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ABSTRACT

We explore whether early childhood human-capital investments are complementary to those made later in life. Using the Panel Study of Income Dynamics, we compare the adult outcomes of cohorts who were differentially exposed to policy-induced changes in pre-school (Head Start) spending and school-finance-reform-induced changes in public K12 school spending during childhood, depending on place and year of birth. Difference-in-difference instrumental variables and sibling- difference estimates indicate that, for poor children, increases in Head Start spending and increases in public K12 spending each individually increased educational attainment and earnings, and reduced the likelihood of both poverty and incarceration in adulthood. The benefits of Head Start spending were larger when followed by access to better-funded public K12 schools, and the increases in K12 spending were more efficacious for poor children who were exposed to higher levels of Head Start spending during their preschool years. The findings suggest that early investments in the skills of disadvantaged children that are followed by sustained educational investments over time can effectively break the cycle of poverty.

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An online appendix is available at http://www.nber.org/data-appendix/w23489

I. Introduction

Children born to less-advantaged households and communities typically experience lower levels of educational attainment, employment, earnings, health, and well-being as adults than children born to more-advantaged ones (Chetty, Hendren, and Katz, 2016). Differences between individuals from more- and less-advantaged backgrounds manifest early in childhood and tend to grow as children age (Fryer and Levitt, 2006; Currie and Thomas, 1999; McLeod and Kaiser, 2004; Heckman and Mosso, 2014). Accordingly, remediating the ill-effects of childhood poverty may require early investments in the skills of disadvantaged children that are followed by sustained investments over time. To examine this issue, we study whether early childhood investments designed to promote school readiness among disadvantaged children that are followed up with increases in public school spending are particularly effective at improving their long-run outcomes.

While the policy question is important in its own right, we test the long-standing hypothesis in economics that, because skills beget skills, children who benefit from early human capital investments may benefit more from later investments (Cunha and Heckman, 2007). If such dynamic complementarities exist, it is argued, policies that promote early human capital formation can (a) make subsequent policies to promote human capital more productive, and (b) may be ineffective if not followed by subsequent investments.[1] To examine this question, some studies examine whether the effect of human capital intervention varies by pre-intervention skill level (e.g. Garcia and Gallegos, 2017; Lubotsky and Kaestner, 2016; Aizer and Cunha, 2012). Because these studies do not use exogenous variation in prior skills, they do not speak directly to whether early and late human capital policies exhibit dynamic complementarity. Addressing this critique, other studies examine whether the benefits of human capital investments vary among those who were exposed to non-investment skill shocks such as hurricanes, or rainfall during gestation (e.g. Adhvaryu, Molina, Nyshadham and Tomayo, 2017). Because these studies do not rely on human capital investments *per se* for variation in initial skills, they do not examine whether human capital investments made at different stages of the life course exhibit dynamic complementarity.[2] To test for dynamic complementarities in human capital investments (in the policy sense, as suggested by

[1] If early childhood investments for disadvantaged youth make subsequent investments in these children more productive, one can justify policies that redistribute resources toward disadvantaged children during their early years on *efficiency* grounds without any appeal to equity issues, fairness, or social justice (Heckman and Mosso, 2014).

[2] These studies, however, are informative about whether investments are more or less productive for individuals who are exposed to some previous human capital shock. In Section II, we discuss how this is related to the idea that human investments may exhibit dynamic complementarity, but point out that they are not the same thing.

Heckman and colleagues) requires an examination of the marginal effect of one human capital investment among individuals who were exposed to a previous human capital investment. While dynamic complementarities may not exist between *any* two human capital investments, they are most likely to exist between two education-related interventions in which one (a school-readiness program) is designed to help children benefit from the other (public K-12 schools). As such, our setting is particularly well-suited for studying dynamic complementarities.[3]

To test for dynamic complementarity we examine the interaction between two exogenous and independent human capital investment "shocks." The first exogenous shock to human capital investment is the rollout of Head Start, the largest early childhood intervention program in the US, which increased access to early childhood education and pediatric care for low-income children. The second exogenous shock to human capital investment is the implementation of court-ordered school finance reforms (SFRs) which (a) reduced differences in public K12 school spending between affluent and poor neighborhoods within states, and (b) increased (on average) the level of per-pupil spending at public K12 schools.[4] To isolate the effects of these two major policies, we exploit temporal and geographic variation in exposure to these policy-induced investment "shocks" and analyze the life trajectories of individuals born between 1950 and 1976, and followed through 2015 using the Panel Study of Income Dynamics (PSID). Identifying the interaction effects between two human capital investments requires that (a) one credibly identify the effects of each investment individually on the same individuals, and (b) that each of the human capital investments is independent of the other (Almond and Mazumder, 2013). We present numerous tests to show that these conditions are likely satisfied in our setting. While test scores have been the traditional focus of evaluations of Head Start and K12 spending, the effects of interventions on long-run outcomes may go undetected by test scores.[5] Consequently, we explore effects on an

[3] Investments are "broadly defined as actions specifically taken to promote learning" in Heckman and Mosso (2014). As such, it is reasonable that some interaction between education-related interventions are what the theory is really about. A few concurrent working papers also rely on two investments. In the only other paper to examine two education-related interventions, Gilraine (2016) finds that the benefits of accountability due to NCLB in later grades are larger among students exposure to accountability in earlier grades. Looking at a health and an education intervention, Rossin-Slater and Wust (2017) find that the effects of access to pre-school was *smaller* among those who had access to home visits during infancy. In related work with a health intervention, Bhalotra and Venkataramani (2017) find that the benefits of antibiotic treatment for blacks decline in measures of the severity of institutionalized segregation. Also Malamud, Pop-Eleches and Urquiola (2016) examine whether the benefits of attending a better school vary by parental access to abortion near the time of conception.

[4] See Card and Payne, 2002; Murray, Evans, and Schwab, 1998; Hoxby, 2001; Jackson, Johnson, and Persico, 2014 for a more complete disucssion of the effects of SFRs on public school spending.

[5] E.g. Heckman, Pinto, and Savelyev, 2014; Jackson, forthcoming; Chetty et al., 2011; Ludwig and Miller, 2007.

array of adult outcomes including educational attainment, earnings, poverty, and incarceration.[6]

To identify the causal effect of early childhood investments, we exploit geographic variation in the timing of the rollout of Head Start across counties. In our difference-in-difference models, we compare the adult outcomes of individuals who were from the same childhood county but were exposed to different levels of Head Start spending, because some were four years old when Head Start spending levels were low (or non-existent) while others were four years old when Head Start spending levels were higher. To validate our models, we show that (a) the identifying variation in Head Start spending is unrelated to family, community, and other policy changes, (b) our estimated Head Start effects are robust to instrumental variable models that use only variation in Head Start spending due to the timing of Head Start rollout in the childhood county, (c) our estimated Head Start effects are robust to using within-family, across-sibling variation in Head Start spending exposure, (d) Head Start spending in a child's county during non-Head Start eligible ages (ages 1 through 3 and 5 through 10) is unrelated to student outcomes conditional on Head Start spending during the target age (age 4), and (e) Head Stat spending has no effect on non-poor populations that were largely ineligible for the program.

To identify the causal effects of public K12 school spending, we exploit geographic variation in the timing of court-ordered SFRs. Following Jackson, Johnson, and Persico (2016), we predict the spending change that each district would experience after the passage of a court-mandated SFR based on the type of reform and the characteristics of the district before reforms. Using instrumental variables models, we examine whether SFR-exposed cohorts (young enough to have been in school during or after a SFR) have better outcomes relative to SFR-unexposed cohorts (those who were too old to be affected by a SFR) in districts predicted to experience larger reform-induced spending increases. We present several empirical tests showing that the within-district variation in per-pupil spending induced by SFRs is exogenous to other family, community, and policy changes in the district. We also show that our K12 school spending effects are robust to using within-family, across-sibling variation in SFR-induced K12 public-school spending, and that SFR-induced spending changes during non-school age years are unrelated to outcomes.

To explore the relationship between early- and later-childhood human capital investments,

[6] In a recent related study, Carneiro and Ginja (2014) study the long-run effects of Head Start participation on health and behavioral problems. We explore a wider array of adult socioeconomic outcomes. A key focus of our paper is on how the effectiveness of Head Start spending varies by the quality of the public schools students subsequently attend.

we combine both identification strategies to estimate the effects of the interaction between Head Start spending and public K12 spending. We can test for dynamic complementarities based on two sources of variation. Namely, some districts experienced increases in school spending due to a SFR when Head Start was available in the county, while other districts experienced similar K12 spending increases when Head Start was not available. This fact allows one to test if the effects of K12 spending increases due to SFRs are higher with greater public pre-K investments than without them. Similarly, Head Start was rolled out in different counties both before and after the local school districts experienced increases in K12 spending due to SFRs. This fact allows one to test if the effects of Head Start spending are larger in areas that have higher levels of K12 spending due to the passage of a court-ordered SFR. For this interaction effect to be identified, it requires that individuals that are exposed to both a SFR and Head Start are not somehow different from those that are exposed to only a SFR or only exposed to Head Start. One can only be confident that this condition is satisfied if both policy changes are independent of each other. We argue that because SFRs occurred at the state level (affecting all public schools in a state at the same time), while Head Start (a federal program) was introduced in certain counties within states at different times, these two policies are largely independent of each other. More formally, we show that (a) the raw correlation between the two policy instruments is only 0.15, (b) conditional on controls there is no association between Head Start spending and SFR-induced changes in K12 spending, and (c) using partial F-statistics, there is sufficient policy variation in Head Start spending and K12 spending for the effect of each to be identified and for the interaction between the two to be identified.

For children from low-income families, on average, increases in Head Start spending increased educational attainment and adult earnings and reduced the likelihood of both poverty and incarceration in adulthood. We find no effect of Head Start spending on the outcomes of non-poor children. Consistent with Jackson, Johnson, and Persico (2016), increases in public school K12 spending improved this same array of outcomes in adulthood. We also find strong and robust evidence of dynamic complementarity. Among poor children exposed to a 10% reduction in K12 spending, exposure to a typical Head Start center has small statistically insignificant effects on educational attainment, wages, incarceration, and adult poverty. However, even in our conservative models, among poor children exposed to a 10% increase in K12 spending, exposure to a typical Head Start center leads to 0.59 additional years of education, being 14.8 percentage points more likely to graduate high school, 17% higher wages, being 4.7 percentage points less likely to be incarcerated, and being 12 percentage points less likely to be poor as an adult. The fact that the long-run benefits of Head Start spending depend on the subsequent level of K12 spending may help explain why some studies find positive effects of Head Start and others do not.[7] Looking at the marginal effects of K12 spending, for low-income children, increasing public K12 spending by 10% has small effects on educational attainment, adult wages, and incarceration when not preceded by Head Start. However, among low-income children exposed to Head Start, that same 10% increase in K12 per-pupil spending increases educational attainment by 0.4 years, increases earnings by 20.6 percent, and reduces the likelihood of incarceration by eight percentage points. The positive interaction effects between Head Start and K12 spending are robust across several models (including sibling comparisons) and are only present among poor children (who were eligible for Head Start). The effect of K12 spending was unrelated to the level of Head Start spending among non-poor children, for whom increasing K12 spending by 10% increased years of education by 0.2 and earnings by 11.7 percent.

We find long-run benefits of public early childhood investments and robust evidence of complementarities between early and later human capital investments for low-income children. The complementarities imply that one could increase *both* equity and efficiency by redistributing spending from well-funded K12 schools toward Head Start programs targeted at poor children. Generally, our results are the first to show that early and sustained complementary investments in the skills of low-income children can be a cost-effective strategy to break the cycle of poverty.

The rest of the paper is organized as follows. Section II outlines our theoretical framework. Section III describes the Head Start program and court-ordered school finance reforms. Section IV presents the data used. Section V describes the empirical strategy. Section VI presents the results. Section VII presents conclusions and a summary discussion.

II. Theoretical Framework

Research in developmental neuroscience highlights the importance of the preschool years in establishing the building blocks of subsequent human capital formation and the interconnectedness of cognitive, non-cognitive, and health formation (Shonkoff and Phillips, 2000). Informed by this research, Cunha and Heckman (2007), theorize that skill development is

[7] For positive effects see Deming (2009), Ludwig and Miller (2007), Garces, Currie, and Thomas, (2002), Carneiro and Ginja (2014). For mixed effects see Zigler et al., (2011), Lipsey, Farran and Hofer (2015).

an interactive, multistage process in which the marginal effect of investments today is higher among those with a greater stock of previously acquired skills. We refer to this characteristic of skills production as dynamic complementary *in skill development.*[8] When this condition holds, investments that increase skills early in life make subsequent skill investments more productive, or "*skills produced at one stage raise the productivity of investment at subsequent stages"* (Heckman and Cunha 2007). We refer to such synergies between *investments* as dynamic complementary *in human capital investments*.[9] If Head Start increases skills and therefore improves school readiness, Head Start may facilitate better learning in the K12 system. If so, insofar as increased spending improves school quality, spending on Head Start and public K12 schools would exhibit dynamic complementarity. This is what we seek to test in this paper.

Note that complementarity is not a given. Compensatory interventions or interventions designed to bring *all* children up to some basic standard of skill, may, *by design*, have smaller benefits for more highly-skilled children.[10] Also, note that human capital investments may exhibit dynamic complementarity for reasons other than dynamic complementary in skill development.[11] To apply these insights to our setting, we outline two ways through which Head Start and K12 spending may interact. The first is a direct channel that operates through dynamic complementary in skill development. The second channel is indirect and may operate through spillovers to other students, and adjustments by actors in the schooling system (Malamud et al., 2017).

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[8] This is what is identified by researchers who examine the effect of interventions for individuals with differing incoming levels of skills (e.g. Garcia and Gallegos, 2017; Lubotsky and Kaestner, 2016; Aizer and Cunha, 2012).

[9] Following the notation from Heckman (2007), the technology of skills production is dynamic. Skills acquired when a child is *t* years old is [a] below

[a] $$\theta_{t+1} = f_t(h_t, \theta_t, I_t)$$

where *t*=1,2,...T, θ_t is a vector of skills at time *t*, parental capabilities are connoted by h_t, and investments during time *t* are connoted by I_t. Investments in time t (I_t) are construed broadly to include parental investments, schooling inputs (i.e., peers, teachers, etc.), and neighborhood and community inputs. For analytical convenience, f_t is assumed to be strictly increasing in I_t. Dynamic complementarity in human capital investments arises when the stocks of capabilities acquired by period *t-1* (θ_t) make investments in period t (I_t) more productive, i.e.,

[b] $$(\partial^2\theta_{t+1})/(\partial\theta_t\partial I_t) > 0.$$

Consider that $\theta_t = f_{t-1}(h_{t-1}, \theta_{t-1}, I_{t-1})$. Because $\partial f_t/\partial I_t > 0$, if [b] holds, then [c] below must also hold.

[c] $$(\partial^2\theta_{t+1})/(\partial I_{t-1}\partial I_t) > 0.$$

In words, dynamic complementarity in skill development implies that there is dynamic complementarity in human capital investments. However, if early investments increase the efficacy of later investments through mechanisms other than increasing skills, the converse does may not hold. We show that this is not the case in our setting.

[10] Such pattern may explain Rossin-Slater and Wust (2017) who find that preschool is less effective for children who received home health visits as infants.

[11] Formally, if equation [b] holds it implies that [c] will also hold. However, the converse is not always true.

The direct channel operates through what we call "alignment." The alignment mechanism is predicated on the idea that the sequence of when skills are taught matters (Knudsen et al., 2006; Newport, 1990; Pinker, 1994) and the fact that K12 systems target students with a specific incoming skill level. Students above the target skill level may benefit less from the K12 system (the K12 system may spend valuable instructional time teaching skills they have already mastered), and students below this target incoming skill level may benefit less from the K12 system (the instruction may require skills they do not possess). Given that poor children, on average, are less likely to be school-ready at kindergarten entry (Fryer and Levitt, 2004; Magnuson and Waldfogel, 2005), Head Start spending, by increasing their skills, may bring them closer to the target such that they benefit more from subsequent investments experienced in the K12 education system. Furthermore, access to pediatric care (provided to Head Start participants) may promote this skill development (Levine and Schanzenbach, 2009; Cohodes, Grossman, Kleiner, Lovenheim, 2015).

Through alignment, Head Start spending increases may not improve outcomes to the same degree in all contexts. In fact, in poorly-funded schools that may align instruction to a low-target skill level, Head Start participation could reduce alignment with the target level by increasing students' incoming skills above the target. In such a scenario, relative to their peers who did not attend preschool, any advantage in skill created by Head Start will diminish over time as children who attended Head Start receive redundant instruction, and their peers who lack access to preschool catch up in elementary school grades. That is, there may be fadeout and lower long-run Head Start effects for program participants who attend poorly-funded K12 schools. In sum, through this channel, on average, the effects of Head Start spending on poor children may be larger in well-funded K12 districts and could be negligible in poorly-funded public school districts.

The first indirect channel is through "spillovers." Research has found that higher shares of low-performing peers or disruptive peers may have deleterious impacts on students (see Sacerdote, 2014). By increasing the human capital of poor children, increases in Head Start spending may affect the subsequent peer composition of the K12 classrooms for *all* children in the county. This could make it easier for the K12 school system to translate resources into better outcomes.[12] The second indirect channel is through "adjustments." The first is an "alignment adjustment." If teachers in the K12 system alter the alignment of their instruction toward an incoming higher-

[12] Neidell and Waldfogel (2010) provide evidence of this channel by documenting spillover effects from preschool between Head Start and non-Head Start children on math and reading achievement.

ability student (in light of a lower share of low-achieving students due to Head Start), the quality of K12 instruction could be affected for all students. Importantly, because these adjustments can move some students closer to the target and others farther from it, the "alignment adjustment" effect could be positive or negative for any given student.[13] There could also be "budget allocation adjustments" that can affect students in different classrooms. For example, lower shares of students requiring remediation or special services (due to Head Start) may allow schools to allocate resources to other productive inputs, which may affect all students in the school.[14]

This is not an exhaustive list of all possible adjustment effects. However, the key takeaways are that (a) policy complementarities reflect both the direct effect due to the technology of skill formation and also some spillover and adjustment effects, and (b) adjustment and spillover effects could lead the interaction between the two interventions to be either positive or negative such that the overall interaction effect is ambiguous in sign. Through the direct channel, both the potential direct effects of Head Start spending and dynamic complementarity will be experienced only by Head Start participants. Through the spillovers channel (without adjustments) all children in the classroom with Head Start participants will have larger K12 spending effects when there is greater Head Start spending. However, through the adjustment channels, *all* children in K12 schools with former Head Start participants may be affected (even if not in the same classrooms). We present empirical evidence to shed light on what mechanisms are most likely at play in our setting.

III. Background and Overview of Head Start and School Finance Reforms

III.A. *Background on Head Start*

Head Start was established in 1964 as part of Lyndon B. Johnson's "War on Poverty," and is a national, federally-funded, early-childhood program with the aim of improving the human capital of poor children. The Head Start curriculum aims to enhance literacy, numeracy, reasoning,

[13] For example, if the K12 system adjusted the target skill level higher in response to Head Start, low skilled children who did not participate in Head Start may now be even more poorly aligned with the new target and may benefit less from the K12 system than if Head Start were not available to their peers.

[14] One of the program components of Head Start is teaching parenting skills; thus, another possible indirect channel is changes in parental quality. However, existing evidence suggests the primary mechanisms operate through the programmatic elements targeted at participating children: (a) enhancing literacy, numeracy, reasoning and problem-solving, and decision-making skills; (b) access to pediatric care; and (c) improved nutrition (Zigler, 2010; Currie and Neidell, 2007). Moreover, we present a test of this using within-family variation. In such tests, we find no indication that siblings of those exposed to higher levels of Head Start spending have improved outcomes (Appendix I). This runs counter to the parental quality mechanism.

problem-solving, and decision-making skills. Head Start includes educational efforts for both parents and children to enhance nutrition in the home and provides nutritious meals for the children. Participating children receive development screenings, and programs connect families with medical, dental, and mental health services.[15,16] Head Start also provides first-time parents with parenting strategies (Zigler et al., 2011). Head Start currently operates more than 19,200 centers and serves more than 900,000 children.[17] Current Head Start expenditures average about $8,700 per enrolled child (in 2015 dollars). This level of per-pupil spending is much lower than those at model preschool programs such as Perry Preschool or Abecedarian (Blau and Currie, 2006).[18] However, per-pupil Head Start spending levels are on the same order of magnitude as the average public K12 per-pupil spending, which is currently about $11,000 (in 2015 dollars).[19]

Because we seek to explore the effects of Head Start spending on longer-run adult outcomes (among those who are adults today), we study the effects of Head Start at the inception of the program (1965 through 1980). Head Start was initially launched as an eight-week, summer-only program in 1965 and then became a primarily part-day, nine-month program in 1966. Head Start is mainly funded federally.[20] To open a new Head Start center, local organizations (typically non-profit organizations, for-profit agencies, or school systems) apply to the federal government for grant funds. Grantees provide at least 20% of the funding. After approval, Head Start grants are awarded directly to applying organizations subject to three-year grant cycles.[21] Each grantee must comply with student-to-teacher ratio guidelines and other standards outlined in the Head Start Act. During the first 15 years of the program, the average student-to-teacher ratio in a Head Start classroom was roughly 17:1 (Zigler, 2010).[22] During this early era of the program, the majority of

[15] http://www.acf.hhs.gov/programs/ohs/about/head-start
[16] An OEO report of 1967 documents Head Start accomplishments in the first two years on child health that include 98,000 eye defects treated; 900,000 cases of dental problems addressed (5 cavities per child); 740,000 without polio vaccinations received vaccines; and 1,000,000 were given measles vaccinations.
[17] See Appendix Figure A1 for the national, annual enrollment in Head Start between 1965 and 2013.
[18] Head Start spending per enrollee is about 60% of spending levels observed in model preschool programs.
[19] There is considerable variability around this national average in individual states. States spending the least per-pupil included Utah ($6,555), Idaho ($6,791), Arizona ($7,208), Oklahoma ($7,672) and Mississippi ($8,130).
[20] Head Start funds were allocated to states proportionately based upon each state's relative number of children living in families with income below the poverty line and the relative number of public assistance recipients in each state. Head Start in collaboration with the Medicaid Early Pediatric Screening, Diagnosis, & Treatment Program (EPSDT) provided comprehensive prevention and treatment services to preschool children.
[21] As documented in Ludwig and Miller (2007), the poorest 300 counties initially received grant assistance to apply for funding at the program's inception.
[22] This student-to-teacher ratio is higher than the prevailing student-to teacher ratios in the model preschool programs of the Perry Preschool (5.7 children per teacher), the Abecedarian Project (6 children per teacher), and Chicago Child Parent Center and Expansion Program (8-12 children per teacher) (Cunha, Heckman, Lochner and Masterov, 2006;

Head Start children were enrolled in part-day centers (as opposed to full-day programs, which are 6 or more hours per day such as Abecedarian), and often part-year (GAO report, 1981).[23]

Head Start was targeted at pre-school age children (3 through 5) and this target changed somewhat over time. While most Head Start enrollees were four years old at enrollment, this was not strictly the case. The early Summer programs enrolled older preschool children, while full-year programs were primarily for younger preschool children three years of age or older up to the age when they are eligible for kindergarten or first grade (OCD, 1970). At each center, at least 90% of enrollees had to be from families whose income was below the federal poverty line, and at least 10% of children had to have a disability.[24] The top panel of Figure 1 plots the raw national Head Start enrollments between 1960 and 1994. Between 1965 and 1970, most of the enrollment in Head Start was in summer-only programs. However, from 1972 and after that, most enrollment was in full-year Head Start. As such, the early rollout of Head Start represented *both* increases in Head Start participation *and* enhancements in the Head Start programs themselves. Another notable pattern is the decline in Head Start enrollments between 1969 and 1972. During this period, full-year Head Start programs enrollment was increasing at the same time that summer-only program enrollment was declining (somewhat more rapidly). To relate these enrollments to participation rates at the individual child level, for each kindergarten entry cohort we computed the cumulative likelihood across all age-eligible years that an income-eligible child would enroll in Head Start.[25] To avoid double-counting individuals who enrolled in both the summer program

Fuerst and Fuerst, 1993; Carneiro and Ginja, 2014). Note also the much smaller scale of these model programs as the Perry and Abecedarian programs each served just over 100 disadvantaged children.

[23] We are unable to identify which of these options a local Head Start center offered children who attended (part-day vs full-day; part-year vs. full-year). Summer-only programs were phased out by 1981 (Gibbs et al., 2011).

[24] Children who are 4 years old and whose family income is below the federal poverty guidelines (or is on public assistance programs AFDC or SSI) are eligible for the program. Beginning in 1972 (as part of the Economic Opportunity Act Amendment) at least 10% of children per center must have a disability (irrespective of the family income of these children). In 1969, a provision was added allowing children from families above the poverty level to receive Head Start services for a fee. A fee schedule for non-poor participants in Head Start was required; fees were prohibited for families below the poverty line. The eligibility criteria was mostly unchanged during the period of the program we analyze (Source: 45 CFR (Code Federal Regulations), Parts 1301 to 1311, Early Childhood Learning and Knowledge Center: http://eclkc.ohs.acf.hhs.gov/hslc; www.eric.ed.gov; Zigler and Valentine, 1979).

[25] The ratio of enrolled students to the income-eligible age-eligible population in a given year *is not* the same as a specific cohort's participation rate by kindergarten entry. To illustrate this point, suppose for simplicity that Head Start had only the summer program. For example, the annual enrollment rate in summer programs was about 22 percent between 1965 and 1967. The cohort of income-eligible children entering kindergarten in 1965 could only have enrolled at age 5 and would have a 22 percent participation rate. However, the cohort of income-eligible children that entered kindergarten in 1966 could have enrolled at age 4 or 5, so (assuming that participants enroll for one year and not multiple years) their cohort's participation rate by kindergarten entry would be 44 percent (i.e., the sum of the likelihood of participation during ages 4 or 5: 22 + 22). All subsequent cohorts could have enrolled at ages 3,4, or 5,

and the full-year programs, we assume that 40 percent of full-year enrollees were previously in a summer program. The lower panel of Figure 1 depicts our estimated likelihood of Head Start enrollment (across all age-eligible years) by kindergarten entry cohort. The likelihood of Head Start enrollment among poor children reached 86% for income-eligible cohorts entering kindergarten in 1969, fell in the early 1970s, and stabilized around 63% by 1990. This is similar to the Garces et al (2002) estimate of two-thirds. Our participation rates of between 63 and 85 percent are important to keep in mind as we interpret the magnitudes of our intent-to-treat estimates (in Section VI). Figure 1 (top panel) also plots the share of 3- and 4-year-olds enrolled in full-time daycare over time (as reported in the Current Population Survey). This figure highlights that Head Start rollout coincides with a period in which most children were not in formal, full-time pre-school, and also coincides with a general increase in the proportion of children ages 3 to 4 enrolled in full-time pre-school. In the context of the estimated effects of Head Start during this rollout period, the counterfactual option in the early years is primarily home care, as opposed to some other full-time pre-K program (as might be the case with present-day public pre-K expansions).

We use Head Start spending as a way to measure both the presence of the program and also the quality, size, and extent of the program. While Head Start spending per enrollee may seem like a natural proxy for quality, such a measure fails to capture changes in spending that work through expansions in access.[26] Because the target population for Head Start is poor pre-schoolers and most enrolles are 4 years old, our measure of Head Start spending is federal Head Start spending per *poor* four-year-old in the county. Between 1965 and 1980, the average county with a Head Start center spent about $4,000 per poor child and about $5,300 per enrollee (in year 2000 dollars).

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so that post-1966 cohorts' participation rate by kindergarten entry (across all age-eligible years) is the running total annual summer enrollment ratio for the three years preceding kindergarten entry. Similarly, assuming that participants enroll for one year and not multiple years, a specific cohort's full-year participation rate by kindergarten entry is the running total annual full-year enrollment ratio for the two years preceding kindergarten entry.

[26] The expansion of Head Start involved both increases in the number of enrolled children and increases in spending per enrolled child. Head Start spending per enrollee increases do not capture increases in the total number of children affected by Head Start, so that spending per poor four-year-old in the county is a more appropriate measure. To illustrate this point, we collected data on Head Start spending per enrollee and Head Start spending per poor 4-year old at the state level between 2003 and 2014 (years for which both sets of data are available). Using within-state changes in spending over time, a 10% increase in spending per poor four-year-old is associated with only a 0.243% increase in spending per enrollee, on average. However, it is also associated with a 1.2% increase in the number of Head Start participants, and a 6.5 percentage-point increase in the percentage of poor four-year-olds enrolled (Appendix Table B1). While spending per poor 4-year-old is sensitive to both increases in funding per enrollee and increases in total enrollment, increases in spending per enrollee are unrelated to increases in enrollment. These patterns make clear that for studying the rollout of Head Start, spending per enrollee would be an inappropriate measure, and that spending per poor 4-year old is a much better measure.

There is considerable variation in timing of the establishment of Head Start centers. However, in most counties, the *first* Head Start center was established between 1965 and 1970.[27] The geographic variation in the timing of the rollout of Head Start is central to our empirical strategy to isolate exogenous variation in Head Start spending across birth cohorts within a county.

III.B. *Background on School Finance Reforms*

The other major human capital interventions we study are the increases in public K12 school spending caused by court-ordered school finance reforms (SFRs). In most states, before the 1970s, local property taxes accounted for most resources spent on K12 schooling (Howell and Miller, 1997). Because the local property tax base is typically higher in areas with higher home values, and there are high levels of residential segregation by socioeconomic status, heavy reliance on local financing contributed to affluent districts' ability to spend more per student. In response to large within-state differences in per-pupil spending across wealthy/high-income and poor districts, state supreme courts overturned school finance systems in 28 states between 1971 and 2010. Because of these court decisions, many states implemented legislative reforms that led to important changes in public education funding.[28] Most of these court-ordered SFRs changed the parameters of spending formulas to reduce inequality in school spending and weaken the relationship between per-pupil school spending and the wealth and income level of the district.

As pointed out in Hoxby (2001), the effect of a SFR on school spending depends on (a) the type of school funding formula introduced by the reform and, (b) how the funding formula introduced interacts with the specific characteristics of a district. To capture some of this complexity, we follow Jackson, Johnson, and Persico (2016) and categorize reforms into four types. Foundation plans guarantee a base level of per-pupil spending and are designed to increase per-pupil spending for the lowest-spending districts. Spending-limit plans prohibit per-pupil

[27] Figure A2 presents each county in the United States color-coded by the year of its first Head Start center.

[28] The first of these successful cases is the California case, *Serrano v. Priest*, decided in 1971. Challenges to state school finance systems were argued on either equity or adequacy grounds. The early challenges (1971- mid 1980s) were won on equity grounds. For "equity cases," local financing was found to violate the responsibility of the state to provide a quality education to all children. "Equity cases" sought to weaken the relationship between the quality of educational services and the fiscal capacity of the district. The more recent challenges (late 1980s onwards) were mounted on adequacy grounds. "Adequacy cases" rely on the fact that most states have a constitutional provision requiring the state to provide some minimum "adequate" level of quality schools for all children (Lindseth, 2004). They were argued on the grounds that low per-pupil spending levels in certain districts meant that the state had failed to meet its obligation. Between 1970 and 1990, 10 and 4 states had court-ordered reforms argued on equity and adequacy grounds, respectively.

spending levels above some predetermined amount. Such plans tend to reduce spending for high spending districts and may reduce long-run spending for all districts. Reward-for-effort plans match locally-raised funds for education with additional state funds (often with higher match rates for lower-income areas). Such plans tend to increase spending for all districts with larger increases in low-income districts. Equalization plans typically tax all districts and redistribute funds to lower-wealth and lower-income districts. These reform/formula types are not mutually exclusive.

In existing work Card and Payne (2002), Jackson, Johnson, and Persico (2016) and Hoxby (2000) find that court-ordered SFRs that lead to the implementation of different funding formulas had different effects on district spending by pre-reform income and spending levels.[29] In particular, Jackson, Johnson and Persico (2016) find that reforms that lead to "reward-for-effort" formulas tended to increase per-pupil K12 spending in all districts; spending limits led to pronounced spending reductions in high-spending districts; foundation plans led to the largest spending increases in low-income districts; and equalization plans were more equalizing by pre-reform spending levels than by pre-reform income levels. These already established systematic patterns allow us to predict how much K12 school spending increases in each district as a function of the reform type introduced (by the state) and the pre-reform characteristics of the district. Because these relationships are unrelated to decisions made by individual districts or demographic shifts that may affect public school spending levels, we can use this prediction to isolate the causal relationship between reform-induced K12 spending increases and students' longer-run outcomes.

IV. DATA

We compiled data on annual Head Start spending at the county level, and public K12 school spending at the school district level. The Head Start spending data come from the National Archives Record Administration, Inter-university Consortium for Political and Social Research,

[29] To illustrate how the introduction of different formula types affected districts by pre-reform income and spending levels, we replicate the analisis in Jackson Johnson and Persico (2016) in Appendix C. Appendix Figures C1 and C2 present event-study plots of the natural log of per-pupil spending at the district level (after removing both district and year fixed effects). Year 0 is the first year of the first court order in the state, year "-5" is five years before the first court order, and year "5" is five years after the initial court order. For each court order, we link all formula changes that occurred within three years to that court-ordered SFR. Figure C1 shows the evolution of per-pupil spending for districts in the bottom and top quartiles of per-pupil spending in 1972 (the year preceding the first court-ordered SFR) after court orders that led to the implementation of different kinds of funding formula plans. Figure C2 presents similar plots for districts in the top and bottom quartiles of the state income distribution in 1963. Figures C1 and C2 show that court-ordered SFRs that lead to the implementation of different funding formulas had different effects on districts by pre-reform income and spending levels.

and Surveillance, Epidemiology, and End Results population data. These are combined to form a county-level panel of Head Start spending per poor 4-year-old in the county between 1965 and 1980.[30] Public K12 education funding data come from several sources that are combined to form a panel of per-pupil spending for US school districts in 1967 and annually from 1970 through 2000 and are linked to a database of SFRs from Jackson, Johnson, and Persico (2016).[31] To avoid confounding nominal with real changes in spending, we convert both Head Start and K12 school spending across all years to 2000 dollars using the Consumer Price Index (CPI).

Our individual-level data on long-run outcomes come from the Panel Study of Income Dynamics (PSID, 1968-2015), and our analysis sample includes individuals born between 1950 and 1976 who were followed into adulthood. These PSID cohorts straddle both the rollout of Head Start programs across the country and the implementation of the early waves of court-ordered SFRs.[32] We include all information on PSID individuals between 1968 and 2015.[33] We linked persons in the PSID using their census blocks during childhood to school spending data, SFR data, and Head Start spending data.[34] We then match the earliest available childhood residential address to the school district boundaries that prevailed in 1969 to avoid complications arising from endogenously changing district boundaries over time. We detail the algorithm in Appendix D. Among potentially treated cohorts, almost all (97%) of the earliest address information we use is

[30] Further details on the PSID data are in Appendix D.

[31] The Census of Governments has been conducted every five years since 1972 and records school spending for every school district in the US. The Historical Database on Individual Government Finances (INDFIN) contains annual district finance data for a sub-sample of districts from 1967, and 1970 through 1991. After 1991, the Common Core data (CCD) School District Finance Survey (F-33) includes data on school spending for every school district in the US. Details on how these databases were compiled and the coverage of districts in these data are in Appendix E.

[32] The share of individuals potentially exposed to Head Start expenditures at age 4 increases significantly with birth year over the 1950-1976 birth cohorts analyzed in the PSID sample. Two-thirds of the sample grew up in a state that was subject to a court-mandated SFR between 1971 and 2000 (the first court order was in 1971).

[33] The PSID maintains high wave-to-wave response rates of 95-98%. Studies have concluded that the PSID sample of heads and wives remains representative of the national sample of adults (Fitzgerald, Gottschalk, Moffitt, 1998a,b; Becketti et al, 1988). Additionally, we perform a supplementary analysis of sample attrition in the PSID, and find no evidence of selective attrition among our study sample (Appendix Table D1). In particular, among original sample children, baseline 1968 family and county characteristics do not jointly significantly predict the likelihood of attrition or the likelihood of being observed as an adult.

[34] The PSID began interviewing a national probability sample of families in 1968. These families were re-interviewed each year through 1997, when interviewing became biennial. All persons in PSID families in 1968 have the PSID "gene," which means that they are followed in subsequent waves. When children with the "gene" become adults and leave their parents' homes, they become their own PSID "family unit" and are interviewed in each wave. The original geographic cluster design of the PSID enables comparisons in adulthood of childhood neighbors who have been followed over the life course. Moreover, the genealogical design implies that the PSID sample today includes numerous adult sibling groupings who have been members of PSID-interviewed families for more than four decades. We include both the Survey Research Center component and the Survey of Economic Opportunity component, commonly known as the "poverty sample," of the PSID sample.

from *before* the policies we study were enacted so that bias due to residential sorting in response to the policies is negligible. We verify this empirically.[35] We also merge in county-level characteristics from the 1960 Census, and information on the timing of other key policy changes during childhood (e.g., school desegregation, hospital desegregation, Title I, rollout of other "War on Poverty" initiatives and expansion of safety net programs—described in Section V) from multiple data sources.[36]

We define low-income children as those whose average parental income (between ages 12 and 17) fell in the bottom quartile.[37] Among cohorts born between 1963-1976 for whom parental income at age four is observed, roughly 80% of those whom we classify as low-income were below the federal poverty line at age four, and 93% of those who were below the poverty threshold at age four are classified as low-income by our definition. The analytic sample includes 15,232 individuals from 4,990 childhood families, 1,427 school districts, 1,120 counties, across all 50 states. From this point forward, we refer to children who are low income as "poor" children, and those not from low-income families (as defined above) as "non-poor" children. We examine a broad range of adult outcomes. These include 1) educational outcomes—whether graduated from high school, years of completed education; 2) labor market and economic status outcomes (in real 2000 dollars)— log wages, family income, annual incidence of poverty in adulthood[38] (ages 20-50); and 3) criminal involvement and incarceration outcomes—whether ever incarcerated (jail or prison) and the annual incidence of incarceration in adulthood. Table 1 contains descriptive statistics for various childhood measures and adult outcomes in our analytic sample.

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[35] As a check on endogenous mobility (in Appendix H), we re-estimated all models limiting the analysis sample to those who lived at their (earliest) childhood residence prior to the enactment of Head Start programs in their respective county or SFR in their district. As expected, we find similar results to those with the full sample, so that endogenous residential mobility is not a source of bias in our analysis.

[36] The data we use include measures from 1968-1988 Office of Civil Rights (OCR) data; 1960, 1970, 1980, and 1990 Census data; 1962-1999 Census of Governments (COG) data; Common Core data (CCD) compiled by the National Center for Education Statistics; Regional Economic Information System (REIS) data; county-level Title I/ESEA spending (NARA); the comprehensive case inventory of court litigation regarding school desegregation over the 1955-1990 period (American Communities Project), and major plan implementation dates in large districts (compiled by Welch/Light); and American Hospital Association's Annual Survey of Hospitals (1946-1990) and the Centers for Medicare Provider of Service data files (dating back to 1960s) to identify the precise date in which a Medicare-certified hospital was established in each county of the US (an accurate marker for hospital desegregation compliance).

[37] Because the earliest year in which parental income is available is 1967 due to when the PSID data collection started, we cannot observe family income at age four for those born before 1963. However, we can observe average family income during adolescence (ages 12 through 17) for all individuals in our analytic sample, which serves as a good permanent income measure. We use this to form our group of likely Head Start eligible individuals.

[38] Based on the family income-to-needs ratio and federal poverty thresholds by family structure and household size.

V. EMPIRICAL STRATEGY

V.A. *Identifying the effects of Head Start Spending*

Our measure of Head Start spending is total federal Head Start spending in a county per poor four-year-old (in 2000 CPI-adjusted real dollars). Our research design takes advantage of the staggered introduction across geographic areas of Head Start programs and the resulting spending increases during the program's rollout. Before the rollout of Head Start to an area, there is no spending on Head Start. However, after the introduction of Head Start in a county, spending levels typically increase for several successive years. Figure 2 shows an event-study plot of Head Start spending per poor-four-year-old before and after rollout in areas that had high and low Head Start spending in 1980 (the end of the sample period under study). Note that year "zero" is the year of the establishment of the first Head Start center in a county.

In the high-spending counties, once the first center is established, spending per poor four-year-old increases rapidly. As expected, the increase is much larger in the high-spending counties (from zero to about $5,000 per poor 4-year old) than the low-spending counties. However, spending is highest during the first year and then falls after that. This initial increase and subsequent fall is an artifact of the large national enrollment in summer-only programs that were phased out in the following years (Figure 1). The initial increase in Head Start spending due to the summer-only programs is also evident in counties with low Head Start spending in 1980. In essence, almost all counties experienced a transitory increase in Head Start spending, due to the ubiquitous introduction of summer-only programs that falls over time. However, high-spending counties expanded enrollment (and spending) in full-year programs that was sustained over time, while the low-spending counties did not increase spending on full-year programs and reverted to near zero Head Start spending within four years.

The left panel of Figure 2 reveals that, among children born within a few years of each other in the same county, some were four years old when there was no Head Start spending in their county, and others were four years old at the end of the phase-in stage when spending levels were high. If higher levels of Head Start spending improve outcomes, one should observe that (a) the post-rollout cohorts should have better outcomes than the pre-rollout cohorts, and (b) improvements between pre- and post-rollout cohorts should be larger in counties with larger sustained increases in Head Start spending. Figure 2 reveals exactly this pattern for years of educational attainment (measured in adulthood) among poor children. The event study shows that areas with small increases (middle panel) and those with large increases in Head Start spending (right panel) were on the same trajectory among cohorts who were older than four years old when the first Head Start center was established (i.e., years -5 through year 0). However, the post-rollout cohorts have much better outcomes in high Head Start spending counties than in low-spending counties. This provides a graphical representation of our empirical strategy.

Our preferred difference-in-difference (DiD) strategy uses all this variation in timing and dosage. That is, we compare the differences in long-run outcomes across birth cohorts from the same childhood county that experienced larger increases in Head Start spending at age 4, to the differences in outcomes across the same birth cohorts within other childhood counties that experienced small (or no) increases in Head Start spending at age 4. These DiD type comparisons are implemented in a regression framework by estimating [1] by Ordinary Least Squares (OLS).

[1] $$Y_{icb} = \beta^{DiD} \cdot HS_{cb}^{age\ 4} + \gamma \cdot C_{icb} + \theta_c + \tau_b + \varepsilon_{icb}.$$

In [1], Y_{icb} is the outcome of individual i, from childhood county c, in birth cohort b. The variable of interest ($HS_{cb}^{age\ 4}$) is Head Start spending per poor four-year-old in county c (in year 2000 dollars), when birth cohort b was age 4. To rely only on within-county variation in Head Start spending across cohorts, [1] includes childhood county fixed effects (θ_c); and to account for cohort effects we include birth-year fixed effects (τ_b). We also include an extensive set of childhood-family and individual characteristics, and county-level coincident policy changes as control variables (C_{icb}) that we detail in Section V.C. The idiosyncratic error term is ε_{icb}.

There are two identifying assumptions. First, counties that experienced increased Head Start spending over time (where most of the variation occurs at rollout) were not already on a trajectory of improving or deteriorating outcomes over time. Second, counties that saw larger or smaller increases in Head Start spending did not also undergo other unobserved changes that would also affect outcomes. Figure 2 suggests that the first condition is satisfied. To show that the second assumption is likely satisfied, we examine whether areas that had higher levels of Head Start spending may have also introduced policies and programs that may have improved child outcomes.

To test this, we estimated the marginal effect of Head Start spending levels that prevailed when individuals were different ages, conditional on the level of Head Start spending when they were four (shown in the left panel of Figure 3 and Table H8). Higher levels of Head Start spending at age four are associated with improved adult outcomes, while the spending levels at ineligible ages (age 1 through 3 or 5 through 10) are not.[39] If areas with high levels of Head Start spending also implemented other policies that would promote better outcomes, then Head Start spending at age 5, 3, or 7 would systematically be associated with better outcomes. This is clearly not the case. As another check on this variation, we regress each person's years of education and wage on our rich set of individual, family, and neighborhood characteristics and other social safety net programs. The fitted values from these regressions are effect-size weighted indices of childhood family and community socioeconomic factors (Appendix Table H2). Conditional on school-district and birth-year fixed effects only, there is no association between Head Start spending and these predicted outcomes. Taken together, this is compelling evidence that our variation is valid. However, to assuage any lingering worries, we also implement a second strategy.

Because local areas with high versus low levels of Head Start spending may differ in ways that could confound our comparisons, our second identification strategy relies only on the variation in the availability of any local Head Start center at age four. To do this, we instrument for Head Start spending per poor four-year-old in county c ($HS_{cb}^{age\ 4}$), with an indicator variable of whether a Head Start center existed in one's childhood county at four years old ($Exposed\_HS_{cb}^{age\ 4}$). Formally, we estimate the following system of equations by two-stage-least-squares (2SLS)

[2] $$\widehat{HS}_{cb}^{age\ 4} = \pi_{hs,1} \cdot Exposed\_HS_{cb}^{age\ 4} + \pi_{hs,1} \cdot C_{icb} + \theta_c + \tau_b.$$

[3] $$Y_{icb} = \beta^{2SLS} \cdot \widehat{HS}_{cb}^{age\ 4} + \gamma \cdot C_{icb} + \theta_c + \tau_b + \varepsilon_{icb}.$$

The identifying assumptions in this 2SLS model are weaker than those for the DiD model. This model is identified if (a) counties that establish a Head Start center were not already on a trajectory of improving or deteriorating outcomes over time, and (b) counties that established a Head Start center did not also undergo other unobserved changes that would also affect outcomes. The right panel of Figure 3 (and Table H9) shows the effect of rollout (as opposed to spending) by age on years of education for poor children.[40] Reassuringly, there is an effect of having access to Head Start at age 4 but no effect of having access to Head Start for any other age (conditional on access at age 4). Figures 2 and 3 suggest that the identifying conditions are satisfied. Furthermore, in Section VI.B we present further evidence to support a causal interpretation of our estimates.

[39] Appendix Figure H1is an analogous figure for adult wages.
[40] Appendix Figure H1is an analogous figure for adult wages.

V.B. *Identifying the effects of K12 School Spending*

Our measure of K12 public school spending during childhood, ppe_{idb}^{5-17}, is the *natural log* of average public K12 school spending per-pupil (in real 2000 dollars) during school-age years (ages 5 through 17) in an individual's childhood school district.[41] We refer to this as K12 spending. Individuals who turned 17 years-old during the year of the passage of a court-ordered SFR in their state should have completed secondary school by the time reforms were enacted. Such cohorts (and older cohorts) are "SFR *unexposed*". Individuals who turned 16 years old or younger during the year of the passage of the first court-ordered SFR in their state would likely have attended primary or secondary school when reforms were implemented. Such cohorts are "SFR *exposed.*" One can estimate the SFR exposure effect on outcomes for individuals from a particular district by comparing the change in outcomes between SFR-exposed and SFR-unexposed birth cohorts from that district. Some districts experienced larger spending increases due to a court-ordered SFR than others. We exploit this fact and test for a causal effect of per-pupil spending during childhood by testing whether the difference in outcomes between SFR-exposed and SFR-unexposed cohorts from the same school district (i.e., the SFR exposure effect) tends to be larger for those districts that experienced larger reform-induced K12 spending increases (i.e., a SFR dose-response effect). Our identifying assumption is that the spending changes caused by the reforms within districts were unrelated to other district-level changes that could have affected adult outcomes directly.

Following Jackson, Johnson, and Persico (2016), we quantify the relationship between K12 spending and adult outcomes by using only the variation above in school spending associated with the passage of a court-mandated SFR. Specifically, using the PSID, we estimate equation [4] by 2SLS. All common variables are defined as in [1].

$$[4] \qquad Y_{idcb} = \beta \cdot \widehat{ppe}_{idb}^{5-17} + \gamma \cdot C_{idcb} + \theta_d + \tau_b + \varepsilon_{idcb}.$$

To rely only on variation across birth cohorts within districts, we include school district fixed effects (θ_d); to account for time trends and cohort effects, we include birth-year fixed effects (τ_b); and to account for life cycle effects, we include flexible controls for age (cubic). Our endogenous regressor is ppe_{idb}^{5-17}, and $\widehat{ppe}_{idb}^{5-17}$ are fitted values from a first stage.

The excluded instruments in the first stage are measures of exposure to a SFR interacted

[41] The average level of district per-pupil spending across all school-age years provides a summary measure of the level of financial resources available in the individual's childhood school district during all their school-going years (ages 5 through 17 corresponding to expected grades K12). We use the natural log of this average measure to capture the fact that school spending likely exhibits diminishing marginal product.

with measures of dosage (to account for the fact that some districts have larger reform-induced spending increases than others). Our exposure measure, $SFRExp_{idb}$, is the number of years individual *i* in birth cohort *c* from childhood district *d* is expected to have been in school after the passage of the first court-ordered SFR in their home state. This exposure measure varies at the state birth-cohort level and goes from 0 (for those who were age 17 or older the year of the state's first court ordered SFR) to 12 (for those who were ages 5 and younger the year of the state's court ordered SFR). To capture variation in dosage conditional on exposure, in the first stage we also include the two-way interaction between $SFRExp_{idb}$ and a district-level predictor of the spending change caused by the state court-ordered SFR in that district ($\widehat{dose}_d$). More formally, the first stage regression is as in [5] below

[5] $\widehat{ppe}_{idb}^{5-17} = \pi_1\left(SFRExp_{idb} \times \widehat{dose}_d\right) + \pi_2(SFRExp_{idb}) + \gamma_1 \cdot C_{idcb} + \theta_{d1} + \tau_{b,1}.$

By construction, our measure $\widehat{dose}_d$ is unrelated to endogenous decisions made by districts after reforms. Specifically, $\widehat{dose}_d$, is a weighted average of reform type (implemented at the state level), pre-reform district income levels, pre-reform district spending levels and their interactions. To form $\widehat{dose}_d$, we use the full universe of school districts and regress per-pupil spending on (a) indicators for years of SFR exposure, interacted with reform type, interacted with pre-reform spending levels in 1972; and (b) indicators for years of SFR exposure, interacted with reform type, interacted with pre-reform median income levels in 1963, and region-specific year fixed effects. This regression models how per-pupil spending evolves in a district after the passage of a court-ordered SFR as a function of the funding formula introduced in the state, the school spending level in the district, and the economic characteristics of the district *prior* to reforms. We take the fitted values from this regression to obtain a predicted reform-induced spending change for each district (based on these exogenous variables). See Appendix F for details. Because $\widehat{dose}_d$ is estimated using *all* school districts while we estimate effects using districts represented in the PSID sample, our approach is a two-sample-2SLS.[42] To assuage any concerns regarding $\widehat{dose}_d$, Table H2 shows that the estimated point estimates obtained when using only variation in SFR exposure are almost identical (albeit less precise) than those that use both exposure and exposure times dosage.

To show that $\widehat{dose}_d$ captures meaningful variation in K12 spending caused by court-

[42] This approach was popularized by Angrist and Krueger (1992) and has been used in several other settings (e.g., Bjorklund and Jantti, 1997; Currie and Yelowitz, 2000; Dee and Evans, 2003).

mandated SFRs, Figure 4 shows the evolution of K12 spending among individuals in the PSID sample from districts with high predicted dosage (i.e. $\widehat{dose}_d$>0) and those with no predicted increases (i.e. $\widehat{dose}_d \leq 0$).[43] We create "event-time" indicator variables denoting the year an individual turned 17 minus the year of the first court order in the childhood state of individual i. The "-5" cohort are individuals who were 22 years old at the passage of a court-ordered SFR, the "-1" cohort was 18 years old at the passage of a court-ordered SFR, and the "5" cohort was 12 years old at the passage of a court-ordered SFR in their state. We then estimate a regression model predicting school-age K12 spending as a function of year fixed effects, district fixed effects, and the event-time indicators interacted with whether the district is predicted to have increased K12 spending due to the passage of a court-ordered SFR. Because the outcome is in logs, the values represent percent changes in average school-age spending relative to the cohort from the same district that was 17 the year of the first court-ordered SFR.

Consistent with the timing of court-ordered SFRs being exogenous to underlying trends in school spending, both districts with lower and higher predicted dosage were on similar pre-reform trajectories as similar districts in non-reform states. Consistent with $\widehat{dose}_d$ isolating real variation in dosage, cohorts that turned 5 years old during the year of the initial court order (cohort 12) in districts with $\widehat{dose}_d > 0$ experience a 19% increase in school-age per-pupil spending, while the same cohorts in districts with $\widehat{dose}_d \leq 0$ experience a 5% increase. The timing of the initial court-ordered SFR in the state interacted with the predicted reform-induced spending increase for the district (based on state reform type interacted with pre-reform district characteristics) isolates exogenous variation in school spending.

If our identification strategy is valid and K12 spending affects outcomes, outcome differences across exposed and unexposed cohorts should follow similar patterns to those of K12 spending. The right panel of Figure 4 shows this for years of educational attainment. Areas that had small (gray line) and large (black line) reform-induced increases in K12 spending were on similar trajectories among the unexposed cohorts (years -8 through year 0). However, the post-

[43] Roughly two-thirds of districts in reform states are predicted to experience spending increases in the first 8 years due to court-ordered SFRs. As one can see from Figure 4, because K12 spending tended to increase in states following court-ordered SFRs in general, there are small increases in K12 spending within 12 years post reform even in districts with predicted initial decreases. As such, we refer to all districts as having high- or low-predicted increases.

SFR cohorts (years 0 through 12) experienced much larger increases in years of education in the high-predicted K12 spending increase districts than in the low-predicted K12 spending increase districts. This figure depicts graphically the variation that undergirds our identification strategy.

The key identifying assumptions are that (a) districts that experienced spending increases due to a SFR were not on different trajectories before reforms, and (b) there were no coincident district-level policies or changes that confound our analysis. Figure 4 shows that this first condition is likely satisfied. We also test the second condition. If other coincident policies were driving the results (that were not targeted to school-age children), increased school spending might improve outcomes of those who were in the same district but not of school-going age. To test this, we instrument for the K12 spending levels that prevailed in an individual's childhood district when they were between the ages of 18 to 22 (i.e., non-school-going age), and we find no effect on adult outcomes (Appendix Table H1). Also, we regress each person's years of education and adult wages on our rich set of individual, childhood family and neighborhood characteristics, and other social safety net programs. The fitted values from these regressions are effect-size weighted indices of childhood family and community socioeconomic factors (Appendix Table H3). Conditional on school-district and birth year fixed effects only, there is no association between instrumented K12 spending and these predicted outcomes – further evidence that our identifying variation is valid. This suggests that the effects are not driven by confounding policies; rather, any effects likely emerge through the hypothesized channels. While these tests are not dispositive, they support a causal interpretation of the main findings. To assuage lingering concerns, we present additional tests in Section VI.

V.C. *Testing for Dynamic Complementarity*

To test whether the marginal effect of increased Head Start spending varies by the level of K12 spending and vice versa, we estimate the effects of public pre-K and K12 spending on adult outcomes with the inclusion of the interaction between Head Start spending at age 4 ($HS_{icb}^{age\ 4}$) and the natural log of public K12 spending between the ages of 5 and 17 (ppe_{idb}^{5-17}). All models are estimated separately for poor and non-poor children, as we do not expect to find significant effects of Head Start spending nor evidence of dynamic complementarity among non-poor children (at least through direct channels as they are not income-eligible for Head Start). We define $\text{INT}_{idb} = \left(HS_{icb}^{age\ 4} \times ppe_{idb}^{5-17}\right)$. We estimate two different models in our analysis.

The DiD-by-2SLS model. In the first model we use the within-county, across-cohort DiD variation in Head Start spending ($HS_{icb}^{age\,4}$). We instrument for the log of public K12 spending, ppe_{idb}^{5-17}, with ($SFRExp_{idb}$) and ($SFRExp_{idb} \times \widehat{dose}_d$). We instrument for, INT_{idb} with ($HS_{icb}^{age\,4} \times SFRExp_{idb} \times \widehat{dose}_d$) and ($HS_{icb}^{age\,4} \times SFRExp_{idb}$).[44] Because a school district may be a smaller unit of observation than a county, all models include district fixed effects (which subsumes county effects). The resulting model is [6], where $\widehat{ppe}_{idb}^{5-17}$ and $\widehat{INT}_{idb}$ are fitted values from first-stage regressions.[45]

[6] $$Y_{icb} = \beta_{HS} \cdot HS_{cb}^{age\,4} + \beta_{k12} \cdot \widehat{ppe}_{idb}^{5-17} + \beta_{int} \cdot (\widehat{INT}_{idb}) + \gamma \cdot C_{icb} + \theta_d + \tau_b + \varepsilon_{idb}.$$

The 2SLS-by-2SLS model. In the second model, we instrument for all spending variables. We instrument for Head Start spending ($HS_{icb}^{age\,4}$) using exposure to any Head Start center at age 4 ($Exposed\_HS_{cb}^{age\,4}$). We instrument for the log of public K12 spending, ppe_{idb}^{5-17}, with ($SFRExp_{idb}$) and ($SFRExp_{idb} \times \widehat{dose}_d$). We instrument for, INT_{idb} with ($Exposed\_HS_{cb}^{age\,4} \times SFRExp_{idb} \times \widehat{dose}_d$) and ($Exposed\_HS_{cb}^{age\,4} \times SFRExp_{idb}$). The resulting model is as in [7], where $\widehat{ppe}_{idb}^{5-17}$ and $\widehat{INT}_{idb}$ and $\widehat{HS}_{cb}^{age\,4}$ are *all* fitted values from first-stage regressions.[46]

[7] $$Y_{icb} = \beta_{HS} \cdot \widehat{HS}_{cb}^{age\,4} + \beta_{k12} \cdot \widehat{ppe}_{idb}^{5-17} + \beta_{int} \cdot (\widehat{INT}_{idb}) + \gamma \cdot C_{icb} + \theta_d + \tau_b + \varepsilon_{idb}.$$

The interaction effect between pre-K and K12 spending can be identified because (a) among counties that faced similar increases in Head Start spending (or had any Head Start center), some were located in school districts that experienced larger (or smaller) increases in K12 spending due to the passage of a court-ordered reform, and (b) among cohorts from districts that faced similar increases in K12 spending due to the passage of a court-ordered reform, some grew up in counties that had higher (or no) levels of Head Start spending when those cohorts were age 4. We

[44] While intuition would lead one to expect us to use all the two-way interactions between $HS_{icb}^{age\,4}$, $\widehat{dose}_d$, and $SFRExp_{idb}$, we do not use ($HS_{icb}^{age\,4} \times \widehat{dose}_d$) as an excluded instrument because $\widehat{dose}_d$ cannot affect outcomes unless it is interacted with SFR exposure. This would simply introduce noise and weaken the first stage. The variable $\widehat{dose}_d$ only ever enters our models when interacted with $SFRExp_{idb}$.

[45] Where $\hat{X}_1 = \widehat{ppe}_{idb}^{5-17}$ and $\hat{X}_2 = \widehat{INT}_{idb}$, and $w \in \{1,2\}$,
$\hat{X}_w = \pi_{w1}(SFRExp_{idb} \times dose_c) + \pi_{w2}(SFRExp_{idb}) + \pi_{w3}\left(SFRExp_{idb} \times \widehat{dose}_d\right) \cdot HS_{cb}^{age\,4} + \pi_{w4}(SFRExp_{idb}) \cdot HS_{cb}^{age\,4} + \gamma_w C_{idb} + \theta_{wd} + \tau_{wb}$.

[46] Where $\hat{X}_1 = \widehat{ppe}_{idb}^{5-17}$, $\hat{X}_2 = \widehat{INT}_{idb}$, $\hat{X}_3 = \widehat{HS}_{cb}^{age\,4}$ and $g \in \{1,2,3\}$,
$\hat{X}_g = \pi_{g1}(SFRExp_{idb} \times dose_c) + \pi_{g2}(SFRExp_{idb}) + \pi_{g3}\left(SFRExp_{idb} \times dose_c \times Exposed\_HS_{cb}^{age\,4}\right) + \pi_{g4}\left(SFRExp_{idb} \times Exposed\_HS_{cb}^{age\,4}\right) + \gamma_g C_{idb} + \theta_{gd} + \tau_{gb}$.

show in Section V.C.1. that the interaction can be identified in our data.

To further reduce the possibility of confounding effects, vector C_{idb} includes a variety of individual, childhood family, and childhood county controls. These include parental education and occupational status, parental income, mother's marital status at birth, birth weight, child health insurance coverage, gender; and the adult economic and incarceration outcomes include flexible controls for age (cubic). C_{idb} also includes birth-year fixed effects by region and race, birth-cohort linear trends interacted with various 1960 characteristics of the childhood county (poverty rate, percent black, average education, percent urban, and population size). Also, to avoid confounding our effects with that of other policies that overlap our study period, C_{idb} includes controls for childhood county-by-birthyear measures of school desegregation, hospital desegregation, community health centers, state funding for kindergarten, Title I school funding, imposition of tax limit policies, average childhood spending on food stamps, Aid to Families with Dependent Children, Medicaid, and unemployment insurance (Johnson, 2013; Chay, Guryan, & Mazumder, 2009; Hoynes, Schanzenbach, and Almond, 2016). Standard errors are clustered at the state level.

To provide visual evidence of complementarity, Figure 5 plots the estimated changes in years of educational attainment for cohorts before and after a court-ordered SFR for districts with high predicted spending increases (i.e., $\widehat{dose}_d$>0) and those with no predicted increases (i.e., $\widehat{dose}_d \leq 0$), separately for children with and without a local Head Start center at age 4. <u>Note that this is the variation used in the 2SLS-by-2SLS models.</u> The left panel shows that SFR-treated cohorts and SFR-untreated cohorts experienced similarly small changes in educational attainment in districts that had small increases in K12 spending and were not exposed to Head Start at age four (grey line). However, among cohorts that had county Head Start spending at age four, school-age years of exposure to SFRs led to increases in educational attainment relative to those who were not exposed to SFRs. This pattern is consistent with Head Start making even small increases in K12 spending effective for poor children. However, if the two policies are complementary, one should see similar patterns and greater increases in completed education for large increases in K12 spending. This is precisely what we document in the right panel of Figure 5. Here we see that in districts that experienced large increases in K12 spending after a SFR, exposed cohorts achieve more years of education than unexposed cohorts, and there is a dose-response relationship with the number of school-age years of exposure to larger reform-induced spending increases. Importantly, the relative increase in years of education is larger among those SFR-exposed cohorts that were from counties with a Head Start center at age four than among those SFR-exposed cohorts that did not have a Head Start center at age four. Furthermore, if one compares the effects across the two panels, one can see that the benefits of Head Start spending (the difference between the grey and black line in each panel) are larger among exposed cohorts that experience larger K12 spending increases. In sum, Figure 5 presents flexible semi-parametric evidence that Head Start and K12 school spending exhibit dynamic complementarity. The lack of any differential pre-trending in either panel illustrates that the parallel trends assumption likely holds, not just for each policy (Figures 2 through 4), but also for the *interaction* between the two policies.[47]

V.C.1. *Testing for Sufficient Variation to Identify the Interaction Effects*

Identification of our key parameter of interest is based on the interaction between the two policy instruments. For our inference to be valid, these policy instruments need to be largely independent of each other. This is necessary for two reasons. First, if there were a high correlation between the two policy instruments, a model predicting both the base effects and the interaction effect could be under-identified. In such a scenario, there would be a weak first stage for the interaction, conditional on the instruments for the base effects. Second, if those areas that were most likely to have high levels of Head Start spending were also likely to have experienced increased K12 spending due to a SFR, then areas that were exposed to high levels of both may differ from areas that were only exposed to only one, or none in unobserved ways. Because our interaction is essentially a comparison of Local Average Treatment Effects (LATEs), if there is treatment heterogeneity, the resulting interaction effect may simply reflect a difference in LATEs rather than a true interaction effect.

We show that this is not a problem in our setting. First, the correlation between Head Start

[47] If the alignment channel is at play, complementarity would be largest for cohorts that were exposed to a SFR in kindergarten (right after Head Start) than for those exposed later. In our setting, cohorts exposed to an SFR at younger ages were also exposed for a longer period, and experienced larger average increases in K12 spending. As such, we cannot credibly decouple treatment age, duration, and dosage effects. However, the visual patterns in Figure 5 are consistent with greater complementarities when the SFR occurs in the early schooling years. Specifically, the cohorts that are exposed to an SFR for more years benefit much more from Head Start than those exposed to a SFR for fewer years (e.g., 8-10 years vs 2-6 years). We take this as suggestive evidence of the alignment channel. To present another suggestive test, we estimate our main models and interact all of our K12 spending variables with indicators measuring the age at which the SFR was implemented in one's childhood state. This is by no means a conclusive test, but the results (Appendix K) suggest that the dynamic complementarity effects we observe are driven by those cohorts that were exposed to an SFR before the age of 9. Specifically, for those who were older than 8 at the time of the passage of an SFR the coefficient on the interaction with Head Start spending is not statistically significant. However, the point estimates on the interaction remain positive for those groups so that we cannot rule out complementarities for this later-treated population. We take this as suggestive evidence in support of our hypothesized mechanism.

spending and instrumented ln(K12 spending) is only 0.15, and conditional on our controls, there is no association between Head Start spending and SFR-induced changes in K12 spending (Appendix Table H6). This suggests that our interaction effects are not based on populations different from that used for the base effects; thus, we are not comparing two different LATEs. Also, following Angrist and Pischke (2009), we compute a series of first-stage F-statistics for each set of excluded instruments, conditional on the other excluded instruments. The first-stage F-statistic on the excluded instruments for K12 spending (i.e., predicted SFR dosage times years of SFR exposure) is 22.41 and 23.01 in models without and with Head Start variables included, respectively (Appendix Table H7). Additionally, the first-stage F-statistic on the excluded instruments for Head Start spending (i.e., the existence of a Head Start center at age 4) is 59.17 and 60.76 in models without and with the K12 instruments included, respectively. Finally, the first-stage F-statistic on Head Start Exposure times SFR dosage times SFR exposure is 42.46, conditional on Head Start Exposure, SFR exposure, and SFR dosage times SFR exposure. In sum, there is sufficient variation in Head Start spending and SFR-induced changes in K12 spending for the effect of each to be identified and for the interaction between the two to be identified.

VI. RESULTS

We present results from specification [6] that exploits all the within-district, across-cohort variation in Head Start spending and instruments for K12 public school spending using the SFR instruments, and specification [7] that instruments for both Head Start spending and K12 spending. To facilitate interpretation of the base effects of K12 spending and Head Start spending when the interaction between the two is included, both K12 spending and Head Start spending are centered on their respective means. Thus, the coefficient on Head Start is the marginal effect of Head Start spending at the average level of K12 spending, and the coefficient on K12 spending is the marginal effect of K12 spending at the average level of Head Start spending. To organize our discussion, we first discuss the base effects of K12 spending (in logs) and Head Start spending, present empirical evidence that these estimated base effects are unbiased, and then discuss the estimated interaction effects. We present our estimated effects on education outcomes, followed by adult economic outcomes, and finally incarceration.

VI.A. *Estimating the Base Effects of Head Start and K12 Spending*

Table 2 presents the estimates for poor (bottom income quartile during childhood) children.

Column 1 presents the DiD-2SLS estimates of the effects on the probability of graduating from high school. The coefficient on Head Start spending per poor four-year-old is 0.025 (p-value<0.01). That is, increasing Head Start spending per poor 4-year-old in the county by $1,000 (roughly a 25% increase) increases the likelihood of graduating from high school by 2.5 percentage points for a poor child exposed to the average level of K12 spending. Given that the average level of Head Start spending, conditional on having any Head Start program in the county, is about $4,230, this implies that, for poor children, having access to the average Head Start program increased the likelihood of graduating from high school by roughly 10 percentage points. Column 2 presents effects for the 2SLS-2SLS design that instruments for all spending variables. The coefficient on Head Start spending per poor four-year-old is 0.0408 (p-value<0.1). In this model, one cannot reject that the DiD-2SLS models and the 2SLS-2SLS models yield different results. However, in the fully instrumented model, the effect of Head Start spending is slightly *larger* and less precisely estimated. Importantly, in both cases, one can reject that the effect of Head Start spending is zero. Because the DiD-2SLS estimated Head Start effects tend to be smaller, and the results are similar to the fully instrumented model, we take a conservative approach and focus on the DiD-2SLS results. However, we do report all results from the 2SLS-2SLS models.

Increases in Head Start spending can affect outcomes through increases in Head Start participation, increases in the quality and scope of Head Start services, and can also indirectly affect outcomes through peer effects in the K12 system due to having better-prepared schoolmates. While existing studies have focused on the effect of *enrolling in* Head Start as participants, we estimate the effect of Head Start spending on *all* eligible children. Because there are multiple channels through which spending effects may emerge, we provide a sense of how our spending effects relate to the participation effects in the extant literature.

We estimate that the rollout of Head Start increased Head Start participation for poor children by about 75 percentage points. We come to this conclusion in two ways.[48] First, using national data, for cohorts entering kindergarten after 1966, the likelihood of Head Start enrollment (full-time or part-year) among income-eligible children was 63% (Figure 1). Because centers can enroll 10% of non-poor children, the participation rate among income-eligible children *could have*

[48] The PSID survey data employed in Garces, Currie, and Thomas (2002) are retrospective data collected in the 1995 wave. There are some concerns about potential measurement error and recall bias in using this retrospective survey information about Head Start participation and some missing information. See Appendix G for further discussion.

been as low as 57 percent. Roughly 80% of poor children born after 1962 in the PSID resided in a county with a Head Start center at age four during this period (this is consistent with national figures). Assuming that only children with a Head Start center in their local area at age four will participate, this implies a Head Start participation rate of 0.57/0.8=0.71 (i.e. 71 percentage points), conditional on having a Head Start center in the county at age 4. We arrive at a similar estimate using retrospective survey questions from the PSID. In the 1995 survey wave as part of a special module on early childhood, adults were asked about whether they had ever participated in a Head Start program. These data may have a number of limitations such as recall bias (See Appendix G). However, in these data, Head Start rollout increases Head Start participation among poor children by about 80 percentage points. We use 75 percentage points as our "ballpark" estimate of the increase in the likelihood of Head Start participation (among poor children) due to the rollout of the average Head Start center in the county during our study period.

If all of our estimated effect of having Head Start access was due to Head Start enrollment (and there were no spillover effects to other poor children), our participation margin effect implies a treatment-on-the-treated effect of 0.1/.75=0.129, or 13.3 percentage points. This is similar to the estimated enrollment effect of Head Start in existing studies.[49] However, most existing studies of Head Start focus on full-year Head Start programs. If one makes the conservative assumption that there is no effect of summer-only programs or part-time programs, a back-of-the-envelope calculation yields an implied treatment-on-the-treated effect of full-year Head Start on the likelihood of high school graduation of 15.3 percentage points.[50] This estimate is in line with the larger of the participation margin effects in the literature. However, we cannot rule out that some modest portion of our effects are driven by (a) improvements in the quality and scope of Head Start centers (full day versus half day, full time versus summer only, better teachers, etc.), and (b) spillovers from Head Start participants to poor non-participants in the K12 school system.

[49] For example, Garces, Currie, and Thomas (2002) find that participating in Head Start increases the high school graduation rates for white by 20 percentage points, with no statistically significant effect for blacks. Deming (2009) finds that Head Start participation increases high school graduation by 11 percentage points for blacks with a small effect for whites, and increases high school graduation by 16 percentage points for those with low maternal test scores. Weikart, Marcus and Xie (2000) find that the average effect is 14 percentage points.

[50] The average enrollment rate among eligible children was 52% after the initial ramp up period (for cohorts entering kindergarten after 1966). This implies a full-year Head Start participation rate of about 0.52/0.8=0.65 conditional on having a Head Start center in the county at age 4. If one makes the conservative assumption that there is no effect of summer only programs or part-time programs so that *all* of our estimated intention-to-treat effect was due to *full-year* Head Start enrollment, an assumed upper-bound full-year Head Start participation margin effect implies a treatment-on-the-treated effect on the likelihood of high school graduation of 0.1/.65=0.153.

As expected, the coefficient estimates for K12 spending are very similar to those presented in Jackson, Johnson, and Persico (2016). The coefficient on the log of K12 spending during the school-age years is 1.10 (p-value<0.01). Increasing K12 school spending (across all 12 school-age years) by 10% increases the likelihood of high school graduation by about 11 percentage points for a poor child exposed to the average level of Head Start spending (Column 1). Relative to baseline, this is about a 15% increase. The estimates indicate that increasing Head Start spending by $4,000 would have roughly the same effect on high school graduation as increasing K12 spending by 10% across all school-age years (for poor children).[51]

Columns 3 and 4 present a similar pattern for completed years of education for poor children. The more conservative DiD-2SLS estimates reveal that increasing Head Start spending per poor 4-year old in the county by $1,000 increases the years of educational attainment by 0.077 years (p-value<0.01) for a poor child exposed to the average level of K12 spending. At average Head Start spending levels, a Head Start center is estimated to increase years of education by roughly a third of a year. As discussed above, the 2SLS-2SLS estimate is larger and less precise and indicates that increasing Head Start spending per poor 4-year old in the county by $1,000 increases the years of educational attainment by 0.2255 years (p-value<0.1) for a poor child exposed to the average level of K12 spending. Increasing school-age K12 spending by 10% increases the number of years of completed education by about 0.4 years for a poor child exposed to the average level of Head Start spending.

Results for non-poor children (top 3 income quartiles during childhood) are in Table 3. The estimated K12 spending effects on the education outcomes are positive, sizable, and statistically significantly different from zero. This indicates that increases in K12 spending improve the educational outcomes of not only the poor but also the non-poor. The more conservative DiD-2SLS point estimates indicate that increasing K12 spending in the district by 10% increases the likelihood of high school graduation by 2.3 percentage points, and increases years of educational attainment by about 0.24 years for a non-poor child exposed to the average level of Head Start spending. These estimated K12 spending benefits are smaller for more affluent children than for poor children, but they are positive, statistically significant, and economically important. In contrast to the positive K12 spending effects, for children from non-poor families, increasing

[51] During the sample period, a 10% increase in K12 spending is roughly equal to increasing per-pupil K12 spending by $480 each year over 12 years (about $4300 in present value terms assuming a 7% interest rate).

county Head Start spending has very small, insignificant effects. For both education outcomes, one cannot reject that the effect on the non-poor is zero, and one *can* reject that the Head Start effect is the same for both poor and non-poor children.[52] This suggests that (a) there are no spillover effects of Head Start spending on non-poor children and that (b) increases in Head Start spending are not associated with other broad policies that improve the outcomes of non-poor children.

The fact that we find no effect of Head Start spending for non-poor children is important. If local areas that increased Head Start spending introduced other policies that improve outcomes of all children, one would observe positive Head Start spending effects for the non-poor children. We find no such pattern. Our result, instead, implies that neither our variation in Head Start spending nor the rollout of Head Start is associated with any policies that improved the outcomes of local children who were ineligible to participate in Head Start. This coupled with the fact that Head Start spending only influences outcomes for those who were four years old at the time shows that we only see effects for children who were both income- and age-eligible for Head Start. This serves as another falsification test, of sorts, and bolsters the credibility of the research design.

The adult economic outcomes we examine are wages and the annual incidence of poverty between the ages of 20 and 50. Our models use all available person-year observations for ages 20–50 and control for a cubic in age to avoid confounding life cycle and birth cohort effects. Columns 5 through 8 in Table 2 present these results for children from poor families. Looking at wages, in the more conservative DiD-2SLS models (column 5) the coefficient on the log of public K12 school spending is 2.056 (p-value<0.1) and that on Head Start spending per poor 4-year-old is 0.023 (p-value<0.01). That is, for children from poor families exposed to average levels of Head Start spending, increasing K12 spending by 10% is associated with about 20.5% higher adult wages. Similarly, for these same children, at average public K12 spending levels, increasing Head Start spending by $4,230 per poor 4-year-old (the average spending amount) is associated with 9.87% higher wages for poor children. The results in the 2SLS-2SLS (where both Head Start spending and K12 spend are instrumented for) are similar; increasing K12 spending by 10% is associated with about 12.6% higher adult wages, and increasing Head Start spending by $4,230 per poor 4-year-old is associated with 15.29% higher wages for poor children. These effects (DiD-

[52] We pooled the samples and estimated a single model where we interacted all variables with poverty status, and tested for equality of coefficients between poor and non-poor children for our key explanatory variables. We present the results of this test for our two main adult outcomes in the conservative DiD-2SLS models in Appendix Table J1. Our tests reject that the estimates are the same for the two populations.

2SLS vs 2SLS-2SLS estimates) are not statistically distinguishable from each other.

Columns 5 and 6 of Table 3 present the effects on adult wages for non-poor children. Similar to the educational outcomes, there are positive effects of K12 spending, but no effect of Head Start spending on the wages in adulthood of those from non-poor families. In the DiD-2SLS models, the coefficient on the log of K12 public school spending is 0.7351 (p-value<0.05), and that on Head Start spending per poor 4-year-old is 0.0069 (p-value>0.1). That is, for children from non-poor families exposed to average levels of Head Start spending, increasing K12 spending by 10% is associated with 7.35% higher earnings between the ages of 20 and 50, while increasing Head Start spending is associated with no difference in earnings. The 2SLS-2SLS results (column 6) tell the same basic story as the DiD-2SLS models.

The pattern of estimates for the annual incidence of poverty in adulthood in columns 7 and 8 of Tables 2 and 3 mirror those for adult wages. A family is poor if their income-to-needs ratio is below the federally-determined threshold for poverty. Furthermore, while adult poverty is *related* to family income and wage, it is a measure of hardship. Among poor children, Head Start spending is associated with large, statistically significant reductions in the annual incidence of poverty in adulthood (Table 2); while Head Start has small, insignificant effects on the adult outcomes of non-poor children (Table 3). However, increases in public K12 spending are associated with significant reductions in the likelihood of poverty in adulthood for all children, on average.

The final outcome we examine is the probability that an individual has ever been incarcerated (Column 9 and 10 of Tables 2 and 3). In the DiD-2SLS model, for poor children (Table 2), a $1,000 increase in Head Start spending reduces the likelihood of being incarcerated by 0.6 percentage points (p-value<0.01). This implies an average Head Start rollout effect (i.e., an increase of $4,320) of 2.5 percentage-points lower likelihood of adult incarceration (at average public K12 spending level). If one were to ascribe all of this effect to the participation margin for full-year Head Start, it would imply a Head Start participation effect of a five-percentage-point reduction in the probability of ever being incarcerated. Effects of this magnitude are in line with the results from Garces, et al. (2000). Column 9 also shows that increasing K12 per-pupil spending by 10% (at average Head Start spending levels) reduces the likelihood of adult incarceration by eight percentage points (p-value<0.05). The magnitude of this effect is in line with the estimated reductions in incarceration associated with increased schooling (Lochner and Moretti, 2003), and reductions in crime associated with attending a better school (Deming, 2011). Note, however, that this is the first paper to document a causal relationship between increased public school K12 spending and reduced risks of adult incarceration. The 2SLS-2SLS models in column 10 yield similar patterns, but with somewhat larger Head Start effects and wider confidence intervals. Looking at non-poor children (Table 3), we find no effect of either Head Start or K12 spending on the likelihood of adult incarceration among non-poor children. We attribute this to the low levels of incarceration among non-poor children. Importantly, *as with the other outcomes*, Head Start spending has no impact on those who were not income-eligible to participate.[53]

VI.B. *Testing for Bias due to Unobserved Family Differences*

While we have presented much evidence that our variation is exogenous to other policies that may have been implemented in a locality, we have not *yet* ruled out the possibility that our results are driven by unobserved differences across treated and untreated families within local areas. To do this, we rely on variation *within* families and compare the outcomes of siblings who were different ages at Head Start rollout or at the time of a court-ordered SFR, but were raised in the same household with the same parents. This approach accounts for observed and unobserved shared family characteristics that predict outcomes. We achieve this by augmenting [6] and [7] to include sibling fixed effects (see Appendix Table H4). In such models, effects are similar to those in Table 2 so that unobserved family differences cannot explain the main pattern of results.

VI.C. *Evidence of Dynamic Complementarity Effects*

Before presenting the magnitudes of any complementarity effects, we first establish whether such effects exist. Specifically, in the estimation of [6] and [7], we test whether the coefficient on the interaction is positive and statistically significantly different from zero. In Table 2, for children from poor families, across both the DiD-2SLS models and the 2SLS-2SLS models, the coefficient on the interaction is positive for high school graduation, years of education, and adult wages. The positive interaction effect in the DiD-2SLS models is statistically significant at the 10% level for high school graduation, the 1% level for years of completed education and the 5% level for adult wages. In the 2SLS-2SLS models, the positive interaction effect is statistically significant at the 1% level for high school graduation, the 10% level for years of completed education and the 1% level for adult wages. For the two adverse outcomes, adult poverty and ever

[53] Because schools tend to be segregated by parental socioeconomic status, any indirect spillover effects of Head Start on non-Head Start enrollees will likely be experienced by poor children.

being incarcerated, the coefficients on the interaction terms are negative and statistically significant at, *at least*, the 10% level in any of the models. Across all outcomes for poor children, and across all specifications, increases in Head Start spending raise the marginal effect of K12 spending and vice versa. In contrast, there is no such relationship for children from non-poor families (Table 3). For none of the outcomes is the coefficient on the interaction term statistically significant, and the signs of the coefficients across outcomes do not go in the same direction.[54] That is, Head Start spending had no direct or indirect effect on the outcomes of non-poor children.

To show the impact of these interaction effects, we present the marginal effects of each intervention evaluated at different levels of the other. Specifically, using the regression estimates, we compute the marginal effect of increasing Head Start spending per poor four-year-old by \$4,230 when there is a 10% decrease, no increase, and a 10% increase in K12 spending (conditional on the direct effect of the change in K12 spending). Similarly, we compute the marginal effect of increasing K12 spending by 10% where there is no Head Start in the county and counties with average Head Start spending (\$4,230). The estimated marginal effects for each model is presented in the lower two panels of Tables 2 and 3.

Looking at high school graduation among poor children, the DiD-2SLS models indicates that having a Head Start center with a 10% decrease in K12 spending increases high school going by a statistically insignificant 6.3 percentage points. However, having a Head Start center with a 10% increase in K12 spending increases high school going by a 14.87 percentage points (*p*-value<0.01). The 2SLS-2SLS results are similar and suggest that having a Head Start center with a 10% decrease or 10% increase in K12 spending increases high school going by 7.6 and 26.9 percentage points, respectively. In both models, the marginal effect of Head Start is more than twice as large when followed by a 10% increase in K12 spending than when followed by a 10% decrease. Also, in both models, the marginal effect of Head Start when there is a 10% decrease in K12 spending (though economically meaningful) cannot be distinguished from zero is a statistical sense. We now quantify the interaction concerning the marginal effect of K12 spending. The DiD-

[54] We formally test that the marginal effects of Head Start and the "HeadStart*K12" interaction are different for poor children and non-poor children for years of education and adult wages. We do this by stacking the data and testing for equality of the coefficients. We present this test for years of education and wages in Appendix K. We can reject that the estimates are the same for the two populations at the 5% level for years of education and the 10% level for adult wages. The tests for the other outcomes are not in the Table; however, the *p*-value on the hypothesis that the effects are the same for poor and the non-poor is 0.043 for years of education, 0.155 for high school graduation, 0.0596 for adult wages, 0.011 for adult poverty, and 0.46 for adult incarceration.

2SLS results indicate that increasing K12 spending across all school-age years by 10% increases the likelihood of graduating high school by 6.7 and 11 percentage points, with and without Head Start, respectively. Similarly, the 2SLS-2SLS results indicate that increasing K12 spending across all school-age years by 10% increases the likelihood of graduating high school by 4.55 and 14.16 percentage points, with and without head Start, respectively. Similar comparisons for children from non-poor families reveal that the effect of K12 spending on the outcomes of the non-poor is similar irrespective of the level of Head Start, and Head Start has no effect on the outcomes of the non-poor irrespective of the level of K12 spending. Because these are similar to, but more conservative than the 2SLS-2SLS estimates, we focus on these models for the remaining outcomes.

The pattern of results for years of completed education is similar to those for high school graduation. The DiD-2SLS results are presented graphically in Figure 6 (the underlying estimates are in Table 2 and 3). For poor children (left panel), access to the average Head Start center increases completed education by 0.0533 years with a 10% reduction in K12 spending, increases education by 0.32 years with no change in K12 spending, and increases education by 0.599 years with a 10% increase in K12 spending. While the effect of Head Start with a reduction in K12 spending cannot be distinguished from zero, the effect when coupled with a 10% increase in K12 spending is statistically significant at the 1% level. For non-poor children (right panel), there is no effect of Head Start irrespective of the increase in K12 spending. Looking at the effect of K12 spending, for poor children, increasing K12 spending by 10% increase the years of education by 0.13 and 0.4 years, without and with Head Start, respectively. The effect of K12 spending is more than twice as large among poor individual exposed to Head Start than those who are not. For children from non-poor families (who are not eligible for Head Start), increasing K12 spending by 10% lead to about a 0.23 more years of education irrespective of the Head Start exposure.

In sum, these patterns suggest important dynamic complementarity between early childhood education spending and public K12 spending for the educational outcomes of poor children. In fact, due to the dynamic complementary for poor children, the pattern of results indicate that in areas with Head Start programs, increases in K12 spending both increased outcomes for all students and simultaneously reduced educational attainment gaps. The fact that there is no evidence of complementarity for non-poor children is important. It suggests that our main effects are not simply picking up some strange LATE for those places that happen to be exposed to both high K12 spending levels and Head Start. If our effects were due to this, one would observe positive interaction effects for all children in such districts. Instead, we find no interaction effects for the non-poor – indicating that our diagnostic tests were likely valid and further supports that our empirical strategy credibly identifies the interaction effects.

Commensurate with the educational outcomes, there is evidence of complementarity between Head Start spending and public K12 spending in the production of adult economic outcomes for children from poor families. Because for non-poor children there are no interaction effects for any outcome, we focus the remainder of the discussion on the results for poor children. Figure 7 presents the marginal effect on adult wages of K12 spending by Head Start access (and vice-versa). For poor children (left panel), access to Head Start (with average funding levels) increases adult wages by 2.7% (p-value>0.1) when coupled with a 10% K12 spending decrease, increases it by 9.8% when there is no change in K12 spending (*p*-value<0.01), and increase wages by 17% when coupled with a 10% increase in K12 spending (*p*-value<0.01). As shown in Table 2, the 2SLS-2SLS estimates are similar. The dynamic complementarities are sufficiently large that the marginal effect of the same increases in Head Start spending on the adult wage is about 70% larger when K12 spending increases by 10% than with no change. Looking at the effects of K12 spending increases, a 10% increase in K12 spending leads to 13% higher wages without Head Start, and 20% higher wages with Head Start (both effects are significant at the 1% level). The 2SLS-2SLS estimates are somewhat smaller, but show the same pattern of much larger marginal impacts of K12 spending among cohorts who are exposed to Head Start.

The effects on the annual incidence of adult poverty are consistent with those on education and wages (Columns 7 and 8 of Table 2). For poor children, increasing Head Start spending from zero to average levels reduces the annual incidence of poverty in adulthood by about 3 percentage points (p-value>0.1) when coupled with a 10 reduction in K12 spending, a 7.6 percentage point reduction when coupled with no change in K12 spending (*p*-value<0.01), and reduces adult poverty by 12 percentage-points when coupled with a 10% increase in K2 spending (*p*-value<0.01). As with the adult wage, the 2SLS-2SLS estimates are similar, and the marginal effect of the same increases in Head Start spending on the adult wage is about 60% larger when K12 spending increases by 10% than with no change. The marginal effects of K12 spending tell the same story. A 10% increase in K12 spending leads to 3.3 and 7.96 percentage-points lower adult poverty without and with Head Start, respectively. The effect of the K12 spending increase with Head Start is significant at the 1% level and is more than twice as large as the effect with no Head

Start. The marginal effects from the 2SLS-2SLS model are almost identical (columns 7 and 8 of Table 2) so that these patterns are quite robust.

As with the other adult outcomes, the reduction in the lifetime risks of incarceration associated with improvements in access to early education is larger when there are greater subsequent K12 school investments and *vice versa*. The marginal effects are presented in (Columns 9 and 10 of Table 2). For poor children, increasing Head Start spending from zero to average levels has no effect on the likelihood of incarceration when coupled with a 10% reduction in K12 spending. However, this same increase in Head Start exposure leads to a 2.5 percentage point reduction when coupled with no change in K12 spending (p-value<0.1), and a 4.73 percentage point reduction when coupled with a 10% increase in K2 spending (p-value<0.01). Looking to the effect of K12 spending on the likelihood of being incarcerated, the marginal effects are larger with Head Start than without. A 10% increase in K12 per-pupil spending reduces the likelihood of being incarcerated by 5.8 percentage points with no Head Start spending (p-value<0.05), and by eight percentage points with Head Start (p-value<0.01).

VI.D. *Is Parenting Quality Part of the Story?*

In Section II, we posited that the Head Start effects are driven by the components targeted to children. Because parent counseling was a component of Head Start, it is possible that these dynamic complementarities emerge through improvements in parenting quality. Because we have data on siblings with the same parents, we can test for improvements in parenting quality. We use only the sample of older siblings who were not themselves exposed to Head Start and test whether those with younger siblings who were exposed to Head Start have improved outcomes. If improvements in parenting quality is a part of the story, the older siblings of exposed younger siblings should have better outcomes than the older siblings of unexposed younger siblings. However, if the Head Start effects are driven by the services provided to the children, there should be no effect. In these models (Appendix I), we find older siblings are unaffected by Head Start exposure of the younger sibling. This suggests that (a) parenting quality is not part of the story, (b) our Head Start spending effects reflect real investments in the human capital of poor children and (c) our effects are not due to other confounding policies aimed at poor children.

VI.E. *Are the Complementarity Effects Driven by Other Coincident Policies?*

Even though our estimation equations control for several coincident polices directly, one may worry that our main results are driven by some complementarity between K12 spending and some other policy. To test for this directly, we augment our main model in equation [6] to also include (a) interactions between food stamp spending in one's county between ages 0 to 4 with K12 spending, and (b) county-level spending on Medicaid between ages 0 and 4 interacted with K12 spending. In these models, the point estimates on the interaction between Head Start spending and K12 spending are virtually unchanged.[55] This provides further evidence that our estimated effects are not confounded by dynamic complementarities with other policies.

VII. Benefit-Cost Considerations: putting the magnitudes in perspective

It is helpful to consider how the presence of dynamic complementarity affects the optimal allocation of resources to the K12 system versus to early childhood education (for poor children). In any given location, if average outcomes are maximized, the marginal dollar spent on Head Start will yield the same effect on outcomes as an equivalent expenditure on K12 education.

It is helpful to define some parameters. The proportion of poor children in a county is p. The average per-student cost of rolling out the average Head Start center is the cost of increasing Head Start spending per poor-4-year old by $4,320. The average cost of this increase is simply $4320*p$. The marginal effect of rolling out the average Head Start center for a county (π_{HS}), is a poverty-weighted average of the effect of a $4,320 increase in Head Start spending on low-income children ($\delta_{HS,\ poor}$), and that for non-poor children ($\delta_{HS,non}$). Because Head Start has no effect on non-poor children, this simplifies to [8] below.

[8] $$\pi_{HS} = p\delta_{HS,poor}.$$

To equate the marginal effects of spending on Head Start to that of spending on the K12 system, we need to define the change in K12 spending that would lead to the same expenditure as an increase of $4,320 in Head Start spending per poor-4-year old. During our sample period, K12 spending was roughly $4,000 per student per year *on average*. Assuming a 7% interest rate, spending $4,000 for 12 years is equivalent to $34,000 in present value terms. Thus, an equivalent expenditure at the *student* level would be a $4320p/34000=(p*12.7)\%$ increase in K12 spending. We define $\delta_{K12,poor}$ and $\delta_{K12,non}$ as the effect of increasing K12 spending by one% on poor and

[55] We take this evidence as suggestive. While we have subjected our estimated Head Start spending and K12 spending effects to extensive specification checks of validity, we have not done so for Food Stamp or Medicaid spending. For this reason, we take this only to indicate the robustness of our main effects and not as any indication regarding the interaction between these other policies and K12 spending. Exploration into these issues is outside the scope of this paper.

non-poor children, respectively. The marginal effect of the equivalent increase in K12 spending on the average child in the county is therefore

[9] $$\pi_{K12} = (p\delta_{K12,poor} + (1-p)\delta_{K12,non})(12.7p)$$

The ratio shown in [10] between these two equations π_{HS}/π_{K12} is the relative effectiveness of rolling out Head Start (from having no center) and spending the same amount across all children from that same cohort in the county in the K12 system.

[10] $$\frac{\pi_{HS}}{\pi_{K12}} = \frac{p\delta_{HS,poor}}{(p\delta_{K12,poor}+(1-p)\delta_{K12,non})(12.7\text{p})} = \frac{\delta_{HS,poor}/(12.7)}{p(\delta_{K12,poor}-\delta_{K12,non})+\delta_{K12,non}}$$

The relative marginal effect of Head Start rollout and the equivalent spending in the K12 system is a function of the poverty rate p as long as $\delta_{K12,poor} \neq \delta_{K12,non}$. Specifically, if $\delta_{K12,poor} > \delta_{K12,non}$, then this ratio is falling in p, and if $\delta_{K12,poor} < \delta_{K12,non}$ this ratio is increasing in p. Intuitively, if non-poor children are more responsive than poor children to increases in K12 spending (i.e. $\delta_{K12,poor} < \delta_{K12,non}$), then the marginal benefit of increased K12 spending declines with the poverty rate so that the *relative* effectiveness of Head Start spending increases with the poverty rate. The converse is also true.

To show the relationship between this ratio and the poverty rate at the average level of K12 spending, in Figure 8 we plot this ratio against the poverty rate, where this ratio is evaluated at the mean level of K12 (i.e., using the empirical estimates from Table 2). We show this for adult wages (effects are similar for other outcomes). Because our empirical model is linear in Head Start spending but linear in the *log* of K12 spending, the marginal effect of K12 spending will fall relative to that for Head Start at higher levels of K12 spending even without any complementarity. To show this relationship, on the left, we impose the condition that there is no interaction effect and then plot the resulting π_{HS}/π_{K12} against the poverty rate where the present value is evaluated at the average K12 spending levels, 10% above this average and 10% below this average.

As one would expect, on the left panel, this ratio is falling with the poverty rate. This reflects the fact that K12 spending has a larger effect on poor children so that the average benefits of K12 spending are larger in higher poverty areas. Also as expected, (even where there is dynamic complementarity) the ratio is higher when evaluated at higher levels of K12 spending. Interestingly, with no dynamic complementarity, the relative marginal benefit of rolling out a Head Start center lies below that of K12 spending so long as the poverty rate is above about 20 percent. With no dynamic complementarity, this is true even in areas that spend 10% above average in the

K12 system. To illustrate how dynamic complementarity affects these ratios, we allow for dynamic complementary (i.e., using the empirical estimate of the interaction term from Table 2) and then evaluate and present these same ratios (right panel). As one can see, evaluated at the average, the basic pattern is the same. However, with dynamic complementarity, the ratios are very different at K12 spending levels 10% above and below the average. Where complementarities exist, in areas that spending 10% higher than average in the K12 system, this ratio lies above 1 at all poverty levels, so that the marginal impact of rolling out Head Start on average wages is larger than the effects of spending that same money in the K12 system. The flipside of this result is that in areas that spend less than 10% lower than the average, this ratio lies below 1 for all poverty levels. This means that in areas with low levels of K12 spending, the marginal dollar is better spent in the K12 system than on Head Start.

In essence, these patterns support the idea that, when such dynamic complementarities exist between early and late human capital investments, in some locations, there may be no equity-efficiency tradeoff when shifting resources toward compensatory early education programs (Cunha and Heckman, 2007). More specifically, our estimates indicate that, for a district that spent $4,500 per-pupil (about 10% above the average K12 spending level), the marginal dollar spent on Head Start led to between 1.5 and 2.5 times the improvement in adult outcomes as that spent on K12 education. Accordingly, at such spending levels, one could redistribute money from the K12 system towards Head Start and have *both* better average outcomes and a more equitable distribution of adult outcomes. Overall, the patterns in Figure 8 suggests that during our sample period, the marginal dollar had a roughly equal effect on adult outcomes overall at levels close to the national averages that prevailed at that time. The patterns also indicate that, when resources are allocated efficiently, localities with higher levels of Head Start spending should have higher levels of K12 spending and vice versa. Empirically, the correlation between per-pupil spending and Head Start spending is roughly 0.35. This implies that, in general, localities may be taking advantage of these complementarities, but that further optimization is likely possible.

VII.A SUMMARY AND CONCLUSIONS

This study provides new evidence on the life-cycle effects of Head Start and K12 school spending. We explore dynamic complementarities between human capital investments made in pre-school and those that subsequently occur in the K12 system. We use children's differential exposure to Head Start spending (at age 4) and court-ordered school finance reforms (SFRs) (between the ages 5 through 17), depending on place and year of birth, to examine whether the marginal effect of Head Start spending on children's adult outcomes are larger among individuals who were subsequently exposed to SFR-induced K12 spending increases. We present extensive tests to document that the policy-induced variation in Head Start spending and K12 public school spending we exploit is unrelated to other childhood family, community, or policy changes.

For non-poor children, SFR-induced K12 spending increases led to significant improvements in educational and economic outcomes, while increases in Head Start spending had no effect. However, for poor children, both Head Start spending increases and SFR-induced K12 spending increases led to significant improvements in educational outcomes, economic outcomes, and reductions in the likelihood of incarceration. Importantly, the long-run effects of increases in Head Start spending are amplified when followed by attending schools that experienced SFR-induced increases in K12 per-pupil spending. Across all the outcomes, the marginal effect of the same increase in Head Start spending was more than twice as large for students from K12 school districts that spent at the 75th percentile of the distribution than those from K12 school districts that spent at the 25th percentile. Similarly, the benefits of K12 school-spending increases on adult outcomes were larger among poor children who were exposed to higher levels of Head Start spending during their pre-school years. For poor children, the combined benefits of growing up in districts/counties with *both* greater Head Start spending and K12 per-pupil spending are significantly greater than the sum of the independent effects of the two investments in isolation.

There are two important caveats to our work. First, because the counterfactual childcare and pediatric care may be better today than in the late 1960s and 70s, the marginal effect of Head Start may be smaller today than in the earlier period that we study.[56] Second, public school spending levels during the period we study were lower than current levels. If school spending exhibits diminishing marginal product, the effects presented here may be larger than one would observe with similar spending increases today. These caveats do not minimize the importance of the findings or their profound implications for policy. However, they do suggest that the

[56] In the early period of Head Start, most poor children would have received home care, while today, as many as one-third of Head Start participants may have attended some other form of formal childcare (Kline and Walters, 2016; Feller et al., 2016). The proportion of three- and four-year-olds in school has increased from roughly 10 percent in 1964 to almost 40 percent by 1995 (source: US Census Bureau, CPS October Supplement, 1964-2010; see Figure 1). Also, while most poor children currently receive pediatric care through Medicaid and SCHIP, during the period under study many children would only have received such care through Head Start.

contemporary magnitude of the effects may be smaller than those we present here. At the same time, the returns to education have increased, so the consequences of access to high-quality human capital investments from preK-12 are large.

The cumulative nature of skill development is likely responsible for the pattern of results. Our findings highlight the importance of modeling early and later educational investments jointly and may explain some disparate results on the effects of Head Start. Indeed, our finding that the long-run effects of Head Start are larger among individuals who attended better-resourced schools may provide an explanation for why Head Start may have been more successful for more socioeconomically-advantaged populations (Currie and Duncan, 1995) and why there is a fade out of the effects of Head Start on test scores as students age (Currie and Duncan, 2000). The key policy implication of our findings is that human capital investments made in, and sustained throughout, child developmental stages (pre-school; elementary/middle school; adolescence) may yield greater returns than separate, isolated, short-lived reforms not sustained beyond the year in which they are implemented. The findings point to the critical role early-life investments can play in narrowing long-run gaps in well-being, and they also highlight the importance of sustained investments in the skills of disadvantaged youth.

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Table 1:
Summary Statistics of the Analytic Dataset

	All (N=15,232)	Poor Child (N=6,373)	Non-Poor Child (N=8,859)
Adult Outcomes:			
High School Graduate	0.85	0.71	0.89
Years of Education	13.29	12.29	13.61
Ln(Wages), at age 30	2.49	2.24	2.56
Adult Family Income, at age 30	$48,655	$35,372	$52,448
In Poverty, at age 30	0.08	0.18	0.05
Ever Incarcerated	0.05	0.08	0.04
Age (range: 20-50)	30.8	30.3	31.0
Year born (range: 1950-1976)	1962	1962	1962
Female	0.44	0.43	0.44
White	0.87	0.66	0.93
Childhood school variables:			
Any Head Start Center in county, age 4	0.33	0.33	0.34
Post rollout: Head Start spending per poor 4-year old, age 4	$4,103	$4,204	$4,072
Child attended Head Start*	0.04	0.19	0.02
Child attended any pre-school program	0.23	0.31	0.23
School District Per-pupil spending (average, ages 5-17)	$4,366	$4,031	$4,470
Any court-ordered school finance reform, age 5-17	0.13	0.11	0.14
Cond'l on any: # of exposure yrs. to school finance reform	7.37	6.90	7.50
1960 District Poverty Rate (%)	21.52	28.25	19.35
Childhood family variables:			
Income (average, ages 12-17)	$54,488	$22,520	$65,130
Income-to-needs ratio (average, ages 12-17)	3.05	1.31	3.62
Mother's years of education	11.84	10.61	12.24
Father's years of education	11.82	10.04	12.36
Born into two-parent family	0.90	0.74	0.95
Low birth weight (<5.5 pounds)	0.07	0.07	0.07

Note: All descriptive statistics are sample weighted to produce nationally-representative estimates of means. Dollars are CPI-U deflated in real 2000 $. "Poor kid" is defined here as children whose parents were in the bottom quartile of the income distribution (approximately 80% of whom were below the poverty line). Analysis sample includes 15,232 individuals (218,594 person-year observations ages 20-50), from 4,990 childhood families, 1,427 school districts, 1,120 childhood counties and all 50 states.

*Child-specific pre-K attendance & Head Start program participation info collected retrospectively in 1995 survey IW.

Table 2: *Marginal Effects of Head Start Spending and Public Per-Pupil Spending and Their Interaction: Poor Children*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Prob(High School Grad)		Years of Completed Education		Ln(Wage), ages 20-50		Annual Incidence of Poverty, age 20-50		Prob(Ever Incarcerated)	
	DiD-2SLS	2SLS-IV	DiD-2SLS	2SLS-IV	DiD-2SLS	2SLS-IV	DiD-2SLS	2SLS-IV	DiD-2SLS	2SLS-IV
Head Start Spending$_{(age\ 4)}$	0.02503***	0.04089*	0.07721***	0.2255*	0.02334***	0.03615*	-0.01808***	-0.02576*	-0.006002*	-0.02024*
	(0.006942)	(0.02453)	(0.01992)	(0.1212)	(0.004503)	(0.01956)	(0.005302)	(0.01385)	(0.003494)	(0.01082)
(SFR) Instrumented Ln(PPE)$_{(age\ 5-17)}$	1.1016***	1.4163***	4.0399**	4.0218**	2.0561***	1.2596***	-0.7923***	-0.7971***	-0.8080**	-1.1822***
	(0.3268)	(0.3390)	(1.6751)	(1.7856)	(0.4348)	(0.2690)	(0.2969)	(0.2903)	(0.3397)	(0.4550)
Head Start Spending$_{(age\ 4)}$*ln(PPE)$_{(age\ 5-17)}$	0.1012*	0.2273***	0.6460***	0.8345*	0.1698**	0.2561***	-0.1079**	-0.1852***	-0.05169*	-0.1808*
	(0.05454)	(0.06518)	(0.2354)	(0.4824)	(0.06985)	(0.07191)	(0.04267)	(0.05038)	(0.02777)	(0.1076)
Marginal Effects of 10% increase in K12 Spending by Head Start access:										
No Head Start$_{(age\ 4)}$	0.0673***	0.0455	0.1307	0.0492	0.1338***	0.0176	-0.0336	-0.0014	-0.0589**	-0.0418**
	(0.0236)	(0.0316)	(0.1274)	(0.1064)	(0.0349)	(0.0219)	(0.0301)	(0.0153)	(0.0283)	(0.0193)
Head Start Center access$_{(age\ 4)}$	0.1102***	0.1416***	0.4040***	0.4022**	0.2056***	0.1260***	-0.0792***	-0.0797***	-0.0808***	-0.1182***
	(0.0327)	(0.0339)	(0.1675)	(0.1786)	(0.0435)	(0.0269)	(0.0297)	(0.0290)	(0.0340)	(0.0455)
Marginal Effects of Head Start with 10% increase or decrease in K12 Spending:										
w/10% decrease	0.0630	0.0768	0.0533	0.6010	0.0269	0.0446	-0.0308	-0.0306	-0.0035	-0.0092
	(0.0481)	(0.1169)	(0.1393)	(0.5937)	(0.0284)	(0.0921)	(0.0206)	(0.0568)	(0.0229)	(0.0487)
Average	0.1059***	0.1730*	0.3266***	0.9540*	0.0987***	0.1529*	-0.0765***	-0.1090*	-0.0254*	-0.0856*
	(0.0294)	(0.1038)	(0.0843)	(0.5129)	(0.0190)	(0.08275)	(0.0224)	(0.0586)	(0.0148)	(0.0457)
w/10% increase	0.1487***	0.2691***	0.5999***	1.3070***	0.1706***	0.2613***	-0.1221***	-0.1873***	-0.0473***	-0.1621***
	(0.0217)	(0.0968)	(0.1209)	(0.5068)	(0.0408)	(0.0841)	(0.0351)	(0.0674)	(0.0138)	(0.0772)
Number of Person-year Observations	--	--	--	--	55,706	55,706	88,124	88,124	--	--
Number of Children	5,419	5,419	5,419	5,419	5,613	5,613	6,373	6,373	4,536	4,536

Robust standard errors in parentheses (clustered at childhood state level)

*** p<0.01, ** p<0.05, * p<0.1

Data: PSID geocode Data (1968-2015), matched with childhood school and neighborhood characteristics. Sample includes all individuals born 1950-1976 whose parents were in the bottom quartile of the income distribution, and who have been followed into adulthood.

Models: (non-Instrumented & Instrumented) Head Start Spending per poor 4-year old at age 4 in the county and instrumented ln(school district per-pupil spending during ages 5-17) are centered around their respective means, to facilitate interpretation of the main effects evaluated at roughly the mean; the average SFR-induced increase in school-age spending is about 10%. Results are based on 2SLS-IV models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific birth year trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race; controls for 1960 county characteristics (poverty rate,% black, education,% urban, population size, each interacted with linear cohort trends; controls for county-level per-capita gov't safety net expenditures average during childhood; and controls for childhood family characteristics (parental income/education, mother's marital status at birth, birth weight, gender), and age (cubic). The first-stage model includes as predictors the school-age years of exposure to school finance reform interacted with the quartile of the respective school district's predicted reform-induced change in school spending based on the timing and type of court-ordered reform interacted with 1970 (within-state) district income and spending percentile categories. The instrument used for Head Start spending per poor 4-year old is an indicator for whether there was any Head Start center in the county at age 4 (based on the program's rollout timing variation only). There exists a significant first-stage. The marginal effects related to Head Start access are based on the average county Head Start spending when there is a center (~$4,230 (in real 2000 dollars))--i.e., marginal effects are evaluated for roughly a $4K increase in Head Start spending (to contrast impact of having access vs no access to Head Start center).

Table 3: *Marginal Effects of Head Start Spending and Public Per-Pupil Spending and Their Interaction: Non Poor Children*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Prob(High School Grad)		Years of Completed Education		Ln(Wage), ages 20-50		Annual Incidence of Poverty, age 20-50		Prob(Ever Incarcerated)	
	DiD-2SLS	2SLS-IV	DiD-2SLS	2SLS-IV	DiD-2SLS	2SLS-IV	DiD-2SLS	2SLS-IV	DiD-2SLS	2SLS-IV
Head Start Spending$_{(\text{age 4})}$	0.000014	-0.02227	0.008866	-0.05426	0.006901	0.01932	-0.000085	-0.008452	-0.001274	0.000937
	(0.003432)	(0.01864)	(0.01635)	(0.1071)	(0.005408)	(0.02433)	(0.001716)	(0.005692)	(0.001705)	(0.006974)
(SFR) Instrumented Ln(PPE)$_{(\text{age 5-17})}$	0.2386**	0.4671**	2.4192**	2.1565+	0.7351**	0.4155*	-0.1383**	-0.1868+	-0.09837	0.1298
	(0.1197)	(0.2351)	(1.1645)	(1.5314)	(0.3035)	(0.2366)	(0.06316)	(0.1304)	(0.2161)	(0.2758)
Head Start Spending$_{(\text{age 4})}$*ln(PPE)$_{(\text{age}}$	0.01688	0.09666	0.02972	0.4144	0.02577	-0.01603	0.005707	-0.000716	-0.02568	-0.01128
	(0.02347)	(0.08062)	(0.1937)	(0.3706)	(0.03090)	(0.1020)	(0.01459)	(0.04094)	(0.02271)	(0.06604)
Marginal Effects of 10% increase in K12 Spending by Head Start access:										
No Head Start$_{(\text{age 4})}$	0.0167+	0.0058	0.2294+	0.0404	0.0626*	0.0483	-0.0162*	-0.0184	0.0010	0.0177
	(0.0123)	(0.0252)	(0.1427)	(0.1534)	(0.0360)	(0.0408)	(0.0091)	(0.0148)	(0.0147)	(0.0122)
Head Start Center access$_{(\text{age 4})}$	0.0239**	0.0467**	0.2419**	0.2157+	0.0735**	0.0416*	-0.0138**	-0.0187+	-0.00984	0.01298
	(0.0120)	(0.0235)	(0.1165)	(0.1531)	(0.0303)	(0.0237)	(0.00632)	(0.0130)	(0.0216)	(0.02758)
Marginal Effects of Head Start with 10% increase or decrease in K12 Spending:										
w/10% decrease	-0.0071	-0.1351	0.0249	-0.4048	0.0183	0.0885	-0.0028	-0.0355	0.0055	0.0087
	(0.0228)	(0.0921)	(0.1189)	(0.4858)	(0.0150)	(0.1253)	(0.0058)	(0.0290)	(0.0138)	(0.0334)
Average	0.00006	-0.0942	0.0375	-0.2295	0.0292	0.0817	-0.00036	-0.0358	-0.0054	0.0040
	(0.0145)	(0.0788)	(0.0692)	(0.4531)	(0.0229)	(0.1029)	(0.0073)	(0.0241)	(0.0072)	(0.0295)
w/10% increase	0.0072	-0.0533	0.0501	-0.0543	0.0401	0.0749	0.0021	-0.0361	-0.0163	-0.0008
	(0.0099)	(0.0792)	(0.0941)	(0.4730)	(0.0341)	(0.0960)	(0.0122)	(0.0303)	(0.0099)	(0.0467)
Number of Person-year Observations	--	--	--	--	90,771	90,771	130,470	130,470	--	--
Number of Children	7,983	7,983	7,983	7,983	8,195	8,195	8,859	8,859	5,140	5,140

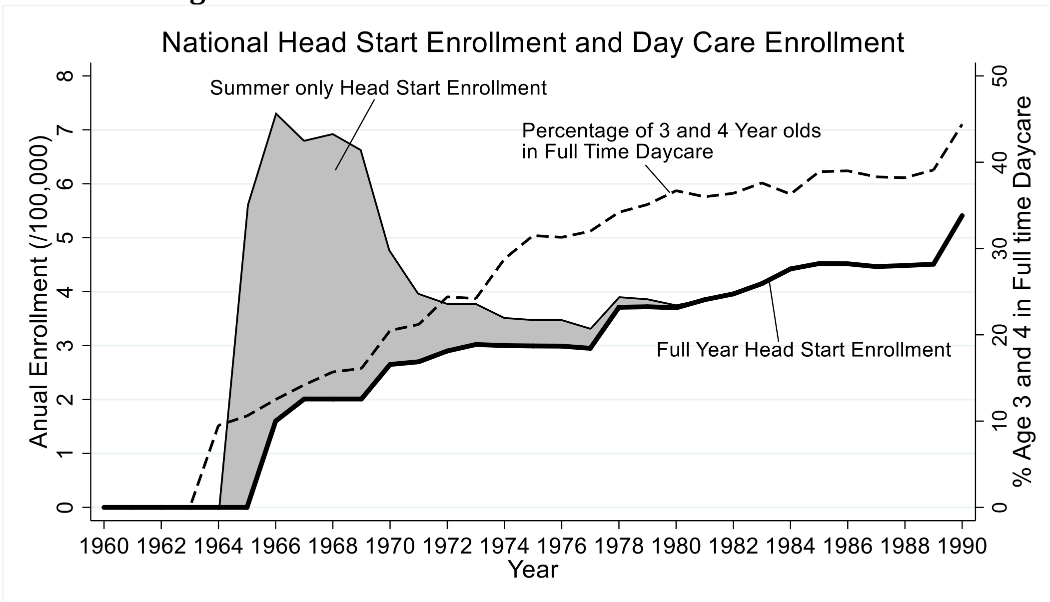
Robust standard errors in parentheses (clustered at childhood state level)

*** p<0.01, ** p<0.05, * p<0.1

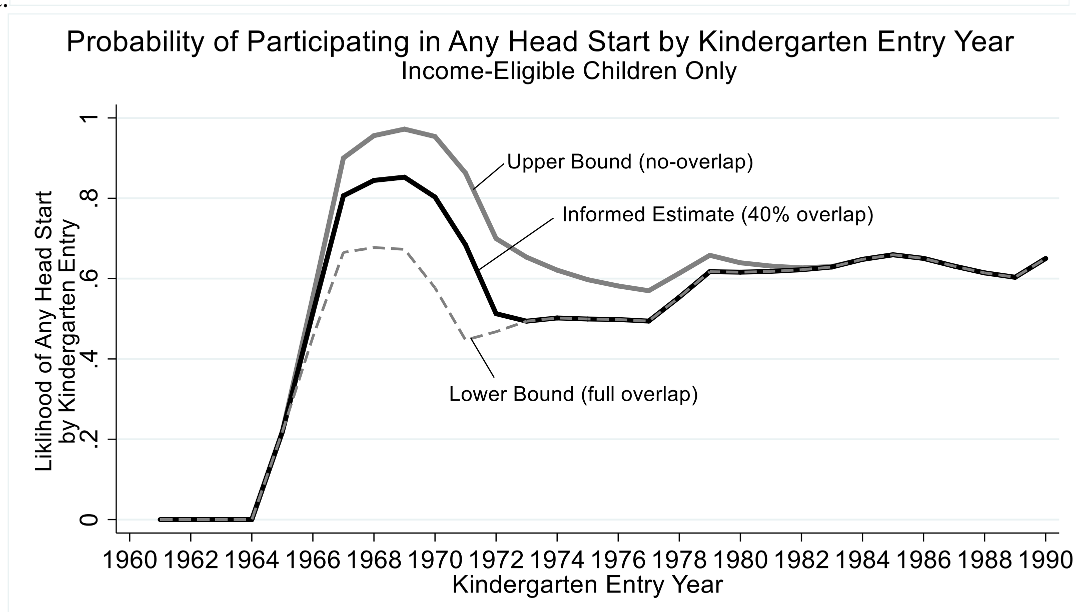
Data: PSID geocode Data (1968-2015), matched with childhood school and neighborhood characteristics. Sample includes all individuals born 1950-1976 whose parents were NOT in the bottom quartile of the income distribution, and who have been followed into adulthood.

Models: (non-Instrumented & Instrumented) Head Start Spending per poor 4-year old at age 4 in the county and instrumented ln(school district per-pupil spending during ages 5-17) are centered around their respective means, to facilitate interpretation of the main effects evaluated at roughly the mean; the average SFR-induced increase in school-age spending is about 10%. Results are based on 2SLS-IV models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific birth year trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race; controls for 1960 county characteristics (poverty rate,% black, education,% urban, population size, each interacted with linear cohort trends; controls for county-level per-capita gov't safety net expenditures average during childhood; and controls for childhood family characteristics (parental income/education, mother's marital status at birth, birth weight, gender), and age (cubic). The first-stage model includes as predictors the school-age years of exposure to school finance reform interacted with the quartile of the respective school district's predicted reform-induced change in school spending based on the timing and type of court-ordered reform interacted with 1970 (within-state) district income and spending percentile categories. The instrument used for Head Start spending per poor 4-year old is an indicator for whether there was any Head Start center in the county at age 4 (based on the program's rollout timing variation only). There exists a significant first-stage. The marginal effects related to Head Start access are based on the average county Head Start spending when there is a center (~$4,230 (in real 2000 dollars))--i.e., marginal effects are evaluated for roughly a $4K increase in Head Start spending (to contrast impact of having access vs no access to Head Start center).

Figure 1: *National Enrollment in Head Start Over Time*



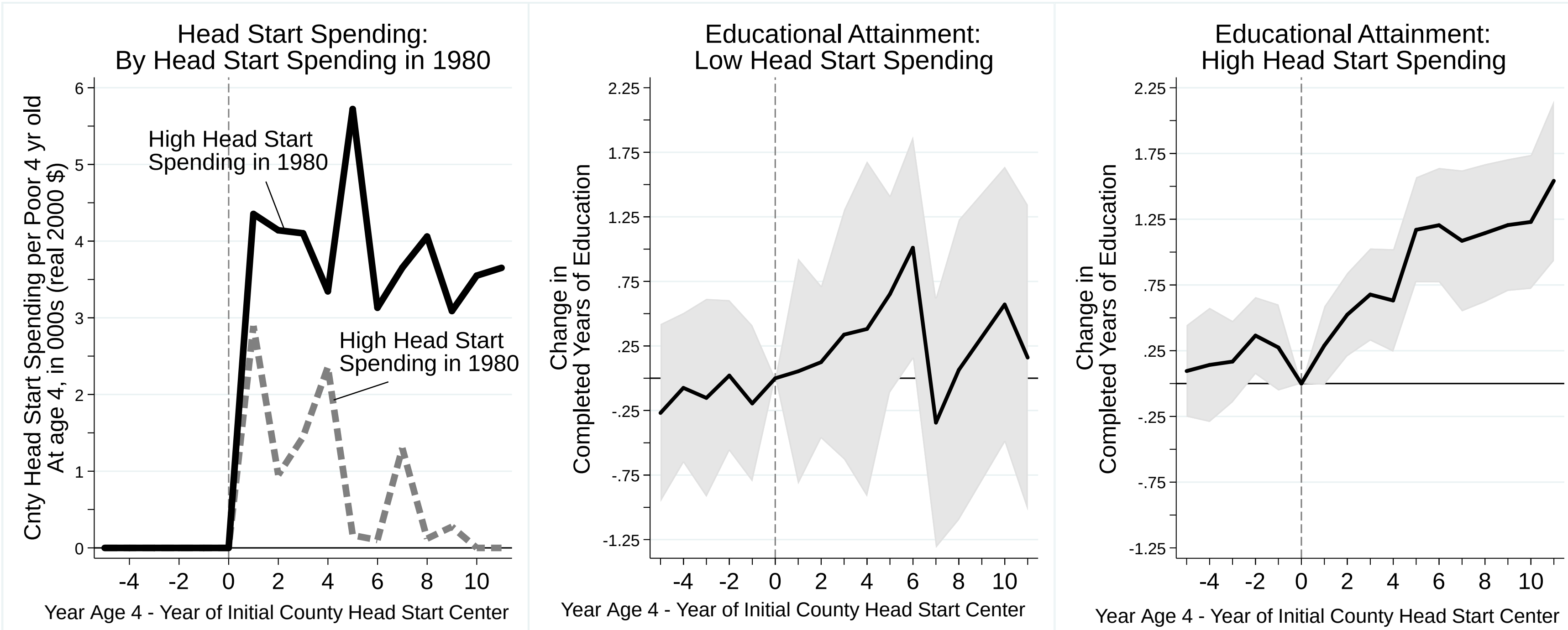
a.



b.

Notes: National counts of 3,4, and 5-year-olds are derived from Integrated Public Use Microdata Series (IPUMS) decennial censuses from 1960 to 1990. Counts for non-census years are completed using linear interpolation. The percentage of children ages 3 and 4 who are enrolled in full-year daycare are as reported in the Current Population Survey (link: http://www.census.gov/hhes/school/data/cps/historical/.) Head Start enrollment figures are from the Head Start fact sheet (link: https://eclkc.ohs.acf.hhs.gov/hslc/data/factsheets/2015-hs-program-factsheet.html). In panel b, the participation rate is the cumulative probability of enrolling in Head Start across all age-eligible years prior to Kindergarten entry (note: this is not the same as the fraction of eligible enrollees in a given year, but is the sum of these annual probabilities across age-eligible years prior to kindergarten entry. The upper bound assumes that each enrollee in full years and summer only programs is unique (no overlap). The lower bound assumes that all full-year enrollees, were possible, were also in a summer program (full overlap). The informed estimates assume that, where possible, 40 percent of the full year enrollees were previously summer enrollees.

Figure 2: *Evolution on Head Start Spending and Educational Attainment at Rollout (Poor Children)*

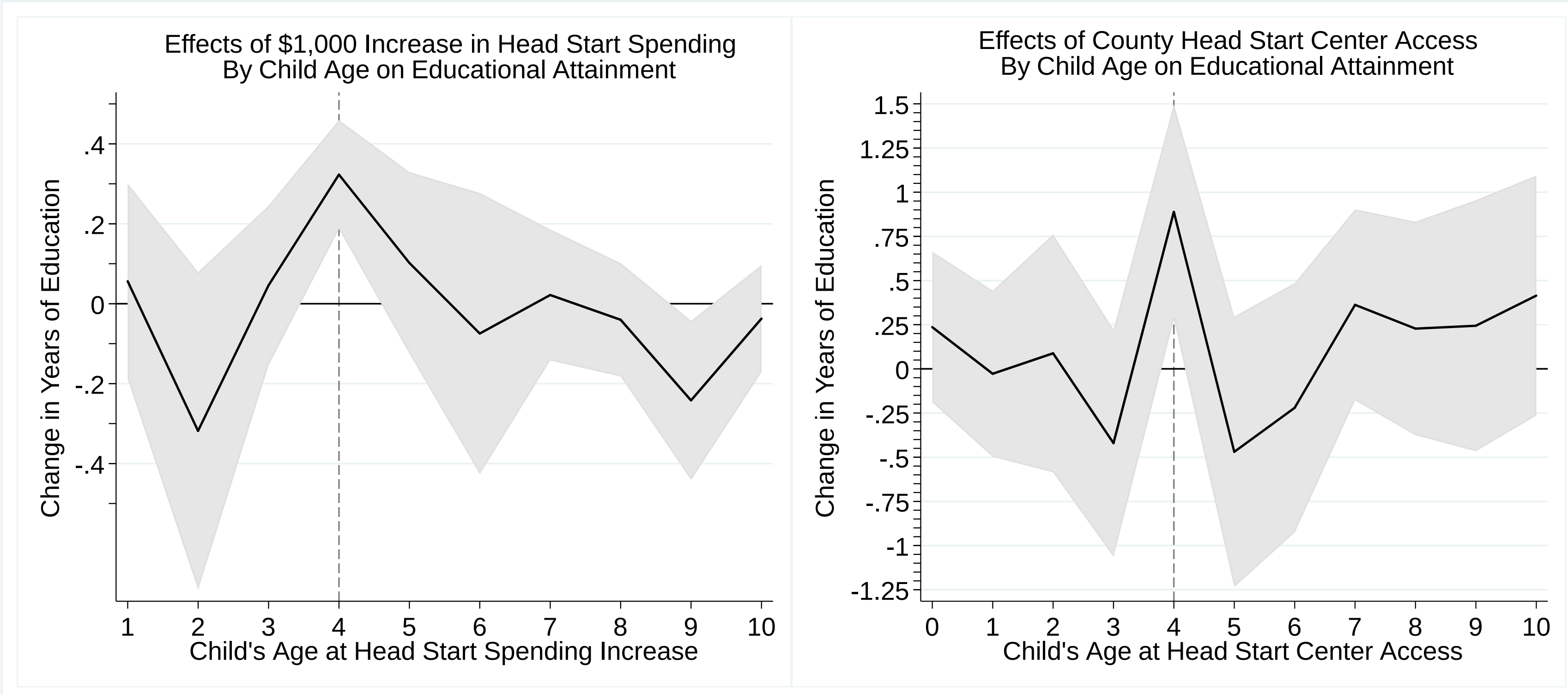


Data: Analysis sample includes PSID individuals born 1950-1976 who have been followed into adulthood. "High Head Start spending" is defined here as counties in the top quartile of Head Start spending among all US counties after rollout; "Low Head Start spending" defined here as bottom quartile of Head Start spending among all US counties after rollout or no spending.

Models: Results are based on event study models of educational attainment on children's exposure to county Head Start spending per poor 4-year-old at age 4 as a function of the timing of the rollout of the program in the county. The figures present the event-study plots for both high and low spending counties (in 1980). The shaded grey region in the event study plots for years of education depict the 90% confidence interval for each event-year. The models include childhood county fixed effects, race*census division-specific birth year trends; controls for 1960 county characteristics (poverty rate,% black, education,% urban, population size) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income & education, mother's marital status at birth, birth weight, gender).

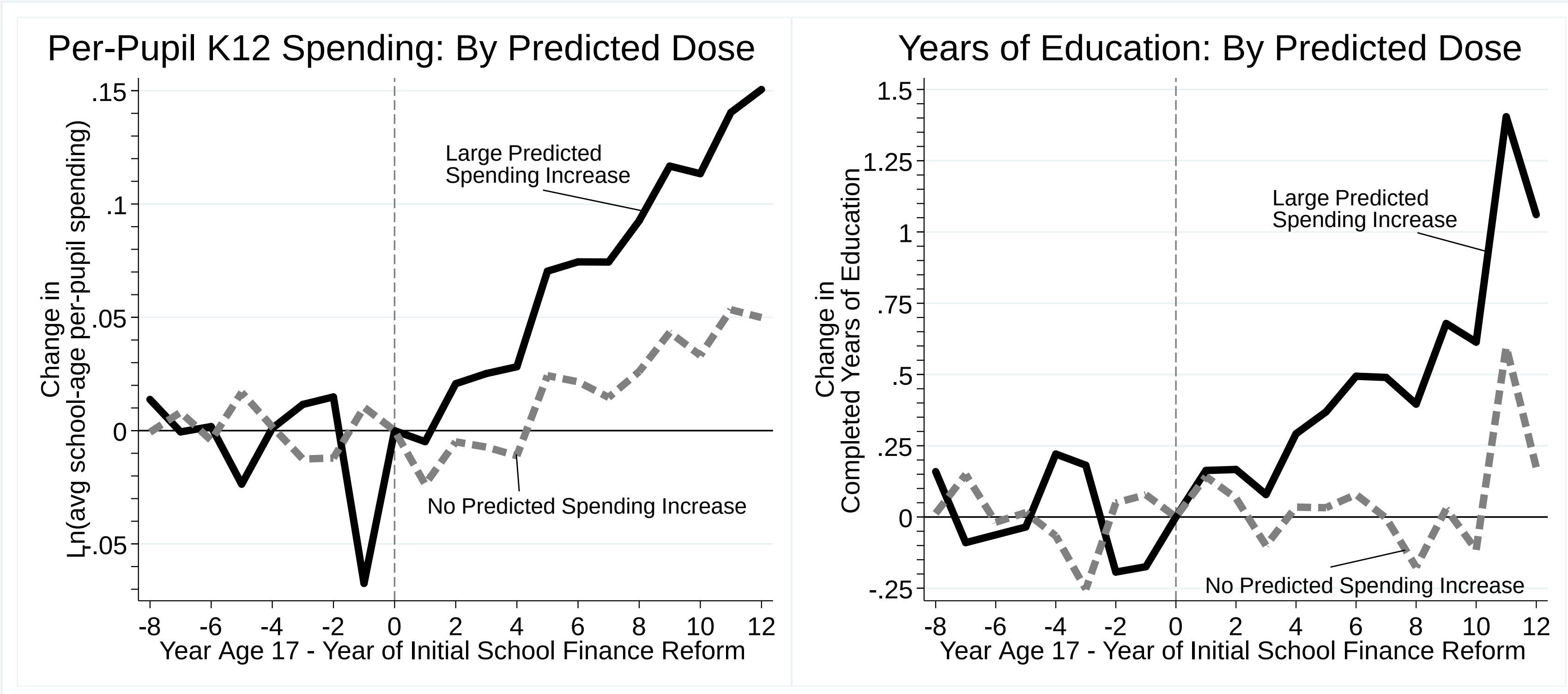
Figure 3:

Effects of Head Start Spending and Access: by Age of Spending and Access (Poor Children)



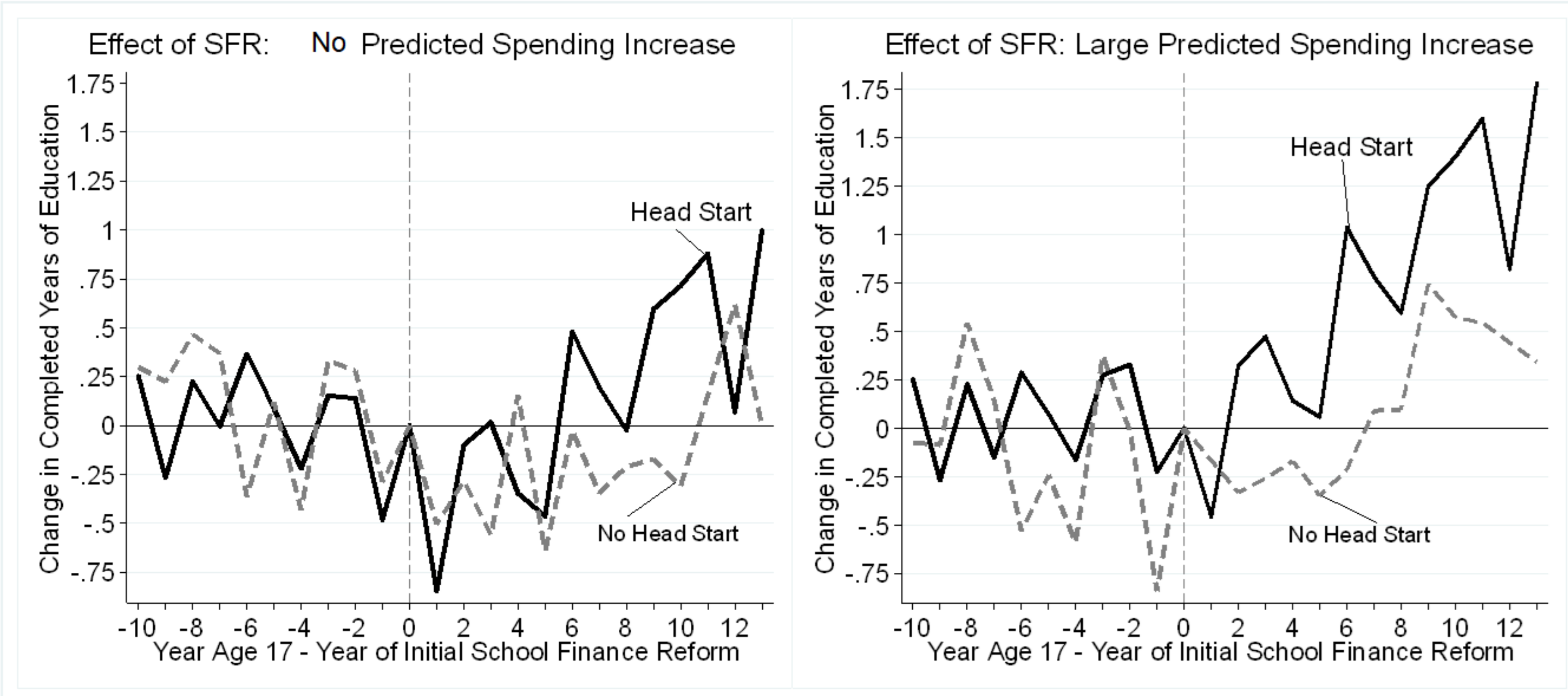
These figures present the marginal effects of Head Start spending in an individual's childhood county at different ages, conditional on the level of Head Start spending in the childhood county at age 4 (when such spending should have an effect). The shaded grey region in the event study plots depict the 90% confidence interval for each rollout age estimate. The sample is poor children only. Models include the full set of controls as in Tables 2 and 3. The coefficients on the non-eligible years 1 through 3 and 5 through 10, are all conditional on spending at age 4. The coefficient for spending at age 4 is based on a model with no other ages included. For the regression estimates underlying this model for years of education attained, see Appendix Table H6.

Figure 4: *Evolution of K12 Spending and Educational Attainment after SFR Reform (All Children)*



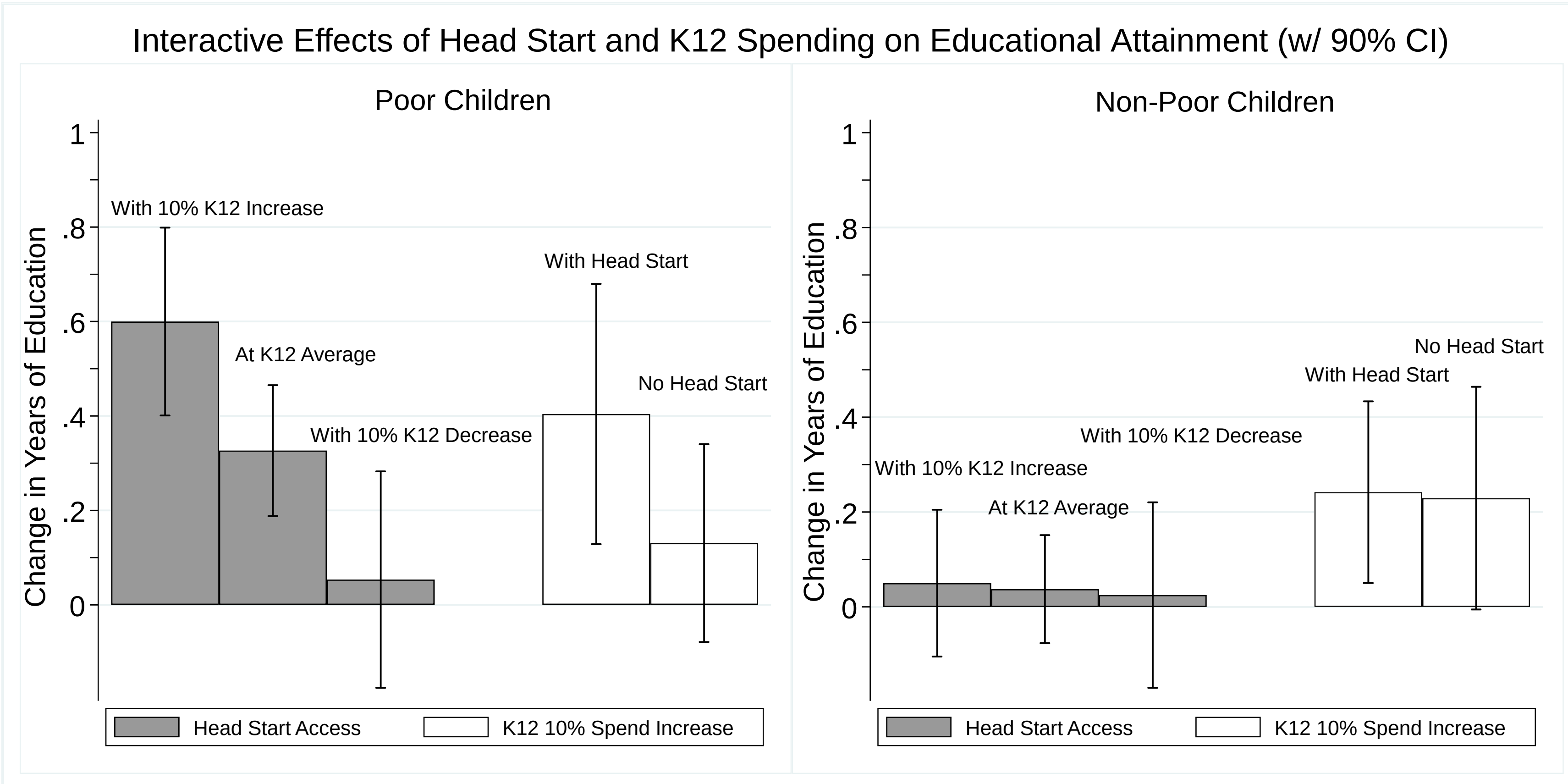
<u>Models:</u> The event study figures use school district's predicted reform-induced change in spending based on the timing and type of court-ordered reform interacted with 1970 (within-state) district income and spending percentile categories—the solid black line shows estimated effects for districts with a predicted reform-induced K12 spending increase ($\widehat{dose}_d > 0$) whereas the solid grey line shows the corresponding effects for districts with low predicted reform-induced K12 spending increases or a decrease $\widehat{dose}_d \leq 0$. Roughly two-thirds of districts in reform states had predicted spending increases. The event study models include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific birth year trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, rollout of "War on Poverty" & related safety-net programs (community health centers, food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years)), timing of state-funded Kindergarten introduction and timing of tax limit policies; controls for 1960 county characteristics (poverty rate,% black, education,% urban, population size) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education, mother's marital status at birth, birth weight, gender).

Figure 5: *Effect of K12 Spending on Year of Completed Education: by Head Start Exposure Status (Poor Children)*



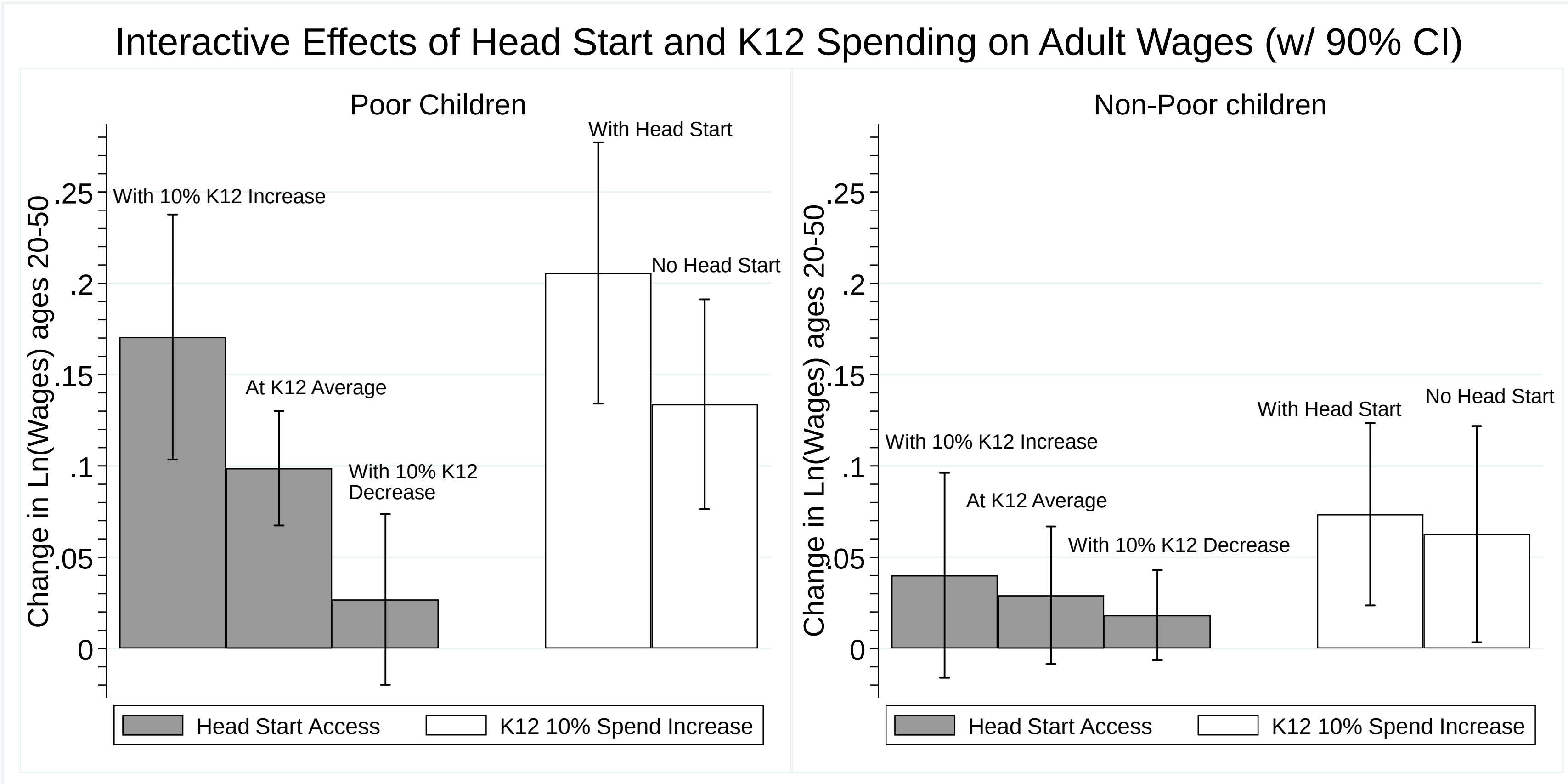
Models: The event study figures use school district's predicted reform-induced change in spending based on the timing and type of court-ordered reform interacted with 1970 (within-state) district income and spending percentile categories--right panel shows estimated effects for districts with a predicted reform-induced K12 spending increase ($\widehat{dose}_d > 0$) whereas the left panel shows the corresponding effects for districts with low predicted reform-induced K12 spending increases or a decrease $\widehat{dose}_d \leq 0$. Roughly two-thirds of districts in reform states had predicted spending increases. These estimated effects are presented both for children whose county had no Head Start center at age 4 (grey line), and those who were exposed to any county Head Start spending at age 4 (black line), to highlight the role of dynamic complementarity. The event study models include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific birth year trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, rollout of "War on Poverty" & related safety-net programs (community health centers, food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years)), timing of state-funded Kindergarten introduction and timing of tax limit policies; controls for 1960 county characteristics (poverty rate,% black, education,% urban, population size) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education, mother's marital status at birth, birth weight, gender).

Figure 6: *Effect of Head Start Spending by K12 spending Levels and vice versa on Educational Attainment*



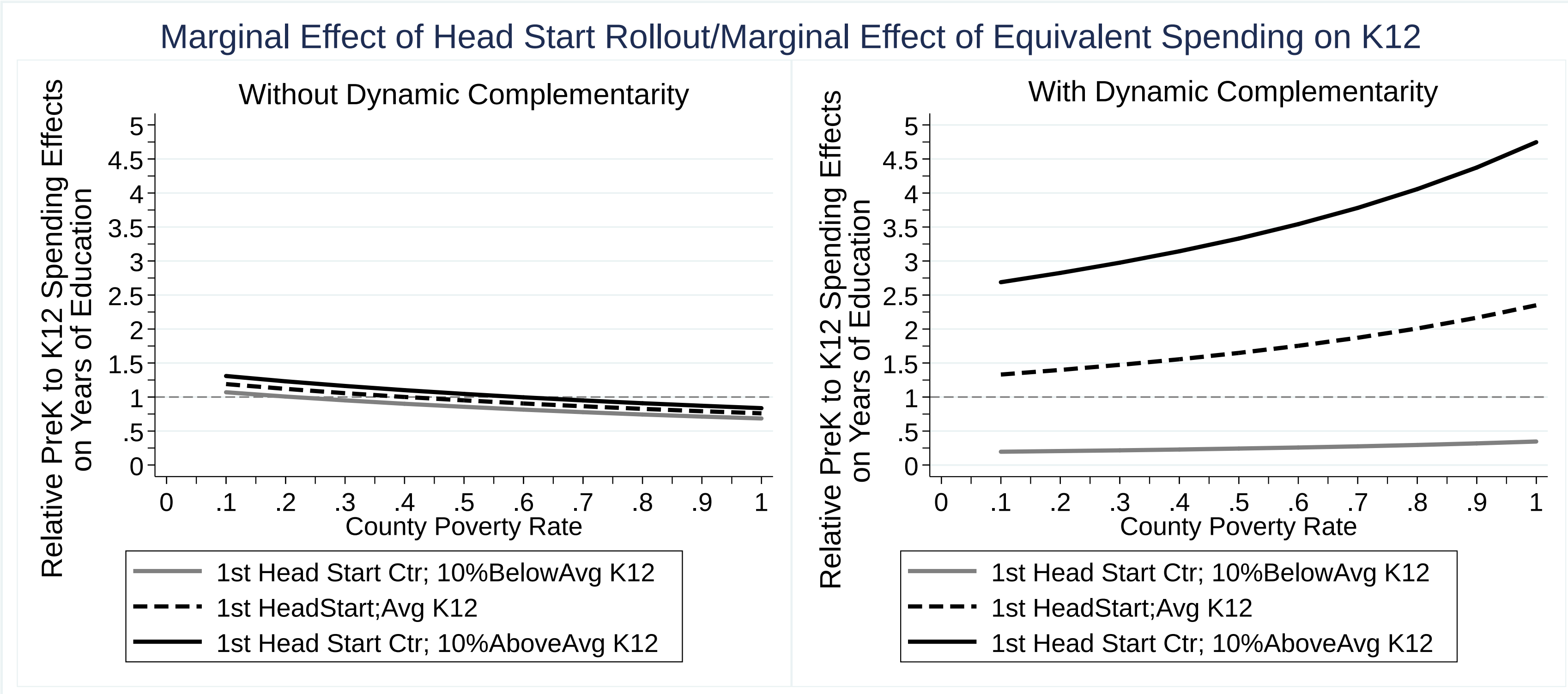
Note: The reported marginal Effects based upon 2SLS-Difference-in-Difference model results are presented in columns 3 from Tables 2 and 3. The reported marginal effects and the standard errors were computed using the delta method.

Figure 7: *Effect of Head Start Spending by K12 spending Levels and vice versa on Wages*



Note: The reported marginal Effects based upon 2SLS-Difference-in-Difference model results are presented in columns 5 from Tables 2 and 3. The reported marginal effects and the standard errors were computed using the delta method.

Figure 8: *Ratio between the Effect of Head Start Spending and K12 spending Levels by Poverty Level in the County*



Note: The reported marginal Effects based upon 2SLS-Difference-in-Difference model results presented in columns 2 and 3 from Tables 2 and 3. The solid grey lines plot the ratio between the marginal effect of spending on Head Start and the effect of spending that same amount on the K12 system (in present value-terms). This ratio presented in the solid grey line is evaluated at average levels of Head Start spending and K12 spending during the sample period. The dashed grey line presents this same ratio evaluated at $1000 above the average K12 spending levels assuming no dynamic complementarity, while the solid black line presents this ratio evaluated at $1000 above the average K12 spending levels using the estimated interaction effects. The difference between the solid black lines and the dashed grey lines reflect the marginal contribution of dynamic complementarity to changes in this ratio as one increases K12 spending above the average.