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EdWorkingPaper No. 20-201

Timing in Early Childhood Education: How Cognitive and Achievement Program Impacts Vary by Starting Age, Program Duration, and Time Since the End of the Program

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VERSION: February 2020

Suggested citation: Li, Weilin, Greg J. Duncan, Katherine Magnuson, Holly S. Schindler, Hirokazu Yoshikawa, and Jimmy Leak. (2020). Timing in Early Childhood Education: How Cognitive and Achievement Program Impacts Vary by Starting Age, Program Duration, and Time Since the End of the Program. (EdWorkingPaper: 20-201). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/5tvg-nt21

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February 2, 2020

Acknowledgment: We are grateful to the following funders of the National Forum on Early Childhood Policy and Programs: the Birth to Five Policy Alliance, the Buffett Early Childhood Fund, Casey Family Programs, the McCormick Tribune Foundation, the Norlien Foundation, Harvard University, and an Anonymous Donor. We are also grateful to the Institute of Education Sciences [#R305A110035] and the Eunice Shriver Institute for Child Health and Human Development [5R01HD073172-04] for supporting this research, to Abt Associates, Inc. and the National Institute for Early Education Research for making their data available to us, and to Larry Schweinhart for helpful comments.

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Abstract

This paper uses meta-analytic techniques to estimate the separate effects of the starting age, program duration, and persistence of impacts of early childhood education programs on children's cognitive and achievement outcomes. It concentrates on studies published before the wide scale penetration of state-pre-K programs. Specifically, data are drawn from 67 high-quality evaluation studies conducted between 1960 and 2007, which provide 993 effect sizes for analyses. When weighted for differential precision, effect sizes averaged .26 sd at the end of these programs. We find larger effect sizes for programs starting in infancy/toddlerhood than in the preschool years and, surprisingly, smaller average effect sizes at the end of longer as opposed to shorter programs. Our findings suggest that, on average, impacts decline geometrically following program completion, losing nearly half of their size within one year after the end of treatment. Taken together, these findings reflect a moderate level of effectiveness across a wide range of center-based programs and underscore the need for innovative intervention strategies to produce larger and more persistent impacts.

Keywords: meta-analysis; early childhood education; program impact; timing.

Timing in Early Childhood Education: How Cognitive and Achievement Program Impacts Vary by Starting Age, Program Duration, and Time Since the End of the Program

Gaps between more and less advantaged children in academic-related skills emerge early in life and increase substantially by the time children enter school (Duncan & Magnuson, 2011). This well-documented concern has led policymakers and practitioners to focus on programs that hold the promise of improving children's school readiness and preventing the emergence of achievement gaps before children enter formal schooling. Early childhood education (ECE) has been an important part of these efforts because of its demonstrated effectiveness in improving disadvantaged children's foundation for learning by boosting children's early skills.

Despite several decades of ECE program evaluation research, many questions about how to maximize its effectiveness in early learning remain unanswered. Two of the most important questions – whether programs produce better outcomes if they start earlier in life or last for longer periods of time – have not been adequately addressed by prior studies. Consequently, policymakers and administrators continue to make difficult decisions in the absence of good research findings. For example, they must decide about whether it is better to allocate limited resources to support ECE programs that serve larger numbers of children for a shorter period of time or to invest in longer-lasting programs (or perhaps a sequence of programs) that serve a smaller number of children. Questions about the extent to which early program impacts persist beyond the end of the programs are also important for assessing the ability of ECE to reduce later achievement gaps as well as to build foundational skills to be able to profit from subsequent K-12 curricula.

Although a wide variety of ECE programs have been evaluated in the last 50 years, scholarly discussions about early education have been dominated by individual evaluations of a small number of ECE program models, most prominently Perry Preschool and the Abecedarian Project. Most efforts to summarize this literature consist of narrative reviews that attempt to uncover commonalities among programs with large positive effects (Barnett, 1995). Such an approach privileges the best-known studies, and overlooks what can be learned from a comprehensive and systematic analysis of all existing evaluation research. In this study, we employ meta-analytic techniques to examine whether age of entry and program duration are associated with ECE program effectiveness in a large group of rigorously evaluated programs. We also estimate the extent to which ECE program impacts persist beyond program completion, and whether several program factors (such as duration) increase the persistence of program impacts after they end. Throughout, we concentrate studies published before the wide scale penetration of state-pre-K programs, although provide a brief review of those programs in our discussion section.

Background

Developmental research and theory point to early childhood as an important and sensitive period in which experiences and environmental influences interact with genetic predispositions to foster the acquisition of cognitive skills and later academic achievement as well as other capacities (Shonkoff & Phillips, 2000). Children's healthy development and skill growth is best promoted by environments that provide sensitive, responsive caregiving and a variety of learning opportunities that are rich in language and tailored to a child's capabilities and needs. It is not just that more information is learned in enriching environments; both human and animal studies in neurobiology suggest that experiences in the earliest years affect the brain architecture and

neurocognitive functions that will shape future cognitive, social, and emotional development, as well as physical and mental health (Knudsen, Heckman, Cameron, & Shonkoff, 2006; Sapolsky, 2004).

Age of Entry and Program Duration

Cognitive and language abilities are described as highly plastic neuro-cognitive skills because they are strongly shaped by environmental experiences during the early childhood period (Shonkoff et al., 2016). Thus, it is hardly surprising that ECE programs often improve scores on developmental tests that measure a variety of language, achievement and cognitive skills (Camilli et al., 2010). The relative plasticity of skill development during differing time periods *within* the first five years, however, is not well articulated. Starting in infancy, responsive caregiving and language-rich interactions are associated with greater success in reaching developmental milestones generally, as well as stronger early language development more specifically (Tamis-LeMonda, Bornstein, & Baumwell, 2001). Based on these findings, if center-based experiences for infants and toddlers created such environments, we would expect to find an "earlier is better" pattern of effects.

Starting ECE programs as early in life as possible is also consistent with the idea of cumulative advantage, in which children who enter preschool or kindergarten with better skills are likely to continue to outperforming other students because they are given more learning opportunities by the educational environments they experience than are other children. This was termed the "Matthew effect" by education and psychology researchers (Stanovich, 1986; Walberg & Tsui, 1983). This concept has become central to the arguments of Cunha and Heckman's (2007) on the importance of early skill development. They describe a model of

learning in which early efforts to boost skills in vulnerable young children can improve the productivity of later schooling and enrichment.

Because most ECE programs start at a specific age, and do not systematically and intentionally enroll children at differing ages, estimating how program impacts vary by age of entry is complicated. To do so, requires comparisons either across programs that vary along age-of-entry or from large population studies in which the preschool experiences are not well defined. Such comparisons might not be as challenging if the pattern of findings suggested a clear answer—but they do not. Looking across the most prominent ECE programs in the first three years of life one can find relatively small effects on cognitive development (e.g., Love et al., 2002) as well as much larger impacts (Duncan & Sojourner, 2013). Studies that look across a range of age of entry suggests a more complicated non-linear association between age of entry and outcomes. For example, Loeb and colleagues found the largest positive effects of center-based care for children who first experienced the setting at ages 2-3 compared with those who started earlier or later (Loeb, Bridges, Bassok, Fuller, & Rumberger, 2007).

One possible explanation for these differences is that not all center-based ECE programs are the same, and how children in these programs experience these setting may depend on their ages. Although some ECE settings for infants and toddlers are able to ensure that all children get individual attention from caregivers and rich language interactions with adults, other ECE environments may not be sufficiently enriching or interactive to promote the development of all the young children in the room. Although it might be easier to structure center and classroom experiences to stimulate learning for four-year-olds, such experiences may come too late to build some foundational skills and neurodevelopmental structures. Finally, starting at a later age

necessitates a shorter period of time being exposed to the enriching learning setting prior to entry into K-12 schooling.

Age of entry into an early childhood education setting is correlated with, but conceptually distinct from, program duration. In general, children who enter at a young age tend to experience more cumulative time in these settings. However, program duration also differs based on program design. For example, some programs may set age requirements in such a way that children only attend for one or two years, as is the case with many school-based prekindergarten programs or center-based Early Head Start programs. Only a very small number of ECE program models span infancy through the preschool years with the specific goal of providing an ECE experience based on a coherent and articulated model of early education and development across this period.

Abundant literature suggests that the number of years spent in K-12 or post-secondary education is linked to labor market success, with additional years bringing greater skills and higher earnings (Card, 1999). Given the cumulative nature of learning, it is reasonable to expect that greater time spent in developmentally stimulating environments before formal school entry should also yield higher levels of academic skills. However, this conclusion is likely to depend on the extent to which ECE programs are able to articulate and implement learning activities that differentiate instruction and activities as they develop over time based on children's mastery of skills and content.

Developmentally-informed activity sequences likely vary as a function of the explicit and implicit curriculum. For example, a preschool curriculum that does not adapt to children's developmental progress and prior classroom experience could be redundant. If children continue to experience more of the same activities rather than increasingly complex, differentiated

learning experiences, they may gain much less from a second or third year in the same program than they did in the first year. Existing skills may be solidified, but few new skills may be learned. On the other hand, if learning is not primarily fostered through the explicit curriculum and is instead dependent on implicit child directed learning and scaffolded interactions with teachers (rather than say intentional instruction on specified topics), then such individualized approaches to learning may maximize children's learning throughout the time they are in center-based ECE settings.

When researchers have considered how length of time within an ECE program relates to academic related outcomes the results have been mixed. If the a priori expectation is that attending a program for twice as long should result in twice the impact, the results tend to be disappointing. The Chicago Parent Child (CPC) early education program found that two program years boosted children's school readiness more than one program year as measured by skills in kindergarten. However, the early benefits for attending two years faded during the remainder of elementary school such that years of attendance did not predict program outcomes. Reynolds and colleagues concluded "children reach a threshold of performance at 1 year beyond which there are diminishing returns over time" (Reynolds, 1995, p. 22).

Schweinhart and Weikart (1988) and Sprigle and Schaefer (1985) reached similar conclusions in their analyses of ECE program impacts. Given the additional costs of another year of preschool, Reynolds (1995) suggested that policymakers would be better off allocating funds to assure that all children are able to attend ECE for one year rather than supporting a smaller number of children for two years.

These findings contrast with the conclusions of several recent studies that find predominantly larger effects for two years of ECE program attendance compared with one year,

which typically corresponded to attending at both ages three and four compared with just at age four. A longer-run follow-up of CPC found that two years of program participation was associated with some scattered positive effects on school performance (higher test scores and lower grade retention); criminal behavior (fewer juvenile petitions), but not educational or occupational attainment (Arteaga et al., 2014). A non-experimental analysis of the Abbot Preschool program in New Jersey produced mixed results on this issue, finding that an additional year of early education significantly improved children's vocabulary but did not yield larger gains in their print awareness or math skills (Barnett & Lamy, 2006). Wen and colleagues found that two years of Head Start were more effective than just one year in terms of producing gains in academic and language outcomes (Leow, Wen, & Kormacher, 2014; Wen, Leow, Hans-Vaughn, & Marcus, 2012; see also Domtrovich and colleagues 2013). At the same time, however, the analysis of Jenkins et al. (2016) found that children who entered Head Start centers as three year-olds showed more cognitive growth if they transitioned to pre-K centers at age four than if they remained in the Head Start program.

Using meta-analysis to examine patterns of findings across a larger set of program models, Gorey (2001) found that programs with durations in excess of 3 years had larger effects on cognitive and achievement outcomes than 1- or 2-year-long interventions (Gorey, 2001). It is worth noting, however, that the difference in effects was not proportional, as three-year effects were only 9% larger than one- or two-year effects. A more recent meta-analysis found no significant effect of treatment duration on program impacts on achievement and cognitive outcomes (Camilli et al., 2010).

Based on the presumption that greater exposure to enriched early learning environments would produce larger effects, we hypothesize that, with starting age held constant, multi-year

programs will produce significantly larger end-of-treatment impacts than single-year programs. At the same time, the absence of developmentally sequenced curricula in many preschool settings reduces our confidence that past programs will show this hypothesized patterns of impacts.

Persistence of ECE Effects

The declining magnitude of early education program impacts on achievement and cognitive outcome as children progress through elementary school, sometimes referred to as fadeout, convergence or catch-up, has raised questions about whether ECE is a worthy investment (Bailey et al., 2017; Abenavoli, 2019). Intervention programs may simply accelerate mastery of content that would have occurred anyway, with later similarities between groups attributed to the presumption that children in the control group simply "catch-up" after the program is terminated (Ackerman, 2007). Others argue that the fade-out of impacts is not inevitable but reflects the fact that preschool is not an "inoculation" against the adverse effects of subsequent low-quality schooling (Currie & Thomas 2000; Lee & Loeb, 1995).

Many studies of individual ECE programs show sizable program impacts on children's academic skills at program completion, but then document shrinking differences between treatment and control groups at later time points. In the case of the Infant Health and Development Program (which included an intensive center-based ECE component between ages 1 and 3), large cognitive impacts at age 3 had mostly disappeared by age 5 (Duncan and Sojourner, 2013). More recently, the Head Start Impact Study, which conducted an investigation of nationally representative centers, demonstrated modest effects on children's pre-academic outcomes after one year of program participation, but Head Start children's advantage in academic skills had disappeared entirely by the spring of first grade (Puma, et al., 2010). In the

first random assignment evaluation of a state-wide pre-K program, Lipsey et al. (2018) found null or even negative achievement impacts in third grade. Despite differences in the rates of home-based care among control-group children, Weiland et al. (2019) found similar longer-run patterns in a lottery-based evaluation of the Boston pre-K program.

This pattern of disappearing impacts over time is not universal. Although the CPC program impacts on achievement declined after program completion, a significant impact attributed to preschool and school year participation was still found in seventh grade for math and reading skills (Reynolds & Temple, 1998; Reynold 2000). Likewise, the Abecedarian program evaluation results have persisting impacts on several domains of achievement and cognitive (IQ) outcomes in young adulthood (Campbell et al., 2002).

Given the sizable number of studies that have collected medium- and long-term follow-up data, the question of persistence is particularly well-suited to meta-analytic study. Camilli et al. (2010) conducted a meta-analysis of ECE interventions for children ages 3-5 that measured cognitive domains and compared an ECE treatment to a no-treatment control group. Their average overall effect size was .23 sd, but they also estimated a -.24 sd drop in effect size between the time children were between 3 to 5 years old and when they were older than 10 years of age at the time of measurement. In a study of 33 programs, Aos and colleagues (2004) found that each additional year post-program completion was associated with a -.03 sd decline in test scores, holding constant program and research study design quality. Further calculations led to an estimate that by the end of high school, a “real-world” ECE program with an initial impact of .12 sd, would increase test scores by only .08 sd.

The presence and degree of ECE program impact “fadeout” for children’s cognitive and achievement skills may depend on the specific cognitive domain or types of outcomes

assessments under consideration. For example, if preschool instruction may concentrate on narrower set of defined knowledge for which impacts may be easier to demonstrate on specific assessment in the short run (e.g. identifying letters or numbers). Yet, the defined knowledge of print conventions or the alphabet most children master in the early school years because it is typically the target of explicit instruction. Broader skills, including those that are unconstrained such as a reasoning, vocabulary, and background knowledge, are not easily measured by brief assessments. However, because developing unconstrained knowledge and skills requires high quality and varied learning experiences, which may require a higher "dose" of program exposure for generating both initial and sustained impact (Bailey et al., 2017). This might argue for ECE impacts on unconstrained skills, lasting longer than impacts on specific areas of achievement, which might be more fleeting.

IQ, which includes a mix of skills across the range of cognitive development, was historically one of the most often measured outcomes of early childhood education (Zigler & Trickett, 1978). Zigler attributed IQ's popularity as an outcome to the fact that several IQ scales had good measurement properties, were strongly predictive of later schooling outcomes, and measured a broad range of cognitive skills that might be covered by early childhood education programs. Yet, he cautioned that established measures of IQ blend three types of cognitive skills: formal cognitive skills (e.g. reasoning, processing speed), achievement skills, and motivation for test performance. Thus, as a measure that combines a range of skills, it is hard to know what to make of declining or persisting program impacts on IQ. This is further complicated by evidence of rapid decline in size of positive impacts even in high quality programs, as well as variation in the extent to which long-term impacts on IQ persist in later years. For example, the large IQ impacts measured shortly after the completion of the Perry Preschool Project had completely

disappeared by age 8, although effects on academic achievement persisted into middle adulthood (Schweinhart et al., 1993). Abecedarian generated IQ impacts that persisted well beyond age 8, despite declines in the size of these effects in the first two years post-program (Campbell, Pungello, Miller-Johnson, Burchinal, & Ramey, 2001; Hojman, 2015). In short, evidence from prior evaluations does not clearly provide suggestion about how the type of outcome being assessed might be associated with patterns of impact persistence.

The current study

This paper address three questions related to the timing of ECE programs: (1) Do programs targeting younger children produce larger impacts than those targeting older children; (2) Does the duration of a program affect the magnitude of impacts; and (3) How persistent are ECE program impacts over time? Impact persistence was also addressed in a more differentiated manner by asking whether program impacts last longer for achievement compared with cognitive outcomes, longer-duration programs, and earlier-starting programs. Specifically, this paper aims to test the following hypotheses:

Hypothesis 1: ECE programs targeting younger children will produce larger end-of-treatment impacts than programs targeting older children.

Hypothesis 2: Longer-duration ECE programs will produce larger end-of-treatment impacts than shorter-duration programs.

Hypothesis 3: ECE program effects will decrease as the time since the end of treatment increases.

Hypothesis 4: ECE program effects on outcomes that are more sensitive to instruction will fade out more quickly than effects on outcomes that are less sensitive to instruction.

Hypothesis 5: Impacts from programs targeting older children will fade out more quickly than impacts from programs targeting younger children.

Hypothesis 6: Impacts from programs with shorter durations will fade out more quickly than impacts from programs with longer durations.

In order to test these hypotheses, we analyzed data in a meta-analytic database that includes 273 rigorously conducted experimental and quasi-experimental studies since 1960. Although a subset of these studies was included in the Camilli et al. (2010) analysis, we added eight studies published between 2000 and 2007 (including the National Head Start impact study and six evaluations of pre-K programs) as well as 44 studies published or released during the Camilli review period but not included in the Camilli et al. (2010). Finally, our quality thresholds led us to exclude 94 studies included in Camilli et al. (2010).

Method

Data

We focused exclusively on early childhood education (ECE) programs, defined as structured, center-based early childhood education classes, day care with some educational component, or center-based child care. These include widely implemented pre-school programs such as Head Start and other demonstration interventions conducted by researchers. Programs included in our study must have provided educational services to children at program sites and may also have provided services to families and/or staff.

A multi-step data collection and evaluation process was used for determining what studies would be included in our database (Figure 1). The first step was to conduct a comprehensive search of the literature from 1960 to 2007. The 2007 date marked the year in which we engaged in our systematic review of the thousands of documents that met our search

criteria (see below). Coding these documents took several years and our grant funding enabled us to finish the coding tasks but not to update our document search for the literature published after 2007. We highlight some of the most noteworthy contributions to the ECE literature in our discussion section.

To generate our EC database, we conducted keyword searches (Appendix 1) in ERIC, PsycINFO, EconLit, and Dissertation Abstracts databases, resulting in 9,617 documents, as any given program may produce a series of such documents. The team then manually searched the websites of policy institutes (e.g., RAND, Mathematica, NIEER) and state and federal departments (e.g., U.S. Department of Health and Human Services), as well as references mentioned in collected studies and other key early childhood education reviews. This search produced another 692 documents. In sum, 10,309 documents for potential inclusion in the ECE portion of the database were identified.

[INSERT FIGURE 1 HERE]

The research team next developed criteria for the inclusion of studies into our meta-analytic database. The vast majority (91%) of the 10,313 documents that were found were excluded because they violated as least one inclusion criteria. Most of the excluded documents were not actual research studies but were commentaries or reviews. In addition to being an ECE intervention or program reported on from 1960 to 2007, studies had to have a treatment and control/comparison group, and not simply assess the growth of one group of children over time. Each of the estimates in the study must have been based on groups that included at least 10 participants and incurred less than 50% attrition. Studies were excluded if they assessed children with medical disorders or learning disabilities, or interventions took place outside of the United

States. Our resulting overall database includes 277 studies of programs or interventions for children starting between birth and age 5.

About one-third of the studies used random assignment with the remainder following quasi-experimental designs such as change models, individual or family fixed effects models, regression discontinuity, difference in difference, propensity score matching, interrupted time series, instrumental variables, or some other types of matching. Studies that used quasi-experimental designs were also included if they had pre- and post-test information on the outcome or established baseline equivalence of groups on demographic characteristics determined by a joint test. A general goal of our meta-analysis project was to use more rigorous inclusion criteria than previous meta-analyses and to assure that the quality of included quasi-experimental studies be as close as possible to approximating random assignment. To that end, a virtue of the systematic approach a formal meta-analysis affords is that it enables researchers to test for whether effect sizes and relationships differ systematically by inclusion criteria such as having a random assignment design.

Coding the studies. A team of nine graduate research assistants at three universities, under the guidance of the senior authors, were trained as coders during a 3- to 6-month process that included instruction in evaluation methods, using the coding protocol, and computing effect sizes. Trainees were paired with experienced coders in multiple rounds of practice coding. Before coding independently, research assistants also passed a reliability test comprised of randomly selected codes from a randomly selected study. In order to pass the reliability test, research assistants had to calculate 100% of the effect sizes correctly and achieve 80% agreement with a master coder for the remaining codes. In instances when research assistants were just under the threshold for effect sizes, but were reliable on the remaining codes, they

underwent additional effect size training before coding independently and were subject to periodic checks during their transition. Questions about coding were resolved in weekly research team conference calls usually involving all four principal investigators, and decisions were kept in an annotated codebook so that decisions about ambiguities could be recalled when coding subsequent studies. In a few instances, codes were added and previously coded studies were adjusted accordingly to account for the new additions.

Hierarchical structure of the database. The database consists of three levels of data: study, contrast, and effect size. *Studies* are defined as independent investigations of ECE programs. *Contrasts* are group comparisons within study (e.g. Head Start vs. non-Head Start, literacy intervention vs. no literacy intervention). Each contrast can have multiple dependent measures collected at different time points during, at, and after program completion. Those dependent measures are recorded in the database as *effect sizes*, or standardized comparisons of treatment and control groups on a set of outcome measures which, in the full meta-analytic data set, include cognition, achievement, behavior, socio-emotional skills, health, and other domains.

The current paper focuses on a subset of our overall database. First, we exclude sub-contrasts, which consist of subgroups such as male vs. female or black vs. white children (as we lack the statistical power to examine such subgroups). Second, we focus exclusively on effect sizes for cognitive and achievement outcomes. All in all, these restrictions reduced the number of effect sizes in our analysis from 15,804 to 1,045, which are drawn from 67 studies in the database.

Effect size computation. Each study's outcome measures were coded into standardized mean difference effect sizes, which were computed using the Comprehensive Meta-Analysis computer software program (CMA; Borenstein, Hedges, Higgins, & Rothstein, 2005). We used

the Hedges' *g*-based definition of effect sizes, which adjusts standardized mean differences (Cohen's d) to account for bias arising from small sample sizes. We included how we calculated effect size estimates in Appendix Table 19. To avoid potential domination by outliers, we truncated the 9 effect size estimates with values above 1.5 sd, although in practice this truncation had little effect on our key results. The most negative effect size was -0.70 sd.

Measures

Outcomes. Outcome measures for this analysis included assessments of child cognition and achievement. Cognitive outcomes in our categorization, following Christian, Morrison, Frazier, and Massetti (2000), are considered less sensitive to general preschool instruction than achievement outcomes. These include IQ, attention, vocabulary, task persistence, theory of mind, and syllabic segmentation such as elision and rhyming. Achievement measures tend to be readily amenable to instruction, narrower in scope and more completely measurable. Letter recognition is the most obvious example, but this category also includes reading, math, numeracy other than conservation of number, and other achievement tests.

Program timing. The key independent variables of interest for our analyses are three measures of timing: starting age, length of program, and elapsed time since the end of the program. Starting age is the age of the child, scaled in years, at the beginning of the intervention/program. Program length is the amount of time, scaled in years, over which services are offered to participating children. The persistence of program impacts are estimated with the length of time, scaled in years, between the end of the program and when the given effect size impact measurement took place.

Analytic approach

Random-effects modeling. Since effect size estimates are nested within contrasts, which in turn are nested within studies, we accounted for the potential lack of independence across effect size estimates by implementing a set of 3-level random-effect meta-analytic models. Our models were based on assumptions of (1) studies included in this analysis were a random selection from a larger population of studies, (2) true effect sizes in the population of studies are normally distributed, (3) effect size estimates from different contrasts are independent (i.e. uncorrelated), (4) effect size estimates from different studies are independent, and (5) effect size estimates from different contrasts within the same study are dependent with a compound symmetric variance-covariance structure. We used the rma.mv() method of the <metaphor> package in R to implement this set of models (Viechtbauer, 2010).

Weights. Following best meta-analytic best practices, we weighted our random-effect models by the precision of the effect size estimates. Specifically, the precision of effect sizes is the inverse of the squared standard error of the effect size estimates that were generated by the CMA program.

Control variables. Following Shager et al. (2010), we included a measure of whether the study was conducted before or after 1980, whether the study was for research demonstration purpose, whether the measured outcome was not measured by a performance test (two categories, one representing observational measure and the second indicating all of other methods, whether the reported estimates were based on children actually taking up the treatment (treatment-on-the-treated) as opposed to just being offered the treatment.

In addition, we considered a number of variables of study quality, all of which were scaled so that higher scores represented higher-quality research practices. These included: (1) the

reported reliability of the given outcome measure, as indicated by whether the alpha, was higher than .88, (2) the study was random assignment (vs. quasi-experimental) design, (3) the study controlled for baseline measures, (4) the study reported insignificant differences in groups at baseline, (5) data collectors were blinded, (6) the comparison group did not receive equivalent alternative services, (7) the study had attrition rate lower than the sample median rate of 16%, which was calculated from the original sample size at assignment and the sample size at measurement; (8) the study was published in a peer reviewed journal; and (9) bias was not observed in the study. We then summed these nine dichotomous quality indicators into a quality index.

Imputation. Studies included in our analysis had missing values in some covariates. We conducted multiple imputation to address this missing data issue. Specifically, we used the predictive mean matching method (PMM; Heitjan and Little 1991; Schenker and Taylor 1996) to generate 40 imputed datasets. The PMM is a semi-parametric imputation method that imputes observed values whose predicted values are the closest to the missing observation's predicted values from the simulated models. Because the PMM imputation values are restricted to the observed values and free from the structural imputation models, the PMM provides generally high quality imputations (van Buuren & Groothuis-Oudshoorn, 2011).

We also included several demographic characteristics of the participants, with the percent of participating children who were male and dummy variables indicating whether participants were mostly (i.e., >50%) (1) White (the reference group); (2) Black; or (3) Hispanic; (4) whether none of these group proportions exceeded 50%; and a catchall (5) racial/ethnic composition missing. Some 84% of studies reported the percentage of their child samples who came from low-income families; we split these studies at the median percentage low income: 90.8%. Our

measure of program characteristics focused on whether the treatment included any sort of family support services, such as parenting education or provision of material resources to parents.

Testing our hypotheses. To test hypotheses 1 and 2 regarding effects of program starting age and duration, we restricted our analytic sample to effect size estimates from outcomes assessed during the treatment and around the end of treatment. Since quite a few studies assessed end-of-treatment program impacts somewhat before the final weeks of the program or slightly after the program ended rather than right at the end, we define the "end of treatment" impact estimate to be the ones closest to the end of the treatment but within a radius of 25% of the program duration. We also measured the time between the measurement and the end of treatment and included it as a covariate in the study in our preliminary regressions. Since we found this timing measure to be uniformly insignificant, we dropped it from the final analyses. The 25% restriction effectively excluded program mid-point outcome measurements. Based on this definition, some 90 effect sizes from 11 studies were assessed within 25% of the end of the treatment; 180 effect sizes from 23 studies were assessed right at the end of the treatment; and 132 effect sizes from 18 studies were assessed within 25% following the end of the treatment. For effect size estimates from outcomes assessed during the treatment, we recoded our measure of program duration to the assessment point. For example, the Abecedarian study had a program length of 60 months. We adjusted the program length to 10 months for treatment outcomes assessed 10 months after the beginning of the treatment. For outcomes assessed at the end of the treatment or after the treatment, we measured program length as 60 months.

To test hypotheses 3 to 6, we restricted our analytic sample to effect size estimates from outcomes assessed at the end of the treatment and after the treatment. When testing hypothesis 3 on fadeout, we estimated both linear and, for time since the end of program, categorical models.

For the latter, we distinguished outcomes that were assessed: (1) at the end of treatments; (2) 0-1; (3) 1-2; (4) 2-4; and (5) 4 or more years following the end of program. When testing hypotheses 4 to 6 regarding what types of outcomes or programs will have more enduring impacts, we created three corresponding interaction terms with the assessment time of the effect size estimate interacting with (1) whether the effect size estimate was from cognitive outcomes (less sensitive to instruction than achievement outcomes), (2) program starting age, and (3) program duration. We drew our inferences for hypotheses 4 to 6 from estimates of these three interaction terms.

Results

An overview of our studies and their average effect sizes on cognitive and achievement outcomes (taken around the end of treatment) are presented in Figure 2. Reflecting their approximate contributions to our weighted results, "bubble" sizes are proportional to the square root of their sample sizes. A full list of studies with their associated starting years, average effect sizes, average end of treatment effect sizes, average starting age, and average program duration is provided in Appendix Table 1. A relatively large proportion of our studies began in the 1960s, with another concentration launched between the early 1990s and early 2000s. Average effect sizes for most studies are positive, although a handful are negative. The weighted trend line in average effect sizes shows a slightly negative slope, which may reflect the changing nature of conditions facing children not enrolled in early childhood education programs (Duncan and Magnuson, 2013).

[INSERT FIGURE 2 HERE]

Table 1 provides descriptive statistics on dependent variables, key timing variables and other control measures. At .16 sd, the weighted mean of the 993 effect size estimates in our database is modest, although there is very large variability around that mean. Moreover, the .16

sd average includes a number of effect size estimates measured years after the end of the programs. If we restrict our calculation to just end-of-treatment effect sizes, the weighted average was .26 sd. This indicates that the typical end-of-treatment impact from the broad set of evaluations conducted over the 47 years between 1960 and 2007 is noteworthy but considerable smaller than the end of treatment effect sizes found for the most famous early childhood programs – Perry and Abecedarian.

[INSERT TABLE 1 HERE]

Table 1 also shows the distribution of studies by a host of other quality and demographic indicators. Most studies did not use random assignment, did not or could not blind data collectors to the treatment status of the children, were not published in peer-reviewed journals, did not adjust for baseline covariates, and were based on treatment-on-the-treated estimates. With regard to demographics, most programs targeted low-income children and many targeted black children. It proved important to adjust for these covariates since they are systematically correlated with both our key timing variables and effect sizes. Appendix Table 2 presents correlations among these covariates.

Publication bias is a potential concern for meta-analytic studies such as ours. It is encouraging that Table 1 shows that most of the effect size estimates in our data base did not appear in refereed journals. A more formal approach to publication bias is with the funnel plot for end-of-treatment effect sizes shown in Figure 3. Each dot represents the average effect size for contrasts in each of the 43 studies that included estimates of impacts at the end of treatment. Bias is a possible problem if smaller studies (the ones with the larger standard errors shown in the bottom portion of the graph) tend to produce more consistently positive impacts (Bornstein et al., 2009), although in the case of the early childhood education, smaller studies might reflect

higher quality programs implemented by developers. Although not obvious from Figure 3, Egger's test for asymmetry of the funnel plot (Egger et al., 1997), which is based on the intercept from the linear regression of normalized effect estimate (i.e., the estimate divided by its standard error) against precision (i.e., reciprocal of the standard error of the estimate), is significantly different than zero (t = 2.6746; df = 35; p = .0113).

Bornstein et al. (2009) recommend two approaches to address the issue of publication bias. One is to assess the possible impact of the bias introduced by conducting a sensitivity test in which the sample of impacts is restricted to those generated by the largest studies. The other approach is "trim-and-fill," which generates simulated effect sizes and standard errors so that the funnel plot becomes symmetric and publication bias is minimized. Results of both approaches are reported below.

[INSERT FIGURE 3 HERE]

Hypothesis 1 regarding starting age

On average, effect sizes were taken from programs that began their treatments at age 3.47 (Table 1). A scatter plot of all effect sizes sorted by starting age in Figure 4 provides a better idea of the nature of the raw data we are working with. In most cases, vertical columns of effect size estimates come from a single study and indicate considerable effect size clustering by study. For example, data in the left-most column of effect sizes are all generated from the 1978 Project CARE program and all were measured at the end of that program's treatment. Some 51.2% of the variance across our entire set of 993 effect size estimates were accounted for by study. This heavy clustering confirms the wisdom of our multilevel modeling strategy.

[INSERT FIGURE 4 HERE]

Wide variation in cognitive and achievement impact estimates at all starting ages is apparent in Figure 4; a weighted trend line fit to these effect sizes has a modest positive slope, indicating somewhat smaller effect sizes with earlier-starting programs. Adjusting for the varying numbers of impact estimates coming from the various studies reversed the sign of this trend line (Model 1 of Table 2), with effect sizes estimated to decrease by a statistically insignificant .029 sd per year of starting age. Adjusting for study quality and demographic composition produced a less negative coefficient (-.019 sd per year). By also incorporating adjustments for program duration, the seventh column in Table 2 provides our best estimate of the impact of starting age on effect sizes: increasing program starting age by a year was associated with a statistically insignificant -.057 sd per year decrease in effect sizes. The estimates for program starting age remained significant and changed only slightly in magnitude when we included during-treatment effect sizes in the analyses (Models 1, 2, 3 in Table 3). Appendix Table 5 provides results from a regression in which starting age is characterized with dummy variables and suggests the possibility of a threshold around a starting age of 3. Both unadjusted and adjusted coefficients from this regression are shown in Figure 5.

[INSERT TABLES 2, 3 AND FIGURE 5 HERE]

Complete regression results for the fully-controlled model, including coefficients on the control variables themselves, are presented in Appendix Tables 3 and 4. Relatively few study characteristics generated significant results. Studies with significant baseline differences between treatment and control groups or higher levels of attrition were associated with smaller effect sizes, while studies that did not include baseline characteristics were associated with larger ones. Studies with greater than 50% children of color generated larger although statistically insignificant effect sizes.

Hypothesis 2 regarding length of program

Treatment duration averaged 1.75 years, with clusters of studies of either very short durations or close to one year in length (Table 1 and Figure 6). Figure 6 shows a modest decrease in average effect size by length of program. In the multilevel models with and without study quality and demographic composition, length of program was negatively associated with effect size estimates (Models 4-7 in Table 2). That is, contrary to our expectations, each added year of length to a program was associated with a decrease in effect size, with our preferred estimate being -.085 sd.

[INSERT FIGURE 6 HERE]

In light of this unexpected negative relationship between program duration and end-of-treatment impact, we wondered whether this was driven by an offsetting association in which most of the longer programs also started at an earlier age. One useful piece of evidence comes from an examination of whether the negative relationship also held within the several studies that measured impacts over the course of their treatments. For this analysis, we included both end-of-treatment (25% radius) effect sizes as well as the during-treatment effect sizes that had been excluded from analyses reported thus far. As described before, we adjusted program duration to the assessment point for effect size estimates from outcomes assessed during the treatment. Figure 7 shows that in most cases, the longer the program, the smaller and the effect size.

[INSERT FIGURE 7 HERE]

There could also be an interaction effect between starting age and program duration. For example, effect sizes could be larger for a program that started early and lasted longer. Therefore, we conducted a model with interaction effects of starting age and program duration.

We did not find that the coefficient on this interaction effect met conventional levels of statistical significance (Appendix Table 6).

Hypothesis 3 regarding time since end of program

Although nearly half (45%) of all effect sizes were measured around the point of program completion, there was still substantial variation in time between end of treatment and effect size measurement (Figure 8). A weighted trend line in Figure 8 showed a drop from about .20 sd to .10 sd over the course of five years – about .02 sd per year. Weighting the data and controlling for the other timing measures produced an annual fadeout estimate of -.006 sd per year; adjusting for study characteristics produced similar estimate of linear persistence at -.006 sd per year (columns 1 and 3 of Table 4).

A closer look at Figure 8 suggests that a linear model might not do justice to the patterns of falling effect sizes. When we characterized these timing effect in a more flexible way using a set of dummy variables, we found what approximates more of a geometric decline (columns 2 and 4 of Table 4). Thus, our full-control model suggests that much of the eventual decline in effect sizes occurs within a year of the end of treatment, and the later decline does not continue in a linear manner. We discuss some of the implications of this rapid decline in the discussion section.

[INSERT FIGURE 8 AND TABLE 4 HERE]

Hypothesis 4 regarding interaction between outcome types and time since end of program

We tested whether the timing of effects differed for cognitive and achievement measures by adding an interaction variable of "whether achievement measure" and "years since end of treatment" (column 1 in Table 5). We found no evidence of more enduring effects for outcomes that were more or less sensitive to preschool instruction.

[INSERT TABLE 5 HERE]

Hypothesis 5 regarding interaction between program length and time since end of program

A second interaction hypothesis regarding persistence is that longer programs produce longer-lasting effects than shorter programs. We tested this hypothesis with an interaction between length of treatment and years since end of treatment. The results suggested significantly more persistence of effects for studies with longer duration than those with shorter program duration (β = .006; *se* = .002; $p < .001$).

Hypothesis 6 regarding interaction between program starting age and time since end of program

A third persistence-interaction hypothesis is that programs starting in a child's early years have longer-lasting effects. We tested this hypothesis with an interaction between starting year and years since end of treatment. The results suggested significantly more persistence of effects for studies that started early than those that started late (β = -.004; *se* = .001; $p < .01$).

Sensitivity Analyses and Robustness Checks

We have defined "end-of-treatment" effect sizes according to whether they were assessed within a radius of 25% of the program duration. When we shortened the radius distance to 10%, our estimates of the effects of starting age and program duration were somewhat less negative, while our estimates of effect size persistence and persistence interactions were quite similar to those obtained with a 25% radius (Appendix Tables 7, 8, and 9, which parallel models presented in Tables 2, 3, and 4).

As mentioned above, one approach for assessing the possible bias revealed in the funnel plot in Figure 2 is to re-estimate key models using only observations from larger studies. We

reran the regressions presented in Table 2, 3, and 4 using data based on the largest 50% of our contrasts (i.e. studies with sample sizes greater than 178; see Appendix Tables 10, 11 and 12). Our estimates of starting age were not significant when only larger studies were included. Estimates of effect size persistence from larger studies were similar with those from all studies. In addition, all three estimates of persistence interactions were significant when only larger studies were included, indicating for larger studies, significantly more persistence of effects for outcomes that were less sensitive to preschool instruction than outcomes more sensitive to instruction, more persistence of effects for programs with longer duration than those with shorter duration, and more persistence of effects for programs with relatively older children than those with infants and/or toddlers.

We also used a trim-and-fill approach to remove publication bias in effect size estimates (Duval & Tweedie, 2000). Because trim-and-fill cannot be used to generate simulated covariates, we resorted to a series of bivariate comparisons of the average effect size of: studies targeting children younger and older than 3 years old when the treatments began, and studies with treatments that lasted less and more than 2 years. Consistent with Table 2, 3, and 4 results regarding starting age, the average effect size of studies targeting children younger than 3 years old was a statistically significant .068 sd higher than for studies targeting older children. The trim-and-fill estimate of this difference was similar – a statistically significant difference of .053 sd. Thus, trim-and-full adjustments do not alter the conclusion that effect sizes are somewhat larger in studies starting earlier in life.

The trim-and-fill adjustment also confirmed our conclusions with regard to program duration. The average effect size of shorter-duration studies was a statistically insignificant .029

sd higher than for studies that lasted longer. The trim-and-fill estimate of this difference favored shorter studies by a statistically significant .043 sd.

When coding our outcome assessment, we have grouped vocabulary outcomes into cognitive outcomes are that not sensitive to instruction because preschools that provide literacy activities and instruction often fail to provide language rich environments (Justice et al., 2008). However because vocabulary skills could be impacted by a child's language environment, we grouped vocabulary outcomes into cognitive outcomes that are sensitive to instruction and conducted models in Tables 2, 3, and 4. Appendix Tables 13, 14, and 15 presented results of this set of analysis, which are quite similar to corresponding results in Tables 2, 3, and 4.

When estimating effects of the starting age, program duration, and persistence of impacts of early childhood education programs, we used effect sizes from outcomes assessed at various developmental stages. However because effects of these timing factors could vary according to when the outcomes were measured, we examined associations between timing variables and effect sizes of outcomes measured at school-entry age. We presented results in Appendix Tables 16, 17, and 18. Unfortunately, number of effect sizes at school-entry age were so small that most of our models with covariates did not converge.

In addition, timing variables may have differential associations with effect sizes in achievement and cognitive measures. Hence we included interactions between timing variables and the indicator for achievement or cognitive measures. We included this set of results in Appendix Tables 19, 20, and 21. We found programs that started later generated larger effects on end-of-treatment cognitive than achievement outcomes, as did programs with longer durations. The pattern of interactions in Appendix Table 19 shows higher initial impacts on achievement

than cognitive outcomes but the achievement impacts quickly fall below the cognitive impacts. We expand on these results in the discussion section.

Discussion

The data presented in this paper are drawn from the most comprehensive meta-analytic database of early childhood programs in the United States produced to date. They comprise about 1,000 effect sizes generated by 67 studies conducted over a 47-year period. The criteria used for screening studies led us to include those with high quality standards, and adjust for quality differences across the studies.

The overall (weighted) magnitude of ECE impact on both cognitive and achievement scores averaged .26 sd at the time of program completion. This average reflects a moderate level of effectiveness and is comparable with what has been reported in the literature on short-term impacts of Head Start programs more generally (Shager et al., 2010) and for literacy impacts in the recent Head Start impact study specifically (USDHHS, 2010). At the same time, it is considerably smaller than some of the end-of-treatment impacts generated by demonstration efficacy studies such as the Perry Preschool Project and Abecedarian Program. Thus, almost half a century of evaluation research confirms that, on average, center-based early education programs that begin between birth and 5 years of age produce positive effects of moderate magnitude on measures of cognition and achievement taken close to program exit.

Beyond this overall finding of ECE program effectiveness, further analyses of the extent to which timing considerations influenced impacts revealed some expected and a few unexpected results. As we hypothesized, effect sizes were somewhat larger for children who started programs at younger ages. Also as expected, treatment effects persisted beyond the end of the programs, but declined exponentially, losing half of their end-of-treatment values within just a

year. Our linear estimate of fadeout (-.022 sd) is similar to estimates from two prior meta-analyses that examined persistence of early childhood education effects (Aos et al. 2004; Camilli et al., 2010), but we find that a linear function does not capture the time path of fadeout very well, which is characterized by a rapid immediate decline, rather than a slow decremental decline.

We found that impacts on cognitive measures tended to be smaller at the end of treatment but persist longer than impacts on achievement measures. This result runs counter to findings from the Perry Preschool evaluation in which achievement impacts lasted longer than IQ effects, although would be in keeping with the possibility that achievement gains fade as control-group children receive more and more direct instruction on achievement skills once they enter primary school. This implies that ECE curricula that are designed to build broader cognitive skills, which are usually thought of as less sensitive to instruction (e.g., vocabulary), may produce longer-lasting impacts. For example, effective vocabulary instruction is relatively rare in ECE programs, which tend to focus their instruction on narrower school-readiness skills such as identifying letters and numbers (Bowne, Yoshikawa, & Snow, 2016).

The value of persisting cognitive skills, in the absence of achievement skill differences, is unclear. Theoretically, improved cognitive skills, such as increased vocabulary and executive functioning, are thought to be of importance because they improve children's ability to learn in the classroom—and these skills may be especially important in later grades when instruction is more likely to come in the form of independent work and in which broader comprehension is especially relevant. Yet, the fact that they are not accompanied by lasting achievement impacts, which might be driven by improved cognitive skills, is a bit puzzling. One possible explanation

is that the achievement measures are not well aligned with later school content or are not sensitive enough to capture meaningful differences in achievement skills.

The diminishing persistence of program impacts over time may be due to the fact that many teachers fail to differentiate instruction for beginning and advanced students, which will reduce the likelihood of continued academic growth relative to the expected growth of comparison children who have not learned the content in preschool (Bennett, Desforges, Cockburn, & Wilkinson, 1984; Engel et al., 2012). Although these patterns of declining impacts are sometimes called "fadeout," in the context of early-grade learning, when achievement gains are rapid for almost all children, converging achievement trajectories of children in the pre-K and comparison groups are better described as "catch up." Unfortunately, we lack causal evaluations of interventions of differential instruction in early childhood in the US to be able to test this hypothesis. Moreover, a recent meta-analysis of studies pairing ECE programs with high-quality early-grade classroom experiences failed to find consistent evidence of positive interactions (Bailey et al., 2019).

Although the data reported in this paper confirm the positive impacts of a wide range of center-based, early education programs throughout the early childhood period, the larger magnitude of effects reported for infant-toddler services compared with preschool programs for 4-year-olds raises important questions about how we should allocate our ECE policy dollars. Although impacts of the Early Head Start program were small (Love et al., 2002), our meta-analysis showed that programs aimed at younger children tended to produce substantially larger impacts than programs starting later. This suggests the importance of redoubling efforts to develop scalable and effective early-childhood program models for infants and toddlers, while at the same time being aware of the fact that the high staff ratios in center-based care for infants

and toddlers increases their expense substantially (e.g., Duncan and Sojourner, 2013) and that the preschool programs that follow these early infant and toddler programs may amplify or diminish their effects.

More nuanced descriptions of the center-based services and curricula delivered in these programs, followed by analyses of their relation to the different developmental needs of infants, toddlers, and preschoolers, would provide some of the additional data needed to interpret the aggregated results reported in this paper. Unfortunately, our data set does not include the detailed information on indicators of developmentally appropriate practice required to address these questions.

While this paper is based on a rich meta-analytic database that covers almost 50 years of research, there are certainly limitations to the conclusions we can draw. First, our research funding enabled us to code up literature published between 1960 and 2007, but not studies published more recently than 2007. A major development in US ECE programs over the past two decades has been the growth of state-based pre-kindergarten programs and evidence from very few pre-K evaluations are included in this paper. Phillips et al. (2017) compile evidence on pre-K programs through 2017 and find "striking uniformity" in the evidence that pre-K programs are successful in improving school-readiness skills at the end of the pre-K year. Longer-run effects have rarely been assessed with random-assignment research designs. Two exceptions were noted above: Lipsey et al. (2018) found null or even negative achievement impacts in third grade in the Tennessee Voluntary PreK program, while Weiland et al. (2019) found similar null to negative patterns for the Boston pre-K program. The possibility that attending a pre-kindergarten program might hurt children's longer-run prospects is novel, disturbing and very much in need of further replication. Gormley et al. (2018) find scattered positive impacts of the

Tulsa's pre-K program in their eighth-grade follow-up, although rely on propensity-score matching methods rather than random assignment to generate their impact estimates.

Second, we can say nothing about the general effects of center care (e.g., from national survey studies) because research design limitations of those studies led them to fail to meet our selection criteria. In addition, our meta-analysis focused solely on studies conducted in the United States and its protectorates, due to the very different research contexts of evaluations of ECE in other nations. That said, recent reviews that focus on the effects of preprimary education in low- and middle-income countries offer additional insights that are worthy of attention (Engle et al., 2011; Britto et al., 2017; Rao et al., 2017).

Another limitation is that characteristics of children likely to select into these studies may have changed over time as center-based and preschool programs spread in the 1980s and 1990s (Duncan and Magnuson, 2013). Therefore, although we controlled for some demographic features of populations such as SES and race, we were unable to control for other population characteristics such as parent education and family structure. In terms of the timing findings, there are limits to understanding the pace of decline of ECE program effects because virtually all studies lack data about the quality of subsequent schooling or family changes. And finally, all of the impacts presented in the paper compare children in early education programs to children in a diverse array of counterfactual conditions, most of which are not well documented in the evaluation studies we analyze.

As with any longitudinal impact study, another limitation to our analysis is that cognitive measures, even if given the same label, may not be truly comparable at different ages and developmental periods. This introduces a confound between age and measurement that is impossible for us, or indeed for any researchers, to untangle.

In summary, this study shows that center-based, ECE for disadvantaged children, on average, produces positive impacts of nearly one-quarter of a standard deviation on cognitive skills and achievement scores at program completion. These data confirm the plasticity of cognition and learning in early childhood and the potential capacity of interventions to improve developmental outcomes that persist for several years beyond the end of program treatments, despite their diminishing magnitude over time. These findings also highlight the need for new intervention approaches that increase the impacts of additional months (or years) of program services, as well as modified curricula for the early grades of school that build on the gains produced by effective early childhood programs.

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Note: Asterisks (*) indicate papers or research reports for the studies in the meta-analysis.

Figure 1. Inclusion Criteria of Analyzed Studies.

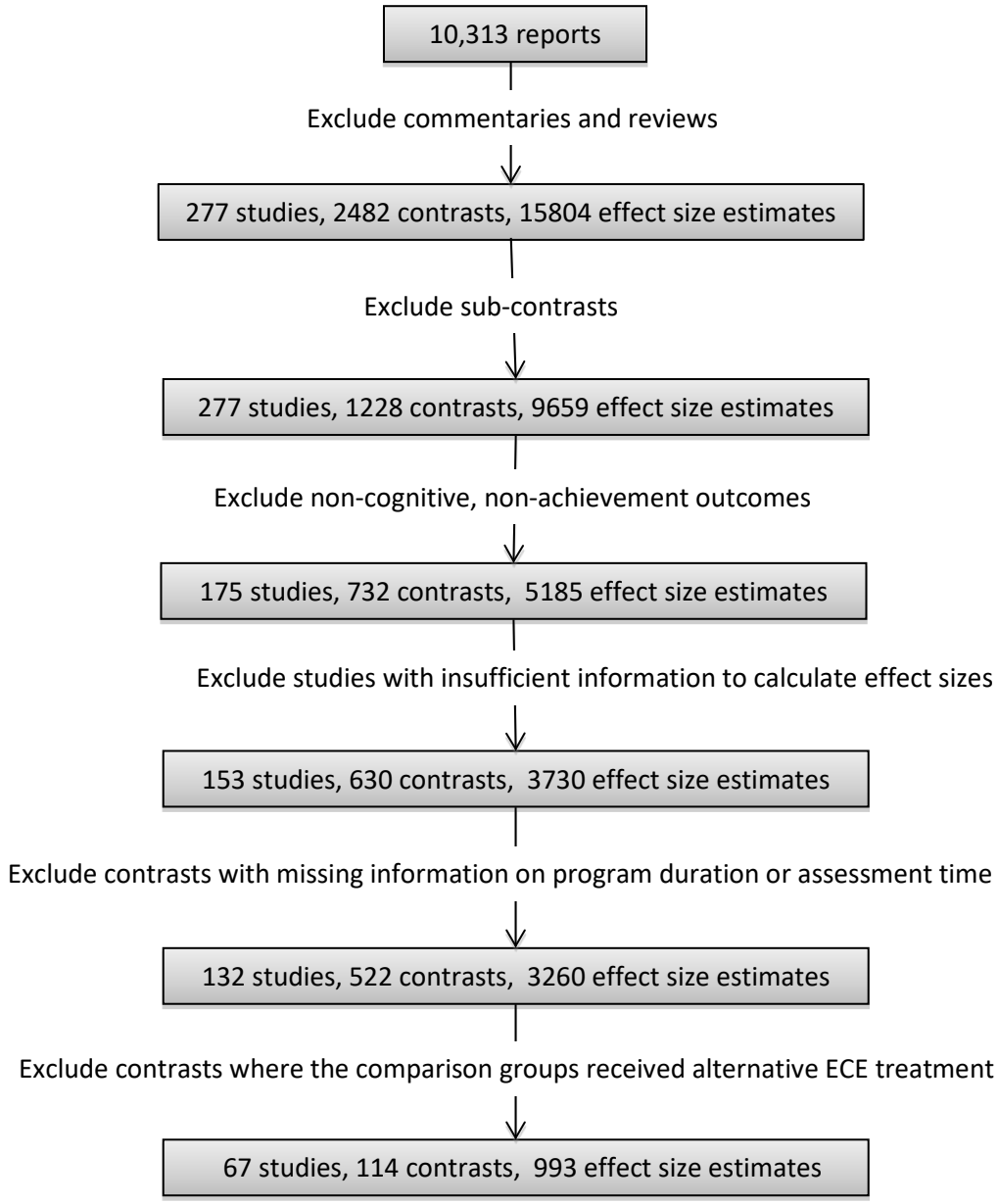
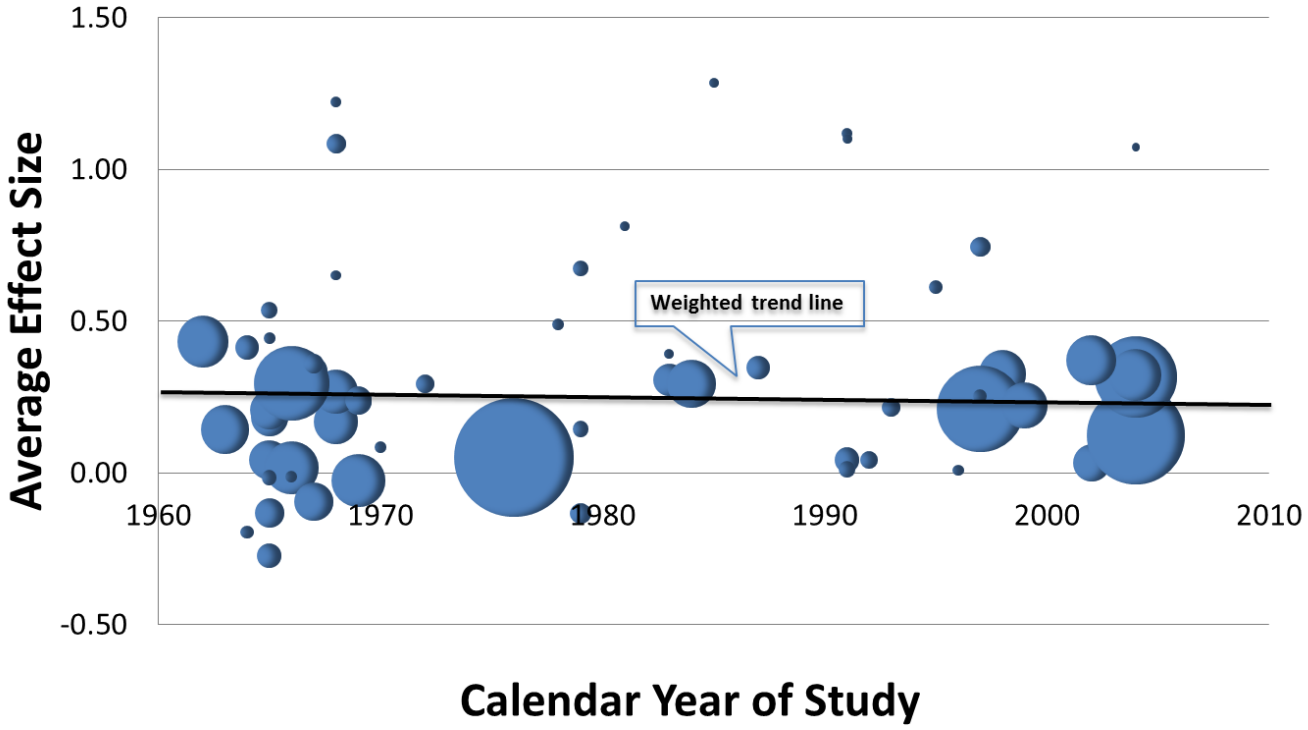
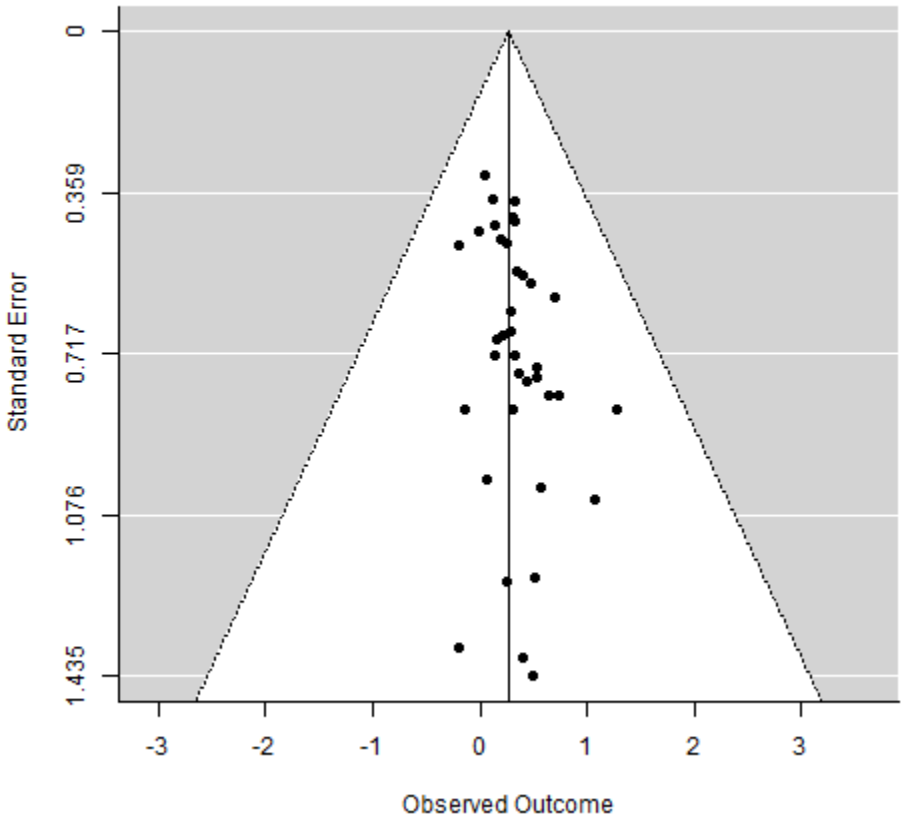


Figure 2. Study Average End-of-Treatment Effect Sizes by Year of Study.



Note: Bubble Area is proportional to the square root of the number of subjects in the study

Figure 3. Funnel plot for end-of-treatment effect sizes at study level.



Note: Dots represent study-level end-of-treatment effect sizes, calculated from 43 studies.

Figure 4. Distribution of Effect Sizes by Starting Age.

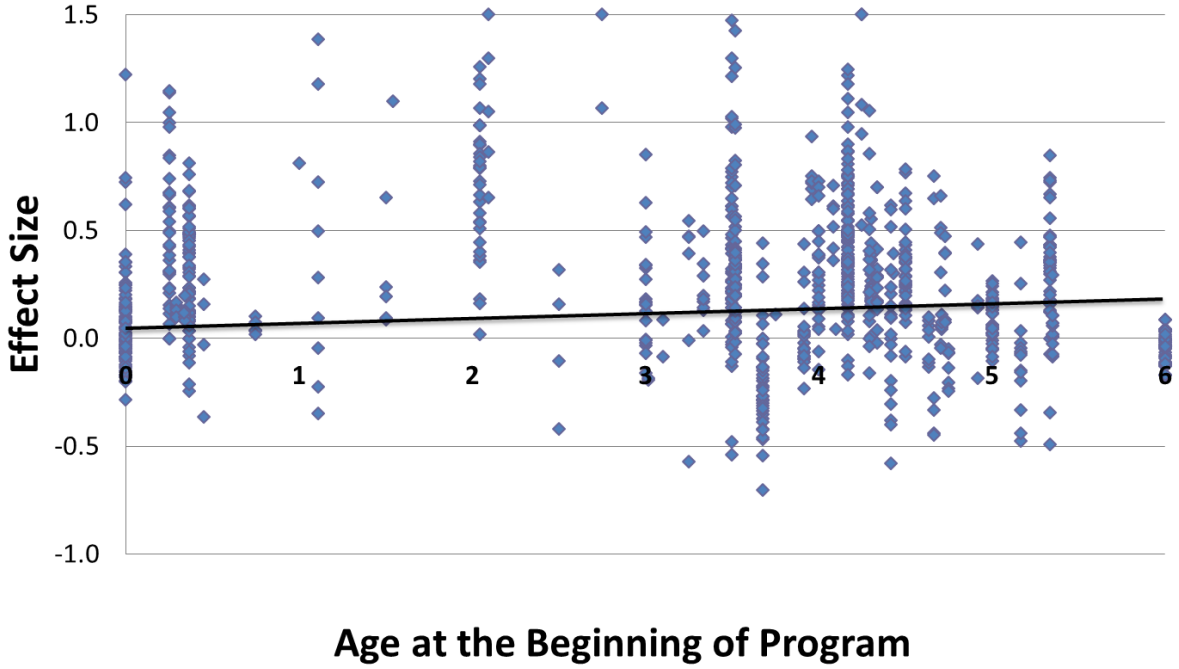
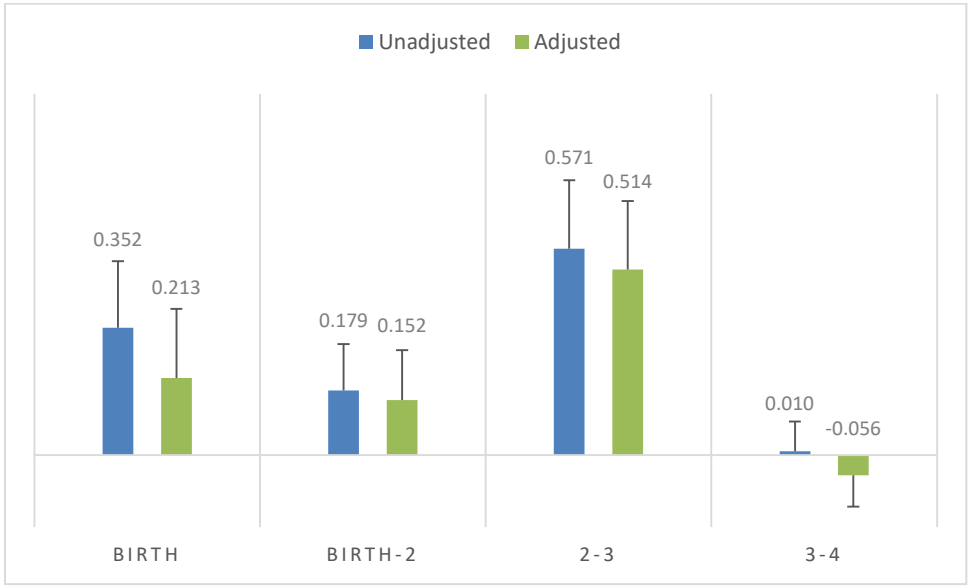


Figure 5. Coefficients on Starting Age Unadjusted and Adjusted for Covariates



Note: Bars indicates standard errors of coefficients.

Figure 6. Distribution of Effect Sizes by Length of Treatment.

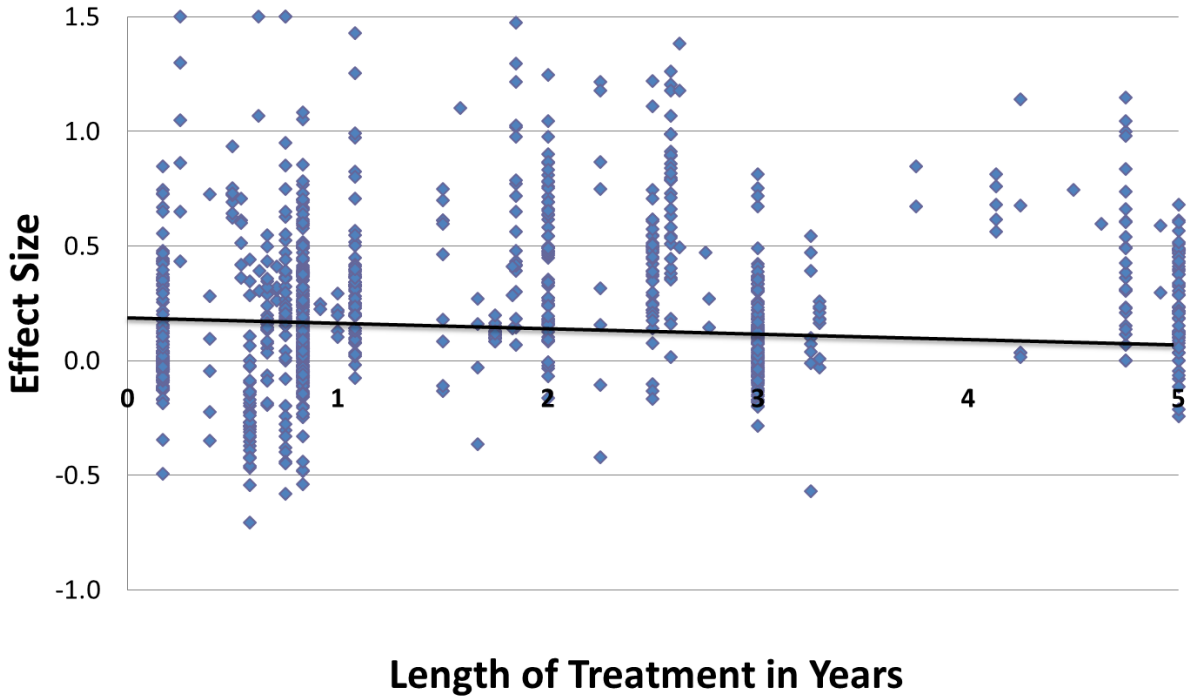


Figure 7. Duration effects by contrast. During treatment and end-of-treatment (25% radius) effect sizes were included. The HLM estimate of duration effect after including all contrasts, controlling for covariates, and weighted by the inverse variance weight of each effect size multiplied by the inverse of the number of effect sizes within a contrast is -.095 (*se* = .019).

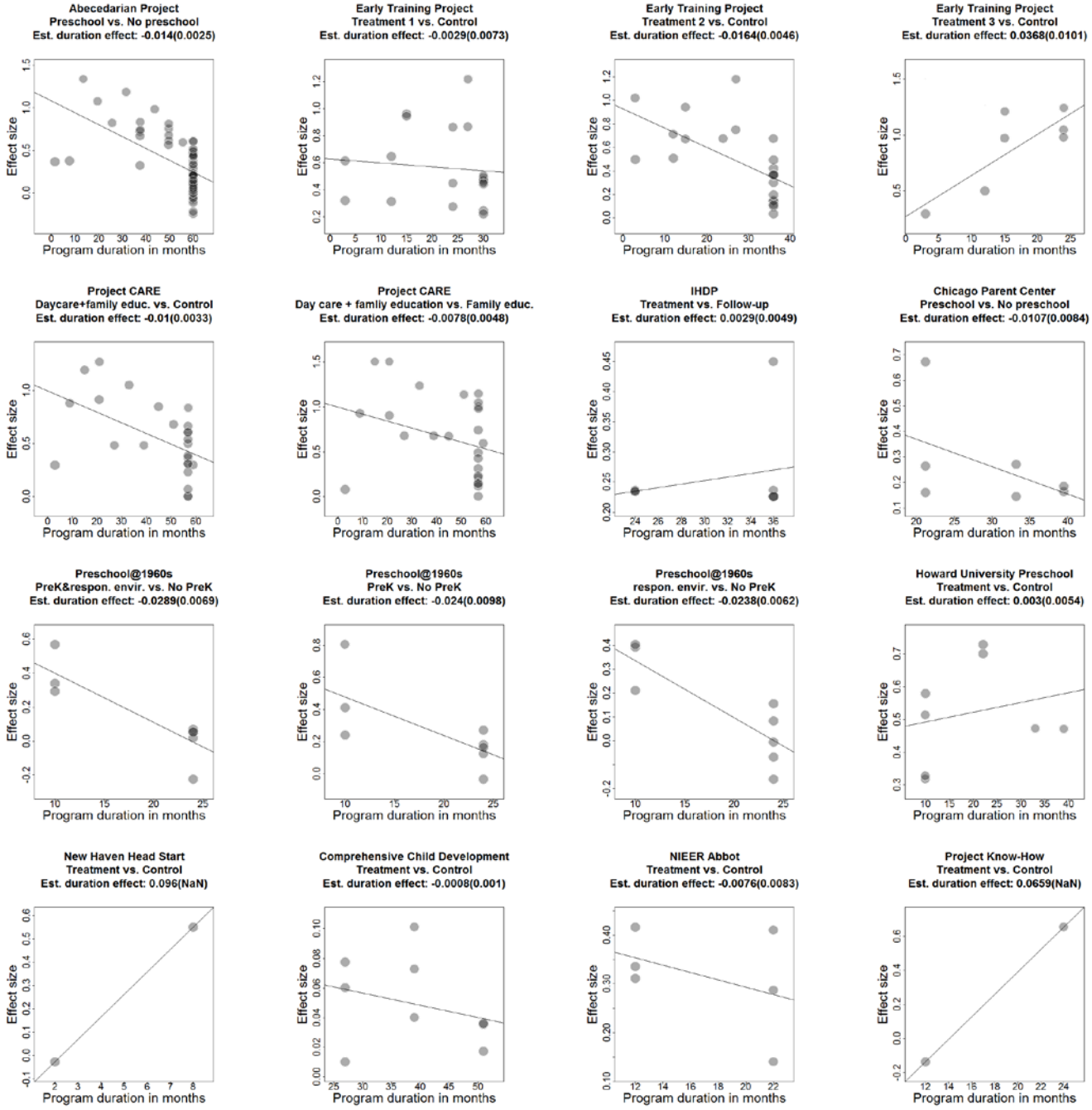


Figure 8. Distribution of Effect Sizes by Time of Measurement.

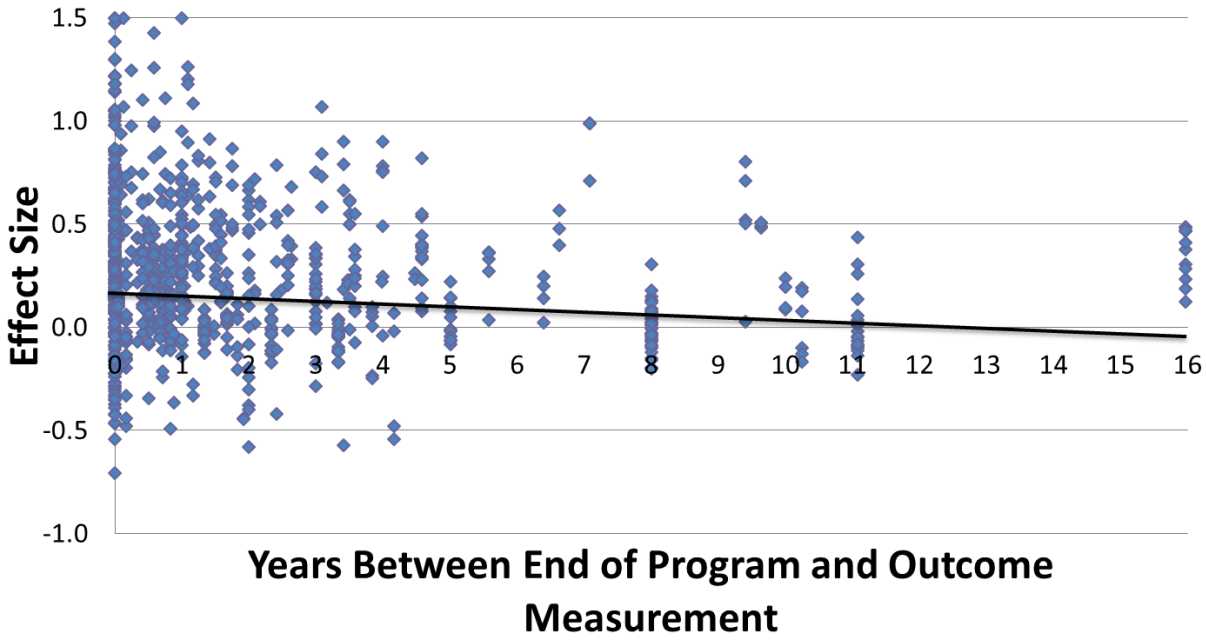


Table 1 Descriptive Statistics For Dependent And Independent Variables

	Mean (SD) or proportion (Unimputed)		# of effect sizes	# of contrasts	# of studies	Mean (SD) or proportion (Imputed)	
	Unweighted	Weighted				Unweighted	Weighted
Effect size at the end of treatment	0.30 (0.38)	0.26 (0.28)	407	65	43	0.30 (0.38)	0.28 (0.24)
Effect size after the end of treatment	0.22 (0.31)	0.11 (0.22)	586	70	40	0.22 (0.31)	0.09 (0.23)
Effect size at and after the end of treatment	0.25 (0.34)	0.16 (0.26)	993	114	67	0.25 (0.34)	0.20 (0.26)
Starting age of treatment (in yrs)	3.25 (1.86)	3.47 (2.04)	993	114	67	3.25 (1.86)	3.65 (1.67)
Length of treatment (in yrs)	1.75 (1.42)	1.45 (1.24)	993	114	67	1.75 (1.42)	1.13 (0.91)
Time since end of treatment (in yrs)	2.29 (3.67)	2.45 (3.69)	993	114	67	2.29 (3.67)	1.25 (1.89)
Achievement Measure	0.38	0.38	379	71	39	0.38	0.42
Cognitive Measure	0.63	0.64	628	94	61	0.63	0.70
Passive control group	0.78	0.65	777	88	51	0.78	0.56
Study did not use random assignment	0.65	0.73	641	87	47	0.65	0.74
Any significant differences at baseline	0.35	0.34	345	32	19	0.35	0.42
Bias was observed in study	0.56	0.57	560	57	28	0.56	0.52
Data collectors not blinded	0.60	0.69	592	99	57	0.60	0.80
Measurement method							
Observational rating	0.07	0.09	69	20	12	0.07	0.07
Performance test	0.86	0.87	853	106	62	0.86	0.84
Other measurement method	0.07	0.04	71	11	8	0.07	0.09
Study not from a peer refereed journal	0.65	0.67	643	83	47	0.65	0.71
Treatment on the treated	0.93	0.89	921	110	64	0.93	0.93
Baseline covariates not included	0.76	0.55	759	86	55	0.76	0.50
Attrition							
High attrition (> .25)	0.25	0.29	244	38	22	0.25	0.30
Medium attrition (.16 - .25)	0.30	0.40	297	28	19	0.30	0.18
Low Attrition (<.16)	0.43	0.26	424	56	40	0.43	0.25
Attrition information missing	0.03	0.06	28	9	6	0.03	0.26

Table 1 Descriptive Statistics For Dependent And Independent Variables (Continued)

	Mean (SD) or proportion (Unimputed)		# of effect sizes	# of contrasts	# of studies	Mean (SD) or proportion (Imputed)	
	Unweighted	Weighted				Unweighted	Weighted
Low reliability (< .88)	0.27	0.36	264	26	15	0.27	0.15
Missing Reliability	0.62	0.57	620	97	58	0.62	0.74
Study conducted before 1980	0.71	0.53	704	68	36	0.71	0.35
Race							
>50% White	0.12	0.09	123	13	10	0.12	0.07
>50% Black	0.43	0.29	423	40	27	0.43	0.20
>50% Hispanic	0.04	0.06	41	7	6	0.04	0.06
No Racial Group > 50%	0.13	0.22	128	21	14	0.13	0.44
Race Missing or Other	0.28	0.34	278	33	19	0.28	0.24
% Low Income (>90.8%)	0.57	0.34	568	60	41	0.57	0.33
% Low Income (<90.8%)	0.32	0.48	321	47	21	0.32	0.53
% Low Income Missing	0.10	0.19	104	7	6	0.10	0.14
% Male	0.46 (0.16)	0.41 (0.2)	993	114	67	0.46 (0.16)	0.43 (0.18)
Missing Gender	0.37	0.50	370	47	28	0.37	0.37
Any Family Support Services	0.79	0.84	783	69	38	0.79	0.71
No Family Support Services	0.06	0.03	62	17	11	0.06	0.07
Any Family Support Services Missing	0.15	0.13	148	28	21	0.15	0.21
Research/Demonstration Programs	0.49	0.32	489	27	20	0.49	0.11
Quality Index	3.79 (1.55)	3.5 (1.33)	373	41	26	3.74 (1.68)	3.91 (1.62)

Note: These data are based on 993 effect sizes, drawn from 114 contrasts and 67 studies.

Table 2. Random Effect Models of Program Impacts and Treatment Starting Age and Length: End-of-Treatment Effect Sizes Only

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Starting Age of Treatment (in yrs)	-0.029	-0.021	-0.019				-0.057
	[0.026]	[0.029]	[0.034]				[0.036]
Length of Treatment (in yrs)				-0.046 *	-0.05 *	-0.071 **	-0.085 ***
				[0.022]	[0.022]	[0.024]	[0.026]
Quality Index	No	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes
Variance Accounted for by Contrast	2.7%	2.1% - 3.1%	1.9% - 2.5%	3.1%	2.9% - 3.5%	2.2% - 2.5%	1.7% - 2.6%
Variance Accounted for by Study	3.3%	2.9% - 4.1%	2.2% - 2.7%	3.6%	3.2% - 3.9%	2.1% - 2.6%	2.2% - 2.9%
Number of Effect Sizes	407						
Number of Contrasts	65						
Number of Studies	43						

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Table 3. Random Effect Models of Program Impacts and Treatment Starting Age and Length: During-Treatment and End-of-Treatment Effect Sizes

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Starting Age of Treatment (in yrs)	-0.035	-0.023	-0.013				-0.048
	[0.027]	[0.029]	[0.035]				[0.036]
Length of Treatment (in yrs)				-0.062 ***	-0.061 ***	-0.061 ***	-0.064 ***
				[0.011]	[0.011]	[0.011]	[0.012]
Quality Index	No	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes
Variance Accounted for by Contrast	3.0%	2.3% - 3.2%	1.6% - 2.6%	2.0%	2.0% - 3.4%	2.2% - 2.6%	1.5% - 2.5%
Variance Accounted for by Study	3.9%	3.8% - 4.7%	2.6% - 4.0%	5.9%	4.1% - 5.9%	2.4% - 2.9%	2.4% - 3.7%
Number of Effect Sizes	484						
Number of Contrasts	68						
Number of Studies	45						

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Table 4. Hierarchical Linear Models of Program Impacts and Assessment Time since End of Treatment

	End-of-treatment and post-treatment effect sizes			
	Model 1	Model 2	Model 3	Model 4
Starting age of treatment (in yrs)	-0.078 **	-0.065 **	-0.057 *	-0.050 +
	[0.024]	[0.024]	[0.026]	[0.026]
Length of treatment (in yrs)	-0.063 **	-0.050 *	-0.072 **	-0.059 **
	[0.023]	[0.023]	[0.023]	[0.023]
Time since end of treatment (in yrs)	-0.006 **		-0.006 **	
	[0.002]		[0.002]	
At the end of treatment		*Reference*		
0 to 1 yr beyond treatment		-0.125 ***		-0.116 ***
		[0.024]		[0.025]
1 to 2 yrs beyond treatment		-0.144 ***		-0.140 ***
		[0.023]		[0.024]
2 to 4 yrs beyond treatment		-0.155 ***		-0.151 ***
		[0.028]		[0.029]
> 4 yrs beyond treatment		-0.144 ***		-0.142 ***
		[0.022]		[0.022]
Quality Index	No	No	Yes	Yes
Covariates	No	No	Yes	Yes
Variance Accounted for by Contrast	3.1%	3.1%	2.3% - 2.4%	2.2% - 2.3%
Variance Accounted for by Study	3.4%	3.3%	2.2% - 2.4%	2.2% - 2.3%
Number of Effect Sizes	993			
Number of Contrasts	114			
Number of Studies	67			

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) Regressions are weighted by the inverse variance of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Table 5. Hierarchical Linear Models of Program Impacts with Interactions with Time Since End-of-Treatment (EOT)

	End-of-treatment and post-treatment effect sizes		
	Model 1	Model 2	Model 3
Starting age of treatment (in yrs)	-0.056 *	-0.057 *	-0.051 *
	[0.026]	[0.025]	[0.026]
Length of treatment (in yrs)	-0.072 **	-0.076 ***	-0.069 **
	[0.023]	[0.023]	[0.023]
Time since end of treatment (in yrs)	-0.007 **	-0.027 ***	-0.003 +
	[0.002]	[0.006]	[0.002]
Achievement * Time since EOT	0.002		
	[0.003]		
Length * Time since EOT		0.006 ***	
		[0.002]	
Starting age * Time since EOT			-0.004 **
			[0.001]
Quality Index	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Variance Accounted for by Contrast	2.3% - 2.4%	2.2% - 2.3%	2.2% - 2.3%
Variance Accounted for by Study	2.2% - 2.4%	2.1% - 2.2%	2.1% - 2.3%
Number of Effect Sizes		993	
Number of Contrasts		114	
Number of Studies		67	

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) Regressions are weighted by the inverse variance weight of each effect size multiplied by the inverse of the number of effect sizes within a program. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Table 6. Contrast-Level Fixed-Effect Model for Assessment Time since End of Treatment.

	End-of-treatment and post-treatment effect sizes	
	Model 1	Model 2
Time since end of treatment (in yrs)	-0.009 +	-0.009 +
	[0.005]	[0.005]
Quality Index	No	Yes
Covariates	No	Yes
Number of Effect Sizes	977	
Number of Contrasts	98	
Number of Studies	57	

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) Regressions are weighted by the inverse variance weight of each effect size multiplied by the inverse of the number of effect sizes within a program. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 1: List of Studies Included in Analysis

Study Name	Starting Year	Average Effect Size	Average End-of-Treatment Effect Size	Number of End-of-Treatment Effect Size	Average Follow-up Effect Size	Number of Follow-up Effect Size	Average Starting Age	Average Program Duration
Early Training Project	1962	0.50	0.51	30	0.43	86	4.17	2.24
Preschool @ Cambridge, MA	1962	0.21	0.07	10	0.22	8	3.02	1.71
The Perry Preschool	1962	0.39			0.36	55	3.52	1.06
Head Start program on cognitive growth	1963	0.54	0.54	6			4.08	0.54
Howard University Preschool Program	1964	0.37	0.47	2	0.09	4	3.25	2.17
Full-Day Preschool with curriculum on language, social skills, & health	1964	0.75	0.74	5	0.75	1	3.96	0.50
Karnes Ameliorative Curriculum Pre-K	1965	1.28	1.28	2			2.75	0.63
National Head Start and Summer Head Start Program, 1965-1968	1965	0.02			0.02	92	5.59	0.44
Head Start and Summer Head Start in St. Louis, MO	1965	0.41			0.41	13	4.00	0.83
Summer Head Start in New Jersey	1965	0.44			0.44	1	5.17	0.17
Pre-K in NY	1965	0.14	0.14	5	0.15	2	4.00	0.83
Summer Head Start in Kearney, NE	1965	0.08			0.08	2	5.00	0.17
Summer Head Start in Cambridge, MA	1965	0.14	0.14	5			4.92	0.17
Summer Head Start in Allerhand	1965	0.33			0.33	39	5.33	0.17
Summer Head Start in Lincoln, NE	1965	0.03			0.03	15	5.35	0.17
Head Start in Montgomery County, MD	1966	-0.27			-0.27	8	4.42	0.75
Preschool in Fremont CA	1966	-0.19	-0.19	41			3.67	0.58
Urbana Infant Home Tutoring Program	1966	0.89			1.10	1	1.54	1.29
Head Start effects by Nummedal and Stern	1967	-0.13	-0.13	13			5.17	0.83
Greensboro Demonstration Nursery Center	1967	0.81	0.81	1			1.00	3.00
Project Know-How	1967	0.19	0.65	1	0.15	4	1.50	1.83
Yale Child Welfare Research Program	1968	1.22	1.22	1			0.00	2.50
Home Oriented Preschool Education	1968	0.57	0.57	30			3.50	1.73
Nedler intervention on Mexican American three year olds	1968	0.65	0.65	3			3.00	0.75
Direct instruction Head start vs. Regular Head Start	1968	0.43	0.43	8			4.50	0.83
Head Start in rural areas, MN	1968	-0.02	0.70	2	-0.37	4	4.67	0.75
Louisville Head Start Curriculum Comparison	1968	0.01			0.01	20	3.92	0.83
ETS Head Start Longitudinal Study	1969	0.33	0.33	4			3.50	0.71
A comparison of Psycholinguistic Ability of 1st graders (Head Start vs No Head Start)	1969	1.08			1.08	1	4.25	0.83

Study Name	Starting Year	Average Effect Size	Average End-of-Treatment Effect Size	Number of End-of-Treatment Effect Size	Average Follow-up Effect Size	Number of Follow-up Effect Size	Average Starting Age	Average Program Duration
Planned Variation in Head Start	1969	0.30	0.30	12			4.72	0.83
Buffalo Early Childhood Education Project	1970	-0.01			-0.01	4	2.50	2.25
Abecedarian Project	1972	0.37	0.25	49	0.41	20	0.37	4.54
Parent-Child Home Program @ Pittsfield, MA	1974	0.70	0.52	3	0.72	29	2.04	2.58
Infants in community day care center	1976	0.39	0.39	9			1.11	1.14
Project CARE	1978	0.61	0.49	34			0.25	3.74
Currie and Thomas - NLSCM fixed effect study of Head Start	1978	-0.13			-0.13	6	3.50	0.83
Field Teacher Mother Parent Training	1979	0.67			0.67	2	0.00	0.50
Head Start Bilingual Bicultural Development Project	1979	0.31	0.31	26			4.31	0.83
Head Start in New Haven, CT	1979	0.26	0.55	1			3.52	0.42
Child care v. Home care for middle-income white families	1981	0.01			0.01	4	0.45	1.67
SC High Scope Pre-K	1983	0.21			0.21	1	4.42	0.75
Children of Poverty: A Multi-level Analysis of the Determinants of Intellectual Development	1983	0.74	0.74	1			0.00	4.50
Infant Health and Development Program (IHDP)	1984	0.04	0.15	18	0.01	72	0.00	3.00
Chicago Parent Center	1985	0.21	0.19	4	0.16	7	3.50	2.89
DC Early Learning Pre-K	1987	0.04			0.04	1	4.10	0.75
Even Start National Evaluation	1991	0.04			0.04	3	4.00	0.67
Effects of Project Preschool PLUS bilingual program vs other LEP students	1991	0.61			0.61	5	4.17	0.83
North Carolina Pre-K Evaluation	1991	0.24			0.24	12	4.42	0.83
Comprehensive Child Development Program	1991	0.05	0.05	6			0.75	3.25
Kentucky Educational Reform Act (KERA) Preschool Program	1992	0.18			0.18	3	3.58	0.75
Georgia Pre-K Program	1993	0.23			0.23	2	4.00	0.92
Michigan School Readiness Program Longitudinal Evaluation	1995	0.27			0.27	23	4.30	0.75
Early Head Start Research and Evaluation Project	1996	0.12	0.13	7	0.12	9	0.32	1.75
Effects of Preschool on Kindergarten readiness	1997	1.12	1.50	1	0.99	3	4.25	0.75
ECLS-K Head Start Study	1997	-0.03	-0.01	6	-0.03	18	4.70	0.83
Language intervention for late talkers	1997	1.07	1.07	5			2.09	0.25
Parent-child intervention for depressed mothers	1997	0.43			0.43	1	0.25	0.25
Southeastern Head Start program of high quality	1998	0.35	0.35	2			4.43	0.63
Third Even Start evaluation	1999	-0.09	-0.19	2	0.00	2	3.06	0.67

Study Name	Starting Year	Average Effect Size	Average End-of-Treatment Effect Size	Number of End-of-Treatment Effect Size	Average Follow-up Effect Size	Number of Follow-up Effect Size	Average Starting Age	Average Program Duration
National Head Start Impact Study First Year	2002	0.21	0.21	18			3.92	0.67
Tulsa, Oklahoma universal pre-K	2002	0.39	0.39	14			4.50	0.83
NIEER Abbott preschool interim report	2004	0.28	0.26	6	0.15	3	4.40	1.17
Five State Pre-K RD Evaluation, Michigan	2004	0.32	0.32	3			4.00	0.83
Five State Pre-K RD Evaluation, New Jersey	2004	0.32	0.32	3			4.00	0.83
Five State Pre-K RD Evaluation, Oklahoma	2004	0.32	0.32	3			4.00	0.83
Five State Pre-K RD Evaluation, South Carolina	2004	0.37	0.37	2			4.00	0.83
Five State Pre-K RD Evaluation, West Virginia	2004	0.29	0.29	3			4.00	0.83

Appendix Table 2: Correlation Matrix of All Predictor Variables for During-Treatment, End-of-Treatment, and After-Treatment Effect Sizes

	Effect size	Starting age of treatment (in yrs)	Time since end of treatment (in yrs)	Length of treatment (in yrs)	Achievement Measure	Passive control group	Study did not use random assignment	Any significant differences at baseline	Bias was observed in study	Observational rating	Other measurement method
Effect size	1.00	0.03	-0.25	-0.02	0.03	0.25	0.16	0.12	-0.06	-0.03	0.02
Starting age of treatment (in yrs)	0.03	1.00	-0.39	-0.78	0.23	0.52	0.72	-0.08	0.32	-0.13	-0.11
Time since end of treatment (in yrs)	-0.25	-0.39	1.00	0.49	-0.03	-0.14	-0.17	-0.12	-0.08	0.00	-0.07
Length of treatment (in yrs)	-0.02	-0.78	0.49	1.00	-0.05	-0.34	-0.53	-0.15	-0.03	-0.01	0.01
Achievement Measure	0.03	0.23	-0.03	-0.05	1.00	-0.04	0.31	-0.12	0.14	-0.13	-0.18
Passive control group	0.25	0.52	-0.14	-0.34	-0.04	1.00	0.39	0.13	0.24	-0.03	0.05
Study did not use random assignment	0.16	0.72	-0.17	-0.53	0.31	0.39	1.00	-0.07	0.27	-0.18	-0.07
Any significant differences at baseline	0.12	-0.08	-0.12	-0.15	-0.12	0.13	-0.07	1.00	-0.02	0.14	0.35
Bias was observed in study	-0.06	0.32	-0.08	-0.03	0.14	0.24	0.27	-0.02	1.00	-0.11	0.08
Observational rating	-0.03	-0.13	0.00	-0.01	-0.13	-0.03	-0.18	0.14	-0.11	1.00	-0.08
Other measurement method	0.02	-0.11	-0.07	0.01	-0.18	0.05	-0.07	0.35	0.08	-0.08	1.00
Data collectors not blinded	0.18	0.81	-0.33	-0.67	0.23	0.43	0.66	-0.05	0.15	-0.10	-0.12
Study not from a peer refereed journal	-0.24	0.00	-0.02	0.02	-0.07	-0.20	-0.07	-0.52	-0.04	0.10	-0.27
Treatment on the treated	0.12	0.42	-0.22	-0.17	0.16	0.24	0.27	-0.18	0.25	-0.19	-0.17
Baseline covariates not included	0.31	-0.07	-0.02	0.22	0.18	-0.02	0.09	-0.11	0.06	-0.02	-0.27
High attrition (> .25)	-0.27	-0.47	0.23	0.39	-0.09	-0.41	-0.45	-0.01	0.16	0.19	0.03
Medium attrition (.16 - .25)	-0.17	0.18	0.16	-0.06	-0.08	0.29	0.01	-0.18	-0.05	-0.03	-0.13
Attrition information missing	0.35	0.23	-0.30	-0.20	0.27	-0.03	0.34	0.17	0.02	-0.15	0.26
Low reliability (< .88)	-0.19	0.31	0.09	-0.21	-0.11	0.29	0.14	-0.14	0.19	-0.05	-0.10
Missing Reliability	0.12	-0.07	-0.03	0.16	0.24	-0.13	-0.01	-0.07	-0.04	-0.04	-0.09
Study conducted before 1980	-0.11	0.33	0.09	-0.28	-0.31	0.47	0.12	-0.01	0.05	-0.10	-0.19
>50%Black	0.07	0.11	0.11	-0.04	-0.03	0.21	0.18	0.01	-0.11	0.06	-0.15
>50%Hispanic	-0.06	0.09	-0.03	-0.10	0.13	-0.02	0.05	-0.08	-0.15	0.01	-0.09
Race Missing or Other	0.18	0.00	-0.03	-0.02	0.06	-0.26	0.25	-0.24	-0.16	-0.03	-0.15
No Racial Group > 50%	-0.04	-0.23	-0.08	0.18	-0.15	0.06	-0.44	0.30	0.22	-0.01	0.33
%Low Income (>90.8%)	0.07	-0.17	-0.03	0.28	-0.12	0.07	-0.30	-0.25	-0.10	-0.05	-0.21
% Low Income Missing	0.11	-0.05	0.00	0.01	0.13	-0.45	0.17	-0.32	-0.41	-0.10	-0.12
% Male	-0.08	0.04	-0.02	-0.02	-0.15	0.48	-0.18	0.37	0.39	0.11	0.13
Missing Gender	-0.08	0.25	0.02	-0.24	-0.12	0.02	0.16	-0.45	-0.15	-0.15	-0.21
Any Family Support Services	-0.07	-0.18	0.13	0.22	0.03	0.08	-0.09	0.20	0.43	0.04	0.16
Any Family Support Services Missing	0.08	0.17	-0.08	-0.20	0.03	-0.23	0.25	-0.14	-0.32	-0.05	-0.14

	Data collectors not blinded	Study not from a peer refereed journal	Treatment on the treated	Baseline covariates not included	High attrition (> .25)	Medium attrition (.16 - .25)	Attrition information missing	Low reliability (< .88)	Missing Reliability	Study conducted before 1980
Effect size	0.18	-0.24	0.12	0.31	-0.27	-0.17	0.35	-0.19	0.12	-0.11
Starting age of treatment (in yrs)	0.81	0.00	0.42	-0.07	-0.47	0.18	0.23	0.31	-0.07	0.33
Time since end of treatment (in yrs)	-0.33	-0.02	-0.22	-0.02	0.23	0.16	-0.30	0.09	-0.03	0.09
Length of treatment (in yrs)	-0.67	0.02	-0.17	0.22	0.39	-0.06	-0.20	-0.21	0.16	-0.28
Achievement Measure	0.23	-0.07	0.16	0.18	-0.09	-0.08	0.27	-0.11	0.24	-0.31
Passive control group	0.43	-0.20	0.24	-0.02	-0.41	0.29	-0.03	0.29	-0.13	0.47
Study did not use random assignment	0.66	-0.07	0.27	0.09	-0.45	0.01	0.34	0.14	-0.01	0.12
Any significant differences at baseline	-0.05	-0.52	-0.18	-0.11	-0.01	-0.18	0.17	-0.14	-0.07	-0.01
Bias was observed in study	0.15	-0.04	0.25	0.06	0.16	-0.05	0.02	0.19	-0.04	0.05
Observational rating	-0.10	0.10	-0.19	-0.02	0.19	-0.03	-0.15	-0.05	-0.04	-0.10
Other measurement method	-0.12	-0.27	-0.17	-0.27	0.03	-0.13	0.26	-0.10	-0.09	-0.19
Data collectors not blinded	1.00	-0.09	0.38	-0.05	-0.51	0.05	0.29	0.13	0.02	0.26
Study not from a peer refereed journal	-0.09	1.00	-0.05	0.02	0.30	0.00	-0.34	0.17	-0.03	0.18
Treatment on the treated	0.38	-0.05	1.00	0.20	-0.26	0.00	0.15	0.06	0.05	0.15
Baseline covariates not included	-0.05	0.02	0.20	1.00	-0.09	-0.15	0.33	-0.10	0.18	-0.11
High attrition (> .25)	-0.51	0.30	-0.26	-0.09	1.00	-0.30	-0.38	-0.04	-0.04	-0.16
Medium attrition (.16 - .25)	0.05	0.00	0.00	-0.15	-0.30	1.00	-0.26	0.56	-0.36	0.41
Attrition information missing	0.29	-0.34	0.15	0.33	-0.38	-0.26	1.00	-0.23	0.32	-0.40
Low reliability (< .88)	0.13	0.17	0.06	-0.10	-0.04	0.56	-0.23	1.00	-0.72	0.48
Missing Reliability	0.02	-0.03	0.05	0.18	-0.04	-0.36	0.32	-0.72	1.00	-0.43
Study conducted before 1980	0.26	0.18	0.15	-0.11	-0.16	0.41	-0.40	0.48	-0.43	1.00
>50%Black	0.12	-0.13	0.09	-0.06	-0.17	0.16	-0.28	0.04	-0.17	0.27
>50%Hispanic	0.16	0.09	0.01	-0.24	0.02	-0.14	-0.18	-0.07	0.13	-0.05
Race Missing or Other	0.13	0.19	0.04	0.49	-0.15	-0.10	0.33	-0.10	0.13	-0.12
No Racial Group > 50%	-0.33	-0.18	-0.14	-0.17	0.15	0.06	0.12	0.02	-0.02	-0.14
%Low Income (>90.8%)	-0.11	0.03	0.17	0.01	-0.09	-0.05	-0.44	-0.28	0.14	0.08
% Low Income Missing	0.05	0.10	-0.02	0.34	-0.26	-0.02	0.55	-0.07	0.15	-0.27
% Male	-0.05	-0.15	-0.01	-0.32	0.22	0.03	-0.50	0.08	-0.17	0.29
Missing Gender	0.26	0.31	0.04	0.10	-0.27	0.34	0.13	0.36	-0.13	0.37
Any Family Support Services	-0.23	-0.25	-0.16	-0.32	0.34	-0.06	-0.25	0.14	-0.22	-0.14
Any Family Support Services Missing	0.22	0.24	0.13	0.43	-0.30	-0.10	0.37	-0.11	0.18	0.03

	>50% Black	>50% Hispanic	Race Missing or Other	No Racial Group > 50%	%Low Income (>90.8%)	% Low Income Missing	% Male	Missing Gender	Any Family Support Services	Any Family Support Services Missing
Effect size	0.07	-0.06	0.18	-0.04	0.07	0.11	-0.08	-0.08	-0.07	0.08
Starting age of treatment (in yrs)	0.11	0.09	0.00	-0.23	-0.17	-0.05	0.04	0.25	-0.18	0.17
Time since end of treatment (in yrs)	0.11	-0.03	-0.03	-0.08	-0.03	0.00	-0.02	0.02	0.13	-0.08
Length of treatment (in yrs)	-0.04	-0.10	-0.02	0.18	0.28	0.01	-0.02	-0.24	0.22	-0.20
Achievement Measure	-0.03	0.13	0.06	-0.15	-0.12	0.13	-0.15	-0.12	0.03	0.03
Passive control group	0.21	-0.02	-0.26	0.06	0.07	-0.45	0.48	0.02	0.08	-0.23
Study did not use random assignment	0.18	0.05	0.25	-0.44	-0.30	0.17	-0.18	0.16	-0.09	0.25
Any significant differences at baseline	0.01	-0.08	-0.24	0.30	-0.25	-0.32	0.37	-0.45	0.20	-0.14
Bias was observed in study	-0.11	-0.15	-0.16	0.22	-0.10	-0.41	0.39	-0.15	0.43	-0.32
Observational rating	0.06	0.01	-0.03	-0.01	-0.05	-0.10	0.11	-0.15	0.04	-0.05
Other measurement method	-0.15	-0.09	-0.15	0.33	-0.21	-0.12	0.13	-0.21	0.16	-0.14
Data collectors not blinded	0.12	0.16	0.13	-0.33	-0.11	0.05	-0.05	0.26	-0.23	0.22
Study not from a peer refereed journal	-0.13	0.09	0.19	-0.18	0.03	0.10	-0.15	0.31	-0.25	0.24
Treatment on the treated	0.09	0.01	0.04	-0.14	0.17	-0.02	-0.01	0.04	-0.16	0.13
Baseline covariates not included	-0.06	-0.24	0.49	-0.17	0.01	0.34	-0.32	0.10	-0.32	0.43
High attrition (> .25)	-0.17	0.02	-0.15	0.15	-0.09	-0.26	0.22	-0.27	0.34	-0.30
Medium attrition (.16 - .25)	0.16	-0.14	-0.10	0.06	-0.05	-0.02	0.03	0.34	-0.06	-0.10
Attrition information missing	-0.28	-0.18	0.33	0.12	-0.44	0.55	-0.50	0.13	-0.25	0.37
Low reliability (< .88)	0.04	-0.07	-0.10	0.02	-0.28	-0.07	0.08	0.36	0.14	-0.11
Missing Reliability	-0.17	0.13	0.13	-0.02	0.14	0.15	-0.17	-0.13	-0.22	0.18
Study conducted before 1980	0.27	-0.05	-0.12	-0.14	0.08	-0.27	0.29	0.37	-0.14	0.03
>50%Black	1.00	-0.15	-0.26	-0.44	0.29	-0.20	0.19	-0.21	-0.04	-0.01
>50%Hispanic	-0.15	1.00	-0.16	-0.27	0.13	-0.12	0.08	-0.12	0.13	-0.09
Race Missing or Other	-0.26	-0.16	1.00	-0.46	-0.23	0.74	-0.73	0.54	-0.46	0.61
No Racial Group > 50%	-0.44	-0.27	-0.46	1.00	-0.05	-0.34	0.37	-0.21	0.31	-0.40
%Low Income (>90.8%)	0.29	0.13	-0.23	-0.05	1.00	-0.30	0.30	-0.23	-0.06	-0.14
% Low Income Missing	-0.20	-0.12	0.74	-0.34	-0.30	1.00	-0.99	0.54	-0.51	0.64
% Male	0.19	0.08	-0.73	0.37	0.30	-0.99	1.00	-0.51	0.49	-0.62
Missing Gender	-0.21	-0.12	0.54	-0.21	-0.23	0.54	-0.51	1.00	-0.51	0.47
Any Family Support Services	-0.04	0.13	-0.46	0.31	-0.06	-0.51	0.49	-0.51	1.00	-0.82
Any Family Support Services Missing	-0.01	-0.09	0.61	-0.40	-0.14	0.64	-0.62	0.47	-0.82	1.00

Appendix Table 3: Full Model Regression Coefficients for Table 2

	Coefficient	Standard Error	
Intercept	0.297	0.258	
Starting age of treatment (in yrs)	-0.057	0.036	
Length of treatment (in yrs)	-0.085	0.026	***
Quality index	0.021	0.028	
Achievement Measure	0.001	0.017	
Measurement method -- observational rating	0.008	0.036	
Measurement method -- other measurement method	-0.085	0.047	+
Treatment on the treated	-0.034	0.066	
Study conducted before 1980	0.070	0.090	
Race: >50% Black	0.193	0.104	+
Race: >50% Hispanic	0.085	0.114	
No Racial Group > 50%	0.084	0.111	
Race Missing or Other	0.271	0.127	*
% Low Income (>90.8%)	0.010	0.097	
% Male	0.346	0.285	
Any Family Support Services	-0.098	0.079	
Research/Demonstration Programs	0.072	0.120	

Appendix Table 4: Full Model Regression Coefficients for Table 3

	Coefficient	Standard Error	
Intercept	0.328	0.255	
Starting age of treatment (in yrs)	-0.048	0.036	
Length of treatment (in yrs)	-0.064	0.012	***
Quality index	0.014	0.025	
Achievement Measure	-0.012	0.015	
Measurement method -- observational rating	0.007	0.036	
Measurement method -- other measurement method	-0.104	0.045	*
Treatment on the treated	-0.030	0.061	
Study conducted before 1980	0.084	0.093	
Race: >50% Black	0.167	0.096	+
Race: >50% Hispanic	0.089	0.101	
No Racial Group > 50%	0.052	0.107	
Race Missing or Other	0.206	0.122	+
% Low Income (>90.8%)	0.019	0.100	
% Male	0.247	0.287	
Any Family Support Services	-0.093	0.078	
Research/Demonstration Programs	0.086	0.115	

Appendix Table 5. Hierarchical Linear Models of Program Impacts and Starting Age of Treatment Expressed As a Set of Dummy Variables

	End-of-treatment effect sizes			
	Model 1	Model 2	Model 3	Model 4
Length of treatment (in yrs)	-.073 **	-.065 **	-.085 ***	-.085 ***
	(.025)	(.024)	(.026)	(.026)
Starting age of treatment (in yrs)	-.072 *		-.057	
	(.030)		(.036)	
Birth		.352 +		.213
		(.185)		(.192)
Less than 2 years old		.179		.152
		(.128)		(.138)
2 to 3 years old		.571 **		.514 **
		(.190)		(.189)
3 to 4 years old		.010		-.056
		(.083)		(.087)
> 4 years old		*Reference*		*Reference*
Controls	No	No	Yes	Yes
Number of effect sizes	407			
Number of contrasts	65			
Number of studies	43			

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 6. Random Effect Models of Program Impacts with Interactions with Time Since End-of-Treatment (EOT): EOT and Follow-up Effect Sizes

	Model 1	
Starting age of treatment (in yrs)	-0.060	*
	[0.03]	
Length of treatment (in yrs)	-0.075	**
	[0.028]	
Time since end of treatment (in yrs)	-0.006	**
	[0.002]	
Length * Starting age	0.004	
	[0.014]	
Quality Index	Yes	
Covariates	Yes	
Variance Accounted for by Contrast	2.2% - 2.4%	
Variance Accounted for by Study	2.2% - 2.4%	
Number of Effect Sizes	993	
Number of Contrasts	114	
Number of Studies	67	

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) Regressions are weighted by the inverse variance weight of each effect size multiplied by the inverse of the number of effect sizes within a program. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 7. Sensitivity Check: "End-of-treatment" Is Defined As Around End Of The Intervention With The Radius Of 10% Of The Program Duration. Hierarchical Linear Models Of Program Impacts And Treatment Starting Age and Length: End-of-Treatment Effect Sizes Only

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Starting Age of Treatment (in yrs)	-0.008	-0.016	-0.012				-0.042
	[0.031]	[0.034]	[0.043]				[0.053]
Length of Treatment (in yrs)				0.002	0.008	-0.031	-0.061
				[0.036]	[0.037]	[0.047]	[0.059]
Quality Index	No	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes
Variance Accounted for by Contrast	2.6%	2.5% - 3.4%	2.0% - 3.0%	2.6%	2.4% - 3.4%	2.1% - 3.0%	2.0% - 3.0%
Variance Accounted for by Study	4.3%	4.0% - 4.4%	1.9% - 2.6%	4.2%	4.0% - 4.4%	1.7% - 2.7%	1.5% - 2.6%
Number of Effect Sizes				266			
Number of Contrasts				42			
Number of Studies				27			

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 10% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 8. Sensitivity Check: "End-of-treatment" Is Defined As Around End Of The Intervention With The Radius Of 10% Of The Program Duration. Hierarchical Linear Models Of Program Impacts And Treatment Starting Age and Length: During-Treatment and End-of-Treatment Effect Sizes

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Starting Age of Treatment (in yrs)	-0.026	-0.023	-0.007				-0.042
	[0.029]	[0.032]	[0.041]				[0.041]
Length of Treatment (in yrs)				-0.048 ***	-0.049 ***	-0.054 ***	-0.056 ***
				[0.011]	[0.012]	[0.012]	[0.013]
Quality Index	No	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes
Variance Accounted for by Contrast	2.8%	2.8% - 3.0%	2.6% - 3.6%	2.9%	2.5% - 3.0%	2.5% - 3.2%	2.4% - 3.4%
Variance Accounted for by Study	4.5%	4.2% - 5.2%	2.7% - 4.1%	4.7%	4.8% - 5.6%	2.3% - 3.3%	2.2% - 3.6%
Number of Effect Sizes				384			
Number of Contrasts				51			
Number of Studies				34			

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 10% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 9. Sensitivity Check: "End-of-treatment" Is Defined As Around End Of The Intervention With The Radius Of 10% Of The Program Duration. Hierarchical Linear Models of Program Impacts and Assessment Time since End of Treatment.

	End-of-treatment and post-treatment effect sizes			
	Model 1	Model 2	Model 3	Model 4
Starting age of treatment (in yrs)	-0.061 *	-0.055 +	-0.051 +	-0.048 +
	[0.028]	[0.028]	[0.029]	[0.029]
Length of treatment (in yrs)	-0.030	-0.032	-0.054	-0.055
	[0.037]	[0.037]	[0.036]	[0.036]
Time since end of treatment (in yrs)	-0.005 **		-0.005 **	
	[0.002]		[0.002]	
At the end of treatment				
0 to 1 yr beyond treatment		-0.115 ***		-0.101 ***
		[0.025]		[0.027]
1 to 2 yrs beyond treatment		-0.138 ***		-0.134 ***
		[0.025]		[0.026]
2 to 4 yrs beyond treatment		-0.149 ***		-0.140 ***
		[0.03]		[0.031]
> 4 yrs beyond treatment		-0.138 ***		-0.137 ***
		[0.023]		[0.023]
Quality Index	No	No	Yes	Yes
Covariates	No	No	Yes	Yes
Variance Accounted for by Contrast	3.2%	3.2%	2.4% - 2.5%	2.3% - 2.5%
Variance Accounted for by Study	3.4%	3.4%	2.2% - 2.4%	2.2% - 2.4%
Number of Effect Sizes	952			
Number of Contrasts	110			
Number of Studies	64			

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) Regressions are weighted by the inverse variance of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 10. Hierarchical Linear Models of Program Impacts and Treatment Starting Age and Length, Only Larger Studies Included: End-of-Treatment Effect Sizes Only.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Starting Age of Treatment (in yrs)	0.038	0.03	0.07				-0.015
	[0.03]	[0.033]	[0.074]				[0.082]
Length of Treatment (in yrs)				-0.009	0.001	-0.216 *	-0.229 +
				[0.039]	[0.04]	[0.097]	[0.132]
Quality Index	No	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes
Variance Accounted for by Contrast	1.2%	1.2% - 1.4%	2.0% - 2.8%	1.5%	1.4% - 1.8%	1.5% - 2.1%	1.8% - 2.5%
Variance Accounted for by Study	2.7%	2.8% - 3.4%	3.9% - 6.0%	3.4%	3.3% - 3.8%	1.7% - 3.4%	1.8% - 4.1%
Number of Effect Sizes	120						
Number of Contrasts	21						
Number of Studies	13						

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 11. Hierarchical Linear Models of Program Impacts and Treatment Starting Age and Length, Only Larger Studies Included: During-Treatment and End-of-Treatment Effect Sizes.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Starting Age of Treatment (in yrs)	-0.005	0.001	0.03				-0.003
	[0.029]	[0.031]	[0.042]				[0.041]
Length of Treatment (in yrs)				-0.047 ***	-0.047 ***	-0.057 ***	-0.057 ***
				[0.012]	[0.013]	[0.013]	[0.014]
Quality Index	No	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes
Variance Accounted for by Contrast	1.6%	1.6% - 1.8%	1.3% - 1.6%	1.6%	1.6% - 2.2%	0.9% - 1.3%	1% - 1.4%
Variance Accounted for by Study	3.1%	3.1% - 3.5%	1.1% - 2.1%	3.0%	3.0% - 3.3%	0.5% - 1.6%	0.8% - 1.7%
Number of Effect Sizes	176						
Number of Contrasts	26						
Number of Studies	16						

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 12. Hierarchical Linear Models of Program Impacts and Assessment Time since End of Treatment, Only Larger Studies Included

	End-of-treatment and post-treatment effect sizes			
	Model 1	Model 2	Model 3	Model 4
Starting age of treatment (in yrs)	0.001	0.000	0.015	0.014
	[0.036]	[0.034]	[0.032]	[0.036]
Length of treatment (in yrs)	0.020	0.008	0.004	-0.004
	[0.047]	[0.045]	[0.042]	[0.049]
Time since end of treatment (in yrs)	-0.003		-0.004 +	
	[0.002]		[0.002]	
At the end of treatment				
0 to 1 yr beyond treatment		-0.117 ***		-0.106 ***
		[0.027]		[0.029]
1 to 2 yrs beyond treatment		-0.133 ***		-0.128 ***
		[0.026]		[0.026]
2 to 4 yrs beyond treatment		-0.144 ***		-0.131 ***
		[0.032]		[0.033]
> 4 yrs beyond treatment		-0.122 ***		-0.121 ***
		[0.024]		[0.024]
Quality Index	No	No	Yes	Yes
Covariates	No	No	Yes	Yes
Variance Accounted for by Contrast	1.9%	1.8%	0.9% - 1.1%	1.3% - 1.6%
Variance Accounted for by Study	1.7%	1.5%	0.8% - 0.9%	1.1% - 1.3%
Number of Effect Sizes	577			
Number of Contrasts	55			
Number of Studies	34			

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) Regressions are weighted by the inverse variance of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 13. Random Effect Models of Program Impacts and Treatment Starting Age and Length: vocabulary outcomes grouped in cognitive category. End-of-Treatment Effect Sizes Only.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Starting Age of Treatment (in yrs)	-0.029	-0.021	-0.017				-0.053
	[0.026]	[0.029]	[0.034]				[0.036]
Length of Treatment (in yrs)				-0.046 *	-0.05 *	-0.07 **	-0.083 **
				[0.022]	[0.022]	[0.024]	[0.026]
Quality Index	No	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes
Variance Accounted for by Contrast	2.7%	2.1% - 3.1%	2.2% - 2.5%	3.1%	2.9% - 3.5%	2.2% - 2.5%	2.2% - 2.6%
Variance Accounted for by Study	3.3%	2.9% - 4.1%	2.2% - 2.6%	3.6%	3.2% - 3.9%	2.1% - 2.5%	2.2% - 2.5%
Number of Effect Sizes				407			
Number of Contrasts				65			
Number of Studies				43			

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services. (4) Vocabulary outcomes are grouped in cognitive outcomes - sensitive to instruction.

Appendix Table 14. Random Effect Models of Program Impacts and Treatment Starting Age and Length: vocabulary outcomes grouped in cognitive category. During-Treatment and End-of-Treatment Effect Sizes.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Starting Age of Treatment (in yrs)	-0.035	-0.023	-0.009				-0.043
	[0.027]	[0.029]	[0.035]				[0.036]
Length of Treatment (in yrs)				-0.062 ***	-0.061 ***	-0.059 ***	-0.061 ***
				[0.011]	[0.011]	[0.011]	[0.012]
Quality Index	No	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes
Variance Accounted for by Contrast	3.0%	2.3% - 3.2%	1.6% - 2.7%	2.0%	2.0% - 3.4%	2.4% - 2.6%	1.6% - 2.6%
Variance Accounted for by Study	3.9%	3.8% - 4.7%	2.6% - 4.0%	5.9%	4.1% - 5.9%	2.4% - 2.9%	2.4% - 3.5%
Number of Effect Sizes	484						
Number of Contrasts	68						
Number of Studies	45						

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services. (4) Vocabulary outcomes are grouped in cognitive outcomes - sensitive to instruction.

Appendix Table 15. Random Effect Models of Program Impacts and Assessment Time since End of Treatment: vocabulary outcomes grouped in cognitive category.

	End-of-treatment and post-treatment effect sizes			
	Model 1	Model 2	Model 3	Model 4
Starting age of treatment (in yrs)	-0.078 **	-0.065 **	-0.057 *	-0.050 +
	[0.024]	[0.024]	[0.026]	[0.026]
Length of treatment (in yrs)	-0.063 **	-0.050 *	-0.072 **	-0.059 **
	[0.023]	[0.023]	[0.023]	[0.023]
Time since end of treatment (in yrs)	-0.006 **		-0.006 **	
	[0.002]		[0.002]	
At the end of treatment		*Reference*		*Reference*
0 to 1 yr beyond treatment		-0.125 ***		-0.116 ***
		[0.024]		[0.025]
1 to 2 yrs beyond treatment		-0.144 ***		-0.140 ***
		[0.023]		[0.024]
2 to 4 yrs beyond treatment		-0.155 ***		-0.151 ***
		[0.028]		[0.029]
> 4 yrs beyond treatment		-0.144 ***		-0.143 ***
		[0.022]		[0.022]
Quality Index	No	No	Yes	Yes
Covariates	No	No	Yes	Yes
Variance Accounted for by Contrast	3.1%	3.1%	2.3% - 2.4%	2.2% - 2.3%
Variance Accounted for by Study	3.4%	3.3%	2.2% - 2.4%	2.2% - 2.3%
Number of Effect Sizes	993			
Number of Contrasts	114			
Number of Studies	67			

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) Regressions are weighted by the inverse variance of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services. (4) Vocabulary outcomes are grouped in cognitive outcomes - sensitive to instruction.

Appendix Table 16. Random Effect Models of Program Impacts and Treatment Starting Age and Length: End-of-Treatment Effect Sizes at School-Entry Age

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Starting Age of Treatment (in yrs)	0.014	0.004	doesn't converge				
	[0.02]	[0.028]					
Length of Treatment (in yrs)				-0.009	0.004	doesn't converge	doesn't converge
				[0.023]	[0.032]		
Quality Index	No	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes
Variance Accounted for by Contrast	1.1%	1.0% - 1.3%		1.2%	1.0% - 1.4%		
Variance Accounted for by Study	1.0%	0.9% - 1.5%		1.0%	0.9% - 1.5%		
Number of Effect Sizes				209			
Number of Contrasts				36			
Number of Studies				24			

Note: Effect sizes were at school-entry age (4.75 to 6 years of age). (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services. (4) Vocabulary outcomes are grouped in cognitive outcomes - sensitive to instruction.

Appendix Table 17. Random Effect Models of Program Impacts and Treatment Starting Age and Length: During-Treatment and End-of-Treatment Effect Sizes at School-Entry Age.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Starting Age of Treatment (in yrs)	0.018	0.017	0.079 **				0.151 *
	[0.024]	[0.03]	[0.029]				[0.061]
Length of Treatment (in yrs)				-0.013	-0.011	-0.054 +	0.089
				[0.027]	[0.033]	[0.032]	[0.06]
Quality Index	No	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes
Variance Accounted for by Contrast	1.4%	1.4% - 1.9%	0.3% - 0.7%	1.5%	1.4% - 1.9%	0.4% - 0.8%	0.2% - 0.6%
Variance Accounted for by Study	1.6%	1.6% - 2.0%	0.1% - 0.4%	1.6%	1.6% - 2.0%	0.2% - 0.7%	0.1% - 0.3%
Number of Effect Sizes				232			
Number of Contrasts				42			
Number of Studies				26			

Note: Effect sizes were at school-entry age (4.75 to 6 years of age). (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services. (4) Vocabulary outcomes are grouped in cognitive outcomes - sensitive to instruction.

Appendix Table 18. Random Effect Models of Program Impacts and Assessment Time since End of Treatment: Effect Sizes at School-Entry Age.

	End-of-treatment and post-treatment effect sizes			
	Model 1	Model 2	Model 3	Model 4
Starting age of treatment (in yrs)	0.082	0.053	doesn't converge	doesn't converge
	[0.075]	[0.067]		
Length of treatment (in yrs)	0.087	0.056		
	[0.084]	[0.076]		
Time since end of treatment (in yrs)	0.033			
	[0.061]			
At the end of treatment		*Reference*		
0 to 1 yr beyond treatment		-0.016		
		[0.053]		
1 to 2 yrs beyond treatment		0.409 *		
		[0.163]		
2 to 4 yrs beyond treatment		-0.021		
		[0.285]		
> 4 yrs beyond treatment				
Quality Index	No	No	Yes	Yes
Covariates	No	No	Yes	Yes
Variance Accounted for by Contrast	2.0%	1.9%		
Variance Accounted for by Study	2.1%	2.1%		
Number of Effect Sizes	313			
Number of Contrasts	53			
Number of Studies	36			

Note: Effect sizes were at school-entry age (4.75 to 6 years of age).(1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) Regressions are weighted by the inverse variance of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 19. Random Effect Models of Program Impacts and Interactions between Timing Variables and Cognitive Outcomes: End-of-Treatment Effect Sizes

	Model 1	Model 2	Model 3	Model 4	Model 5
Academic Achievement	0.008	0.007	0.009	0.01	0.217 *
	[0.03]	[0.031]	[0.027]	[0.029]	[0.107]
Starting Age of Treatment (in yrs)	-0.028	-0.018			-0.047
	[0.026]	[0.034]			[0.036]
Academic Achievement * Starting Age of Treatment (in yrs)	-0.004	-0.003			-0.041 *
	[0.009]	[0.009]			[0.02]
Length of Treatment (in yrs)			-0.043 +	-0.069 **	-0.065 *
			[0.023]	[0.025]	[0.028]
Academic Achievement * Length of Treatment (in yrs)			-0.006	-0.006	-0.052 *
			[0.011]	[0.012]	[0.026]
Quality Index	No	Yes	No	Yes	Yes
Covariates	No	Yes	No	Yes	Yes
Variance Accounted for by Contrast	2.7%	2.1% - 2.5%	3.1%	2.2% - 2.5%	2.2% - 2.6%
Variance Accounted for by Study	3.3%	2.2% - 2.6%	3.6%	2.1% - 2.6%	2.2% - 2.7%
Number of Effect Sizes			407		
Number of Contrasts			65		
Number of Studies			43		

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 20. Random Effect Models of Program Impacts and Interactions between Timing Variables and Cognitive Outcomes: During-Treatment and End-of-Treatment Effect Sizes

	Model 1	Model 3	Model 4	Model 6	Model 7
Academic Achievement	-0.063 *	-0.063 *	0.039	0.043 +	0.172 *
	[0.026]	[0.026]	[0.025]	[0.026]	[0.081]
Starting Age of Treatment (in yrs)	-0.038	-0.018			-0.042
	[0.027]	[0.035]			[0.037]
Academic Achievement * Starting Age of Treatment (in yrs)	0.015 +	0.016 +			-0.026 +
	[0.008]	[0.008]			[0.015]
Length of Treatment (in yrs)			-0.049 ***	-0.048 ***	-0.042 **
			[0.012]	[0.012]	[0.014]
Academic Achievement * Length of Treatment (in yrs)			-0.027 *	-0.029 **	-0.059 **
			[0.011]	[0.011]	[0.021]
Quality Index	No	Yes	No	Yes	Yes
Covariates	No	Yes	No	Yes	Yes
Variance Accounted for by Contrast	2.9%	1.6% - 2.6%	3.1%	2.2% - 2.5%	1.5% - 2.5%
Variance Accounted for by Study	3.8%	2.7% - 3.9%	4.7%	2.4% - 3.0%	2.4% - 3.7%
Number of Effect Sizes	484				
Number of Contrasts	68				
Number of Studies	45				

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix Table 21. Random Effect Models of Program Impacts and Interactions between Timing Variables and Cognitive Outcomes Since End-of-Treatment.

	End-of-treatment and post-treatment effect sizes			
	Model 1	Model 2	Model 3	Model 4
Academic Achievement	-0.028 *	0.099 ***	-0.026 *	0.102 ***
	[0.012]	[0.02]	[0.012]	[0.021]
Starting Age of Treatment (in yrs)	-0.077 **	-0.065 **	-0.059 *	-0.049 +
	[0.024]	[0.024]	[0.028]	[0.029]
Length of treatment (in yrs)	-0.061 **	-0.046 +	-0.074 **	-0.058 *
	[0.023]	[0.023]	[0.024]	[0.024]
Time since end of treatment (in yrs)	-0.007 **		-0.007 **	
	[0.002]		[0.002]	
Academic Achievement * Time since end of treatment (in yrs)	0.002		0.002	
	[0.003]		[0.003]	
At the end of treatment				
0 to 1 yr beyond treatment		-0.036		-0.023
		[0.027]		[0.028]
Academic Achievement * 0 to 1 yr beyond treatment		-0.196 ***		-0.203 ***
		[0.027]		[0.028]
1 to 2 yrs beyond treatment		-0.086 **		-0.090 **
		[0.027]		[0.027]
Academic Achievement * 1 to 2 yrs beyond treatment		-0.138 ***		-0.135 ***
		[0.029]		[0.03]
2 to 4 yrs beyond treatment		-0.080 *		-0.075 *
		[0.032]		[0.033]
Academic Achievement * 2 to 4 yrs beyond treatment		-0.173 ***		-0.178 ***
		[0.029]		[0.03]
> 4 yrs beyond treatment		-0.130 ***		-0.127 ***
		[0.025]		[0.025]
Academic Achievement * > 4 yrs beyond treatment		-0.061 +		-0.067 *
		[0.032]		[0.033]
Quality Index	No	No	Yes	Yes
Covariates	No	No	Yes	Yes
Variance Accounted for by Contrast	3.2%	3.3%	2.8% - 2.9%	2.9% - 3.0%
Variance Accounted for by Study	3.6%	3.6%	2.8% - 3.0%	2.9% - 3.1%
Number of Effect Sizes	993			
Number of Contrasts	114			
Number of Studies	67			

Note: (1) Standard errors in parentheses. + p<.1, * p <.05, ** p <.01, *** p <.001. (2) "End-of-treatment" is defined as around end of the intervention with the radius of 25% of the program duration. (3) Regressions are weighted by the inverse variance weight of each effect size. Quality index is number of quality factors, including whether the control group was passive, whether the attrition rate was below 16%, whether the study used random assignment, whether there were any significant differences at baseline, whether bias was observed in study, whether the data collectors not blinded, whether the study was published in a peer refereed journal, whether baseline covariates not included, and whether the dependent measure was of high reliability (>= .88). Covariates include whether cognitive measure, measurement method (observational rating, performance test, other measurement method), whether the

estimate is treatment on the treated, whether the study conducted before 1980, whether the study was greater than 50% white, black, Hispanic, of mixed races, or other/missing races, whether the study had greater than 90.8 % low income participants, what percentage of males vs females were in the study, and whether the study had any family support services.

Appendix 1. Keyword Searches in Meta-Analysis.

When determining what studies would be included in our database, we conducted a comprehensive search of the ECE literature from 1960 to 2007. We conducted keyword searches in ERIC, PsycINFO, EconLit, and Dissertation Abstracts databases, resulting in 9,617 documents, as any given program may produce a series of such documents.

Our keyword searches included two rounds. The initial search was meant to capture studies that would have been similar to what Abt had included (mainly programs serving 3 to 5 year-olds). We searched studies published between 2003 and 2007 using the following search terms:
(early childhood education) or (preschool) or (head start) or (pre-k*).

Because we also wanted to locate any additional studies serving 0 to 3 year-olds that were not in Abt's database or were not captured by our above search terms, we used the following terms to search studies published between 1960 and 2007:
early childhood education or preschool or head start or pre-k* or early intervention and toddler* or infant* + child care or childcare and center or daycare or day care or nursery school.

Appendix 2. Calculation of Effect Size Estimates.

Definition of Effect Size

An effect size offers a standardized measure of the magnitude of the difference between two groups. Effect size statistics were developed to enable comparisons of measures in different units; obviously, saying that an intervention is associated with a gain of one unit in an outcome measure means something different if the measure is a ten-point scale with mean of 5 and a standard deviation of two versus if the measure is a two-hundred point scale with a mean of 100 and a standard deviation of 15. Effect sizes enable researchers to compare figures presented in different measures in a meaningful way.

Comment Effect Size Statistics

The most common effect size statistics are Cohen's d, Hedge's g, the correlation coefficient r, and the odds ratio.

Cohen's d simply takes the difference between two group means and standardizes them with division by a common standard deviation. That is,

$$d = \frac{\bar{x}_1 - \bar{x}_2}{s_{pooled}}.$$

Hedge's g makes an adjustment to Cohen's d to account for bias in the d estimator when sample sizes are small. Specifically, it multiplies d by an adjustment factor J: $J = 1 - \frac{3}{4(n_1 + n_2) - 9}$, and g=d*(J).

The **correlation coefficient r** is a very familiar presentation of effect sizes; correlations may be converted into Cohen's d statistics with the following formula:

$$d = \frac{2r}{\sqrt{1 - r^2}}$$

Correlations may also be represented with Fisher's Z statistics, which is an adjustment that avoids certain problems associated with the standard error of the correlation effect size statistic (Lipsey & Wilson, 2001). Fisher's Z is related to the correlation coefficient as follows:

$$Z = 0.5 \ln\left(\frac{1+r}{1-r}\right);$$

Odds ratios or log odds ratios also give a standard form of effect size. These are used for dichotomous outcomes; say y is a dichotomous outcome variable, A (B) represents the count of events (non-events) in group 1, and C (D) represents the count of events (non-events) in group 2.

	Events (y=1)	Non-events (y=0)	Total
Group 1	A	B	N_1=A+B
Group 2	C	D	N_2=C+D

Then the odds ratio is given by $OR = \frac{A*D}{B*C}$. If dichotomous outcomes are represented by proportions of people manifesting the outcome (or event) in each group (i.e., the proportion for whom the indicator on the outcome variable =1), and group sample sizes are known, the event and non-event counts can of course be estimated ($A=p_1*N_1$, $D=(1-p_2)*N_2$, etc.). If any cell frequency equals 0, add 0.5 to each cell and the odds ratio can be calculated using those adjusted numbers. Odds ratios can be converted to Cohen's d with the following formula:

$$d = \ln(OR) * \frac{\sqrt{3}}{\pi}.$$