Publicly Provided Preschool and Maternal Labor Supply: Evidence from Three Head Start Natural Experiments

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Abstract

A considerable literature has tried to understand how the low-income preschool program Head Start affects participating children, but this program is likely to influence the decisions of other family members as well. For a low-income mother, access to Head Start likely provides a massive childcare cost subsidy, which might influence her labor supply decisions. To identify the impact of Head Start on the work decisions of single mothers, we will exploit variation from three natural experiments. First, we exploit the substantial funding and enrollment expansion to Head Start in the 1990s that varied over time and across metropolitan areas to measure labor supply responses. We use the Current Population Survey to compare mothers with age-eligible children to mothers with children below the age threshold. On average, these Head Start expansions are associated with a 2 percentage point increase in the employment rate of single mothers, which would imply that about 40 percent of the mothers who gain access to the program enter employment. We corroborate this evidence with two additional natural experiments: the initial, staggered roll-out of Head Start in the 1960s and the Head Start Impact Study randomized control trial in 2002. In both cases, we find that Head Start is associated with an increase in employment for some mothers. Evidence from the impact study suggest effects appear to be largest when a mother does not have younger children or when Head Start offers full day programing. This suggests that viewing publicly provided early childhood education programs like Head Start as a bundle of family-level treatments can perhaps shed new light on the short run impacts, mid run fade out, and long-run improvements associated with Head Start.

JEL: H42, H52, I28, J13, J22

1. Introduction

There has been a sustained interest in understanding whether early childhood education for low-income children can reduce the achievement gap and facilitate economic mobility. 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, finding that Head Start leads to short-run cognitive and non-cognitive gains, which fade out quickly over time (Bitler, Domina & Hoynes 2014; Puma et al. 2012), but seem to reappear in the long-run, positively affecting high school graduation, earnings, and second generation outcomes.[1]

However, not only does Head Start directly affect a child's development, it might also affect the decisions of other family members which may indirectly affect the child's outcomes (Gelber & Isen 2013). For example, Head Start may provide an implicit childcare subsidy for low-income single mothers with young children. In 2011 single mothers using center-based childcare paid an average of $2.48 per hour for care, approximately 35% of the hourly federal minimum wage (Herbst 2015).[2] These childcare costs cut into wages and reduce the net benefit associated with employment. Public provision of preschool provides a subsidy for childcare. In theory, this subsidy will reduce some of the costs associated with employment, leading to higher net wages, potentially changing employment decisions. This could result in long-lasting, indirect impacts of Head Start on the family. Narrowly focusing on child outcomes ignores these additional potential benefits.

[1] See for example Bailey, Sun & Timpe (2018), Barr & Gibbs (2018), Carneiro & Ginja (2014), Demming (2009), Ludwig & Miller (2007),Thompson (2018).
[2] This pattern has been true at least since the 1990s, when average hourly costs for center-based care were approximately 35% of the hourly minimum wage (Herbst 2015).

This paper provides new evidence that the implicit childcare subsidy provided by Head Start leads to increased employment among single mothers. During the 1990s, the United States congress expanded funding for Head Start preschool for low-income three- and four-year-olds and to a smaller extent Early Head Start for low-income infants and toddlers. Starting with the Head Start Expansion and Quality Improvement Act of 1990, funding per age-eligible child nearly tripled over the next ten years. Accompanying the sizeable increases in funding was dramatic growth in enrollment. Head Start enrollment nearly doubled between 1989 and 1999 (see Figure 1). By exploiting close geographic variation in spending increases in a generalized fixed effects framework, we compare single women with 3- and 4-year-olds to single women with children under 3 to measure whether and to what extent funding amounts per child affect maternal labor supply. By comparing single mothers with eligible children to single mothers with ineligible children in the same metropolitan area, we account for region specific characteristics or trends that might have led to increased funding and also affected employment of single mothers. This generalized triple difference approach estimates the causal impact of Head Start funding on employment as long as funding is not correlated with unobserved local characteristics that affect the employment decisions of mothers of three- and four-year-olds, but not mothers with only younger children.

We combine data from several sources to measure effects of the 1990s Head Start expansion. We construct metropolitan-level measures of Head Start expenditure per 3- and 4-year-old using the Consolidated Federal Funds Reports (CFFR). We link increases in expenditure to increased enrollment using the Current Population Survey (CPS) October education supplement and state-level head start enrollment data. Finally, we use the March CPS Annual Social and Economic Supplement (ASEC) between 1983 and 2000 to compare single mothers with age-

eligible children to other single mothers with dependent children within the same metropolitan area.

Consistent with Head Start expansion subsidizing childcare, we find that a $500 increase in per child Head Start spending increases pre-school enrollment in the CPS October supplement by 3.7 percentage points (8.6 percent). We also find that a $500 increase in per child Head Start spending increased annual employment rates by 2 percentage points among single mothers with young children, suggesting that the probability of being employed increased by 54 percent among single mothers when their child enrolled in preschool. This also resulted in more average weeks worked and higher wage earnings. These impacts are largest among mothers who are less-educated, never married, and from minority backgrounds. Surprisingly, we find that mothers with both age-eligible children and younger children in the home also respond. Our estimates are robust to individual controls, samples used, time varying demographic trends, and policy controls (such as the EITC and welfare reform which also drastically changed during this period).

We corroborate these results with two additional natural experiments. First, we evaluate employment among single mothers during the introduction and rollout of Head Start in 1965 using a similar strategy. Once again, despite significant data constraints, we find suggestive evidence that single mothers' employment in the late 1960s may have responded to having access to Head Start. Second, using the Head Start Impact Study (HSIS), the 2002 randomized control trial evaluation of Head Start, we evaluate the impact of Head Start enrollment on maternal employment for approximately 3,200 households with three- or four-year-olds. Having a child enrolled in Head Start marginally increases the probability of being in the labor force by 14 percent. These impacts of Head Start enrollment are 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 childcare costs. We find little evidence of persistent effects in the Head Start Impact Study.

Our analysis of these natural experiments mirrors prior work exploring the effects of Head Start on children, but sheds new light on how maternal labor supply is affected. The maternal labor supply response to childcare subsidies we find is not exclusive to one decade but is observed across cohorts and time. Our findings demonstrate that single low-income mothers with young children increase labor force attachment when given a childcare subsidy through Head Start.

This research contributes to a rich literature documenting the program effects of Head Start. Most prior work on Head Start in particular and early education more generally focuses on benefits to children only, neglecting the benefits to mothers and society more generally. Prior research has recognized the perplexing nature of the impacts of Head Start, with short-run cognitive impacts that fade-out quickly (Currie & Thomas 1995; Puma et al. 2012) but long-run positive educational and economic effects (Barr & Gibbs 2018; Carneiro & Ginja 2014; Deming 2009; Garza, Thomas & Currie 2002; Ludwig & Miller 2007). This has often been attributed to the development of non-cognitive skills, but Head Start could also be changing parents' behavior and the family context (Gelber & Isen 2013). Access to Head Start enables women to return to work one year earlier and may increase future labor force attachment, earnings, and overall household income in childhood. This may be an additional channel to help explain why short-run benefits fade out for children and reemerge in the long-run.

Our work intersects with a growing literature exploring the effects of subsidized childcare provision on maternal labor supply. Research on mothers' labor supply suggest that from 1960 to 1990, prior to the Head Start expansion, single mothers without a younger child responded to

kindergarten eligibility (Gelbach 2002) and the staggered roll out of kindergarten (Cascio 2009) by increasing labor supply. Recent research exploring maternal employment during the late 1990s and early 2000s, a decade after the Head Start expansion, present mixed findings. While some research suggests the effects of universal preschool and kindergarten enrollments on maternal labor supply were small at best and concentrated among low-income mothers (Cascio and Schanzenbach, 2013) and single mothers (Fitzpatrick 2010, 2012), Soldani (2015) finds labor supply effects that persist up to five years.[3] With the exception of Soldani (2015), prior literature addresses effects at a local or state level rather than national in scope, presumably due to the difficulty of identifying effects at the national level. We make a valuable contribution by assessing impacts at a national level using program expansion for identification. We analyze an understudied time period coinciding with rapid increases in maternal labor supply, and our results shed light on changing secular trends in labor supply responsiveness to subsidized childcare among single mothers. We compare our findings to previous work, focusing on similar settings but during prior and subsequent time periods. The 1990s saw rapid expansion in preschool access and enrollment among low-income families, which might help explain why research examining the end of the decade saw little response within this group to universal access to preschool and kindergarten. Blau and Tekin (2007) do explore the effect of 1996 welfare reform childcare subsidies on maternal employment, but these changes likely had a minimal impact on employment rates (Meyer and Rosenbaum, 2001). To the best of our knowledge, no prior research exploits the increases in Head Start funding in the early 1990s to evaluate the impact of widespread changes in preschool and early preschool availability on the employment decisions of single mothers. We analyze

[3] There is also work documenting that access to early childhood schooling increases maternal labor supply in other countries (Carta & Rizzica, 2018; Gathmann & Sass, 2018).

multiple decades, and find consistent results during both times of rapidly changing maternal labor supply and times of more stable maternal labor supply.

Our analysis of single mothers in particular provides information about responses in a segment of the population often targeted in welfare reform due to their high risk for poverty and disadvantage (Kimmel, 1995). Although promoting self-sufficiency of low-income mothers remains a policy priority, single mothers report difficulty securing high quality childcare and balancing work with family (Council of Economic Advisors, 2015). Past research documents associations between family structure and income mobility of children, educational outcomes of children, and the probability of children's family structure as adults (Musick & Mare, 2007). Given the interdependence of family structure and poverty, our focus on single mothers makes a valuable contribution to welfare policy conversations about the role of childcare subsidies to encourage intergenerational mobility and the welfare-to-work transition of these mothers. This research therefore positions Head Start as not exclusively addressing educational outcomes of children, but as a program simultaneously operating in the anti-poverty space.

2. Head Start and Its Potential Impacts on Maternal Labor Supply

Head Start is a federally funded preschool education program serving economically disadvantaged children across the United States. The program aims to increase school readiness, health, and social development for low-income children in an effort to reduce persistent educational attainment gaps between these children and their more advantaged peers (Gibbs, Ludwig, & Miller, 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) support, their family receives Supplemental Security Income (SSI), they

are homeless, or if they are a foster child. Head Start initially began in 1965 as part of President Lyndon B. Johnson's War on Poverty, and although it began as a small summer program, it quickly grew to be the largest early childhood education program for low-income children in the United States.

In theory, public provision of preschool provides a subsidy for childcare. Because women often provide primary care for their children, 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 childcare provider) and time at home caring for her child herself (Fitzpatrick 2010). In this framework, a childcare subsidy reduces some of the costs associated with employment, leading to higher net wages, potentially inducing some mothers to substitute away from home time and enter the labor market after the childcare subsidy is introduced. Additionally, the subsidy potentially increases labor supply on the intensive margin as well. Although income effects put downward pressure on labor supply, substitution effects are likely to dominate for low-income mothers, and we expect mothers to increase hours on the intensive margin as well. Thus, offering Head Start to children likely affects mothers by changing employment feasibility which could affect her overall labor force attachment. On the one hand this might increase household income experienced during childhood, while on the other it might reduce the amount of quality parent-child interactions. This could result in long-lasting impacts on the family.

As explained by Kose (2018), prior to 1990, the Federal Government apportioned Head Start funds to states based on need as determined by the number of families receiving welfare benefits, the number of unemployed adults, and the number of children living below the poverty line. Local administrators who could provide at least 20% of their own funding applied to states

for Head Start fund 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. 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. As a result, substantial geographic variation in funding per eligible child existed prior to the 1990 expansion in Head Start (Currie and Neidell, 2007).

In 1990, Congress pass the Head Start Expansion and Quality Improvement Act, thereby providing substantially more funding for teacher salaries, teacher training, facilities, and family services (see Figure 2). The expansion sought to improve the quality of the educational programming as well as increase the number of children enrolled. Program administrators, allocated additional funds to states based on the previous proportion of funding they received, resulting in geographic variation in funding increases. This variation in funding changes provides a natural experiment with which to study the program effects on maternal labor supply. In 1994, the early preschool program expanded in scope with the introduction of the Early Head Start program, which targeted children younger than three in order to better address the comprehensive needs of low-income children. However, Early Head Start remained small, serving less than three percent of eligible children and accounting for only eight percent of Head Start funding by 2009 (Hoffman, 2010).

3. Data

For our analysis we rely on two main data sources. The first is the annual Consolidated Federal Funds Report (CFFR) from 1983 to 2010 (Consolidated Federal Funds Report: County Areas, 2011). These reports provide detailed municipality level information on federally funded

items, including payments for Head Start.[4] Funds are then aggregated up to the metropolitan area, as this is the level of geography available in the CPS. We then aggregate up the Surveillance, Epidemiology, and End Results Program (SEER) annual county- level population estimates by age to estimate the annual metropolitan population of three- and four-year-olds (National Cancer Institute, 2017). Using this measure we are able to 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, except in 2000, when the government began to advance funds from the prior year's appropriation (1.4 billion dollars in 2000), and thus do not appear in the CFFR. As demonstrated in Figure 3, we measure dramatic increases in funding following program expansion, with average funding increasing by more than 300 percent. Further, we measure dispersion in Head Start funding per child. Figure 4 demonstrates that average funding increases were accompanied by an increased reach of the Head Start program as significantly more counties received funding over time. However, metropolitan area level funding amounts remained proportional to pre-expansion levels, as demonstrated in Figure 5.

We combine the CFFR data with the CPS ASEC from 1983 through 2000 (Flood, King, Rodgers, Ruggles, & Warren, 2018). We restrict our analysis to variation before the year 2000, as we are explicitly interested in understanding what happened during the 1990s. From the CPS, we collect information for all single mothers with young children, defined as a women with children 5 and under in the home. We use the household roster to determine if the mother has children of a given age in the home. In the ASEC supplement, participants report on employment during the previous calendar year. For this reason, we are interested in mothers who currently have a four- or

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

five-year-old in the home, as the child would have likely been three or four in the previous calendar year and age-eligible for Head Start. 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 as well, 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.[5]

Our baseline sample includes 21,672 single mothers with a child under the age of 5 who was observed between 1984 and 2000 in the CPS ASEC. Women living outside of metropolitan areas are not includes, because they can only be assigned the funding level in the remainder of the state, which might introduce measurement error. 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 average increases in Head Start funding. Between 1990 and 1999, metropolitan area-level Head Start funding per year per age-eligible child increased by $363 (2017$) in below average increase areas, and by $675 in above average increase areas. Single mothers with children under five had similar characteristics, regardless of age eligibility for Head Start. Single mothers with age-eligible children were slightly more likely to be employed, more likely to be employed full-year, worked more weeks and had higher wage income than single mothers with younger children. They were also slightly more educated, younger, and had more children on average. However, these differences between single mothers with and without age-eligible children are similar in high and low funding metropolitan areas.

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

4. Empirical Approach

Enrollment. Using Head Start funding per child to proxy for access to Head Start enrollment implicitly assumes that additional Head Start funding increases enrollment. To test this assumption, we first estimate the relationship between Head Start funding and school enrollment using two different data sources. The CPS October supplement includes measures of current school enrollment for children three and older. Using the children observed during this supplement, we estimate the impact of metropolitan area level Head Start funding on the probability three- and four-year-olds are enrolled in pre-kindergarten as follows

$$In\ PreK_{it} = \beta_1 HS\ funding\ per\ child_{mt-1} + X_{it}'\Gamma + \phi_m + \delta_t + \varepsilon_{it} \qquad (1)$$

$In\ PreK_{it}$ is the binary outcome of being enrolled in pre-kindergarten, estimated over the sample of three- and four-year-olds. The coefficient β_1 captures the percentage point change in pre-kindergarten enrollment when there is a $500 increase in Head Start funding per child at the metropolitan area level (m).[6] Unlike later regressions, this specification does not difference out trends using non age-eligible children in the same metropolitan area because (1) children under age three are not asked about schooling, and (2) very few children over four are enrolled in preschool.[7] Although pre-kindergarten enrollments in the CPS are general and not specific to Head Start, we observe increases in pre-kindergarten enrollments following expansions in Head Start funding.[8]

[6] We include a vector of individual level controls (age indicators, race, ethnicity, education), policy controls in t-1 (maximum EITC refund eligibility, availability of Temporary Assistance for Needy Families (TANF) waiver in the metropolitan area, maximum TANF benefit, presence of States Children's Health Insurance Program (SCHIP), and the real state and federal minimum wage), and state level demographic trends (race, marital status, and education percentiles). We also include a metropolitan area fixed effect, to compare children from the same area, and year fixed effects to control for year shocks and secular trends in preschool attendance. We weight observations by the individual probability weights provided in the CPS.

[7] Estimation similar to equation (3) comparing three- and four-year-olds' preschool enrollment to older children's preschool enrollment provide similar coefficients, but increases the precision.

[8] Although this seems like a setting that would lend itself well to an instrumental variables estimation capable of relating pre-kindergarten enrollment to maternal labor supply, this is potentially problematic for several reasons.

We also examine state-level annual Head Start enrollment between 1988 and 1999 from the Kids Count data center (Kids Count Data Center, 2018). Using the SEER population estimates, we construct the state-level Head Start enrollment rate among three- and four-year-olds and estimate the following equation:

$$HS\ rate_{st} = \beta_1\ HS\ funding\ per\ child_{st-1} + \phi_s + \delta_t + \varepsilon_{st} \quad (2)$$

In equation (2), Head Start funding per capita is measured at the state-level, so the coefficient β_1 represents the percentage point increase in the Head Start enrollment rate associated with a $500 increase in state-level Head Start funding per child. Using the state-level data we also explore the impact of Head Start expansion on enrollment of children younger than three, through Early Head Start.

Maternal Employment. Identification of the causal effect of preschool enrollment on maternal labor supply is difficult given likely connections between a mother's desire for her child to be educated and a mother's labor market options. To investigate whether preschool enrollment influences maternal labor supply, we focus on the potentially exogenous and heterogeneous expansion of the Head Start program. To investigate whether Head Start availability affects maternal labor supply, we exploit variation in per child Head Start funding across both geography and time. One concern with this generalized fixed effects approach is that it is unclear why certain municipalities saw increases in Head Start funding after the national expansion while others did not. When Head Start was expanded nationally in 1990, the additional funds were allocated to states based on the previous proportion of funding they received. If, for example, local

First, enrollment outcomes and employment outcomes are estimated using different samples, with slightly different specifications. Although the October CPS survey also includes monthly employment surveys, we would not be able to observe the first stage outcome for the control group because preschool enrollment is not reported for children younger than age three. Second, it is not clear the exclusion restriction holds, as Head Start funding might affect maternal labor supply through other channels, and finally, the first stage relationship estimated in the October supplement is fairly weak. For these reasons, we do not provide instrumental variable estimates and instead focus on estimating the reduced form impacts of changes in funding levels directly on maternal labor supply.

administrators were more likely to apply for and secure funding in areas where single mothers had a greater propensity to work, the estimated coefficients would be biased. Another identification concern arises if area-specific shocks coincide with the Head Start expansion. To account for potential policy endogeneity, we expand our analysis to a generalized triple difference approach using the age of a child as an additional source of identification. We argue that comparing single mothers with eligible children to single mothers with ineligible children (who are close in age) in the same metropolitan area produces causal estimates, because local changes experienced by mothers of young children likely had similar effects regardless of whether their children were born before or after the eligibility deadline.

To estimate the effect of per child Head Start funding on mother's employment in the previous year, we compare single mothers with age-eligible children to single mothers with non-eligible younger children in the same metropolitan area as follows:

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

The primary outcome of interest is the binary indicator for whether the woman was employed at all last year. The coefficient β_1 captures the effect of Head Start funding per child in the previous year on employment for single mothers who had a child that was age-eligible in the previous year, relative to women who did not have an eligible child. By including the metropolitan area fixed effect (ϕ_m), we compare mothers in the same metropolitan area. As such, any change in metropolitan-level Head Start funding that correlates with local trends in the employment of single mothers is controlled for and captured in β_2. The year fixed effect controls for national changes over time in both employment rates and Head Start funding. We include a vector individual level

controls (race, ethnicity, education), controls for policies in the previous year (maximum EITC refund eligibility, availability of Temporary Assistance for Needy Families (TANF) waiver in the state, maximum TANF benefit, presence of States Children's Health Insurance Program (SCHIP), and the real state and federal minimum wage), and state level demographic shares (race, marital status, and education percentiles).[9] In all regressions, observations are weighted by the individual probability weights provided in the ASEC. To account for potentially correlated errors among individuals in the same metropolitan area, we cluster standard errors at the metropolitan area level (Bertrand, Duflo, & Mullainathan, 2004).

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 non-eligible children if the Head Start expansion had not occurred and affected them. This 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.

5. Results

Impact on Enrollment. Estimation results using both the individual-level enrollment data from the October CPS and the state-level Head Start enrollment data are reported in Table 2. From the October CPS, a $500 increase in per child Head Start funding in a metropolitan area associates with a 3.7 percentage point increase in the probability of a three- or four-year-old being enrolled in pre-kindergarten. This represents an 8.6 percent increase off of a base of 43 percent enrolled in pre-kindergarten. Among children with a mother with a high school degree or less, a group more

[9] A special thanks to Kearney and Levine (2013) for providing data on state level policy and demographics.

likely to be Head Start eligible, the effect is an increase in enrollment of 4.8 percentage points, or approximately 15 percent. When looking at state-level Head Start enrollment, $500 of state-level per child Head Start funding increased enrollment among three- and four-year-olds by 4.9 percentage points, more than doubling Head Start enrollment on average. When analyzing Early Head Start enrollments, prior to 1995, the year Early Head Start began, the effect of Head Start funding on enrollments on children younger than three was small and insignificant. However, when including years following the initiation of Early Head Start, the coefficient increases to 0.21 percentage points and becomes statistically different from zero, this is an order of magnitude smaller than the enrollment impact for 3- and 4-year-olds and is not statistically different from the estimate in Column (5).

Both data sources suggest that increases in Head Start funding associate with expansions in enrollment, a finding that underscores the mechanism behind labor supply response, making estimates of the effect that Head Start funding had on maternal labor supply more meaningful. We now calculate the employment impacts for a given increase in preschool funding.

Impact on Maternal Employment. Given the observed increase in preschool enrollment, we next estimate the effects of Head Start funding on maternal employment decisions in Table 3. In column (1), we observe that a $500 increase in per child Head Start funding is associated with a 2.0 percentage point increase in the probability of being employed when considering all single mothers with children ages 5 and under between 1983 and 2000. From an average employment rate of 62 percent, this represents a 3.2 percent increase. The findings suggest that along the participation margin, Head Start funding induced increases in labor supply among single mothers. Also, given the enrollment impacts we can construct the Wald estimate, suggesting that 54 (.02/.037) percent of women who had an additional child enroll entered employment.

We measure Head Start's impacts on other labor market measures to better understand intensive margin changes such as work weeks and hours worked. However, because the data set is a repeated cross-section, we will not be able to fully separate the extensive and intensive margins. The increase in Head Start funding increased the full-time employment rate by an insignificant 1.5 percentage point and the part-time employment rate by an insignificant 0.5 percentage point. This is consistent with some of the increase in employment going to full-time employment and some going to part-time employment. However, given the cross-sectional nature of the data, 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 nine percent increase in annual weeks worked and an 8.3 percent increase in usual hours worked. If the entire two percentage point increase in employment were due to new entrants working full-time (40 hours), this would translate into a 3.6 percent increase in hours worked at the mean. The larger hours increase of 8.3 percent suggests there were intensive margin adjustments in weekly hours worked in addition to extensive margin entry.[10] We also estimate that wage earnings increased by 20 percent. These findings suggest that the Head Start expansion facilitated increased attachment to the labor market through intensive margin channels. Previous work evaluating the costs and benefits of Head Start have not considered the addition household income children receive when Head Start facilitates a return to work.

Event Study. As a robustness measure, we explore trends in single mothers' employment before and after the expansion in Head Start funding graphically. In particular we are interested in seeing how employment of single mothers of age-eligible children trend relative to single mothers

[10] Similarly, if all new entrants worked full year (52 weeks), this would only increase annual weeks worked by 4.3 percent at the mean, suggesting there is some intensive margin adjustment in annual weeks worked.

with younger children in metropolitan areas that experienced large and small increases in funding. To do this we estimate the following equation:

$$Ever\ Employed\ last\ yr._{it} =$$

$$\sum_{\tau=83,84/85}^{99/00} \beta_{\tau-1}(Child\ 3\ or\ 4\ last\ yr.)_{it} * (year = \tau) + \gamma(Child\ 3\ or\ 4\ last\ yr.)_{it} + X_{it}'\Gamma + \phi_m + \delta_t + \varepsilon_{it} \quad (4)$$

The outcome is once again any employment in the previous calendar year, but now the β_τ coefficients trace out the difference in employment over time between single mothers with age-eligible children and single mothers in the same metropolitan area without age-eligible children. For power, years are grouped into bins (1983, 1984-1985, 1986-1987 etc.) and the interaction with 1990 is excluded to make this the reference period.[11] The regression outlined in equation (4) does not solely capture changes in employment due to Head Start Funding. Other policies, such as the EITC and TANF, were also changing during the 1990s, and policy changes in welfare and taxation might have differentially affected mothers with age-eligible children. For example, Looney and Manoli (2013) show that much of the rise in employment among single mothers in the 1990s was concentrated among mothers with young children, but that mothers with young children were also more likely to have multiple children, thereby affecting the maximum earned income tax credit the woman was eligible to receive. To separately identify the effects of Head Start we need another source of variation, so we separate metropolitan areas by the percent change in per child Head Start funding between 1989 and 1999. We then separately estimate equation (4) for individuals in metropolitan areas in the bottom half of the increase distribution and in the half of the distribution (weighting metropolitan areas by the population of three- and four-year-olds). The bottom half of the distribution includes 172 metropolitan areas, where the increase in funding per child was less

[11] The figure is similar, but more imprecise if *Child 3 or 4 in t-1* is interacted with each year individually.

than 209% between 1989 and 1999, with an average increase of 142%. The top half includes 142 metropolitan areas, where the increase in funding per child was greater than 209% between 1989 and 1999, with an average increase of 291%. Panel A of Figure 6 plots the coefficients for both of these regressions. For metropolitan areas in the bottom half of the distribution, employment trends before Head Start expansion were flat and not significantly different from zero. This continued following the expansion of Head Start. For metropolitan areas in the top half of the distribution, areas that experienced relatively large increases in Head Start funding, employment trends before the expansion were not significantly different from zero, but visually there is a downward trend. If anything, single mothers with age-eligible children in metropolitan areas with large increases in Head Start funding were becoming less likely to be employed relative to other single mothers before Head Start expansion.

To account for potentially different trends by treatment status, we also estimate the equation for both samples but include linear trends for women with a 3- to 4-year-old child. These coefficients are plotted in Panel B of Figure 6. Prior to 1990 employment of single mothers with age-eligible children in high expansion areas is no longer trending down relative to single mothers with younger children. Under both specifications, the coefficients in high expansions areas begin to rise, following the Head Start program expansion in 1990, suggesting that single mothers with age-eligible children in highly funded areas were perhaps more likely to be employed. However, the gaps between single mothers in high funded versus low funded areas are not statistically significant (and especially imprecise when linear trends are included). This would suggest that increases in employment among single mothers with age-eligible children were largest in areas that saw the largest increases in Head Start funding.

Robustness. Appendix Table A1 demonstrates that estimates are robust to the inclusion of controls for other work related policies (such as the EITC, TANF benefits, and minimum wages), and the inclusion of mothers in non-metropolitan areas. The estimates are also robust to including metropolitan area by year fixed effects, essentially controlling for changes in metropolitan area trends. We find similar results when comparing single mothers with eligible children to all mothers with children under age 18, rather than using only mothers of young children as a comparison group. Estimates are not sensitive to adjusting the ASEC sample to include only one observation per person rather than treating the data as repeated cross sections using all observations.[12] Because Early Head Start began in 1995, the control group in column (1) is potentially contaminated as some children younger than three would be eligible for Head Start after 1995. When we restrict the sample to pre-Early Head Start years in column (6), the coefficient increases to 3.5 percentage points and is highly significant, suggesting this is not a concern. Finally, if we include linear trends for mothers with age-eligible children as in the figure we estimate a similar effect of 1.8 percentage points.

Heterogeneity. We next consider heterogeneous treatment effects in Table 4 by estimating equation (1) for various demographic groups. Consistent with less educated mothers being more likely to be eligible, $500 of Head Start funding per child has a larger effect of 3.6 percentage points, or 6.6 percent, for single mothers with a high school degree or less. As expected, the effects for mothers with any college education (who are likely not impacted by Head Start) are close to zero and insignificant. When looking by race and ethnicity, the effects are largest for minority single mothers (2.6 percentage points), with no significant effect for Non-Hispanic White single

[12] In theory, we could check for effects by linking mothers across CPS survey waves to create a two-year panel. In practice, the linking ID for the ASEC in IPUMS only goes back to 1989, which excludes most pre-period mothers. Additionally, about 9.4 percent of ASEC respondents were oversampled, preventing linking from one year to the next for these observations.

mothers. We find similar employment responses among single mothers even in households where grandmothers, potential care substitutes, resides. A single mother's labor supply response may differentially respond depending on whether she has younger children at home in addition to a Head Start eligible child. We hypothesized that mothers with age-eligible children who also had younger children would be less affected by the Head Start expansion. However, as seen in column (3) of Table 3, employment responses for mothers of age-eligible children and younger children were indistinguishable from employment responses of mothers with age-eligible children and no younger children. This suggests that the presence of younger children did not dampen the employment effects of Head Start for single mothers and stands in contrast to previous work finding differential effects prior to 1990 (Cascio, 2009; Gelbach, 2002).

We expect single mothers to have quite different employment behavior, considering differences in family settings, earning dynamics, and family resources (Blau & Takin 2007). Household structure and the mother's potential role as a primary or secondary earner differs by marital status. Existing work exploring the impact of safety net programs on single women often do not differentiate between previously married and never married mothers, but we find observational differences between these mothers. We find that single mothers are generally younger and less educated, with never married mothers even more negatively selected on characteristics predictive of labor market participation. To further understand heterogeneity of effects, we separate estimates by mother's marital history in Appendix Table A2. We find that among never married mothers, a $500 per child increase in Head Start funding resulted in an employment increase of 3.2 percentage points. On the other hand, we find no responses among previously married mothers (separated, divorced, or widowed), suggesting that overall responses

among single mothers were concentrated on never married mothers. When we expand our analysis to married mothers, there is a smaller 1.5 percentage point increase in annual level employment.

6. Generalizability to Other Settings and Time Periods

6.1 1960s Head Start Initial Rollout

We now evaluate a similar, but earlier setting for understanding employment responses to Head Start. The introduction of the Head Start program commenced with a summer program in 1965 and quickly expanded to a full-year program as part of President Lyndon B. Johnson's War on Poverty (Vinovskis 2005). Most Head Start centers in the 1960s provided half-day care only and relied on parent involvement in volunteering or working. Up to 70% of centers directly employed parents, and the employment opportunities typically overlapped with preschool hours. Accompanying the new funding for Head Start was dramatic growth in preschool enrollment, with over half a million children served during its first year (Zigler & Muenchow 1992). The rollout was rapid, with area-level funding levels stabilizing by 1970 (Barr & Gibbs 2018). The geographic variation in the timing of funding for a metropolitan area provides a natural experiment that we exploit in a generalized fixed effects framework. A concern when comparing estimates across datasets is that employment measures and sampled populations differ slightly. Because this analysis is similar to the approach used to evaluate the 1990s expansion, we provide information about nuanced differences in the analysis compared to the 1990s evaluation rather than provide detailed explanation for this section.

Dataset and Empirical Approach. This dataset covers earlier years when far fewer mothers with young children worked and the share of children in single mother households was much lower. Additionally, the program had broader scope in the late 1960s compared to the scope of the program during the 1990s and at the time of the HSIS. Although Head Start programming in the late 1960s included educational content, goals related to cognitive gains did not dominate

programming. The program in its infancy took a multi-faceted approach to school readiness by providing health screenings, vaccinations, dental screenings, nutritional services, linkages to community service providers, and parent education in addition to educational content (Vinovskis 2005).[13] We measure Head Start exposure using metropolitan area by year data from the National Archives and Records Administration from 1964-1970 (Community Services Administration 1981).[14] We aggregate grant dollars to the MSA level. By exploiting timing and geographic variation in spending increases in a generalized fixed effects framework, we compare women with a child between ages three to five to women with children under age three to measure whether and to what extent funding amounts per child affect female labor supply. As Barr and Gibbs (2018) argue, the timing of funding in a metropolitan area over the first five years of the program was not strongly correlated with pre-existing area-level trends.

We link increases in Head Start expenditure to maternal employment using the CPS March ASEC between 1964 and 1970 to compare mothers with age-eligible children to other mothers with dependent children within the same metropolitan area (Flood et al.2018; Mare & Winship 2001). Prior to the CPS redesign in 1968, respondents reported whether or not they had a child between ages 0-2 and 3-5, preventing us from constructing more precise age-eligibility treatment windows. Our sample includes 1,116 single mothers observed in the CPS ASEC who have at least one child under five living in the household. The underlying population served by Head Start differed in the 1960s compared to the 1990s and later. Many fewer adults attended college and many fewer mothers had never been married in the 1960s. Appendix Table A3 reports demographic differences between single mothers with age-eligible children and those with

[13] The effects of the early program on parents may have operated through more than simply a childcare channel during the program rollout, and we do not separate effects.
[14] Head Start expenditure data were primarily provided by Bailey & Goodman-Bacon (2015), with the authors adding expenditures found in CSA records for missing counties.

children under the age threshold. Our main outcome of interest is the extensive margin measure for whether a mother was in the labor force during the previous year.

We focus on Head Start expenditures per age-eligible child for comparability to the 1990s analysis. We include a vector of individual controls (race, education), controls for policies in the previous year (state-level average payments from the Aid to Families with Dependent Children program, metro area presence of federally funded community health centers, metro area per capita federal grants for health related Community Action Programs, the presence of a state minimum wage that exceed the federal level, and the real federal minimum wage), and state level demographic trends (race, fraction of households with a married couple, and education level).[15] Prior to 1968, the CPS only identified 15 metropolitan areas. Furthermore, geographic boundaries of several of these metro areas changed in 1968, so we restrict the analysis to 11 metropolitan areas with unchanging geographies through the sample years. These data limitations constrain our ability to precisely detect employment effects.

Results. We find evidence that Head Start may have improved labor market outcomes for single mothers with age-eligible children. In Table 5, column (1), we estimate that a $500 increase (in 2017 dollars) in spending per eligible child increased the probability that a single mother was in the labor force during the previous year by 32 percentage points. The average increase in Head Start spending per child over the rollout period was $161 (in 2017 dollars), suggesting that the effect was approximately 10 percentage points for the average single mother. The findings suggest that along the participation margin, Head Start funding induced some single mothers to enter the labor market. We observe increases in employment during the previous year as well, as shown in

[15] AFDC information drawn from Wexler & Engel (1999); CHC and CAP data provided by Bailey & Goodman-Bacon (2015); State min. wage info found in Sutch (2010); State-level demographics obtained from the United State Census library.

column (2) of Table 5.[16] We find that Head Start funding increased the probability of working at least one or two quarters, with effects weakening as quarters increase. We do not detect significant effects when looking at wage income changes, as seen in column (6). Barr & Gibbs (2018) suggest that while the timing of when Head Start started in a metropolitan area, the level of funding may be endogenous. We therefore estimate effects using the availability of Head Start in a metropolitan area rather than funding levels, and we find similar labor supply responses, as seen in Appendix Table A4. When evaluating responses among other mothers, we find less evidence of effects, as shown in Appendix Table A5. We observe a positive insignificant labor force effect of 1.5 percentage points among all mothers, and we detect no effects among mothers with lower education levels or no younger children. Despite data limitations, our evaluation provides suggestive evidence that single mothers in particular increased labor force participation when given access to Head Start during the late 1960s.

6.2. Head Start Impact Study

As stated earlier, in order for this empirical approach to produce causal effects, it must be the case that single mothers with age-eligible children would have behaved like single mothers with slightly younger children if the Head Start funding expansions had not occurred. Through the graphical analysis and various robustness checks, this identifying assumption appears to hold. To further test the relationship between Head Start access and maternal labor supply, we will 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 by lottery access

[16] The variable "Employed" shows evidence of miscoding in the original data. Observations coded as working between 50-52 weeks during the previous year are also reported as not in the labor force during the previous year. For this variable, we report uncorrected data, and results are larger and more significant when alternatively coding these observations as not employed during the previous year.

to Head Start. 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 also conducted each year, soliciting information about family arrangements and broad measures of maternal labor force participation. Using this experimental variation we validate the patterns observed from the 1990s and 1960s analysis. The HSIS also allows us to explore heterogeneity by family structure characteristics (such as presence of younger children and marital status) and program generosity (availability of full-day programing).

Dataset and Empirical Approach. This section briefly introduces our data along with information on key variables, and Appendix B 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. The sample is weighted to be nationally representative of the Head Start population.[17] As seen in Table 6, the treatment and control groups are similar across baseline characteristics in Fall 2002, consistent with randomization. However, the experimentally induced access to Head Start does significantly change child care arrangements. Treated children are 74 percentage points more likely to attend Head Start, and 55 percentage points more likely to be enrolled in center-based care.[18] Access to Head Start shifts some children away from home-based daycares (8 percentage points) but mostly from staying at home (47 percentage points). This would suggest that for many,

[17] As previous work has noted, it was easier to contact families given access to Head Start so many of these "treated" units were interviewed earlier during each wave and were less likely to attrite. We follow the method used by Bitler, Domina, and Hoynes (2018) and use characteristics from the initial wave, month of interview, Head Start Center fixed effects, and non-response indicators to predict treatment status using a logit model and construct wave-specific inverse probability weights which are used throughout.

[18] 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 (Gelbach & Isen, 2013) while others enrolled at the center of application (Feller et al., 2016). It is unclear what share followed each path.

access to Head Start moves childcare 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 our maternal labor supply outcomes of interest on an indicator for randomized treatment status, and we can estimate treatment on the treated effects using two stage least squares where we use treatment status to instrument for Head Start enrollment.[19] This will allow us to determine how access and enrollment in Head Start affects maternal labor supply. From the parent interviews we identify if the mother is participating in the labor force, if she is currently employed, and if she is employed full-time (weekly hours ≥35) or part-time. It should be noted that these measures are different than those in the CPS, as it only captures current employment, not annual employment. We first estimate impact for the full sample, then see if the effects are larger when the Head Start center offers full day services or if there are not younger children in the household.[20]

Results. In Table 7, we report the impacts of Head Start on maternal labor supply (treatment on the treated effects).[21] In the full sample we see a marginally significant 4.4 percentage point (14 percent) increase in the probability of being employed full-time. Although the experiment is too underpowered to detect, it appears as though approximately half of this increase is due to shifting into employment at the extensive margin while the other half is due to an intensive margin increase from part-time to full-time employment. We next explore how program generosity affects labor supply. If the Head Start center the family applied to offers full-

[19] 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 B for details.
[20] Whether or not the center offers full day services is determined from the center-based care director's interview. For all children attending a childcare 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 if the director reported full-day programing available for 50 percent or more of the enrolled students.
[21] The reduced form intent to treat effects are provided in Appendix Table A6.

day programming, Head Start enrollment increases full-time employment by 7.7 percentage points (24 percent). Conversely, there is no impact of Head Start from centers that do not offer full-day programming. This would suggest that less generous childcare arrangements did not change maternal labor supply. When splitting by the presence of younger children there are no stark statistical differences. Mothers with children under three are marginally less likely to work part-time, while mothers without children under three are marginally more likely to be in the labor force.

As household structure and the mother's potential role as a primary or secondary earner differs by marital status, we estimate the impact of Head Start on labor supply for never married, separated/divorced/widowed mothers, and married mothers in Table 8.[22] We see small, insignificant effects among married mothers whose child attends Head Start, moderate, insignificant negative effects among previously married mothers, and large, statistically significant effects among never married mothers when their child enrolls in Head Start. Never married mothers are 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. This pattern matches that observed from the 1990s expansion.

In Tables 9 and 10 we also explore heterogeneity in the availability of full-day services and presence of younger children when conditioning on marital status. We find that all of the effects from Table 7 are concentrated among never married mothers. Never married mothers who applied to centers that offered full-day services are 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 child enrolls in Head Start. There is no impact for mothers in centers that do not

[22] Marital status is measured in Fall 2002 at the beginning of the experiment and held fixed throughout.

offer full day programming. Similarly, never married mothers with younger children do not become more likely to be employed when their child enrolls in Head Start. However, never married mothers without younger children are more likely to be in the labor force (14.6 percentage points) and employed full-time (13.9 percentage points) when their child enrolls in Head Start.

The HSIS sample is relatively small, and many of the coefficients are estimated imprecisely with large coefficients, suggesting the experiment might be underpowered to fully detect employment effects. However, we do find evidence that Head Start provides an implicit childcare subsidy by moving children from home-based care to center-based care. As such we see access to Head Start enrollment increasing employment (and full-time employment) among never married mothers without younger children and who applied to Head Start centers that offer more generous full-day programming.

Persistence. From the Head Start Impact Study, we explore how experimentally induced Head Start enrollment affects maternal labor supply for up to 5 years after the preschool treatment. Using the same strategy as before, we look at how Head Start enrollment affects labor force participation and employment up through third grade. We suspect groups with the strongest initial treatment response would be most likely to demonstrate persistent effects, and 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 A2), employment (Appendix Figure A3), or full-time employment (Appendix Figure A4). 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 starting in 2006 (once all children have reached first grade). These effects are sometimes significant. 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. More work or a larger sample is needed to make more conclusive statements about the persistence of these effects.

7. 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 participation in the labor market by 1.6 percentage points and increasing their usual hours worked by 8.3 percent. Effects were strongest for less educated mothers, minorities, and never married mothers. This finding suggests that childcare 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 extend to the late 1960s Head Start rollout and Head Start Impact Study, providing compelling evidence that our findings 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 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. Our finding 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.

Although we recognize that composition effects play into earnings changes, we briefly explore the costs and benefits of Head Start in terms of the additional earnings to single mothers observed in the 1990s setting. The increased household income earned as a consequence of the Head Start expansion suggests that the Head Start program not only provided educational services to children in low-income families, but had spillover effects in leading to improved financial security of single mothers with young children. From a policy perspective, examining the increased salary to single mothers in light of the costs of the Head Start expansion provides additional information on the program's cost efficiency. For a $500 increase in Head Start funding per eligible child, the average salary of single mothers with an eligible child increased contemporaneously by 20 percent, translating into an average salary increase of $2,347 (2017$). 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. This would suggest that income for single mothers increased immediately by $0.87 for each dollar that was spent on the program. The added income is a contemporaneous measure and does not including the value of potential increases in the future or potential reductions in parent-child time. Additionally, the estimate does not make adjustments for potential decreases in welfare transfers to the households, further contributing to the benefits of the program. Although we focus on single mothers, a segment of the population of interest to policy makers, these benefits were only part of broader social benefits reaped by the program's expansion. In addition to the benefits we measure for single mothers, many evaluation studies of early childhood program have examined how a participating child's future earnings compare with the cost of providing early childhood programs. These studies imply that there are substantial benefits for each child, ranging from $1.60 to $5.90 for every $1 spent (Bartik, Gormley, & Adelstein, 2012; Cascio & Schanzenbach, 2013; Duncan, Ludwig, & Magnuson, 2010; Heckman et al., 2010; Ludwig & Miller, 2007). Duncan and Magnuson's (2013) meta-analysis of Head Start in particular implies a benefit-cost ratio to a child of over $2 for every $1 spent on Head Start. Additionally, girls' participation in Head Start has measurable positive intergenerational transmission effects on their children in the form of improved educational outcomes, reduced teen pregnancy, and less participation in crime (Barr & Gibbs, 2018). Social benefits in terms of reduced transfer payments and remedial education expenditures are additional areas of potential benefit of Head Start expansion, as is reduced involvement in criminal activities (Heckman et al., 2010), and we hope to see research in the future measuring these effects. Our result imply that

providing access to quality educational opportunities to young children not only affects children's human capital accumulation but is also effective in improving employment among single mothers. These findings suggest that Head Start plays an important role in the anti-poverty space as a part of the portfolio of government means-tested programs.

Our results suggest only little evidence of persistent effects for at least five year; however, more work is needed to more precisely understand the long-run influence of subsidized childcare on maternal labor supply over a longer time horizon. Our focus on single mothers in urban areas holds importance alone, and we remain cautious in generalizing our findings to other demographic groups. Married and single mothers display measurable differences in labor market attachment, and our research leaves room for future work to explore the effects of subsidized childcare in other family structures and locations. Overall, the access to Head Start explains a small but non-negligible increase in employment rates among single mothers with young children, and is another potential mechanism to examine when considering the short-run impacts, middle-run fade-out, and long run resurgence of impacts of Head Start on children's outcomes.

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Tables and Figures

Table 1. Summary Statistics for Single Mothers with Any Children Under Five, 1984-2000

	Below Average Increase in Funding from 1989 to 1999		Above Average Increase in Funding from 1989 to 1999	
	Had 3-4 Year old Last Year	No 3-4 Year old Last Year	Had 3-4 Year old Last Year	No 3-4 Year old Last Year
Ave. Increase in Head Start per Child	363		675	
Employed Last Year	0.67	0.64	0.60	0.56
Employed Full-Year Last Year	0.39	0.30	0.35	0.25
Employed Part-Year Last Year	0.28	0.35	0.25	0.31
Weeks Worked Last Year	27.59	24.26	24.69	20.82
Wage Income (2017$)	14454.57	11526.93	12260.03	9428.19
High School or Less	0.68	0.71	0.71	0.74
Some College	0.25	0.22	0.23	0.22
College Graduate	0.07	0.06	0.06	0.05
Non-Hispanic White	0.44	0.43	0.37	0.35
Non-Hispanic Black	0.42	0.42	0.40	0.38
Non-Hispanic Other	0.02	0.02	0.02	0.02
Hispanic	0.11	0.12	0.21	0.24
Age	29.03	25.69	29.31	26.05
Number of Children	2.24	1.59	2.28	1.68
Age of Youngest Child	3.47	1.33	3.51	1.35
Observations	4,733	6,482	4,258	6,199

Notes: CPS ASEC 1983-2000. Sample means are weigted, using the individual level ASEC weights.

Table 2. Impact of 1990s Head Start Expansions Funding on Pre-school Enrollment

	In Pre-Kindergarten		State Level Head Start Enrollment Rate		
	All Education	Mother HS or Less	Enrollment Rate 3-4	Enrollment Rate 0-2	Enrollment Rate 0-2
	(1)	(2)	(3)	(4)	(5)
Head Start Funding per Child (3-4 yr.)$_{t-1}$	0.037*	0.048*	0.049***	0.0021***	0.0016
	(0.019)	(0.026)	(0.008)	(0.0007)	(0.0010)
Years of Data	1983-1999	1983-1999	1988-1999	1988-1999	1988-1994
Dependent Mean	0.43	0.32	0.081	0.002	0.001
Observations	31,360	15,133	539	539	294

Notes: Data for columns (1)-(2) from the CPS October education supplement 1983-2000 repeated cross sections. Sample restricted to 3-and 4-year-olds in the October Supplment. Data for columns (3)-(5) from Kids Count Data Center. The level of observation is the state by year level Head Start enrollment from 1988-1999. The dependent variable "In Pre-Kindergarten" indicates if the child is currently enrolled in Pre-Kindergarten. In columns (1)–(2) 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 childs health insurnace program (SCHIP) in place. These regressions are weighted using the individual monthly CPS weights. In Columns (3)-(5) 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. Standard errors are corrected for clustering at the MSA (for columns (1)-(2)) or state level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Impact of MSA-level Head Start Funding on Labor Market Outcomes of Single Mothers with children under Five in 1990s

				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$_{t-1}$	0.020**	0.015	0.005	0.090**	0.083**	0.199*
*Have Child 3-4 in t-1	(0.008)	(0.011)	(0.007)	(0.037)	(0.037)	(0.102)
Head Start Funding per Child$_{t-1}$	0.013	0.017	-0.005	0.039	0.066	0.001
	(0.024)	(0.023)	(0.014)	(0.112)	(0.102)	(0.256)
Have Child 3-4 in t-1	0.015	0.049***	-0.034***	0.129**	0.087*	0.260**
	(0.012)	(0.015)	(0.010)	(0.050)	(0.051)	(0.125)
Dependent Mean	0.62	0.45	0.18	24.2	22.2	11,793
Observations	21,672	21,672	21,672	21,672	21,672	21,672

Notes: Data from the CPS ASEC 1983-2000 repeated cross sections. Sample restricted to single women with a child 5 or younger. 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, whether the state has a childs health insurnace 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Heterogeneity in Impact of MSA-level Head Start Funding on Labor Market Outcomes of Single Mothers in 1990s

	Outcome: Any Employment in t-1							
Sample:	All	HS or Less	Any College	Non-Hispanic White	Non-White and Hispanics	Grandmother in Home	No Grandmother in Home	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Head Start Funding per Child$_{t-1}$	0.020**	0.036***	-0.006	0.011	0.026***	0.031***	0.009	0.019*
*Child 3-4 in t-1	(0.008)	(0.012)	(0.010)	(0.018)	(0.007)	(0.010)	(0.009)	(0.011)
Head Start Funding per Child$_{t-1}$	0.013	-0.003	0.028	0.024	0.003	0.017	0.014	0.009
	(0.024)	(0.030)	(0.026)	(0.037)	(0.028)	(0.036)	(0.026)	(0.024)
Child 3-4 in t-1	0.015	0.001	0.042**	0.042**	-0.003	0.041*	0.010	0.090***
	(0.012)	(0.015)	(0.016)	(0.018)	(0.015)	(0.025)	(0.011)	(0.018)
Head Start Funding$_{t-1}$								
*Child 3-4 in t-1								0.014
*Youngest 0-2 in t-1								(0.018)
Head Start Funding$_{t-1}$								0.009
*Youngest 0-2 in t-1								(0.011)
Child 3-4 in t-1								-0.226***
*Youngest 0-2 in t-1								(0.027)
Youngest 0-2 in t-1								0.044***
								(0.017)
Ave. Employment Rate	0.62	0.55	0.81	0.72	0.55	0.58	0.63	0.62
Observations	21,672	15,753	5,905	8,222	13,431	5,096	16,566	21,672

Notes: Data from the CPS ASEC 1983-2000 repeated cross sections. Sample restricted to women with a child 5 or younger. 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, whether the state has a childs health insurnace 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. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Impact of MSA-level Head Start Funding on Labor Market Outcomes of Single Mothers with children 5 or under in the 1960s

	In Labor Force (1)	Employed (2)	In LF >1quarter (3)	In LF >2quarter (4)	In LF >3quarter (5)	Inverse Hyperbolic Sine of Wage Income (6)
Head Start Funding per Child$_{t-1}$	0.319**	0.241*	0.327**	0.132**	0.035	-0.936
*Have Child 3-5	(0.136)	(0.128)	(0.125)	(0.057)	(0.043)	(1.564)
Head Start Funding per Child$_{t-1}$	-0.127**	-0.062	-0.141***	-0.061**	-0.019	0.566
	(0.040)	(0.045)	(0.038)	(0.027)	(0.014)	(0.383)
Have Child 3-5	-0.213	-0.304	-0.257*	-0.125	-0.023	-0.267
	(0.157)	(0.182)	(0.133)	(0.089)	(0.045)	(1.264)
Dependent Mean	0.278	0.478	0.198	0.137	0.042	11,863
Observations	1,116	1,116	1,116	1,116	1,116	1,116

Notes: Data from the CPS ASEC 1964-1970 repeated cross sections. Sample restricted to single women with a child 5 or younger in an MSA which does not change geography between 1964 and 1970. 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 payments from the Aid to Families with Dependent Children program, presence of federally funded community health centers, presence of Community Action Programs, state minimum wage, and federal minimum wage. The outcomes in column (6) are the inverse hyperbolic sine of the value, to include zeroes.. The variable "Employed" shows evidence of miscoding in the original data where observations coded as working between 50-52 weeks during the prior year are also reported as not in the labor force. For column (2) only, we report uncorrected data here, and results are larger and significant when alternatively coding these observations as not employed. All regressions are weighted using the household CPS ASEC weights. Standard errors are corrected for clustering at the MSA level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Childcare 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. Impact of Head Start on Maternal Labor Supply (TOT)

	In Labor Force	Employed	Full-time	Part-time
	(1)	(2)	(3)	(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. Impacts of Head Start on Maternal Labor Supply by Marital Status

	In Labor Force (1)	Employed (2)	Full-time (3)	Part-time (4)
Head Start*Married	-0.000	0.003	0.029	-0.026
	(0.040)	(0.041)	(0.035)	(0.031)
Head Start*Sep./Divorced/Widowed	-0.001	-0.071	-0.085	0.013
	(0.077)	(0.082)	(0.078)	(0.057)
Head Start*Never Married	0.103**	0.077*	0.115**	-0.038
	(0.045)	(0.045)	(0.047)	(0.034)
Control Mean	0.58	0.49	0.31	0.18
Number of Centers	334	334	334	334
Observations	3,117	3,117	3,117	3,117

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". 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. Impacts of Head Start on Maternal Labor Supply by Marital Status and Head Start Center Program Hours

	In Labor Force	Employed	Full-time	Part-time
	(1)	(2)	(3)	(4)
	HS Center Offers Full Day			
Head Start*Married	0.005	0.014	0.041	-0.027
	(0.053)	(0.053)	(0.049)	(0.040)
Head Start*Sep./Divorced/Widowed	-0.076	-0.139	-0.097	-0.041
	(0.094)	(0.104)	(0.108)	(0.090)
Head Start*Never Married	0.172***	0.140**	0.174***	-0.034
	(0.059)	(0.057)	(0.062)	(0.044)
Control Mean	0.59	0.49	0.32	0.17
Number of Centers	198	198	198	198
Observations	1,829	1,829	1,829	1,829
	HS Center Does Not Offer Full Day			
Head Start*Married	-0.005	-0.018	0.019	-0.037
	(0.060)	(0.064)	(0.048)	(0.053)
Head Start*Sep./Divorced/Widowed	0.081	0.003	-0.093	0.096
	(0.133)	(0.137)	(0.121)	(0.070)
Head Start*Never Married	-0.048	-0.055	0.003	-0.058
	(0.068)	(0.082)	(0.084)	(0.058)
Control Mean	0.56	0.48	0.30	0.19
Number of Centers	113	113	113	113
Observations	1,128	1,128	1,128	1,128

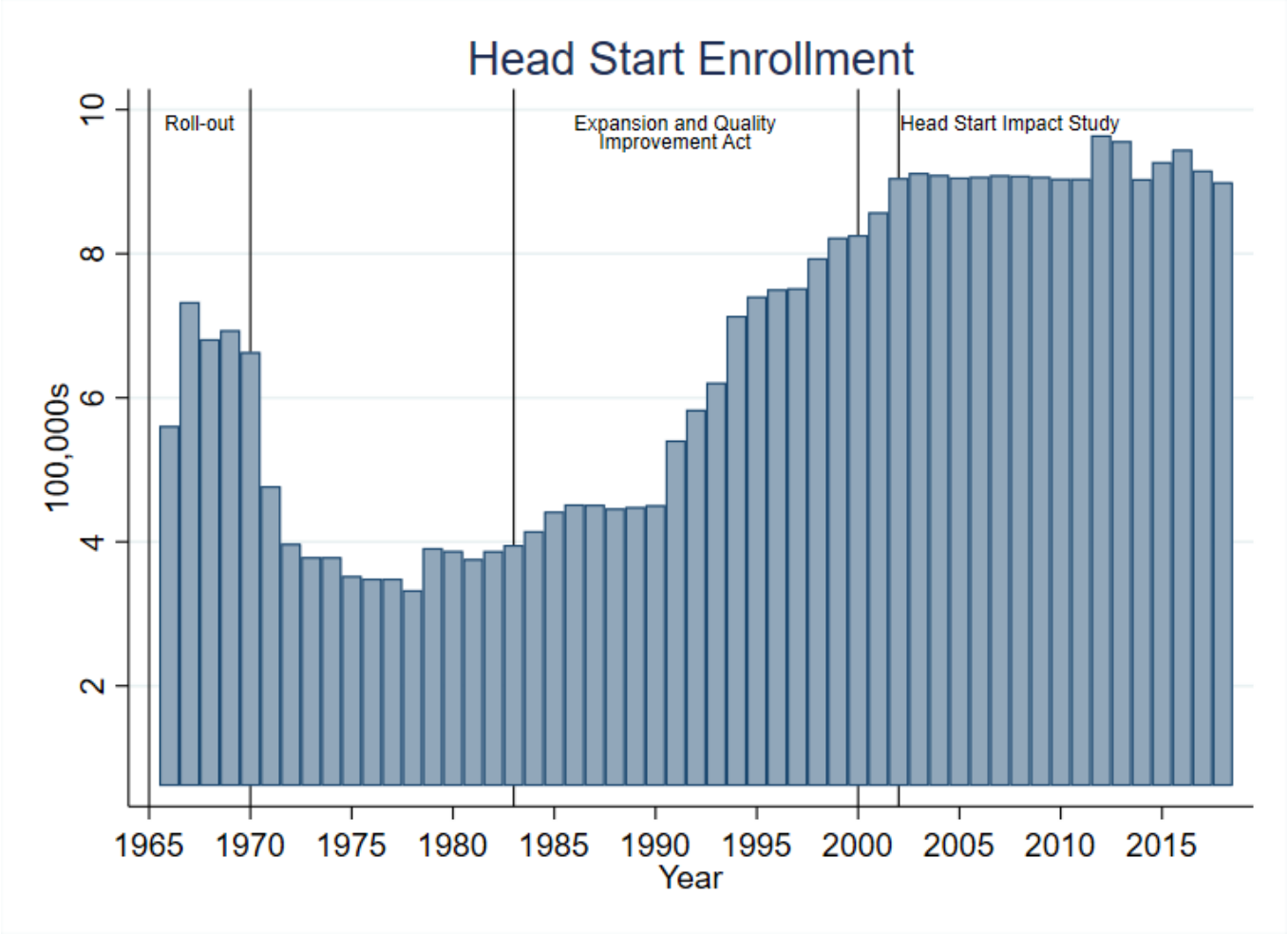
Notes: Sample restricted to households that participated in the Spring 2003 Parent Interview with a mother in the household. In the Labor Force is measured as employed full-time, part-time, looking for work, laid off from work, or in the military. Employed is either full- or part-time employed. Full-time 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 childcare, 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. 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 10. Impacts of Head Start on Maternal Labor Supply by Marital Status and Presence of Younger Children

	In Labor Force	Employed	Full-time	Part-time
	(1)	(2)	(3)	(4)
		Child Under 3		
Head Start*Married	-0.056	-0.092	0.008	-0.100**
	(0.068)	(0.072)	(0.052)	(0.050)
Head Start*Sep./Divorced/Widowed	-0.120	-0.164	-0.098	-0.065
	(0.130)	(0.140)	(0.133)	(0.107)
Head Start*Never Married	0.046	0.063	0.086	-0.023
	(0.078)	(0.075)	(0.070)	(0.053)
Control Mean	0.53	0.44	0.26	0.18
Number of Centers	286	286	286	286
Observations	1,181	1,181	1,181	1,181
		No Child Under 3		
Head Start*Married	0.019	0.047	0.032	0.015
	(0.049)	(0.046)	(0.046)	(0.038)
Head Start*Sep./Divorced/Widowed	0.031	-0.049	-0.112	0.063
	(0.086)	(0.095)	(0.095)	(0.065)
Head Start*Never Married	0.146**	0.095	0.139**	-0.044
	(0.058)	(0.059)	(0.061)	(0.045)
Control Mean	0.61	0.52	0.34	0.17
Number of Centers	321	321	321	321
Observations	1,936	1,936	1,936	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". The presence of younger children was determined by examining the household roster to determine if any children under 3 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.

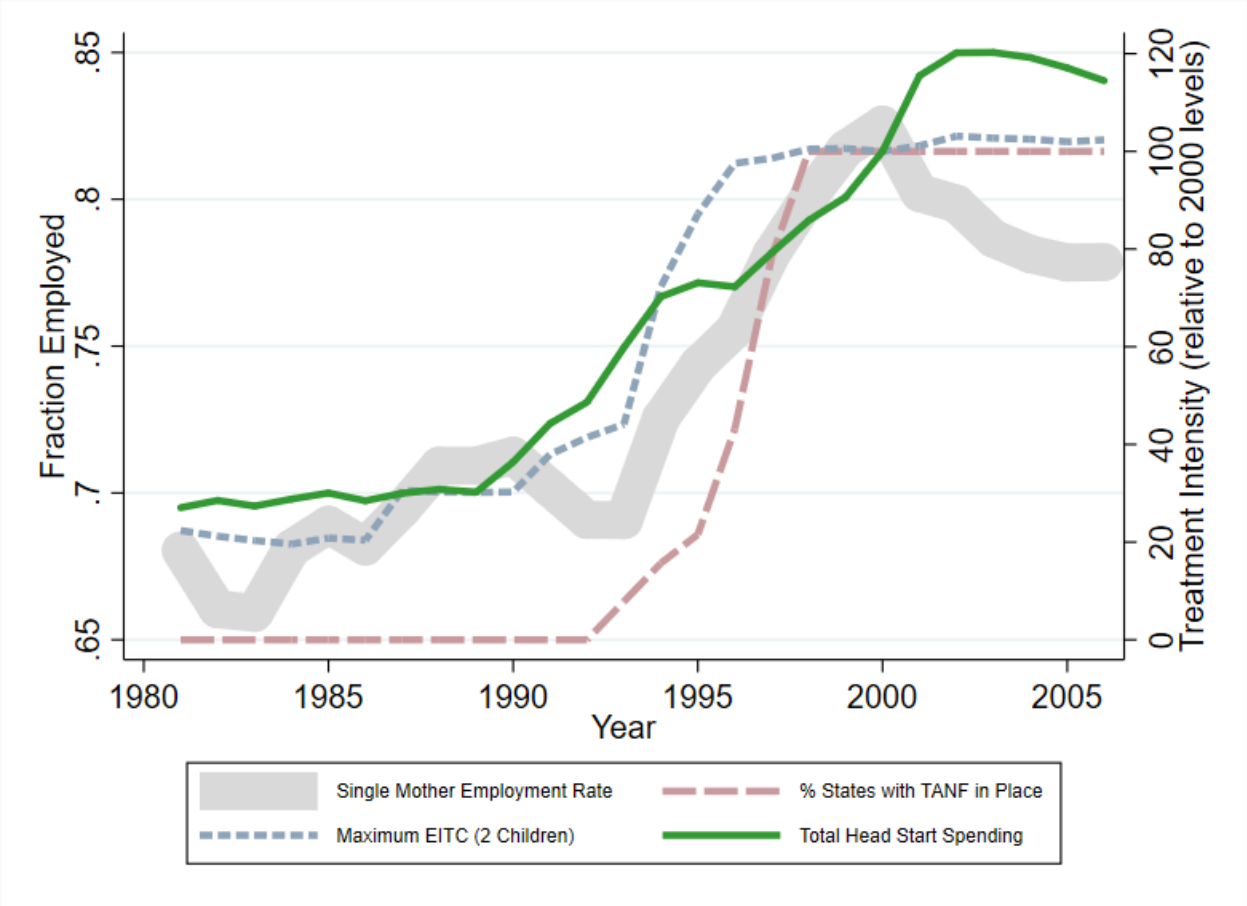
Figure 1. Historical Head Start Enrollment and Timing of Natural Experiments



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. Author's calculations.

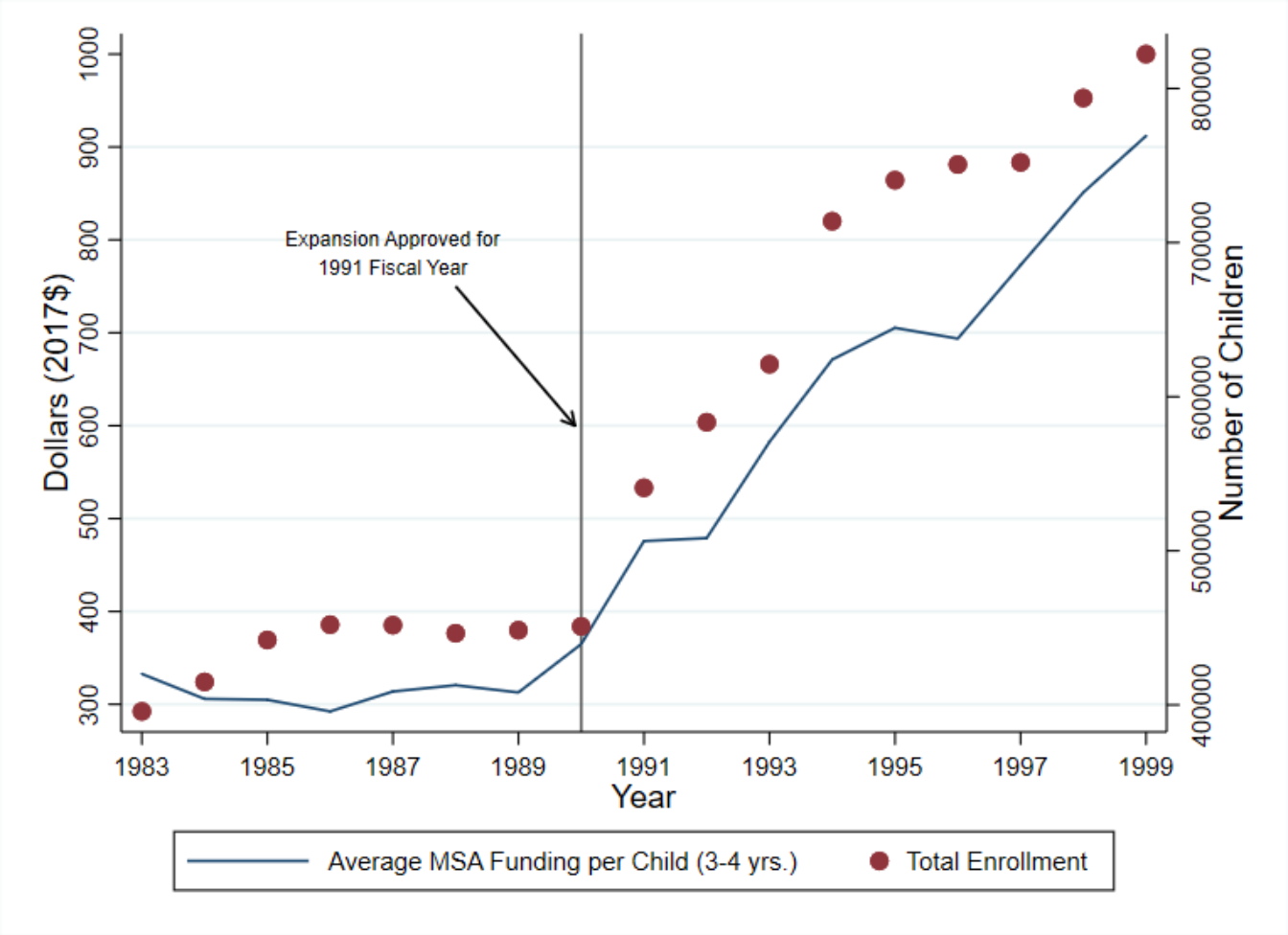
Figure 2. Trends in Annual Employment of Single Mothers and Low-Income Social Policies



Notes: Measures of TANF, the EITC maximum refund, and total Head Start spending are normalized relative to the level of treatment (generosity) in 2000.

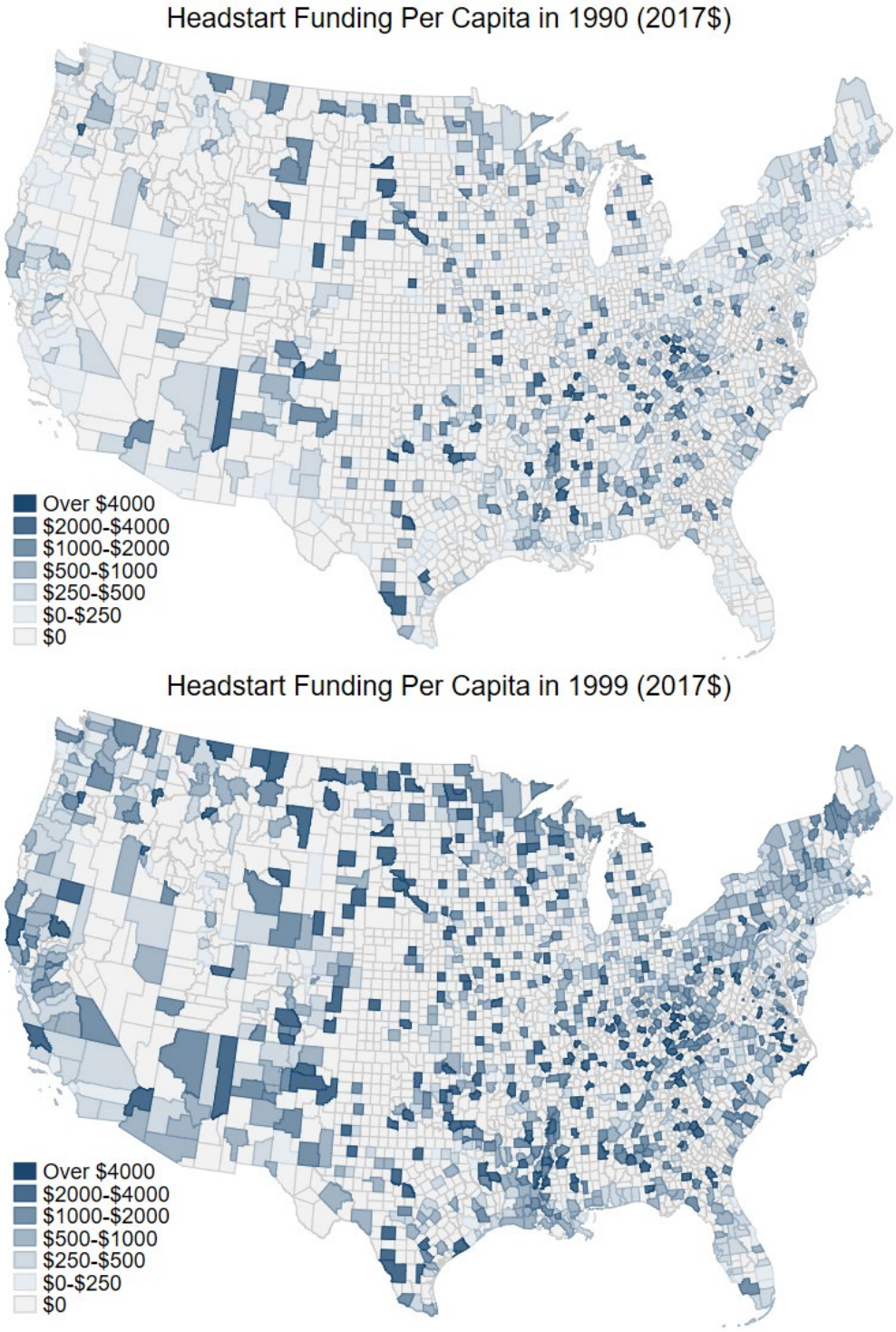
Source: Employment rates constructed from the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) for single women with children between 1982 and 2007, data on TANF waivers generously provided by Kearney and Levine (2013), Maximum EITC refunds calculated from the Tax Policy Center, and Head Start spending provided by the Office of Head Start. Authors' Calculations.

Figure 3. 1990s Expansions in Head Start Funding and Enrollment



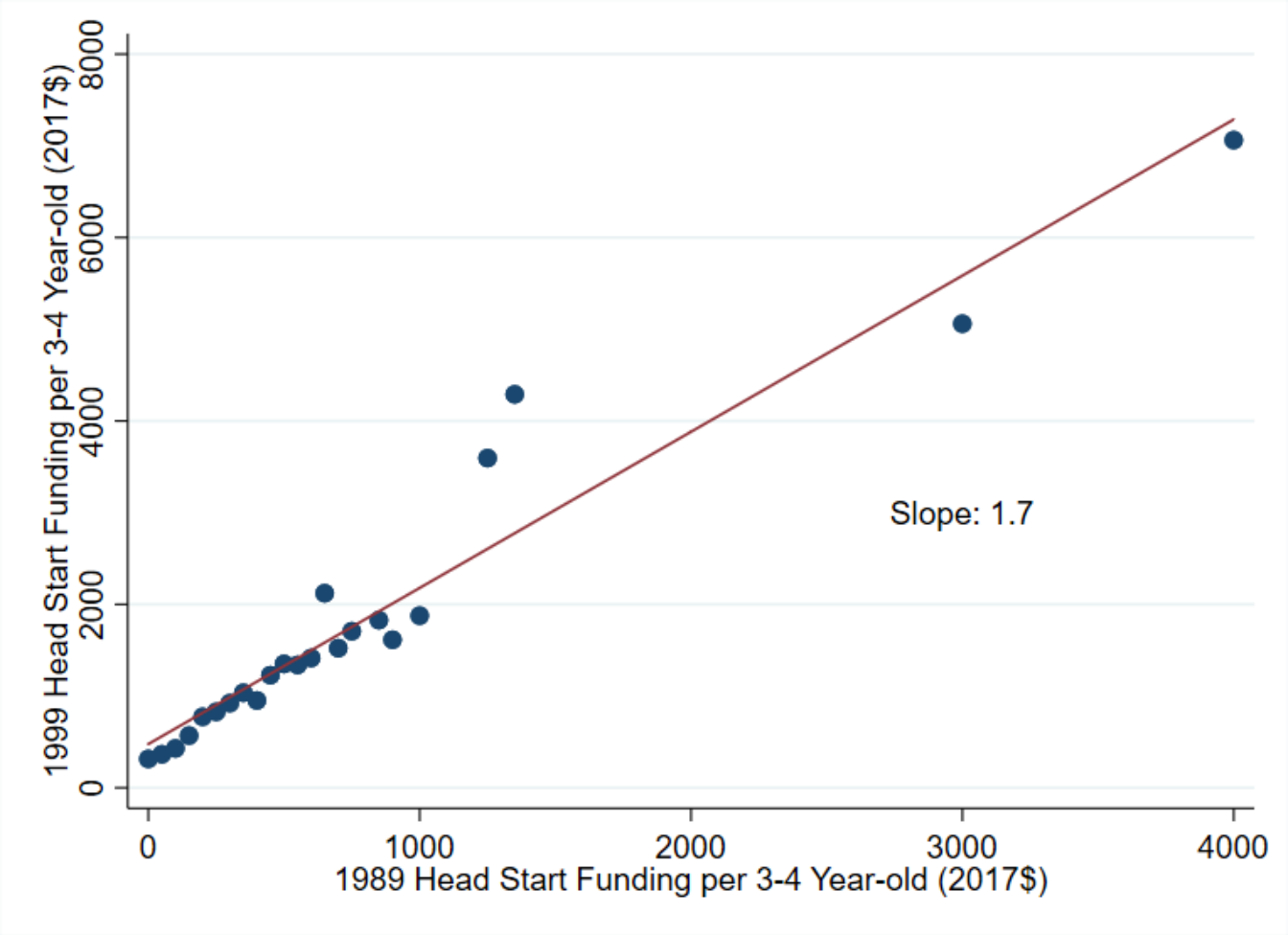
Source: Total enrollment obtained from the Office of Head Start. City level funding obtained from the historic Consolidated Federal Funds Report and aggregated to the MSA-level.

Figure 4. Changes in County-level funding in the 1990s



Source: City level funding obtained from the historic Consolidated Federal Funds Report and aggregated to the county-level for figure. Author's calculations.

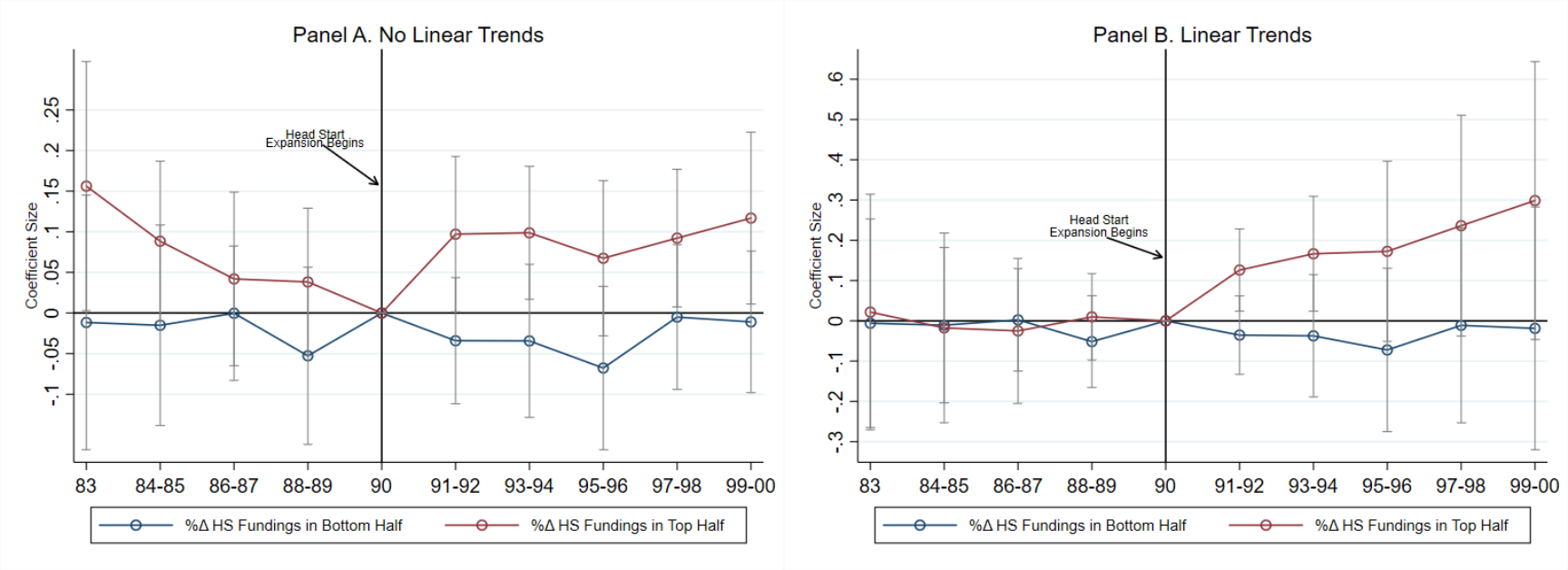
Figure 5. Additional Head Start Dollars in the 1990s were Dispersed Proportionally



Notes: MSA level funding combined into bins of 50 dollar increments with the mean plotted.

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

Figure 6. Trends in Employment of Mothers' with Age-eligible Child Relative to Mothers' with Younger Child by the Percent Change in per capita Head Start Spending between 1989 and 1999



Notes: Coefficients from equation (4) plotted, showing the change in employment rates for single mothers with an age-eligible 3- or 4-year-old, relative to single mothers with children younger than three. 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. In Panel B. mothers with an age-eligible 3- or 4-year-old are allowed to have different linear time trends. Ninety-five percent confidence intervals also provided. To interpret, 0.05 is a five percentage point change.
Source: CPS ASEC 1983-2000. Authors' calculations.

Appendix A: Tables and Figures

Appendix Table A1. Robustness of Impact of MSA-level Head Start Funding on Labor Market Outcomes of Single Mothers in 1990s

	Outcome: Any Employment in t-1						
	No Policy Controls (1)	Include Non-MSA (2)	MSA by Year Fixed Effects (3)	Single Mothers with Children ≤ 18 (4)	Only One ASEC Observation per Person (5)	Pre-Early HS (1995) (6)	Child 3-4 Linear Trends (7)
Head Start Funding per Child$_{t-1}$	0.016*	0.018**	0.018*	0.022***	0.020**	0.035***	0.018*
*Child 3-4 in t-1	(0.009)	(0.007)	(0.011)	(0.008)	(0.009)	(0.010)	(0.010)
Head Start Funding per Child$_{t-1}$	0.015	-0.009		0.014	0.036	-0.038	0.014
	(0.024)	(0.019)		(0.016)	(0.033)	(0.039)	(0.024)
Child 3-4 in t-1	0.012	0.015	0.016	-0.090***	0.007	0.016	-1.703
	(0.012)	(0.011)	(0.014)	(0.011)	(0.015)	(0.013)	(3.244)
Ave. Employment Rate	0.62	0.63	0.62	0.72	0.65	0.57	0.62
Observations	21,672	30,416	21,130	49,605	11,972	13,219	21,672

Notes: Data from the CPS ASEC 1983-2000 repeated cross sections. Sample restricted to single women with a child 5 or younger, except for column (4). Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Column (1) excludes policy controls. Column (2) includes mothers in non-MSA areas, where the funding level assigned is the rest of the Head Start funding in the non-metro area of the state. Column (3) Includes MSA by year fixed effects, rather than MSA and year effects separately, this facilitates a comparison between women in the same MSA and year. Column (4) includes single mothers with any child 18 or younger. Because participants are sampled for several rounds, Column (5) limits the sample to only one observation per woman. Column (6) ends the sample in 1994, to avoid the introduction of early Head Start for younger children which could contaminate the control. Column (7) includes linear trends for women with a 3 or 4 year old in the previous year. Since the treatment is a dose response, the linear trend might over control and capture some of the treatment effect (Wolfers, 2006). 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 childs health insurnace 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. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A2. Impact of Head Start Funding on Labor Market Outcomes of Single Mothers in 1990s by Marital Status

				Inverse Hyperbolic Sine of		
	Employed	Full-time	Part-time	Weeks Worked	Usual Hours Worked	Wage Income
	(1)	(2)	(3)	(4)	(5)	(6)
				Never Married		
Head Start Funding per Child$_{t-1}$	0.032***	0.027***	0.006	0.144***	0.136***	0.295***
*Have Child 3-4 in t-1	(0.008)	(0.010)	(0.007)	(0.034)	(0.036)	(0.091)
Dependent Mean	0.57	0.39	0.18	21.2	20.0	9,113
Observations	15,237	15,237	15,237	15,237	15,237	15,237
				Separated, Divorced, or Widowed		
Head Start Funding per Child$_{t-1}$	0.010	-0.001	0.011	0.043	0.039	0.156
*Have Child 3-4 in t-1	(0.011)	(0.008)	(0.011)	(0.049)	(0.044)	(0.130)
Dependent Mean	0.69	0.53	0.16	28.6	25.5	15,775
Observations	11,368	11,368	11,368	11,368	11,368	11,368
				Married		
Head Start Funding per Child$_{t-1}$	0.015**	0.027**	-0.011	0.073**	0.082**	0.207**
*Have Child 3-4 in t-1	(0.008)	(0.012)	(0.007)	(0.035)	(0.037)	(0.083)
Dependent Mean	0.66	0.42	0.24	27.1	22.1	16,658
Observations	81,166	81,166	81,166	81,166	81,166	81,166

Notes: Data from the CPS ASEC 1983-2000 repeated cross sections. Sample restricted to women with a child 5 or younger. 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, whether the state has a childs health insurnace program (SCHIP) in place, and the maximum EITC the family is eligible to receive. The outcomes in columns (5)-(7) are the inverse hyperbolic sine of the value, to include zeroes. 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.

Appendix Table A3. Summary Statistics for Single Mothers with Any Children Five or Under in the CPS ASEC, 1964-1970

	Had 3-5 Year old	No 3-5 Year old
Ave. Increase in Head Start per Child	161	
In the Labor Force last year	0.26	0.36
Employed last year	0.48	0.53
In LF > 1 quarter	0.17	0.29
In LF >2 quarters	0.14	0.19
In LF >3 quarters	0.05	0.05
Wage Income (2017$)	13678.74	10459.11
Employed at Time of Interview	0.38	0.37
Keeps House at Time of Interview	0.57	0.58
Less than High School Graduate	0.57	0.55
High School or Less	0.89	0.89
Has a child under Three in Family	0.35	1
Non-White	0.45	0.47
Age	31.74	31.09
Observations	817	291

Notes: CPS ASEC 1964-1970. Sample restricted to single women with a child 5 or younger in an MSA which does not change geography between 1964 and 1970. Sample means are weigted, using the level ASEC weights. The variable "Employed Last Year" shows evidence of miscoding in the original data where observations coded as working between 50-52 weeks are also reported as not in the labor force. We report uncorrected data for the "Employed Last Year" variable here, and results remain lower than "In the Labor Force Last Year" when alternatively coding these observations as not employed during the previous year.

Appendix Table A4. Impact of MSA-level Head Start Availibility on Labor Market Outcomes of Single Mothers with children 5 or under in the 1960s

	In Labor Force (1)	Employed (2)	In LF >1quarter (3)	In LF >2quarter (4)	In LF >3quarter (5)	Inverse Hyperbolic Sine of Wage Income (6)
Head Start Funding per Child$_{t-1}$	0.208***	0.095	0.171***	0.087**	0.026	-0.074
*Have Child 3-5	(0.031)	(0.056)	(0.052)	(0.029)	(0.019)	(0.883)
Head Start Funding per Child$_{t-1}$	-0.195***	-0.077	-0.186***	-0.090**	-0.028*	0.448
	(0.030)	(0.052)	(0.037)	(0.030)	(0.015)	(0.582)
Have Child 3-5	-0.127	-0.188	-0.105	-0.091	-0.039	0.392
	(0.139)	(0.168)	(0.104)	(0.075)	(0.026)	(1.104)
Dependent Mean	0.278	0.478	0.198	0.137	0.042	11,863
Observations	1,116	1,116	1,116	1,116	1,116	1,116

Notes: Data from the CPS ASEC 1964-1970 repeated cross sections. Sample restricted to single women with a child 5 or younger in an MSA which does not change geography between 1964 and 1970. Head Start availability is measured as the weighted average of binary variables indicating Head Start availability in the counties within the MSA. Controls include indicators for mother's race and education, state level demographic controls, and policy controls including payments from the Aid to Families with Dependent Children program, presence of federally funded community health centers, presence of Community Action Programs, state minimum wage, and federal minimum wage. The outcome in column (6) are the inverse hyperbolic sine of the value, to include zeroes. The variable "Employed" shows evidence of miscoding in the original data where observations coded as working between 50-52 weeks are also reported as not in the labor force. For column (2) only, we report uncorrected data here, and estimates are larger and highly significant when alternatively coding these observations as not employed. All regressions are weighted using the household CPS ASEC weights. Standard errors are corrected for clustering at the MSA level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A5. Heterogeneity in Employment Response to Head Start Rollout in the late 1960s

	Single	All Mothers	High School or Less	No Younger children
	(1)	(2)	(3)	(4)
Head Start Funding per Child$_{t-1}$	0.319**	0.011	-0.011	-0.011
*Have Child 3-5	(0.136)	(0.023)	(0.021)	(0.030)
Head Start Funding per Child$_{t-1}$	-0.127**	-0.100***	-0.082***	-0.089***
	(0.040)	(0.008)	(0.008)	(0.008)
Have Child 3-5	-0.213	0.002	0.037	0.005
	(0.157)	(0.047)	(0.055)	(0.042)
Dependent Mean	0.278	0.231	0.225	0.247
Observations	1,116	15,316	11,919	11,037

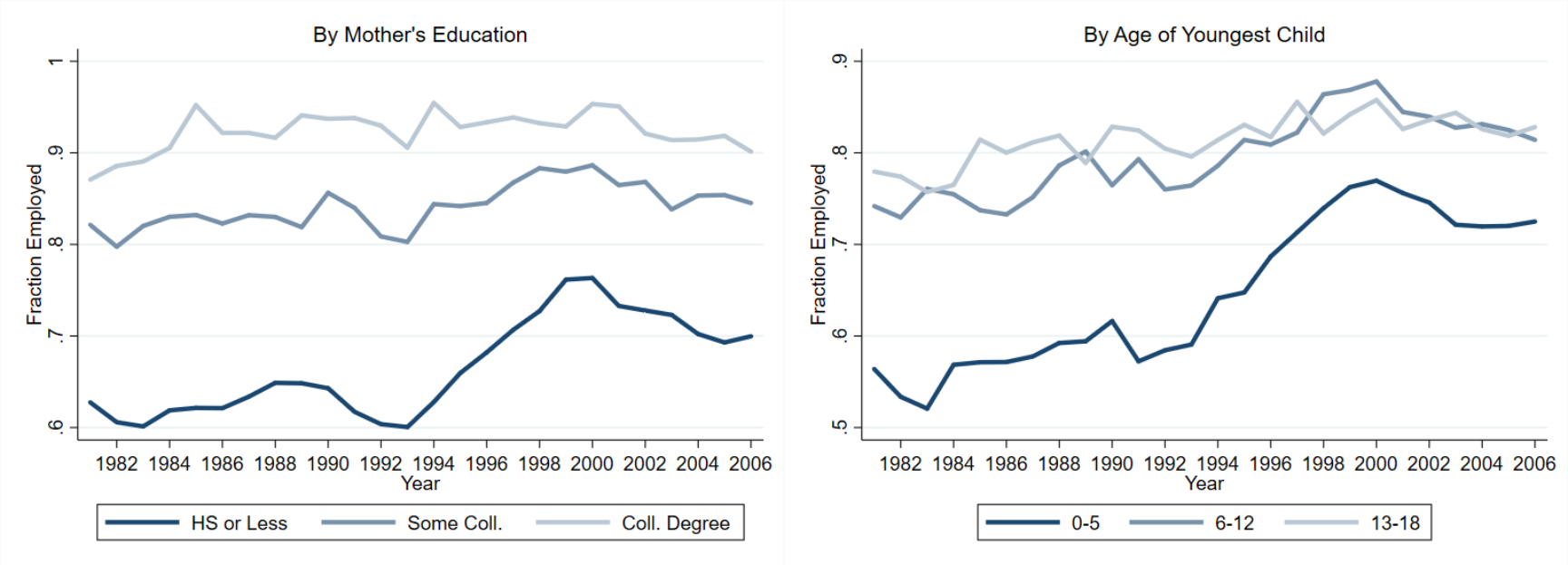
Notes: Data from the CPS ASEC 1964-1970 repeated cross sections. Sample restricted to women with a child 5 or younger in an MSA which does not change geography between 1964 and 1970. Head Start Funding per Child is measured at the MSA level in units of $500 (2017$). Controls include indicators for mother's race and education, state level demographic controls, and policy controls including payments from the Aid to Families with Dependent Children program, presence of federally funded community health centers, presence of Community Action Programs, state minimum wage, and federal minimum wage. All regressions are weighted using the household CPS ASEC weights. Standard errors are corrected for clustering at the MSA level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A6. Reduced Form Impact of Head Start Assignment on Maternal Labor Supply (ITT)

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

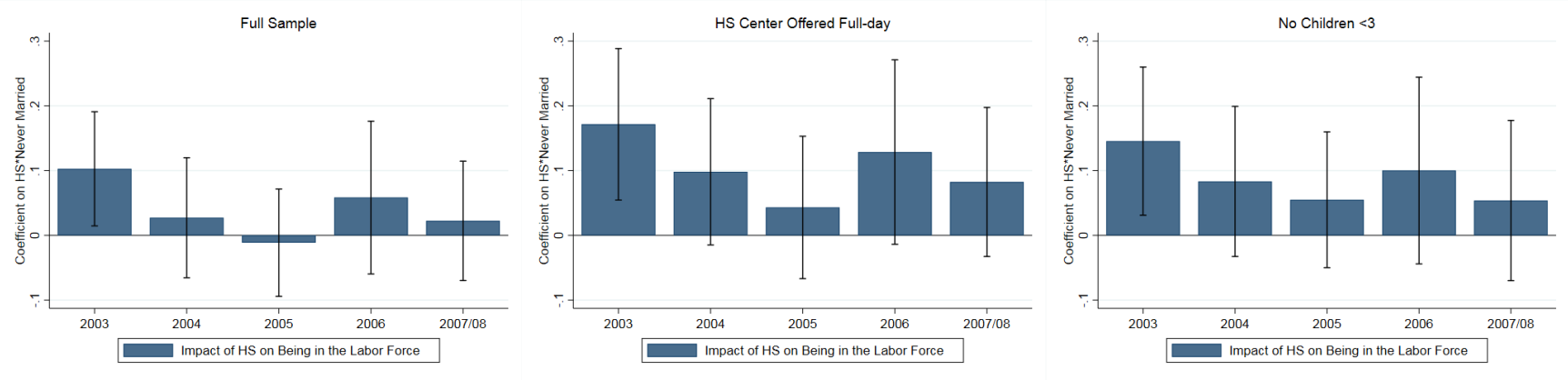
Appendix Figure A1. Trends in Single Mother Employment for Sub-groups



Notes: Recipients report employment during the previous calendar year. As such, the employment rate is lagged to the appropriate year.

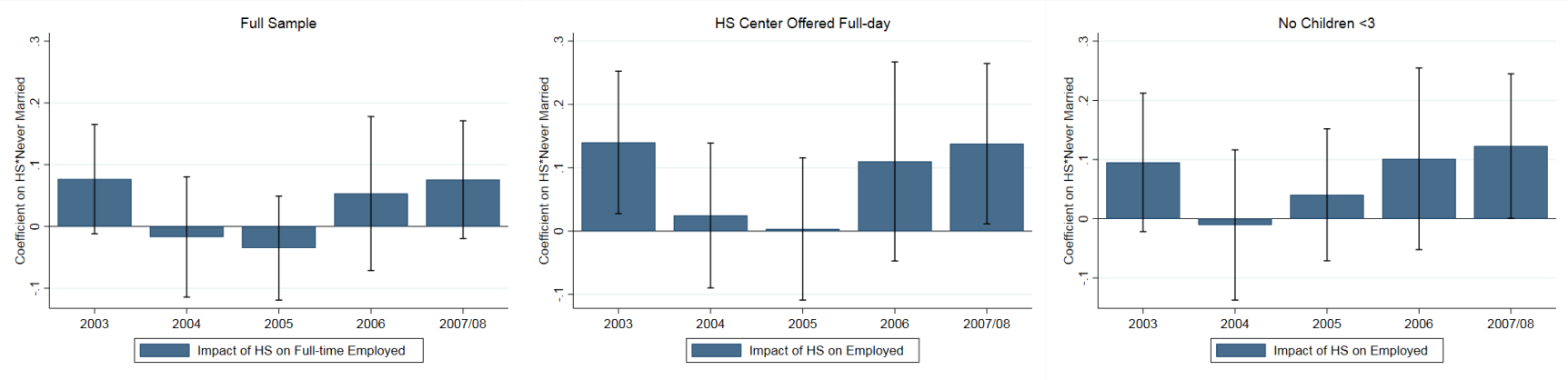
Source: Current Population Survey Annual Social and Economic Supplement (CPS ASEC) for single women with children between 1982 and 2007. Authors' Calculations.

Appendix Figure A2. Persistent Impact of Head Start on Labor Force Participation of Never Married Mothers



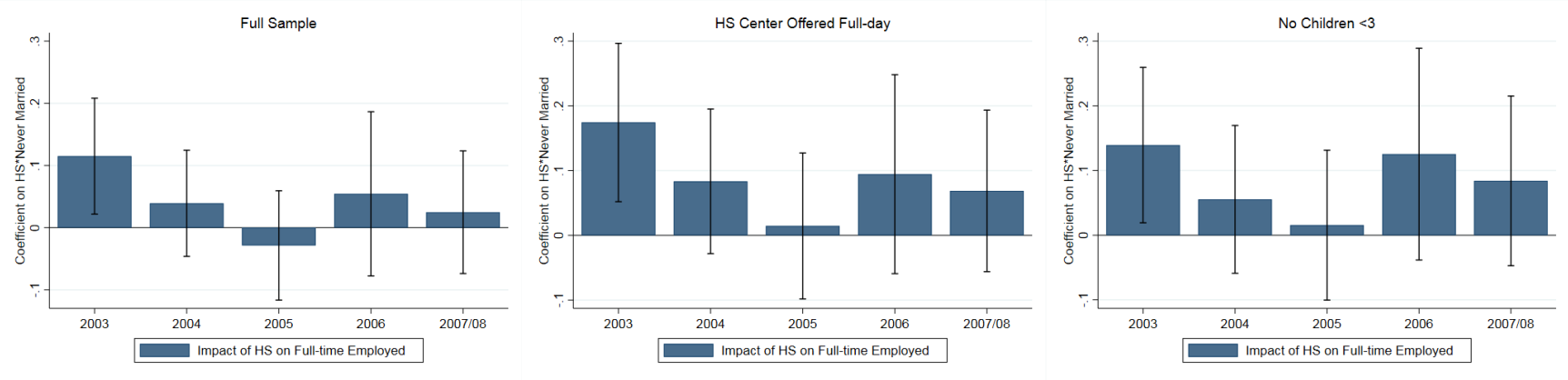
Notes: Bars plot the coefficients from the regression of equation (B.2), where the outcome is the mother's labor force participation status in the spring of each of the given years. Sample restricted to households that participated in the listed Parent Interview in the given year. The Head Start randomized 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.

Appendix Figure A3. Persistent Impact of Head Start on Employment of Never Married Mothers



Notes: Bars plot the coefficients from the regression of equation (B.2), 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.

Appendix Figure A4. Persistent Impact of Head Start on Full-time Employment of Never Married Mothers



Notes: Bars plot the coefficients from the regression of equation (B.2), 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.

Appendix B: Detailed Analysis of the Head Start Impact Study

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, Bell, Cook, & Heid 2010). Puma et al. (2010) 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. The sample is nationally representative of the Head Start population. 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 childcare setting gives context to why Head Start enrollment might be expected to relax a mother's temporal constraints and lead to employment effects (Duncan & 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, so we do not focus on the measure.[23] We combine these two measures to create consistent income bins and then look to see if the household had monthly incomes over $250, $500, and $1000 a month.

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:

$$Head\ Start_i = \alpha_1 Treated_i + \mu_m + \eta_i \qquad \text{(B.1)}$$

$$Y_i = \beta \widehat{Head\ Start}_i + \mu_m + \varepsilon_i$$

$Treated_i$ indicates if the household was randomly assigned to Head Start through the lottery, $Head\ Start_i$ 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 or income 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.[24] 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.[25]

[23] We have repeated the labor supply analysis using these income measures and find some evidence that never married mothers are more likely to have household income over $500. This is concentrated among never married mothers without younger children and at full-day Head Start centers.
[24] About 10 percent of households were interviewed in June or later.
[25] For reference, the first stage coefficient on treated is 0.64 and the f-statistic on this excluded instrument is 1129.

In all specifications, standard errors will be 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 to be representative of the Head Start population. We estimate inverse propensity-score adjusted weights, similar to the approach taken by Bitler, Hoynes and Domina (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 in all analyses.

The family settings and earning dynamics 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. To match the observational analysis from the 1990s, we will also estimate equation (B.1) separately by marital status. The analysis sample is already limited, so to preserve power and maintain disclosure standards, we estimate a variation of equation (B.1), that allows us to estimate the effects for each marital arrangements separately as follows:

$$Y_i = \beta_1 Head\ Start_\iota * \widehat{Never\ Married}_\iota + \beta_2 Head\ Start_\iota * \widehat{Prev.Married}_\iota + \beta_3 Head\ \widehat{Start_\iota *} Married_\iota + \beta_4 Never\ Married_i + \beta_5 Prev.Married_i + \mathrm{B}_6 Married_i + \mu_m + \varepsilon_i \quad \text{(B.2)}$$

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 (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 will 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_1 represents the impact of Head Start availability on the mother's employment outcomes among never married mothers, while β_2 and β_3 represent the effects for previously married and never married mothers. From this regression, we will identify the causal impact of Head Start enrollment on maternal employment and household income, allowing the effect to vary by marital status.