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 in large-scale, linked 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 children induced to participate in Head Start achieved 0.29 more years of education, reflecting a 2.1-percent increase in high school completion, a 8.7-percent increase in college enrollment, and 18.6-percent increase in college completion. Consistent with the program benefitting lower income children, participation in Head Start decreased adult poverty by 12 percent and the receipt of public-program income by 28 percent. Our estimates are smaller in magnitude than those reported in other studies, but nevertheless imply substantial returns to investing in large-scale, publicly funded 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). This now-famous "prep school for poor kids" was aptly named "Head Start" (Levitan 1969), and its educational, health, and social programming aimed to help millions of disadvantaged 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 approximately $7 billion (Haskins and Barnett 2010). Unlike a number of resource-intensive, small-scale "model" programs such as Perry Preschool and Abecederian, Head Start's architects prioritized wide-spread access, calculating that a massive preschool expansion would maximize its poverty-fighting (and political) benefits. From Head Start's inception, skepticism about the quality of this large-scale public pre-school program coupled with difficulties in evaluation have generated controversy about its benefits for children (Westinghouse Learning Corporation 1969, Currie 2001, Duncan and Magnuson 2013, Phillips et al. 2017). Unsurprisingly, convincing evidence regarding the Head Start's long-term poverty fighting benefits has remained even more elusive, thanks to the lack of randomization in the early years of the program, the small sizes of longitudinal samples, and the difficulty of measuring today's adults' access to Head Start decades ago. Whether Head Start achieved its goal of increasing the life opportunities for children remains an open question.

This paper uses newly available large-scale administrative data to provide precise estimates of Head Start's long-term effects on human capital and economic self-sufficiency. Not only do we observe adult outcomes for almost one third of the U.S. population in the long-form 2000 Census and 2001-2013

American Community Surveys (ACS). Links to the Social Security Administration's (SSA) Numident allow us measure Head Start access with great accuracy using county of birth and exact date of birth given school-entry age cutoffs. The resulting analytic sample is four orders of magnitude larger than in longitudinal samples and should also reduce potentially endogenous error in the measurement of childhood Head Start access.

Our research design exploits county-by-year variation in the *launch* of Head Start programs due to exploit the "great administrative confusion" at the Office of Economic Opportunity (OEO) (Levine 1970, Bailey 2012, Bailey and Danziger 2013, Bailey and Duquette 2014, Bailey and Goodman-Bacon 2015), side-stepping concerns that variation in Head Start *funding* is endogenous (Barr and Gibbs 2017, Johnson and Jackson 2017, Thompson forthcoming).[1] Using the launch of Head Start has the additional feature of allowing us to compare cohorts in the same birth county based on their age-eligiblity for the program. If Head Start had long-term effects, we expect to see a trend break in the outcomes of otherwise comparable adults for precisely cohorts who were eligibile for a newly launched Head Start program. Similar to a regression kink methodology (Card et al. 2015), 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.

Our results provide new evidence that a large-scale public preschool program impacted the economic opportunities of poor children. 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 18.6 percent more likely to complete college. Consistent with the program benefitting children at risk of living in poverty, Head Start decreased adult poverty by 12 percent and the likelihood of receiving income from public programs by 28 percent. Although the human capital effects we estimate are much smaller than in previous studies, we conclude with a simple calculation suggesting that access to Head Start nevertheless reduces adult poverty at XXX the cost of other public programs.

[1] For instance, year-to-year variation in Head Start spending may (1) reflect endogenous OEO funding decisions (e.g., more funding for programs that are succeeding), (2) be correlated with other local OEO spending, and (3) be measured with (non-classical) error.

I. THE LAUNCH OF HEAD START IN THE 1960S

In the 1960s, the idea that preschool could improve children's cognitive development was revolutionary and resonated with the OEO as an innovative strategy for poverty prevention. Because poor children started school with significantly less educational background, providing comprehensive preschool could give them a "Head Start," improve their success in school, and address a root cause of poverty.[2]

Head Start first began as an 8-week summer program in 1965 and, after a successful first summer, Johnson announced that Head Start would become a full-year program for children ages 3 to 5 in the subsequent academic year.[3] The director of the OEO wrote 35,000 letters to public health directors, school superintendents, mayors and social services commissioners to encourage applications. As noted by Ludwig and Miller (2007), the OEO focused on generating applications in America's 300 poorest counties.

Head Start's political popularity led to an even faster launch than other War on Poverty programs. By 1966, Head Start had launched 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 detailed analysis of the program's roll-out appears in our Appendix.

Head Start aimed to provide holistic programming to develop both the health and physical abilities of poor children, including their self-confidence and relational skills, verbal and conceptual skills, and parent involvement with children.[4] But the difficulty of starting a new *national* program quickly meant that implementation often deviated from the program's ideals. Not only did Head Start lack curricular

[2] Even in the 1970 Census after the program had begun, children of mothers with less than a high school degree were 40 and 30 percent less likely to enroll in preschool and kindergarten, respectively, than the children of college-educated mothers.

[3] Public kindergarten was not universal in this period (Cascio 2009), so "pre-school" also included programs for five-year-olds.

[4] For instance, Levitan (1969) notes that in Head Start's 1966-7 budget, early childhood education (daily activities and transport) comprised about 70 percent; health services (including immunizations, screenings and medical referrals) and nutrition added another 17-20 percent to its budget; and parent involvement, social services (e.g., helping families cope with crises), and mental health services accounted for the remainder. Spending on health and other services increased over time.

standardization, but, in the early years, programs struggled to find high-quality teachers to achieve the suggested pupil-to-(skilled)-teacher ratio of 15:1. The practical solution was for many centers to rely on para-professionals, most of whom lacked post-secondary education and thirty percent of whom had not finished high school. (Hechinger 1966, Braun and Edwards 1972).[5] In addition, many dimensions of Head Start were phased in slowly. For instance, in 1965 the OEO wrote that "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).

These features of Head Start's implementation imply a specific 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 some effect of Head Start on children over age 5 when Head Start launched. This is because 10 percent of children in full-year Head Start enrolled were 6 or older (Vinovskis 2008) and because these children may benefit in the long-term from younger siblings' participation. Using 6 year olds as a control group (our preferred specification) is, therefore, conservative. Second, we expect the relationship of age at Head Start's launch and adult outcomes to change for children under age 5 when it began. The program's gradual implementation captures gains in capacity—full-year Head Start served only 20,000 children in 1965, but this rapidly increased to 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).[6] Implementation also captures increases in quality, including hiring better teachers, curricular advances, and the phase-in of auxiliary services. Changes in cumulative exposure captures the fact that some children will be eligible for up to three years. A child age 5 when Head Start launched could participate for one year, whereas children age 3 at launch would be age-eligible for three years. The combination of gradual implementation and cumulative exposure implies a

[5] Issues of differences in quality in the implementation of public pre-school continue today. See an overview of this literature by Currie (2001), Cascio and Schanzenbach (2013), and Duncan and Magnuson (2013).

[6] Enrollment in summer Head Start was much higher, but we expect the summer program to have smaller effects than full-year exposure to a Head Start program. Even at the beginning of the program, few experts on the planning committee believed that an 8-week summer program could produce lasting benefits (Vinovskis 2008 citing Edward Zigler). Moreover, in this period, 30 to 40 percent of children in summer Head Start were aged six and older, whereas no more than 10 percent of those in full-year programs were older than five. See Table 1 in Thompson (forthcoming).

non-linear expected relationship between Head Start exposure and adult outcomes. Our discussion of results focuses on cohorts born one year after Head Start launched (age -1), because these children would have been age-eligible for a fully implemented program for three years.

II. Evidence Regarding the Long-Term Effects of Head Start

We review of the evidence regarding Head Start's long-term effects to contextualize our results and frame their contribution.[7] One of the first approaches to examining Head Start's long-term effects 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 documented 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). However, some work has been critical of this approach, noting that sibling comparisons may suffer from well-known sources of endogeneity bias (Griliches 1979, Bound and Solon 1999). Moreover, the small sample sizes in longitudinal data may provide unreliable estimates of Head Start's effects (Miller et al. 2017).

More recent work exploits plausibly exogenous shifts in access to Head Start based on a discontinuity in the OEO's launch of new programs (Ludwig and Miller 2007), income-eligibility thresholds (Carneiro and Ginja 2014), and county-level spending variation (Barr and Gibbs 2017, Johnson and Jackson 2017, Thompson forthcoming). Ludwig and Miller's path-breaking application of RD 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 300-county threshold, they find evidence that Head Start

[7] See also excellent reviews of studies of Head Start's short-term effects (Currie 2001, Cascio and Schanzenbach 2013, Duncan and Magnuson 2013, Gibbs et al. 2014).

reduced childhood mortality and increased the receipt of high-school degrees and college enrollment. Because the 1990 and 2000 Censuses required them to use county of residence in adulthood to proxy for childhood Head Start access, their education results are sensitive to specification and not always statistically significant.[8] 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-level, year-to-year variation in Head Start funding to quantify the program's effects. Using 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 raised adult household income. Similarly, Johnson and Jackson (2017) find that an average level of Head Start spending increases the share of the most disadvantaged quartile of a cohort graduating from high school by 8 percentage points. They also find that an average increase in Head Start spending of around $4000 per poor 4-year-old translates into ITT effects of a 0.40 increase in years of education; an 8 log-point increase in adult wages and a 14.4 log-point increase in adult family income at ages 20 to 50; a 3.5 percentage-point reduction in poverty at ages 20 to 50; and a 2.8 percentage-point reduction in adult incarceration. Assuming these effects are driven by enrollment in Head Start and that 40 percent of poor children attended full-year Head Start (their calculation on p. 23) roughly doubles these magnitudes to a 0.8 of a year of education, 16 log-point increase in adult wages (of men and women), a 29 percentage point increase in adult family income, 7 percentage point reduction in poverty, and a 5.6 percentage point reduction in adult incarceration.[9] Finally, Barr and Gibbs (2017) examine the intergenerational effects of Head Start using a sample from the NLSY and variation in program availability across birth counties and cohorts. Among the daughters of women with less than a high school education,

[8] There is also limited evidence showing the poorest 300 counties got were more likely to get grant-funding for Head Start (see Ludwig and Miller 2007: table II and Pihl 2017).
[9] These effects increase by 30 percent if they were also driven by full-year *and* summer Head Start (p. 23).

they find that the existence of a local Head Start program when a girl was age 4 or 5 is associated with an 11 percentage-point increase in the probability of finishing high school and more than one-half an extra year of schooling completed.

III. DATA AND RESEARCH DESIGN

This study combines the long-form 2000 Census and 2001-2013 ACS with the Numident to evaluate Head Start's long-term effects. The Census/ACS contain adult outcomes for almost one third of the U.S. population. The resulting data are both four orders of magnitude larger than available longitudinal samples and contain a high quality proxy for Head Start access and age eligibility in childhood, which relies on the Numident's county of birth (rather than adulthood residence) and exact date of birth. The data's main disadvantage is that they contain no information on family background, which limits analyses of the lower-income children heavily represented in the program.[10] Our sample of interest is 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). To minimize disclosure concerns at the Census Bureau, we use only observations with non-allocated and non-missing values for all outcomes. (See our Appendix for more details.)

Information on the launch of Head Start programs is based on the work of(Bailey and Goodman-Bacon 2015). Combining this work with our large-scale data permit two refinements to previously used research designs (Barr and Gibbs 2017, Johnson and Jackson 2017, Thompson forthcoming). First, we use only the launch of the Head Start program rather than Head Start spending. This refinement addresses the potential (1) endogeneity of Head Start funding decisions to the program's *performance* and (2) the correlation of year-to-year variation in Head Start funding with that of *other* OEO programs. As shown the Appendix, the correlation between OEO per-capita funding and the launch of Head Start grant is very small and statistically insignificant.[11] Second, we examine changes in the relationship of interest for children who were *age-eligible* for Head Start when it launched (ages 3-5) to those who were age-ineligible (ages 6+).

[10] 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).

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

Age eligibility is determined using exact date of birth in the Numident and school-entry age cutoffs. 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.

We implement these comparisons within a dynamic, event-study framework,

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

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. We follow the literature and also include county characteristics interacted with a linear trend, $\boldsymbol{Z}_{cb}$ (Hoynes et al. 2011, Bailey 2012, Bailey and Goodman-Bacon 2015), but these covariates matter little.[12] Standard errors are corrected for heteroskedasticity and adjusted for an arbitrary within-county covariance structure (Arellano 1987, Bertrand et al. 2004). More detailed variable descriptions and robustness checks are presented in the Appendix.

Because Head Start could affect so many aspects of individuals' lives, we analyze summary measures of human capital and economic self-sufficiency, which permits a powerful test of co-movements of related adult outcomes and limits the number of statistical tests (Kling et al. 2007).[13] 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

[12] 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.

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

professional occupation. Our index of self-sufficiency includes binary indicators of employment, poverty status, and income from public sources, weeks worked, usual hours worked, and the log of labor income.

Our point estimates of interest, $\boldsymbol{\varphi}$, capture the ITT-effects of Head Start on long-term human capital and labor-force productivity outcomes. The set of these effects describe the evolution of outcomes for children born in counties, c, that received Head Start programs (*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 +10, which are compositionally balanced.

Based on Figure 1's predictions, we summarize the event-study estimates using a three-part spline with knots at age 6 and -1. The spline has the added benefit of allowing us to conduct formal trend-break tests at age 6 to characterize whether the large increase in age-eligibility for Head Start at age 5 is associated with a change adult outcomes.

These effects of interest, $\boldsymbol{\varphi}$**,** are intention-to-treat effects and average over all county residents, regardless of whether they used Head Start. To compare our estimates to the literature, we generate average treatment-effects-on-treated children (ATET) by dividing by 0.151, our best estimate of the effect of Head Start launch on program enrollment. (See Appendix for more details.) We construct confidence intervals using a parametric bootstrap procedure that uses 10,000 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). We calculate the ATET for each draw and use the empirical distribution of ATETs to calculate the 95-percent confidence interval. We also use this method to calculate of confidence intervals for other papers without resampling from their data.

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 the spline estimate. The results suggest that Head Start dramatically improved adult human capital. The

standardized index is about one-thirteenth of a standard deviation (sd) (column 2 / sd in column 1) higher for the cohort overall and half a standard deviation for treated children (column 6 / sd in column 1). Although the spline trades flexibility for parsimony, it fits the data well: column 4's estimates are identical to 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 selection of outcomes driving changes in the index. The x-axis is the child's age at the time Head Start launched, and the dashed lines plot the spline segments. Consistent with a causal interpretation, each index component shows a visual trend break at age 6 with increasing ITT estimates as anticipated in Figure 1.

Panels A and B of Figure 2 summarize event-study estimates for two of the most commonly studied human-capital outcomes in the literature: high school graduation and college enrollment. Table 1 shows that treated children were 2.1 percent more likely to complete high school/GED. This is much smaller than effects 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 using the NLSY. In addition, it is one-fifth the size of Johnson and Jackson (2017)'s estimates for a very disadvantaged sample in the PSID; and one-nineth 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.) Although our estimate falls within the confidence intervals of previous studies, this reflects the large uncertainty associated with those estimates.

Similarly, Table 1 shows a statistically significant effect of Head Start on college enrollment. Head Start raised college enrollment by 1.3 percent, which translates into 8.6 percent among treated children. Figure 3B shows that our estimate is one half the size of Garces et al. (2002) and one quarter the size of Ludwig and Miller (2007). (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 a trend-break at age 6 at 1-percent level.

In addition to generating more precise estimates for these commonly studied outcomes, our data

permit a novel exploration of Head Start on other dimensions of human capital, including college completion or higher degrees. Table 1 shows that Head Start participants achieved 2.8 percent higher college graduation rates—implying a 18.5 percent increase among treated children. Consistent with a causal interpretation, column 5 shows evidence of a trend break at the 1-percent level.[14] These effects are one-quarter to one-fifth the size of the model Abecedarian Project (Currie 2001, Barnett and Masse 2007, Duncan and Magnuson 2013). Similarly, completion of professional or graduate degrees increases by 50 percent among treated children. Overall, these gains at virtually every level of educational attainment are captured by an increase of 0.29 years of schooling for treated children. This estimate is much lower 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.[15]

These results may be surprising, given that no other study of preschool has documented effects on post-secondary education. Note, however, that this lack of evidence may reflect 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 all very disadvantaged children and mostly black, and Perry's participants had low IQs.[16] In contrast, Head Start participants were less economically disadvantaged and roughly half were African American in 1970. It is, therefore, likely that many Head Start participants would have faced fewer socio-economic disadvantages and less racism, which makes is less surprising that Head Start may have influenced post-secondary educational outcomes.

Because analyses of model preschool programs have found larger educational effects for boys, Table 1 also presents effects separately by sex. Among participating men, the human capital index increased by a statistically significant over half of a standard deviation. For this group, high school completion rose by 2.8 percent, college attendance rose by 12.5 percent, and college completion rose by 27 percent. The

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

[15] Jackson and Johnson's ITT estimate is 0.0967 per $1000 spent per poor 4-year-old. They translate this into an ATET by multiplying the coefficient by $4000 (the average Head Start spending per poor 4-year-old), so that 0.0967*4/0.75=0.52

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

high school estimates are, again, smaller than others in the literature: Deming (2009) estimates an effect of 11.4 percentage points, while Johnson and Jackson's estimates imply an ATET of 10.6 percentage points for their very disadvantaged sample. However, the college attendance estimates are much larger than Deming's estimate of a statistically insignificant increase of 0.02 percentage points. Together, these shifts raised years of education among treated men by half of a year and the likelihood of completing a professional or doctoral degree by 58 percent, or 2.4 percentage points. The evidence suggests that men treated with Head Start were 19 percent more likely to hold professional jobs, although the trend-break is weakly significant.

Similar to results in model pre-school programs, the human capital index increased by half as much for women: only 27 percent of a standard deviation among treated women. In addition, the relationships of other index components with Head Start exposure were smaller and sometimes statistically insignificant. Completion of high school (or a GED) rose by 1.5 percent, and college attendance rose by 5.5 percent, although the trend-break at age 6 is not statistically significant at conventional levels. Changes in the human capital index appear more directly affected by changes in college completion and professional degrees, which rose by 10.7 percent and 33 percent, respectively. Overall, treated women's years of education rose by 0.19 years and their likelihood of holding a professional job rose by 9 percent.

V. HEAD START'S EFFECTS ON LABOR-MARKET OUTCOMES

The substantial effects of Head Start on human capital naturally raise the question of whether the program improved labor-market outcomes. When examining the economic self-sufficiency aggregated over both sexes, we find that Head Start improved treated adult's self-sufficiency by one sixth of a standard deviation—an estimate that is not statistically different from zero (we omit these results for brevity). This null result likely reflects the fact that men and women's work effort (and likely selection) changed in offsetting ways. Head Start's effects on men's human capital may have led them to increase employment (if the substitution effect of higher potential wages dominates), but the reverse may be true for women (if greater education enables marriage to higher-earning men). Head Start could lead women to lower labor-force participation by raising the income of husbands.

To investigate these effects, Table 2 examines the components of the self-sufficiency index by sex.

Stratifying by sex significantly reduces sample sizes, so we focus our analysis on the spline estimates. For treated men, the self-sufficiency index increased by about one-seventh of a standard deviation. Notably, we find positive effects of Head Start exposure on both the extensive and intensive margins of men's labor-force participation. Column 6 shows that treated men were 2.1 percentage points more likely to have worked for pay, and they worked an average of 5 more days in the previous year (1 week) and 1.2 additional hours per week. 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—perhaps reflecting the role of negative selection into work. Although Head Start reduced the receipt of income from public sources by 3.6 percent, it appears to have little effect on men's poverty.

As expected, the pattern is different for women. Although the self-sufficiency index for treated women was around one-eighth of a standard deviation, their labor-force participation was slightly lower. Column 6 shows that treated women were 1.5 percent *less* likely to work for pay. Women also worked fewer weeks and hours, although these estimates are not statistically different from zero. These reductions in work appear to have *increased* annual wages of working women by around 5 percent, which this is consistent with Head Start engendering positive selection as less-skilled women opt out. Finally, Table 2 suggests that that Head Start reduced treated women's receipt of income from public sources by 27 percent and poverty rates by 15 percent.

Consistent with the intended effects of the program, the effects of Head Start appear more clearly at the lower end of the skill distribution. Aggregating over both men and women, exposure to Head Start decreased the likelihood of adult poverty by 1.1 percentage points among treated individuals. In addition, treated adults were 2.8 percentage points less likely to receive income from public programs.

VI. Reassessing the Long-Term Economics of Head Start on Human Capital

Over the past 20 years, substantial evidence has accumulated that *model* preschool programs may have sizable economic returns (Cunha and Heckman 2007, Heckman et al. 2010, Almond and Currie 2011, Duncan and Magnuson 2013). However, the convincing evidence on the long-run returns to larger-scale, public preschool has remained sparse (Phillips et al. 2017).

This paper provides precise estimates of the long-term effects of Head Start, the nation's longest running, large-scale preschool program. Children exposed to Head Start achieved 0.25 more years of schooling, reflecting the fact that they were 1.9 percent more likely to complete high school, 7.7 percent more likely to enroll in college, and 16.5 percent more likely to complete college. Although these human capital estimates are smaller than in previous work, they nevertheless imply substantial returns to the program. The association between educational attainment and economic fortunes is at its highest level in measured U.S. history (Goldin and Katz 2008), and spill-overs across workers (Moretti 2004) or generations (Black and Devereux 2011) could make the social returns even larger than we capture here (Barr and Gibbs 2017). These estimates also reveal the cost-effectiveness of Head Start as a higher-education intervention. Because Head Start in the early 1970s cost roughly $5,400 per child in 2013 dollars, the implied cost per college enrollee is $80,686 (=$5,400/0.054)—which compares favorably to other programs intending to increase college attendance and graduation (Dynarski et al. 2013).

A second finding is that Head Start improved the adult self-sufficiency of disadvantaged children. Even the imperfect implementation of the program in the 1960s decreased participants' public assistance receipt by 25 percent and adult poverty by 11 percent. The former implies an *annual* cost savings of at least $116 per Head Start enrollee in our Census/ACS sample. After discounting and factoring in the up-front cost of $5,400 per child (and ignoring any other benefits of the program), this implies a return of XXXX to Head Start. The latter effect suggests the cost effectiveness of Head Start in decreasing poverty. Using the supplemental poverty measure for 2012, Temporary Assistance for Needy Families cost $10.24 billion in 2013 dollars and reduced poverty rates of non-elderly adults by 0.65 percent (0.1/15.5) (Short 2013). Ignoring offsetting behavioral changes and deadweight loss, this implies that TANF reduced poverty by 1 percent for every $15.8 billion spent. By comparison, Head Start reduced poverty by 1 percent for every 127 million spent ($1.4 billion/11) spent.

Several reasons suggest that these estimates are conservative. First, our research design differences out any effects on older siblings. Second, our estimates of Head Start's launch on participation may be too large, implying that the ATET effects are larger than we show. Third, other pre-school studies suggest that

Head Start may have affected outcomes that we cannot quantify with the Census/ACS, such as incarceration, health, and longevity. Finally, Head Start today is a higher quality program than when it launched, with costs almost twice as high at $8,000 per child. For all of these reasons, one might anticipate even larger returns to today's programs. Future research should consider these additional outcomes and a more formal cost-benefit analysis to quantify what appear to be considerable social returns to investments in Head Start.

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

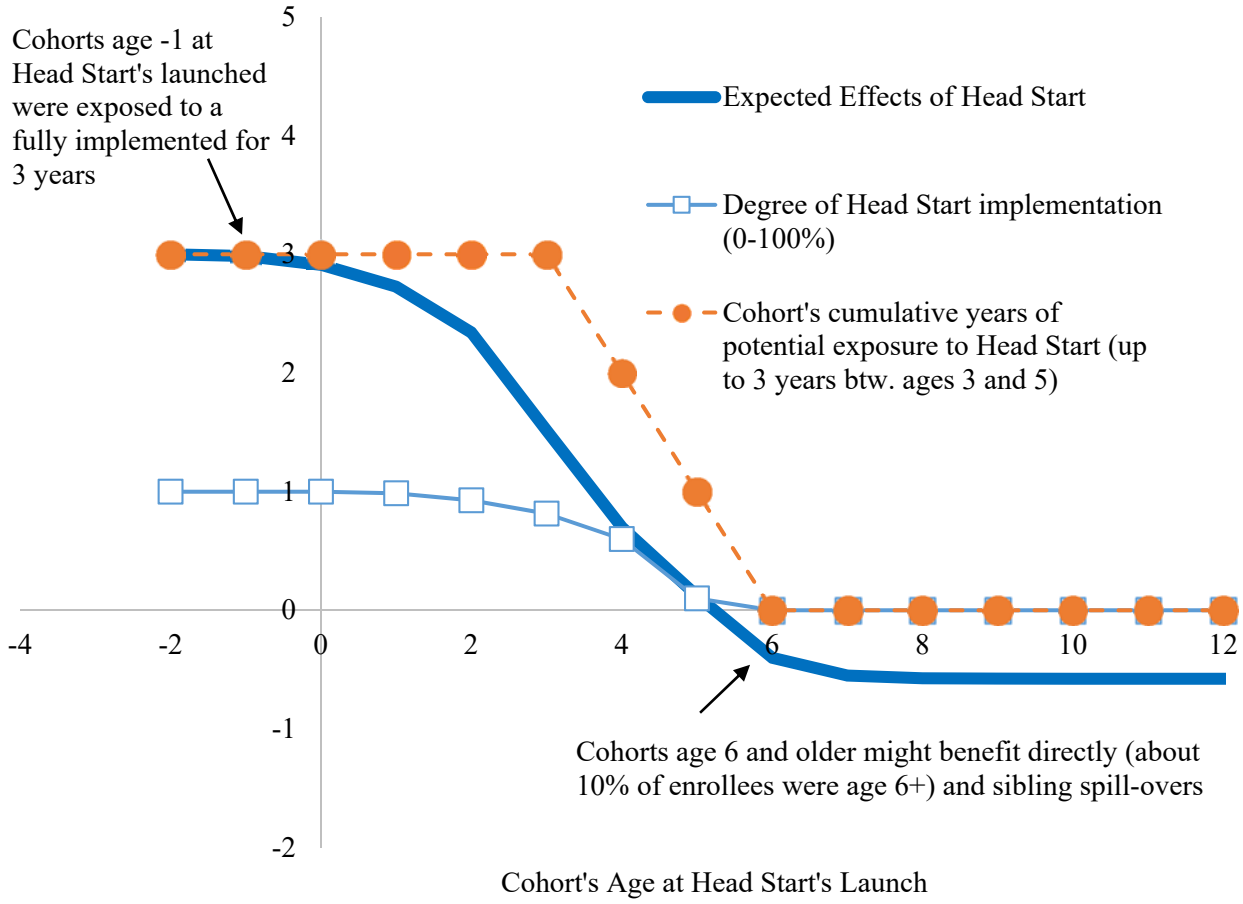


Figure 2. The Effect of Head Start on Adult Human Capital and Self-Sufficiency

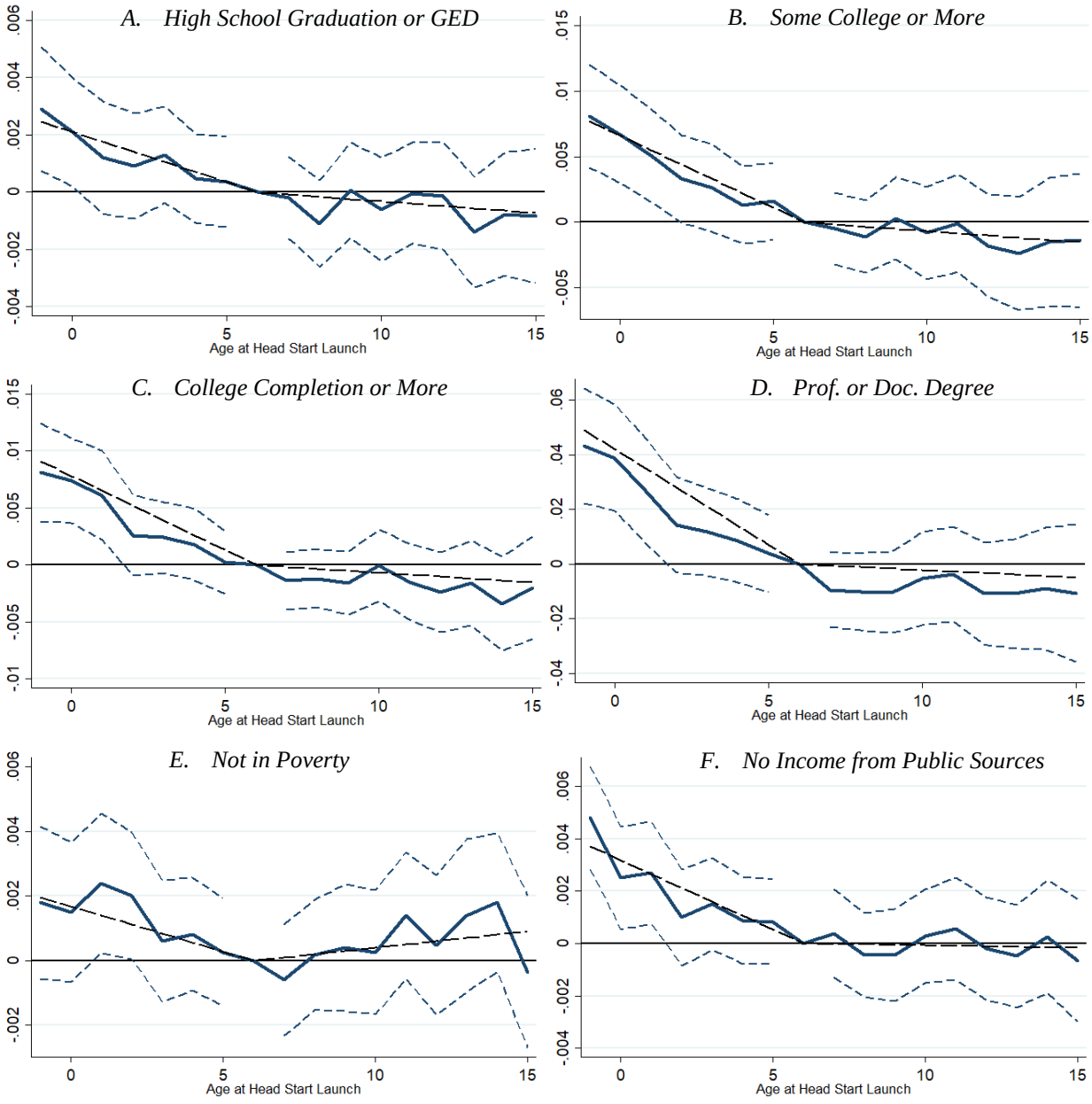
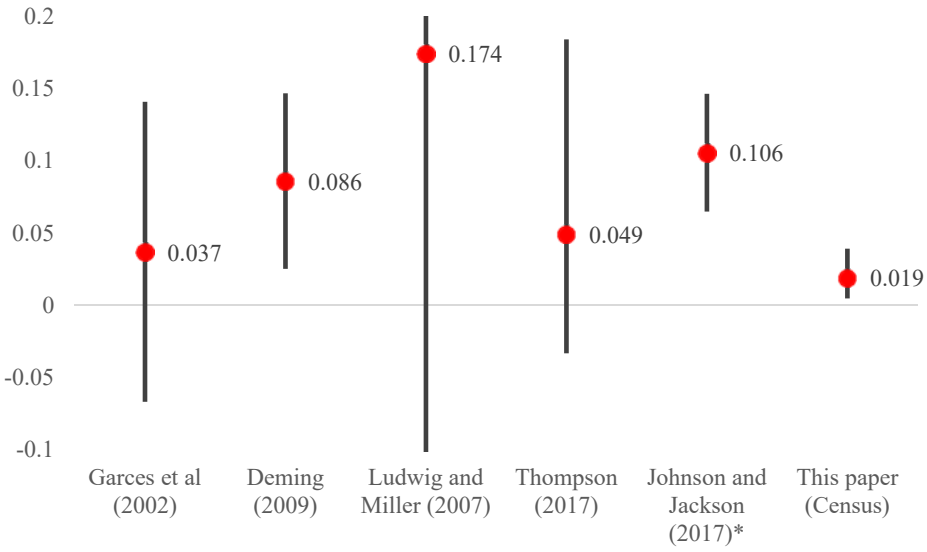


Figure 3. The Magnitude of Head Start's Effects on Education across Studies

A. Effects of Head Start on High School Graduation



B. The Effects of Head Start on College Enrollment



Notes: Circles indicate show implied or reported 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 in Panel A does not include that 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) sample includes PSID individuals born to parents in the bottom quartile of the income distribution.

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

Dependent Variables	(1) Control Mean (St. Dev.)	(2) Event Study at -1 (s.e.)	(3) % Gain	(4) Spline at -1	(5) F-stat on break at 6 [p-value]	(6) ATET - Census [95% CI]
A. Men and Women						
Human capital index	0.014	0.015	7.6^{+}	0.015	14.02	0.100
	(0.198)	(0.003)			[0.00]	[0.056,0.170]
Completed high school/GED	0.92	0.003	0.3	0.002	2.18	0.019
	(0.078)	(0.001)			[0.14]	[0.005,0.039]
Attended some college	0.62	0.008	1.3	0.008	4.84	0.054
	(0.140)	(0.002)			[0.03]	[0.026,0.095]
Completed 4 year college	0.29	0.008	2.8	0.009	11.66	0.054
	(0.122)	(0.002)			[0.00]	[0.026,0.095]
Prof. or doc. degree	0.028	0.002	7.5	0.002	7.36	0.014
	(0.037)	(0.001)			[0.01]	[0.001,0.031]
Years of schooling	13.57	0.043	0.3	0.049	13.06	0.287
	(0.695)	(0.011)			[0.00]	[0.137,0.514]
Has a professional job	0.35	0.007	2.1	0.007	4.97	0.048
	(0.121)	(0.002)			[0.03]	[0.021,0.088]
B. Men						
Human capital index	0.014	0.0206	8.3^{+}	0.020	11.79	0.136
	(0.247)	(0.004)			[0.00]	[0.077,0.232]
Completed high school/GED	0.91	0.0038	0.4	0.003	1.92	0.025
	(0.104)	(0.002)			[0.17]	[-0.001,0.059]
Attended some college	0.59	0.0111	1.9	0.010	5.67	0.074
	(0.176)	(0.003)			[0.02]	[0.033,0.134]
Completed 4 year college	0.29	0.0116	4.0	0.011	9.99	0.077
	(0.150)	(0.003)			[0.00]	[0.036,0.138]
Prof. or doc. degree	0.034	0.003	8.8	0.003	4.73	0.020
	(0.052)	(0.001)			[0.03]	[0.007,0.039]
Years of schooling	13.52	0.0623	0.5	0.063	12.15	0.413
	(0.795)	(0.013)			[0.00]	[0.225,0.707]
Has a professional job	0.34	0.0098	2.9	0.009	2.54	0.065
	(0.153)	(0.003)			[0.11]	[0.025,0.123]
C. Women						
Human capital index	0.015	0.0096	4.2^{+}	0.012	6.50	0.064
	(0.228)	(0.004)			[0.01]	[0.011,0.134]
Completed high school/GED	0.93	0.0021	0.2	0.002	1.42	0.014
	(0.087)	(0.001)			[0.23]	[0.001,0.031]
Attended some college	0.65	0.0054	0.8	0.006	1.20	0.036
	(0.163)	(0.002)			[0.27]	[0.009,0.072]
Completed 4 year college	0.29	0.0047	1.6	0.008	4.63	0.031
	(0.147)	(0.003)			[0.03]	[-0.008,0.079]
Prof. or doc. degree	0.022	0.0011	5.0	0.001	3.78	0.007
	(0.042)	(0.001)			[0.05]	[-0.006,0.023]
Years of schooling	13.62	0.024	0.2	0.038	4.91	0.159
	(0.795)	(0.013)			[0.03]	[-0.010,0.375]
Has a professional job	0.37	0.0051	1.4	0.005	4.35	0.034
	(0.150)	(0.003)			[0.04]	[-0.005,0.083]

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 4). Column 3 computes the percentage increase implied by the ITT estimate relative to the control mean (the ratio of column 2 to column 1) for all components of the index. [+]For the index itself, column 3 computes the change in the ITT estimate from column 2 divided by the standard deviation of the control mean in column 1. 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 (0.151 for the full sample; see Appendix Table A2).

Table 2. The Effect of Head Start on Adult Labor Market Outcomes

Dependent Variables	(1) Control Mean (St. Dev.)	(2) Event Study at -1 (s.e.)	(3) % Gain	(4) Spline at -1	(5) F-stat on break at 6 [p-value]	(6) ATET -Census [95% CI]
A. Men						
Self-sufficiency index	0.035	0.005	2.2[+]	0.005	1.28	0.030
	(0.208)	(0.003)			[0.26]	[-0.009,0.078]
Worked last year	0.91	0.003	0.3	0.003	2.33	0.019
	(0.108)	(0.001)			[0.13]	[0.006,0.038]
Weeks worked last year	44.79	0.153	0.3	0.168	2.46	1.013
	(6.03)	(0.078)			[0.12]	[0.002,2.319]
Usual weekly hours	41.31	0.148	0.4	0.172	2.94	0.980
	(6.04)	(0.088)			[0.09]	[-0.156,2.419]
Log labor income	10.88	0.003	0.0	0.002	0.33	0.022
	(0.301)	(0.004)			[0.57]	[-0.031,0.082]
Log family income/poverty	5.91	0.004	0.1	0.002	0.07	0.023
	(0.302)	(0.005)			[0.79]	[-0.043,0.098]
In poverty[*]	0.070	-0.000	-0.0	-0.001	0.01	-0.002
	(0.095)	(0.001)			[0.90]	[-0.016,0.012]
Rcvd. public program income[*]	0.11	-0.005	-0.5	-0.005	9.93	-0.031
	(0.110)	(0.002)			[0.00]	[-0.065,-0.004]
B. Women						
Self-sufficiency index	0.020	0.004	2.1[+]	0.003	1.50	0.026
	(0.189)	(0.003)			[0.22]	[-0.013,0.073]
Worked last year	0.80	-0.002	-0.2	-0.002	0.15	-0.012
	(0.129)	(0.002)			[0.70]	[-0.042,0.015]
Weeks worked last year	37.66	-0.097	-0.3	-0.093	0.03	-0.642
	(6.80)	(0.109)			[0.87]	[-2.296,0.824]
Usual weekly hours	30.49	-0.108	-0.4	-0.092	0.00	-0.715
	(5.81)	(0.097)			[0.95]	[-2.217,0.584]
Log labor income	10.31	0.006	0.1	0.007	3.94	0.041
	(0.339)	(0.005)			[0.05]	[-0.024,0.119]
Log family income/poverty	5.81	0.008	0.1	0.008	0.68	0.055
	(0.317)	(0.005)			[0.41]	[-0.010,0.137]
In poverty[*]	0.12	-0.003	-0.3	-0.003	4.21	-0.019
	(0.108)	(0.002)			[0.04]	[-0.050,0.007]
Rcvd. public program income[*]	0.12	-0.005	-0.6	-0.003	0.67	-0.033
	(0.115)	(0.001)			[0.41]	[-0.055,-0.018]

Notes: [+]For the economic self-sufficiency index, column 3 computes the change in the ITT estimate in column 2 relative to the standard deviation of the control mean in column 1. [*]In poverty and received public program income are reverse coded when used in the self-sufficiency index. See also Table 1 notes.

[click here for Online Appendices]