

**PREP SCHOOL FOR POOR KIDS:
THE LONG-RUN IMPACTS OF HEAD START ON HUMAN CAPITAL AND ECONOMIC SELF-SUFFICIENCY**

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Abstract

This paper evaluates the long-run effects of Head Start on human capital and economic self-sufficiency using large-scale administrative data. Our research design exploits the county-level rollout of Head Start between 1965 and 1980 together with program eligibility captured by state-level school-entry age cutoffs. Using the restricted 2000-2013 Census/ACS linked to the Numident, we find that the human capital index of children induced to participate in Head Start increased by 10 percent of a standard deviation. Affected children achieved 0.29 more years of schooling, reflecting a 2.1-percent increase in high-school completion, an 8.7-percent increase in college enrollment, and a 19-percent increase in college completion. Head Start also raised the index of economic self-sufficiency by 4 percent of a standard deviation, decreasing adult poverty by 12 percent and public assistance receipt by 29 percent. Our estimates imply substantial, long-term returns to investing in large-scale preschool programs.

JEL Codes: I2, J24, J6

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Convincing evidence on the longer-term impacts of scaled-up pre-k programs on academic outcomes and school progress is sparse, precluding broad conclusions. The evidence that does exist often shows that pre-k-induced improvements in learning are detectable during elementary school, but studies also reveal null or negative longer-term impacts for some programs.

~ Brookings Pre-Kindergarten Task Force of Interdisciplinary Scientists (April 2017)

In 1965, the U.S. began a new experiment in the provision of public pre-school for disadvantaged children. The motivation was simple: “the creation of and assistance to preschool, day care, or nursery centers for 3- to 5-year-olds...will provide an opportunity for a *head start* by canceling out deficiencies associated with poverty that are instrumental in school failure” (United States Senate Committee on Labor and Public Welfare 1964). The program that ensued is the now-famous “prep school for poor kids,” aptly named “Head Start” (Levitan 1969), which aimed to help millions of children escape poverty.

More than fifty years later, Head Start is one of the most popular of the War on Poverty’s programs, serving around 900,000 children annually at a cost of \$9.6 billion in 2017.¹ Unlike expensive, small-scale “model” programs such as Perry Preschool and Abecedarian, Head Start’s architects prioritized wide-spread access, calculating that a massive preschool expansion would maximize its poverty-fighting (and political) benefits. Skepticism about the quality of this large-scale pre-school program coupled with difficulties in evaluation have generated controversy about its short-term benefits for decades (Westinghouse Learning Corporation 1969, Currie 2001, Duncan and Magnuson 2013, Phillips et al. 2017). Convincing evidence regarding the Head Start’s long-term poverty fighting benefits has remained even more elusive, thanks to the lack of program randomization in its early years, small sample sizes of longitudinal surveys, and the difficulty of measuring adults’ access to Head Start decades ago. Whether Head Start achieved its broad goal of increasing the life opportunities for children remains an open question.

This paper uses newly available large-scale administrative data to estimate Head Start’s long-term effects on human capital and economic self-sufficiency. By linking the restricted long-form 2000 Census and 2001-2013 American Community Surveys (ACS) to the exact date and place of birth from the Social

¹ <https://eclkc.ohs.acf.hhs.gov/sites/default/files/pdf/hs-federal-funding-enrollment-history.pdf>

Security Administration's (SSA) Numident file, we observe outcomes for one-quarter of U.S. adults as well as a high quality measure of their access to and eligibility for Head Start as children. The resulting sample is four orders of magnitude larger than longitudinal surveys, and place and exact date of birth ameliorates (potentially endogenous) measurement error in Head Start access.

Our research design exploits the county-level launch of Head Start programs from 1965 to 1980 at the Office of Economic Opportunity (OEO) (Levine 1970, Bailey 2012, Bailey and Danziger 2013, Bailey and Duquette 2014, Bailey and Goodman-Bacon 2015). This approach mitigates concerns about the correlation of Head Start *funding* with program performance and OEO funding for other local programs and eliminates well-known problems of measurement error in archival grant data (Barr and Gibbs 2017). An additional strength of our design is that we compare cohorts born in the same county who were age-eligible for the program when it launched (ages 3-5) to cohorts that were age-ineligible (children 6 and older). Similar to a regression kink design (Card et al. 2015), our key identifying assumption is Head Start's causal effect is the *only* reason for a change in the relationship between a child's age at the program's launch and her outcomes as an adult. Furthermore, the use of a national roll-out accounts for general-equilibrium effects of this large-scale program.

Our results suggest that Head Start increased the human capital and economic self-sufficiency of disadvantaged children. The human capital index of participants was 10 percent of a standard deviation higher. Children participating in Head Start achieved 0.29 more years of education, were 2.1 percent more likely to complete high school, 8.7 percent more likely to enroll in college, and 19 percent more likely to complete college. In addition, Head Start increased economic self-sufficiency in adulthood by almost 4 percent of a standard deviation—gains driven largely by a 12-percent reduction in adult poverty and a 29-percent reduction in public assistance receipt. We find no evidence of reductions in incarceration. Our estimates suggest that the program achieved its goal of increasing children's economic opportunities and delivered potentially large, long-term internal rates of return.

I. THE LAUNCH OF HEAD START IN THE 1960S

In the 1960s, the (then) revolutionary idea that preschool could improve children's cognitive development suggested an innovative strategy for poverty prevention. Because poor children started school

with significantly less educational background, comprehensive preschool could give them a “Head Start,” improving their success in school and addressing a root cause of poverty.

Funded by the OEO, Head Start first began as an 8-week summer program in 1965 and, after a successful first summer, President Lyndon Johnson announced that Head Start would become a full-year program for children ages 3 to 5.² The director of the OEO wrote 35,000 letters to public health directors, school superintendents, mayors and social services commissioners to encourage applications. The OEO also made a special effort to generate applications in America’s 300 poorest counties (Ludwig and Miller 2007).

Head Start’s political popularity led to an even faster launch than other War on Poverty programs. By 1966, Head Start had begun in more than 500 counties where over half of the nation’s children under age 6 resided. By 1970, Head Start had begun in another roughly 900 counties, and federal expenditures on the program reached \$326 million, or \$1.9 billion in 2012 dollars (OEO 1970). This early expansion ensured that by 1970, Head Start existed in roughly half of U.S. counties nationwide and covered 83 percent of U.S. children under age six. Because much of this history is described elsewhere, our quantitative description of the program’s roll-out appears in our Appendix.

Head Start’s architects adopted a holistic approach that aimed to develop children’s health and physical abilities, including their self-confidence and relational skills, verbal and conceptual skills, and parent involvement.³ But the challenges of *quickly* starting a new *national* program meant that implementation often deviated from ideals. Not only did Head Start lack curricular standardization, but programs struggled to find high-quality teachers to achieve the suggested pupil-to-teacher ratio of 15:1. As a practical solution many centers relied on para-professionals, most of whom lacked post-secondary education; thirty percent had not finished high school (Hechinger 1966, Braun and Edwards 1972).⁴ In

² “Pre-school” also included five-year-olds, because public kindergarten was not yet universal (Cascio 2009).

³ Levitan (1969) notes that Head Start’s 1966-7 budget included early childhood education (daily activities and transport, 70 percent); health services (including immunizations, screenings and medical referrals) and nutrition (17-20 percent); and parent involvement, social services (e.g., helping families cope with crises), and mental health services accounted for the remaining budget. Spending on health and other services increased over time.

⁴ Sizable variation in pre-school quality persists today. For an overview see Currie (2001), Cascio and Schanzenbach (2013), and Duncan and Magnuson (2013).

addition, many components of Head Start phased in slowly. For instance, the OEO wrote that in 1965, “the proportion of children receiving treatment for conditions discovered in Head Start medical and dental examinations...was probably under 20 percent. It rose to over 65 percent in 1966, and in 1967 we fully expect it to have reached over 90 percent” (OEO 1967).

Head Start’s implementation implies a pattern in the expected relationship of adult outcomes with a child’s age at the time of the program’s launch, which we plot in Figure 1. First, we expect a limited effect on children who were over age 5 when Head Start launched. The effect may not be zero, because 10 percent of children in full-year Head Start were 6 or older (Vinovskis 2008) and school-age children could still benefit from younger siblings’ participation (Garces et al. 2002). Our preferred specification that uses 6-year-olds as a control group may, therefore, understate Head Start’s effects.

Second, if Head Start had a causal effect, we expect to see a *change* in adult outcomes for children under age 5 when Head Start launched, because these cohorts would have been the first to have been age-eligible *and* have access. But we should not expect an immediate shift in the *level* of outcomes (akin to a regression discontinuity, RD) but rather a shift in the *slope* (akin to a regression kink). One reason is that Head Start’s capacity grew over time—the full-year program served only 20,000 children in 1965 but 160,000 in 1966, 215,000 in 1967 and 1968, and 257,700 in 1970 (OEO 1965, OEO 1966, OEO 1967, OEO 1968, OEO 1970).⁵ In addition, program quality increased over time with better hiring and training of teachers, curriculum development, and the implementation of auxiliary services (e.g., health). We suspect that the speed of implementation varied with the year of Head Start’s implementation—programs starting later could adopt best practices faster. Figure 1’s illustration of implementation is a stylized representation of the quality and capacity increase (solid line, square markers).

We also expect to see larger changes among children who were younger when Head Start launched, simply because they would have been age-eligible for a larger share of their preschool years. A child 5-

⁵ Enrollment in summer Head Start was much higher, but we expect the summer program to have smaller effects. Even in the 1960s, few experts believed that an 8-week summer program could produce lasting benefits (Vinovskis 2008 citing Edward Zigler). Moreover, 30 to 40 percent of children in summer Head Start were aged six and older, whereas only 10 percent of those in full-year programs were older than five. See Thompson (forthcoming)’s Table 1.

years old when Head Start launched could participate for at most one year, whereas a 3-year-old child would be age-eligible for three years. This does not mean that the 3-year-old enrolled for more years—only that the 3-year-old was age-eligible to enroll for three years and, therefore, more likely to do so. Figure 1 illustrates this as a linear relationship (dashed line), but differences in the likelihood of enrollment by age could make this relationship nonlinear. This combination of gradual implementation and cumulative exposure implies a *non-linear* shift in the relationship between age at Head Start launch and adult outcomes (solid line). When summarizing effects, our discussion focuses on children age -1 at program launch who would have been age-eligible for three years for a fully implemented program.

II. LITERATURE REGARDING THE LONG-TERM EFFECTS OF HEAD START

Previous evaluations of Head Start’s long-term effects provide some evidence of the program’s long-term effects on human capital and economic self-sufficiency.⁶ One pioneering approach was the use of family fixed effects with longitudinal data. Building on work by Currie and Thomas (1995), Garces et al. (2002) used the Panel Study of Income Dynamics (PSID) to compare children who participated in Head Start to their siblings who did not. They show that Head Start increased high school graduation rates and college enrollment among whites and reduced arrest rates among blacks. Using a similar research design for more recent cohorts in the National Longitudinal Survey of Youth (NLSY), Deming (2009) finds that Head Start participation had large and positive effects on a summary index of adult outcomes (including high school graduation, college attendance, “idleness,” crime, teen parenthood, and health status). Well-known critiques caution that sibling comparisons may suffer from well-known sources of endogeneity bias (Griliches 1979, Bound and Solon 1999). In addition, small sample sizes in longitudinal surveys may provide unreliable estimates of Head Start’s effects (Miller et al. 2017).

More recent work exploits shifts in access to Head Start using three distinct research designs. The path-breaking application of RD in Ludwig and Miller (2007) exploited the OEO’s special effort to generate grant proposals from the 300 poorest counties. Comparing the outcomes of children on either side of this

⁶ See reviews of studies of Head Start’s short-term effects (Currie 2001, Cascio and Schanzenbach 2013, Duncan and Magnuson 2013, Gibbs et al. 2014).

threshold, they find evidence that Head Start reduced childhood mortality and increased the receipt of high-school degrees and college enrollment. However, because the 1990 and 2000 Censuses required them to use county of residence in adulthood to proxy for childhood Head Start access, measurement error causes their education results to be sensitive to specification and often statistically insignificant.⁷ Carneiro and Ginja (2014) use an RD in state-, year-, and household-based income eligibility cutoffs for more recent Head Start programs. They find that Head Start decreased behavioral problems, the prevalence of some health conditions (including obesity) between the ages of 12 and 17, and crime rates around age 20. They find a positive though statistically insignificant effect on receiving a high-school diploma as well as suggestive evidence that Head Start *reduced* college enrollment.

In work closely related to this paper, three studies make use of county-year variation in Head Start funding in the 1960s and 1970s to quantify the program’s long-term effects. Using a sample of likely eligible children from the NLSY, Thompson (forthcoming) finds that greater funding for Head Start at ages 3 to 6 raised college graduation rates, reduced the incidence of health limitations, and tended to raise adult household income. Focusing on a “high impact” sample, Johnson and Jackson (2017) find that an average level of Head Start and education spending increases the likelihood that children graduated from high school by 8 percentage points and gained 0.39 years of schooling. These children also experienced a 7.8 log-point increase in adult wages, a 14.4 log-point increase in adult family income at ages 20 to 50, a 3.6 percentage-point reduction in poverty at ages 20 to 50, and a 3 percentage-point reduction in adult incarceration. Finally, Barr and Gibbs (2017) examine the intergenerational effects of Head Start using the NLSY and two research designs: family fixed effects and variation in program *availability* across birth counties (also referred to as “roll-out”). To alleviate concerns about the endogeneity of funding levels and measurement error in the National Archives data, their roll-out design uses a binary measure of Head Start access that is equal to one if funding exceeds the 10th percentile of observed funding per four-year-old. They find evidence of large first-generation effects on women (including a gain of a half a year of schooling) and large

⁷ Also, limited evidence shows the poorest 300 counties were more likely to get funding for Head Start (see Ludwig and Miller 2007: Table II and Pihl 2017).

second-generation effects on their children's high school graduation and completed education.

III. DATA AND RESEARCH DESIGN

This study combines the long-form 2000 Census and 2001-2013 ACS with the Numident to shed new light on Head Start's long-term effects. The Census/ACS data represent almost one quarter of the U.S. population and are four orders of magnitude larger than previously used longitudinal samples. Another advantage of these data is that the Numident contains county of birth (rather than adulthood residence) and exact date of birth, which allows a high-quality proxy for Head Start access and age eligibility in childhood. The data's main disadvantage is that they contain no information on family background, which limits subgroup analyses of the lower-income children heavily represented in the program.⁸ Our sample is comprised of children born from 1950 to 1980 in U.S. states where the school-entry age cutoff is known. We additionally limit our sample to individuals who are in their prime-earning years from 2000 and 2013 (ages 25 to 54).⁹ To minimize disclosure concerns at the Census Bureau, we use only observations with non-allocated and non-missing values for all outcomes.

Combining data on the launch of Head Start programs from Bailey and Goodman-Bacon (2015) with the Census/ACS-Numident permits two refinements to previously used research designs (Barr and Gibbs 2017, Johnson and Jackson 2017, Thompson forthcoming). First, we use only variation in the launch of the Head Start program rather than Head Start spending. This refinement addresses (1) the potential endogeneity of Head Start funding decisions to the program's *performance* and (2) the potential correlation of year-to-year variation in Head Start funding with that of *other* OEO programs. It also (3) sidesteps issues of measurement error in the National Archives grant amounts (Barr and Gibbs 2017).¹⁰ Second, we examine changes in outcomes for children who were *age-eligible* for Head Start when it launched (ages 3-5) relative to those who were age-ineligible (ages 6+). Age eligibility is based on exact date of birth in the Numident and school-entry age cutoffs, which alleviates measurement error in defining the potential treatment and

⁸ In 1970, 62 percent of Head Start's participants were from families with annual incomes less than the poverty line for a family of four (~\$4,000) (OEO 1970).

⁹ We find no evidence that Head Start affected survival to 2000 (see Appendix).

¹⁰ Thompson (forthcoming) also tries this strategy but notes that his estimates in the NLSY are statistically insignificant.

control groups. Similar to a regression-kink design, our identifying assumption is that the causal effect of Head Start is the only reason for a *change* in the relationship between a child’s age at the program’s launch and her outcomes as an adult. (See Appendix for more information on our sample and support for this research design.)

We implement these comparisons within the following event-study framework,

$$(1) \quad Y_{bct} = \theta_c + \alpha_t + \delta_{s(c)b} + \mathbf{Z}'_c b \boldsymbol{\beta} + \text{HeadStart}_c \text{Age}'_{bs(c)} \boldsymbol{\varphi} + \varepsilon_{bct}.$$

Children’s birth years are indexed by $b=1950–1980$, county of birth by c , and Census/ACS year by $t=2000–2013$. Specifications include fixed effects for county of birth, θ_c , year, α_t , and state-by-birth-year, $\delta_{s(c)b}$, which, respectively, capture time-invariant differences across counties, national changes affecting all individuals, and changes in state policies that differentially affect birth cohorts. Although covariates matter little, we follow the literature and include county characteristics, \mathbf{Z}_c interacted with a linear trend in year of birth, b (Hoynes et al. 2011, Bailey 2012, Bailey and Goodman-Bacon 2015).¹¹

Our point estimates of interest, $\boldsymbol{\varphi}$, capture the intent-to-treat (ITT) effects of Head Start on long-term human capital and economic self-sufficiency. The set of these effects describe the evolution of outcomes for children born in counties, c , that received Head Start programs ($\text{HeadStart}=1$) with dummy variables capturing the children’s age relative to the state school-entry cutoff at the time the program began, T_c^* . We treat school age (age 6 before the school entry cut-off date) as the omitted category, because these children would have been unlikely to have attended Head Start rather than public school. Although the event study estimates range from -15 (1965–1980) to 30 (1980–1950), we focus on years -1 to $+15$, which are compositionally balanced across counties. Standard errors are corrected for heteroskedasticity and adjusted for an arbitrary within-county covariance structure (Arellano 1987, Bertrand et al. 2004).

Based on Figure 1’s predictions, we also summarize the event-study estimates using a three-part spline with knots at age 6 and -1 . The spline has the added benefits of allowing a parsimonious method to test for a pre-trend, which would result in failing to find a trend-break at age 6 in adult outcomes.

¹¹ County characteristics include the 1960 poverty rate, log county population, population share over age 65, under age 5, living in an urban setting, and non-white.

Our outcomes of interest are summary measures of human capital and economic self-sufficiency, which permit tests of co-movements of related adult outcomes and limits the number of statistical tests (Kling et al. 2007).¹² A shortcoming of this approach is that, because indices weight each component equally, large changes in one dimension are averaged with potentially opposite-signed or zero effects in other dimensions. We, therefore, also examine the individual index components. The human capital index includes four binary variables indicating achievement of a given level of education or greater: high school or GED, some college, a 4-year college degree, and a professional or doctoral degree; years of schooling, and an indicator for working in a professional occupation. Our index of self-sufficiency includes binary indicators of employment, poverty status, income from public sources, family income, and income from other non-governmental sources; continuous measures of weeks worked, usual hours worked, the log of labor income, log of other income from non-governmental sources, and log ratio of family income to the federal poverty threshold.

These ITT effects average over all children in a county, regardless of whether they participated in Head Start. To compare our estimates to the literature, we generate average treatment-effects-on-treated children (ATET) by dividing by 0.149, our best estimate of the effect of Head Start launch on program enrollment using both administrative and 1970 Census data (see Appendix for details.) We construct confidence intervals using a parametric bootstrap procedure using 10,000 independent draws from normal distributions with means and standard deviations equal to the point estimates and standard errors from the reduced-form and first-stage estimates (Efron and Tibshirani 1993). Because we cannot resample from data used in other Head Start papers, we also use this method to recalculate confidence intervals for other estimates in the literature for comparison.

IV. HEAD START'S EFFECTS ON HUMAN CAPITAL

Table 1 summarizes this evidence for all outcomes in the human capital index for the fully-exposed cohort (age -1 at Head Start's launch). Column 2 shows the event-study estimate, while column 4 contains

¹² Following Kling et al. (2007), we standardize outcome measures for each individual using the mean and standard deviation in the control group (ages 6-7 at launch), and we recode outcomes so that increases indicate improvements in human capital.

the spline estimate. The results suggest that Head Start significantly improved adult human capital. The standardized index increases by 1.5 percent of a standard deviation (column 2) for the cohort and 10 percent of a standard deviation for treated children (column 6). Although the spline trades flexibility for parsimony, it fits the data well: column 4's ITT-estimates are identical to those in column 2 to the hundredth. Column 5 presents the test statistic for the equality of spline segments for ages 6 and older and -1 to 5. For the human capital index, the data reject the null of no trend-break at the 1-percent level.

Figure 2 summarizes the event-study results for a subset of outcomes driving changes in the index. Similar to Figure 1, the x-axis is the child's age at the time Head Start launched, the solid line the event-study estimates, and the dashed lines the spline. Consistent with a causal interpretation, each index component exhibits a trend break at age 6 as anticipated.

Panels A and B of Figure 2 summarize event-study estimates for two of the most commonly studied human-capital outcomes: high school graduation and college enrollment. Table 1 shows that treated children were 1.9 percentage points more likely to complete high school/GED (column 6)—a 2.1-percent increase relative to the control mean (column 7). This is smaller than estimates reported in related studies of Head Start's effects in the 1960s. Figure 3A shows that it is roughly half the size of Garces et al. (2002)'s sibling comparison in the PSID and Thompson (forthcoming)'s spending design in the NLSY. In addition, it is one-fifth the size of Johnson and Jackson (2017)'s spending design estimates for the very disadvantaged sample in the PSID; and one-ninth the size of Ludwig and Miller (2007)'s RD estimates using the Census. (It is one quarter the size of Deming et al. (2009)'s sibling comparison for Head Start in the 1990s for more recent cohorts.) Although our estimate falls within the confidence intervals of previous studies, this reflects the large uncertainty of those estimates.

Similarly, Table 1 shows a statistically significant effect of Head Start on college enrollment. Head Start raised college enrollment by 5.4 percentage points, or 8.7 percent. This estimate is half the size of Garces et al. (2002) and one quarter the size of Ludwig and Miller (2007) (Figure 3B).

(The magnitude of the increase in college enrollment of 0.05 is only slightly smaller than Deming (2009)'s NLSY sibling comparison for Head Start in the 1990s.) Consistent with the visually evident trend-break in

Figure 2B, we reject the null hypothesis of no trend break at age 6 at the 5-percent level.

In addition to generating more precise estimates for these commonly studied outcomes, our large-scale data permit a novel evaluation of the effects of Head Start on other dimensions of human capital, including college completion or higher degrees, which previous data have not been able to detect. Table 1 shows that participating children achieved 19 percent higher college graduation rates. Consistent with a causal interpretation, the trend break is statistically significant at the 1-percent level.¹³ These estimates are one-quarter to one-fifth the size of those found for the Abecedarian Project (Currie 2001, Barnett and Masse 2007, Duncan and Magnuson 2013). Similarly, completion of professional or doctoral degrees increased by 50 percent among treated children. These gains across the education distribution are summarized in a 0.29-year increase in schooling. This estimate is smaller than Johnson and Jackson (2017)'s estimate of 0.52 years for very disadvantaged children, but it is highly statistically significant and does not appear driven by a pre-trend.¹⁴

These large effects on college and higher degrees may be surprising, given that no other study of preschool has documented effects on post-secondary education. This lack of evidence may reflect, in part, the small longitudinal samples or the small scale of model pre-school programs. Differences in the participating children may also matter. Abecedarian and Perry's participants were *very* disadvantaged children and mostly black, and Perry's participants had low IQs.¹⁵ In contrast, Head Start was not exclusively for poor or African-American children in the 1960s and 1970s.¹⁶ Consequently, Head Start's participants on average likely faced fewer socio-economic disadvantages and less racism relative to other model programs, making it less surprising that they experienced gains in post-secondary education.

Because analyses of model preschool programs have found different educational effects for boys, Table 1 stratifies by sex. Among participating men, the human capital index increased by a statistically

¹³ The similarity of column 2 for some college and college completion is correct. This does not imply that every person that Head Start induced to enter completed college. Head Start likely helped some individuals enter and others complete college.

¹⁴ Jackson and Johnson's ITT estimate is 0.0967 per \$1000 spent per poor 4-year-old, or an ATET of $0.0967 * 4 / 0.75 = 0.52$

¹⁵ The model Perry Preschool Program, which focused on lower IQ children, had no measured effects on postsecondary outcomes (Anderson 2008).

¹⁶ The OEO encouraged interactions between poor and less disadvantaged children, allowing 15 percent (later 10 percent) of participants to come from non-poor families. Roughly two-thirds of children in the 1969-1970 full-year programs had mothers with less than a high school degree. Mothers of about 7 percent of children had attended or graduated from college.

significant 14 percent of a standard deviation. For this group, high school completion rose by a statistically insignificant 2.7 percent, college attendance rose by 13 percent, and college completion rose by 27 percent. The high school estimates are smaller than others in the literature, but the college attendance estimates tend to be larger. Head Start cumulatively raised years of education among treated men by 0.41 years and the likelihood of completing a professional/doctoral degree by 59 percent. The evidence suggests that men treated with Head Start were 19 percent more likely to hold professional jobs.

The human capital index increased by less among women, at only 7 percent of a standard deviation. Completion of high school (or a GED) rose by a statistically insignificant 1.5 percent, and college attendance rose by 5.7 percent (although the trend-break is not statistically significant). For women, changes in the human capital index appear driven by increases in higher degrees, including an 11-percent increase college completion and 36-percent increase in professional degrees. Treated women's schooling rose by 0.17 years and their likelihood of holding a professional job rose by 9.5 percent.¹⁷

V. HEAD START'S EFFECTS ON ECONOMIC SELF-SUFFICIENCY

The substantial effects of Head Start on human capital suggest a potential for effects on economic self-sufficiency. Table 2 shows that an index of economic self-sufficiency aggregated over both sexes increased by 4 percent of a standard deviation. Consistent with Head Start affecting less skilled individuals, the program decreased the likelihood of adult poverty by 12 percent and receipt of public assistance income by 29 percent. However, Table 2 shows little effect of Head Start on labor-force participation or wage income. This null result may reflect the fact that men and women's work effort (and potentially selection) changed in offsetting ways. Whereas Head Start's effect on men's human capital may have led them to increase employment (e.g., the substitution effect dominates), the reverse may be true for women (e.g., the income effect dominates as more education allows them to marry higher-earning men).

Panels A and B of Table 2 are consistent with this hypothesis. Because the self-sufficiency

¹⁷ Our appendix reports estimates of Head Start's effects on human capital by race and sex. Effects for nonwhites are generally statistically insignificant, because they constitute only 15 percent of the overall sample. The overall patterns suggest that Head Start's effects are largest among white men (13 percent of a std.) and smaller effects among white and non-white women (5-6 percent of a std., respectively). Effects for non-white men were small or negative and imprecise.

estimates are noisier and stratifying by sex reduces sample sizes, we focus our discussion on the spline estimates. For treated men, the self-sufficiency index increased by 3 percent of a standard deviation. We also find positive effects of Head Start exposure on both the extensive and intensive margins of men's labor-force participation. Treated men were 2.1 percent more likely to have worked for pay (column 7), worked an average of one more week and one more hour per week (column 6). Consistent with these estimates reflecting the causal effect of Head Start, we find evidence of a marginally significant trend-break at age 6. Interestingly, the combined effects of increased human capital and labor-force participation do not appear to have affected annual wages. This is consistent with Head Start increasing negative labor-force selection: the fact that the marginal participants tended to be less skilled, and therefore lowered the cohort's wages on average.¹⁸ Head Start had little effect on men's poverty, but the program is associated with a 27-percent decline in public assistance receipt among treated men. Reductions in public assistance are also consistent with negative selection, because male public assistance recipients receive high rates of disability income.¹⁹

The pattern is different for women. The self-sufficiency index increased by 4 percent of a standard deviation among women treated with Head Start, largely driven by a 28-percent reduction in public assistance receipt and a 16-percent reduction in poverty. However, women's labor-force participation on the extensive and intensive margins *fell* slightly, albeit not significantly. These reductions in work appear to have *increased* annual wages of working women by around 4 percent, which is consistent with Head Start inducing *positive* selection (e.g., less-skilled women opting out).²⁰

VI. NEW EVIDENCE ON THE LONG-TERM RETURNS TO HEAD START

Over the past 20 years, substantial evidence has accumulated that *model* preschool programs have sizable economic returns (Cunha and Heckman 2007, Heckman et al. 2010, Almond and Currie 2011, Duncan and Magnuson 2013). However, convincing evidence on the long-run returns to larger-scale, public

¹⁸ Assuming all new labor market entrants came from the left-hand tail of the skill distribution, the NLSY shows a negative impact of such selection on wages is 7.4 percent. Because we find a 2.2 percent increase in log annual wages, this implies that Head Start led to an estimated 9.9-percent increase. This number is consistent with our NLSY estimate that men's potential wages increased by 9.4 percent (see Appendix).

¹⁹ We find no evidence of decreases in incarceration among men (see Appendix).

²⁰ Our appendix reports estimates of Head Start's effects on self-sufficiency by race and sex. Effects for nonwhites are generally statistically insignificant. The overall patterns suggest that Head Start's effects on self-sufficiency are largest among white men and women (3 and 4 percent of a std., respectively). See also footnote 17.

preschool has remained sparse (Phillips et al. 2017).

Using newly available administrative data, this paper provides novel estimates of the long-term effects of Head Start, the nation's longest running, large-scale public preschool program. We find that Head Start had large effects on participants' human capital. Head Start children achieved 0.29 more years of schooling, reflecting the fact that they were 2.1 percent more likely to complete high school, 8.7 percent more likely to enroll in college, and 19 percent more likely to complete college. A second finding is that Head Start increased adult self-sufficiency, reducing the likelihood of adult poverty by 12 percent and public assistance receipt by 29 percent.

A full accounting of the costs and benefits of Head Start is beyond the scope of this paper, but we summarize the implications of our estimates using earnings *potential* (Neal and Johnson 1996, Deming 2009). (Using potential earnings circumvents problems with selection.) The NLSY79, which allows for controls for ability, suggests a private internal rate of return to Head Start of 7.7 percent, which ranges from around 4 percent for women to 11 percent for men. As an alternative, the internal rate of return of putting one child through Head Start is 2.4 percent using only savings on public assistance expenditures (estimated at \$9,967 in the *Survey of Income and Program Participation*) (see Appendix).

Several reasons suggest that these estimates are conservative. First, our research design differences out sibling spill-over effects, which tends to reduce the estimated effect sizes. Second, reports of income and public assistance receipt may be severely underreported in major national surveys (Bound et al. 2001, Meyer et al. 2015), suggesting estimates of Head Start's effect on public assistance may be understated. Third, adding increases in tax revenues, reductions in deadweight loss from public assistance transfers, or underreporting in public assistance income would serve to increase our estimates of the returns to Head Start. Finally, estimates of the returns to Head Start ignore benefits through improvements in outcomes not measured here. For instance, they ignore the extent to which more education engenders better health, longevity, or well-being. Our findings that Head Start achieved its goal of reducing adult poverty and delivered sizable long-term returns on investments in the 1960s are, therefore, suggestive of potentially larger social long-term returns.

VII. REFERENCES

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Figure 1. Expected Effects of Head Start on Adult Outcomes by Cohort's Age at Launch

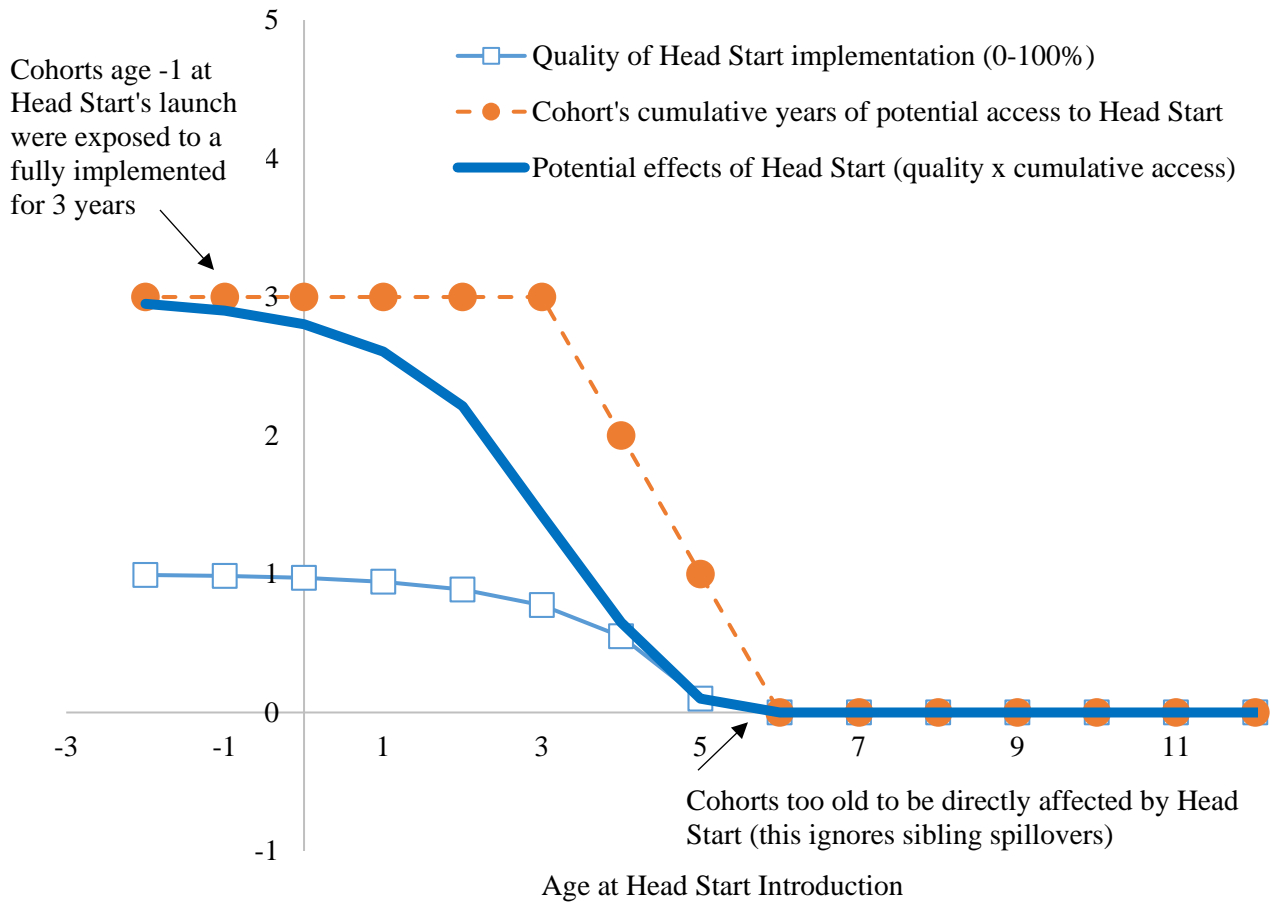


Figure 2. The Effect of Head Start on Adult Human Capital and Self-Sufficiency

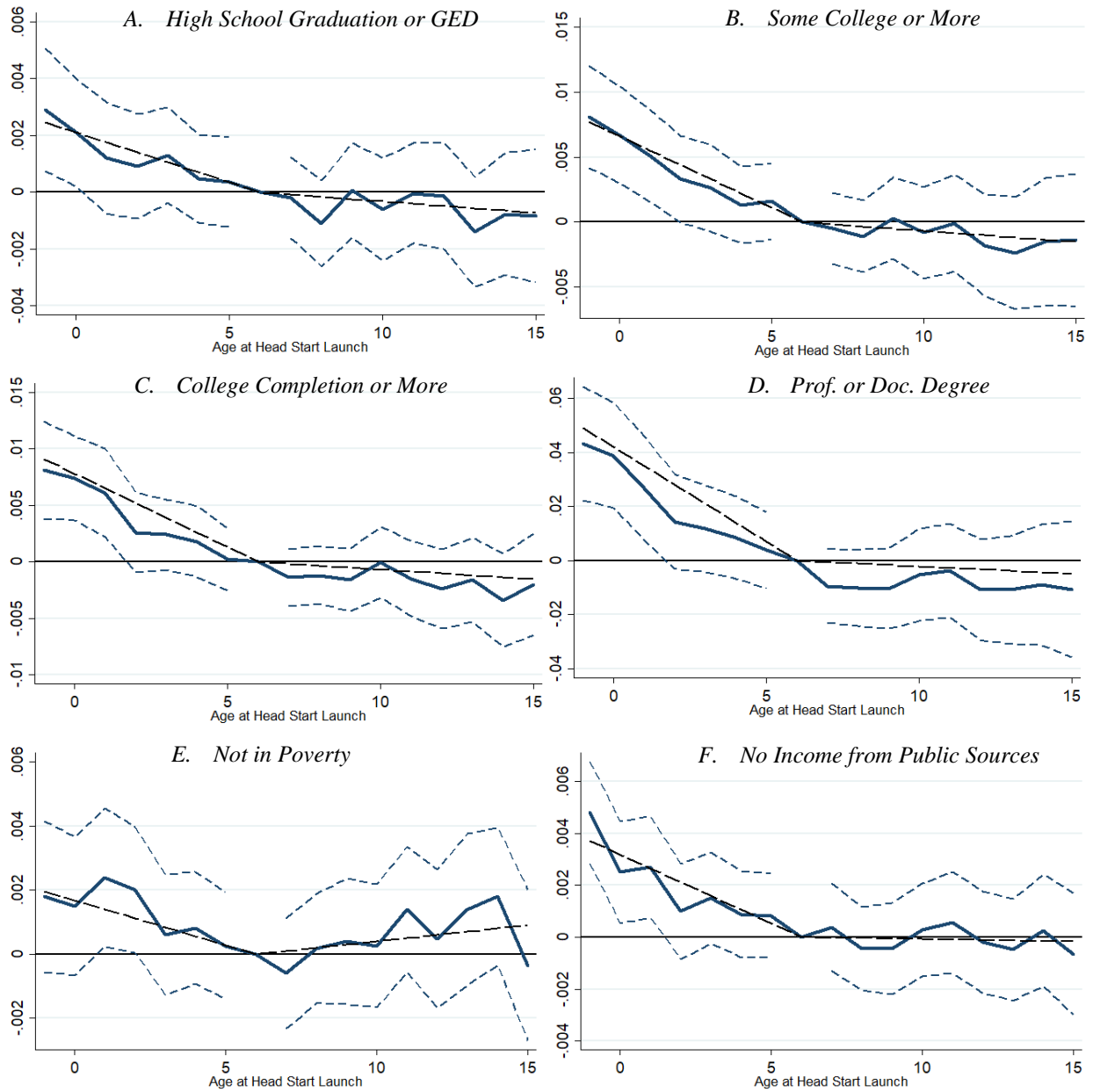
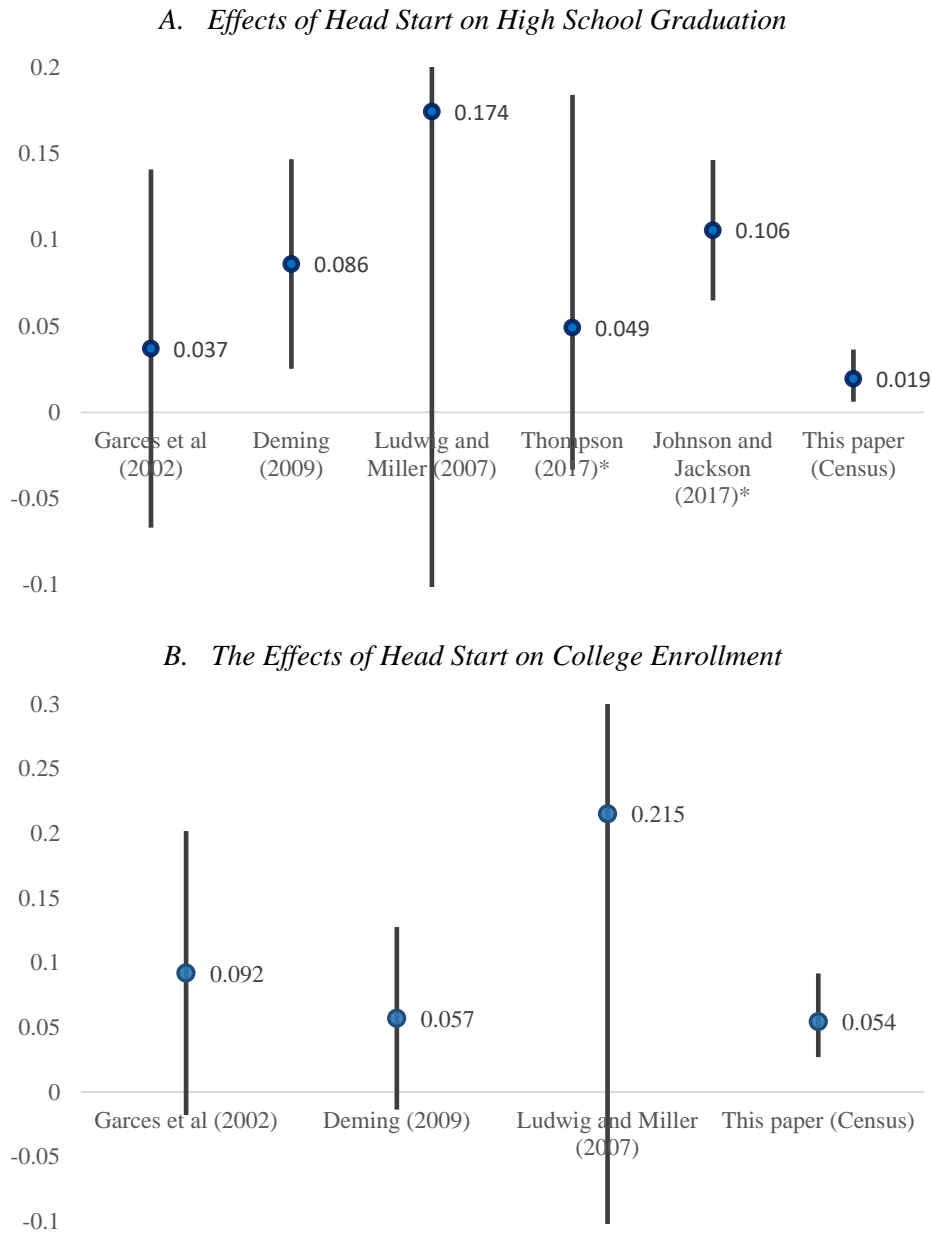


Figure 3. The Magnitude of Head Start’s Effects on Education across Studies



Notes: Circles indicate the reported or derived ATET from different studies. For sibling fixed effect studies, the ATET is directly reported in the papers. For other studies, we scale the reported ITT by the reported first-stage estimate and constructed 95-percent confidence intervals using the standard error of the first stage as described in the text. Because Johnson and Jackson (2017) does not report a standard error on the first stage, the confidence interval reported for this study in Panel A does not include this first-stage uncertainty. We limited the y-axis range so that the confidence intervals for most studies could be read from the figure. The confidence intervals for Ludwig and Miller (2007) fall outside the y-axis range and are [-0.54,1.47] in panel A and [-0.67,1.82] in panel B. Bars indicate the reported 95-percent confidence interval for sibling fixed-effect models or constructed for the ITT studies as described in the text. See Appendix for more details on the exact figures used. *Johnson and Jackson (2017) and Thompson (2017) sample likely eligible samples of the PSID and NLSY79: individuals born to parents in the bottom quartile of the income distribution, and parents with no college education, respectively.

Table 1. The Effect of Head Start on Adult Human Capital

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variables	Control mean (std.)	Event study at -1 (s.e.)	ITT % Gain	Spline at -1	F-stat on break at 6 [p-value]	ATET [95% CI]	ATET % Gain
<i>A. Men and Women</i>							
Human capital index	0.014 (0.198)	0.015 (0.003)		0.015	14.02 [0.00]	0.101 [.058,.162]	
Completed high school/GED	0.92 (0.078)	0.003 (0.001)	0.3%	0.002	2.18 [0.14]	0.019 [.006,.036]	2.1%
Attended some college	0.62 (0.140)	0.008 (0.002)	1.3%	0.008	4.84 [0.03]	0.054 [.027,.092]	8.7%
Completed 4 year college	0.29 (0.122)	0.008 (0.002)	2.8%	0.009	11.66 [0.00]	0.054 [.027,.092]	19%
Prof. or doc. degree	0.028 (0.037)	0.002 (0.001)	7.5%	0.002	7.36 [0.01]	0.014 [.001,.03]	50%
Years of schooling	13.57 (0.695)	0.043 (0.011)	0.3%	0.049	13.06 [0.00]	0.291 [.141,.494]	2.1%
Has a professional job	0.35 (0.121)	0.007 (0.002)	2.1%	0.007	4.97 [0.03]	0.049 [.022,.085]	14%
<i>B. Men</i>							
Human capital index	0.014 (0.247)	0.0206 (0.004)		0.020	11.79 [0.00]	0.136 [.08,.216]	
Completed high school/GED	0.91 (0.104)	0.0038 (0.002)	0.4%	0.003	1.92 [0.17]	0.025 [-.001,.056]	2.7%
Attended some college	0.59 (0.176)	0.0111 (0.003)	1.9%	0.010	5.67 [0.02]	0.074 [.034,.127]	13%
Completed 4 year college	0.29 (0.150)	0.0116 (0.003)	4.0%	0.011	9.99 [0.00]	0.077 [.037,.131]	27%
Prof. or doc. degree	0.034 (0.052)	0.003 (0.001)	8.8%	0.003	4.73 [0.03]	0.02 [.007,.037]	59%
Years of schooling	13.52 (0.795)	0.0623 (0.013)	0.5%	0.063	12.15 [0.00]	0.413 [.23,.665]	3.1%
Has a professional job	0.34 (0.153)	0.0098 (0.003)	2.9%	0.009	2.54 [0.11]	0.065 [.025,.116]	19%
<i>C. Women</i>							
Human capital index	0.015 (0.228)	0.0096 (0.004)		0.012	6.50 [0.01]	0.066 [.012,.134]	
Completed high school/GED	0.93 (0.087)	0.0021 (0.001)	0.2%	0.002	1.42 [0.23]	0.014 [.001,.031]	1.5%
Attended some college	0.65 (0.163)	0.0054 (0.002)	0.8%	0.006	1.20 [0.27]	0.037 [.01,.072]	5.7%
Completed 4 year college	0.29 (0.147)	0.0047 (0.003)	1.6%	0.008	4.63 [0.03]	0.032 [-.008,.08]	11%
Prof. or doc. degree	0.022 (0.042)	0.0011 (0.001)	5.0%	0.001	3.78 [0.05]	0.008 [-.006,.023]	36%
Years of schooling	13.62 (0.795)	0.024 (0.013)	0.2%	0.038	4.91 [0.03]	0.17 [-.01,.376]	1.2%
Has a professional job	0.37 (0.150)	0.0051 (0.003)	1.4%	0.005	4.35 [0.04]	0.035 [-.005,.083]	9.5%

Notes: In column 1, the control mean and standard deviation are calculated using the cohorts ages 6 and 7 at the time Head Start was launched. Column 2 presents the estimated intention-to-treat (ITT) effect evaluated at birth cohort of

full exposure (-1, see figure 1). Columns 3 and 7 compute the percentage increase implied by the ITT or ATET, respectively, relative to the control mean (the ratio of column 2 or 6 to column 1) for components of the index. Column 4 presents the ITT spline estimate evaluated at -1. Column 5 presents the F-statistic and p-value for the test of a trend-break in the spline at age 6. The ATET estimate in column 6 divides the ITT effect at -1 by the estimate of receiving a Head Start grant on school enrollment at school age 5, 0.149 (s.e. 0.022) for the full sample and 0.151 (s.e. 0.022) for men and 0.145 (s.e. 0.022) for women; see Appendix Table A4).

Table 2. The Effect of Head Start on Adult Economic Self-Sufficiency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variables	Control mean (std.)	Event study at -1 (s.e.)	ITT % Gain	Spline at -1	F-stat on break at	ATET [95% CI]	ATET % Gain
<i>A. Men and Women</i>							
Self-sufficiency index	0.026 (0.16)	0.0055 (0.0024)		0.0048	4.24 [0.04]	0.037 [.005,.076]	
Worked last year	0.84 (0.10)	0.0010 (0.0014)	0.1	0.0011	0.685 [0.41]	0.007 [-.012,.027]	0.83%
No. weeks worked last year	41.1 (5.3)	0.047 (0.0746)	0.1	0.055	0.610 [0.43]	0.313 [-.684,1.39]	0.76%
Usual hours works per week	35.7 (4.9)	0.0334 (0.0795)	0.1	0.056	1.09 [0.30]	0.224 [-.841,1.35]	0.63%
Log labor income	10.3 (0.35)	0.0039 (0.0056)		0.0048	2.33 [0.13]	0.026 [-.048,.11]	
Log family income/poverty	5.87 (0.31)	0.0069 (0.0058)		0.0059	1.52 [0.22]	0.046 [-.03,.13]	
In poverty*	0.10 (0.08)	-0.0018 (0.0012)	-1.8	-0.0020	3.23 [0.07]	-0.012 [-.004,.030]	-12%
Rcvd. public assistance*	0.11 (0.09)	-0.0048 (0.0010)	-4.3	-0.0037	7.13 [0.01]	-0.032 [.018,.052]	-29%
<i>A. Men</i>							
Self-sufficiency index	0.031 (0.20)	0.005 (.003)		0.006	4.24 [0.21]	0.03 [-.01,.077]	
Worked last year	0.91 (0.11)	0.003 (0.001)	0.3	0.003	2.33 [0.13]	0.019 [.001,.041]	2.1%
Weeks worked last year	44.79 (6.03)	0.153 (0.078)	0.3	0.168	2.46 [0.12]	1.01 [.002,2.221]	2.3%
Usual weekly hours	41.31 (6.04)	0.148 (0.088)	0.4	0.172	2.94 [0.09]	0.98 [-.161,2.317]	2.4%
Log labor income	10.88 (0.30)	0.003 (0.004)		0.002	0.33 [0.57]	0.022 [-.035,.083]	
Log family income/poverty	5.91 (0.30)	0.004 (0.005)		0.002	0.07 [0.79]	0.023 [-.04,.092]	
In poverty*	0.070 (0.10)	-0.000 (0.001)	-0.5	-0.001	0.01 [0.90]	-0.002 [-.02,.016]	-2.9%
Rcvd. public assistance*	0.11 (0.11)	-0.005 (0.002)	-4.2	-0.005	9.93 [0.00]	-0.03 [-.056,-.01]	-27%
<i>B. Women</i>							
Self-sufficiency index	0.02 (0.18)	0.006 (0.003)		0.004	4.24 [0.10]	0.04 [.001,.088]	
Worked last year	0.80 (0.13)	-0.002 (0.002)	-0.2	-0.002	0.15 [0.70]	-0.012 [-.043,.016]	-1.5%
Weeks worked last year	37.66 (6.80)	-0.097 (0.109)	-0.3	-0.093	0.03 [0.87]	-0.668 [-2.312,.858]	-1.8%
Usual weekly hours	30.49 (5.81)	-0.108 (0.097)	-0.4	-0.092	0.00 [0.95]	-0.745 [-2.238,.595]	-2.4%
Log labor income	10.31 (0.34)	0.006 (0.005)		0.007	3.94 [0.05]	0.043 [-.025,.12]	
Log family income/poverty	5.81 (0.32)	0.008 (0.005)		0.008	0.68 [0.41]	0.057 [-.01,.137]	
In poverty*	0.12 (0.11)	-0.003 (0.002)	-2.3	-0.003	4.21 [0.04]	-0.019 [-.047,.004]	-16%
Rcvd. public assistance*	0.12 (0.12)	-0.005 (0.001)	-4.1	-0.003	0.67 [0.41]	-0.034 [-.06,-.014]	-28%

Notes: *In poverty and received public program income are reverse-coded when used in the self-sufficiency index.
See also Table 1 notes.

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