Understanding The Impact:

The Effects of Head Start on Mothers' Labor-Force Participation[*]

Jacob Light[†]

Abstract The United States Department of Health and Human Services emphasizes a holistic approach to early childhood education and family development in its Head Start programming for low-income families. Head Start, since its inception, has been the subject of rigorous evaluation and analysis. Nearly all of the analysis, however, has focused on children's cognitive and social development. In this paper, I assess the role Head Start plays in parents' labor-force decisions. Using data from the Head Start Impact Study (HSIS), a randomized longitudinal study of first-time Head Start applicants, I develop and estimate a model for the labor-force participation of low-income mothers with access to Head Start. I observe increased labor-force participation rates among HSIS mothers randomly assigned access to Head Start for both cohorts, which dissipate when the child reaches kindergarten age. The positive labor-force outcomes were most pronounced among subgroups of mothers with the greatest relative economic disadvantage before Head Start, while the most well-off subgroups decreased their labor force participation after receiving access to Head Start.

[*] I thank Professor Martha Bailey for her dedicated supervision, thorough feedback, and exceptional advice, and ICPSR at the University of Michigan for assistance in securing the data for this study.
[†] University of Michigan, jdlight@umich.edu

Introduction

The Office of Economic Opportunity's Community Action Program initiated the Head Start Program in 1965 as a part of President Lyndon B. Johnson's War on Poverty. The program began as an intervention to reduce disparities in children's K-12 educational outcomes. Prior to the creation of Head Start, early childhood education programs were largely inaccessible to low-income families. As a result, low-income students entered public schools at an immediate educational disadvantage to more affluent students— many of whom had been in pre-school for several years. Today, Head Start is operated by the United States Department of Health and Human Services and emphasizes health and wellness, educational and social development, and a family environment conducive to child development. Appropriations for Head Start, in real dollars, have grown nearly 1,600% since the program was initiated. Enrollment in Head Start programs, meanwhile, has grown to more than 950,000 students (United States Department of Health and Human Services 2014).

The effects of Head Start have been the subject of passionate debate since its creation. Critics of Head Start cite the fading of Head Start's academic effects as evidence of the program's ineffectiveness. Supporters present evidence of long-term economic and social benefits experienced by Head Start participants. The debate surrounding the merits of Head Start reached a climax during the 2013 sequester debate, when a $407 million (5.6%) cut to Head Start's funding caused a 6% reduction in enrollment capacity.

The short- and long-term effects of Head Start on students' academic, social, and economic development have been extensively studied. Understudied are the effects of Head Start on participating parents. Head Start functions much like a childcare subsidy; parents who enroll their children in Head Start do not need to pay for center-based childcare or watch their children while their children are in Head Start. The theoretical effect of such an opportunity is ambiguous. Parents who would otherwise spend their time watching their children may choose to enter the labor force, while parents who no longer need to pay for child care may choose to work less when they have access to Head Start. Without empirical evidence, it is difficult to fully account for the effects of Head Start on parents who also benefit from the program.

This paper analyzes the effects of Head Start on the labor-force outcomes of mothers with participating children. I use annual survey data collected through the Head Start Impact Study (HSIS) between 2002-06 to conduct my analysis, taking advantage of its randomization to estimate the causal effects of Head Start on participant outcomes. I observe modest improvements in labor-force outcomes for mothers randomized into access in both cohorts of the HSIS sample. For mothers of the cohort of three-year-old first-time Head Start applicants, I find that access to Head Start increases the likelihood of full-time employment by 4.81% during the first year of Head Start. Mothers of four-year-old first-time applicants were 6.46% more likely to participate in the labor force in the year their children had access to Head Start. The effects were heterogeneous and offsetting across subgroups. The least advantaged subgroups in the pre-HSIS survey experienced the greatest positive long-term labor effects from access to Head Start, while the most advantaged subgroups experienced negative long-term labor effects. These results confirm many of the canonical labor-leisure model as applied to Head Start.

The rest of the paper proceeds as follows. Section I presents a simple labor-supply framework for predicting responsiveness to Head Start. Section II surveys the existing empirical literature related to Head Start and the economics of childcare subsidies. Section III describes HSIS, the data used in the study. Section IV outlines my empirical strategy of analysis. Section V presents results, which I discuss in Section VI, and Section VII concludes.

I. Theoretical Model

The impact of Head Start can be understood within the canonical labor-leisure model, modified to incorporate childcare costs. Assume that a mother seeks to maximize wellbeing $U(C, L)$ through her allocation of leisure time (L) and consumption of market goods (C).[1] Each hour a mother works, she must pay for childcare, as captured in the budget constraint

(1) $$C \leq (w - p)h + (F + V),$$

where the price of childcare (p) acts as a tax on a mother's hourly wage (w) in each hour (h) a mother works. Therefore, her consumption is constrained by the sum of her income, net childcare costs, paternal contributions to household income (F), and nonwage income (V). In the model, a mother first chooses to participate in the labor force and, contingent on labor-force participation, chooses the degree to which she works through hours worked (h). A mother's take-home hourly wage $(w - p)$ decreases with the price of childcare.

[1] See Blau (2003) for a more extensive treatment of the model. For simplicity, the model assumes that the only childcare options are paid care and maternal care. The model also assumes that only one child requires care. I relax this assumption in my empirical analysis. The model assumes no fixed cost of labor and ignores any utility derived from the educational and socially developmental benefits borne onto Head Start participants.

A change in a mother's take home wage has two effects on her degree of labor-force participation: a substitution effect and an income effect. Consider the effects of a shock that decreases childcare costs to p' such that $(w - p') > (w - p)$. An increase in the mother's wage allows her to consume at the same level working fewer hours; this income effect increases her consumption of leisure. However, the opportunity cost of leisure increases to the higher wage; this substitution effect pushes her to increase her hours worked. The dominance of one effect over the other depends on three factors: a mother's preferences, pre-Head Start labor force participation, and other nonwage income.

For mothers not in the labor force, a change in the take home wage can have only a substitution effect. Mothers out of the labor force cannot increase their leisure allocation; thus, there is no income effect for mothers not participating in the labor force. Mothers with significant aversions to work will remain unemployed, while some mothers will opt to enter the labor force when faced with a higher effective wage. Therefore, an increase in the effective wage resulting from access to Head Start will have an unambiguous nonnegative effect on labor-force participation. For mothers already working, however, the net result of the income and substitution effects is theoretically ambiguous.

Figure 1A demonstrates this analysis, which I also describe algebraically in the revised budget constraint

(2) $$C \leq wh - (max[h - h_1, 0])p + (F + V).$$

In equation (2), h_1 is the time a child spends in Head Start. During the hours a child participates in Head Start, a mother's childcare cost is 0. For each hour a mother works in excess of the time her child participates in Head Start, she has the same take home wage $(w\text{-}p)$ as in equation (1).

Mothers with access to Head Start experience income and substitution effects of opposing directions. The magnitude and net allocation resulting from these effects depends on a mother's consumption-leisure allocation prior to receiving access to Head Start.[2] The effect of access to Head Start on the labor-force participation of mothers initially not working is unambiguously positive. Mothers working h hours before enrolling their children in Head Start, where $h_1 < h < h_2$, experience an insolated income effect, leading them to increase leisure consumption and decrease hours worked. The effect on mothers who initially worked between $0 < h < h_1$ hours is

[2] The analysis uses standard assumptions of monotonicity, concavity, and transitivity in preferences from microeconomic theory.

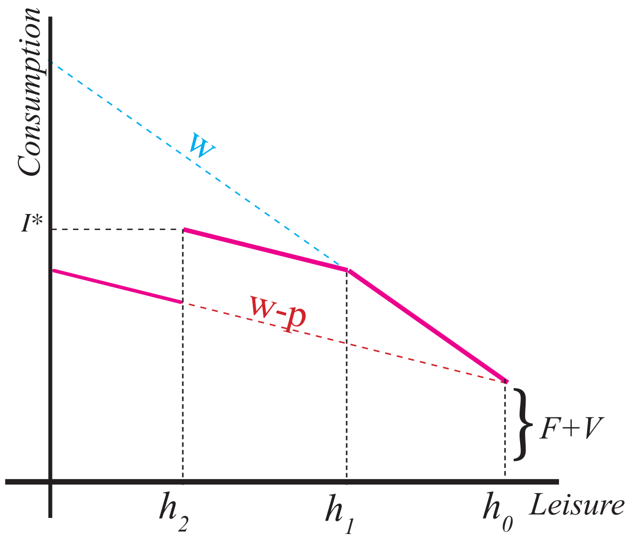


Figure 1A- Simple budget constraint

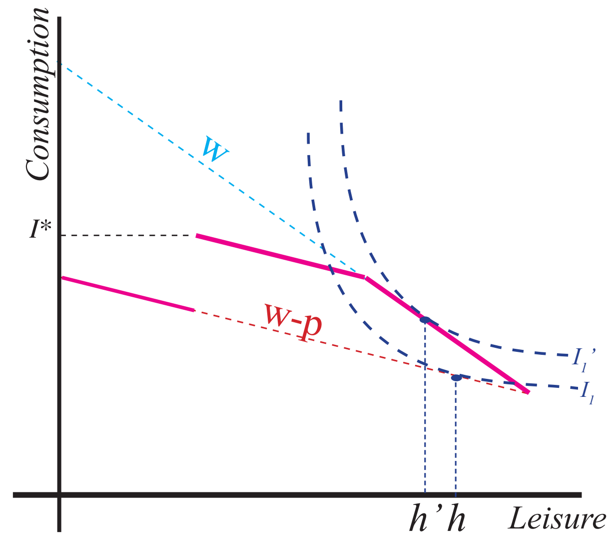


Figure 1B- Budget constraint
with indifference curves

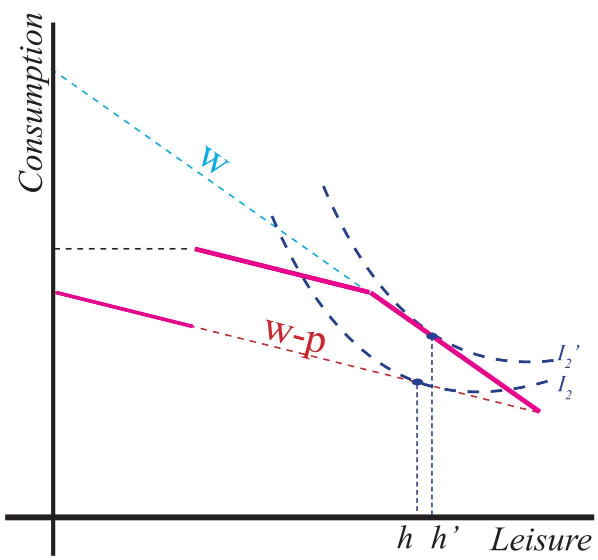


Figure 1C- Budget constraint
with indifference curves

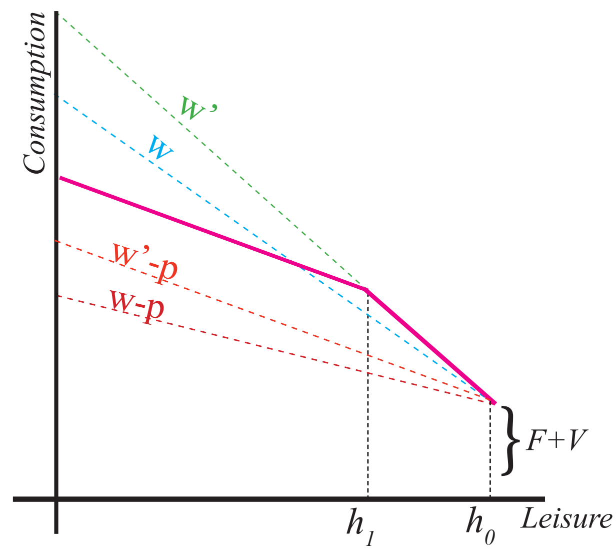


Figure 1D- Budget constraint
with returns to experience

theoretically ambiguous. The model cannot predict whether the income or substitution effect dominates. The dominant effect depends on the magnitudes of w and $w - p$, as well as a mother's preferences U. I demonstrate this through the indifference curves in Figures 1 B-C. For a mother with indifference curve I_1, as in Figure 1B, the substitution effect dominates following an increase in the effective wage. This mother would increase her labor-force participation when she has access to Head Start. Meanwhile, for a mother with indifference curve I_2, as in Figure 1C, the income effect dominates when her effective wage increases and she decreases her labor-force participation. Experimental data provides the best lens to construct indifference curves and model the decisions faced by mothers in this ambiguous section of the Head Start impact.

Adding an additional complication, Head Start is a means-tested program, so mothers are only eligible to enroll their children if their income falls below 130% of the national poverty level. I represent the cutoff of a mother's Head Start eligibility in Figure 1A when she works more than h_2 hours. When $h > h_2$, her household income is greater than the 130% of poverty level Head Start eligibility cutoff, represented in Figure 1 as I^*, and she looses access to Head Start. The eligibility scheme of Head Start disincentives mothers earning below I^* from working more than h_2 hours. The discrete value of h_2 depends on a mother's wage (w) and non-maternal family income ($F+V$).

The effects of Head Start on a mother's budget constraint extend beyond a child's Head Start participation. For example, consider the model of labor market decisions that includes the returns to job experience and tenure shown in Figure 1D. Mothers who are brought into the labor force as a result of access to Head Start may experience returns on job tenure and experience, represented by $w' > w$. Following the completion of Head Start, all mothers enroll their children in a public kindergarten program, free at all income levels. Mothers who entered into the labor force while their children were in Head Start may face an increase in take home wages, while the effects of income and substitution effects are similar to those described in Figure 1A.

Economic theory provides strong insight into the labor decisions a mother faces when considering childcare costs. However, the models fall short of predicting the effects of access to a subsidized childcare service, such as Head Start, when the magnitudes of the income and substitution effects are ambiguous. Empirical analysis can fill in some of the gaps in economic theory by predicting which outcomes, if any, are most affected by access to Head Start and for whom the effects are most significant.

II. Literature Review

Since its inception, Head Start has been the subject of extensive empirical study. Nearly all of the existing empirical literature has analyzed the role Head Start plays in a child's social and academic development. Absent from the literature is a thorough analysis of the program's effect on parent outcomes. Head Start acts, in many ways, as a simple childcare subsidy. I argue in this paper that the unique scale, design, and political significance of Head Start within the childcare sphere. Thus, I briefly review the literature studying the effects of Head Start on child outcomes. Second, I thoroughly survey empirical studies on the impact childcare subsidies. Finally, I review the history of Head Start with close attention to the programs Head Start to for participating parents.

A. Overview of Head Start Effects on Participants' Academic and Social Outcomes

Extensive study of the academic and social outcomes affected by a child's participation in Head Start began soon after the program's establishment in 1965. Existing research has been conducted using a variety of samples and analytical strategies and has evolved alongside Head Start. I provide only a brief overview of some of the recent and influential findings on the role Head Start plays in a child's short- and long-term development to frame the contemporary understanding of Head Start.[3]

The Head Start Impact Study is the most recent and exhaustive study to identify Head Start effects on participants' learning and social development. The Head Start Impact Study collects data from a national experimental study to observe cognitive and non-cognitive outcomes for Head Start participants. Among students with access to, at most, one year of Head Start (four-year-old cohort), students entered kindergarten with significantly higher literacy and verbal skills. However, these benefits dissipated almost entirely by the end of the initial observation period (first grade). For students with access to, at most, two years of Head Start (three-year-old cohort), the study finds early benefits in literacy and math that fade by the end of the student's first year in Head Start (Head Start Impact Study Final Report 2010). A similar "fading out" effect is well documented in the longitudinal Head Start literature (Deming 2009, Currie and Thomas 1995, e.g.).

Despite the persistent convergence in academic performance between Head Start participants and non-participants, Head Start participants tend to have better long-term academic

[3] Gibbs et al. (2012) provide a comprehensive treatment of the historical and present Head Start literature.

and social outcomes than non-participants. Garces, Thomas, and Currie (2002) use sibling data from the PSID to estimate the effect of Head Start participation on adult social and economic outcomes (income, education, and incarceration rates). They find, through OLS estimates, that white Head Start participants in their sample were 21.7% more likely to graduate from high school. Black Head Start participants in their sample, however, were 11.7% less likely than nonparticipants to be incarcerated or charged with a crime as an adult. Moreover, Ludwig and Miller (2007) find suggestive evidence, through regression discontinuity analysis, of positive and sustained educational and health impacts resulting from Head Start enrollment. Whereas educational benefits appear to fade out in the short term, the empirical literature suggests positive lifetime impacts of Head Start on various social, academic, and economic outcomes.

As HSIS data allows researchers to observe experimental Head Start data, analysis of the effects of Head Start has taken on renewed interest. Sabol and Chase-Lansdale (2014) uses HSIS data to study parental changes in educational outcomes and the transitions into the labor force through Head Start. They focus their analysis on changes in education level from the baseline observation. In their analysis, they find suggestive evidence of positive educational returns to Head Start access, primarily concentrated among parents with some college education but less than a Bachelor's Degree. They also labor-force outcomes of parents out of the labor force, pre-treatment, who move into the labor force during the HSIS observation period. Among these parents, they do not find a significant treatment effect. Their brief treatment of labor-force outcomes in the HSIS sample looks narrowly at mothers transitioning into the labor force. In this paper, I expand the analysis to consider the dynamics of transitions between employment states across years for the full sample of parents, rather than a restricted set of parents initially out of the labor force.

B. Survey of Empirical Literature Studying Effects of Childcare Subsidies

Over the last twenty years, existing research analyzing the employment decisions of parents of young children, particularly maternal employment, has expanded dramatically. Jean Kimmel (1995) finds that the price of childcare significantly affects single mothers' employment decisions. Through probit analysis of cross-sectional data from the 1987 and 1988 Survey of Income and Program Participation, Kimmel finds that the price elasticity of employment differed among single by race. She found that white mothers in her sample were highly price elastic (-1.36) to childcare subsidies, while single black mothers' employment was far less price elastic

(-0.35). Kimmel's estimates that full subsidization of early childcare could lead to a 132-percent increase in single white mothers' likelihood of employment. Despite estimates of such large and heterogeneous effects, Kimmel provides no explanation for these racial differences. Berger and Black (1992) find effects similar to those described by Kimmel. They run probit analysis using a dataset of single mothers eligible for Louisville 4C and Kentucky Title XX childcare subsidy programs to estimate the subsidy effect on the likelihood of labor-force participation. They estimate a 25.3% response in employment rates of poor single mothers to the subsidy. They do not find, however, that a subsidy would have a significant effect on hours worked. Their probit analysis, while insightful, may lack external validity due to a small, geographically concentrated sample.

Tekin (2007) extends the literature through analysis of data collected post-welfare reform. He uses data from the 1997 National Survey of America's Families (NASF) to estimate part- and full-time employment decisions in a behavior model of single mothers. In his model, a mother chooses to work (full-time, part-time, or not at all), whether to pay for childcare, and whether to accept a subsidy. Tekin estimates the effects of a subsidy on a mother's a employment status using a multinomial logit regression. He finds that mothers working full-time have stronger wage elasticity to the full-time wage rate than part-time mothers to the part-time wage rate (0.874 and 0.431, respectively). Their findings suggest that mothers who work full-time are more sensitive to the price of childcare (and, thus, subsidization of childcare programs) than mothers working part-time.

The aforementioned studies focus exclusively on subsidy effects on poor single mothers. Michalopoulos et al. (1992) use SIPP data to simulate differential childcare decisions between single and married mothers. They estimate a structural model in which mothers already in the labor force choose simultaneously their child care consumption and labor market participation. From their estimates, they conclude that single and married mothers' hours worked are price inelastic with respect to childcare subsidies (0.0014 and 0.0018, respectively). However, they only consider mothers already in the labor force. This result shows a relative inelasticity in the move from part- to full-time employment in the presence of a subsidy.

Accessible childcare and subsidization of early childhood care and education programs has been studied outside of its effects on labor-force participation. Martinez-Beck (2009) analyzes economic outcomes for recipients of the Child Care Subsidy. Through regression analysis the American Community Survey/Supplemental Survey for 2001 (ACS/SS01), Martinez-Beck identifies somewhat contradictory economic effects of the CCS in her logistic regression analysis. She finds that CCS recipients were more likely to remain employed across quarters, yet they were less likely to exceed the threshold for CCS eligibility than non-recipients.

From the literature presented, two trends emerge. Childcare subsidies appear to have a zero-to-slightly positive effect on mothers' labor-force participation. However, the effects of subsidies appear to be heterogeneous with among subgroups. I consider these effects in my empirical methodology in Section IV. The literature surveyed, however, relies heavily on national longitudinal studies vulnerable to selection bias. Because mothers are able to choose their labor status during all periods of observation, it is not possible to isolate the effects of a childcare subsidy on the outcome of observation. I extend the existing literature using recent data from the experimental Head Start Impact Study, which allows me to identify causal effects of access to subsidized Head Start on mothers' labor-force outcomes.

C. Head Start- Program Overview

Head Start was launched in 1965 as a War on Poverty program under President Lyndon B. Johnson. The program has evolved from a summer pre-kindergarten program into a full academic year or calendar year early childhood care and education program, serving poor children age 3-5. The program is operated by the United States Department of Health and Human Services, whose ideals are reflected in Head Start's mission of holistically promoting a participant's academic preparation, physical wellness, social development. Additionally, Head Start emphasizes the role of the family in fostering a positive developmental environment (Aguiar 2012). Head Start serves nearly 1 million students each year, with federal appropriations for Head Start in 2013 totaled $7.57 billion (United States Department of Health and Human Services 2014). Head Start is locally administered, and all affiliates must adhere to the national program guidelines.

A means-tested program, the criteria for Head Start eligibility were most recently enumerated in the bipartisan Improving Head Start for School Readiness Act of 2007. Head Start serves, primarily, children of families whose income falls below the Federal poverty line (enrollment from families with income between the poverty line and 130 percent of the poverty line is limited to 35 percent).[4] Children become eligible to participate at age 3 and may participate until they are of compulsory school age. Finally, programs must make at least 10 percent of enrollment available to students with disabilities.

Of greatest interest to this study is the degree of involvement Head Start asks of the parents in the program. Head Start emphasizes positive parenting and family development in its program objectives. A Head Start center begins its family partnership with a mandatory goal setting session and follows up with a participant's family throughout the duration of the program. A Head Start center regularly connects parents to continuing education opportunities, employment training programs, psychological and substance abuse counseling services, and emergency crisis assistance. While not compulsory, the Head Start performance standards instruct that parents be "encouraged to observe children as often as possible and to participate with children in group activities" (US Department of Health and Human Services 2006). The services provided and expectations set by Head Start oblige active parent involvement. Through the institutional support in finding a job and achieving balance between family and work, Head Start further pushes participating parents into the labor force. These program characteristics, in addition to the national scope of the program, make Head Start singular among childcare programs in the United States. These unique program components motivate study of Head Start's unique affect on parents' employment decisions.

III. Data

This paper uses data from the Head Start Impact Study (HSIS) to conduct my analysis. The HSIS was conducted to satisfy a 1998 congressional mandate to study the effects of Head Start on child learning and development and parental behavior. The initial study, conducted 2002-2006, followed a nationally representative sample of first-time applicants to Head Start in 2002.[5] The HSIS followed two cohorts of Head Start applicants: a three-year-old cohort of students with two years of Head Start eligibility and a four-year-old cohort of students with one year of Head Start eligibility. The study tracked educational, developmental, and social growth

[4] Up to 10% of enrollment can fail to meet these eligibility criteria if the family has demonstrated abrupt financial hardship or successfully appeals for an exemption.

[5] To ensure that the HSIS did not affect Head Start enrollment, the study only collected data from centers where applicant demand overwhelmed center capacity. Thus, the sample is representative of Head Start applicants to overflow centers- a nuanced, yet important, distinction from the population of Head Start applicants. This sample was representative of 84.5% of the total universe of three- and four-year-olds newly entering Head Start across the country.

during a child's pre-elementary through early elementary studies. The first round of the study followed both cohorts until the spring of the child's first grade year. In addition to collecting data on children's performance, the HSIS conducted annual surveys of the cohorts' parents. These surveys also collected data on parenting behaviors, interactions with children, involvement in their children's school, and basic economic and demographic information.

The data in Table 1 describes the Head Start Impact Study sample. Column (1) summarizes descriptive characteristics of the HSIS sample in the 2002 HSIS parent survey. To give context for the demographics of Head Start participants, I summarize data from the 2002 Current Population Survey (CPS) in column (2).[6] Clearly apparent, parents in the HSIS sample, meant to represent the population of Head Start applicants, is generally less educated and has lower income than the national average. The average monthly household income of a HSIS participant is $1,647, less than 30% of the national average monthly income. In contrast with national averages, mothers of Head Start applicants are predominately nonwhite, had their first child in their early twenties, and have less than a high school education. Mothers and fathers of Head Start applicants in the study were less likely to work full-time than parents in the CPS sample. Only 33.4% of mothers in the 2002 HSIS parents survey reported full-time labor employment.

The HSIS used randomization to evaluate the effects of Head Start. From a sample of Head Start centers at enrollment capacity, first-time applicants were assigned by random lottery into a "treatment" or "control" group. Children in the "treatment group" were able to enroll Head Start in the year they applied. Children in the control group of the four-year-old cohort were not given access to Head Start. Children in the control group of the three-year-old cohort (who, potentially, had another year of Head Start eligibility) were not given access to Head Start in 2002, but allowed to reapply in the second year of their eligibility. Randomization, through the lottery, balances observed and unobserved characteristics between the treatment and control groups. Therefore, any contrast in observed outcomes between the groups should be a consistent estimate of the treatment effect of access to Head Start.

Descriptive analysis of the sample verifies that such balance was achieved in practice.

---

[6] I summarize data from the March 2002 CPS. To allow for the closest comparison to the HSIS sample of parents with Head Start-aged children, I restrict the sample to parents of children ages 5 and below.

Table 1 breaks the sample down by cohort and test group. Columns (5) and (8) give the clearest insight into the degree of similarity between test groups. With a single exception, treatment and control groups in both cohorts are statistically indistinguishable in the initial observation. The single exception, a larger percentage of white mothers in the control group of the three-year-old cohort than in the treatment group, may simply have occurred by chance. The relative uniformity of the test groups between cohorts controls against the confounding influence of observed group characteristics in my analysis.[7]

The bottom section of Table 1 documents instances of imperfect adherence to test group assignment. While the HSIS had influence over study participants' access to Head Start, it did not enforce group assignment. Therefore, many participants assigned to the treatment group did not participate in Head Start (designated "no-shows"). Some participants assigned to the control group were able to enroll in Head Start (designated "crossovers"), often at a different Head Start center than the one designated in the HSIS random assignment. Analysis using randomized treatment group assignment gives me consistent estimators of the effects of access to Head Start, but violations of random assignment make it difficult to detect the impact of Head Start over time. While these violations may obscure Head Start's impact, any effects found to be significant using the randomized assignments can be attributed to Head Start causally.

Throughout my analysis, I consider the possible effects of participant attrition on my results. The HSIS saw lower response rates among parents in the control group in each survey year in the full sample. Inclusion in my analysis is predicated on response in the 2002 HSIS survey. Therefore, I am most interested in attrition trends within the restricted sample of mothers with complete 2002 responses. Table 2 displays OLS estimates of the effect of test group assignment on nonresponse in each survey following the baseline survey. I regress an indicator of survey nonresponse on test group assignment and the set of covariates that I use in my labor force analysis (listed in Table 3). From Table 2, it is clear that mothers in the treatment group had higher response rates in my restricted sample. The effect of group assignment is most pronounced in the four-year-old cohort, wherein the effect is statistically different from a null effect in Year 2 and marginally significant in Year 3. The effect grows over time, both in magnitude and significance.

[7] A technical note, it was the children, not parents, who were randomly assigned through lottery. The balance of the child sample described in the HSIS Final Report lends further confidence to the quality of randomization.

Table 1- Descriptive Statistics

HSIS 2002 Parent Survey Full Sample, CPS March 2002 Restricted Sample

		Full Sample	National	Three-Year-Old Cohort			Four-Year-Old Cohort		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother's Demographic	White	0.303	0.667	0.270	0.316	-2.190**	0.325	0.321	0.130
		(0.46)	(0.47)	(0.44)	(0.47)		(0.47)	(0.47)	
	Black	0.307	0.104	0.324	0.349	1.420	0.229	0.228	0.020
		(0.46)	(0.31)	(0.47)	(0.48)		(0.42)	(0.42)	
	Hispanic	0.360	0.176	0.381	0.306	0.830	0.412	0.418	-0.230
		(0.48)	(0.38)	(0.49)	(0.46)		(0.49)	(0.49)	
	Age when had first child	20.498	25.548	20.612	20.387	1.060	20.421	20.522	-0.44
		(4.45)	(5.46)	(4.58)	(4.39)		(4.37)	(4.39)	
	Married	0.447	0.806	0.424	0.443	-0.840	0.456	0.486	-1.17
		(0.50)	(0.40)	(0.49)	(0.50)		(0.50)	(0.50)	
	Born in USA	0.684	0.823	0.739	0.726	0.490	0.622	0.621	-0.44
		(0.46)	(0.38)	(0.44)	(0.45)		(0.49)	(0.49)	
	Monthly Household Income (2006 Dollars)	$1,647	$5,842	$1,609	$1,603	0.10	$1,684	$1,709	-0.36
		(1142)	(5895)	(1168)	(1137)		(1068)	(1213)	
	Highest Grade Completed	11.268	13.369	11.396	11.336	0.630	11.174	11.087	0.770
		(1.95)	(2.83)	(1.80)	(2.00)		(2.03)	(2.03)	
	High School Graduate	0.608	0.872	0.646	0.635	0.540	0.571	0.563	0.29
		(0.49)	(0.33)	(0.48)	(0.48)		(0.50)	(0.50)	
	Number of Children	2.423	2.124	2.424	2.366	1.030	2.461	2.428	0.510
		(1.22)	(1.13)	(1.24)	(1.15)		(1.25)	(1.18)	
Economic	Food Stamps	0.476	0.110	0.514	0.486	1.200	0.426	0.469	-1.650
		(0.50)	(0.31)	(0.50)	(0.50)		(0.49)	(0.50)	
	Mother Works Full Time	0.334	0.409	0.339	0.334	0.190	0.334	0.323	0.430
		(0.47)	(0.49)	(0.47)	(0.47)		(0.47)	(0.47)	
	Mother Works Part Time	0.160	0.203	0.167	0.166	0.090	0.142	0.165	-1.200
		(0.37)	(0.40)	(0.37)	(0.37)		(0.35)	(0.37)	
	Dad full time	0.745	0.875	0.748	0.707	1.380	0.765	0.750	0.490
		(0.44)	(0.33)	(0.43)	(0.46)		(0.42)	(0.43)	
	Dad part time	0.089	0.045	0.081	0.093	-0.660	0.096	0.089	0.340
		(0.28)	(0.21)	(0.27)	(0.29)		(0.29)	(0.28)	
HSIS	Crossover	0.049			0.133			0.106	
		(0.216)			(0.34)			(0.31)	
	No Show	0.113		0.164			0.223		
		(0.32)		(0.37)			(0.42)		
	N	4,442	11,513	1,464	985	2,449	1,182	811	1,993

Note: Table 1 presents descriptive statistics summarizing the HSIS sample.Column (1) summarizes full sample characteristics. Column (2) uses CPS 2002 data to describe characteristics of mothers with children under 5, a proxy for mothers of children of Head Start age. Columns (3) and (6) summarize average characteristics for parents in the treatment group for the 3 and 4 year-old cohorts, respectively. Columns (4) and (7) summarize average group characteristics for the 3 and 4 year-old cohorts, respectively. Columns (5) and (8) present regression estimates of the difference in observed characteristic by test group in standard deviations. With the exception of Columns (5) and (8) and where otherwise specified, values are given as a percentage of the sample. N provides the total sample size. For most observations, the actual number of responses to survey questions was slightly lower than N (the response rate to the 2002 HSIS parent survey was approximately 80%).

Table 2- Attrition Analysis
HSIS 2002-2006 Surveys

	Three-Year-Old Cohort (1)	Four-Year-Old Cohort (2)
Year 1	-0.00433	0.0152
	-0.0132	-0.0134
Year 2	0.018	0.0438**
	-0.0156	-0.0189
Year 3	0.0232	0.0347*
	-0.0166	-0.0188
Year 4	0.0255	
	-0.0178	
n	1572	1312

Note: Table 2 gives OLS estimates of attrition in the HSIS parent surveys. Each outcome variable is a binary indicator of survey response, The values listed in Columns (1) and (2) are the OLS estimates of the coefficient on a binary variable indicating test group, where 1 = treatment group.
*** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 3- Regression Covariates

Mother's race (Black, Hispanic)
Mother was a teen mom
Mother’s age in 2002
Mother is a high school graduate
Mother is married
Number of children

Nonresponse from parents in the control group is unsurprising. Parents in the control group do not benefit directly from inclusion in the HSIS study and may feel less compelled to complete the lengthy survey in each year. Differences between the three-year-old and four-year-old cohorts support this explanation. Participants in the three-year-old cohort had the option to reapply to Head Start in year 1, while participants in the four-year-old cohort were ineligible. A model in which models who are more connected to Head Start are more likely to complete a HSIS survey would predict greater nonresponse rates in the four-year-old cohort. If this were the case, then attrition in the three-year-old cohort will give greater weight to treatment effects on mothers in the control group who received access to Head Start, and will dampen my estimates of treatment effects in years 2-4. While this model of nonresponse fits the data, it fails to predict the effect of attrition on results within the four-year-old cohort. Thus, I rely on inclusion of covariates in my analysis to reduce variance and the robustness of my analysis to determine any bias in my estimates within the four-year-old cohort.

IV. Empirical Methodology

My analysis exploits the randomization into Head Start access in the HSIS sample.[8] I examine for the three- and four-year-old cohorts separately because "treatment" between the cohorts differs. Difference-in-means analysis yields estimates of the effect of Head Start access on an outcome measure without bias. My basic model, which I employ for each outcome domain, uses a simple linear regression:

$$y_{i,t} = \alpha + \beta\, HS_i + \sum_{t=1}^{T} \rho_t\, Year_t\, HS_i + \boldsymbol{\varphi X_i} + \varepsilon_{i,t}\,, \tag{3}$$

where $y_{i,t}$ is the outcome of interest (mother's full-time work, for example), HS_i is a dummy indicator of treatment group assignment, $Year_t$ is a dummy indicator for each year t of observation, and $Year_tHS_i$ is the interaction between the year and Head Start treatment group assignment, and $\boldsymbol{X_i}$ is a row vector containing the covariates listed in Table 3.[9] The parameter α captures the baseline level of y for the average control mother, β captures the average baseline difference in level of y between treatment and control mothers (which, from random sampling and confirmed by Table 1, should be nearly 0), $\boldsymbol{\varphi}$ is a column vector capturing covariate effects, and $\varepsilon_{i,t}$ captures the error. With the model specified as in equation (3), the ρ_t capture the effect of access to Head Start on the outcome y in year t for each year following the initial observation.

[8] I focus my analysis on mothers' labor-force participation rates (as defined by the Bureau of Labor Statistics), full-time employment status, and part-time employment status.

[9] t ranges from 1 to T, where $T = 3$ for the four-year-old cohort and $T = 4$ for the three-year-old cohort.

Equation (3) attributes any differential changes in means among treatment groups to Head Start access. However, broader economic conditions may also play a significant role in determining labor-force outcomes for the entire HSIS sample. To account for any variation in outcomes caused by changes across years, I modify the basic model as follows:

(4) $$y_{i,t} = \alpha + \beta\, HS_i + \sum_{t=1}^{T} \mu_t Year_t + \sum_{t=1}^{T} \rho_t\, Year_t\, HS_i + \boldsymbol{\varphi X_i} + \varepsilon_{i,t}$$

where all notation remains as previously defined and μ_t captures year *t* fixed effects.

I run my model with and without the covariate vector $\boldsymbol{X_i}$. The covariates I include in $\boldsymbol{X_i}$ appear in the HSIS analysis or the empirical literature. While most of the characteristics included in $\boldsymbol{X_i}$ are not time-varying (race, age when first gave birth, e.g.), some characteristics are observed in and can change each year (marital status, education level, e.g). Because these variables change over time, it is possible that observed demographic characteristics may, in fact, be correlated with Head Start treatment across time. To avoid biased estimates, I use dummy indicators for a mother's status in 2002 as the demographic covariate in the final regression. Inclusion of covariates helps my model account for unexplained variance. However, predication of my model on completion of the 2002 survey restricts the sample available for analysis. This is a significant drawback, because a smaller sample limits the variation my model can explain. Consequently, I present full sample estimates of equation (4) with and without covariates.

The empirical literature, HSIS, and theoretical model suggest that effects of Head Start should vary across subgroups. In practice, I interact Head Start treatment with demographic characteristics to study subgroup effects. The use of difference-in-means interactions preserves the statistical power of the model while allowing for subgroup analyses. This potentially reveals subgroup impacts in opposing directions hidden in full sample analysis. For example, the substitution effect of Head Start treatment may dominate labor-force outcomes for single mothers, for whom only one parent contributes to household income, while an income effect might dominate for married mothers, who have a significant source of non-labor income (spousal earnings). These opposing effects would result in a small effect when aggregated but represent treatment effects among subgroups.

I study heterogeneity in Head Start effects across groups using equation (5):

(5) $$y_{i,t} = \alpha + \beta\, HS_i + \sum_{t=1}^{T} \mu_t Year_t + \tau Group_i + \sum_{t=0}^{T} \rho_t\, Year_t\, HS_i + \sum_{t=0}^{T} \theta_t\, Year_t\, Group_i + \kappa\, HS_i Group_i + \sum_{t=0}^{T} \gamma_t\, Year_t\, HS_i\, Group_i + \boldsymbol{\varphi X_i} + \varepsilon_{i,t}\ ,$$

where $Group_i$ is a dummy indicator for subgroup and γ_t captures year-subgroup-treatment effects. The model tests the full sample for changes in y resulting from access to Head Start with greater statistical power than analysis using a restricted sample.

As I describe in Section III, I use lottery assignment into the group that receives access to Head Start as the treatment parameter, rather than Head Start attendance. Using Head Start enrollment would allow me to study directly the effect of participation in Head Start on labor-force outcomes for participating mothers. However, I would not be able to draw causal inferences from the comparisons. If participation in Head Start were correlated with some unobservable characteristic related to labor-force outcomes, my estimates would be biased. Such a scenario is easy to conceive; if mothers who want to work are more driven to seek out childcare for their children, then they might find a way to circumvent control group assignment to gain access to Head Start. Using randomized group assignment eliminates any bias introduced by imperfect group adherence. Randomization ensures that any differences in outcomes result from assignment to a group with access to Head Start. Therefore, my estimates rely on intent-to-treat (ITT) group assignment to estimate causal effects of access to Head Start on labor-force outcomes of interest.

V. Results

A. Three-Year-Old Cohort

I study "treatment" effects of Head Start access on three labor-force outcomes: labor-force participation (as defined by the Bureau of Labor Statistics), full-time employment, and part-time employment. In Table 4, I present OLS estimates of equations (3) and (4). In columns (1), (4), and (7), I regress each labor-force outcome y on the year-treatment group interaction, as specified in equation (3). I control for year fixed effects in the regression output listed in columns (2), (5), and (8). Finally, for the estimates listed in columns (3), (6), and (9), I run the full regression with year fixed effects and a set of baseline individual characteristics as covariates. I focus my attention on these estimates from the model with covariates, which is the best statistical fit with the data. In most instances, the sign of the estimates of Head Start access are consistent between the models that include year fixed effects, while the magnitudes of the magnitudes of the point estimates vary slightly. I plot these estimates in Figure 2.

I document a 3.5% increase (2.09 percentage points) in labor-force participation among sample mothers due to access to Head Start during the "treatment" year— the first year a child in the treatment group is granted access to Head Start.[10] The treatment effect on labor-force participation oscillates in the years following the treatment year; I find that access to Head Start causes greater than 6.5% (4 percentage points) increases in labor-force participation in years 2 and 4, while I observe a null effect in the third year.

A null effect of treatment on labor-force participation may arise for three different reasons: it may reflect no change in all labor-force outcomes; it may be the result of cancelling flows between full- and part-time employment and mothers leaving the work force; or it may be the result of mothers transitioning between part-time and full-time employment. The last explanation best describes treatment effects in the first year of observation. I find a statistically significant 14% (4.81 percentage points) increase in full-time employment rates for treatment mothers accompanying a 17.5% (2.92 percentage point) decrease in part-time employment. Thus, the modest change in labor-force participation rates hides a relatively large treatment effect of access to Head Start on full-time employment.

In year 3, the effects of access to Head Start on the sample decreases across outcomes. This makes sense because all children in the three-year-old cohort are able to enroll in free public kindergarten. Thus, in year 3, control mothers' budget constraints resemble those of mothers in the treatment group. The point estimates in Table 4 bear out this theoretical prediction. In year 4 of the study, however, I estimate an 18% (6.05 percentage point) increase in the likelihood of full-time employment and a 7.4% (4.16 percentage point) increase in labor-force participation among mothers in the treatment group. While not statistically significant, these estimates may be suggestive of a lag in treatment effects within the sample.

Motivated by the existing literature and the HSIS methodology, I analyze treatment-subgroup interactions to test for heterogeneity in outcomes. My theoretical model suggests that a mother's response to Head Start access depends on household income and the extent to which a mother is already working. I present estimates of the treatment-subgroup interaction effects in Table 5 and in Figures 4-5. In year 1, I find positive treatment effects for mothers already in the labor force and black mothers (who have the greatest labor-force participation rate in the pre-treatment HSIS sample). I estimate a 29% (12.8 percentage points) increase in full-time employment for black mothers resulting from access to Head Start. I find the strongest negative

[10] Hereafter, I refer to the "treatment" year (the first year an HSIS participant can access Head Start) as Year 1, the next year as Year 2, etc. I refer to the initial observation as the baseline year or pre-treatment year.

effects of Head Start access on labor-force participation rates among white mothers and mothers not in the labor force pre-treatment.[11] For all demographic subgroups, I estimate a null or small treatment effect in year 1.

Subgroup effects stemming from access to Head Start appear heterogeneous with respect to two distinct categories in the latter years of the HSIS.[12] For black mothers and single mothers, access to Head Start increases the likelihood of working full-time. For mothers, Hispanic mothers and mothers not previously in the labor force, access to Head Start leads to increased labor-force participation. Moreover, the effect appears to trend positively in the subsequent years following "treatment." Hispanic mothers had the lowest labor-force participation rate pre-treatment of all subgroups analyzed. I find a lagged positive treatment effect for Hispanic mothers on full- and part-time labor-force participation.

For the remaining subgroups (white mothers and mothers who worked before receiving access to Head Start), access to Head Start has a slight negative effect on labor-force outcomes. This effect is clearly visible in Figure 3C. In the treatment year, Head Start leads to a dramatic increase in part-time employment, which, for many of these mothers, is a reduction in labor-force participation. In the final years of the survey, these mothers are slightly less likely to be employed part-time than mothers in the control group, reflecting a response by similar mothers in the control group to universal access to public kindergarten across the sample. In the year 4 survey, white mothers and mothers in the labor force pre-treatment who received access are far less likely to participate in the labor force than their counterparts in the control group. An income effect of access to Head Start that pulls mothers out of the labor force is most pronounced among white mothers, initially. However, by the end of the study, all of these subgroups of mothers are far less likely to be in the labor force as a result of access to Head Start. I estimate a highly significant 16.6% decrease in labor-force participation during in year 4 among mothers who worked full-time before receiving access to Head Start.

The divide that appears in my analysis of heterogeneous outcomes produces the same groups formed when each of the subgroups are split by baseline real income. Black, Hispanic,

---

[11] Labor force non-participation, in this case, is not necessarily non-productive; mothers who are enrolled in education or job training programs are considered "not working," for example.

[12] I study heterogeneous effects using subgroups that appear frequently in the empirical literature studying mothers' sensitivity to childcare subsidies. However, these subgroups are not mutually exclusive. For example, I study treatment-race and treatment-marital status interactions. I find a higher incidence of single parent households among black mothers than white and Hispanic mothers.

and single mothers, on average, reported income below the average income for the full HSIS sample in the pre-treatment survey, while mothers in the labor force and white mothers reported average monthly incomes above the full HSIS average in the pre-treatment survey. Though many estimates are not statistically significant, such clustering of outcomes among is suggestive of heterogeneous responses to Head Start. I consider the theoretical explanations of such heterogeneity with respect to the labor model in Section VII.

B. Four-Year-Old Cohort

I repeat my analysis with the four-year-old cohort and give estimates of the basic model in Table 6 and Figure 5. I disaggregate the data by cohort because access to Head Start differs between cohorts during the HSIS observation period. As with the three-year-old cohort, I observe positive treatment effects on labor-force participation during the treatment year. I estimate that access to Head Start led to a 10.9% (6.46 percentage points) increase in mothers' labor-force participation in the "treatment year." The year 1 treatment effect on labor-force participation is driven by modest increases in both part- and full-time employment, as well as mothers looking for work. In year 2, the aggregate treatment effects dampen to small and statistically indistinguishable from null levels, as is the case in the three-year-old cohort. This dissipation is consistent across cohorts.[13]

In my heterogeneity analysis, I find that subgroup-treatment interaction effects for the four-year-old cohort resemble those in the three-year-old cohort in sign. I present estimates of treatment-subgroup interaction effects in Table 7 and plot them in Figures 6-7.[14] Treatment effects are positive for single mothers and black mothers in year 1, particularly effects on full-time employment. For black mothers in the sample, I observe a statistically significant 41% (18.6 percentage points) increase in full-time employment due to Head Start access in the "treatment" year. Hispanic mothers in the treatment group were 7.3% more likely to be working part-time than Hispanic mothers in the control group in year 1, a 48.2% increase in part-time participation. White mothers in the treatment group, meanwhile, were less likely to work in year 1 than white

---

[13] Because the HSIS follows participants through first grade, the HSIS collects one fewer observation for the four-year-old cohort than the three-year-old cohort. Thus, a participant enters kindergarten in year 2 of the study.

[14] As in my heterogeneity analysis of the three-year-old cohort, I graph subgroup separately based on subgroup average baseline monthly income. Figure 6 plots estimates of treatment-subgroup interactions for subgroups with average income above the HSIS average, while Figure 7 plots estimates of treatment-subgroup interactions for subgroups with average income below the HSIS average.

mothers in the control group; I observe a marginally significant 32.5% (10.1 percentage points) decrease in full-time employment among white mothers in the treatment group.

Subgroup effects in years 2 and 3 are slightly less volatile for the four-year-old cohort than in the three-year-old cohort. Similar to the three-year-old cohort, treatment effects bend slightly towards 0% for most subgroups across outcomes. None of the treatment effects are statistically distinguishable from null effects in year 2. In year 3, I observe a slight divergence from null effects. For many subgroups of mothers, the treatment-subgroup interaction in year 3 is similar in magnitude and direction to the year 1 treatment effect, although the effects are estimated with less precision.

C. Robustness Check

A few adjustments test the robustness of my model. To ensure that my results do not arise from my choice of covariates or omitted variable bias, I run my full sample regression different covariate controls. I exclude variables used in my final model, substitute complementary observations (marital status in 2002 for both parents in the household in 2002, e.g.), and add other demographic variables observed in the HSIS dataset (pre-treatment depression status, nationality, e.g.).[15] Qualitatively, my results are unchanged, though the point estimates vary as expected across the different model specifications. In general, the sample is unchanged when covariates are added or removed, as the sample still relies on survey response in 2002.

I also check the robustness of my estimates by limiting the sample to parents who completed the HSIS survey in each year. Restricting the sample to HSIS participants with a full set of observations reduces the noise from inconsistent sample response, but significantly decreases the sample size. I compare OLS estimates from the restricted and full samples in Figure 8. With the exception of Figure 8E, my OLS estimates appear to be consistent with the restricted sample. In both cohorts, I find that restricting the sample to mothers who report in each year dampens the magnitude of my estimated Head Start treatment effect slightly, but they are generally consistent in sign.

[15] Omitting and adding demographic variables generally leaves the sample unchanged. First, I study robustness to the set of covariates listed in Table 3. Next, I consider replacements for variables listed in Table 3. For example, I use an indicator variable coding for both parents living in the household to replace marital status and an indicator for the HSIS participant child as the oldest child in the household to replace number of children in household. Finally, I run my regressions using the covariates listed in Table 3 with other observed demographic characteristics from the 2002 pre-treatment survey, including depression status, nationality, language spoken at home, and real income. My analysis is robust with respect to these specifications of the model.

Table 4- Annual Effects of Head Start Access on Labor Force Outcomes
HSIS Three-Year-Old Cohort

	In Labor Force			Full Time			Part Time		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean Pre-Treatment		0.591			0.337			0.167	
Treatment-Year 1	0.00317	0.00424	0.0209	-0.0189	0.0234	0.0481**	-0.00755	-0.0418**	-0.0292
	(0.0160)	(0.0226)	(0.0261)	(0.0159)	(0.0214)	(0.0244)	(0.0132)	(0.0199)	(0.0229)
Treatment- Year 2	0.0258	0.0170	0.0417	0.0138	-0.00103	0.0179	0.0121	0.00507	0.0140
	(0.0165)	(0.0260)	(0.0295)	(0.0167)	(0.0256)	(0.0295)	(0.0142)	(0.0220)	(0.0255)
Treatment- Year 3	0.0345**	-0.0161	-0.00684	0.0352**	-0.0401	-0.00755	-0.00500	0.0111	-0.00795
	(0.0166)	(0.0264)	(0.0299)	(0.0170)	(0.0270)	(0.0310)	(0.0140)	(0.0226)	(0.0258)
Treatment- Year 4	0.0546***	0.0410	0.0436	0.104***	0.0381	0.0605*	-0.00147	-0.00143	-0.0269
	(0.0168)	(0.0264)	(0.0301)	(0.0175)	(0.0275)	(0.0319)	(0.0144)	(0.0232)	(0.0275)
Year Fixed Effects	N	Y	Y	N	Y	Y	N	Y	Y
Covariates	N	N	Y	N	N	Y	N	N	Y
n-observations	9,404	9,404	6,803	9,404	9,404	6,803	9,404	9,404	6,803
n-mothers	2,218	2,218	1,573	2,218	2,218	1,573	2,218	2,218	1,573
R^2	0.002	0.003	0.097	0.005	0.010	0.063	0.000	0.001	0.005

Note: Table 4 presents OLS regression estimates for the effects of access to Head Start on the labor force outcomes listed in the top row. Standard errors are adjusted for heteroskedasticity. The unit of observation is mothers in the HSIS sample by year. I cluster point estimates by mother's participant ID in each regression. Each observation is measured as a binary {0,1} outcome, and the treatment-year effect represents the change in average outcome participation for mothers in the treatment group during year t. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Figure 2- Labor Outcomes, 3 Year-Old Cohort

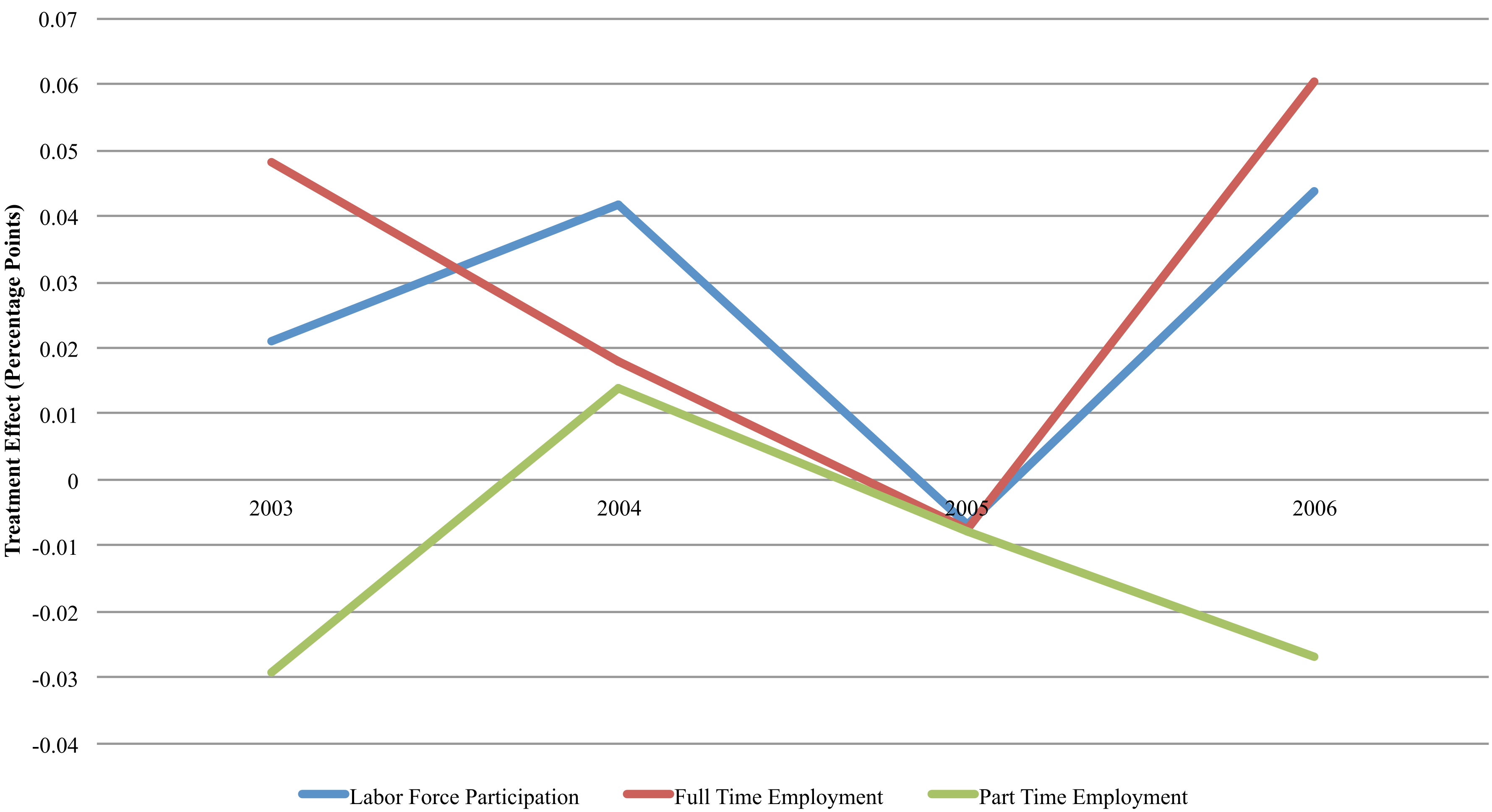


Table 5 - Annual Effects of Head Start Access on Labor Force Outcomes, Heterogeneity
HSIS Three-Year-Old Cohort

PANEL A. DEMOGRAPHIC CHARACTERISTICS	WHITE			BLACK			HISPANIC			SINGLE		
	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Subgroup Mean Pre-Treatment	0.525	0.262	0.174	0.726	0.448	0.151	0.503	0.284	0.173	0.663	0.376	0.164
Mean Real Income Pre-Treatment	$1,745			$1,398			$1,653			$1,317		
Treatment-Year 1	-0.0475	-0.108**	0.0524	0.00925	0.128**	-0.0370	0.0188	-0.0296	-0.0117	0.00784	-0.00804	-0.0240
	(0.0583)	(0.0508)	(0.0486)	(0.0531)	(0.0526)	(0.0452)	(0.0525)	(0.0473)	(0.0469)	(0.0512)	(0.0477)	(0.0449)
Treatment- Year 2	-0.0548	-0.0762	0.0763	-0.0330	0.0317	-0.0762	0.0879	0.0500	0.00235	-0.0188	0.000271	-0.0564
	(0.0629)	(0.0628)	(0.0556)	(0.0624)	(0.0631)	(0.0523)	(0.0601)	(0.0578)	(0.0518)	(0.0582)	(0.0582)	(0.0508)
Treatment- Year 3	-0.0606	-0.0656	0.0132	0.0196	0.0151	0.0124	0.0491	0.0520	-0.00843	0.0154	0.0151	-0.0398
	(0.0663)	(0.0623)	(0.0546)	(0.0603)	(0.0673)	(0.0529)	(0.0632)	(0.0644)	(0.0540)	(0.0590)	(0.0614)	(0.0519)
Treatment- Year 4	-0.0674	-0.000686	-0.0205	-0.0244	0.0390	-0.0683	0.113*	-0.0181	0.0879	-0.0188	0.0491	-0.0964*
	(0.0674)	(0.0670)	(0.0611)	(0.0591)	(0.0671)	(0.0553)	(0.0630)	(0.0646)	(0.0541)	(0.0591)	(0.0626)	(0.0545)
n-observations	6,829	6,829	6,829	6,829	6,829	6,829	6,829	6,829	6,829	7,012	7,012	7,012
n-mothers	1339	1339	1339	1339	1339	1339	1339	1339	1339	1339	1339	1339
R^2	0.085	0.057	0.006	0.097	0.062	0.007	0.076	0.046	0.007	0.097	0.063	0.007

PANEL B. PRE-TREATMENT LABOR STATUS	In Labor Force			Full Time			Part Time			Not Working		
	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time
	(1)	(2)	(3)	(4)	(5)	(6)	(10)	(11)	(12)	(7)	(8)	(9)
Subgroup Mean Pre-Treatment	1	0.570	0.282	1	1	0	1	0	1	0	0	0
Mean Real Income Pre-Treatment	$1,691			$1,920			$1,578			$1,423		
Treatment-Year 1	0.0457	0.0235	0.0430	0.0360	-0.0295	0.0766*	-0.0925	-0.0368	-0.0531	-0.0457	-0.0235	-0.0191
	(0.0490)	(0.0402)	(0.0396)	(0.0460)	(0.0495)	(0.0408)	(0.0575)	(0.0654)	(0.0701)	(0.0490)	(0.0402)	(0.0290)
Treatment- Year 2	0.0286	-0.0126	0.0530	-0.0753	-0.133**	0.0575	0.0147	0.109	0.0588	-0.0286	0.0126	-0.0215
	(0.0554)	(0.0511)	(0.0454)	(0.0520)	(0.0554)	(0.0454)	(0.0674)	(0.0728)	(0.0686)	(0.0554)	(0.0511)	(0.0359)
Treatment- Year 3	0.0223	-0.0266	-0.0400	0.0183	3.83e-05	0.00846	-0.0128	-0.126	0.0432	-0.0223	0.0266	0.0106
	(0.0575)	(0.0562)	(0.0458)	(0.0520)	(0.0573)	(0.0466)	(0.0662)	(0.0771)	(0.0685)	(0.0575)	(0.0562)	(0.0382)
Treatment- Year 4	-0.103*	-0.00289	-0.0792*	-0.166***	-0.0819	-0.0570	0.0107	0.00831	0.0133	0.103*	0.00289	0.0199
	(0.0574)	(0.0575)	(0.0479)	(0.0520)	(0.0578)	(0.0488)	(0.0689)	(0.0785)	(0.0680)	(0.0574)	(0.0575)	(0.0411)
n-observations	6,738	6,738	6,738	6,738	6,738	6,738	6,738	6,738	6,738	6,738	6,738	6,738
n-mothers	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294
R^2	0.226	0.380	0.041	0.349	0.173	0.035	0.146	0.084	0.282	0.349	0.173	0.041

Note: Table 5 presents OLS regression estimates for treatment-subgroup interactions. Pre-treatment mean observations are collected from the 2002 HSIS survey. Standard errors are adjusted for heteroskedasticity. The unit of observation is mothers in the HSIS sample by year. I cluster point estimates by mother's participant ID in each regression. Each observation is measured as a binary {0,1} outcome, and the treatment-year effect represents the change in average outcome participation for mothers in the treatment group during year t. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

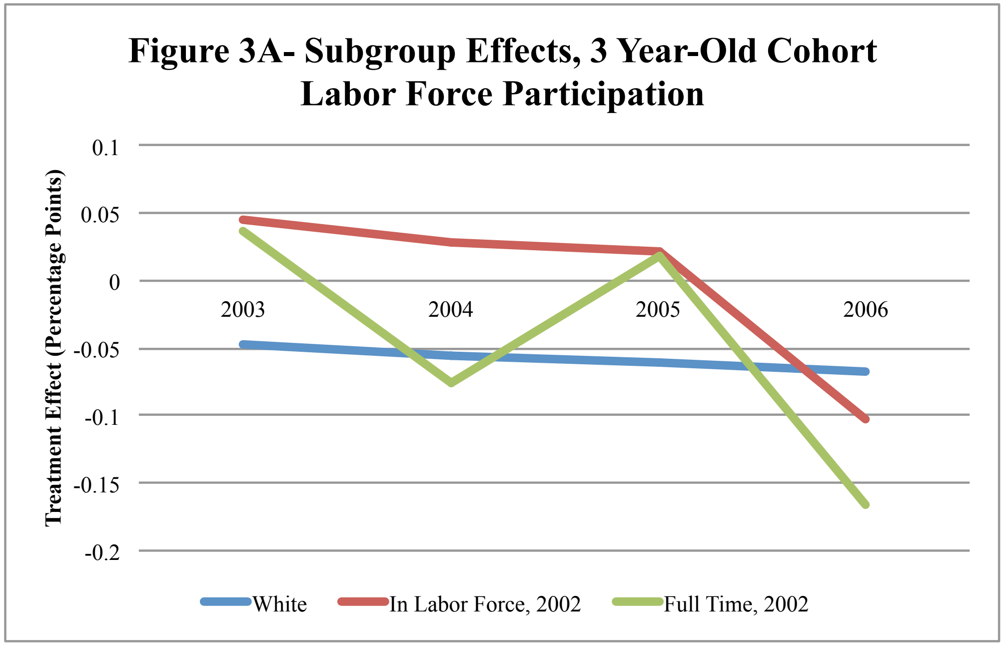


Figure 3A- Subgroup Effects, 3 Year-Old Cohort Labor Force Participation

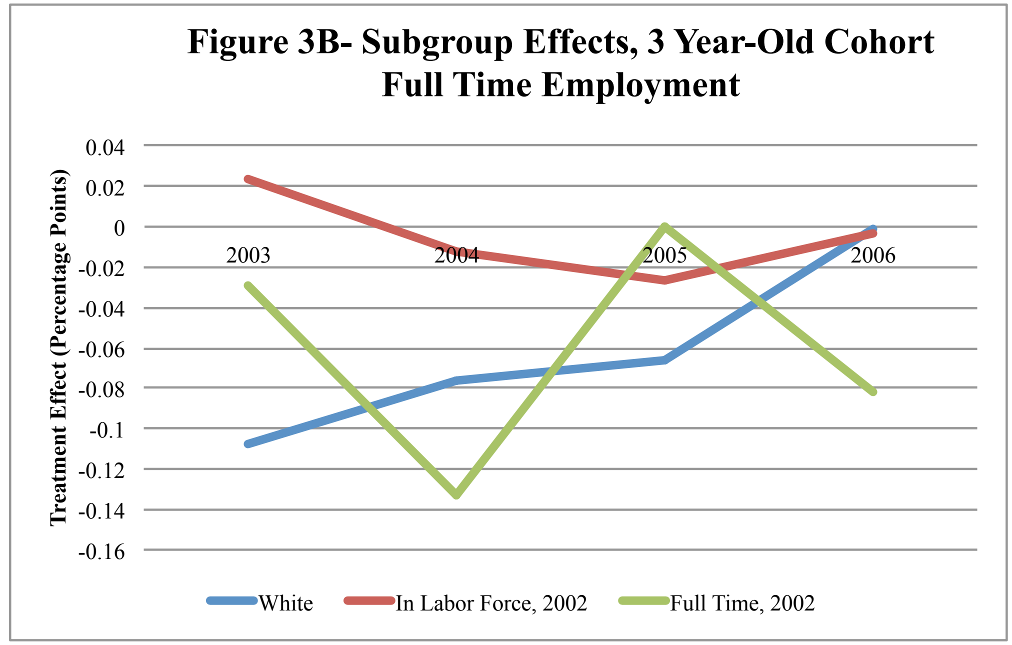


Figure 3B- Subgroup Effects, 3 Year-Old Cohort Full Time Employment

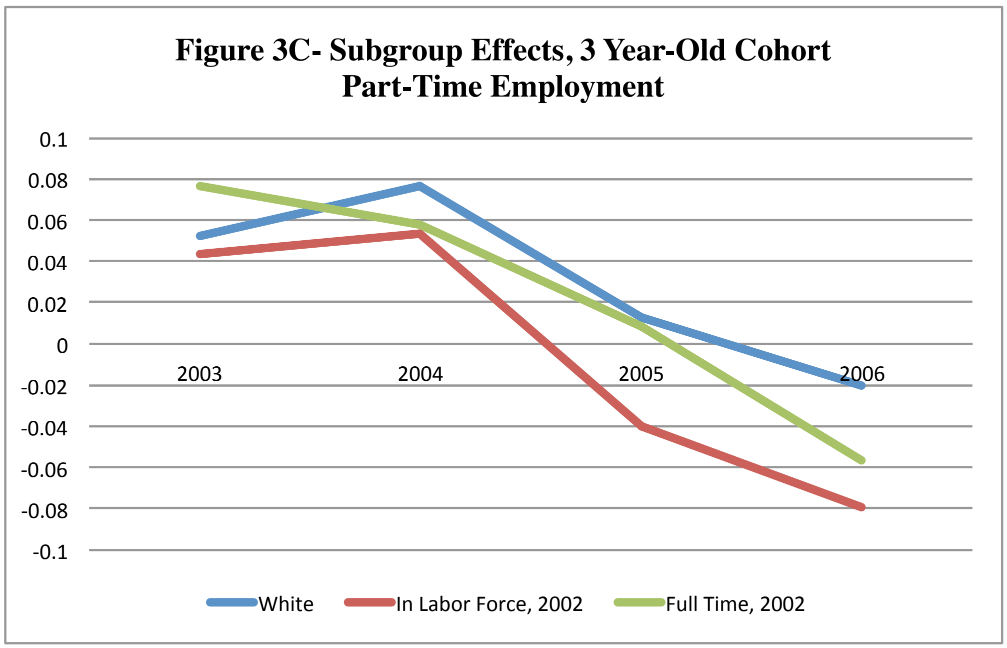


Figure 3C- Subgroup Effects, 3 Year-Old Cohort Part-Time Employment

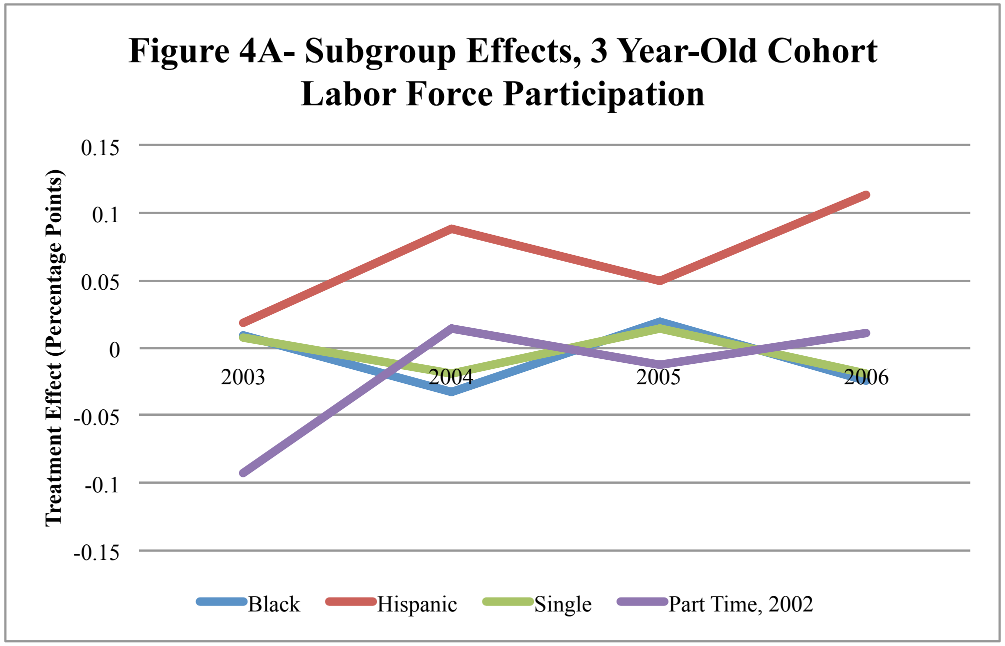


Figure 4A- Subgroup Effects, 3 Year-Old Cohort
Labor Force Participation
Treatment Effect (Percentage Points)
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
2003
2004
2005
2006
Black
Hispanic
Single
Part Time, 2002

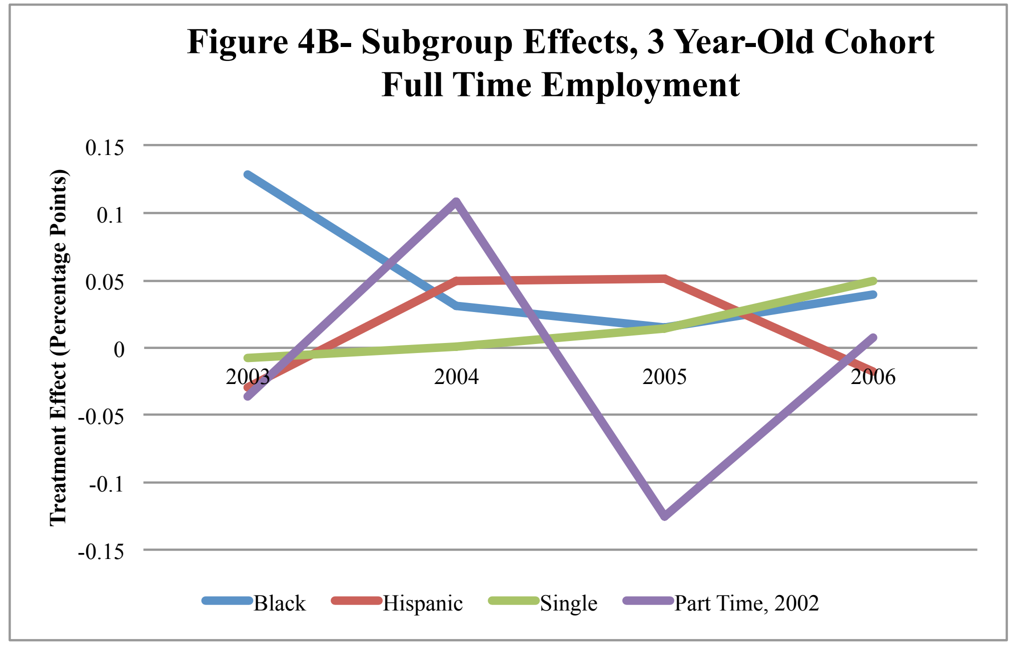


Figure 4B- Subgroup Effects, 3 Year-Old Cohort
Full Time Employment
Treatment Effect (Percentage Points)
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
2003
2004
2005
2006
Black
Hispanic
Single
Part Time, 2002

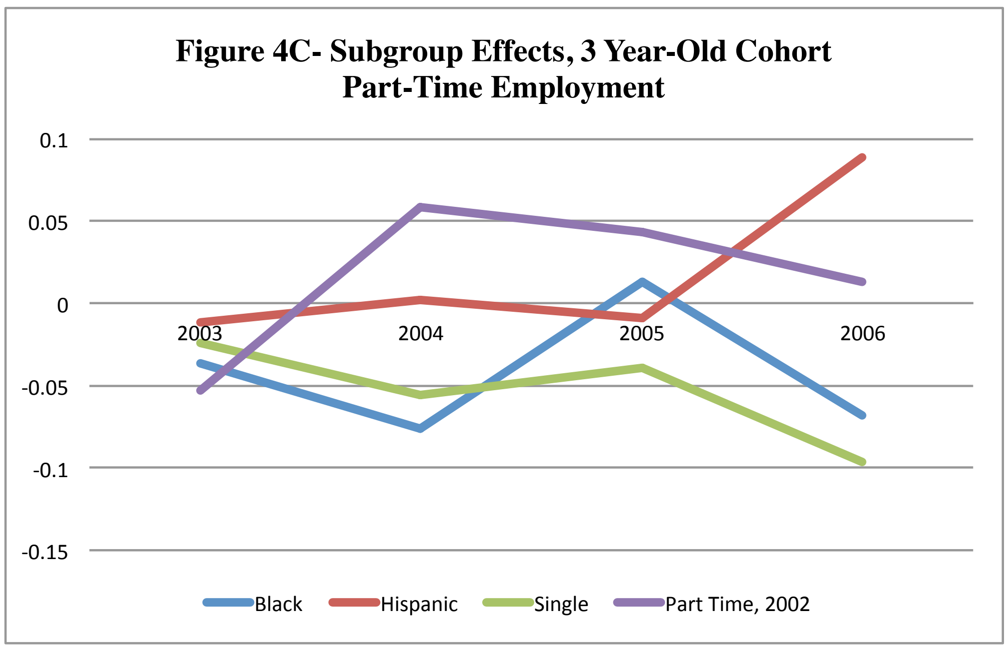


Figure 4C- Subgroup Effects, 3 Year-Old Cohort
Part-Time Employment
0.1
0.05
0
-0.05
-0.1
-0.15
2003
2004
2005
2006
Black
Hispanic
Single
Part Time, 2002

Table 6- Annual Effects of Head Start Access on Labor Force Outcomes
HSIS Four-Year-Old Cohort

	In Labor Force			Full Time			Part Time		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean Pre-Treatment	0.591			0.337			0.151		
Treatment-Year 1	0.0130	0.0350	0.0646**	-0.00931	-0.00919	0.0133	0.0119	0.0315	0.0379
	(0.0178)	(0.0250)	(0.0277)	(0.0167)	(0.0242)	(0.0263)	(0.0146)	(0.0223)	(0.0249)
Treatment- Year 2	0.000371	0.00574	0.0158	0.00467	-0.0169	-0.00206	-0.0237	-0.00481	-0.0229
	(0.0189)	(0.0291)	(0.0324)	(0.0181)	(0.0286)	(0.0313)	(0.0148)	(0.0254)	(0.0281)
Treatment- Year 3	0.0413**	0.0196	0.00476	0.0511***	0.00725	-0.0138	-0.00175	0.00583	0.00622
	(0.0189)	(0.0301)	(0.0337)	(0.0187)	(0.0306)	(0.0344)	(0.0154)	(0.0263)	(0.0289)
Year Fixed Effects	N	Y	Y	N	Y	Y	N	Y	Y
Covariates	N	N	Y	N	N	Y	N	N	Y
n-observations	5,995	5,995	4582	5,995	5,995	4582	5,995	5,995	4582
n-mothers	1,733	1,733	1,338	1,733	1,733	1,338	1,733	1,733	1,338
R^2	0.001	0.002	0.088	0.002	0.002	0.059	0.000	0.001	0.013

Note: Table 6 presents OLS regression estimates for the effects of access to Head Start on the labor force outcomes listed in the top row. Standard errors are adjusted for heteroskedasticity. The unit of observation is mothers in the HSIS sample by year. I cluster point estimates by mother's participant ID in each regression. Each observation is measured as a binary {0,1} outcome, and the treatment-year effect represents the change in average outcome participation for mothers in the treatment group during year t. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

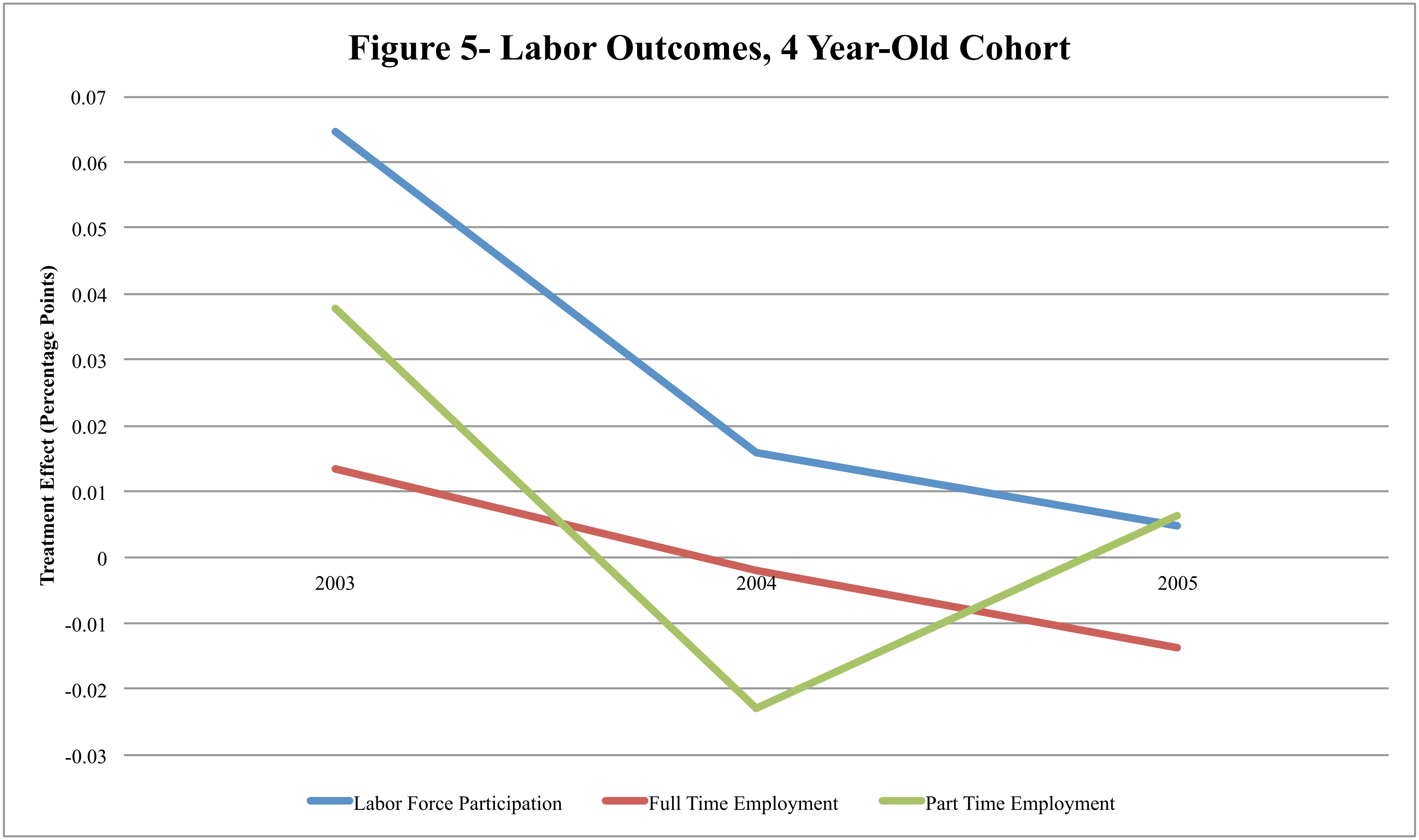


Figure 5- Labor Outcomes, 4 Year-Old Cohort
Treatment Effect (Percentage Points)
0.07
0.06
0.05
0.04
0.03
0.02
0.01
0
-0.01
-0.02
-0.03
2003
2004
2005
Labor Force Participation
Full Time Employment
Part Time Employment

Table 7- Annual Effects of Head Start Access on Labor Force Outcomes, Heterogeneity
HSIS- Four-Year-Old Cohort

PANEL A. DEMOGRAPHIC CHARACTERISTICS	WHITE			BLACK			HISPANIC			SINGLE		
	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Subgroup Mean Pre-Treatment	0.556	0.310	0.176	0.746	0.457	0.112	0.486	0.281	0.152	0.669	0.397	0.141
Mean Real Income Pre-Treatment	$1,745			$1,398			$1,653			$1,317		
Treatment-Year 1	-0.0523	-0.101*	-0.0339	0.0595	0.186**	-0.0729	-0.0163	-0.00601	0.0734	0.0254	0.0921*	-0.0409
	(0.0573)	(0.0553)	(0.0538)	(0.0740)	(0.0736)	(0.0597)	(0.0542)	(0.0512)	(0.0487)	(0.0551)	(0.0537)	(0.0482)
Treatment- Year 2	-0.0521	-0.0696	0.0193	0.0479	0.0658	-0.0223	-0.0105	0.00792	0.0135	0.108*	0.0945	-0.00311
	(0.0692)	(0.0677)	(0.0622)	(0.0791)	(0.0855)	(0.0693)	(0.0643)	(0.0614)	(0.0572)	(0.0652)	(0.0646)	(0.0562)
Treatment- Year 3	-0.0407	-0.142*	-0.00342	0.0556	0.120	-0.0254	-0.0640	0.0236	0.0146	0.0259	0.0368	-0.00749
	(0.0736)	(0.0743)	(0.0641)	(0.0810)	(0.0885)	(0.0686)	(0.0673)	(0.0672)	(0.0587)	(0.0674)	(0.0701)	(0.0581)
n-observations	4,665	4,665	4,665	4,665	4,665	4,665	4,665	4,665	4,665	4,774	4,774	4,774
n-mothers	1338	1338	1338	1338	1338	1338	1338	1338	1338	1338	1338	1338
R^2	0.072	0.056	0.011	0.087	0.061	0.013	0.075	0.053	0.011	0.087	0.059	0.012

PANEL B. PRE-TREATMENT LABOR STATUS	In Labor Force			Full Time			Part Time			Not Working		
	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time	In Labor Force	Full Time	Part Time
	(1)	(2)	(3)	(4)	(5)	(6)	(10)	(11)	(12)	(7)	(8)	(9)
Subgroup Mean Pre-Treatment	1	0.584	0.267	1	1	0	1	0	1	0	0	0
Mean Real Income Pre-Treatment	$1,691			$1,920			$1,749			$1,423		
Treatment-Year 1	0.00873	0.0286	0.0612	0.0112	0.0915*	-0.0393	0.0220	-0.109	0.156**	0.0387	0.0333	-0.0612
	(0.0242)	(0.0365)	(0.0442)	(0.0513)	(0.0546)	(0.0458)	(0.0666)	(0.0720)	(0.0784)	(0.0506)	(0.0429)	(0.0442)
Treatment- Year 2	-0.0387	-0.0333	0.0136	-0.0629	-0.0387	0.00740	-0.0980	-0.00160	-0.0469	0.0862	0.0866	-0.0136
	(0.0506)	(0.0429)	(0.0524)	(0.0588)	(0.0621)	(0.0531)	(0.0798)	(0.0842)	(0.0776)	(0.0596)	(0.0535)	(0.0524)
Treatment- Year 3	-0.0862	-0.0866	0.0761	-0.0821	-0.0387	-0.0564	0.0447	-0.101	0.0945	0.0203	0.129**	-0.0761
	(0.0596)	(0.0535)	(0.0541)	(0.0605)	(0.0641)	(0.0554)	(0.0803)	(0.0868)	(0.0758)	(0.0627)	(0.0603)	(0.0541)
n-observations	4,627	4,627	4,627	4,627	4,627	4,627	4,627	4,627	4,627	4,627	4,627	4,627
n-mothers	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294	1294
R^2	0.379	0.192	0.055	0.249	0.450	0.055	0.145	0.084	0.320	0.379	0.192	0.015

Note: Table 7 presents OLS regression estimates for treatment-subgroup interactions. Pre-treatment mean observations are collected from the 2002 HSIS survey. Standard errors are adjusted for heteroskedasticity. The unit of observation is mothers in the HSIS sample by year. I cluster point estimates by mother's participant ID in each regression. Each observation is measured as a binary {0,1} outcome, and the treatment-year effect represents the change in average outcome participation for mothers in the treatment group during year t. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

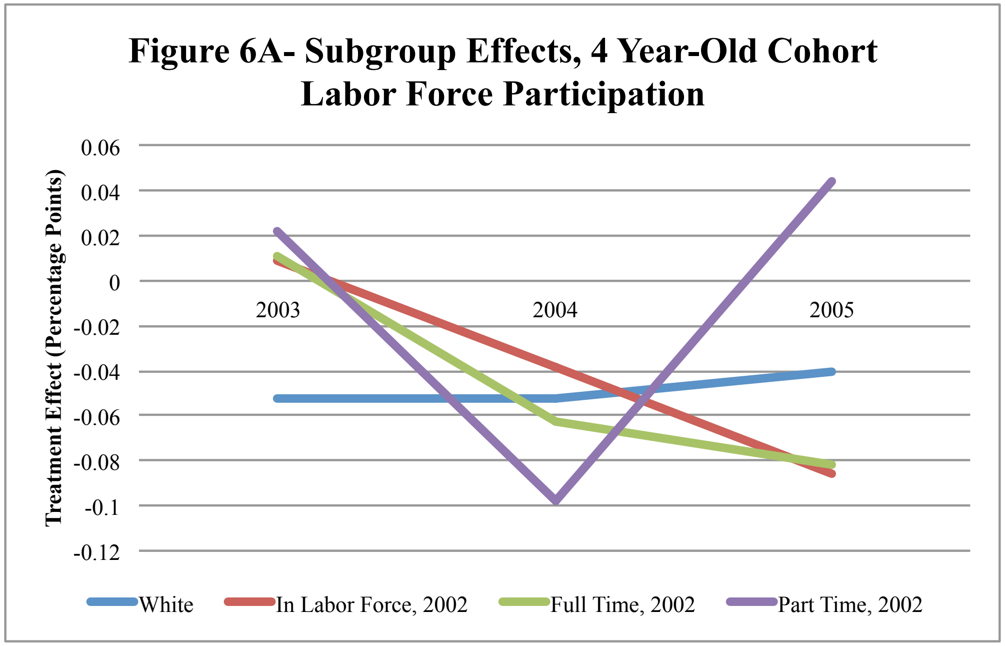


Figure 6A- Subgroup Effects, 4 Year-Old Cohort
Labor Force Participation
Treatment Effect (Percentage Points)
0.06
0.04
0.02
0
-0.02
-0.04
-0.06
-0.08
-0.1
-0.12
2003
2004
2005
White
In Labor Force, 2002
Full Time, 2002
Part Time, 2002

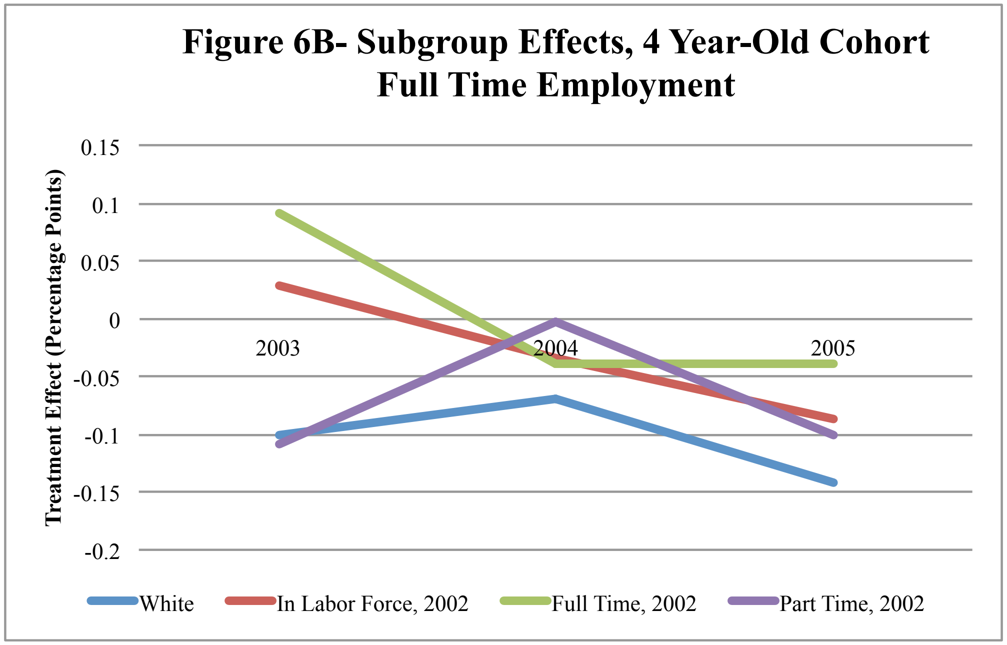


Figure 6B- Subgroup Effects, 4 Year-Old Cohort
Full Time Employment
Treatment Effect (Percentage Points)
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-0.2
2003
2004
2005
White
In Labor Force, 2002
Full Time, 2002
Part Time, 2002

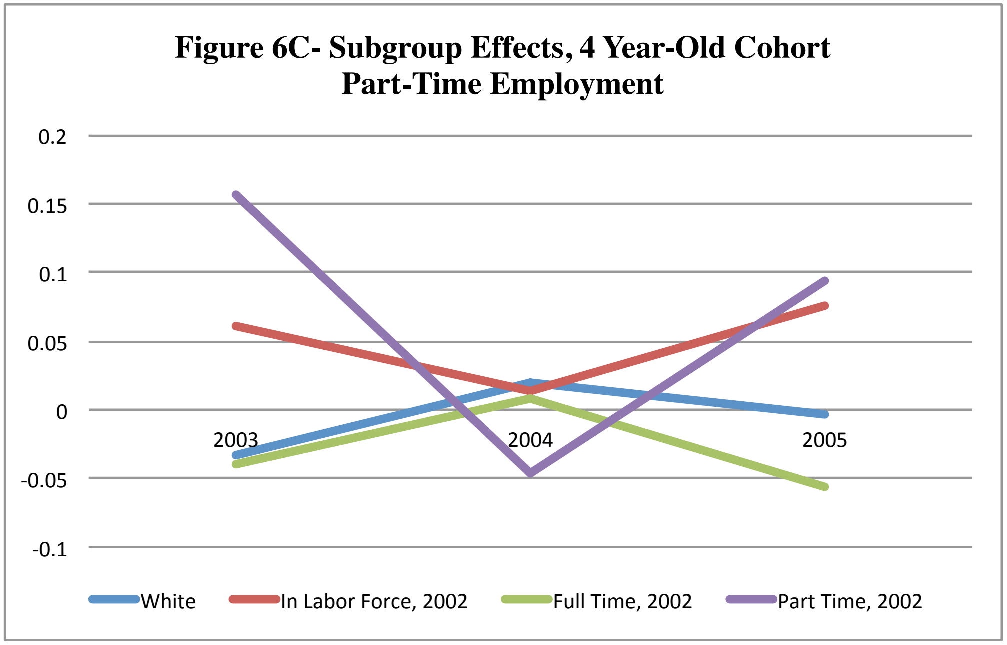


Figure 6C- Subgroup Effects, 4 Year-Old Cohort
Part-Time Employment
0.2
0.15
0.1
0.05
0
-0.05
-0.1
2003
2004
2005
White
In Labor Force, 2002
Full Time, 2002
Part Time, 2002

Figure 7A- Subgroup Effects, 4 Year-Old Cohort
Labor Force Participation

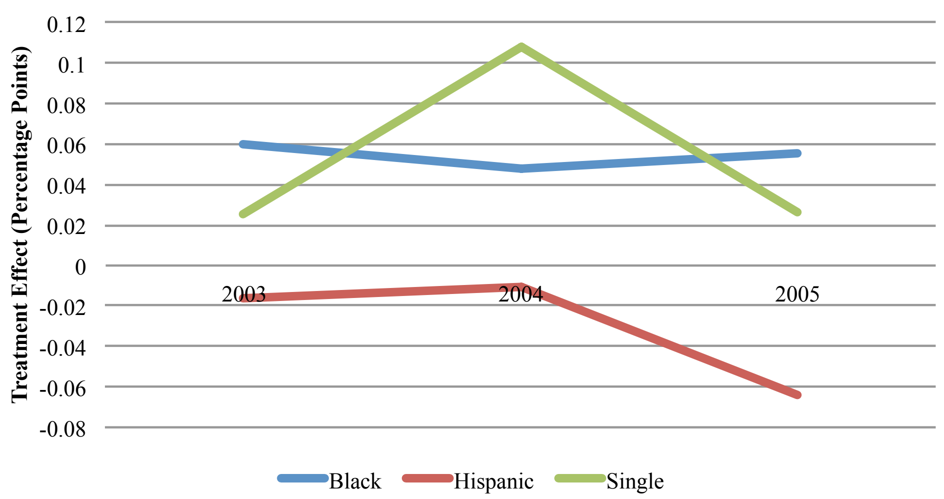


Figure 7B- Subgroup Effects, 4 Year-Old Cohort
Full Time Employment

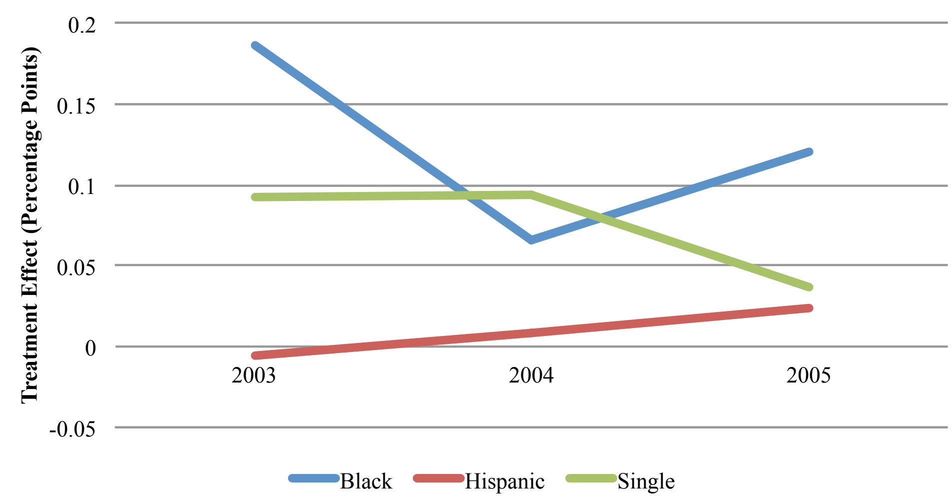
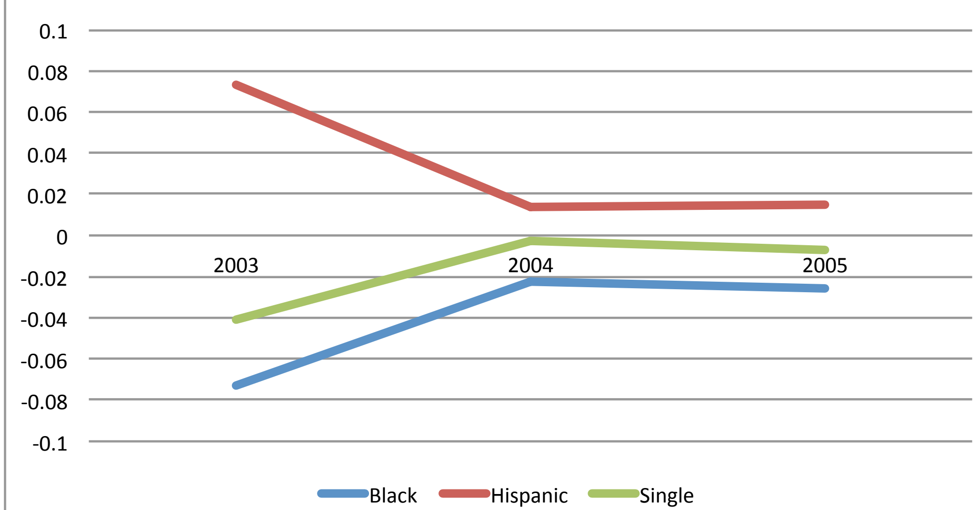


Figure 7C- Subgroup Effects, 4 Year-Old Cohort
Part-Time Employment



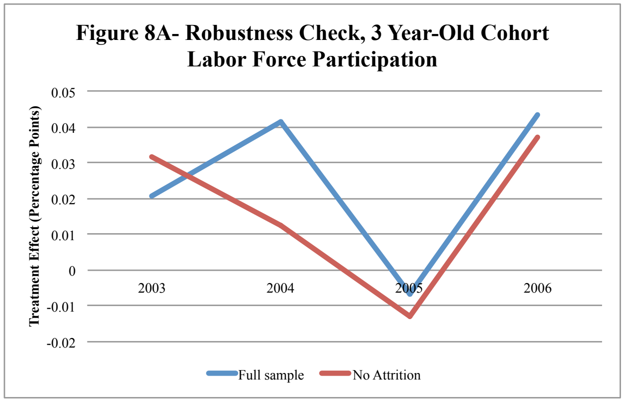


Figure 8A- Robustness Check, 3 Year-Old Cohort
Labor Force Participation
Treatment Effect (Percentage Points)
0.05
0.04
0.03
0.02
0.01
0
-0.01
-0.02
2003
2004
2005
2006
Full sample
No Attrition

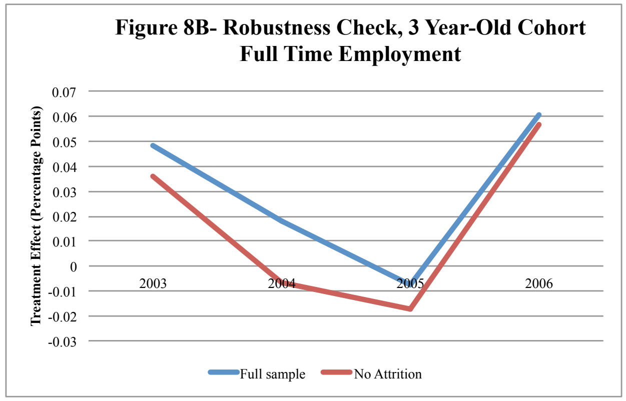


Figure 8B- Robustness Check, 3 Year-Old Cohort
Full Time Employment
Treatment Effect (Percentage Points)
0.07
0.06
0.05
0.04
0.03
0.02
0.01
0
-0.01
-0.02
-0.03
2003
2004
2005
2006
Full sample
No Attrition

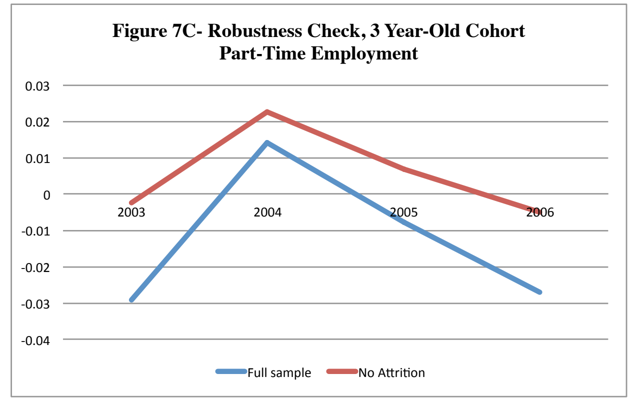


Figure 7C- Robustness Check, 3 Year-Old Cohort
Part-Time Employment
0.03
0.02
0.01
0
-0.01
-0.02
-0.03
-0.04
2003
2004
2005
2006
Full sample
No Attrition

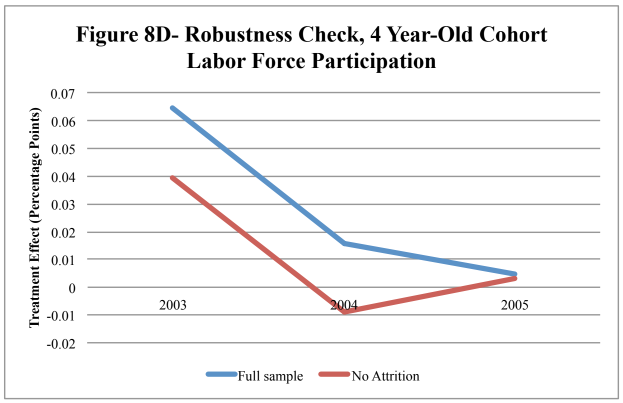


Figure 8D- Robustness Check, 4 Year-Old Cohort
Labor Force Participation
Treatment Effect (Percentage Points)
0.07
0.06
0.05
0.04
0.03
0.02
0.01
0
-0.01
-0.02
2003
2004
2005
Full sample
No Attrition

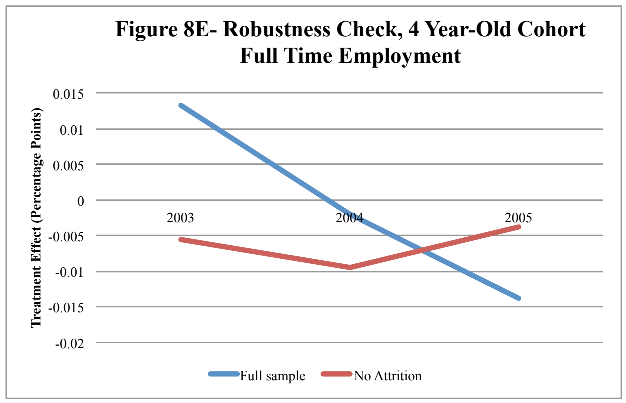


Figure 8E- Robustness Check, 4 Year-Old Cohort
Full Time Employment
Treatment Effect (Percentage Points)
0.015
0.01
0.005
0
-0.005
-0.01
-0.015
-0.02
2003
2004
2005
Full sample
No Attrition

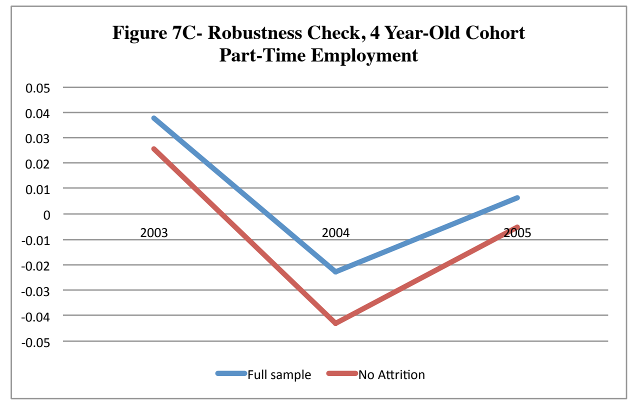


Figure 7C- Robustness Check, 4 Year-Old Cohort
Part-Time Employment
0.05
0.04
0.03
0.02
0.01
0
-0.01
-0.02
-0.03
-0.04
-0.05
2003
2004
2005
Full sample
No Attrition

VI. Discussion

For both the three-year-old and four-year-old cohorts, I find suggestive evidence of positive labor-force outcomes resulting from Head Start during the first year of access. These estimates align with predictions of the standard labor-leisure model. Mothers who can enroll their children in Head Start face a higher effective wage than when they must pay for child care. These mothers work for a greater take home wage, pulling mothers who were previously not working into the work force and, to a certain extent, creating an incentive for mothers already in the labor force to increase their labor-force participation. The statistically significant increases in full-time employment among mothers in the three-year-old cohort and labor-force participation among mothers in the four-year-old cohort with access to Head Start are strong evidence of these positive effects.

By the time a child enters kindergarten, the effects begin to dissipate in both cohorts. Thus, the budget constraints of mothers who are not assigned access to Head Start resemble those of mothers in the treatment group after first year of observation. For both cohorts, treatment effects across outcomes in the kindergarten year are small and not statistically different from a null effect. In my theoretical model, I propose that any changes in labor-force outcomes following the treatment year result from returns to labor-force experience in the treatment year. The null treatment effects on labor-force participation in the kindergarten year suggest that the returns to experience are small or insignificant for Head Start mothers.

In the year 4 observations of the three-year-old cohort, I find a resurgence of positive treatment effects on labor-force participation driven by a shift from mothers not working pre-HSIS. These effects are different from those in the four-year-old cohort. I propose three explanations of this trend. First, the increase in full-time labor-force participation could be the result of an extra year of access to the professional development services provided by Head Start, which helps parents identify and achieve their economic goals. Mothers in the four-year-old cohort only have one year of access to these services, which may be insufficient to yield the long-term treatment effects apparent in the three-year-old cohort. Alternatively, differences in long-term treatment outcomes between the three- and four-year-old cohorts may be a result of the length of observation. With further data, treatment effects may re-emerge with the same lag as in the three-year-old cohort. Finally, with respect to the theoretical model, it may be the case that low-income and low-educated mothers experience delayed returns to tenure.

The theoretical model suggests that mothers should have an immediate response when given access to Head Start. It may not always be possible for mothers to have such an elastic response, in practice. A mother's ability to enter the workforce depends on the aggregate labor demand and her employment qualifications. Therefore, a mother's transition into the labor force may be a protracted process of training (which is not counted as labor-force participation) and job searching. Year 1 observations, on average, occurred 6 months after the initial observation. Thus, some lag in effects may be explained by the dynamics of labor-force participation not considered in the theoretical model.

The effects of Head Start treatment were heterogeneous across subgroups. For some groups of mothers, including black mothers and mothers not working before Head Start treatment, access to Head Start resulted in large increases in labor-force participation (particularly full-time employment for black mothers and part-time employment for mothers not working pre-treatment). For white mothers, access to Head Start actually decreased the likelihood of labor-force participation, pulling mothers who worked full-time before treatment out of the labor force altogether. These subgroup effects are similar across both cohorts. When I split the subgroups along the mean wage and education levels, the outcomes appear somewhat more homogeneous. White mothers and mothers working before treatment had higher household income levels and were more likely than the HSIS average to be high school graduates. Meanwhile black mothers, single mothers, and mothers not working in the baseline year were, on average, less educated and less wealthy than the average mother in the HSIS sample.

These heterogeneous effects are also consistent with the theoretical model. The model suggests that income effects dominate for mothers closest to the Head Start eligibility threshold because additional income would make them ineligible for a Head Start subsidy. Labor-force outcomes for white mothers, the wealthiest subgroup and the subgroup with a household income closest to the eligibility cutoff, support this prediction at statistically significant levels in both cohorts. For mothers in the labor force furthest from the eligibility threshold, the model is unable to predict the direction of a treatment effect. For black mothers, access to Head Start led to increases in labor-force participation. For single mothers and Hispanic mothers, the effects were less clear.

This study has a few limitations that motivate continued study. As is common in experimental studies, HSIS group assignment did not guarantee treatment. Such non-compliant parents are considered "crossovers" and "no-shows" in the study, and account for 5 and 11% of the sample, respectively. While I wish to study the causal effect of Head Start participation on labor-force outcomes, I only observe causal effects of access to Head Start. Thus, my intention-to-treat estimates of the effects of Head Start may underestimate the effects on those who use Head Start services (Gupta 2011).

Another drawback of the HSIS methodology, parent surveys relied entirely on self-reported data, which may be inaccurate. Although it is not clear that the inaccuracies were systematic, misreporting may limit the precision of my analysis. I found this to be an issue when analyzing self-reported education level. Education level is a nondecreasing economic outcome. However, I find that nearly 15% of mothers report a lower educational attainment in Year 1 survey than in the pre-treatment survey. This effect did not vary across treatment groups. To address this issue, specifically, I compare estimates with and without a high school graduate covariate and find little effect on point estimates.

Attrition in the HSIS sample also limits my analysis. High attrition in both the treatment and control group (nonresponse rates averaged 20% in each survey year) restrict the observations I can use in my analysis and attenuate the precision of my estimates. Because background demographic data was collected only in the pre-treatment year, inclusion of race, age, and teen mom covariates in my analysis excludes mothers who did not complete the survey in the 2002. This automatically restricts my sample to about 80% of the full HSIS sample and may bias my estimates. Attrition rates among control mothers, particularly in the four-year-old cohort, exceed those of mothers in the treatment group. To offset any bias and error from the restricted sample, I include a large and robust set of covariates to capture variance in my estimates. However, attrition is a significant limitation of my analysis.

Finally, I am limited in the degree to which I can observe changes in labor-force outcomes. While the HSIS collects labor-force participation data from mothers and fathers in each survey, it only observes the level of participation (full-time, part-time, looking for work, e.g.). Thus, I am limited to analyzing large jumps in labor-force participation. I am unable to detect, for example, small increases in hours worked unless such an increase transitions a mother from part- to full-time labor. This may explain the relatively weak fit of my data in explaining treatment effects on part-time employment. Full-time employment and mothers out of the labor force are somewhat more homogeneous, while there are varying degrees of part-time employment. The relative variance in part-time employment as an outcome measure is not captured in a discrete measure. Thus, I find part-time employment to be the noisiest outcome in my analysis.

VII. Conclusion

This paper provides the first analysis of the effects of access to Head Start on the parents of participants. In the context of a broad literature observing student-level impacts and ongoing policy debate regarding the efficacy of the program, my analysis considers a different segment of Head Start stakeholders. In my analysis, I find suggestive evidence of positive labor-force outcomes resulting from access to Head Start. With access to Head Start, mothers in the three-year-old cohort of the Head Start Impact Study (HSIS) sample were 4.81% more likely to work full-time, while mothers in the four-year-old cohort were 6.46% more likely to be in the labor force. These effects were heterogeneous with respect to relative economic advantage. Subgroups of mothers with the greatest average pre-Head Start income, particularly white mothers, were more likely to decrease the degree of labor-force participation when they had access to Head Start. Conversely, the least advantaged subgroups of mothers in the sample with access to Head Start (black mothers, in particular) were more likely to substituted their time into employment. I find that first year treatment outcomes are consistent in sign for first time applicants in both the three- and four-year-old cohorts of the HSIS sample. When participants enter kindergarten (and all mothers in the sample have access to public education services), the positive labor-force outcomes fade slightly in the full sample.

My analysis fits in and adds to the broader policy analysis of the Head Start program. I find that the average mother in the HSIS sample substitutes some of the time otherwise allocated towards non-labor activities into the labor force when during the first year she has access to Head Start. Increased participation in the labor force generates private benefits through income and skill accumulation, and could contribute to economic stability in the long run. An increase in labor-force participation can have positive public benefits, in the form of greater tax revenue and a decreased reliance on social programs, and social externalities (Rolnick and Grunewald 2003). Such benefits might suggest that Head Start is not large enough to maximize the social benefit of the program. However, the effects of Head Start on labor-force outcomes are heterogeneous among subgroups. In light of the effects on more advantaged subgroups in my sample, my findings might recommend revision of the program design to better incentivize labor-force participation, similar to the Earned Income Tax Credit.

In this paper, I consider exclusively the effect of access to Head Start on mothers' labor-force participation. Mothers who leave the labor force when they receive access to Head Start, however, are not necessarily unproductive. They may return to school, enroll in a job-training program, or leave to take care of younger children. Further study of the effects of access to Head Start on a broader set of economic outcomes will complement the findings of this study.

Works Cited

Aguiar, Gretchen. 2012. "Head Start: A history of implementation." (Doctoral Dissertation). University of Pennsylvania, UMI Dissertations Publishing.

Berger, Mark C. and Dan A. Black. 1992. "Child Care Subsidies, Quality of Care, and the Labor Supply of Low-Income, Single Mothers." *The Review of Economics and Statistics*, 74(4): 635-642.

Blau, David. 2003. Means-Tested Transfer Programs in the United States (1st Edition). University of Chicago Press, Chapter 6 on "Child Care Subsidy Programs." pp. 443-516.

Bureau of Labor Statistics. 2002. "Current Population Survey Ruggles, Steven, J. Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek. *Integrated Public Use Microdata Series: Version 5.0*. Minneapolis: University of Minnesota, 2010. (accessed January 14, 2015).

Currie, Janet and Duncan Thomas. 1995. "Does Head Start Make a Difference?" *The American Economic Review*, 85(3): 341-364.

Deming, David. 2009. "Early Childhood Intervention and Life-Cycle Skill Development: Evidence from Head Start." *American Economic Journal: Applied Economics* 1(3): 111-134.

Garces, Eliana, Duncan Thomas, and Janet Currie. 2002. "Longer-Term Effects of Head Start ." *American Economic Review*, 92(4): 999-1012.

Gibbs, Chloe, Jens Ludwig, and Douglas L. Miller. 2012. Head Start Origins and Impacts. In Martha Bailey and Sheldon Danizger (Eds.), *Legacies of the War on Poverty* (39-65). New York: Russell Sage Foundation.

Gupta, Sandeep. 2011. "Intention-to-treat concept: A review." *Perspectives in Clinical Research* 2(3): 109-112.

Kimmel, Jean. 1995. "The Effectiveness of Child-Care Subsidies in Encouraging the Welfare-to-Work Transition of Low-Income Single Mothers." *The American Economic Review*, 85(2): 271-275.

Ludwig, Jens and Douglas L Miller. 2007. "Does Head Start Improve Children's Life Chances? Evidence from a Regression Discontinuity Design." *The Quarterly Journal of Economics*, 122(1): 159-208.

Martinez-Beck, Ivelisse. 2009. "Employment Outcomes for Low-income Families Receiving Child Care Subsidies in Illinois, Maryland, and Texas (Grant #90YE0070)." Washington, D.C.: Administration for Children and Families, U.S. Department of Health and Human Services.

Michalopolous, Charles, Philip K. Robins, and Irwin Garfinkel. 1992. "A Structural Model of Labor Supply and Child Care Demand." *The Journal of Human Resources*, 27(1): 166-203.

OECD. 2014. *Education at a Glance 2014: OECD Indicators,* OECD Publishing.

Rolnick, Arthur, and Rob Grunewald. 2003. *Early Childhood Development: Economic Development with a High Public Return*: Federal Reserve Bank of Minneapolis. https://www.minneapolisfed.org/publications/fedgazette/early-childhood-development-economic-development-with-a-high-public-return.

Sabol, Terri J, and P. Lindsay Chase-Lansdale. 2014. "The Influence of Low-Income Children's Participation in Head Start on Their Parents' Education and Employment." *Journal of Policy Analysis and Management* 34(1): 136-161.

Tekin, Erdal. 2007. "Child care subsidies, wages, and the employment of single mothers." *The Journal of Human Resources* 42(1): 453–487.

United States Department of Health and Human Services, Administration for Children and Families. 2010. "Head Start Impact Study. Final Report." Washington, DC.

United States Department of Health and Human Services. Administration for Children and Families. Office of Planning, Research and Evaluation. Head Start Impact Study (HSIS), 2002-2006 [United States]. ICPSR29462-v5. Ann Arbor, MI: Inter-university Consortium for Political and Social Research[distributor], 2014-03-21. doi:10.3886/ICPSR29462.v5.

United States Department of Health and Human Services. 2006. *Head Start Program Performance Standards and Other Regulations.* 45 CFR 1301-1311.

United States Department of Health and Human Services. 2014. "Head Start Program Facts Fiscal Year 2013."

Appendix

Table A1- HSIS Survey Timeline					
	Fall 2002	**Spring 2003**	**Spring 2004**	**Spring 2005**	**Spring 2006**
3 Year-Old Cohort	HS (Baseline)	HS	HS	Kdg	1st
4 Year-Old Cohort	HS (Baseline)	HS	Kdg	1st	n/a
Notation	Baseline Year	Year 1	Year 2	Year 3	Year 4

Variable Definitions

Labor-force outcomes- labor force is observed in each survey through the variable P1WORKMO, which captures the following responses: (1) working full-time (35 hours or more per week), (2) working part-time, (3) looking for work, (4) laid off from work, (5) in school/training, (6) in jail/prison, (7) in military, (8) keeping house, (9) something else. I define the variables in Table A2 using this data.

Table A2 - Labor Force Variable Definitions									
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Mother In Labor Force	x	x	x	x			x		
Mother Working Full Time	x						x		
Mother Working Part-Time		x							
Mother Not Working			x	x	x	x		x	x

Table A3- Characteristic Variable Definitions				
	Year Available	**Variable Code**	**Variable Type**	**Description**
Hispanic	2002	P1MHISP	Binary	
White	2002	P1MWH	Binary	
Black	2002	P1MBL	Binary	This question was skipped for parents who reported race as white. Thus, I fill in {0} if P1MWH = 1.
Married	All	P1MARMO	Binary	
Single/Not Married	All	P1MARMO	Binary	All outcomes besides married (separated, divorced, widowed, never married)
High School Graduate	All	P1GRMO	Binary	Education level is observed as a discrete categorization. I define high school graduate as mothers who report at least high school completion
Teen Mom	2002	P1BIRTH	Binary	Reported as age when first gave birth. I define Teen Mom = 1 when P1BIRTH < 20.
Age	2002	P1MBYR	Integer	Reported as birth year. I subtract from survey year (2002) to calculate age in years.
Number of Children	All	P1REL[n]	Integer	Parents reported the relatinoship of each member of the household. I count the number of children as the sum of each brother, stepbrother, sister, or stepsister reported, plus the participant child.
Child ID	All	HSIS_CHILDID	Integer	
Treatment Group	All	CHILDRESULTGROUP	Binary	{0 = CONTROL, 1 = TREATMENT}
Cohort	All	CHILDCOHORT	Binary	
Crossover	All	CROSSOVER	Binary	
No Show	All	NOSHOW	Binary	
Year	All	Imputed	Integer	Each observation is coded with a year variable representing years since baseline survey in 2002.