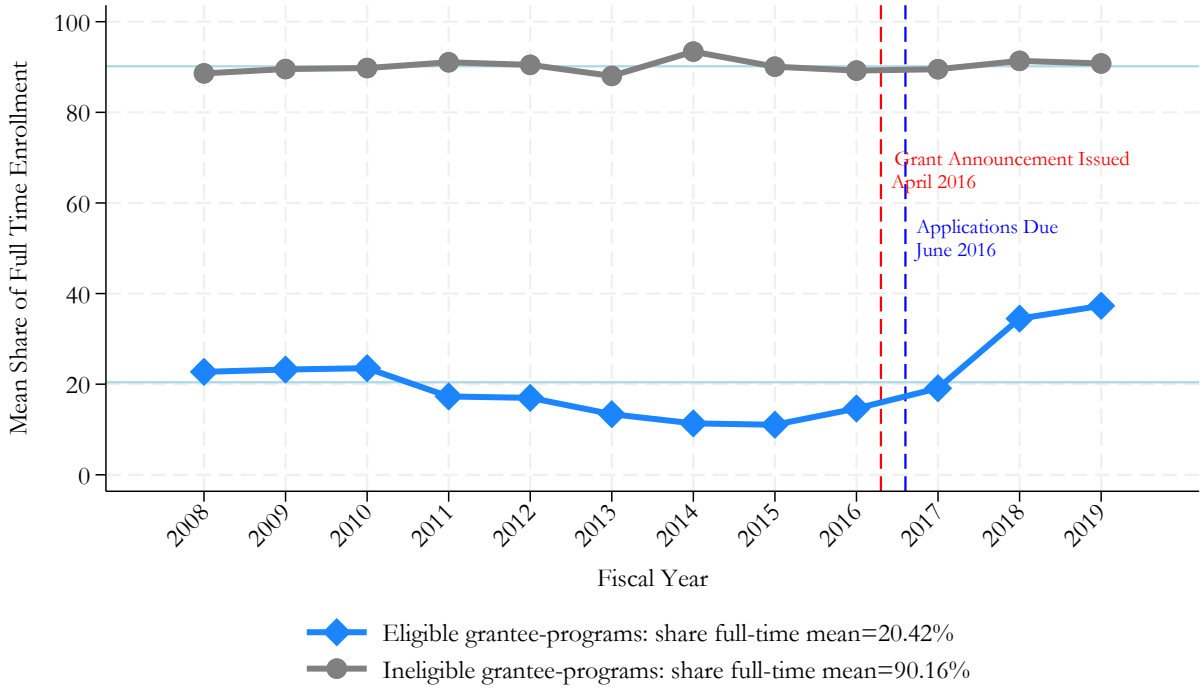


Notes: Data are reported as 3-year moving averages (endpoints are single years).

Source: Current Population Survey, October School Enrollment Supplement, 2000–2023.

Figure 2: Share Full-Time Enrollment in Head Start

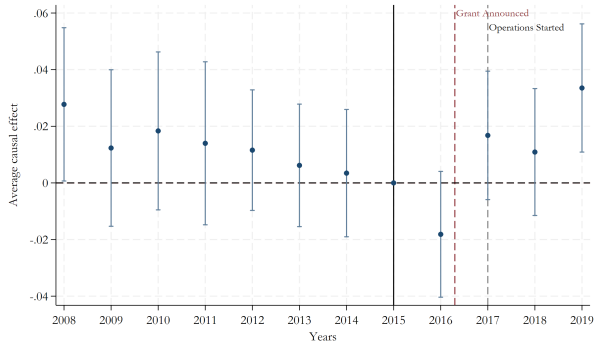
Trends by Eligibility to Apply for Supplemental Full-Day Grant Status



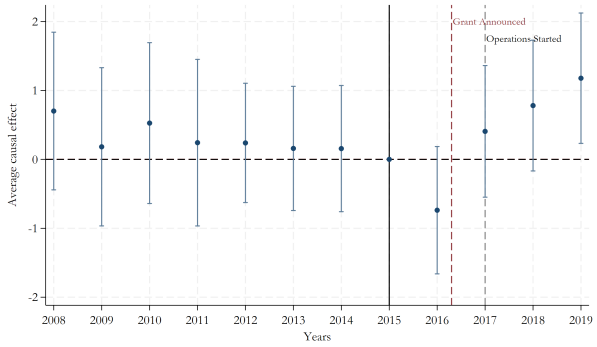
Notes: This figure shows annual full-time enrollment in Head Start by eligibility to apply for supplemental full-day grant status. We define the treatment group based on a binary indicator that equals one if any Head Start grantee is eligible to apply for the supplemental grant because they had less than 40 percent full-time enrollment in 2015 at the grantee-program level.
Source: Head Start Program Information Reports (PIR), 2008–2019. The PIR is a publicly available administrative dataset collected annually by the Office of Head Start (OHS) for the program years labeled by the spring of the service year (e.g., 2017 refers to the 2016-2017 service year).

Figure 3: Event studies: The Effect of Head Start Expansions on Labor Supply of Single Mothers with Young Children 0–4

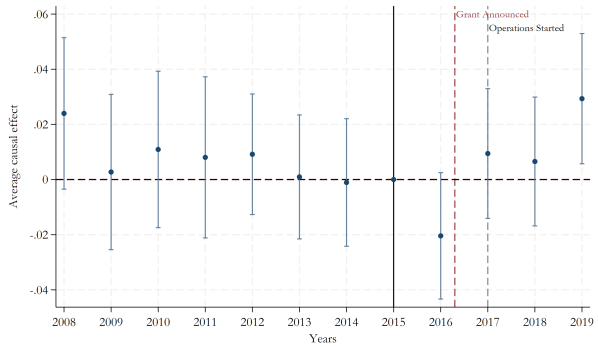
(a) Employed



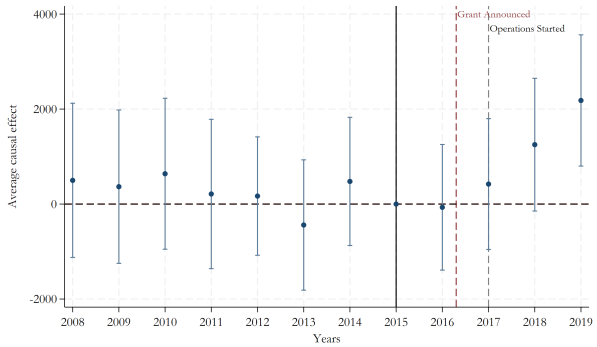
(b) Hours worked



(c) Wage income >0



(d) Wage income (2015 USD)



Notes: Figures present estimated coefficients and 95% confidence intervals using a event-study type specification described in Equation 2. The outcome data come from the ACS data. Treatment variable is determined based on Head Start center data merged with the PUMA crosswalk, which are from the Head Start center locator. The regression specification uses PUMA and year fixed effects, and control variables for race and ethnicity. Standard errors are clustered at the PUMA level.

Online Appendix: Supplemental Tables and Figures

Tables

Table A.1: Summary Statistics for Mothers using American Community Survey and Treatment Variables

	All mothers	Mothers 0-4 children	Single mothers 0-4 children	Single mothers 0-4 children treated PUMAs	Single mothers 0-4 children untreated PUMAs
	(1)	(2)	(3)	(4)	(5)
Employed	0.76	0.70	0.79	0.79	0.78
	(0.43)	(0.46)	(0.41)	(0.41)	(0.41)
Usual hours per week	27.57	25.05	28.20	28.05	28.40
	(18.55)	(18.98)	(17.33)	(17.25)	(17.44)
Wage > 0	0.72	0.67	0.76	0.76	0.75
	(0.45)	(0.47)	(0.43)	(0.43)	(0.43)
Wages (2015 USD)	28256.23	24856.37	18509.04	18163.96	18978.60
	(40595.19)	(38529.66)	(25204.11)	(23855.46)	(26924.07)
Non-White	0.27	0.27	0.44	0.37	0.54
	(0.44)	(0.45)	(0.50)	(0.48)	(0.50)
Hispanic	0.19	0.20	0.24	0.23	0.25
	(0.39)	(0.40)	(0.43)	(0.42)	(0.43)
PUMA-at least one treated center	0.60	0.60	0.58	1.00	0.00
	(0.49)	(0.49)	(0.49)	(0.00)	(0.00)
PUMA-Share of treated centers	0.53	0.54	0.50	0.86	0.00
	(0.47)	(0.47)	(0.47)	(0.26)	(0.00)
N	2597536	921005	182707	107076	75631

Notes: Labor outcomes come from the American Community Survey (ACS), 2008–2019. Treatment variables are constructed from Head Start center data from the Head Start Center Locator, merged to Public Use Microdata Areas (PUMAs) using a PUMA crosswalk. Treated PUMAs are those containing at least one treated center—i.e., a center affiliated with a grantee program eligible to apply for supplemental funding.

Table A.2: The Effect of the Head Start Expansions on Full-time

Head Start Enrollment: First Stage Sensitivity using PUMA-level Treatment

	Baseline	Time-varying state Pre-K enrollment	2015 state Pre-K enrollment flexible trends	2015 state Pre-K enrollment& 2015 unemployment flexible trends	PUMA linear trends
	(1)	(2)	(3)	(4)	(5)
A: Weighted by Population 0-4					
PUMA had at least one treated center*Post	10.445***	10.911***	7.605***	5.590***	4.179
	(1.860)	(1.946)	(1.995)	(2.035)	(2.769)
Preperiod Mean Y	53.96				
% Effect (100*Coef/Mean)	19.36	20.22	14.09	10.36	7.74
Obs	8884	8881	8884	8884	8884
B: Not weigthed					
PUMA had at least one treated center*Post	12.278***	12.117***	12.355***	12.260***	4.789***
	(1.226)	(1.263)	(1.468)	(1.440)	(1.574)
Preperiod Mean Y	46.95				
% Effect (100*Coef/Mean)	26.15	25.82	26.32	26.11	10.20
Obs	8884	8881	8884	8884	8884
PUMA FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
State Pre-k enroll.		Y			
2015 Pre-k enroll. trends			Y	Y	
2015 unemployment trends				Y	
PUMA Linear Trend					Y

Notes: This table reports estimated coefficients and standard errors using a difference-in-difference (DD) model described in Equation 1. The outcome is grantee-program enrollment. The treatment is assigned based on a binary indicator that equals one if any Head Start center located in a given PUMA is affiliated with a grantee program eligible to apply for supplemental funding. The post dummy is an indicator for post-policy years, defined as post-2016. Panel A uses the county population of 0 to 4 year olds as weights in the regression, and Panel B reports unweighted regression results. The outcome data come from the PIR data, 2008–2019. Treatment variables are determined based on Head Start center data merged with the PUMA crosswalk, which are from the Head Start center locator. Each regression uses PUMA and year fixed effects. Standard errors are clustered at the PUMA level. $*$ p<0.10, $**$ p<0.05, $***$ p<0.01.

Table A.3: The Effect of the Head Start Expansions on Full-time

Head Start Enrollment: First Stage Sensitivity using Grantee-program-level Treatment

	Baseline	Time-varying state Pre-K enrollment	2015 state Pre-K enrollment flexible trends	2015 state Pre-K enrollment& 2015 unemployment flexible trends	PUMA linear trends
	(1)	(2)	(3)	(4)	(5)
A: Weighted by Population 0-4					
Treatment*Post	14.270***	14.263***	12.549***	11.969***	10.224***
	(1.792)	(1.808)	(1.921)	(1.898)	(2.769)
Preperiod Mean Y	53.96				
% Effect (100*Coef/Mean)	26.44	26.44	23.25	22.18	18.95
Obs	8884	8881	8884	8884	8884
B: Not weigthed					
Treatment*Post	14.886***	14.839***	14.861***	14.892***	9.731***
	(1.059)	(1.072)	(1.226)	(1.224)	(1.582)
Preperiod Mean Y	46.952				
% Effect (100*Coef/Mean)	32	32	32	32	21
Obs	8884	8881	8884	8884	8884
Program-Grantee FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
State Pre-k enroll.		Y			
2015 Pre-k enroll. trends			Y	Y	
2015 unemployment trends				Y	
PUMA Linear Trend					Y

Notes: This table reports estimated coefficients and standard errors using a difference-in-difference (DD) model described in Equation 1. The outcome is grantee-program enrollment. The treatment is assigned based on a binary indicator that equals one if any Head Start grantee is eligible to apply for the supplemental grant because they had less than 40 percent full-time enrollment in 2015. The post dummy is an indicator for post-policy years, defined as post-2016. Panel A uses the county population of 0 to 4 year olds as weights in the regression, and Panel B reports unweighted regression results. The data come from the PIR data, 2008–2019. Each regression uses grantee-program and year fixed effects. Standard errors are clustered at the grantee-program level. $*$ p<0.10, $**$ p<0.05, $***$ p<0.01.

Table A.4: The Effect of Head Start Expansions on Labor Supply of Mothers with Young Children 0-4: Single versus Married Mothers

	Past 12 months			
	Employed (1)	Usual hours worked per week (2)	Wage income > 0 (3)	Wage income (2015 USD) (4)
Single Mothers				
Panel A: At least one treated center in a PUMA				
PUMA had at least one treated center*Post	0.015***	0.705***	0.014**	1152.860***
	(0.006)	(0.247)	(0.006)	(323.548)
Mean Y (pre)	0.776	27.705	0.749	17701.395
Mean Y (post)	0.809	29.465	0.779	20609.184
Post-Pre Mean Y	0.033	1.760	0.030	2907.789
% Effect (100*Coef/Mean)	1.950	2.545	1.828	6.513
Mean X (Dummy)	0.586	0.586	0.586	0.586
R-Squared	0.044	0.047	0.043	0.109
Panel B: Share of treated centers in a PUMA				
PUMA share of treated centers*Post	0.015**	0.783***	0.014**	1339.719***
	(0.006)	(0.260)	(0.006)	(339.116)
% Effect (100*Coef/Mean)	1.966	2.826	1.837	7.568
Mean X (Share)	0.504	0.504	0.504	0.504
R-Squared	0.044	0.047	0.043	0.109
Obs	182707	182688	182707	182707
Married Mothers				
Panel C: At least one treated center in a PUMA				
PUMA had at least one treated center*Post	-0.011***	-0.234	-0.011***	-1101.485***
	(0.003)	(0.143)	(0.003)	(355.878)
Mean Y (pre)	0.669	23.785	0.635	25553.758
Mean Y (post)	0.689	24.750	0.655	29925.840
Post-Pre Mean Y	0.020	0.966	0.020	4372.083
% Effect (100*Coef/Mean)	-1.644	-0.984	-1.783	-4.310
Mean X (Dummy)	0.623	0.623	0.623	0.623
R-Squared	0.049	0.044	0.044	0.092
Panel D: Share of treated centers in a PUMA				
PUMA share of treated centers*Post	-0.010***	-0.211	-0.011***	-891.920**
	(0.003)	(0.146)	(0.004)	(361.000)
% Effect (100*Coef/Mean)	-1.538	-0.886	-1.674	-3.490
Mean X (Share)	0.554	0.554	0.554	0.554
R-Squared	0.049	0.044	0.044	0.092
Obs	738298	738046	738298	738298
PUMA FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Notes: This table reports estimated coefficients and standard errors using a difference-in-difference (DD) model described in Equation 1, where treatment is assigned based on a binary indicator that equals one if any Head Start center located in a given PUMA is affiliated with a grantee program eligible to apply for supplemental funding. The post dummy is an indicator for post-policy years, defined as post-2016. Each column reports the results from a separate regression using the labor market outcomes listed in the header. The outcome data come from the ACS data, 2008–2019. Treatment variables are determined based on Head Start center data merged with the PUMA crosswalk, which are from the Head Start center locator. Each regression uses PUMA and year fixed effects, and control variables for race and ethnicity. Standard errors are clustered at the PUMA level. $*$ p<0.10, $**$ p<0.05, $***$ p<0.01.

Table A.5: The Effect of Head Start Expansions on

Labor Supply of Single Mothers with Young Children 0-4, by Race

	Past 12 months			
	Employed (1)	Usual hours worked per week (2)	Wage income > 0 (3)	Wage income (2015 USD) (4)
White Mothers				
Panel A: At least one treated center in a PUMA				
PUMA had at least one treated center*Post	0.020***	0.753**	0.019**	1333.022***
	(0.007)	(0.320)	(0.008)	(487.381)
Mean Y (pre)	0.786	28.026	0.754	18762.916
Mean Y (post)	0.809	29.338	0.773	21396.709
Post-Pre Mean Y	0.024	1.311	0.020	2633.792
% Effect (100*Coef/Mean)	2.564	2.688	2.461	7.105
Mean X (Dummy)	0.655	0.655	0.655	0.655
R-Squared	0.057	0.057	0.056	0.136
Panel B: Share of treated centers in a PUMA				
PUMA share of treated centers*Post	0.024***	0.914***	0.022***	1679.278***
	(0.008)	(0.332)	(0.008)	(494.509)
% Effect (100*Coef/Mean)	3.087	3.260	2.960	8.950
Mean X (Share)	0.577	0.577	0.577	0.577
R-Squared	0.057	0.057	0.056	0.136
Obs	109627	109601	109627	109627
Black Mothers				
Panel C: At least one treated center in a PUMA				
PUMA had at least one treated center*Post	0.016	0.973**	0.014	1086.346**
	(0.012)	(0.484)	(0.012)	(543.181)
Mean Y (pre)	0.772	27.655	0.757	16267.460
Mean Y (post)	0.813	29.946	0.798	19540.776
Post-Pre Mean Y	0.041	2.291	0.040	3273.316
% Effect (100*Coef/Mean)	2.030	3.519	1.829	6.678
Mean X (Dummy)	0.411	0.411	0.411	0.411
R-Squared	0.082	0.090	0.081	0.136
Panel D: Share of treated centers in a PUMA				
PUMA share of treated centers*Post	0.015	1.250**	0.013	1610.979**
	(0.013)	(0.556)	(0.013)	(632.815)
% Effect (100*Coef/Mean)	1.951	4.521	1.759	9.903
Mean X (Share)	0.312	0.312	0.312	0.312
R-Squared	0.082	0.090	0.081	0.136
Obs	48496	48501	48496	48496
PUMA FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Notes: This table reports estimated coefficients and standard errors using a difference-in-difference (DD) model described in Equation 1, where treatment is assigned based on (1) a binary indicator that equals one if any Head Start center located in a given PUMA is affiliated with a grantee program eligible to apply for supplemental funding in Panel A, and (2) the fraction of centers within a PUMA that are affiliated with eligible grantee-programs in Panel B. The post dummy is an indicator for post-policy years, defined as post-2016. Each column reports the results from a separate regression using the labor market outcomes listed in the header. The outcome data come from the ACS data, 2008–2019. Treatment variables are determined based on Head Start center data merged with the PUMA crosswalk, which are from the Head Start center locator. Each regression uses PUMA and year fixed effects, and control variables for race and ethnicity. Standard errors are clustered at the PUMA level. $*$ p<0.10, $**$ p<0.05, $***$ p<0.01.

Table A.6: Robustness Checks, Placebo Comparisons:

The Effect of Head Start Expansions on Labor Supply of Mothers with Older Children

	Youngest Child Aged between 5 and 6		Youngest Child Aged between 7 and 11	
	Married Mothers	High Educated Mothers	Married Mothers	High Educated Mothers
A: Employed				
PUMA had at least one treated center*Post	0.0044	0.0071	0.0021	0.0009
	(0.0059)	(0.0066)	(0.0037)	(0.0042)
Y-mean	0.733	0.828	0.771	0.854
X-mean	0.621	0.599	0.622	0.597
R-Squared	0.056	0.062	0.042	0.041
Obs	208741	100714	488227	234501
B: Usual hours worked per week				
PUMA had at least one treated center*Post	0.3978	0.3382	0.1897	0.2423
	(0.2464)	(0.3040)	(0.1623)	(0.1991)
Y-mean	26.041	30.673	27.682	31.975
X-mean	0.621	0.599	0.622	0.597
R-Squared	0.052	0.064	0.039	0.046
Obs	208653	100667	488019	234416
C: Wage income last year > 0				
PUMA had at least one treated center*Post	0.0049	0.0055	0.0059	0.0041
	(0.0061)	(0.0069)	(0.0039)	(0.0046)
Y-mean	0.691	0.790	0.726	0.813
X-mean	0.621	0.599	0.622	0.597
R-Squared	0.052	0.061	0.039	0.042
Obs	208741	100714	488227	234501
D: Wage income (2015 USD)				
PUMA had at least one treated center*Post	-563.6203	-905.2317	461.6417	897.8153
	(627.8956)	(1073.5003)	(423.7155)	(701.0212)
Y-mean	29771.214	47378.495	31313.759	49323.261
X-mean	0.621	0.599	0.622	0.597
R-Squared	0.087	0.069	0.068	0.045
Obs	208741	100714	488227	234501
Ind. Control: Race, Hispanic	Y	Y	Y	Y
PUMA FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Notes: This table reports estimated coefficients and standard errors using a difference-in-difference (DD) model described in Equation 1, where treatment is assigned based on a binary indicator that equals one if any Head Start center located in a given PUMA is affiliated with a grantee program eligible to apply for supplemental funding. The post dummy is an indicator for post-policy years, defined as post-2016. For each outcome panel, separate regressions are estimated for each of the four column-defined subgroups. The outcome data come from the ACS data, 2008–2019. Highly educated mothers are defined as those with a college degree or more. Treatment variables are determined based on Head Start center data merged with the PUMA crosswalk, which are from the Head Start center locator. Each regression uses PUMA and year fixed effects, and control variables for race and ethnicity. The Y-mean refers to the analytical sample mean. Standard errors are clustered at the PUMA level. $*$ p<0.10, $**$ p<0.05, $***$ p<0.01.

Table A.7: Robustness Checks, Alternative Specifications:

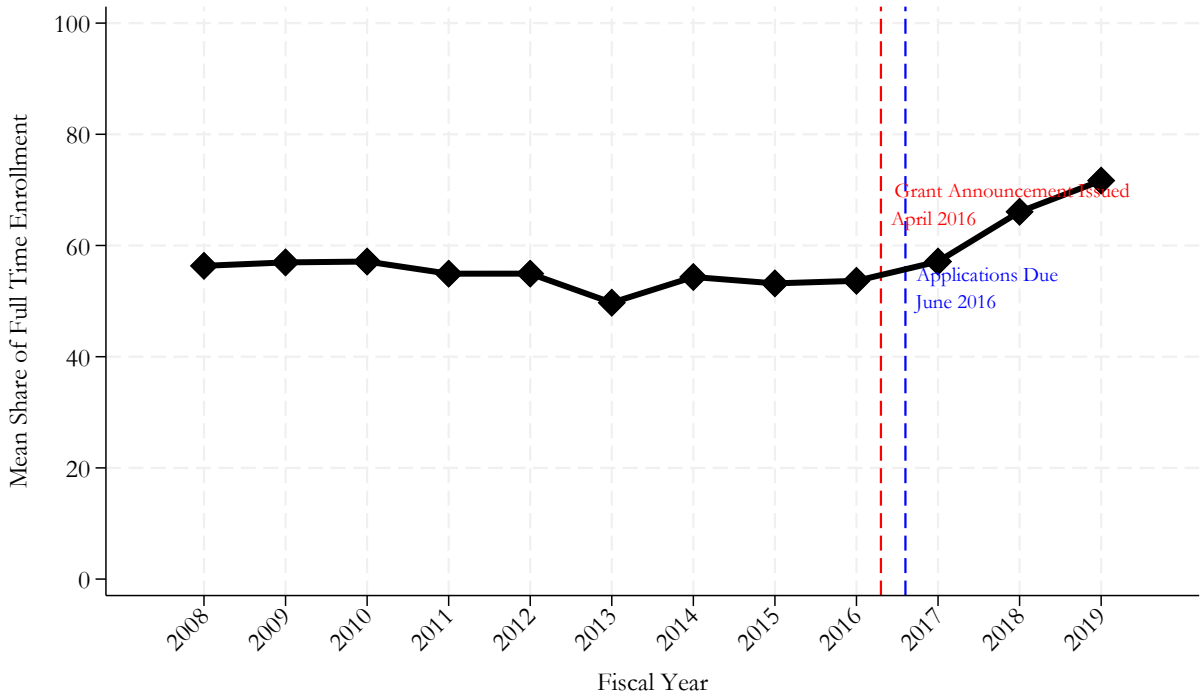
The Effect of Head Start Expansions on Labor Supply of Single Mothers with Young Children 0-4

		Control for			
	Baseline	Time-varying State Pre-K Enrollment	2015 State Pre-K Enrollment Flexible Trends	2015 State Pre-K Enroll. & Unemployment Flexible Trends	Puma Linear Trends
A: Worked last year (past 12 months)					
PUMA had at least one treated center*Post	0.0151***	0.0132**	0.0150**	0.0153**	0.0292***
	(0.0058)	(0.0059)	(0.0063)	(0.0063)	(0.0093)
Y-mean	0.788	0.788	0.788	0.788	0.788
X-mean	0.586	0.587	0.586	0.586	0.586
R-Squared	0.044	0.044	0.044	0.044	0.063
Obs	182707	182552	182707	182707	182707
B: Usual hours worked per week (past 12 months)					
PUMA had at least one treated center*Post	0.7050***	0.6605***	0.5926**	0.5851**	1.0263***
	(0.2470)	(0.2538)	(0.2690)	(0.2674)	(0.3976)
Y-mean	28.180	28.182	28.180	28.180	28.180
X-mean	0.586	0.586	0.586	0.586	0.586
R-Squared	0.047	0.047	0.047	0.047	0.067
Obs	182688	182533	182688	182688	182688
C: Wage income last year > 0 (past 12 months)					
PUMA had at least one treated center*Post	0.0137**	0.0110*	0.0121*	0.0122*	0.0234**
	(0.0059)	(0.0060)	(0.0064)	(0.0064)	(0.0097)
Y-mean	0.759	0.759	0.759	0.759	0.759
X-mean	0.586	0.587	0.586	0.586	0.586
R-Squared	0.043	0.043	0.043	0.043	0.061
Obs	182707	182552	182707	182707	182707
D: Wage income 2015 USD (past 12 months)					
PUMA had at least one treated center*Post	1152.8601***	1052.2348***	1205.5225***	1155.6414***	865.1384
	(323.5484)	(333.2203)	(343.9095)	(343.4462)	(559.2033)
Y-mean	19053.665	19050.605	19053.665	19053.665	19053.665
X-mean	0.586	0.587	0.586	0.586	0.586
R-Squared	0.109	0.110	0.109	0.110	0.131
Obs	182707	182552	182707	182707	182707
Ind. Control: Race, Hispanic	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
PUMA FE	Y	Y	Y	Y	Y
State Pre-k enroll.		Y			
2015 Pre-k enroll. trends			Y	Y	
2015 Unemployment trends				Y	
PUMA Linear trends					Y
N States	49	49	49	49	49
N PUMA	2080	2080	2080	2080	2080

Notes: This table reports estimated coefficients and standard errors using a difference-in-difference (DD) model described in Equation 1, where treatment is assigned based on a binary indicator that equals one if any Head Start center located in a given PUMA is affiliated with a grantee program eligible to apply for supplemental funding. The post dummy is an indicator for post-policy years, defined as post-2016. For each outcome panel, separate regressions are estimated for each of the specifications in each column. The outcome data come from the ACS data, 2008–2019. Treatment variables are determined based on Head Start center data merged with the PUMA crosswalk, which are from the Head Start center locator. Each regression uses PUMA and year fixed effects, and control variables for race and ethnicity. The Y-mean refers to the analytical sample mean. Standard errors are clustered at the PUMA level. $*$ p<0.10, $**$ p<0.05, $***$ p<0.01.

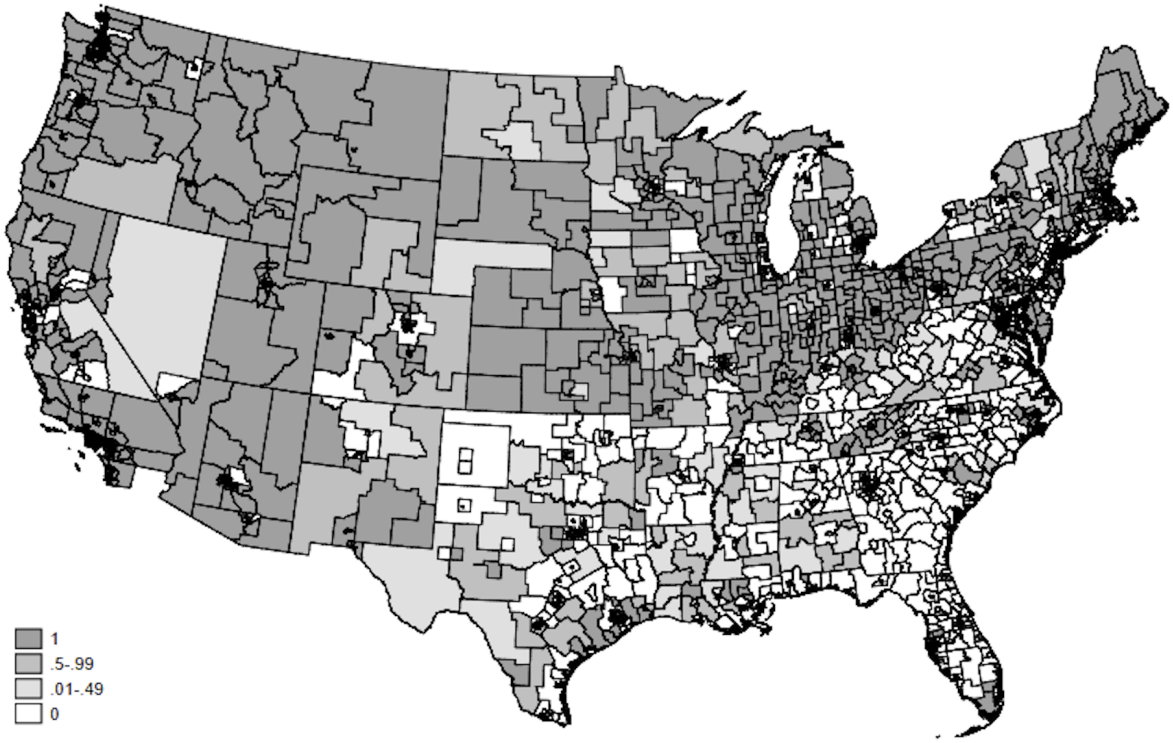
Figures

Figure A.1: Share Full-Time Enrollment Trends in Head Start



Notes: This figure shows annual full-time enrollment in Head Start, weighted by the county-level number of 0-4 year olds.
Source: Head Start Program Information Reports (PIR), 2008–2019. The PIR is a publicly available administrative dataset collected annually by the Office of Head Start (OHS) for the program years labeled by the spring of the service year (e.g., 2017 refers to the 2016-2017 service year).

Figure A.2: Share of Head Start Centers Affiliated with Eligible Grantees in Each PUMA



Notes: This figure displays the share of Head Start centers within a geographic area, as defined by PUMAs, that were associated with grantees eligible to apply for supplemental duration expansion funding.