Access to Head Start and Maternal Labor Supply: Experimental and Quasi-experimental Evidence

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Abstract

We explore how access to Head Start impacts maternal labor supply. By relaxing child care constraints, public preschools like Head Start might lead mothers to reallocate time between employment, child care, and other activities. Using the 1990s enrollment and funding expansions and the 2002 Head Start Impact Study randomized control trial, we show that Head Start increases short-run employment and wage earnings of single mothers without reducing quality parent-child interactions. Even before including long-run benefits to children, the short-run benefit to single mothers and the government is \$0.93 per dollar. Head Start is a family-level treatment with impacts beyond children.

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1 Introduction

Over the past 70 years, female labor supply has dramatically increased (U. S. Bureau of Labor Statistics, 2006). As the labor market has become more accessible to women, more mothers and potential mothers face joint decisions about work and family. Empirically, the role of childrearing has unequally fallen upon women in the United States (Sayer et al., 2004). As both employment and childrearing require substantial time and resource commitments, women facing these decisions often face trade-offs (Fitzpatrick, 2010). These constraints might be particularly binding for single mothers. Publicly provided child care through early education programs for children may relax a mother's time and monetary constraints, leading to changes in the way she allocates time and resources (Kimmel, 1998). Constraints on access to child care due to the COVID-19 pandemic have renewed interest in policy solutions for assisting working mothers. This has coincided with proposals to provide universal access to public, federally provided preschool. These calls have been met with questions about how publicly provided preschool affects maternal labor supply and if it crowds out other parental investments in children.

We explore these questions by examining a public preschool program already funded by the federal government: Head Start. Started in 1965, Head Start remains the largest provider of early education services to low-income children in the United States. Research on the Head Start program focuses almost exclusively on child outcomes, but Head Start might also affect the decisions of other family members. For many families, child care is a large work-related expense that cuts into potential wages and reduces the net benefit associated with employment.[1] Access to Head Start may provide an implicit child care subsidy, lead to higher net wages, and potentially change employment decisions for targeted families, such as low-income, single mothers with young children, in the short-term. In addition, facilitating women's return to work one year earlier may impact future labor force attachment, earnings

[1]Even after accounting for low-income child care assistance, average hourly center-based child care costed approximately 35% of the federal minimum wage both in the 1990s and recently(Herbst, 2015).

trajectories, and overall household income. This could result in long-lasting, indirect impacts of Head Start on the family. In this research, we explore Head Start's impact on maternal labor supply of single mothers in both the short- and long-run. We then see how these Head Start induced changed in maternal employment correlate to other parenting investments and children's outcomes to shed light on broader questions about how access to publicly provided child care affects maternal labor supply and income, and if these effects are accompanied by changes in parenting or child outcomes.

This paper provides new evidence that Head Start increases employment among single mothers by examining variation from the 1990s Head Start expansions, which we supplement with the 2002 Head Start Impact Study randomized control trial (see Appendix Figure A.1). We focus on single mothers, as they are more likely to meet eligibility requirements than married mothers. We also expect them to be more responsive as time constraints on employment and child care are more likely to bind than for two parent households.[2] Starting with the Head Start Expansion and Quality Improvement Act of 1990, the United States congress expanded funding for Head Start preschool for low-income three- and four-year-olds. During the 1990s, both total funding and funding per age-eligible child approximately tripled while Head Start enrollment nearly doubled between 1989 and 1999 (see Figure 1). As Head Start dollars were allocated to states based on preceding census population counts, these expansions led to largely proportional, formulaic increases in state-level funding, a pattern which empirically carried over to the local metropolitan area level. These increases in local Head Start funds led to higher local preschool enrollment and greater access to Head Start.

We explore the relationship between Head Start access and maternal labor supply by linking individual-level employment data from the 1984-2000 Current Population Survey (CPS) to metropolitan-level Head Start expenditure per three- and four-year-old, constructed from the Consolidated Federal Funds Reports (CFFR). To identify the impact of Head Start

[2]As Moffitt (1992) notes, labor supply elasticities for single mothers are likely smaller than for married mothers as they must simultaneously care for and economically support their children. Married mothers might have more scope to respond, but less necessity. As such, it is ambiguous whether married mothers would be more or less responsive.

access on maternal labor supply we compare employment outcomes of single mothers with three- and four-year-olds (eligible children) to single mothers with seven-, eight-, and nine-year-olds (ineligible children) in the same metropolitan area before and after funding (and enrollment) increases. Comparing single mothers with eligible children to single mothers with ineligible children in the same metropolitan area controls for local characteristics or trends that might be correlated with both funding increases and employment of single mothers, allowing us to estimate the causal relationship.

We find that a $500 increase in per child Head Start spending (a little less than the average funding increase over the decade) increased annual employment of single mothers with age-eligible children by 1.9 percentage points relative to single mothers with older children in the same local area. Head Start funding increases also resulted in more average hours and weeks worked as well as higher wage earnings. Consistent with Head Start providing a child care subsidy, these impacts are largest among subgroups with lower baseline employment rates and hourly wages, such as less-educated, never married, and minority mothers. Our estimates exhibit parallel pre-trends, are robust to different counterfactual comparison groups, and are unexplained by other policies, such as the EITC and welfare reform, which also changed during this period.

We corroborate these results using the Head Start Impact Study (HSIS), a pre-existing 2002 randomized control trial with information on approximately 3,200 households. Having a child enrolled in Head Start led to marginally significant increases in maternal labor supply in the full sample, with large, significant impacts concentrated among never married mothers, mothers without younger children, and in Head Start centers that offered full-day programs. This would suggest that publicly provided preschool is more effective at increasing maternal labor supply when more hours of care are provided, and when women do not face additional child care costs. Despite these contemporaneous effects, we lack precision to identify persistent changes in labor force attachment.

This work adds to the growing literature exploring the effects of subsidized child care provision on maternal labor supply. Existing work has identified effects of both explicit child care subsidy programs (Blau and Tekin, 2007; Meyer and Rosenbaum, 2001), as well as implicit child care subsidies through public school access. Research exploiting staggered kindergarten rollout (Cascio, 2009) and kindergarten age eligibility rules (Gelbach, 2002) prior to 1990 find that single women increase their labor supply when their youngest child goes to kindergarten. However, research exploiting universal kindergarten or preschool expansions in the late 1990s and early 2000s, find little evidence of labor supply responses (Cascio and Schanzenbach, 2013; Fitzpatrick, 2010, 2012).[3] There is work documenting the impact of early childhood schooling on maternal labor supply in other countries, but these programs are often more generous and universal in nature and, unlike Head Start, are situated in a more encompassing transfer program setting.[4] We add to this literature by focusing on labor supply responses to subsidized child care in the 1990s, when preschool access and enrollment expanded rapidly, which might help explain why research examining the end of the decade saw little response to universal preschool and kindergarten. Past research has focused less on low-income families, and giving attention to this population provides valuable information. Our setting allows us to descriptively explore the trade-offs between maternal employment, parenting investments, and children's outcomes adding a new contribution to the literature on maternal labor supply.

Because mothers traditionally provide an outsized share of child care, publicly provided preschool programs like Head Start could create a tension in mothers' investments in their children (Gensowski et al., 2020). Increasing maternal employment could facilitate more financial investments, but it could also limit parent-child time investments. Similar to Baker et al. (2008), we start to unpack this potential trade-off, but for older, more disadvantaged children eligible for Head Start. In contrast to Baker et al. (2008), we find in the HSIS that the subgroups with the largest employment increases do not see declines in other parental

[3]One exception is (Soldani, 2021), who exploits kindergarten age rules and finds positive labor supply effects that persist up to five years.

[4]See Bauernschuster and Schlotter (2015); Carta and Rizzica (2018); Gathmann and Sass (2018); Haeck et al. (2015).

activities with children, like reading, math, or attending cultural events. This suggests work-encouraging public preschool policies do not necessarily crowd-out quality parental investments when they increase labor supply. We also find in the HSIS that the subgroups with the largest increase in employment tend to experience the largest gains in children's test scores. Although other factors might be at play, these correlations provide suggestive evidence that maternal labor supply does not impose a learning penalty on children, and perhaps, might even foster cognitive improvements by providing access to resources (e.g., income, maternal mental health).

This research also adds context to a rich literature documenting the program effects of Head Start. Most prior work on Head Start focuses on benefits to children only,[5] neglecting the benefits to mothers and society more broadly. There is surprisingly little work that evaluates the impact of Head Start on parental behavior in general, and maternal labor supply in particular.[6] Understanding Head Start's affect on mothers can help contextualize the program's impacts on children.

Our results suggest that there is an immediate short-run benefit of the program to single mothers and to the government not previously considered. Without counting long-run benefits to children, we estimate that after tax, after transfer income of single mothers increases by \$0.55 for every dollar spent on Head Start, and that the government recoups \$0.38 per dollar, due to changes in welfare payments and tax revenue in these households. Given growing interest and concern about both child care constraints and the government's potential role, we provide new evidence that access to public preschool increases employment and income of single mothers, and this relationship should be considered when evaluating

[5] See for example (Bailey et al., 2021; Barr and Gibbs, 2022; Carneiro and Ginja, 2014; Currie and Thomas, 1995; Deming, 2009; Duncan and Magnuson, 2013; Garces et al., 2002; Johnson and Jackson, 2019; Kline and Walters, 2016; Ludwig and Miller, 2007; Ludwig and Phillips, 2007; Puma et al., 2012; Thompson, 2018)

[6] To the best of our knowledge only two papers have explored the impact of Head Start on parenting behavior (Ansari et al., 2016; Gelber and Isen, 2013), one working paper examines the impact of Head Start on household income-to-needs (Schochet and Padilla, 2019), and three examine parental education and employment (Pihl, 2022; Sabol and Chase-Lansdale, 2015; Schiman, 2021). As we discuss in the next section, our work provides a more complete picture by examining separate settings and exploring heterogeneous subgroup effects that match theoretical predictions.

the impacts, costs, and benefits of programs like Head Start.

2 Publicly Provided Preschool and Maternal Labor Supply

In theory, public provision of preschool programs like Head Start implicitly provide a subsidy for child care. Because women often provide primary care for their children, reducing the cost of replacing maternal care with nonmaternal care likely shifts female labor force participation (Kimmel, 1998). In a traditional two-good model describing a mother's labor supply, a mother chooses between labor supply (with the help of a paid child care provider) and time at home caring for her child herself (Fitzpatrick, 2010). In this framework, a child care subsidy reduces some of the costs associated with employment, leading to higher net wages, potentially inducing some mothers to substitute away from home production and enter the labor market after the child care subsidy is introduced. Although income effects from a child care subsidy put downward pressure on labor supply, substitution effects likely dominate for constrained, low-income mothers, potentially leading to increases in labor supply on the intensive margin as well. Thus, offering Head Start to children likely affects mothers by changing the costs and feasibility of employment, which could affect her overall labor force attachment.

Maternal labor supply could change mothers' income investments in their children (Gensowski et al., 2020; Løken et al., 2012). Income investments in children can improve cognitive and behavioral outcomes for children in the short run (Almond and Currie, 2011) as well as improve long-term educational outcomes (Timpe, 2019). Financial resources may be most impactful for young children and children from disadvantaged backgrounds (Almond and Currie, 2011). Employment may also improve mental health, social connections, skill development, and the stability of family routines, all of which could enhance the quality or performance of mothers in family roles (Gensowski et al., 2020; Herbst, 2017). However, more employment might reduce the amount of time spent in parent-child interactions (Baker et al., 2008; Løken et al., 2018), which are important for child development. Prior literature connecting maternal employment to parenting and child outcomes has generally focused on infants and very young children (Baker et al., 2008; Herbst, 2017; James-Burdumy, 2005; Løken et al., 2018), suggesting a need to better understand connections during the preschool years.

To date there is limited, but growing, evidence on how Head Start affects maternal labor supply in the short- and long-run. Using the HSIS, Sabol and Chase-Lansdale (2015) briefly examine parental labor supply. Because they are focused on educational and human capital investments of parents, they only look at how treated households that *did not* work during the fall of the Head Start treatment year adjust their labor supply in future years. Because the study focused on educational and human capital investments, the authors do not examine potential short-run labor supply changes contemporaneously during the treatment year due to lower child care costs as well as potential persistent effects among those that initially responded. This work does not tell us how access to Head Start might reduce work related costs and affect mother's labor supply decisions. Schiman (2021) uses the HSIS to explore impacts on maternal education, transfer payments, and labor supply, but she does not examine differences by presence of younger children, program generosity, or see how labor supply patterns relate to children's cognitive scores and other parental time investments in children (Gelber and Isen, 2013). Two working papers (Long, 2016; Pihl, 2022), exploit variation in grant writing aid given to the 300 poorest counties during the 1960s rollout and find some evidence of lower employment during the early years of Head Start. We shed new light on the relationship between Head Start and maternal employment by looking at several periods in the history of the program and by relating employment impacts for mothers to impacts for children.

The lack of previous work on Head Start and maternal labor supply is in part due to the nature of the program. Head Start is nationally administered, resulting in little exogenous spatial variation. When there is plausibly exogenous spatial variation (such as the 1960s rollout or the 1990s expansions) there are not high quality administrative data. It is in part for this reason that we explore both a natural experiment and a randomized experiment. Although each experiment faces data limitations, together they provide consistent evidence of Head Start leading to stronger labor force attachment.

3 Empirical Setting: Head Start Funding Expansions in the 1990s

Head Start is a federally funded preschool education program serving economically disadvantaged children in the United States. The program aims to increase school readiness, health, and social development for low-income children to reduce persistent educational attainment gaps between these children and their more advantaged peers (Gibbs et al., 2013). Children between ages three and five are eligible if their household income is below the federal poverty threshold, their household receives Temporary Assistance for Needy Families (TANF), their family receives Supplemental Security Income (SSI), they are homeless, or if they are a foster child. Head Start began as a small summer program in 1965 as part of President Lyndon B. Johnson's War on Poverty, and quickly became the largest early childhood education program for low-income children in the United States.[7] Although Head Start required providers to comply with educational standards, the program was marked by variance in sponsoring organizations, size of individual providers, overhead costs, and labor costs. For example, public, private for-profit, and private nonprofit schools receive funding.

From Head Start's inception, program implementation was rapid and lacked clear processes for allocating funding, resulting in geographic variation. In 1965, the new Office of Economic Opportunity sent 35,000 letters throughout the country to invite application (Bailey et al., 2021; Levine, 1970), with special attention given to the 300 poorest counties (Ludwig and Miller, 2007). Grants from across the country were submitted and evaluated in

[7]In the 1960s, many parents worked as Head Start teachers and assistants (Gibbs et al., 2013). Over time, the program shifted to focus more on educational quality. With the 1990 Quality Improvement Act's emphasis on professionalizing the program, the program increasingly used more qualified teachers and adhered to performance standards (Gibbs et al., 2013). Nationally, by 1997 58% percent of Head Start teacher had at least an associate's degree (Zill et al., 2003). With fewer employment opportunities at Head Start centers for less educated mothers of Head Start children, this is likely not driving any employment response in the 1990s.

an arbitrary environment of "great administrative confusion" (Bailey and Goodman-Bacon, 2015) using a "wild sort of grant-making operation" (Bailey et al., 2021). Although initial levels of Head Start funding were correlated with local characteristics, the timing of Head Start introduction was not (Bailey et al., 2021; Barr and Gibbs, 2022). Once grants were approved, grantees had a high probability of maintaining funding, which perpetuated funding variation over time. Our identification strategy exploits policy variation that builds on this initial geographic variation, but, as explained later, also includes a within-MSA counterfactual group of mothers to partial out any local characteristics that might affect our outcome.

In 1990, Congress passed the Head Start Expansion and Quality Improvement Act, thereby providing substantially more funding to increase the number of children enrolled and improve the quality of the educational programming (e.g., increased teacher salaries, training, and facilities). Additional expansions in 1992, 1994, and 1998 led to sharp increases in both funding and enrollment throughout the decade (see Figure 1).[8][9] Funding was allocated in two steps (Head Start Act, 1988). First, each state received an annual amount equal to what was received by grantees in the state in 1981, adjusted for inflation. This perpetuated some pre-existing state-level geographic variation in funding. Next, any extra funding was distributed to states in a formulaic way, giving 1/3 weight to the number of children on AFDC and 2/3 weight to the number of children under 6 living in households below the poverty line, as measured in the preceding census. States were then given flexibility in how they distributed within-state funds. Local administrators who could provide at least 20% of their own funding applied to states for Head Start funding through a competitive grant writing process, and states awarded funds to local preschool providers. The process rewarded cost-effectiveness, although states gave preference to prior applicants. The substantial geographic variation in funding per eligible child that existed prior to the 1990

[8] This variation was first used by Kose (2021) to explore the impact of Head Start on test scores in Texas.

[9] The expansions of Head Start did little to affect center hours. Full-day programming was funded beginning in 1982 (Klein, 1992), and by 1997 only 24% of funded Head Start slots were for full-day, 5 days per week instruction (Robin et al., 2006).

expansion (Currie and Neidell, 2007; Kose, 2021) persisted throughout the expansion period. Since federal Head Start dollars were allocated according to Census population counts, the additional appropriations led to largely proportional increases in state-level Head Start funding. The allocations resulted in geographic variation in funding increases, which when combined with preferential treatment of prior grantees, carried over to metropolitan areas. As seen in Figure 2, local areas experienced proportional increases in funding, and relative funding ranks were stable during the 1990s. The interaction between fund distribution rules and pre-existing geographic variation largely led to formulaic increases in funding that were not driven by outcomes of interest, such as maternal employment.[10] Our identification strategy compares single mothers with children who were age eligible for Head Start to mothers with older children in the same metropolitan area over time in a triple difference specification to account for any local area trends that could affect the employment of single mothers. This allows us to identify the effects of increasing the supply of Head Start on the labor supply of eligible, single mothers.

The potential for Head Start to impact maternal labor supply in part depends on the counterfactual child care situation mothers would rely on. Feller et al. (2016) report that in 2002, 47% of children not offered placement in Head Start received home-based care while 26% received center-based care. This suggests that as late as 2002, home-based care remained the most common care counterfactual to Head Start. During the 1990s, some states were also increasing access to state-run public preschool options, largely for 4-year-olds (Cascio and Schanzenbach, 2013). We address contemporaneous changes in state-run preschools in our empirical approach, and find that this does not impact our estimated effects.

[10] Due to data limitations, we use funding per child, rather than funding per child in poverty. Sensitivity analyses verify that funding variation was not the result of our use of total child population counts.

4 Data

Our analysis relies on two main data sources. The first is the annual Consolidated Federal Funds Report (CFFR) from 1983 to 2000 (U. S. Census Bureau, 2011). These reports provide detailed municipality level information on federally funded items, including payments for Head Start.[11] Funding amounts were aggregated to the county level using county codes available in the CFFR data. Next we used the Census 1990 county to metropolitan area crosswalk to aggregate to the metropolitan area, as this is the level of geography available in the CPS. We then aggregate up annual county-level population estimates by age from the Surveillance, Epidemiology, and End Results Program (SEER) to estimate the annual metropolitan population of three- and four-year-olds (National Cancer Institute, 2017). Using this measure, we construct Head Start funding per age-eligible child, which we convert to real 2017 dollars using the personal consumption expenditures price index from the Bureau of Economic Analysis. In general these funding reports track total national spending on Head Start very closely during our sample period. Appendix D provides more detail on these data.

As demonstrated in Figure 1, we measure dramatic increases in funding following program expansion, with average funding increasing by $547 (200 percent) between 1989 and 1999.[12] Across the country this resulted in higher per child funding in areas with pre-existing funds and increased reach of the Head Start program (see Appendix Figure A.2). We assigned funding dollars to the smallest labor market possible. For women living in a metropolitan area, we assigned funding within the metropolitan area. For women living outside of metropolitan areas where we only had state-level geography, we assigned the funding level in the remainder of the state.

We combine the CFFR data with the CPS Annual Social and Economic Supplement

[11] From 1991 on these funds are recorded under code 93.600. Prior to that they are coded as 13.600.

[12] These increases are due to changes in funding, not the number of age-eligible children; results are essentially unchanged if we denominate by the number of age-eligible children in a baseline year.

(ASEC) from 1984 through 2000 (Flood et al., 2018). The CPS does not provide measures of Head Start eligibility. Because of reports that 30-50% of children attending Head Start are not income eligible (Besharov and Morrow, 2007), and because income is potentially endogenous, we do not impute program eligibility using income. Instead, we rely on other observable characteristics to tag potentially eligible households. For example, mother's marital status or education are both predictive of household poverty status, the main Head Start eligibility criteria. Among mothers during our sample period, having a high school degree or less increases the probability of being below the 100% poverty threshold by 11.7 percentage points, while being a single mother increases the probability by 25.7 percentage points, over twice as much. Being never married has an even larger 45.7 percentage point effect on this probability. Because single parenthood is a highly predictive tag of Head Start eligibility, and because the trade-off between employment and home production/child care is readily transparent for single mothers (e.g., no concerns about secondary earners or intra-household bargaining), we focus on Head Start's effect on single women. Although married mothers could also respond to publicly provided child care, we do not focus on them, given their lower probability of being Head Start eligible. We also explore impacts by education, race/ethnicity, and more detailed marital status distinctions as some of these groups are more or less likely to be impacted by the funding expansion.

From the CPS, we collect information for all single mothers with children in the home as captured by the household roster. In the ASEC supplement, participants report employment during the previous calendar year. Our main outcome of interest is the extensive margin measure for ever employed in the previous calendar year, which we define to equal one if the woman worked any weeks during the previous year, and zero if not. Additionally, we consider work intensity by constructing other outcomes, such as the binary measure for full-time employment in the previous year, part-time employment in the previous year, the number of weeks worked, usual hours worked, and wage income.[13]

[13]When looking at the number of weeks worked, hours worked, and wage income, we estimate models using the inverse hyperbolic sine transformation, to include mothers who did not work and had a zero value.

For representativeness, our baseline sample includes single mothers from all over the country in both metropolitan and non-metropolitan areas. The sample includes 33,791 single mothers with either an age-eligible child (3-4) or a child 7-9 (our counterfactual group) in the 1984-2000 CPS ASEC.[14] In Table 1, we provide basic summary statistics separately for single women with and without an age-eligible child in the previous year in metropolitan areas that experienced below and above median increases in Head Start funding. Between 1990 and 1999, annual metropolitan area-level Head Start funding per age-eligible child increased by $372 (2017$) in below median increase areas, and by $693 in above median increase areas.

5 Empirical Approach

To examine the impact of Head Start access on maternal labor supply we will estimate a generalized triple difference, comparing single mothers with age-eligible children (three- or four-years-old last year) to single mothers with older (7, 8, or 9) school-aged children *in the same metropolitan area*, as follows

$$\begin{aligned} Y_{it} = {} & \beta_1 \textit{HS funding per child}_{mt-1} * (\textit{Child 3 or 4 last yr.})_{it} \\ & + \beta_2 \textit{HS funding per child}_{mt-1} + \beta_3 (\textit{Child 3 or 4 last yr.})_{it} + X_{it}'\Gamma + \phi_m + \delta_t + \varepsilon_{it} \end{aligned} \tag{1}$$

The primary outcome of interest is the binary indicator for whether the woman (i) reported in year t being employed at all last year. The coefficient β_1 captures the effect of Head Start funding per child in the previous year on employment among single mothers with an age-eligible child in the previous year, relative to women with an ineligible grade school child. The metropolitan area fixed effect (ϕ_m) makes this a comparison of mothers in the same metropolitan area.[15] As such, any local trend or characteristic that affects the em-

Results are nearly identical if we instead add one and then take the natural log.

[14]Because of the one-year lag in reported employment outcomes, this means we focus on single mothers with a four- or five-year-old in the home, as the child would have been three or four in the previous calendar year and age-eligible for Head Start, and single mothers with a child that is currently 8, 9, or 10, as they would have been between seven and nine.

[15]Since state-level Head Start enrollment data is available, it is possible to estimate the change in em-

ployment of single mothers and is correlated with metropolitan-level Head Start funding is controlled for and captured in β_2. The year fixed effect controls for national changes over time in employment rates and Head Start funding. We also include a vector of individual-level race, ethnicity, and education controls, state-level demographic shares (race, marital status, and education groups), and policy controls (household specific maximum federal EITC refund, presence of a TANF waiver in the state, maximum TANF benefit for a family of three, presence of States Children's Health Insurance Program (SCHIP), and real state minimum wage).[16] In all regressions, observations are weighted by the individual probability weights provided in the ASEC.[17] To account for potentially correlated errors among individuals in the same metropolitan area, we cluster standard errors at the metropolitan area level.[18]

As explained above, the expansion of Head Start funds was formulaically allocated to states, building on idiosyncratic variation that arose during the early Head Start years. However, allocation of Head Start funds within a state is more flexible, potentially resulting in local funding changes that correlate with unobserved area-specific shocks or trends that affect employment of single mothers. Table 1 reveals systematic differences between single mothers with age-eligible children in places that experienced large and small increases in funding. However, including single mothers with slightly older, ineligible children (ages 7-9) provides an additional counterfactual comparison and accounts for region-specific trends, policies, or characteristics that correlate with Head Start funding changes and impact single mothers' employment. As seen in column (7) of Table 1, along most dimensions, the dif-

ployment associated with each additional student enrolled. We provide this state-level analysis in Appendix Table A.1. However, there are several reasons we do not conduct all of our analysis at the state-level. First, state-level enrollment data only begins in 1988, eliminating most of the pre-treatment period. Second, we are concerned about local labor market conditions, which are better captured by the within-MSA comparisons, rather than within-state. Third, most Head Start centers were placed in urban areas, so a state-level analysis would likely dilute the treatment.

[16]Thanks to Kearney and Levine (2015) for providing data on state level policies and demographics.

[17]We weight to correct for potential endogenous sampling in the CPS (Solon et al., 2015). Results were qualitatively similar when estimating without the ASEC weights.

[18]As Head Start federal funding formulas depend on state poverty rates, we might expect variation to be correlated across MSAs within a state. When we cluster at the state-level the standard errors are mostly unchanged, but sometimes smaller (see Appendix Table A.2). We report the more conservative standard errors clustered at the metropolitan level.

ferences between single mothers with and without age-eligible children are not significantly correlated with whether or not the metropolitan area experienced an above median or a below median increase in Head Start funding.[19] A triple difference allows us to isolate the affect of Head Start funding expansions on the employment of single mother's with Head Start aged children.

Our specification fundamentally relies on a parallel trends identifying assumption, namely, that single mothers with age-eligible children would have behaved like mothers in the same metropolitan area with slightly older, non-eligible children if the Head Start expansion had not occurred. The assumption seems reasonable as all single mothers in a metropolitan area face the same local labor market conditions, but we also check the potential validity of this assumption by examining whether "effects" are detectable before the funding expansion. We focus on short-run effects in this context due to incremental changes in funding year to year. With mobility and changes in household structure over time, identification from short-run changes in funding in this setting is not suited to evaluate long-run effects.[20]

With the triple difference estimation structure, remaining threats to validity must be correlated with Head Start funding expansions, but affect the employment behavior of single mothers with age-eligible and ineligible children differently. One concern is the roll-out or expansion of state-funded public preschool programs during the 1990s (Cascio and Schanzenbach, 2013). We test explicitly to see if our estimates are biased by the roll-out of state preschool programs. Large welfare policies, such as the EITC and TANF were reformed during this period. As these changes were simultaneously affecting families (Kleven, 2019),

[19] We focus on mothers of 7- to 9-year-olds rather than 5- and 6-year-olds because, depending on their month of birth, some of them might still be Head Start eligible while the others are entering kindergarten, which could also influence maternal labor supply decisions.

[20] The mother fixed effect strategy has been important in Head Start literature evaluating child outcomes (Currie and Neidell, 2007; Deming, 2009; Garces et al., 2002), but is less feasible when evaluating maternal labor supply at different points. First, Head Start funding increases over time, creating mechanical correlation with a mother's age, preventing us from disentangling Head Start effects from life-cycle employment effects. Second, within family differences in Head Start enrollment may be endogenous to maternal labor supply, making it hard to study mothers' outcomes. Third, available longitudinal data sets do not contain enough information to use. For example, the Children and Youth sample of the NLSY contain very few observations of age-eligible children during the Head Start funding expansion and provide imprecise information on the timing of Head Start. We therefore do not pursue a mother fixed effects approach as a source of variation.

we verify our results are not driven by coinciding policy changes. In the robustness section, we evaluate the importance of these alternative policies and exploit other counterfactual and placebo comparisons to verify the result is robust.

6 Results

Impact on Enrollment. Using Head Start funding per child to proxy for access to Head Start enrollment implicitly assumes that additional Head Start funding increases enrollment. We directly test this by estimating the relationship between Head Start funding and school enrollment using the CPS October education supplement. The October supplement includes measures of current school enrollment for children three and older. Using the children, we estimate the same generalize triple difference outlined above, to see the impact of metropolitan area level Head Start funding on the probability of being in school for three- and four-year-olds relative to seven-, eight-, and nine-year-old children, and results are seen in Table 2. Because the education supplement is asked in a separate survey wave, the analysis sample is different, but it still only includes children of single mothers.

A $500 increase in Head Start funding per age-eligible child is associated with a 6 percentage point increase in the probability of a three- or four-year-old with a single mother being in school. This is a 7.9 percent increase off of a base of 76 percent school attendance.[21] Enrollment effects are similar across most group, although they are small for more-educated and married mothers (Appendix Table A.3). As seen in columns (2) and (3) of Table 2, the effect of Head Start funding on age-eligible school enrollment is large and significant when limiting the sample to states that do not have state-run pre-kindergarten programs during our sample period or when including a binary control for whether a state-run pre-kindergarten program is present. We see a similar pattern when using annual, state-level Head Start en-

[21] The October CPS also reports public or private school enrollment. However, as both public and private schools received Head Start grant funding, it is not clear that we should only focus on public schools. Respondents might not know how to report a private school supported by a public Head Start grant. If we look at public and private school enrollment separately, the effects are concentrated among public enrollment.

rollment counts by age (Kids Count Data Center, 2018) (see Appendix Table A.4). Increases in school enrollment and subsequent impact on maternal employment associated with Head Start funding are not driven by alternative state-funded preschool programs.

Impact on Maternal Employment. In column (1) of Table 3, we observe that a \$500 increase in per child Head Start funding is associated with a 1.9 percentage point increase in the probability of being employed among single mothers with age-eligible children relative to single mothers with elementary-aged children. From an average employment rate of 70 percent, this represents a 2.7 percent increase, suggesting Head Start funding induced increases in labor supply among single mothers. Given the increases in enrollment from Table 2, these estimates imply a employment elasticity with respect to enrollment of 0.34.[22] The Wald estimate would suggest that about 32 (0.019/0.06) percent of women who had a child enroll entered employment. However, as increased funding also leads to higher funding per student, and was also meant to improve Head Start quality, we do not interpret this Wald estimate in the pure instrumental variables sense.

We measure Head Start's impacts on other labor market measures. Because the data set is a repeated cross-section, we cannot fully separate the extensive and intensive margins. The increase in Head Start funding increases the full-time employment rate by 1.7 percentage points and the part-time employment rate by an insignificant 0.2 percentage points, suggesting most of the increase in employment goes to full-time employment. However, we do not know if new entrants became full-time workers, or if some part-time workers became full-time workers, and new entrants became part-time workers. We also see a 7.2 percent increase in annual weeks worked and a 7.6 percent increase in usual hours worked. If the entire 1.9 percentage point increase in employment were due to new entrants working full-time (40 hours), this would translate into a 3 percent increase in hours worked at the mean. The larger hours increase of 7.6 percent suggests there were intensive margin adjustments in weekly hours worked in addition to extensive margin entry. The effects on weeks worked

[22] Enrollment increased by 6.0 percentage points off of a base of 76 percent, implying an elasticity of $0.027/0.079 = 0.34$.

similarly imply intensive margin adjustments. Given that individuals are working 7.6% more hours and 2.1 weeks more, this implies a 15% increase in total hours worked during the year.[23] We estimate that average wage earnings increase by 15.3 percent.[24] These findings suggest the Head Start expansion facilitated increased attachment to the labor market through both the extensive and intensive margins. Additional household income associated with access to Head Start is a benefit that has not been considered in the previous work.[25]

Examining Pre-Trends. We graphically explore trends in single mothers' employment before and after the expansion in Head Start funding. Because treatment intensity is increasing over time, we document how employment of single mothers of age-eligible children trends relative to single mothers with older children in metropolitan areas that experienced large and small increases in funding. To do this we estimate the following equation separately for single mothers with age-eligible children and our comparison mothers:

$$Ever\ Employed\ last\ yr._{it} = \sum_{\tau=1986}^{2000} \beta_\tau * (year = \tau) + X_{it}'\Gamma + \phi_m + \varepsilon_{it} \tag{2}$$

The outcome is any employment for woman i in the previous calendar year, but now the β_τ coefficients trace out the employment over time for single mothers by child age-group. The interaction with 1990 is excluded to make it the reference period.[26] We estimate equation (2) primarily to plot groups specific level trends rather than differences. But, it also allows us to account for possible differential effects of other large welfare policies that could affect maternal employment, such as state-funded preschool, the EITC, and welfare reform, which

[23]Number of weeks worked increased by 2.1 weeks to 31.5 (7.2% of the mean of 29.39 weeks). Usual hours worked increased by 2 to 27.7 (7.6% of the mean of 25.74 hours). The change in total annual hours was $(31.5 * 27.7)$–$(29.4 * 25.7) = 117$, equivalent to a 15% increase in average total hours worked during a year.

[24]Because the CPS repeatedly surveys individuals, it is possible to create a two year linked panel which would facilitate within person comparisons and extensive/intensive margin decomposition. However, because of the rotating nature of the CPS, the sample would be reduced to only 3,690 individuals.

[25]Household income also increases but to a lesser extent, suggesting these single mothers have other sources of income besides wage income that are weakly, negatively impacted.

[26]The CPS began reporting over 150 additional metropolitan area codes in 1986. We restrict the sample to 1986 to maintain a balanced panel of metropolitan areas. See Appendix Figure A.3 for a longer pre-trend timeframe with the smaller subset of metropolitan areas that were identified back to 1983.

could affect mothers with different aged children differently.[27] One drawback of this group specific estimation is that it does not directly exploit the within MSA variation between mothers with age-eligible and ineligible children. However, as shown in detail in Appendix B, stacked event study estimates that do exploit this variation are similar.

To further verify these patterns are driven by Head Start funding, and not other concurrent policies, we separately estimate equation (2) for our two groups of mothers in metropolitan areas with above and below median increases in Head Start funding. The bottom half of the distribution includes 184 metropolitan areas, where the average increase in funding per child between 1989 and 1999 was 120%. The top half includes 185 metropolitan areas, where the average increase in funding per child between 1989 and 1999 was 332%.

Panel A of Figure 3 plots the coefficients for mothers in areas that experienced below median increases in Head Start funding. Prior to 1990, previous year employment trends were similar for both treatment and comparison mothers and not significantly different from zero. This continued following the expansion of Head Start, with a slight, insignificant rise in employment for both groups in the late 1990s. For metropolitan areas that experienced relatively large, above median increases in Head Start funding, pre-1990 employment differences between mothers with age-eligible children and older children are not significantly different from zero. However, after the initial Head Start expansion in 1991, there is a consistent, significant increase in employment of single mothers with age-eligible children relative to mothers with older children, similar to the dose-response increase in funding and enrollment.[28] The gap in employment between eligible and in-eligible mothers grows wider over time in areas that experience larger Head Start funding increases, but not in places that experienced small increases in funding. This pattern is consistent with Head Start expansions increasing employment of single mothers with age-eligible children, rather than other,

[27]For example, work requirements that accompanied TANF were relaxed for mothers with young children, and Looney and Manoli (2013) show that mothers with young children were also more likely to have multiple children, thereby affecting the maximum earned income tax credit the women were eligible to receive.

[28]See Appendix Figure A.4 for a state-level Head Start enrollment event study.

concurrent policies in the 1990s which would affect both high- and low-funding areas.[29]

Robustness. We next verify estimates are robust to alternative estimation specifications, carefully accounting for other concurrent policies (such as state pre-k, the EITC, and welfare reform). As seen in Panel A of Table 4, estimates are robust to limiting the sample to states without a state-funded preschool program prior to 2000 and controlling for whether or not a state-funded preschool program is present. Similarly, including state by year or even more stringent MSA by year fixed effects to control for policies or trends at the state or MSA-level (such as state pre-school programs) does not affect outcomes.[30] All employment outcomes are robust to accounting for the rise in state-funded public preschool for 4-year-olds (Appendix Table A.5). They are also robust to allowing the two major federal policies in the 1990s, the EITC and TANF, to differentially affect mothers with age-eligible and in-eligible children. A more detailed exploration of the role of concurrent welfare policies suggests the effects are not driven by these alternative policies (see Appendix B and Appendix Table A.6).

Our results are also robust to changing the sample to provide different counterfactual groups, treatment groups, or placebo treatment groups (Panel B of Table 4). Employment estimates are similar if we including mothers with children under three as the counterfactual, rather than children that are between seven and nine.[31] In column (2) we exclude mothers with four-year-olds to focus on three-year-olds and see similar effects. This provides compelling evidence that the employment response is not driven by state-funded preschool

[29]The impact by the end of the decade is large, but not inconsistent with overall patterns in employment rates for single mothers. In the CPS only 9.7 percent of single mothers had an age-eligible child and were in high funding MSAs, suggesting that the aggregate employment rate for all single mothers would have only risen by 3.8 percentage points, less than half the total increase in single mother employment rates in the 1990s.

[30]We replicated all analyses while including metropolitan area by year fixed effects. Estimates are virtually unchanged. To date, 10 states have delegated control of TANF to counties, but it is unclear when control was transferred. To some extent our robustness specifications including MSA by year fixed effects controls for this.

[31]Kleven (2019) and Looney and Manoli (2013) show that the general increase in employment among single mothers in the 1990s is largely driven by mothers with younger children. This specification can help rule out that our baseline results are simply driven by mothers of young children being more likely to leave welfare and become employed during this period.

which were directed towards four-year-olds. The treatment effect is similar when we exclude mothers with three-year-olds. Throughout our analysis we have not included mothers of five- and six-year-olds as some of them might still be Head Start eligible or entering kindergarten. In column (4) we see that using mothers of five-year-olds as the treatment group yields large employment effects. This is not surprising as, depending on date of birth, approximately half of these children would have been eligible for Head Start for eight months of the reference period before starting kindergarten. Unlike mothers of five-year-olds, mothers of six-year-olds would have had no Head Start eligibility during the reference period and provide a placebo check. As seen in column (5), using mothers of six-year-olds as the treatment group yield smaller, insignificant employment effects.[32] As a final placebo, we look at how mothers with children under 3 respond to Head Start funding relative to mothers with school age children, and see insignificant effects, close to zero.[33]

We include additional robustness analysis in Appendix B to show that estimates are robust to more specification checks (Appendix Table A.11), using education to tag potentially eligible mothers rather than marital status (Appendix Table A.12), accounting for migration and compositional changes (Appendix Table A.13), and focusing on mothers in city centers where most Head Start dollars went (Appendix Table A.14).

Heterogeneity. We next consider heterogeneous treatment effects in Table 5 by estimating equation (1) for various demographic groups. In general, we find that the groups with lower baseline employment rates are the most responsive. Consistent with less educated mothers being more likely to be eligible, $500 of Head Start funding per child has a larger effect of 2.2 percentage points, or 3.5 percent, for single mothers with a high school degree or less. As expected, the effects for mothers with any college education (who are less likely to be eligible for Head Start) are small and insignificant. Effects are large for minority single

[32]When looking at mothers of five- and six-year-olds, we also restrict the sample to exclude mothers with three- and four-year-olds to avoid mismeasuring spillover treatments.

[33]For brevity, we only include the employment outcomes in Table 4. Estimates for other outcomes across these counterfactual and placebo groups, as well as additional counterfactual groups are included in Appendix Tables A.7, A.8, A.9, and A.10.

mothers (2.6 percentage points), with no significant effect for Non-Hispanic White single mothers (although we cannot reject that the impacts for the two groups are the same).

Household structure and the mother's potential role as a primary or secondary earner differs by marital status, so we expect single mothers to have quite different employment behavior, considering differences in family settings, earning dynamics, and family resources (Blau and Tekin, 2007). By 2002, around 45% of children eligible for Head Start had married mothers (Puma et al., 2012), suggesting a diverse set of mothers who could potentially be impacted by Head Start availability for their children. Existing work exploring the impact of safety net programs on single women often do not differentiate between previously married and never married mothers. However, we find observational differences between these mothers. Single mothers are generally younger and less educated, with never married mothers even more negatively selected on characteristics predictive of labor market participation and significantly more likely to be income eligible. To further understand heterogeneity of effects, we separate estimates by mother's marital history in columns (5)-(7) in Table 5. Among never married mothers, a $500 per child increase in Head Start funding resulted in an employment increase of 2.4 percentage points. On the other hand, we find no responses among previously married mothers (separated, divorced, or widowed), suggesting that overall effects for single mothers are concentrated among never married mothers. This in part can be explained by differences in overall employment rates and average hourly wage rates. Previously married mothers are 13 percentage points more likely to be employed relative to never married mothers, suggesting that the mothers on the employment margin in these groups might be quite different. This pattern by marital status persists across all employment measures (Appendix Table A.15). For completeness we also examine impacts for married mothers, and find no impact on annual employment.

Employment responses for mothers of age-eligible children with younger children in the home were lower, but not statistically different from employment responses of mothers with age-eligible children and no younger children (see column (8) of Table 5). This is consistent with work looking at preschool or kindergarten eligibility (Cascio, 2009; Fitzpatrick, 2010; Gelbach, 2002).

In sum, groups with the largest employment responses have lower average employment rates, suggesting there may be more margin to respond due to having more women out of the labor force. Marginal mothers in these groups are probably different compared to marginal mothers in less responsive groups. Consistent with Head Start subsidizing work-related child care costs, we see that prior to the expansions, less educated, non-White, and never married mothers faced hourly wages that were \$1 to \$4 per hour lower than other single mothers (see Appendix Table A.16).[34] Head Start likely reduces child care costs, as a fraction of wages, the most for these mothers, which might explain why we see the largest responses in these groups, even as enrollment response is similar across groups (Appendix Table A.3).

7 Generalizability to the Head Start Impact Study Randomized Control Trial

Our analysis of the Head Start expansion rests on a parallel trends assumption. While the identifying assumption appears to hold, we recognize that other factors potentially influencing maternal labor supply changed during the 1990s (Kleven, 2019; Meyer and Rosenbaum, 2001). To further test the relationship between Head Start access and maternal labor supply, we supplement our analysis with evidence from the Head Start Impact Study (HSIS), a small scale experiment in 2002 where Head Start applicant families were randomly assigned access to Head Start through a lottery (U.S. Department of Health and Human Services, 2018). This study was conducted during the 2002-2003 academic year with follow-up surveys conducted through 2008 to evaluate the impacts of Head Start on children's cognitive development. Importantly, parental interviews were conducted each year, soliciting information about broad measures of maternal labor force participation. Using this experimental variation we validate the patterns observed from the 1990s. The HSIS also allows us to ex-

[34]We divide wage income by usual hours times usual weeks to roughly estimate hourly wages.

plore heterogeneity by family structure characteristics (such as presence of younger children and marital status) and program generosity (availability of full-day programming). We can also explore correlations between maternal employment, parental investments, and children's outcomes to better understand the trade-off mothers face.

Dataset and Empirical Approach. This section briefly introduces our data along with information on key variables, and Appendix C includes a detailed discussion of the study, methods, and results from the HSIS. The sample includes 4,442 first time Head Start applicants across 353 Head Start centers, with 2,646 children in the treatment group and 1,796 children in the control group. Following the approach taken by Bitler et al. (2014) to address endogenous sampling in the data collection (Solon et al., 2015), we use baseline weights and then augment weights to correct for sample attrition. When exploiting expansions in the 1990s we focused on single mothers to identify the target population. However, since all applicants in the HSIS were Head Start eligible, we do not limit our sample by marital status, but explore heterogeneous effects by marital status later. As seen in Table 6, the treatment and control groups are similar across baseline characteristics in Fall 2002, consistent with randomization. Experimentally induced access to Head Start significantly changes child care arrangements. Treated children were 74 percentage points more likely to attend Head Start, and 55 percentage points more likely to be enrolled in center-based care.[35] Access to Head Start shifted most children away from staying at home (47 percentage points), although some children moved from home-based daycares (8 percentage points). For many, access to Head Start moves child care out of the home, potentially giving the mother more time to engage in the labor force.

Because of the experimental variation, we can estimate intent to treat effects by regressing maternal labor supply outcomes of interest on an indicator for randomized treatment status, and treatment on the treated effects using two stage least squares where we use treatment

[35] 12 percent of children in the control group were able to enroll in a Head Start program. Previous work suggests that some of these children enrolled at a different center (Gelber and Isen, 2013) while others enrolled at the center of application (Feller et al., 2016). It is unclear what share followed each path.

status to instrument for Head Start enrollment.[36] The parent interviews indicate if a mother is currently participating in the labor force, if she is currently employed, and if she is employed full-time (weekly hours $\geq$35) or part-time. These measures differ from those in the CPS, as they only capture current employment, not annual employment. As low-income women transition in and out of employment somewhat frequently, using current employment makes it harder to detect effects. We first estimate impacts for the full sample, then focus on effects when the Head Start center offers full day services or if there are not younger children in the household.

Results. Table 7 reports the impacts of Head Start on maternal labor supply (treatment on the treated effects).[37] In the full sample we see a marginally significant 4.4 percentage point (14 percent) increase in the probability of being employed full-time. If the Head Start center the family applied to offered full-day programming, Head Start enrollment increased full-time employment by 7.7 percentage points (24 percent).[38] Mothers with children under three were marginally less likely to work part-time, while mothers without younger children were marginally more likely to be in the labor force.

Single mothers are likely more constrained in their ability to specialize across employment and child care than married mothers, and are less likely to operate as secondary earners. As in the 1990s, even among unmarried mothers in the HSIS, separated/divorced/widowed mothers had higher baseline attachment than never married mothers and were more positively selected along dimensions predictive of labor force attachment. As such, we estimate the impact of Head Start on labor supply separately for never married, separated/divorced/widowed

[36]In both specifications we restrict the sample to households where the biological or adoptive mother is in the home and include month of interview fixed effects to control for differences in the timing of interviews and adjust standard errors for clustering at the Head Start Center level. See Appendix C for details and exact regression equation. Alternatively, one could use Head Start assignment to instrument for any out-of-home child care. Since most recipients substitute away from home care (see Table 6) this leads to a slightly smaller first stage and larger treatment on the treated estimates.

[37]The reduced form intent to treat effects are provided in Appendix Table A.17.

[38]Importantly, full-day programming is a center based measure, not individual specific. Although there might be selection into who applies to centers that offer full-day, individuals are randomized after this selection. In Appendix Table A.18 we show that treatment and control households are similar when stratified by whether the center offers full-day programming, the presence of a younger child, or marital status.

mothers, and married mothers in Table 8.[39] Never married mothers were 10.3 percentage points more likely to be in the labor force, 7.7 percentage points more likely to be employed, and 11.5 percentage points more likely to be employed full-time.[40]

Table 8 further explores heterogeneity in the availability of full-day services and the presence of younger children by marital status. Never married mothers who applied to centers that offered full-day services were significantly more likely to be in the labor force (17.2 percentage points), employed (14 percentage points), and employed full-time (17.4 percentage points) when their children enrolled in Head Start. Never married mothers without younger children were more likely to be in the labor force (14.6 percentage points) and employed full-time (13.9 percentage points) when their child enrolled in Head Start. These findings suggest never married mothers without younger children and never married mothers with access to full-day care were most likely to respond when Head Start became available.

The HSIS sample is relatively small, and many of the coefficients are estimated imprecisely with large coefficients, suggesting the experiment might be underpowered. However, we do find evidence that Head Start provides access to child care and increases employment among some groups, like never married mothers without younger children and those who applied to Head Start centers that offer more generous full-day programming.

Maternal Employment and Parenting Investments It is not clear how public investments in children through pre-school (and the accompanying changes in maternal employment) relate to parenting investments at home and resulting child outcomes. In some settings, universal child care has been shown to increase maternal employment but lower quality parent-child interactions at home (Baker et al., 2008). However, prior research us-

[39] Marital status is measured in Fall 2002 at the beginning of the experiment and held fixed throughout. We interact marital status rather than stratify the sample to avoid disclosure problems and avoid small samples. Estimates are similar if stratified.

[40] A concurrent paper using the HSIS finds positive impacts for married mothers of 3-year-olds, but no effects for mothers of 4-year-olds and unmarried mothers (Schiman, 2021). Her analysis differs from ours. First, she stratifies by cohort (3 vs. 4) and she does not separately examine effects for previously married and never married mothers. As seen in Appendix C, if we replicate her specification but pool 3- and 4-year-olds we find a pattern similar to ours; moderate, insignificant effects for married mothers, and large significant effects for never married mothers. Both her analysis and ours are consistent with more modest labor supply effects for married mothers and large labor supply effects for never married mothers.

ing the HSIS finds that Head Start is associated with increased time reading with children, increased math involvement, increased time with non-resident fathers, and more child involvement in cultural enrichment activities, with many of these effects persisting beyond the treatment year (Gelber and Isen, 2013; Puma et al., 2012). Building on the analysis of Gelber and Isen (2013), we construct index measures in four domains of parental time investment: reading/language, math, cultural activities, and preventative medical care provision. We find that the subgroups that experienced the largest employment effects also experienced increases in the parental time investments measures (Table 9), with no evidence that maternal employment crowded out other parental time investments.

Stronger maternal labor force attachment could inhibit children's learning. However, an increase in maternal employment and the corresponding income could also directly influence children's cognitive outcomes (Almond and Currie, 2011). Since only Head Start eligibility is randomized, we cannot test this relationship experimentally.[41] However, we can see if the same groups that saw increases in maternal employment also saw improvements in children's cognitive scores. Following Bitler et al. (2014) we explore the impact of Head Start access on children's Peabody Picture Vocabulary Test (PPVT) and Woodcock Johnson III (WJIII) pre-academic skills test by mother's marital status, full-day programming, and presence of younger children (as above). The impact on children's test scores is then plotted against the impact on maternal full-time employment in Figure 4.[42] For both the PPVT and the WJIII there is a strong positive relationship. Mothers whose employment was more responsive to the Head Start treatment had children that experienced the largest cognitive gains.[43] These patterns must be interpreted with caution as both treatment and the size of treatment effects potentially differ across these groups. For example, never married mothers might be more negatively selected on many dimensions and Head Start might have a larger treatment effect on their children, for reasons unrelated to the mother's work status. Similarly, receiving full-

[41]Due to poor income measures in the HSIS, we do not explore connections between income and children's outcomes. See Appendix C for more detail.

[42]The coefficients on cognitive outcomes are reported in Appendix Table A.19.

[43]The patterns is consistent if we estimate standard deviation impacts.

day Head Start programming might relax mothers' time constraints, but also likely represents a more intensive treatment. However, consistent with maternal employment not inhibiting learning and even, potentially, aiding the learning process, the impacts are larger among groups that experienced employment responses. Although far from definitive, this would be consistent with maternal employment and earnings contributing to the short-run cognitive impacts of Head Start.

Persistence. From the Head Start Impact Study, we examine how experimentally induced Head Start enrollment affects maternal labor supply for up to five years after the preschool treatment. Using the same two-stage least squares strategy, we follow children through third grade. We suspect groups with the strongest initial treatment response would be most likely to demonstrate persistent effects, so we explore effects among never married mothers, including those applying to centers with full-day care and those with no children under age three. Among never married mothers we find no evidence of persistent effects on labor force participation (Appendix Figure A.5), employment (Appendix Figure A.6), or full-time employment (Appendix Figure A.7). If we focus on never married mothers at Head Start centers that offer full-day services or without younger children – the groups that experienced the largest effects in the treatment year — we see comparable sized impacts reemerging in 2006 (once all children have reached first grade). Overall we do not find strong evidence that Head Start leads to persistent increases in labor force attachment. Even among the groups with the strongest response during the treatment year, we only find weak, suggestive evidence that labor supply is significantly higher up to five years after the treatment. Controlling for baseline characteristics does not significantly increase precision. This might be because all mothers gain access to public child care when their children entered kindergarten a year or two later. A larger sample or alternative strategy is needed to make more conclusive statements about the persistence of these effects. [44]

[44] We have also looked to see if children's cognitive impacts persist for these groups. Consistent with the absence of long-run employment effects, we do not find long run cognitive effects in these groups. Our empirical strategy used to explore the 1990s is not suited for estimating long-run impacts as the treatment occurs over subsequent years.

8 Discussion & Conclusion

Our study of Head Start reveals that publicly provided preschool had a statistically and economically significant effect on employment outcomes among single mothers with eligible young children, increasing their employment rate by 1.9 percentage points, their usual hours worked by 7.6 percent, and their income by 15.3 percent. Effects were strongest among groups with low baseline employment rates and low hourly wages who were more likely to be eligible, such as less educated mothers, minorities, and never married mothers. Our work suggests that child care subsidies remain an important policy lever in encouraging the welfare-to-work transition of disadvantaged mothers. However, it appears as though the subsidy must be generous enough (full-day) to elicit a strong employment response. Our findings of labor supply responses to Head Start are not unique to one dataset, cohort, or decade but instead reflect an empirical regularity found across cohorts and time. This strengthens the external validity and policy relevance of our findings.

Our estimates from the 1990s and HSIS represent local average treatment effects at two points. These estimates remain difficult to compare. First, the estimate from the 1990s Head Start expansion was calculated as an intent-to-treat estimate, while the HSIS estimate was a treatment on the treated estimate. Second, the employment measure from the 1990s measured whether a mother was employed at any time during the previous year, while the HSIS measured whether a mother was employed at the time of the spring interview. Third, the 1990s analysis focused on single mothers while the HSIS evaluated all mothers. Interpreting these effects relative to each other comes with these caveats in mind. The 1990s analysis yielded a Wald estimate of 34% for single mothers. The HSIS yielded an effect size of 16% for never married mothers. The smaller effect size in the HSIS compared to the 1990s expansion is in part attributed to the less inclusive employment measure used in the HSIS, but also might be due to differences in time (1990s versus 2003), or selection among who opts in to the HSIS experiment.

Our findings are consistent with the previous research of Gelbach (2002) and Cascio (2009), which finds that public provision of educational services for young children led to increased maternal labor supply for single mothers without younger children prior to 1990. Using estimates from Cascio (2009) on the percent increases in employment and enrollment yields an elasticity of 0.38, similar to our estimate of 0.34.[45] Our findings diverge from similar work by Fitzpatrick (2010) and Cascio and Schanzenbach (2013). Both studies explore the impact of universal pre-kindergarten in Oklahoma and Georgia on maternal labor supply (as well as other outcomes). Fitzpatrick (2010) uses a regression discontinuity to explore the employment decisions of mothers with children just above and just below the age eligibility threshold. She finds no systematic evidence of employment effects. Cascio and Schanzenbach (2013) exploit the introduction of these universal programs (in 1995 and 1998) in a difference in differences framework, and only find weak evidence of a short-run employment response in contrast to our finding of stronger effects. We see a potential explanation for the difference. Because means-tested preschool programs like Head Start were available to low-income children in Oklahoma and Georgia before universal eligibility, many children of single mothers were eligible for subsidized preschool even before the expansion to universal pre-kindergarten. Accordingly, pre-kindergarten expansion was likely most salient for families in other parts of the income distribution.

Existing studies of early childhood programs suggest returns of between \$1.60 and \$5.90 for every \$1 spent (Bartik et al., 2012; Cascio and Schanzenbach, 2013; Duncan et al., 2010; Heckman et al., 2010; Kline and Walters, 2016; Ludwig and Miller, 2007). The meta-analysis by Duncan and Magnuson (2013) in particular implies a benefit-cost ratio to a child of over \$2 for every \$1 spent on Head Start.[46]. The short-run increase in maternal employment and income provide another benefit that have not been included when evaluation the costs

[45] Cascio estimates a partial elasticity of 0.79. Given a 12 percent increase in employment and a 15.2 percentage point increase in enrollment off of a preinitiative mean of 0.48, this would yield an elasticity of 0.38 (0.12/(0.152/0.48)).

[46] These estimates also do not include the intergenerational benefits of Head Start (Barr and Gibbs, 2022), or its impact on criminal activity (Heckman et al., 2010; Johnson and Jackson, 2019)

and benefits of Head Start. For a \$500 increase in Head Start funding per eligible child, the average wage income of single mothers with an age-eligible child increased contemporaneously by 15.3 percent, translating into an average increase of \$2,334 (2017\$). This increase in income also affects welfare transfers and tax liability. As seen in Appendix Table A.20, Head Start funding is associated with a significant 21.4% reduction in welfare income (\$461 at the mean), 25.9% increase in federal tax liability (\$255 at the mean), and 20.8% reduction in the EITC refund (\$245 at the mean).[47]

Given these effects, we provide back-of-the-envelope calculations of the net cost of Head Start to the government (the extent to which Head Start immediately "pays for itself") and the immediate net benefit to single mothers. To weigh the overall cost of the Head Start program against the benefit of increased income to single mothers, we estimate the total number of age-eligible children with single mothers in each metropolitan area. Approximately one fifth of age-eligible children lived in single mother households, suggesting that a \$500 increase in funding per child corresponds to approximately a \$2,500 increase per eligible child in a single mother household. With the acknowledgement that composition effects play into earnings changes, this would suggest that income for single mothers increased immediately by \$0.93 for each dollar that was spent on the program. The change in welfare payments and tax revenue would imply the government immediate re-coups \$0.38 $((461+255+245)/2,500)$ for each dollar spent on the program. The net benefit to single mother households (earned income minus forgone welfare and additional taxes) is \$0.55 $((2,334-461-255-245)/2,500)$ for each dollar. The short-run benefit to society (single mother recipients plus the government) is \$0.93 for each dollar spent, suggesting the short-run benefits nearly cover the costs of provision, even before counting long-run benefits to children.[48] These findings suggest

[47]There is a marginally significant reduction in the probability of receiving Medicaid, but no significant effect on the rate of free school lunch, SSI income, Food Stamp value, FICA liability, or state tax liability. Head Start programs generally verified income prior to the academic year, so a child was unlikely to lose program access later in the year due to increased household earnings. The average drop in welfare income is slightly less than would be expected based on the wage income effects in Table 3 and TANF's 50 percent benefit reduction rate, but this is likely due to documented underreporting of welfare income in the CPS (Meyer and Mittag, 2019).

[48]If the short-run increase in maternal employment and income contributes to the improved long-run

that Head Start plays an important role in the anti-poverty space as a part of the portfolio of government means-tested programs.

As we saw in the HSIS, the increase in employment and income from improved access to Head Start did not appear to come at the expense of parent investments and involvement with children outside of school. Our findings are consistent with previous research suggesting public investments in early childhood education and parent investments may be complements for low-income families (Gensowski et al., 2020). Our findings are less aligned with research by Baker et al. (2008) which reports worse parenting following the introduction of universal child care in Quebec, Canada. These differences in results suggest contextual factors such as children's ages when receiving care, living with one versus two parents, and family income may interact with parenting investments as mothers adjust labor supply. This remains an important area for future research. In the HSIS, the increases in maternal employment and children's cognitive scores are positively correlated, suggesting any maternal employment induced by the program did not counteract the goals of the program. Our analysis of the CPS finds an increase in maternal income, and there is suggestive evidence of increased income in the HSIS. Given the existing evidence (Almond and Currie 2011), income investments in children can improve child outcomes and may help explain these findings. Because of imprecision, we detect only limited evidence of persistent effects of Head Start on maternal labor supply. More work is needed to better understand the long-run impacts of subsidized early childhood education and its implicit child care subsidy on maternal labor supply. Overall, access to Head Start explains an economically meaningful increase in employment rates among single mothers with young children. These patterns of responses to Head Start access can help us better understand how public preschool programs affect children, mothers, and families.

outcomes for children, then the existing estimates (Bailey et al., 2021), already capture the long-run value of increasing maternal employment. These benefit estimates however do not include the direct short-run benefit of higher employment, tax revenue, and welfare independence.

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Tables and Figures

Table 1: Summary Statistics for Analysis Sample Single Mothers, 1984-2000

	Below Median Increase in Funding from 1989 to 1999			Above Median Increase in Funding from 1989 to 1999			
	Had 3-4 Year old Last Year (1)	No 3-4 Year old Last Year (2)	Diff. (3)	Had 3-4 Year old Last Year (4)	No 3-4 Year old Last Year (5)	Diff. (6)	(6)-(3) (7)
Change in HS Funding per Child		372			693		
Employed Last Year	0.67	0.77	-0.10	0.63	0.72	-0.09	0.01
Employed Full-Year Last Year	0.38	0.51	-0.13	0.34	0.45	-0.11	0.02
Employed Part-Year Last Year	0.29	0.26	0.03	0.28	0.27	0.02	-0.01
Weeks Worked Last Year	27.29	34.17	-6.88	25.06	30.95	-5.90	0.98
Wage Income (2017 Dollars)	13,969	19,968	-5,999	11,358	15,829	-4,471	1,529***
Non-Hispanic White	0.48	0.52	-0.04	0.45	0.50	-0.05	-0.01
Non-Hispanic Black	0.38	0.36	0.02	0.35	0.30	0.05	0.03**
Non-Hispanic Other	0.02	0.02	0.00	0.02	0.02	-0.00	-0.00
Hispanic	0.11	0.09	0.01	0.17	0.17	0.00	-0.01
Age	29.30	34.65	-5.35	29.40	34.74	-5.34	0.00
Number of Children	2.28	2.20	0.08	2.35	2.30	0.05	-0.03
Age of Youngest Child	3.49	7.55	-4.06	3.48	7.36	-3.88	0.18**
Observations	6,597	7,292		9,433	10,469		

Notes: CPS ASEC 1984-2000. Sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year. Sample means are weighted, using the individual level ASEC weights. Column (7) indicates statistically significant differences between column (6) and column (3) when correcting for clustering at the MSA-level. There were 143 MSAs with below median funding and 147 MSAs with above median funding. p<0.01 ***, p<0.05**, p<0.1*.

Table 2: Impact of 1990s Head Start Expansion Funding on School Enrollment Among Children of Single Mothers

	In School		
	All	States without Pre-K Program	Control for State Pre-K Program
	(1)	(2)	(3)
Head Start Funding per	0.060***	0.032***	0.060***
Child (3-4 yr.)$_{t-1}$ $*$ Age 3-4	(0.014)	(0.008)	(0.014)
Head Start Funding per	-0.021	-0.015	-0.019
Child (3-4 yr.)$_{t-1}$	(0.016)	(0.025)	(0.016)
Dependent Mean	0.76	0.77	0.76
Observations	20,285	5,102	20,285

Notes: Data from the CPS October education supplement 1989-2000 repeated cross sections. Prior to 1989, the metropolitan area identifier is not available in the October supplement. Sample restricted to 3-, 4-, 7-, 8-, and 9-year-olds with single mothers in the October Supplement to be consistent with the main triple difference specification. The dependent variable "In School" indicates if the child is currently enrolled in any school. Head Start Funding per Child is measured at the MSA level in units of \$500 (2017\$). Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the federal and state minimum wage, and whether the state has a child's health insurance program (SCHIP) in place. MSA and year fixed effects are included. These regressions are weighted using the individual monthly CPS weights. To verify that the effects are not driven by simultaneous expansions of state-run public preschools, Column (2) excludes children in states that have not implemented a state pre-kindergarten program by 2000, the end of the sample. Column (3) includes the full analysis sample but additionally controls for whether there is a pre-kindergarten program in the state. Standard errors are corrected for clustering at the MSA level, with 290 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table 3: Impact of 1990s Head Start Funding on Labor Market Outcomes of Single Mothers

				Inverse Hyperbolic Sine of		
	Employed	Full-time	Part-time	Weeks Worked	Usual Hours Worked	Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
HS Funding per Child$_{t-1}$	0.019***	0.017**	0.002	0.072***	0.076***	0.153**
*Have Child 3-4 in t-1	(0.006)	(0.008)	(0.005)	(0.025)	(0.025)	(0.060)
HS Funding per Child$_{t-1}$	-0.017	-0.037**	0.020*	-0.047	-0.080	-0.245
	(0.016)	(0.019)	(0.012)	(0.072)	(0.067)	(0.178)
Have Child 3-4 in t-1	-0.093***	-0.099***	0.006	-0.471***	-0.413***	-1.045***
	(0.009)	(0.010)	(0.007)	(0.039)	(0.038)	(0.091)
Dependent Mean	0.70	0.54	0.16	29.39	25.74	15254.73
Observations	33,791	33,791	33,791	33,791	33,791	33,791

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year. Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the federal and state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. Estimates are similar if the natural log plus one is used. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table 4: Robustness: Impact of 1990s Head Start Funding on Employment of Single Mothers

Panel A. Robustness: Contemporaneous Policy (State Funded Preschool, Welfare Reform, EITC)						
Outcome: Any Employment in t-1	Exlcude States with State-funded Public Pre-K by 2000 (1)	Control for Existence of State-funded Pre-K (2)	State by Year Fixed Effects (3)	MSA by Year Fixed Effects (4)	TANF by Age Controls (5)	EITC by Age Controls (6)
Head Start Funding per Child$_{t-1}$	0.012**	0.019***	0.018***	0.020***	0.014**	0.014**
*Have Child 3-4 in t-1	(0.005)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
Head Start Funding per Child$_{t-1}$	-0.005	-0.018	-0.028*	0.000	-0.015	-0.015
	(0.014)	(0.015)	(0.017)	(0.000)	(0.016)	(0.016)
Have Child 3-4 in t-1	-0.063***	-0.093***	-0.091***	-0.093***	-0.094***	-0.101***
	(0.015)	(0.009)	(0.009)	(0.009)	(0.009)	(0.012)
Dependent Mean	0.76	0.70	0.70	0.69	0.70	0.70
Observations	7,503	33,791	33,791	33,216	33,791	33,791

Panel B. Robustness: Counterfactual and Placebo Samples						
Outcome: Any Employment in t-1	Counterfactual: Child Under 3 (1)	Treated Sub-Sample Child Age 3 (2)	Treated Sub-Sample Child Age 4 (3)	Treated Sub-Sample Child Age 5 (4)	Placebo Treated Child Age 6 (5)	Placebo Treated Child Under 3 (6)
Head Start Funding per Child$_{t-1}$	0.019***	0.019***	0.015**	0.031***	0.013	0.008
*Have Eligible Child in t-1	(0.006)	(0.007)	(0.007)	(0.008)	(0.012)	(0.008)
Head Start Funding per Child$_{t-1}$	-0.006	-0.011	-0.012	-0.005	-0.016	-0.005
	(0.018)	(0.017)	(0.015)	(0.016)	(0.015)	(0.016)
Have Eligible Child in t-1	0.009	-0.090***	-0.080***	-0.086***	-0.037***	-0.146***
	(0.009)	(0.012)	(0.010)	(0.011)	(0.013)	(0.010)
Dependent Mean	0.63	0.71	0.72	0.73	0.74	0.68
Observations	37,286	25,273	25,500	22,326	22,087	32,471

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. In Panel A, sample restricted to single mothers with a 3-, 4-, 7-, 8-, or 9-year-old last year. Column (1) excludes individuals in states with a state-funded preschool program by 2000. Column (2) controls for whether the state currently has state-funded preschool. Column (3) includes state by year fixed effects, to account for state-level policy variation. Column (4) includes MSA by year fixed effects. The sample is smaller because MSA-by-year singletons are dropped. Column (5) includes TANF waiver indicators interacted with age group (3-4) to allow TANF to affect mothers with older and younger children differently. Column (6) is similar to (5), but interacts the household-specific maximum EITC with age group. In Panel B, we estimate equation (1) with different samples. Column (1) includes mothers with children under 3 as the counterfactual, rather than children 7-9. Column (2) excludes mothers with 4-year-olds. Column (3) excludes mothers with 3-year-olds. Column (4) only includes mothers with 5-year-olds in the treatment group. This group is not included in our baseline analysis because not all of the children are age-eligible for Head Start. Column (5) only includes mothers with 6-year-olds in a placebo treatment group. This group is not included in our baseline analysis because they are too old for preschool. Column (6) includes placebo estimates using mothers with children under 3 as the treatment group, compared to mothers with children 7-9. The same weighting, clustering, and controls from Table 3 are used. p<0.01 ***, p<0.05**, p<0.1*.

Table 5: Heterogeneity in the Impact of 1990s Head Start Funding on Labor Market Outcomes of Single Mothers

	Outcome: Any Employment in t-1							
	HS or Less (1)	Any College (2)	Non-Hispanic White (3)	Non-White and Hispanics (4)	Never Married (5)	Separated/ Divorced (6)	Only Married (7)	All Single Moms (Diff. by Age Youngest) (8)
HS Funding per Child$_{t-1}$	0.022**	0.011	0.008	0.026***	0.024***	0.008	0.004	0.017**
*Have Child 3-4 in t-1	(0.009)	(0.008)	(0.010)	(0.007)	(0.008)	(0.008)	(0.004)	(0.007)
HS Funding per Child$_{t-1}$	-0.007	-0.022	-0.022	-0.002	-0.033	-0.003	-0.002	-0.019
	(0.022)	(0.017)	(0.019)	(0.026)	(0.027)	(0.018)	(0.010)	(0.016)
Have Child 3-4 in t-1	-0.112***	-0.052***	-0.074***	-0.106***	-0.087***	-0.073***	-0.085***	-0.069***
	(0.012)	(0.011)	(0.013)	(0.012)	(0.015)	(0.010)	(0.005)	(0.010)
Head Start Funding$_{t-1}$								
*Child 3-4 in t-1								-0.014
*Youngest 0-2 in t-1								(0.024)
Head Start Funding$_{t-1}$								0.035*
*Youngest 0-2 in t-1								(0.020)
Child 3-4 in t-1								0.000
*Youngest 0-2 in t-1								(0.031)
Youngest 0-2 in t-1								-0.171***
								(0.025)
Dependent Mean	0.62	0.85	0.78	0.62	0.61	0.74	0.69	0.70
Observations	23,067	10,721	16,207	17,569	11,729	22,049	111,147	33,791

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year. Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Columns (5)-(7) are mutually exclusive, and column (7) does not include single mothers. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the federal and state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. The coefficients on Head Start funding interacted with having an age-eligible child are significantly different between columns (1) and (2) (p-value of 0.07); not significant between columns (3) and (4); and marginally significant between columns (5) and (6) (p-value of 0.109). p<0.01 ***, p<0.05**, p<0.1*.

Table 6: HSIS Child Care Characteristics and Covariate Balance by Treatment Status, Fall 2002

	Control (1)	Treated (2)	Difference (3)
Child in Head Start	0.12	0.86	0.74***
Child in Center-based Care	0.38	0.93	0.55***
Child in Home Daycare	0.09	0.01	-0.08***
Child at Home	0.53	0.06	-0.47***
In Care of Teacher/Head Start	0.37	0.93	0.55***
In Care of Parent/Guardian	0.48	0.06	-0.43***
In Care of Other	0.14	0.02	-0.13***
Child Female	0.49	0.51	0.01
White NH	0.32	0.30	-0.02
Black NH	0.30	0.30	0.00
Other NH	0.03	0.03	0.00
Hispanic	0.35	0.36	0.02
Race Missing	0.01	0.01	0.00
Mom 20-24	0.27	0.27	-0.01
Mom 25-29	0.33	0.32	-0.01
Mom 30-39	0.31	0.32	0.01
Mom 40+	0.05	0.06	0.01
< High School	0.38	0.37	-0.01
High School	0.32	0.33	0.01
Some College	0.25	0.25	-0.00
College	0.04	0.04	0.00
Educ. Missing	0.01	0.01	-0.00
Married	0.45	0.44	-0.00
Sep./Divorced/Widow	0.16	0.16	0.00
Never Married	0.39	0.39	-0.00
Child Under 3	0.40	0.36	-0.04**
Didn't Respond in Fall 2002	0.21	0.21	0.00
P-value on Joint F-test			0.90
Observations	1,796	2,646	

Notes: All demographic measures constructed from the Fall 2002 Parent Interview. Estimates are weighted using inverse probability weights. $p<0.01$ ***, $p<0.05$**, $p<0.1$*.

Table 7: HSIS Impact of Head Start on Maternal Labor Supply (Treatment on the Treated)

	In Labor Force (1)	Employed (2)	Full-time (3)	Part-time (4)
		All		
Head Start	0.038	0.020	0.044*	-0.024
	(0.029)	(0.029)	(0.027)	(0.022)
Control Mean	0.58	0.49	0.31	0.18
Number of Centers		334		
Observations		3,117		
		HS Center Offers Full Day		
Head Start	0.061	0.045	0.077**	-0.032
	(0.038)	(0.036)	(0.035)	(0.029)
Control Mean	0.59	0.49	0.32	0.17
Number of Centers		198		
Observations		1,829		
		HS Center Does Not Offer Full Day		
Head Start	-0.008	-0.029	-0.010	-0.019
	(0.045)	(0.049)	(0.043)	(0.037)
Control Mean	0.56	0.48	0.30	0.19
Number of Centers		113		
Observations		1,128		
		Child Under 3		
Head Start	-0.020	-0.045	0.023	-0.068*
	(0.049)	(0.048)	(0.039)	(0.035)
Control Mean	0.53	0.44	0.26	0.18
Number of Centers		286		
Observations		1,181		
		No Child Under 3		
Head Start	0.065*	0.051	0.051	-0.000
	(0.037)	(0.036)	(0.035)	(0.027)
Control Mean	0.61	0.52	0.34	0.17
Number of Centers		321		
Observations		1,936		

Notes: Sample restricted to households that participated in the Spring 2003 Parent Interview with a mother in the household. In the Labor Force is measured as employed full-time, part-time, looking for work, laid off from work, or in the military. Employed is either full- or part-time employed. Full-time employed is employed for 35 hours or more a week. Head Start enrollment is instrumented for using original Head Start treatment assignment as the instrument. Month of interview fixed effects are included. All regressions are weighted using inverse probability weights constructed from the Spring 2003 wave. Standard Errors are clustered at the Head Start Center the household applied to and was assigned treatment. p<0.01 ***, p<0.05**, p<0.1*.

Table 8: HSIS Impact of Head Start on Maternal Labor Supply by Marital Status

	All (1)	HS Center Offers Full Day (2)	HS Center Does Not Offer Full Day (3)	Child Under 3 (4)	No Child Under 3 (5)
			In Labor Force		
Head Start*Married	-0.000	0.005	-0.005	-0.056	0.019
	(0.040)	(0.053)	(0.060)	(0.068)	(0.049)
Head Start*Sep./Divorced/Widowed	-0.001	-0.076	0.081	-0.120	0.031
	(0.077)	(0.094)	(0.133)	(0.130)	(0.086)
Head Start*Never Married	0.103**	0.172***	-0.048	0.046	0.146**
	(0.045)	(0.059)	(0.068)	(0.078)	(0.058)
Control Mean	0.58	0.59	0.56	0.53	0.61
Number of Centers	334	198	113	286	321
Observations	3,117	1,829	1,128	1,181	1,936
			Employed		
Head Start*Married	0.003	0.014	-0.018	-0.092	0.047
	(0.041)	(0.053)	(0.064)	(0.072)	(0.046)
Head Start*Sep./Divorced/Widowed	-0.071	-0.139	0.003	-0.164	-0.049
	(0.082)	(0.104)	(0.137)	(0.140)	(0.095)
Head Start*Never Married	0.077*	0.140**	-0.055	0.063	0.095
	(0.045)	(0.057)	(0.082)	(0.075)	(0.059)
Control Mean	0.49	0.49	0.48	0.44	0.52
Number of Centers	334	198	113	286	321
Observations	3,117	1,829	1,128	1,181	1,936
			Employed Full-time		
Head Start*Married	0.029	0.041	0.019	0.008	0.032
	(0.035)	(0.049)	(0.048)	(0.052)	(0.046)
Head Start*Sep./Divorced/Widowed	-0.085	-0.097	-0.093	-0.09	-0.112
	(0.078)	(0.108)	(0.121)	(0.133)	(0.095)
Head Start*Never Married	0.115**	0.174***	0.003	0.086	0.139**
	(0.047)	(0.062)	(0.084)	(0.070)	(0.061)
Control Mean	0.31	0.32	0.30	0.26	0.34
Number of Centers	334	198	113	286	321
Observations	3,117	1,829	1,128	1,181	1,936
			Employed Part-time		
Head Start*Married	-0.026	-0.027	-0.037	-0.100**	0.015
	(0.031)	(0.040)	(0.053)	(0.050)	(0.038)
Head Start*Sep./Divorced/Widowed	0.013	-0.041	0.096	-0.065	0.063
	(0.057)	(0.090)	(0.070)	(0.107)	(0.065)
Head Start*Never Married	-0.038	-0.034	-0.058	-0.023	-0.044
	(0.034)	(0.044)	(0.058)	(0.053)	(0.045)
Control Mean	0.18	0.17	0.19	0.18	0.17
Number of Centers	334	198	113	286	321
Observations	3,117	1,829	1,128	1,181	1,936

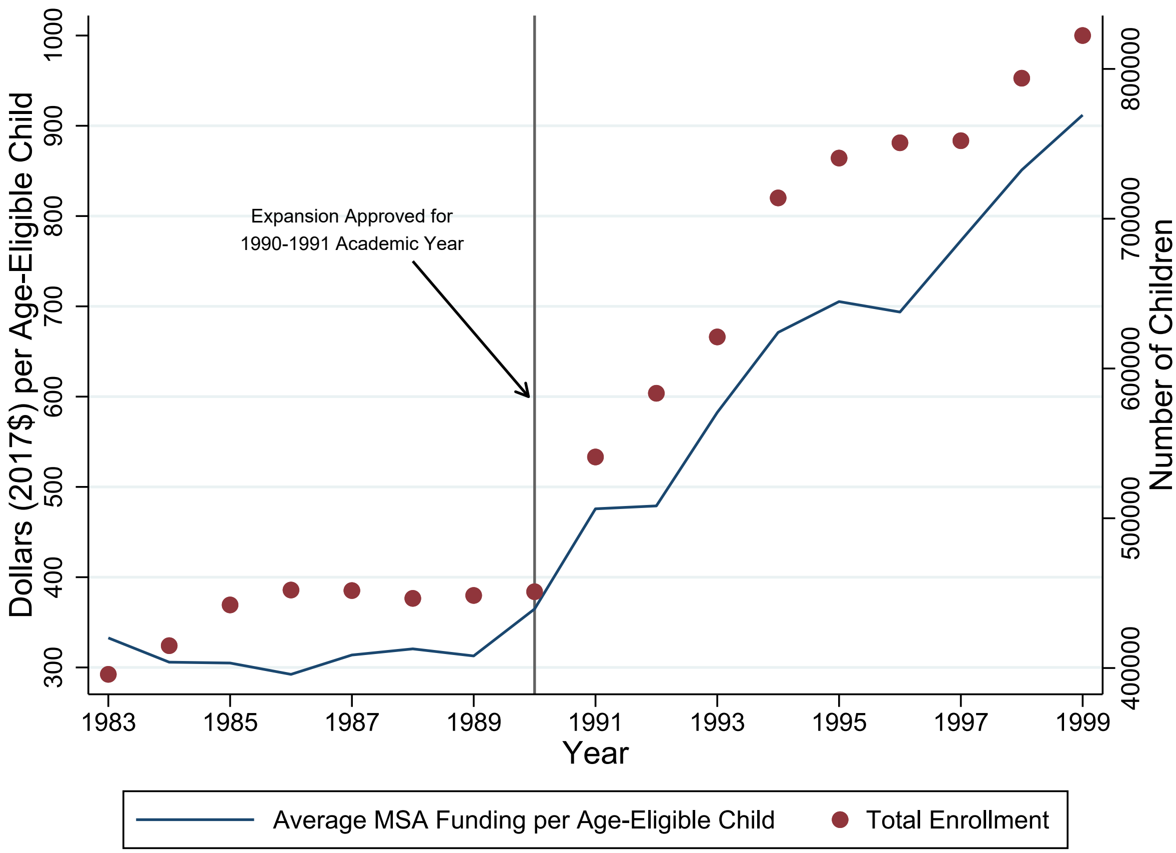
Notes: Sample restricted to households that participated in the Spring 2003 Parent Interview with a mother in the household. In the Labor Force is measured as employed full-time, part-time, looking for work, laid off from work, or in the military. Employed is either full- or part-time employed. Full-time employed is employed for 35 hours or more a week. Head Start enrollment is instrumented for using original Head Start treatment assignment as the instrument. There is no constant included, thus allowing the inclusion of "Married", "Sep./Divorced/Widowed", and "Never Married". Full day offering is determined from the Center Director's interview. Attempts were made to contact the director for each child in center based child care, who was then asked if the center offered full day programming. The presence of younger children was determined by examining the household roster to determine if any children under three were present. Month of interview fixed effects are included. All regressions are weighted using inverse probability weights constructed from the Spring 2003 wave. Standard Errors are clustered at the Head Start Center the household applied to and was assigned treatment. $p<0.01$ ***, $p<0.05$**, $p<0.1$*.

Table 9: HSIS Impact of Head Start on Parental Investment Measures

	All (1)	HS Center Offers Full Day (2)	HS Center Does Not Offer Full Day (3)	Child Under 3 (4)	No Child Under 3 (5)
	Reading and Language Investment Activity Index				
Head Start*Married	0.200***	0.162**	0.172**	0.207**	0.200***
	(0.051)	(0.07)	(0.068)	(0.083)	(0.064)
Head Start*Sep./Divorced/Widowed	0.131	0.267	-0.01	0.133	0.105
	(0.101)	(0.164)	(0.124)	(0.169)	(0.123)
Head Start*Never Married	0.247***	0.244***	0.227**	0.155	0.300***
	(0.069)	(0.093)	(0.11)	(0.118)	(0.083)
Number of Centers	334	198	113	284	320
Observations	3,055	1,791	1,110	1,158	1,897
	Math Investment Activity Index				
Head Start*Married	0.203***	0.154**	0.205***	0.246***	0.181***
	(0.052)	(0.073)	(0.071)	(0.092)	(0.062)
Head Start*Sep./Divorced/Widowed	0.199*	0.358**	0.016	0.07	0.254**
	(0.105)	(0.171)	(0.135)	(0.168)	(0.121)
Head Start*Never Married	0.310***	0.328***	0.274**	0.181	0.383***
	(0.067)	(0.089)	(0.107)	(0.113)	(0.087)
Number of Centers	334	198	113	283	321
Observations	3,088	1,813	1,117	1,168	1,920
	Cultural Activity Attendance Index				
Head Start*Married	0.05	0.01	0.092**	0.021	0.074*
	(0.036)	(0.054)	(0.045)	(0.06)	(0.044)
Head Start*Sep./Divorced/Widowed	0.054	0.039	0.126	-0.131	0.139
	(0.072)	(0.113)	(0.087)	(0.106)	(0.096)
Head Start*Never Married	0.104**	0.076	0.137*	0.032	0.149**
	(0.044)	(0.056)	(0.078)	(0.079)	(0.058)
Number of Centers	334	198	113	286	320
Observations	3,105	1,820	1,131	1,173	1,932
	Child Medical Car Provision Index				
Head Start*Married	0.426***	0.340***	0.522***	0.492***	0.381***
	(0.048)	(0.062)	(0.072)	(0.08)	(0.064)
Head Start*Sep./Divorced/Widowed	0.654***	0.559***	0.760***	0.717***	0.602***
	(0.095)	(0.144)	(0.117)	(0.148)	(0.125)
Head Start*Never Married	0.433***	0.343***	0.534***	0.398***	0.450***
	(0.062)	(0.083)	(0.082)	(0.088)	(0.076)
Number of Centers	333	197	113	280	319
Observations	3,000	1,773	1,071	1,126	1,874

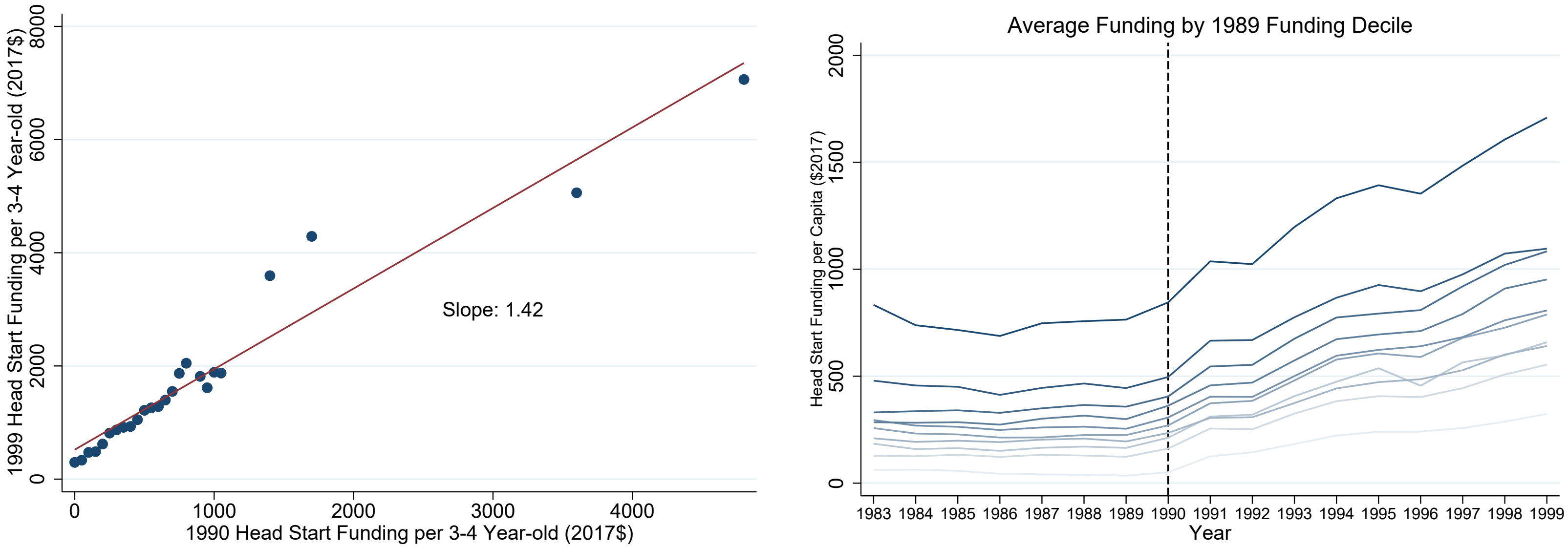
Notes: Sample restricted to households that participated in the Spring 2003 Parent Interview with a mother in the household. Head Start enrollment is instrumented for using original Head Start treatment assignment as the instrument. There is no constant included, thus allowing the inclusion of "Married", "Sep./Divorced/Widowed", and "Never Married". Full day offering is determined from the Center Director's interview. Attempts were made to contact the director for each child in center based child care, who was then asked if the center offered full day programming. Month of interview fixed effects are included. All regressions are weighted using inverse probability weights constructed from the Spring 2003 wave. Parental Investment indices are constructed using parental investment measures from Gelber and Isen (2013). To construct each index we subtract the mean and divide by the standard deviation of the control group for each individual measure. We then average all items in the scale. The reading activity index includes 12 items regarding how often the parent reads to and practices letters and spelling with the child. The math activity index includes 8 items regarding how often the parent practices math and counting with the child. The cultural activity index includes 4 items indicating if the parent has done arts, crafts, or sports with the child, or taken them to a museum, play, or community event. The child medical care index includes 4 items indicating if the child has received dental, vision, hearing, and general medical care. Standard Errors are clustered at the Head Start Center the household applied to and was assigned treatment. ***p<0.01, ** p<0.05, * p<0.1.

Figure 1: 1990s Expansions in Head Start Funding and Enrollment



Source: Total enrollment obtained from the Office of Head Start. City level funding obtained from the historic Consolidated Federal Funds Report and aggregated to the MSA-level. Authors' calculations.

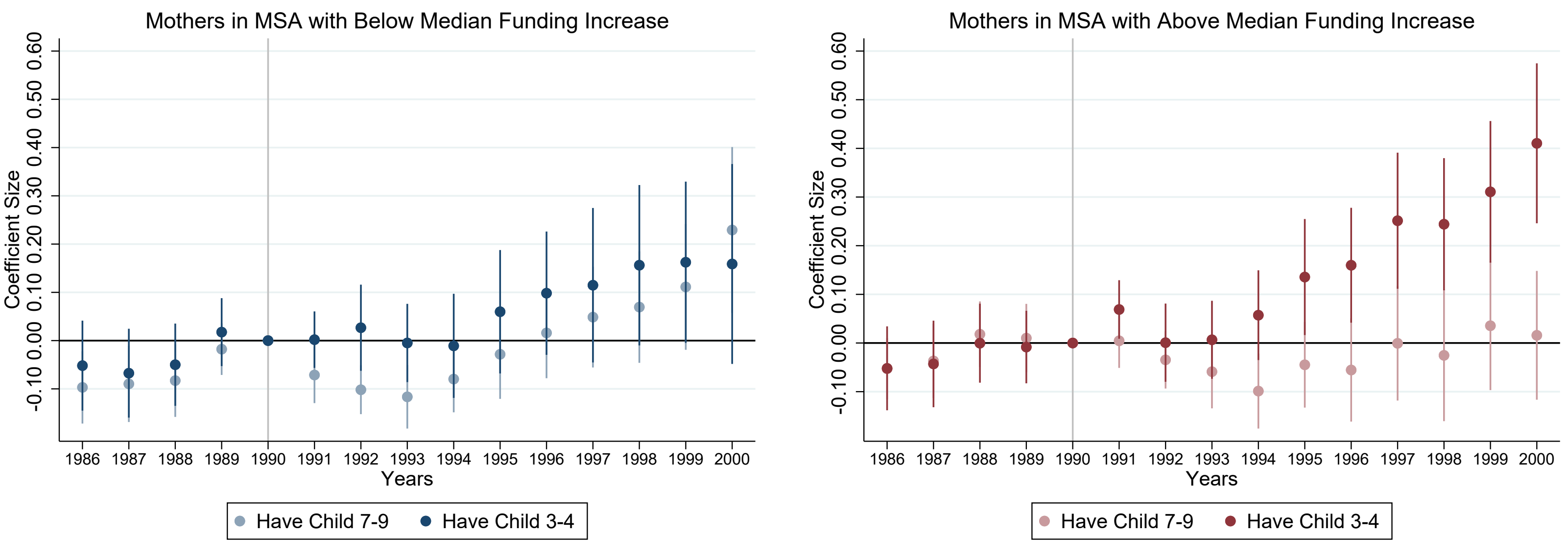
Figure 2: Additional Head Start Dollars in the 1990s were Dispersed Proportionally



Notes: In the left panel, MSA-level funding combined into bins of 50 dollar increments with the mean plotted. In the right panel, MSAs are assigned deciles of per child Head Start spending in 1989. Average funding levels within these bins are then plotted over time. Points where the lines cross would indicate a switch in the relative level of per child funding.

Source: Head Start dollars from the Consolidated Federal Funds Report and aggregated to the MSA-level. Authors' calculations.

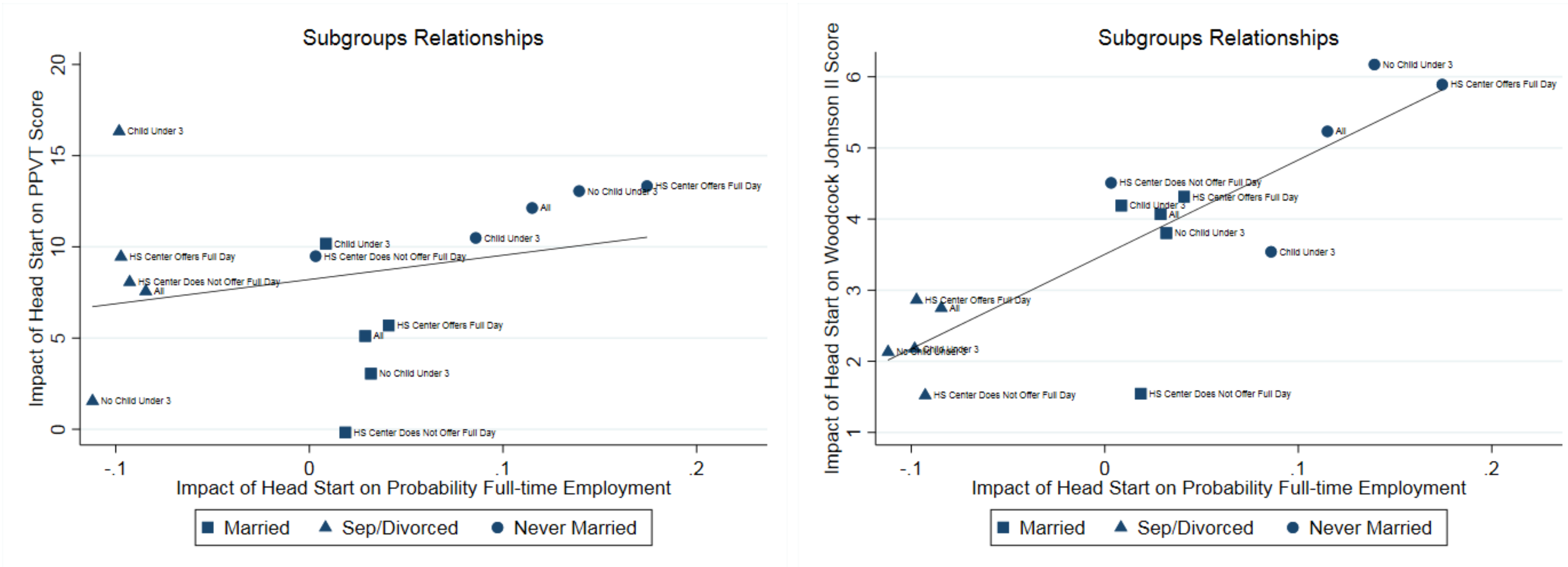
Figure 3: Trends in Employment of Mothers with Age-eligible or Elementary Aged Children in High and Low Head Start Spending-increase Areas



Notes: Coefficients from equation (2) are plotted separately for single mothers with an age-eligible child 3-4, or an ineligible child 7-9, in a MSA with below or above median increase in funding between 1989 and 1999. The outcome is employment during the previous calendar year. In 1986, the CPS began reporting over 150 more metropolitan areas. We restrict the sample to 1986 to maintain a balanced panel of metropolitan areas. Regressions are estimated separately for MSA where the change in per capita Head Start Funding was in the bottom half of the distribution and in the top half of the distribution. Ninety-five percent confidence intervals also provided. To interpret, 0.1 is a ten percentage point change.

Source: CPS ASEC 1986-2000. Authors' calculations.

Figure 4: HSIS Impacts of Head Start on Maternal Employment Correspond to Impacts on Children's Cognitive Scores



Notes: The impact of Head Start enrollment on cognitive scores in the randomized HSIS for various subgroups are plotted along the y-axis. The impact of Head Start enrollment on the probability of the mother working full time are plotted along the x-axis. For reference, the coefficients on cognitive scores are available in Appendix Table A.19 while the coefficients on maternal full-time employment are available in Tables 8. The correlation coefficient for PPVT scores is 0.27 while the correlation coefficient for WJII scores is 0.83. Among never married subgroups the correlation coefficients are 0.96 and 0.66 respectively.

Source: Head Start Impact Study Spring 2003 Child and Parent Surveys. Authors' calculations.

For Online Publication: Appendix A. Additional Tables and Figures

Table A.1: State-level Analysis: Impact of 1990s Head Start Enrollment on Labor Market Outcomes of Single Mothers

				Inverse Hyperbolic Sine of		
	Employed	Full-time	Part-time	Weeks Worked	Usual Hours Worked	Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
Head Start Enrollment per Child$_{t-1}$	0.379***	0.106	0.273***	1.362***	1.330**	3.264**
*Have Child 3-4 in t-1	(0.125)	(0.102)	(0.089)	(0.484)	(0.513)	(1.335)
Head Start Enrollment per Child$_{t-1}$	0.119	0.520	-0.401	1.678	1.452	5.119
	(0.501)	(0.433)	(0.286)	(2.226)	(2.104)	(5.570)
Have Child 3-4 in t-1	-0.103***	-0.093***	-0.010	-0.508***	-0.441***	-1.176***
	(0.014)	(0.012)	(0.009)	(0.060)	(0.060)	(0.159)
Dependent Mean	0.72	0.55	0.16	30.47	26.60	16102.65
Observations	24,220	24,220	24,220	24,220	24,220	24,220

Notes: Data from the CPS ASEC 1988-2000 repeated cross sections. Sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year. Head Start Funding per Child is measured at the state level in units of $500 (2017$). Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the federal and state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. Estimates are similar if the natural log plus one is used. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the state level, with 49 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table A.2: Clustering at State-level: Impact of 1990s Head Start Funding on Labor Market Outcomes of Single Mothers

				Inverse Hyperbolic Sine of		
	Employed	Full-time	Part-time	Weeks Worked	Usual Hours Worked	Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
HS Funding per Child$_{t-1}$	0.019***	0.017**	0.002	0.072***	0.076***	0.153***
*Have Child 3-4 in t-1	(0.004)	(0.007)	(0.006)	(0.019)	(0.018)	(0.047)
HS Funding per Child$_{t-1}$	-0.017	-0.037*	0.020*	-0.047	-0.080	-0.245
	(0.017)	(0.020)	(0.010)	(0.077)	(0.074)	(0.183)
Have Child 3-4 in t-1	-0.093***	-0.099***	0.006	-0.471***	-0.413***	-1.045***
	(0.008)	(0.009)	(0.007)	(0.031)	(0.032)	(0.075)
Dependent Mean	0.70	0.54	0.16	29.39	25.74	15254.73
Observations	33,791	33,791	33,791	33,791	33,791	33,791

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year. Head Start Funding per Child is measured at the MSA level in units of \$500 (2017\$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. Estimates are similar if the natural log plus one is used. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the state level, with 49 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table A.3: Impact of 1990s Head Start Expansion Funding on School Enrollment Among Children of Single Mothers

Mother's Characteristic	Outcome: In School						
	HS or Less (1)	Any College (2)	Non-Hispanic White (3)	Non-White and Hispanics (4)	Never Married (5)	Separated/ Divorced (6)	Only Married (7)
HS Funding per Child$_{t-1}$	0.059***	0.050***	0.062***	0.062***	0.062***	0.067***	0.023**
*Have Child 3-4 in t-1	(0.019)	(0.012)	(0.016)	(0.019)	(0.017)	(0.018)	(0.011)
HS Funding per Child$_{t-1}$	-0.021	-0.008	-0.041*	-0.001	-0.042	-0.012	0.000
	(0.022)	(0.022)	(0.021)	(0.024)	(0.029)	(0.017)	(0.008)
Have Child 3-4 in t-1	-0.789***	-0.643***	-0.713***	-0.774***	-0.758***	-0.728***	-0.677***
	(0.021)	(0.019)	(0.020)	(0.021)	(0.020)	(0.022)	(0.013)
Dependent Mean	0.72	0.82	0.79	0.73	0.69	0.83	0.78
Observations	13,243	7,026	9,586	10,682	10,232	10,037	85,403

Notes: Data from the CPS October education supplement 1989-2000 repeated cross sections. Sample restricted to 3-, 4-, 7-, 8-, and 9-year-olds with single mothers in the October Supplement to be consistent with the main triple difference specification. The dependent variable "In School" indicates if the child is currently enrolled in any school. Head Start Funding per Child is measured at the MSA level in units of \$500 (2017\$). Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, and whether the state has a child's health insurance program (SCHIP) in place. MSA and year fixed effects are included. These regressions are weighted using the individual monthly CPS weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table A.4: Impact of Head Start Expansions on Head Start Enrollment, State-level Analysis

	State-level Head Start Enrollment Rate				
	Ages 3-4			Ages 0-2	
	All	States without Pre-K Program	Control for State Pre-K Program	All	Years Before Early Head Start
	(1)	(2)	(3)	(4)	(5)
Head Start Funding per Child (3-4 yr.)$_{t-1}$	0.049***	0.026***	0.050***	0.002***	0.002
	(0.008)	(0.008)	(0.009)	(0.001)	(0.001)
Dependent Mean	0.081	0.088	0.081	0.002	0.001
Observations	539	143	539	539	294

Notes: Data from Kids Count Data Center. The level of observation is the state by year level Head Start enrollment from 1988-1999. Since within MSA or within state comparisons are not possible, estimates are obtained from the following regression $\textit{HS rate}_{st} = \beta_1 \textit{HS funding per child}_{st-1} + \phi_s + \delta_t + \varepsilon_{st}$. Column (2) limits the sample to states that did not have a state-funded pre-K program before 2000. Column (3) controls for whether or not there is a state-funded pre-K program in the state that year. Head Start Funding per Child is measured at the State level in units of $500 (2017$) and regressions are weighted by the state population of the given age group. Columns (4) and (5) examine Head Start enrollment of children under 3, to explore the impacts of Early Head Start on enrollment. The sample is restricted to pre-1995 observations in column (5) to exclude the period after Early Head Start began. Standard errors are corrected for clustering at the state level, with 49 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table A.5: Robustness of Employment Effects when Accounting for State Public Preschools

	Employed	Full-time	Part-time	Inverse Hyperbolic Sine of Weeks Worked	Inverse Hyperbolic Sine of Usual Hours Worked	Inverse Hyperbolic Sine of Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
Sample: States with No State-funded Public Pre-K Program by 2000						
HS Funding per Child$_{t-1}$	0.012**	0.009	0.003	0.041*	0.047**	0.113*
*Have Child 3-4 in t-1	(0.004)	(0.007)	(0.006)	(0.022)	(0.021)	(0.066)
HS Funding per Child$_{t-1}$	-0.029	-0.037	0.008	-0.147	-0.148	-0.315
	(0.026)	(0.032)	(0.020)	(0.145)	(0.118)	(0.321)
Have Child 3-4 in t-1	-0.062***	-0.070***	0.007	-0.338***	-0.295***	-0.793***
	(0.015)	(0.020)	(0.015)	(0.075)	(0.069)	(0.179)
Dependent Mean	0.76	0.60	0.16	32.43	28.60	16259.04
Observations	7,503	7,503	7,503	7,503	7,503	7,503
Control for Existence of State-funded Public Pre-K Program						
HS Funding per Child$_{t-1}$	0.019***	0.017**	0.002	0.074***	0.078***	0.157***
*Have Child 3-4 in t-1	(0.006)	(0.008)	(0.005)	(0.026)	(0.025)	(0.060)
HS Funding per Child$_{t-1}$	-0.018	-0.033*	0.015	-0.050	-0.084	-0.253
	(0.015)	(0.018)	(0.011)	(0.067)	(0.063)	(0.167)
Have Child 3-4 in t-1	-0.093***	-0.099***	0.006	-0.472***	-0.414***	-1.047***
	(0.009)	(0.010)	(0.007)	(0.039)	(0.038)	(0.092)
Dependent Mean	0.70	0.54	0.16	29.39	25.74	15254.73
Observations	33,791	33,791	33,791	33,791	33,791	33,791
Include State by Year Effects						
HS Funding per Child$_{t-1}$	0.018***	0.017**	0.001	0.067***	0.074***	0.142**
*Have Child 3-4 in t-1	(0.005)	(0.008)	(0.005)	(0.024)	(0.024)	(0.058)
HS Funding per Child$_{t-1}$	-0.028*	-0.047**	0.019	-0.101	-0.133*	-0.334*
	(0.017)	(0.020)	(0.014)	(0.073)	(0.073)	(0.190)
Have Child 3-4 in t-1	-0.091***	-0.098***	0.007	-0.460***	-0.405***	-1.021***
	(0.009)	(0.010)	(0.007)	(0.038)	(0.037)	(0.089)
Dependent Mean	0.70	0.54	0.16	29.39	25.74	15254.73
Observations	33,791	33,791	33,791	33,791	33,791	33,791
Sample: Exclude Mothers of 4-year-olds						
HS Funding per Child$_{t-1}$	0.023***	0.023***	-0.000	0.097***	0.095***	0.200***
*Have Child 3 in t-1	(0.007)	(0.009)	(0.006)	(0.033)	(0.030)	(0.064)
HS Funding per Child$_{t-1}$	-0.016	-0.039*	0.023	-0.048	-0.079	-0.207
	(0.016)	(0.020)	(0.014)	(0.074)	(0.069)	(0.182)
Have Child 3 in t-1	-0.110***	-0.121***	0.011	-0.566***	-0.490***	-1.248***
	(0.011)	(0.012)	(0.008)	(0.046)	(0.046)	(0.106)
Dependent Mean	0.70	0.54	0.16	29.57	25.84	15470.74
Observations	28,059	28,059	28,059	28,059	28,059	28,059

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year. Head Start Funding per Child is measured at the MSA level in units of \$500 (2017\$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. The top panel excludes states that had implemented public preschool prior to 2000. The second panel controls for whether or not the state provides public preschool in the given year. The third panel includes state by year fixed effects, to control for state level preschool funding and preschool enrollment. The bottom panel excludes children who would have been 4 in the previous year, such that 3-year-olds are the only treated children. Most state programs were aimed towards 4-year-olds (Cascio and Schanzenbach, 2013). Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. $p<0.01$ ***, $p<0.05$**, $p<0.1$*.

Table A.6: Relationship Between Head Start Funding and Other Concurrent Social Programs

	Employed No Policy Controls (1)	TANF Waiver (2)	Employed (3)	Max Welfare Benefit (4)	Employed (5)	HS Family Service Grant (6)	Employed (7)	Max EITC (8)	Employed (9)	Employed (10)	Employed (11)
HS Funding per Child$_{t-1}$	0.019***	-0.001	0.019***	-0.800	0.019***	-0.001	0.019***	-0.011	0.019***	0.020***	0.020***
*Have Child 3-4 in t-1	(0.006)	(0.003)	(0.006)	(0.705)	(0.006)	(0.002)	(0.006)	(0.014)	(0.006)	(0.006)	(0.006)
TANF Waiver			0.014								0.006
			(0.013)								(0.014)
Max Welfare Benefit					-0.000***						-0.000***
					(0.000)						(0.000)
HS Family Service Grant							0.002				0.002
							(0.014)				(0.013)
Max EITC									-0.032***	0.053***	0.053***
									(0.006)	(0.007)	(0.007)
# EITC Eligible Children F.E.										X	X
Dependent Mean	0.70	0.28	0.70	616.55	0.70	0.06	0.70	2.72	0.70	0.70	0.70
Observations	33,791	33,791	33,791	33,791	33,791	33,791	33,791	33,791	33,791	33,791	33,791

Notes: Data from the CPS ASEC 1988-2000 repeated cross sections. Sample restricted to single women with a child 3-4 last year or 7-9 last year. Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race and education, and state level demographic controls. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table A.7: Alternative Counterfactual: Impact of 1990s Head Start Funding on Employment of Single Mothers with Age-Eligible Children Relative to Mothers with Children Under 3

	Employed	Full-time	Part-time	Inverse Hyperbolic Sine of Weeks Worked	Inverse Hyperbolic Sine of Usual Hours Worked	Inverse Hyperbolic Sine of Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
HS Funding per Child$_{t-1}$	0.019***	0.015*	0.004	0.086***	0.080***	0.189***
*Have Child 3-4 in t-1	(0.006)	(0.008)	(0.006)	(0.028)	(0.027)	(0.071)
HS Funding per Child$_{t-1}$	-0.006	0.002	-0.007	-0.037	-0.022	-0.179
	(0.018)	(0.019)	(0.010)	(0.081)	(0.078)	(0.190)
Have Child 3-4 in t-1	0.009	0.040***	-0.031***	0.103**	0.060	0.188*
	(0.009)	(0.012)	(0.008)	(0.040)	(0.040)	(0.097)
Dependent Mean	0.63	0.45	0.18	24.26	22.49	13760.74
Observations	37,286	37,286	37,286	37,286	37,286	37,286

Notes: Data from the CPS ASEC 1988-2000 repeated cross sections. Sample restricted to single women with a child under 5 last year. Head Start Funding per Child is measured at the MSA level in units of \$500 (2017\$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. Estimates are similar if the natural log plus one is used. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 289 clusters. $p<0.01$ ***, $p<0.05$**, $p<0.1$*.

Table A.8: Impact of 1990s Head Start Expansion Funding on labor Market Outcomes of Single Mothers with 5- or 6-Year-Olds

				Inverse Hyperbolic Sine of		
	Employed	Full-time	Part-time	Weeks Worked	Usual Hours Worked	Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
	Sample: Have a 5-Year-Old Last Year vs. Have a 7-9 Year-Old Last Year					
HS Funding per Child$_{t-1}$	0.031***	0.028***	0.003	0.136***	0.138***	0.258***
*Have Child 5 in t-1	(0.008)	(0.009)	(0.007)	(0.037)	(0.035)	(0.083)
HS Funding per Child$_{t-1}$	-0.005	-0.030	0.026	-0.003	-0.029	-0.047
	(0.016)	(0.020)	(0.016)	(0.072)	(0.067)	(0.187)
Have Child 5 in t-1	-0.086***	-0.090***	0.003	-0.428***	-0.385***	-0.895***
	(0.011)	(0.012)	(0.010)	(0.047)	(0.045)	(0.106)
Dependent Mean	0.73	0.58	0.15	31.88	27.37	17340.48
Observations	22,326	22,326	22,326	22,326	22,326	22,326
	Placebo Sample: Have a 6-Year-Old Last Year vs. Have a 7-9 Year-Old Last Year					
HS Funding per Child$_{t-1}$	0.013	0.011	0.002	0.058	0.053	0.135
*Have Child 6 in t-1	(0.012)	(0.015)	(0.007)	(0.055)	(0.055)	(0.114)
HS Funding per Child$_{t-1}$	-0.016	-0.044**	0.028*	-0.043	-0.077	-0.174
	(0.015)	(0.020)	(0.016)	(0.069)	(0.063)	(0.178)
Have Child 6 in t-1	-0.037***	-0.052***	0.016*	-0.197***	-0.166***	-0.438***
	(0.013)	(0.015)	(0.009)	(0.057)	(0.058)	(0.125)
Dependent Mean	0.74	0.58	0.16	32.29	27.67	17559.49
Observations	22,087	22,087	22,087	22,087	22,087	22,087

Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to single mothers with either a 5- (top panel) or 6-year-old (bottom panel) last year or a 7-, 8-, or 9-year-old last year *and no child that was 3- or 4- years old last year.* Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the federal and state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. Estimates are similar if the natural log plus one is used. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table A.9: Impact of 1990s Head Start Funding on Labor Market Outcomes Using Different Counterfactual Groups

				Inverse Hyperbolic Sine of		
	Employed	Full-time	Part-time	Weeks Worked	Usual Hours Worked	Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
	Sample: Have Child Under 3 in the Home					
HS Funding per Child$_{t-1}$	0.038***	0.019	0.019**	0.148***	0.151***	0.408***
*Have Child 3-4 in t-1	(0.009)	(0.012)	(0.008)	(0.039)	(0.039)	(0.085)
HS Funding per Child$_{t-1}$	0.001	0.011	-0.010	-0.020	0.009	-0.100
	(0.020)	(0.020)	(0.012)	(0.086)	(0.086)	(0.207)
Have Child 3-4 in t-1	-0.133***	-0.086***	-0.047***	-0.573***	-0.559***	-1.391***
	(0.013)	(0.015)	(0.010)	(0.054)	(0.055)	(0.127)
Dependent Mean	0.59	0.41	0.18	21.76	20.80	13131.98
Observations	26,611	26,611	26,611	26,611	26,611	26,611
	Sample: Have 6 or 7-year-old in the Home					
HS Funding per Child$_{t-1}$	0.046***	0.049***	-0.003	0.193***	0.207***	0.454***
*Have Child 3-4 in t-1	(0.012)	(0.015)	(0.011)	(0.055)	(0.050)	(0.108)
HS Funding per Child$_{t-1}$	-0.018	-0.034	0.016	-0.078	-0.097	-0.243
	(0.020)	(0.025)	(0.019)	(0.092)	(0.085)	(0.221)
Have Child 3-4 in t-1	-0.166***	-0.164***	-0.001	-0.796***	-0.743***	-1.856***
	(0.016)	(0.019)	(0.013)	(0.070)	(0.070)	(0.157)
Dependent Mean	0.69	0.52	0.16	28.95	25.34	15219.49
Observations	15,674	15,674	15,674	15,674	15,674	15,674

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to women with a child of the specified age. Head Start Funding per Child is measured at the MSA level in units of \$500 (2017\$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 289 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table A.10: Placebo Impact of 1990s Head Start Funding on Labor Market Outcomes of Single Mothers with Children Under 3

				Inverse Hyperbolic Sine of		
	Employed	Full-time	Part-time	Weeks Worked	Usual Hours Worked	Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
Head Start Funding per Child$_{t-1}$	0.008	0.012	-0.004	0.031	0.035	0.065
*Have Child Under 2 in t-1	(0.008)	(0.009)	(0.007)	(0.036)	(0.034)	(0.074)
Head Start Funding per Child$_{t-1}$	-0.005	-0.021	0.016	-0.013	-0.024	-0.057
	(0.016)	(0.018)	(0.014)	(0.070)	(0.069)	(0.170)
Have Child Under 2 in t-1	-0.146***	-0.187***	0.041***	-0.809***	-0.670***	-1.745***
	(0.010)	(0.011)	(0.008)	(0.044)	(0.044)	(0.102)
Dependent Mean	0.68	0.50	0.18	27.40	24.55	16745.07
Observations	32,471	32,471	32,471	32,471	32,471	32,471

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to women with a child younger than 3 in the previous year, or 7,8, or 9 in the previous year. This is similar to the baseline specification, but compares outcomes of mothers with a 0-2 year-old to outcomes of counterfactual mothers with a 7-9 year old. Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table A.11: Robustness of the Impact of 1990s Head Start Funding on Labor Market Outcomes of Single Mothers to Various Specifications

	Outcome: Any Employment in t-1		
	Only 1 ASEC Observation per Person (1)	Pre-Early HS ($\leq$1995) (2)	Age-Specific Linear Trends (3)
Head Start Funding per Child$_{t-1}$	0.017***	0.027**	0.019***
*Have Child 3-4 in t-1	(0.006)	(0.011)	(0.006)
Head Start Funding per Child$_{t-1}$	-0.009	-0.043	-0.018
	(0.019)	(0.029)	(0.015)
Have Child 3-4 in t-1	-0.088***	-0.103***	0.001
	(0.010)	(0.011)	(0.023)
Dependent Mean	0.70	0.66	0.70
Observations	22,394	21,858	33,791

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year. Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Because participants are sampled for several rounds, column (1) limits the sample to only one observation per woman. Column (2) ends the sample in 1994, to avoid the introduction of early Head Start for younger children which could contaminate the control. Column (3) includes linear trends for each child specific age (3, 4, 7, 8, and 9). As Wolfers (2006) suggests, including linear trends might over control and capture some of the treatment effect. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table A.12: Using Education to Tag Likely-Eligible Women: Impact of 1990s Head Start Funding on Labor Market Outcomes of Less-Educated Mothers

				Inverse Hyperbolic Sine of		
	Employed	Full-time	Part-time	Weeks Worked	Usual Hours Worked	Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
HS Funding per Child$_{t-1}$	0.010*	0.007	0.003	0.044**	0.040*	0.098*
*Have Child 3-4 in t-1	(0.005)	(0.006)	(0.004)	(0.022)	(0.023)	(0.051)
HS Funding per Child$_{t-1}$	-0.015	-0.003	-0.012	-0.043	-0.053	-0.148
	(0.012)	(0.014)	(0.008)	(0.055)	(0.054)	(0.144)
Have Child 3-4 in t-1	-0.092***	-0.073***	-0.019***	-0.470***	-0.398***	-1.022***
	(0.007)	(0.007)	(0.006)	(0.031)	(0.030)	(0.071)
Dependent Mean	0.63	0.42	0.21	25.84	21.76	10627.02
Observations	83,732	83,732	83,732	83,732	83,732	83,732

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to women with a high school degree or less and with a child 3-4 or 7-9 last year. Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. Estimates are similar if the natural log plus one is used. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. $p<0.01$ ***, $p<0.05$**, $p<0.1$*.

Table A.13: Impact of 1990s Head Start Funding on Labor Market Outcomes of Single Mothers, Excluding Movers

				Inverse Hyperbolic Sine of		
				Weeks	Usual Hours	Wage
	Employed	Full-time	Part-time	Worked	Worked	Income
	(1)	(2)	(3)	(4)	(5)	(6)
HS Funding per Child$_{t-1}$	0.023***	0.018	0.004	0.088**	0.093***	0.187**
*Have Child 3-4 in t-1	(0.008)	(0.011)	(0.007)	(0.038)	(0.036)	(0.083)
HS Funding per Child$_{t-1}$	-0.019	-0.053**	0.034**	-0.078	-0.101	-0.276
	(0.019)	(0.024)	(0.015)	(0.089)	(0.081)	(0.208)
Have Child 3-4 in t-1	-0.094***	-0.099***	0.005	-0.464***	-0.419***	-1.049***
	(0.012)	(0.014)	(0.009)	(0.051)	(0.049)	(0.115)
Dependent Mean	0.70	0.54	0.16	30.24	25.76	16092.00
Observations	22,851	22,851	22,851	22,851	22,851	22,851

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to single mothers who did not move in the previous year with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year. This specification verifies the labor market effects are not driven by compositional changes dues to selective migration. Head Start Funding per Child is measured at the MSA level in units of \$500 (2017\$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. Estimates are similar if the natural log plus one is used. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. p<0.01 ***, p<0.05**, p<0.1*.

Table A.14: Impact of 1990s Head Start Funding on Labor Market Outcomes of Single Mothers in Metropolitan Areas and by Central City Status

				Inverse Hyperbolic Sine of		
	Employed	Full-time	Part-time	Weeks Worked	Usual Hours Worked	Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
				Metropolitan Areas		
Head Start Funding per Child$_{t-1}$	0.026***	0.029***	-0.002	0.110**	0.118***	0.262***
*Have Child 3-4 in t-1	(0.007)	(0.010)	(0.006)	(0.043)	(0.033)	(0.072)
Head Start Funding per Child$_{t-1}$	-0.005	-0.026	0.021	-0.005	-0.033	-0.223
	(0.024)	(0.028)	(0.016)	(0.110)	(0.104)	(0.272)
Have Child 3-4 in t-1	-0.097***	-0.106***	0.008	-0.488***	-0.441***	-1.130***
	(0.011)	(0.013)	(0.008)	(0.055)	(0.048)	(0.112)
Dependent Mean	0.68	0.53	0.15	28.89	25.13	16085.21
Observations	20,395	20,395	20,395	20,395	20,395	20,395
				High Impact Sample: Central City		
Head Start Funding per Child$_{t-1}$	0.038***	0.026**	0.012*	0.130**	0.164***	0.299***
*Have Child 3-4 in t-1	(0.009)	(0.011)	(0.007)	(0.053)	(0.043)	(0.091)
Head Start Funding per Child$_{t-1}$	-0.022	-0.051	0.029	-0.098	-0.131	-0.367
	(0.034)	(0.037)	(0.020)	(0.158)	(0.147)	(0.366)
Have Child 3-4 in t-1	-0.116***	-0.117***	0.001	-0.542***	-0.517***	-1.221***
	(0.012)	(0.016)	(0.011)	(0.061)	(0.055)	(0.125)
Dependent Mean	0.61	0.47	0.14	25.49	22.55	13324.61
Observations	11,633	11,633	11,633	11,633	11,633	11,633
				Lower Impact Sample: Outside Central City		
Head Start Funding per Child$_{t-1}$	0.014	0.032**	-0.017**	0.090	0.068	0.221*
*Have Child 3-4 in t-1	(0.012)	(0.014)	(0.008)	(0.056)	(0.052)	(0.120)
Head Start Funding per Child$_{t-1}$	-0.011	-0.017	0.006	0.010	-0.031	-0.290
	(0.031)	(0.037)	(0.024)	(0.131)	(0.137)	(0.332)
Have Child 3-4 in t-1	-0.073***	-0.091***	0.017*	-0.409***	-0.342***	-0.993***
	(0.016)	(0.018)	(0.010)	(0.074)	(0.067)	(0.162)
Dependent Mean	0.76	0.59	0.16	32.97	28.22	19390.43
Observations	8,758	8,758	8,758	8,758	8,758	8,758

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. In the top panel, sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year in a reported MSA. In the bottom two panels, sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year in one of the 140 MSA where central city status is available. Head Start Funding per Child is measured at the MSA level in units of \$500 (2017\$). Since population measures for the MSA city center and outside the city center are not available, the funding per child is the same for individuals in the same MSA regardless if they are inside or outside the central city. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level with 140 clusters in the top panel, 134 in the middle panel, and 136 in the bottom panel. $p<0.01$ ***, $p<0.05$**, $p<0.1$*.

Table A.15: Impact of 1990s Head Start Funding on Labor Market Outcomes of Mothers by Marital Status

				Inverse Hyperbolic Sine of		
	Employed (1)	Full-time (2)	Part-time (3)	Weeks Worked (4)	Usual Hours Worked (5)	Wage Income (6)
			Never Married			
Head Start Funding per Child$_{t-1}$	0.024***	0.024**	0.001	0.094***	0.103***	0.233***
*Have Child 3-4 in t-1	(0.008)	(0.010)	(0.007)	(0.031)	(0.035)	(0.084)
Dependent Mean	0.61	0.46	0.15	24.38	22.08	10983.37
Observations	11,729	11,729	11,729	11,729	11,729	11,729
			Separated, Divorced, or Widowed			
Head Start Funding per Child$_{t-1}$	0.008	0.008	-0.001	0.022	0.030	0.034
*Have Child 3-4 in t-1	(0.008)	(0.010)	(0.007)	(0.034)	(0.033)	(0.088)
Dependent Mean	0.74	0.58	0.16	32.27	27.83	17707.61
Observations	22,049	22,049	22,049	22,049	22,049	22,049
			Married			
Head Start Funding per Child$_{t-1}$	0.004	0.004	-0.000	0.023	0.019	0.037
*Have Child 3-4 in t-1	(0.004)	(0.006)	(0.005)	(0.020)	(0.019)	(0.044)
Dependent Mean	0.69	0.43	0.26	29.15	22.88	15574.72
Observations	111,147	111,147	111,147	111,147	111,147	111,147

Notes: Data from the CPS ASEC 1984-2000 repeated cross sections. Sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year. Head Start Funding per Child is measured at the MSA level in units of \$500 (2017\$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (4)-(6) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level. p<0.01 ***, p<0.05**, p<0.1*.

Table A.16: Constructed Annual Average Hourly Wage across Subgroups, Pre-Head Start Expansions

	HS or Less (1)	Any College (2)	Non-Hispanic White (3)	Non-White and Hispanics (4)	Never Married (5)	Separated/ Divorced (6)	Married (7)
Ave. Annual Hourly Wage	10.8	15.6	12.7	11.9	11.5	12.6	13.2
(2017 Dollars)	(0.2)	(0.3)	(0.3)	(0.3)	(0.5)	(0.3)	(0.3)
P-value on Difference	0.000		0.014		0.030		

Notes: Data from the CPS ASEC 1984-1989 repeated cross sections, prior to Head Start expansions. Sample restricted to women in the Table 5 pre-1990 analysis sample. Average annual hourly wages constructed by dividing the annual income (in 2017$) by the product of the number of weeks worked and usual hours worked. Average annual hourly wages estimated for education groups, race/ethnicity, and marital status jointly to calculate statistical significance. Estimates are weighted using the individual CPS ASEC weights. Estimates for never married mothers and separated/divorced mothers are both statistically different than the estimate for married mothers. Standard errors corrected for clustering at the MSA level reported in parentheses.

Table A.17: HSIS Impact of Head Start on Maternal Labor Supply (Intent to Treat)

	All (1)	HS Center Offers Full Day (2)	HS Center Does Not Offer Full Day (3)	Child Under 3 (4)	No Child Under 3 (5)
			In Labor Force		
Head Start	0.026	0.041	-0.006	-0.013	0.046*
	(0.020)	(0.025)	(0.032)	(0.032)	(0.026)
Control Mean	0.58	0.59	0.56	0.53	0.61
Number of Centers	334	198	113	286	321
Observations	3,117	1,829	1,128	1,181	1,936
			Employed		
Head Start	0.014	0.030	-0.021	-0.029	0.036
	(0.020)	(0.024)	(0.035)	(0.031)	(0.025)
Control Mean	0.49	0.49	0.48	0.44	0.52
Number of Centers	334	198	113	286	321
Observations	3,117	1,829	1,128	1,181	1,936
			Employed Full-time		
Head Start	0.030	0.051**	-0.007	0.015	0.036
	(0.018)	(0.023)	(0.031)	(0.026)	(0.024)
Control Mean	0.31	0.32	0.30	0.26	0.34
Number of Centers	334	198	113	286	321
Observations	3,117	1,829	1,128	1,181	1,936
			Employed Part-time		
Head Start	-0.017	-0.021	-0.014	-0.044*	-0.000
	(0.015)	(0.019)	(0.026)	(0.023)	(0.019)
Control Mean	0.18	0.17	0.19	0.18	0.17
Number of Centers	334	198	113	286	321
Observations	3,117	1,829	1,128	1,181	1,936

Notes: Sample restricted to households that participated in the Spring 2003 Parent Interview with a mother in the household. In the Labor Force is measured as employed full-time, part-time, looking for work, laid off from work, or in the military. Employed is either full- or part-time employed. Full-time employed is employed for 35 hours or more a week. Head Start enrollment is instrumented for using original Head Start treatment assignment as the instrument. Month of interview fixed effects are included. All regressions are weighted using inverse probability weights constructed from the Spring 2003 wave. Standard Errors are clustered at the Head Start Center the household applied to and was assigned treatment. $p<0.01$ ***, $p<0.05$**, $p<0.1$*.

Table A.18: HSIS Child Care Characteristics and Covariate Balance by Treatment Status and Stratification Subgroup, Fall 2002

	Difference between Treatment and Control						
	Center Programming		Child Under 3		Marital Status		
	Full Day	Not Full Day	Yes	No	Married	Previously Married	Never Married
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Child in Head Start	0.71***	0.77***	0.74***	0.74***	0.75***	0.73***	0.72***
Child in Center-based Care	0.49***	0.65***	0.57***	0.54***	0.58***	0.57***	0.52***
Child in Home Daycare	-0.08***	-0.09***	-0.05***	-0.10***	-0.07***	-0.09***	-0.09***
Child at Home	-0.41***	-0.56***	-0.52***	-0.44***	-0.51***	-0.48***	-0.43***
In Care of Teacher/Head Start	0.49***	0.65***	0.57***	0.54***	0.58***	0.57***	0.53***
In Care of Parent/Guardian	-0.37***	-0.52***	-0.47***	-0.39***	-0.47***	-0.43***	-0.37***
In Care of Other	-0.13***	-0.13***	-0.09***	-0.15***	-0.10***	-0.14***	-0.16***
Child Female	0.01	0.01	0.04	-0.01	0.02	0.06	-0.02
White NH	-0.05**	0.01	-0.01	-0.02	-0.04	-0.02	0.00
Black NH	0.02	-0.02	-0.02	0.01	-0.00	0.00	0.01
Other NH	0.00	-0.01	-0.01	0.01	0.01	-0.02	0.00
Hispanic	0.02	0.03	0.04	0.00	0.03	0.04	-0.01
Race Missing	0.00	-0.01	0.00	0.00	0.01	0.01	-0.00
Mom 20-24	0.00	-0.01	0.02	-0.01	-0.00	-0.03	0.00
Mom 25-29	-0.02	0.01	-0.04	0.01	-0.03	-0.01	0.00
Mom 30-39	0.01	-0.00	0.03	-0.01	0.04	-0.01	-0.02
Mom 40+	0.01	0.02	0.00	0.01	-0.01	0.04**	0.01
< High School	-0.02	0.01	-0.01	-0.00	0.00	0.03	-0.03
High School	0.03	-0.03	-0.00	0.02	-0.00	0.02	0.02
Some College	-0.01	0.02	0.04	-0.03	0.00	-0.05	0.01
College	0.00	0.00	-0.02*	0.02*	-0.00	0.01	0.00
Educ. Missing	0.00	-0.01	-0.01	-0.00	-0.00	0.00	0.00
Married	0.00	0.00	-0.02	0.01	0.00	0.00	0.00
Sep./Divorced/Widow	0.01	-0.01	-0.02	0.02	0.00	0.00	0.00
Never Married	0.00	0.01	0.04	-0.03	0.00	0.00	0.00
Child Under 3	-0.03	-0.03	0.00	0.00	-0.05*	-0.10**	0.01
Didn't Respond in Fall 2002	0.02	-0.02	0.00	0.00	0.00	0.00	0.00
P-value on Joint F-test	0.58	0.74	0.08	0.80	0.78	0.00	0.99
Observations	2,696	1,509	1,372	2,241	1,586	574	1,388

Notes: All demographic measures constructed from the Fall 2002 Parent Interview. Estimates are weighted using inverse probability weights. The difference between treated and control units within each subgroup is reported. Means for subgroups are not reported separately due to disclosure requirements. p<0.01 ***, p<0.05**, p<0.1*.

Table A.19: HSIS Impact of Head Start on Children's Cognitive Scores

	All (1)	HS Center Offers Full Day (2)	HS Center Does Not Offer Full Day (3)	Child Under 3 (4)	No Child Under 3 (5)
			PPVT Item Response Theory Score		
Head Start*Married	5.113	5.688	-0.176	10.168**	3.051
	(3.137)	(4.169)	(4.650)	(4.912)	(4.391)
Head Start*Sep./Divorced/Widowed	7.562	9.458	8.070	16.335	1.547
	(5.653)	(8.889)	(7.288)	(10.049)	(7.031)
Head Start*Never Married	12.131***	13.334***	9.490	10.491*	13.061**
	(3.847)	(4.818)	(6.381)	(5.900)	(5.149)
Control Mean	268.57	263.49	279.16	268.08	268.91
Number of Centers	334	198	113	287	321
Observations	3,078	1,803	1,117	1,170	1,908
			WJII Pre-Academic Skills Standard Score		
Head Start*Married	4.068***	4.313***	1.543	4.189**	3.803***
	(1.175)	(1.554)	(1.630)	(2.051)	(1.364)
Head Start*Sep./Divorced/Widowed	2.748	2.864	1.522	2.177	2.132
	(1.935)	(2.696)	(2.792)	(3.496)	(2.034)
Head Start*Never Married	5.232***	5.891***	4.509**	3.540	6.173***
	(1.264)	(1.754)	(1.929)	(2.180)	(1.585)
Control Mean	88.74	88.85	89.24	87.84	89.34
Number of Centers	333	198	113	285	320
Observations	3,046	1,784	1,106	1,154	1,892

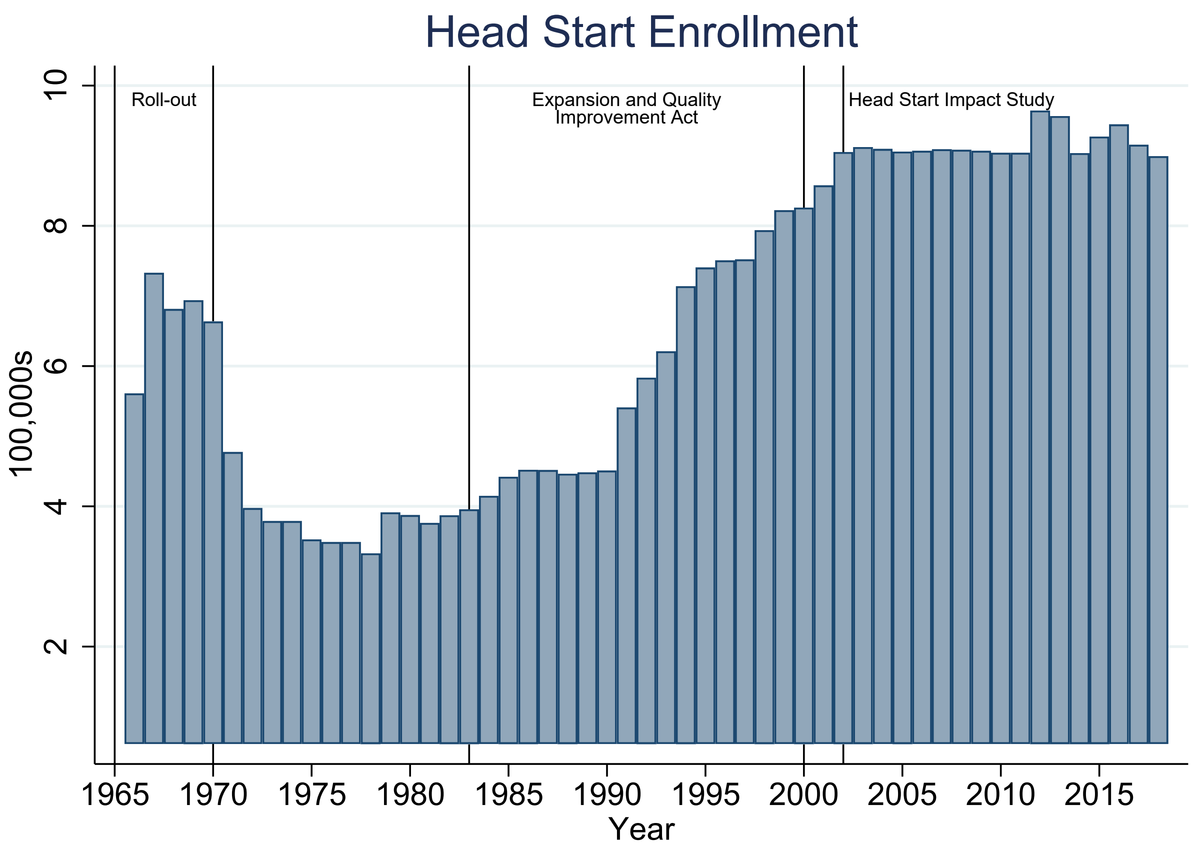
Notes: Sample restricted to households that participated in the Spring 2003 Parent Interview with a mother in the household. PPVT is the Peabody Picture Vocabulary Test Item Resoponse Theory Score. WJIII is the Woodcock Johnson II pre-academic skills standardized score. Head Start enrollment is instrumented for using original Head Start treatment assignment as the instrument. There is no constant included, thus allowing the inclusion of "Married", "Sep./Divorced/Widowed", and "Never Married". Full day offering is determined from the Center Director's interview. The presence of younger children was determined by examining the household roster to determine if any children under three were present. Month of interview fixed effects are included. All regressions are weighted using inverse probability weights constructed from the Spring 2003 wave. Standard Errors are clustered at the Head Start Center the household applied to and was assigned treatment. p<0.01 ***, p<0.05**, p<0.1*.

Table A.20: Impact of 1990s Head Start Funding on Transfer Program Participation of Single Mothers

			Inverse Hyperbolic Sine of						
	School Lunch	Medicaid	Welfare Income	SSI Income	Food Stamps Value	Federal Tax Liability	FICA Liability	State Tax Liability	EITC Refund
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Head Start Funding per Child$_{t-1}$	-0.004	-0.014*	-0.214**	0.014	-0.084	0.259***	0.034	0.053	-0.208***
*Have Child 3-4 in t-1	(0.008)	(0.007)	(0.099)	(0.047)	(0.051)	(0.058)	(0.066)	(0.065)	(0.071)
Head Start Funding per Child$_{t-1}$	0.038**	-0.060***	0.283*	0.135**	0.329**	-0.222	-0.059	-0.281	0.222
	(0.017)	(0.016)	(0.149)	(0.060)	(0.165)	(0.177)	(0.182)	(0.172)	(0.192)
Have Child 3-4 in t-1	-0.149***	-0.010	1.136***	-0.076	0.848***	-1.132***	-0.595***	-0.641***	0.086
	(0.011)	(0.007)	(0.106)	(0.049)	(0.070)	(0.103)	(0.109)	(0.109)	(0.120)
Dependent Mean	0.51	0.22	2152.31	274.58	1508.09	985.25	1204.45	357.97	1179.52
Observations	33,791	33,791	33,791	33,791	33,791	18,286	18,286	18,286	18,286

Notes: Data from the CPS ASEC 1988-2000 repeated cross sections. Sample restricted to single mothers with either a 3- or 4-year-old last year or a 7-, 8-, or 9-year-old last year. Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Mothers in non-metro areas are assigned the Head Start funding level in the non-msa remainder of the state. Controls include indicators for mother's race and education, state level demographic controls, and policy controls, including an indicator for TANF, the maximum TANF benefit for a family of three, the state minimum wage, whether the state has a child's health insurance program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (3)-(9) are the inverse hyperbolic sine of the value, to include zeroes, but the dependent mean in levels is reported. Tax measures are constructed using Census Bureau tax models, not reported by the respondent. Tax measures are only available starting in 1992, leading to a smaller sample. Estimates are similar if the natural log plus one is used. All regressions are weighted using the individual CPS ASEC weights. Standard errors are corrected for clustering at the MSA level, with 290 clusters. p<0.01 ***, p<0.05**, p<0.1*.

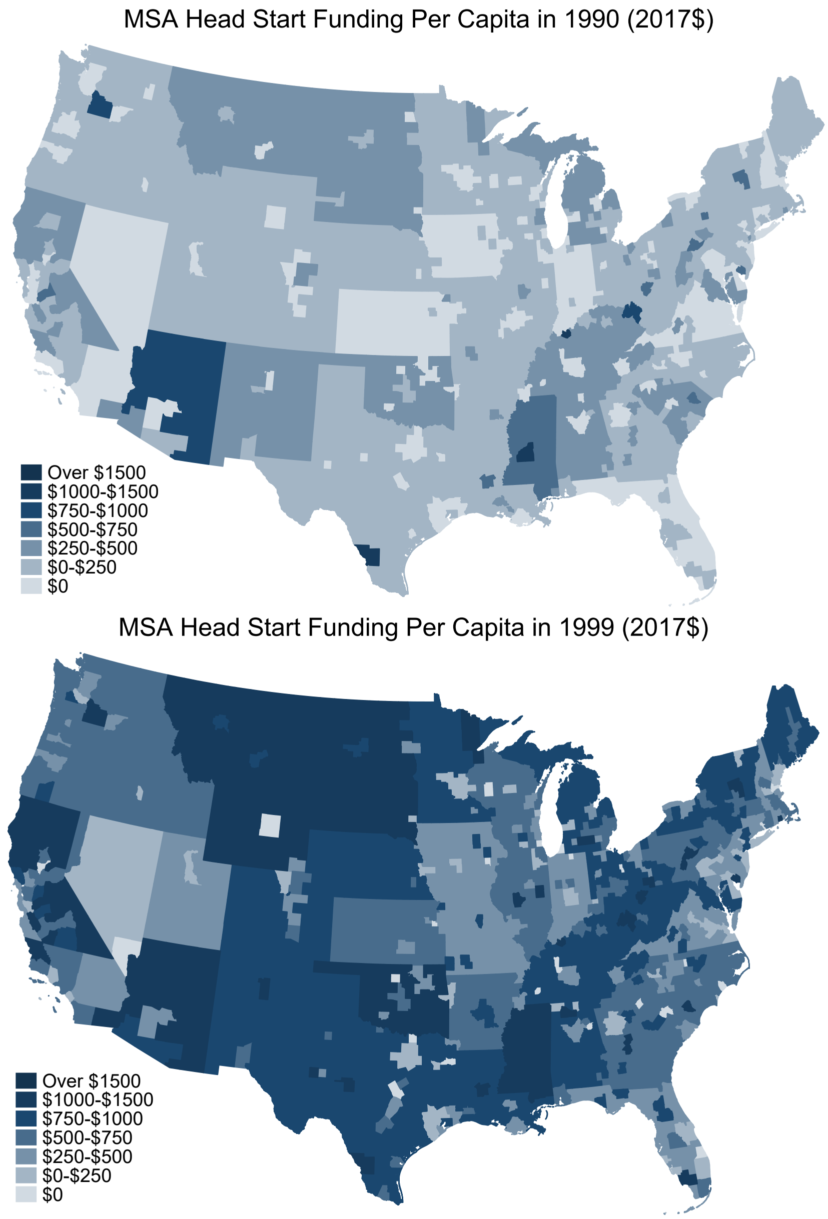
Figure A.1: Historical Head Start Enrollment and Timing of Experimental Evaluations



Notes: National Head Start Enrollment reported in hundreds of thousands. During the 1960s, many students were enrolled in summer programs.

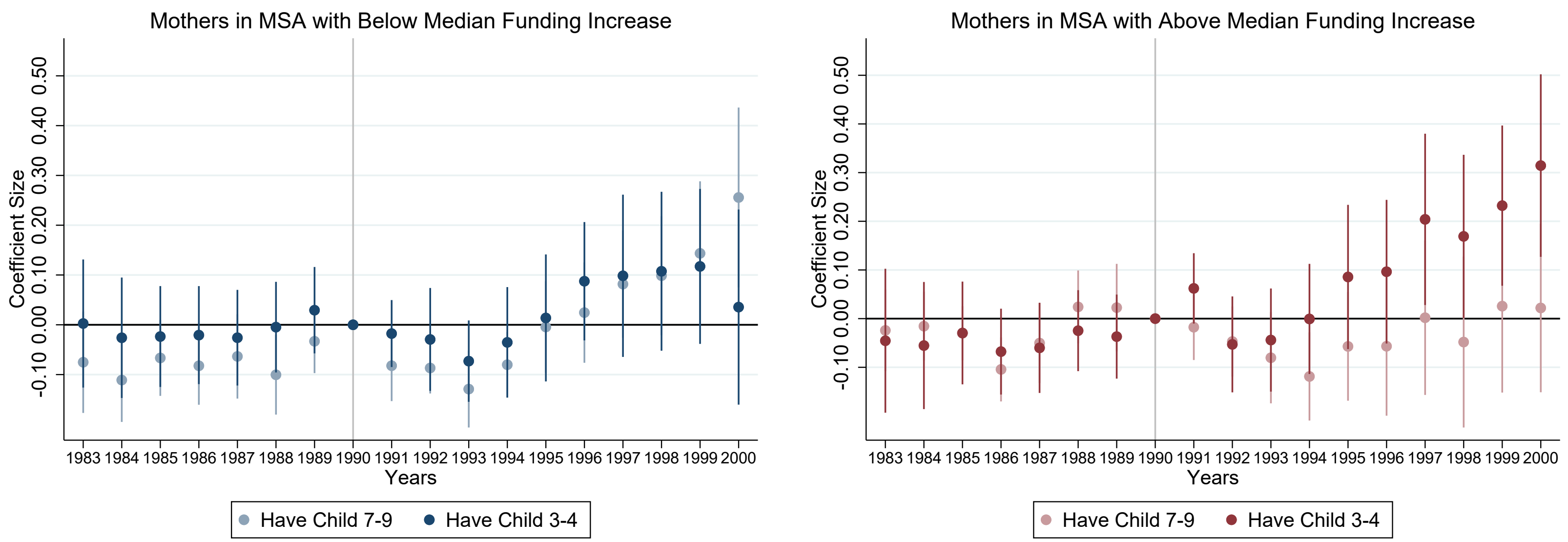
Source: Enrollment rates constructed from Head Start Early Childhood Learning & Knowledge Center national enrollment data. Authors' calculations.

Figure A.2: Changes in Metropolitan-level Funding from 1990 to 1999



Source: City level funding obtained from the historic Consolidated Federal Funds Report and aggregated to the metropolitan-level. Authors' calculations.

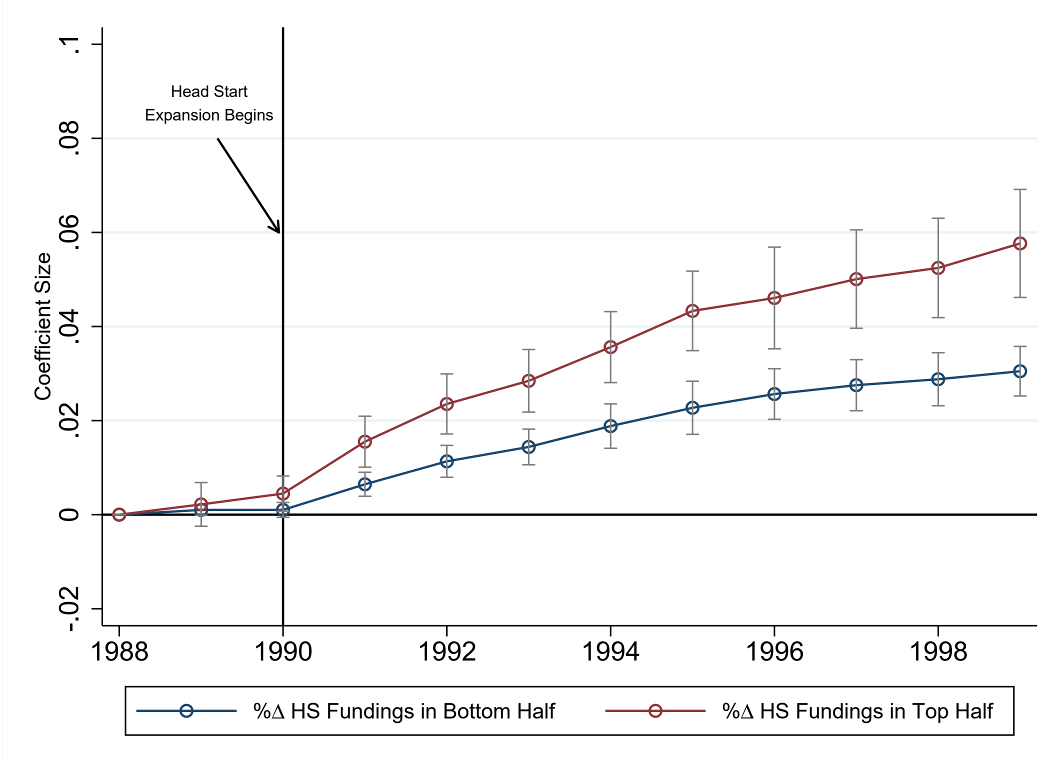
Figure A.3: Trends in Employment of Mothers with an Age-eligible Child and Older Child, Subsample of MSA reported before 1986



Notes: Coefficients from equation (2) are plotted separately for single mothers with an age-eligible child 3-4, or an ineligible child 7-9, in a MSA with below or above median increase in funding between 1983 and 1999. The outcome is employment during the previous 12 months. In 1986, the CPS began reporting over 150 more metropolitan areas. We restrict the sample to the subsample of metropolitan areas that were reported in 1983 to maintain a balanced panel. These metropolitan areas are more populated, urban areas. Regressions are estimated separately for MSA where the change in per capita Head Start Funding was in the bottom half of the distribution and in the top half of the distribution. Ninety-five percent confidence intervals also provided. To interpret, 0.1 is a ten percentage point change.

Source: CPS ASEC 1983-2000. Authors' calculations.

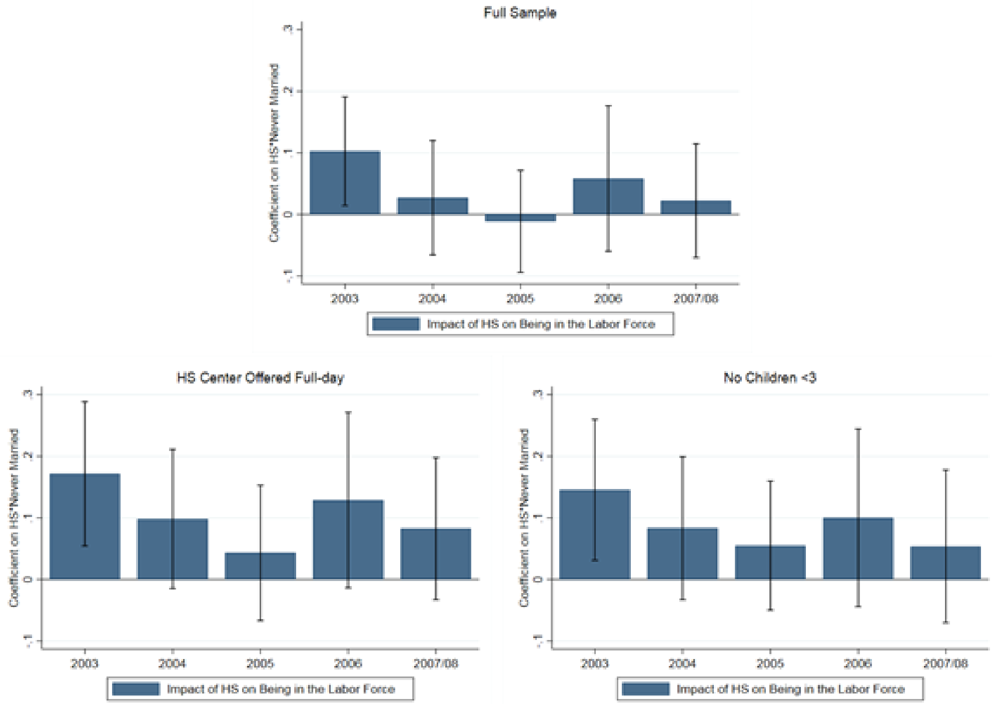
Figure A.4: Trends in State-level Head Start Enrollment per 3-4 year-old



Notes: Coefficients from event study estimates of Head Start enrollment per 3- and 4-year-old. Regressions are estimated separately for states where the change in per capita Head Start Funding was in the bottom half of the distribution and in the top half of the distribution. Ninety-five percent confidence intervals also provided. To interpret, 0.05 is a five percentage point change.

Source: Kids Count Data 1988-1999. Authors' calculations.

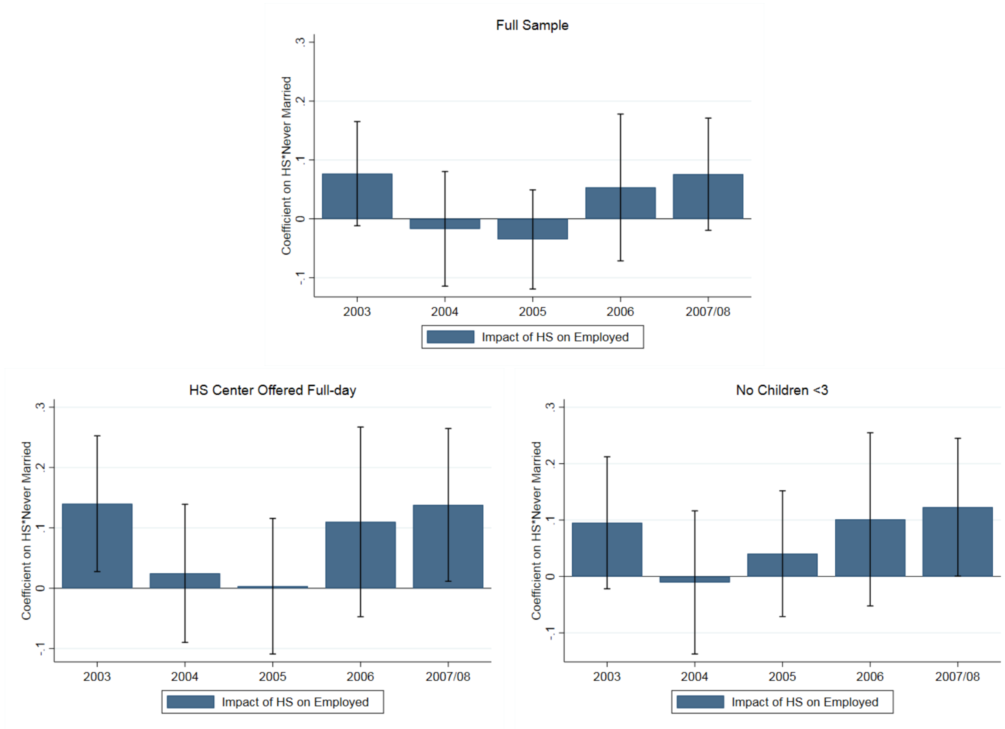
Figure A.5: Persistent Impact of Head Start on Labor Force Participation of Never Married Mothers



Notes: Bars plot the coefficients from the IV regression on Head Start enrollment, where the outcome is the mother's labor force participation status in the spring of each of the given years. Sample restricted to households that participated in the listed Parent Interview in the given year. The Head Start randomized treatment occurred in Fall 2002, and the program lasted through Spring 2003. The survey rounds in 2004-2008 are after the randomized treatment is over. In 2006, only parents of children who were 3-years-old at the time of treatment were re-interviewed. The final follow up was in 3rd grade, which was in 2007 for children in the 4-years-old cohort and 2008 for children in the 3-years-old cohort. Month of interview fixed effects in all years except 2007/08 when the month of interview is not available. All regressions are weighted using inverse probability weights constructed from the respective wave. Standard Errors are clustered at the Head Start Center the household applied to and was assigned treatment. 95 percent confidence intervals are included in black. Small coefficients in 2005 were primarily due to mothers in control households catching up to mothers in treatment households when children in the youngest cohort entered kindergarten.

Source: Head Start Impact Study, obtained through ICPSR. Authors' calculations.

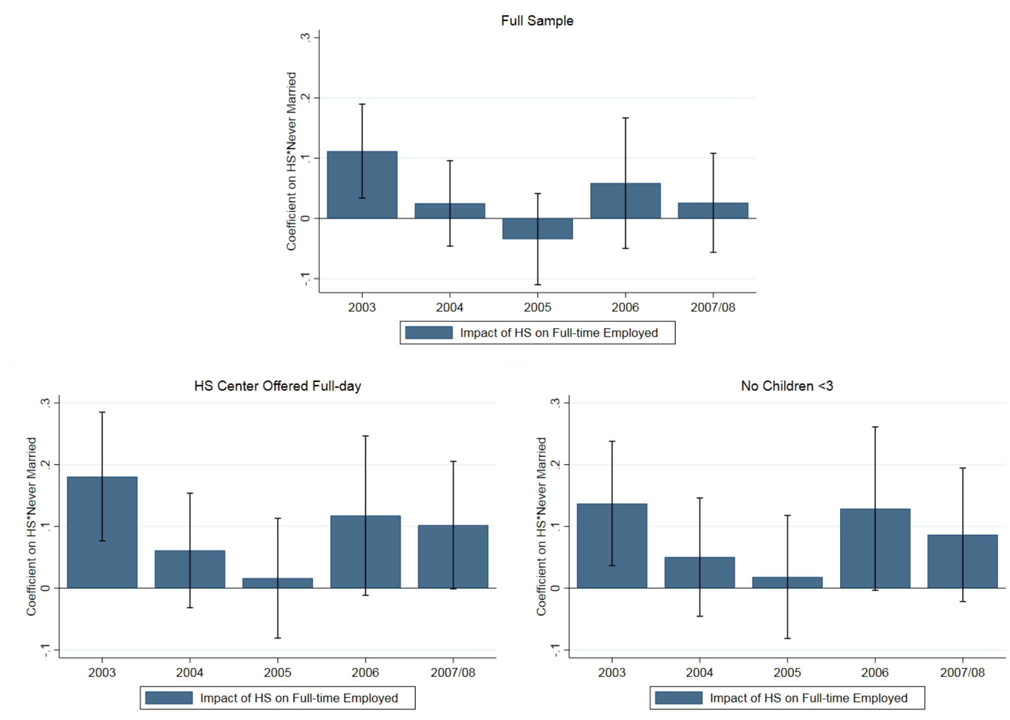
Figure A.6: Persistent Impact of Head Start on Employment of Never Married Mothers



Notes: Bars plot the coefficients from the IV regression on Head Start enrollment, where the outcome is the mother's employment status in the spring of each of the given years. Sample restricted to households that participated in the listed Parent Interview in the given year. The Head Start randomized treatment occurred in Fall 2002, and the program lasted through Spring 2003. The survey rounds in 2004-2008 are after the randomized treatment is over. In 2006, only parents of children who were 3-years-old at the time of treatment were re-interviewed. The final follow up was in 3rd grade, which was in 2007 for children in the 4-years-old cohort and 2008 for children in the 3-years-old cohort. Month of interview fixed effects in all years except 2007/08 when the month of interview is not available. All regressions are weighted using inverse probability weights constructed from the respective wave. Standard Errors are clustered at the Head Start Center the household applied to and was assigned treatment. 95 percent confidence intervals are included in black. Small coefficients in 2005 were primarily due to mothers in control households catching up to mothers in treatment households when children in the youngest cohort entered kindergarten.

Source: Head Start Impact Study, obtained through ICPSR. Authors' calculations.

Figure A.7: Persistent Impact of Head Start on Full-time Employment of Never Married Mothers



Notes: Bars plot the coefficients from the IV regression on Head Start enrollment, where the outcome is the mother's full-time employment status in the spring of each of the given years. Sample restricted to households that participated in the listed Parent Interview in the given year. The Head Start randomized treatment occurred in Fall 2002, and the program lasted through Spring 2003. The survey rounds in 2004-2008 are after the randomized treatment is over. In 2006, only parents of children who were 3-years-old at the time of treatment were re-interviewed. The final follow up was in 3rd grade, which was in 2007 for children in the 4-years-old cohort and 2008 for children in the 3-years-old cohort. Month of interview fixed effects in all years except 2007/08 when the month of interview is not available. All regressions are weighted using inverse probability weights constructed from the respective wave. Standard Errors are clustered at the Head Start Center the household applied to and was assigned treatment. 95 percent confidence intervals are included in black. Small coefficients in 2005 were primarily due to mothers in control households catching up to mothers in treatment households when children in the youngest cohort entered kindergarten.

Source: Head Start Impact Study, obtained through ICPSR. Authors' calculations.

For Online Publication: Appendix B. Additional Robustness Analysis Details

In this section, we discuss additional analyses we conduct to probe the robustness of our estimates using the 1990s Head Start Expansions.

9 Event Study Analysis

As seen in Figure 2 of the paper, funding increases happen over time during the "treatment" period and build on each other. As in equation 1, it is intuitive to consider concurrent funding increases (which increase Head Start open slots) on concurrent employment. But it is less straightforward identifying places that are "treated" and "control" in an over-time event study when treatment intensity changes. In Equation 1, our baseline specification, the controls are not interacted with child age and Head Start funding intensity, meaning we are not allowing other policies and controls to affect the groups differently. By stratifying the estimation in equation 2, we are able to present level trends in employment across different groups. We are also able to relax this restriction and allow the other policies and controls to have a different effect by both age-eligibility and Head Start funding intensity. Perhaps the biggest concern is that we are no longer directly comparing treatment and counterfactual women in the same metro area.

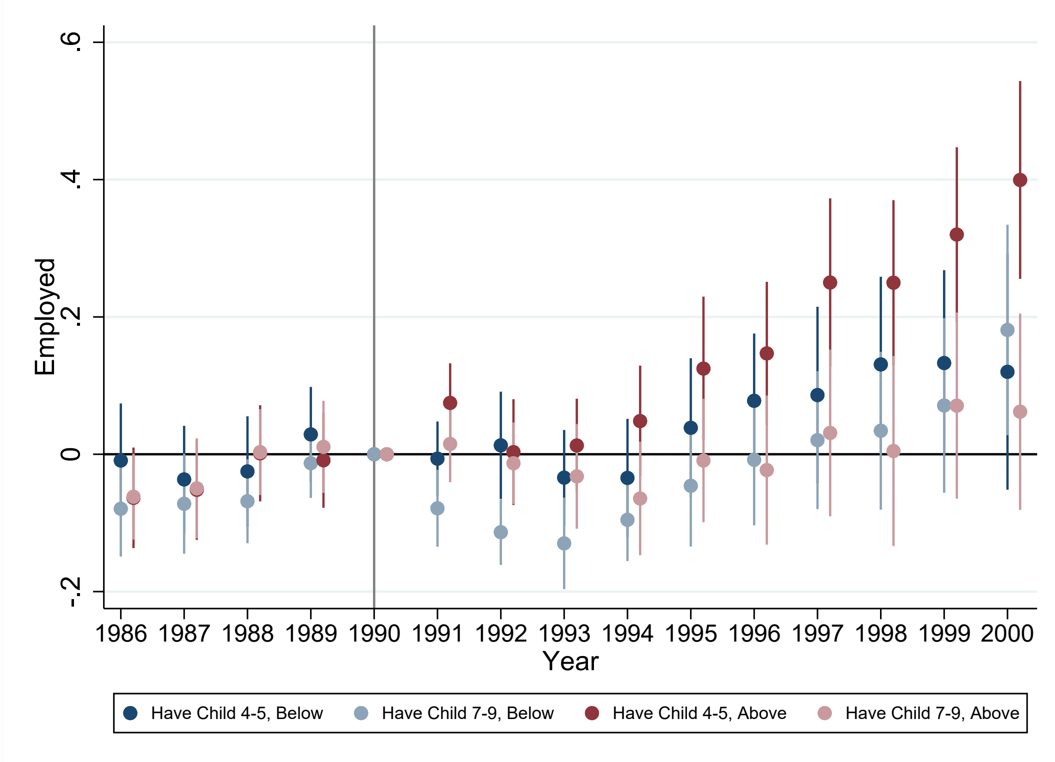
To more closely match the event study to Equation 1, we estimate one fully-interacted regression. This model interacts Head Start funding intensity with Head Start eligibility (Child 3-4) and year effects, including all of the off interactions and direct effects, and including the same interactions for all of our controls, but also include MSA fixed effects (to make a comparison between treatment and counterfactual mothers in the same MSA as in Equation 1). We see an almost identical pattern of results (Appendix Figure B.1), which suggests treatment and counterfactual mothers in the same MSA follow similar trends in the pre-period.

While a fully-interacted model more closely mimics Equation 1, it still does not directly map into Equation 1 due to the inclusion of interactions between controls, age-eligibility, and Head Start funding intensity. We next exclude the added interactions, to match Equation 1 even more closely, yielding Appendix Figure B.2. The spread between treatment and counterfactual groups in the post period is less pronounced. Allowing individual controls and the state demographic controls to vary by age and treatment intensity does not cause the difference. Nearly all of the difference is caused by allowing the maximum EITC refund and the presence of TANF waivers to have different effects by age-eligibility and local Head Start funding intensity. The EITC increases steadily throughout our sample, but there are large increases associated with OBRA93. TANF waivers also begin in 1993 and expand until full adoption in 1998. As seen in columns

(5) and (6) of Table 4, our estimates are robust to allowing the effects of the EITC and TANF to vary by Head Start age-eligibility. This is relevant given that TANF's work requirements differed for mothers with young children, and existing evidence suggests mothers with young children were more responsive to the EITC (Looney and Manoli, 2013).

Figure B.3 plots the event study coefficients that match Equation 1, but allows the EITC and TANF waiver controls to vary by age group and Head Start funding intensity (above or below 50th percentile). These interactive effects are theoretically intuitive. Increases in Head Start funding and slots potentially relaxes childcare constraints for single mothers. By relaxing this cost constraint, mothers face higher returns to work. The EITC increases the returns to work by subsidizing wages for low-income workers. Access to Head Start increases the returns to work, and returns are larger when the EITC expands. These two policies might generate a complementarity by relaxing cost constraints and subsidizing wages, leading to a larger net benefit associated with work. Similarly, additional restrictions like time limits introduced with TANF might remove a welfare wedge keeping single mothers out of the labor force. When interacted with a reduction in the cost of working associated with Head Start and the implicit childcare, this pro-work incentive of TANF might lead to larger responses to the policy interactions.

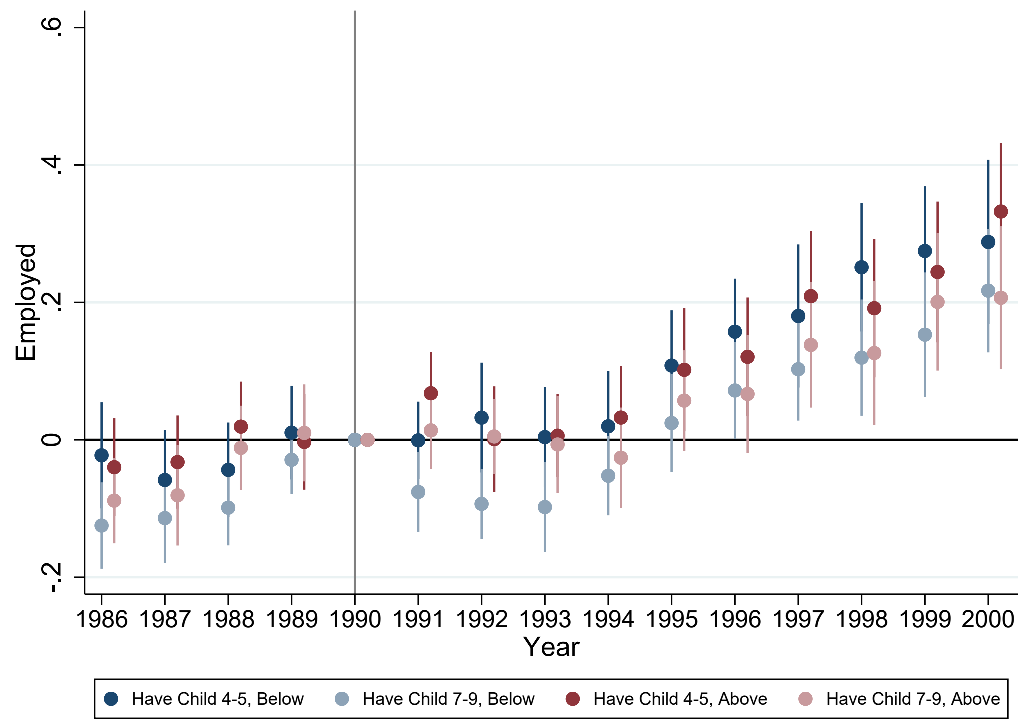
Figure B.1: Non-Stratified Event Study Estimates, with Fully Interacted Controls to Test the Role of Within-MSA Comparison



Notes: Coefficients from the non-stratified event study where controls are fully-interacted but not the MSA fixed effects (allowing us to compare age-eligible and inelgibile mothers in the same MSA) are plotted for single mothers with an age-eligible child 3-4, or an ineligible child 7-9, in a MSA with below or above median increase in funding between 1989 and 1999. The outcome is employment during the previous 12 months. In 1986, the CPS began reporting over 150 more metropolitan areas. We restrict the sample to 1986 to maintain a balanced panel of metropolitan areas. Regressions are estimated separately for MSA where the change in per capita Head Start Funding was in the bottom half of the distribution and in the top half of the distribution. Ninety-five percent confidence intervals also provided. To interpret, 0.1 is a ten percentage point change.

Source: CPS ASEC 1986-2000. Authors' calculations.

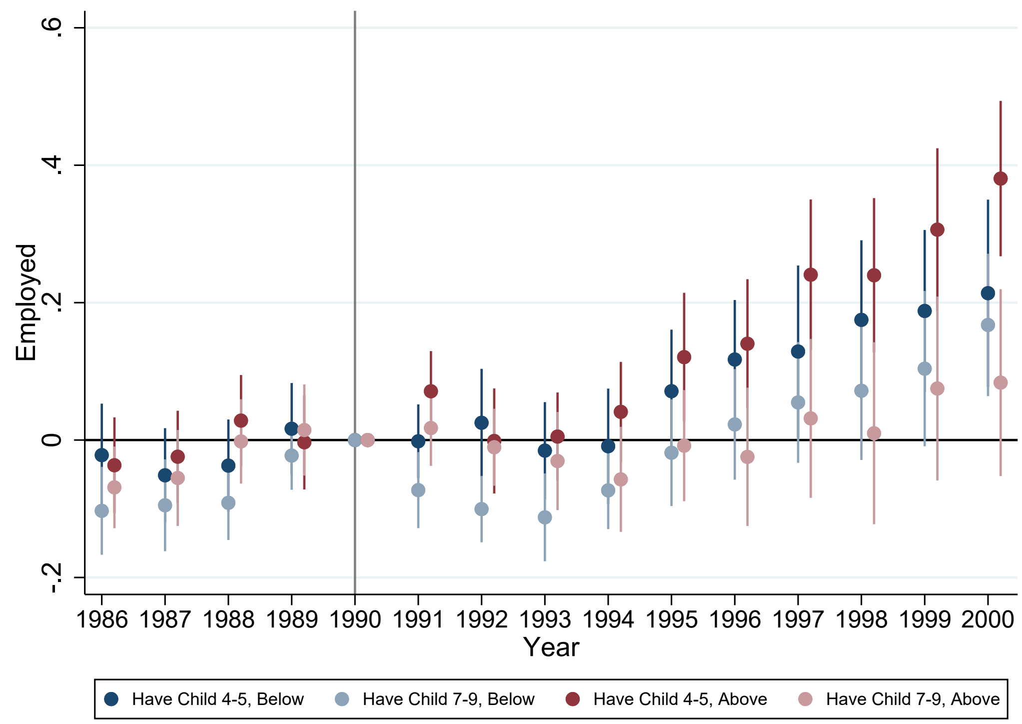
Figure B.2: Non-Stratified Event Study Estimates, with Uninteracted Controls to More Closely Match Equation (1)



Notes: Coefficients from the non-stratified event study where controls are not interacted (to more closely map into equation (1)) are plotted for single mothers with an age-eligible child 3-4, or an ineligible child 7-9, in a MSA with below or above median increase in funding between 1989 and 1999. The outcome is employment during the previous 12 months. In 1986, the CPS began reporting over 150 more metropolitan areas. We restrict the sample to 1986 to maintain a balanced panel of metropolitan areas. Regressions are estimated separately for MSA where the change in per capita Head Start Funding was in the bottom half of the distribution and in the top half of the distribution. Ninety-five percent confidence intervals also provided. To interpret, 0.1 is a ten percentage point change.

Source: CPS ASEC 1986-2000. Authors' calculations.

Figure B.3: Non-Stratified Event Study Estimates, with EITC and TANF Fully Interacted



Notes: Coefficients from the non-stratified event study where the EITC and TANF waiver controls are fully-interacted are plotted for single mothers with an age-eligible child 3-4, or an ineligible child 7-9, in a MSA with below or above median increase in funding between 1989 and 1999. The outcome is employment during the previous 12 months. In 1986, the CPS began reporting over 150 more metropolitan areas. We restrict the sample to 1986 to maintain a balanced panel of metropolitan areas. Regressions are estimated separately for MSA where the change in per capita Head Start Funding was in the bottom half of the distribution and in the top half of the distribution. Ninety-five percent confidence intervals also provided. To interpret, 0.1 is a ten percentage point change.

Source: CPS ASEC 1986-2000. Authors' calculations.

10 Robustness

10.1 Sensitivity to Other Large Welfare Policies in the 1990s

As both the EITC and traditional welfare were changing during the 1990s, we explore whether our estimates are sensitive to these policies. In our baseline analysis we control directly for these programs. In Table 4 we show that controlling for TANF or the EITC and allowing these effects to vary by treatment status does not explain the effect of Head Start funding among the Head Start eligible. We explore this further in Appendix Table A.6. In column (1) we show that the estimate is similar if we exclude our policy controls.

To further verify that we are not capturing the effect of other low-income policy during the period, we estimate equation 1, but use the presence of a TANF waiver, the maximum welfare benefit, the presence of a Head Start Family Services Center Grant, and the maximum EITC benefit the household is eligible to receive as outcomes (Appendix Table A.6), as suggested by Pei et al. (2019).[49] Head Start funding per child is not predictive of these policies. In addition, controlling for these policies separately or jointly has no effect on our main coefficient of interest.[50]. Our effects do not appear to be driven by alternative policies during the period.

10.2 Alternative Counterfactuals

In the manuscript we include estimates of the employment effect using single mothers with a child under three as the counterfactual, rather than single mothers with a child ages 7 to 9. As seen in Appendix Table A.7 we see a similar pattern across all of our employment outcomes. Kleven (2019) and Looney and Manoli (2013) show that the general increase in employment among single mothers in the 1990s is largely driven by mothers with younger children and suggest labor supply trends in the 1990s are driven by welfare reform. This specification can help rule out that our baseline results are simply driven by mothers of young children being more likely to leave welfare and become employed during this period. We also estimate the effect of Head Start on maternal employment on more restricted samples, that are potentially more similar. In Panel A of Appendix Table A.9 we identify the sample of single mothers that have a child under age 3. We then

[49]From 1991-1995, the Head Start Family Service Center program provided 65 grants to local Head Start centers to connect Head Start parents with community resource to target literacy, employability, and substance abuse. Local grants lasted 3 years, and the average grant was $250,000 per year (U. S. Department of Health and Human Services, 2000). Evaluation of the program following a randomized control trial concluded that the grants did not affect parental employment (U. S. Department of Health and Human Services, 2000).

[50]In Columns (10) and (11) we also include fixed effects for the number of EITC eligible children. This makes a comparison within family size, mirroring the difference in difference strategies used to evaluate the EITC.

flag mothers amongst this sample that also have a age-eligible child (3-4) as treated. We then see how Head Start funding affects employment of mothers with an age-eligible child among mothers with a child under three. Appendix Table A.9 shows that having an age-eligible child is associated with a 3.8 percentage point increase in employment when restricting the sample to include only single mothers with a child under 3. We also see increases in full-time employment, part-time employment, weeks and hours worked, and wage income in this sample. Limiting the sample to only include mothers with a 6- or 7-year-old, we find that also having an age-eligible child (3- or 4-year-old) is still associated with a 4.6 percentage point increase in employment and similar impacts on full-time employment, weeks and hours worked, and wage income. If anything, the treatment group in these samples have more children on average which would bias our estimates towards zero.

10.3 Specification Checks

First, we show that our estimates are not sensitive to details of the dataset and time period. Households in the CPS are surveyed for four months, exit the survey for eight months, and then are surveyed for four more months. As such, it is possible that some single mothers appear twice in our March ASEC CPS sample. In Appendix Table A.11 we restrict the sample to only include single mothers the first time they appear in the ASEC sample, eliminating the small number of people who might be in the control one year and treatment the next. The effect of Head Start funding on mothers with age eligible children are similar, at 1.7 percentage points. We also verify that our estimates are robust to the introduction of Early Head Start. In 1994, Early Head Start for children under three was introduced. Early Head Start was small, enrolling less than 35,500 children under three (0.3 percent) nationwide by 1999. In comparison, nearly 10 percent of 3 and 4-year-olds were enrolled in Head Start. Early Head Start has remained small, serving less than three percent of eligible children and accounting for only eight percent of Head Start funding by 2009 (Hoffman, 2010). Head Start has a still significant, but larger effect on employment if we cutting off the analysis sample in 1995, to avoid the Early Head Start period. If we include child age-specific linear trends (allowing potentially different trends for mothers of three-year-olds, four-year-olds, seven-year-olds etc.) the point estimate is unchanged.

10.4 Using Mother's Education to Tag Head Start Eligible Households

Mother's education could also be used to identify the sample of likely eligible mothers, rather than marital status. In Appendix Table A.12 we estimate equation (1) for all mothers (both married and single) with a high school degree or less. We estimate a significant one percentage point increase in employment and increases in weeks worked, hours worked, and wage income. These estimates are smaller and less precise, which we would expect with a less predictive eligibility tag.

10.5 Accounting for Migration and Compositional Changes

Another concern is that places that experience larger increases in Head Start funding could be experiencing differential, compositional changes that affect average labor market outcomes. For example, our estimates would be biased if single women with stronger labor force attachment move into places with higher Head Start funding. In Appendix Table A.13 we replicate our results from Table 1, but exclude women who moved in the last year.[51] If anything, the effects are even stronger, suggesting the labor market effects are not driven by compositional changes in our sample that are correlated with the treatment.

A similar concern is that because we condition the sample based on marital status, changes in sample composition may explain results if Head Start expansions influence marriage decisions of mothers. To rule this out, we estimate our triple difference specification including all treatment and counterfactual mothers, regardless of marital status and use the single mother indicator as our outcome. We estimate that increases in Head Start funding predict a significant one percentage point increase in the probability of being single. However, if we stratify by whether the woman moved in the last year, we find that this is entirely driven by mothers who moved. Said another way, single mothers are more likely to move to places that experience Head Start funding increases relative to married mothers. This suggests that after accounting for geographic mobility, our results are not driven by altered marriage patterns in response to Head Start funding.

10.6 Differential Effects in MSAs and City Centers

To be representative, our baseline sample includes single mothers that live in non-metropolitan area state remainders. Eligible and in-eligible women in the non-metro state remainder might not experience the same local labor market, as mothers in the same metropolitan area are likely to. Also, Head Start centers typically lie in urban, city centers, so mothers residing outside the city center are less likely to gain access to Head Start through these expansions. As an additional robustness check, we estimate the impact of Head Start funding on employment and income for mothers in metropolitan areas and for mothers more likely to be impacted by the program in the central city and less likely to be impacted outside the central city (Appendix Table A.14). Patterns are similar for single mothers in metropolitan areas, suggesting that including mothers in non-MSAs is not problematic. Most of the effects are concentrated among mothers in the central city, which we would expect if that is where Head Start is most prevalent.

[51] State of birth is not included in the CPS, so we cannot condition the sample on never having moved, only migration behavior in the last year.

For Online Publication: Appendix C. Head Start Impact Study Analysis Details

The United Stated Department of Health and Human Services conducted the HSIS as part of a Congressional directive to evaluate program effects on child cognitive development (Puma et al., 2012). Puma et al. (2012) provide detailed and descriptive information about the experimental design, and we provide a brief overview of the study. In the Fall of 2002, the study randomized children ages three and four who applied to oversubscribed Head Start centers into a treatment group offered enrollment or a control group denied enrollment at that center that year. The study measures the effect of being exposed to Head Start for the 2002-2003 academic year. Most children in the four-year-old cohort progressed to kindergarten following the year of the study. Many children in the three-year-old cohort continued in some form of early childhood education the following year; however, the study offered Head Start placement to all children in the control group for the academic year following the study. The HSIS collected information on children and their families in the Fall of 2002, Spring of 2003, 2004, 2005. In 2006, only parents of children who were 3-years-old at the time of treatment were re-interviewed. The final follow up was in 3rd grade, which was in 2007 for children in the 4-years-old cohort and 2008 for children in the 3-years-old cohort. Although the study collected rich information on child educational, emotional, social, and physical development, this paper focuses on measures of mothers' demographics, education levels, work status, and occupations. The parent measures generally remain stable across sample waves, allowing for a study of outcomes during the year of treatment and later outcomes. The sample includes 4,442 first time Head Start applicants across 353 Head Start centers, with 2,646 children in the treatment group and 1,796 children in the control group. Although Head Start centers offered placement to all children in the treated group, about fourteen percent of treated children did not enroll at the Head Start center (no shows), and about half of these children enrolled in center-based care elsewhere. Parents, relatives or home-based care providers cared for the remaining "no show" children. About forty percent of children in the control group enrolled in other preschools chosen by their parents, and twelve percent of children in the control group managed to enroll at other Head Start centers (crossovers). According to parent reports, about sixty percent of children in the control group receive care from parents, relatives, or home-based care providers, suggesting that Head Start participation primarily shifts home care. This counterfactual child care setting gives context to why Head Start enrollment might be expected to relax a mother's temporal constraints and lead to employment effects (Duncan and Magnuson, 2013).

Main demographic and family measures from the Fall 2002 baseline balance across the treatment and control groups, as demonstrated in Table 6. Only one of the nineteen demographic measures differs at the five percent significance level, suggesting validity in the experiment's randomization. The p-value on the joint F-test (for all characteristics below the line) is 0.9. As expected, given Head Start eligibility requirements, the mothers in the sample are somewhat disadvantaged. About thirty percent of the sampled mothers identify as White, non-Hispanic, thirty percent as Black, non-Hispanic, and thirty-six percent as Hispanic. Sixty-five percent of the mothers in the sample were not married, with thirty-nine percent of mothers having never married. Thirty-eight percent of mothers had not completed high school or earned a GED certificate by Fall 2002, the time of enrollment. The mothers were young on average, with over a quarter of the mothers' reporting being younger than twenty-five. Between thirty-six and forty percent of mothers have a child in the household who is younger than their Head Start eligible child.

We measure maternal employment at the end of the 2002-2003 academic year to give mothers time to make labor market adjustments. We also measure employment effects in subsequent years through the third grade interview. Mothers report on employment at the time of the interview. We are interested in understanding Head Start's impact on both the decision to work as well as the intensity of labor force attachment. As such our main outcomes of interest are the extensive margin measures of whether a mother is in the labor force, is employed, and full vs. part-time employment status. Labor force attachment is defined to equal one if the woman is employed full-time, part-time, looking for work, laid off from work, or in the military, and zero if not. Another way to capture work intensity is to exam the mother's wage income. Unfortunately, there is only limited coverage of income in the HSIS. Only household income is reported, collected through two survey questions. One question reports the dollar amount of income, and the other reports income bins. Most households reported the household income bin, but many did not report the actual dollar amount. This leads to less precise measures and smaller samples for income measures and we do not focus on these measures.[52]

We will estimate effects separately for mother with and without younger children as well as for mothers who applied to Head Start centers that either offered full-day or part-day services. Whether or not the center offers full day services is determined from the center-based care director's interview. For all children attending a child care center, the center's director was asked whether full-day services were offered. If we focus on children at Head Start Centers we can identify availability of full-time services. Unfortunately, within a given center different answers were given. For this reason we label a Head Start Center as offering full-day

[52]We have repeated the labor supply analysis using these income measures and find that never married mothers are more likely to have monthly household income over $500 but not to have monthly income over $250 or $1,000. An extra $250 a month would result in annual income effects consistent with the effects in Table 3. This is concentrated among never married mothers without younger children and at full-day Head Start centers.

if the director reported full-day programming available for 50 percent or more of the enrolled students.

Empirical Approach. Because applicants in the HSIS were randomized independent of personal characteristics, placement in the treatment and control groups remains uncorrelated with unobserved personal characteristics. Therefore, estimates relating to Head Start's effects remain free from endogeneity concerns. This allows us to estimate the impact of assignment to Head Start ("treatment") on maternal employment and household income measures (representing the intent to treat). Because we also know Head Start enrollment, we can estimate the impact of a child's Head Start enrollment on maternal labor supply in an instrumental variables framework, using the random assignment to Head Start to instrument for Head Start enrollment as follows:

$$\begin{aligned} \textit{Head Start}_i &= \alpha_1 Treated_i + \mu_m + \eta_i \\ Y_i &= \beta \widehat{HeadStart}_i + \mu_m + \varepsilon_i \end{aligned} \tag{C.1}$$

$Treated_i$ indicates if the household was randomly assigned to Head Start through the lottery, $\textit{Head Start}_i$ indicates if the mother had a child who was enrolled in Head Start in the 2002-2003 academic year, and Y_i is one of the employment outcomes outlined above. Month of interview fixed effects are also included (μ_m) to account for any potential differences in employment among those interviewed in March, April, May, or later.[53] The first line is the first stage relationship, while the second line is the causal relationship of interest: the impact of Head Start enrollment on the mother's outcomes.[54] In all specifications, standard errors are corrected for clustering at the Head Start center the family applied to.

All estimates are limited to the samples of parents completing parent interviews in the years of interest. As previous researchers have found, the timing and presence of the baseline interviews and tests vary between the treatment and control groups, with treated children more likely to complete assessments earlier in the academic year. Treatment and control groups experienced differential attrition, which could lead to bias if unaddressed. In all estimations, we correct for sample attrition by augmenting baseline weights, which already account for complex sampling and balancing. We estimate inverse propensity-score adjusted weights, similar to the approach taken by Bitler et al. (2014). To estimate propensity scores, we estimate a logit model and baseline weights, which account for sampling design, to estimate the predicted probability of being in the treatment group as a function of baseline characteristics. Additionally, the timing of surveys correlates with sample attrition, and including survey month in the logit model explicitly controls for sample attrition. The resulting inverse propensity-score weights thus correct for sample attrition, and we use these

[53] About 10 percent of households were interviewed in June or later.

[54] For reference, the first stage coefficient on treated is 0.64 and the f-statistic on this excluded instrument is 1129.

in all analyses. Kline and Walters (2016) propose re-weighting individuals by the inverse probability of receiving treatment. Since treatment was randomized at the center-level. They calculate the center-level share of participants that were treated and use the inverse of this measure as the weights. This corrects for any observable differences, but it does not account for attrition. We have estimated all of our results using these inverse probability weights and find a similar pattern of results.

The family settings, earning dynamics, and labor market opportunities are likely quite different by current and previous maternal marital status. Single mothers are more likely to be primary earners, while married mothers might behave like secondary earners. Even among single mothers, never married mothers are more negatively selected on multiple dimensions relative to divorced, separated, and widowed mothers. Never married mothers in the control group have lower employment rates, are less educated, are younger, and have more children. To match the observational analysis from the 1990s, we also estimate equation (C.1) separately by marital status as follows:

$$\begin{aligned} Y_i = {} & \beta_1 \widehat{Head\ Start_i * Married_i} + \beta_2 \widehat{Head\ Start_i * Prev.\ Married_i} \\ & + \beta_3 \widehat{Head\ Start_i * Never\ Married_i} + \beta_4 Married_i + \beta_5 Prev.\ Married_i \\ & + \beta_6 Never\ Married_i + \mu_m + \varepsilon_i \end{aligned} \tag{C.2}$$

The $Head\ Start_i$ indicator is interacted with three mutually exclusive marital status groups: Never Married, Previously Married, and Currently Married. Mother's marital status is only collected in the first parent survey in Fall 2002, so marital status assignment is fixed throughout all of our analysis. As in equation (C.1), we instrument for these interactions using $Treated_i$ interacted with the marital group. We also include the direct effect for each of these groups and do not include a constant. As such, the coefficients β_4, β_5, and β_6 represent the mean of the outcome among never married, previously married, and currently married mothers who do not have children enrolled in Head Start. The coefficient β_3 represents the impact of Head Start availability on the mother's employment outcomes among never married mothers, while β_1 and β_2 represent the effects for previously married and never married mothers. From this regression, we identify the causal impact of Head Start enrollment on maternal employment and household income, allowing the effect to vary by marital status.

Comparison to Schiman (2021)

Schiman (2021) has a concurrent paper that explores the effects of Head Start on maternal labor supply using the HSIS. She finds that Head Start increased full time employment among married mothers of 3-year-olds, with no significant effects among mothers of 4-year-olds or unmarried mothers. She estimates the following two stage least squares equations

$$\begin{aligned} headstart_{ic} &= \theta_0 + \theta_1 Treat_{ic} + \delta X_{ic} + \tilde{\theta}_c + \tilde{\delta}_{month} + \phi W_{ic} + \tilde{\varepsilon}_{ic} \\ Y_{ic} &= \pi_0 + \pi_1 \widehat{headstart}_{ic} + \rho X_{ic} + \gamma_c + \psi_{month} + \mu W_{ic} + \varepsilon_{ic} \end{aligned} \tag{C.3}$$

She include fixed effects for the Head Start center the family applied to, month of interview fixed effects, and weeks elapsed since September 2002 (W_{ic}). In some of her specifications she includes individual covariates (X_{ic}), but not in her baseline exploring heterogeneity by marital status. Standard errors are corrected for clustering at the center level, and observations are unweighted.

This strategy differs from ours in several ways. The first two are not substantive. We do not include center fixed effects, but our estimation is robust to their inclusion. We also do not include the number of weeks since September 2002 linearly, but our estimates are robust to controlling for the number of weeks since September 2002.

There are two more substantive ways our estimation strategies diverge. First Schiman (2021) stratifies by cohort (3 vs. 4), and second, we differ in how we use marital status. Schiman (2021) estimates effects separately by marital status, while we interact marital status with treatment and estimate jointly (for sample size and data disclosure reasons). She also does not separately examine effects for previously married and never married mothers, even though they are demographically quite different. Finally, she uses the derived marital status variable provided in the survey which is based on the Fall 2002 marital status, but includes imputations for 871 mothers, or 20 percent of the sample. Schiman (2021) suggests that differences between her paper and our paper arise because we estimate effects jointly. However, we find that even when estimating separately by marital status, the effects are concentrated among never married mothers. Following her paper, we have attempted to replicate her specification and samples by focusing on Spring 2003 outcomes among mothers whose derived marital status and education was provided. Because some of the mothers who responded in Spring 2003 did not respond to the Fall 2002 parent survey, some of these mother's marital status is imputed.[55]

In Table C.1, we estimate effects for married mothers and unmarried mothers separately for each cohort, 3- and 4-year-olds, using Spring 2003 employment outcomes and derived marital status. This specification maps into the Schiman (2021) strategy, although we do not match her sample size exactly. We also estimate

[55]The HSIS does not provide information on how imputations were made. For our main specification, we do not use the derived values provided by the HSIS, given the uncertainty about whether future values of marital status were used to impute initial marital status (which could be endogenous), whether missing values on marital status were imputed based on employment, or whether variance was taken into account during imputation. This exercise to replicate the Schiman (2021) approach also is a sensitivity analysis which verifies that our results are not sensitive to using listwise deletion for missing data.

this specification on the subsample of unmarried mothers who are never married, to match our analysis. We estimate significant effects on full time employment for married mothers with 3-year-olds similar to Schiman (2021). However, effect sizes for never married mothers in both cohorts are similar in magnitude, but less precise. Pooling the two cohorts, as in our strategy, we only estimate a significant 9.5 percentage point increase in full time employment for never married mothers and smaller, insignificant effects for married mothers. This suggests the larger effects are among never married mothers.

We next see if effects differ when we do not include women with marital status imputations. In Table C.2 we estimate effects for married mothers, unmarried mothers, and never married mothers separately for each cohort using Spring 2003 employment outcomes and Fall 2002 marital status. When imputed observations are excluded, we only estimate significant effects for never married mothers with a 4-year-old. Married mothers and never married mothers with 3-year-olds report large increases in full time employment (9.6 and 10.3 percentage points respective) but neither are significant. Pooling the two cohorts, we estimate a marginally significant 7.2 percentage point increase in full time employment for married mothers and a significant 11.9 percentage point increase in full time employment for never married mothers. Once again, this is consistent with our results where effects are concentrated among never married mothers. These effects are estimated in fully stratified samples, suggesting that the effects detected among never married mothers are not a function of using interactions in our modeling approach. Both her specification and ours would suggest that married mothers perhaps experienced modest increases in full-time employment while never married mothers experienced large increases in full-time employment. This pattern is consistent with the marital status heterogeneity observed in the CPS analysis of the 1990s Head Start expansion.

Table C.1: Comparison to Schiman (2021)

	Married			Unmarried			Never Married		
	Employed	Full-time	Part-time	Employed	Full-time	Part-time	Employed	Full-time	Part-time
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					3-Year-Old Cohort				
Head Start	-0.008	0.133**	-0.131***	0.016	0.053	-0.037	0.061	0.099	-0.038
	(0.057)	(0.057)	(0.045)	(0.056)	(0.054)	(0.041)	(0.065)	(0.073)	(0.052)
Number of Centers		263			264			241	
Observations		870			1,080			819	
					4-Year-Old Cohort				
Head Start	-0.010	0.025	-0.035	0.030	0.006	0.024	0.077	0.115	-0.038
	(0.062)	(0.052)	(0.043)	(0.068)	(0.070)	(0.045)	(0.093)	(0.097)	(0.057)
Number of Centers		221			238			207	
Observations		733			814			543	
					3- and 4-Year-Old Cohort				
Head Start	0.013	0.063	0.050	-0.000	0.021	-0.022	0.030	0.095*	-0.065*
	(0.043)	(0.039)	(0.033)	(0.044)	(0.042)	(0.030)	(0.049)	(0.051)	(0.037)
Number of Centers		315			311			288	
Observations		1,603			1,894			1,362	

Notes: Sample stratified by the mother's reported marital status as recorded in the survey-provided derived marital status measure. This measure is based on the Fall 2002 marital status, but includes imputed values. This is the stratification used by Schiman (2021). Sample excludes women in prison or the military or with missing education. All regressions include center fixed effects, month of interview fixed effects, and weeks elapsed since September 2002. Standard errors corrected for clustering at the center level are provided in parentheses. p<0.01 ***, p<0.05**, p<0.1*.

Table C.2: Comparison to Schiman (2021), Stratified by Fall 2002 Marital Status

	Married			Unmarried			Never Married		
	Employed	Full-time	Part-time	Employed	Full-time	Part-time	Employed	Full-time	Part-time
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					3-Year-Old Cohort				
Head Start	-0.024	0.096	-0.121***	0.022	0.052	-0.030	0.079	0.103	-0.024
	(0.059)	(0.059)	(0.047)	(0.057)	(0.055)	(0.041)	(0.069)	(0.073)	(0.050)
Number of Centers		254			257			233	
Observations		783			976			738	
					4-Year-Old Cohort				
Head Start	0.023	0.063	-0.039	0.027	0.011	0.015	0.173	0.234**	-0.061
	(0.064)	(0.052)	(0.043)	(0.075)	(0.075)	(0.051)	(0.109)	(0.111)	(0.064)
Number of Centers		214			231			194	
Observations		680			739			482	
					3- and 4-Year-Old Cohort				
Head Start	0.020	0.072*	-0.052	0.001	0.021	-0.020	0.056	0.119**	-0.062*
	(0.044)	(0.039)	(0.033)	(0.045)	(0.043)	(0.032)	(0.051)	(0.053)	(0.037)
Number of Centers		308			305			279	
Observations		1,463			1,715			1,220	

Sample stratified by the mother's reported marital status in the initial Fall 2002 survey wave. This is the marital status reported in Fall 2002, not the derived measure that includes imputed marital status. As such, women who responded in Spring 2003, but not Fall 2002 are excluded. Sample excludes women in prison or the military or with missing education. All regressions include center fixed effects, month of interview fixed effects, and weeks elapsed since September 2002. Standard errors corrected for clustering at the center level are provided in parentheses. p<0.01 ***, p<0.05**, p<0.1*.

For Online Publication: Appendix D. Calculating Head Start Funding per Child from Consolidated Federal Funds Report Data

Our ability to quantify Head Start funding relies on publicly available annual Consolidated Federal Funds Reports (CFFR). From 1983 to 2010, CFFRs document how municipalities in the United States accounted for the use of federal funds. These reports provide detailed municipality level information on federally funded items, including payments to grantees for Head Start. For this study, we focus only on Head Start expenditures. Prior to 1991, Head Start expenditures were recorded in the CFFRs under code 13.600, and beginning in 1991, they were recorded using code 93.600. In order to calculate total funding for a MSA, we aggregated funds two times. First, funding amounts were aggregated to the county level using FIPS county codes in the CFFR data. We allocated all dollars for grantees to their own county. Second, we aggregated county-level Head Start funding each year to the metropolitan area. Using metropolitan areas aligns the geographic units with the CPS. We are interested in labor market responses of mothers, and metropolitan areas more closely relate to a mother's labor market compared to her county.

Aggregating neighboring counties up to the metropolitan area and focusing on urban areas minimize concerns about grantees funding children outside their own county. There is evidence that by 1994, grantees (most often in rural areas) sometimes had networks to serve children in neighboring counties (Currie and Neidell, 2007). Other than the year 1994, there is not good information on the degree that funding served children in neighboring counties. For the year 1994, using the mapping of grantees to children's counties from Currie and Neidell (2007), we find that mapping grantees to metropolitan areas (rather than counties), most funding stayed within a metropolitan area. Specifically, 83% to 86% of children served by a particular Head Start grantee in a metropolitan area attended school in the same metropolitan area. We detected a small amount of funding that crossed metropolitan areas. Between 1-3% of children served by a particular grantee in a metropolitan area attended school in another metropolitan areas. Most dollars moving out of metropolitan areas went to rural areas. Thirteen to fourteen percent of children served by a particular metropolitan-area grantee attended school in rural areas, suggested that most funding dollars shared outside a metropolitan area involved less populated areas that were not part of the analysis in this study. The flow of funding out of metropolitan areas to rural areas works against us finding results. As a robustness test, we interacted the fraction of children funded within the same metropolitan area with per child Head Start funding, and interaction terms were not significant predictors of any maternal labor supply outcomes. Due to the use of a triple difference design comparing mothers within the same metropolitan area to account for metropolitan area characteristics like this, the direction of the bias posed by this issue working against us, incomplete information on grantee networks over time, and limitations on linking grantees to CFFR reports, we make no adjustments for the instances when funding may go to children outside a metropolitan area.

To aggregate from county to metropolitan area, we relied on the crosswalk between FIPS county codes and metropolitan areas defined by the U. S. Census Bureau in 1990. Metropolitan areas are composed of whole counties, which allows us to avoid making decisions about how to split funding at the county level across areas. In the CFFR data we make two minor changes. First, we update the Dade County, FL FIPS code (12025) to the time consistent Miami-Dade County, FL FIPS (12086) after the county change. Second, because independent city South Boston, VA joined the surrounding county of Halifax County, VA in 1995, we add the independent city of South Boston, VA (FIPS 51780) to the Halifax County, VA (FIPS 51083) to create a consistent series over our analysis sample.

Metropolitan area Head Start funding was divided by the number of children ages 3 and 4 in the metropolitan area to obtain a nominal estimate of funding per child. Finally, we converted nominal funding into real values in 2017 dollars using the personal consumption expenditures price index from the Bureau of Economic Analysis.

To adjust Head Start funding for the number of children in a metropolitan area, we used population estimates of the number of children ages 3 and 4 in a metropolitan area. County-level population estimates came from the Surveillance, Epidemiology, and End Results Program (National Cancer Institute, 2017). Population data for Alaska and Hawaii during our sample period is incomplete, so we limit the sample to counties in the continental US. Again, to geographically align population estimates, we aggregated county-level population to the metropolitan area. We relied on the crosswalk from counties to metropolitan areas defined by the U. S. Census in 1990.