



Using experimental variation to examine the (co-)development of cognitive and social-emotional skills in early childhood

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How cognitive and social-emotional skills emerges in childhood is a central question in developmental psychology. Despite a proliferation of theory and empirical work, the field has struggled to establish causal effects. We employed a novel approach to test the stability and codevelopment of cognitive and social-emotional skills in early childhood using longitudinal randomized controlled trial data from the nationally representative Head Start Impact Study ($n = 4,667$). Capitalizing on the study's clustered design, we computed treatment effects on both skills for each cluster ($k = 84$). Using meta-analytic techniques, we found that changes to children's cognitive skills persisted at a rate of approximately 40% one year after program end and 30% two years after program end. Changes to social-emotional skills persisted at a rate of approximately 20% at both timepoints, though estimates were statistically non-significant. We did not observe strong evidence of transfer effects from cognitive to social-emotional skills or vice versa, as estimates were generally imprecise and statistically non-significant. However, future work should investigate cognitive to social-emotional transfer processes given that, above and beyond the initial impact of Head Start on social-emotional skills, a 1 SD increase in cognitive skill impacts consistently predicted a $\sim .20$ SD (non-significant) increase in impacts on social-emotional skills one and two years later. A comparison of estimates from our new method and traditional correlational methods indicated that traditional methods produce upwardly biased auto-regressive, but not cross-lagged, estimates. We encourage researchers to use within-study meta-analysis to test psychological theories.

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Using experimental variation to examine the (co-)development of cognitive and social-emotional skills in early childhood

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Author Note

The data needed to reproduce the analyses presented in this manuscript (United States Department of Health and Human Services, 2018) are available, with approval, from the Inter-university Consortium for Political and Social Research (ICPSR) at: <https://www.icpsr.umich.edu/web/ICPSR/studies/36968>. The analytic syntax necessary to replicate these findings will be publicly posted, and linked here, prior to publication. This study was not pre-registered.

Abstract

How cognitive and social-emotional skills emerges in childhood is a central question in developmental psychology. Despite a proliferation of theory and empirical work, the field has struggled to establish causal effects. We employed a novel approach to test the stability and co-development of cognitive and social-emotional skills in early childhood using longitudinal randomized controlled trial data from the nationally representative Head Start Impact Study ($n = 4,667$). Capitalizing on the study's clustered design, we computed treatment effects on both skills for each cluster ($k = 84$). Using meta-analytic techniques, we found that changes to children's cognitive skills persisted at a rate of approximately 40% one year after program end and 30% two years after program end. Changes to social-emotional skills persisted at a rate of approximately 20% at both timepoints, though estimates were statistically non-significant. We did not observe strong evidence of transfer effects from cognitive to social-emotional skills or vice versa, as estimates were generally imprecise and statistically non-significant. However, future work should investigate cognitive to social-emotional transfer processes given that, above and beyond the initial impact of Head Start on social-emotional skills, a 1 *SD* increase in cognitive skill impacts consistently predicted a $\sim .20$ *SD* (non-significant) increase in impacts on social-emotional skills one and two years later. A comparison of estimates from our new method and traditional correlational methods indicated that traditional methods produce upwardly biased auto-regressive, but not cross-lagged, estimates. We encourage researchers to use within-study meta-analysis to test psychological theories.

Keywords: skill building, social-emotional, cognitive, transfer, early childhood, randomized controlled trial, meta-analysis

Using Experimental Variation to Examine the (Co-)development of Cognitive and Social-Emotional Skills in Early Childhood

Questions about the emergence, stability, and co-development of social-emotional and cognitive skills have long motivated developmental research. Indeed, disparities in children's skills are apparent by the time they enter school and persist across development (Reardon, 2011). The field has no shortage of theories about why individual differences in social-emotional and cognitive skills emerge, and the intricate ways in which these skills co-develop across childhood. Despite considerable interest in adjudicating theories and isolating intervention targets, the field has struggled to make definitive progress.

Studying skill development is inherently complex. Isolating causal processes is difficult, as skills develop through interactions among genetics, other skills, relationships with adults and peers, and environmental inputs (Bronfenbrenner, 1992; van Geert, 1994). Most studies have relied on correlational methods that suffer from unobserved variables bias and, consequently, cannot produce reliable estimates of causal dynamics (Berry & Willoughby, 2017; Duncan et al., 2004). Researchers have improved upon traditional correlational methods to generate more internally valid estimates (e.g., the Random-Intercept Cross-Lagged Panel Model; Hamaker et al., 2015), but the causal interpretations of these new models remain controversial (Hamaker, 2026; Lüdtke & Robitzsch, 2022).

Researchers have also turned to the long-run effects of randomized controlled trials to make sense of skill development (RCT; Kim et al., 2024), but it is challenging to gain concrete insights within the context of a single study with static point-in-time impact estimates. Indeed, when an impact on a skill is observed, it is impossible to know which processes produced the

effect, especially when interventions target many skills and potential mechanisms, as is often the case for early childhood interventions (Eronen, 2020).

As a result of these methodological limitations, the answers to essential questions about the basic dynamics of cognitive and social-emotional skill development remain unclear. Namely, to what extent do skills at one point in development causally shape skills at later points of development? And does skill development in one domain causally impact skill development in other domains, suggesting transfer processes?

In an effort to advance our understanding of these fundamental questions, the present study employed a new technique that capitalized on variation in the experimental impacts of the Head Start Impact Study (HSIS), a national evaluation of Head Start early education services for 3- and 4-year-old children. Rather than simply relying on naturally occurring variation in children's cognitive and social-emotional development over time, we leveraged intervention-generated variation in both skills. Because the HSIS randomized individual children to receive or not receive services at Head Start centers, which were clustered within grantees, we treat each grantee as a kind of “mini-experiment,” allowing us to directly compare the longitudinal outcomes for grantees that generated larger impacts on skills against the outcomes of other grantees that generated smaller impacts. We argue that the estimates generated from our approach should be more causally informative than estimates generated from correlational models of child-level data.

Below, we provide a review of the literature on within- and cross-domain skill development, with particular attention to methodological challenges, before providing more detail on the methods we introduced in the present study.

Within-Domain Cognitive and Social-emotional Development

Theories about the development of cognitive and social-emotional skills generally assert that rudimentary skills developed earlier in life lay the foundation for the acquisition of sophisticated skills later in development. Psychologists and economists alike have argued that ‘skills beget skills’ through self-productivity, cascades, and positive feedback loops (Ahmed et al., 2023; Cunha & Heckman, 2007; Masten & Cicchetti, 2010). For example, earlier math competencies are thought to lay the groundwork for more advanced math skills (Sarama & Clements, 2009). Likewise, social (mal-)adjustment at one stage of development is expected to cascade forward, shaping subsequent social functioning (Dodge et al., 1986).

Correlational analyses comprise the bulk of studies examining the causal effects of earlier skills on later skills. Correlational work generally finds that children with stronger earlier cognitive and social-emotional skills go on to demonstrate stronger later functioning (Bailey et al., 2018; Perry et al., 2018), and that stability increases across childhood (Bleidorn et al., 2022; Breit et al., 2024). Confounding is a major limitation of correlational work; it is impossible to disentangle whether observed associations reflect the causal effects of the skills of interest or the influence of other stable or dynamic child and contextual characteristics (i.e., Bailey et al., 2018; Berry & Willoughby, 2017; Rohrer & Lucas, 2020).

A series of studies by Bailey, Watts, and colleagues illustrate how confounding limits the inferences drawn from correlational studies. The team observed that much of the apparent stability in math functioning in childhood reflected stable trait-level variation, and they argued that interventions were unlikely to affect the trait-level variation that produced longitudinal stability in child skills (Bailey et al., 2014; Watts et al., 2017). Accordingly, they found that the initial impacts of a randomized controlled trial (RCT) evaluation of Building Blocks, an early math intervention, on children’s math skills persisted at a rate much lower than that predicted by

correlational estimates, and impacts faded following a pattern predicted by the trait-model explanation of longitudinal development (Bailey et al., 2018).

In the past decade, concerns about whether correlational methods can estimate causal effects have motivated the development of several new longitudinal models that aim to eliminate confounding bias. The Random-Intercept Cross-Lagged Panel Model is one such innovation (Hamaker et al., 2026). Similar to the state-trait work on achievement (Bailey et al., 2014), the model attempts to isolate and control for stable between-person variation in skills across time. And indeed, such models generally yield smaller estimated autoregressive effects than those obtained using traditional correlational methods. As an example, take three recent studies using large samples and the RI-CLPM to examine within-domain associations among early cognitive and social-emotional skills: Fisk & Lombardi (2021), Li et al. (2025), Tamayo et al. (2024). Focusing on early childhood to early elementary-age paths across the three papers, 20 of the 21 RI-CLPM paths testing skill-to-skill associations were smaller than traditional correlational estimates. On average, RI-CLPM estimates were smaller by approximately 40%. Across the three studies, the autoregressive estimate from the RI-CLPM across 1 to 2 years was .37 for math, .29 for language, and .27 for social-emotional skills. Thus, methods like the RI-CLPM appear to reduce bias in correlational estimates, though it remains uncertain whether bias is fully eliminated (Hamaker, 2026; Lüdtke & Robitzsch, 2022).

Beyond the realm of correlational research, researchers have turned to longitudinal evaluations of RCTs to try to understand skill development while overcoming confounding issues (Bailey et al., 2024). By randomizing children to receive, or not receive, an intervention, we can be sure that skill differences between the two groups are due to the intervention itself, not preexisting characteristics. However, it can be challenging to examine skill-building processes in

the context of an RCT. In the Building Blocks early math RCT, for example, the authors observed that some portion of the initial impact of the intervention on math skills persisted and some portion faded (Bailey et al., 2018). While informative, we cannot be sure that the portion of the effect on math skills that persisted was the result of the causal effects of earlier math skills on later math skills; perhaps the intervention also boosted social-emotional skills or activated other mediational processes that caused the observed effect on math at follow-up.

To improve the inferences drawn from intervention evaluations, recent meta-analytic work has pulled together short- and long-run effects from multiple educational interventions to examine the extent to which the initial effects of an intervention correspond to its follow-up effects on the same skill. In effect, these meta-analyses capitalize on between-study variation in the extent to which interventions boosted child skills, rather than individual between-child variation in skills from observational data or static point-in-time impact estimates from a single RCT. For example, Watts et al. (2026) meta-analyzed the initial and follow-up impacts of several RCT evaluations of early childhood math programs and observed .4 correspondence between the initial and 1-year follow-up intervention impacts on math skills, suggesting that about 40% of point-in-time exogenous skill gains appeared to persist through auto-regressive processes. In a large and diverse sample of educational RCTs, Hart et al. (2024) meta-analyzed the stability of RCT-induced changes in cognitive and social-emotional skills and found that approximately 45% of initial intervention impacts on both skill domains persisted in the 6 to 12 months after interventions ended. Although not perfect (indeed, observed associations among intervention impacts across time could be confounded by correlated impacts on other skills or contextual factors), analyses of variation in intervention impacts may be helpful data points for triangulation with other experimental and correlational estimates.

Cross-domain Cognitive and Social-emotional Development

Same-domain skill-building describes only one way that early skill gains may shape later development. Many developmental theories argue that cognitive and social-emotional skills are dynamically interconnected, such that gains in one domain may alter gains in other domains. These cross-domain processes are often described as *transfer processes* or *cross-productivities* (Heckman & Cunha, 2007). Developmental psychologists have produced a great deal of theory articulating the ways that social-emotional and cognitive skills may co-develop in childhood. For example, some argue that stronger self-regulation may equip children to learn in distraction-ridden environments (Blair & Raver, 2015; McClelland et al., 2007) and support their cultivation of positive relationships with peers and teachers, increasing their enjoyment of school and, in turn, motivation to learn (Zins, 2004). Strong self-esteem may also support students to seek academic opportunities (Denham & Brown, 2010). At the same time, cognitive processes, like executive functioning, are argued to lay the foundation for emotional self-regulation by providing children with the cognitive capacity to modulate their behavior (Li et al., 2023; Ursache et al., 2012). Succeeding academically may also increase enjoyment of school and social functioning (Miles & Stipek, 2006). One popular theory articulates the ways that language abilities specifically lay essential foundation for positive social interactions that support the development of social-emotional skills (Chow & Wehby, 2018; Stansbury & Zimmermann, 1999; Vygotsky, 1986).

As is the case for within-domain skill-building processes, most studies examining the co-development of early social-emotional and cognitive skills have used correlational methods. Presumably, the same issues of confounding that apply to within-domain associations apply to cross-domain associations. Interestingly, however, recent work by Orth et al. (2024) suggested

that cross-lagged estimates from traditional CLPM's and RI-CLPM's were not systematically different; on average, cross-lagged estimates from both models were around .08 to .10.

The recent early-childhood RI-CLPM studies we introduced earlier show a similar pattern. Li et al. (2025) observed math and language to social-emotional cross-lagged paths around .10 that did not meaningfully vary across the CLPM and RI-CLPM, and near-zero paths from social-emotional functioning to math and language that were consistently small, yet sometimes larger, in the RI-CLPM. Similarly, Tamayo et al. (2024) observed near-zero cross-lagged paths from language to internalizing and externalizing skills and vice versa, with no clear evidence of bias attenuation from the RI-CLPM. Fisk and Lombardi (2021) found that the RI-CLPM produced smaller cross-lagged paths between math skills, prosocial behaviors, and externalizing behaviors than those estimated through simple bivariate correlations.¹ Additional work is needed to determine whether cross-lagged paths consistently suffer less from between-person bias than auto-regressive paths and, if so, why.

Turning to intervention evidence, some work has examined the effects of interventions targeting one skill domain on non-targeted domains as a way of probing cross-domain transfer processes. For example, Cipriano et al.'s (2023) meta-analysis of social-emotional learning RCT and quasi-experiments observed a $\sim .10$ *SD* impact on academic ability. The result could be interpreted as support for the possibility of social-emotional to cognitive transfer. However, given that treatments are often broad in focus, it is challenging to isolate whether impacts on the skills primarily targeted by interventions are necessarily the cause of effects on non-targeted skills. As such, traditional experimental evidence can only yield limited inferences regarding the

¹ However, Fisk and Lombardi (2021) compared simple bivariate correlations without controls to correlations from the RI-CLPM, whereas the other studies compared correlations with controls to correlations from the RI-CLPM; model differences may simply reflect bias attenuation from the inclusion of controls.

mechanisms underlying effects (Rohrer & Lucas, 2023). Meta-analyses of the correspondence between intervention-generated impacts on different skills across time may help to advance our understanding of transfer processes.

Current Study

Thus, building on the methods introduced in Hart et al. (2024) and Watts et al. (2026), the current study aimed to provide a novel examination of the development of social-emotional and cognitive skills using RCT data from the Head Start Impact Study (HSIS; Puma et al., 2012). In the HSIS, children were randomized to a year of Head Start preschool services delivered in centers nested within 84 larger grantee groups. Capitalizing on the clustered design, we estimated intervention impacts for each grantee. We then applied meta-analytic techniques to test whether grantees that generated larger impacts on social-emotional or cognitive skills at posttest also produced larger impacts on the same, and opposite, skill domains measured consistently at follow-up. In a set of additional analyses, we examined whether the results generated using variation in Head Start impacts aligned with the results generated using individual-child-level variation in skills (i.e., traditional method).

This approach allowed us to employ straightforward cross-lagged-panel modeling in data that contained exogenously generated variation in child skills, rather than naturally occurring variation, to examine the development of social-emotional and cognitive skills. In doing so, we improved upon previous correlational designs by guarding against confounding that would otherwise bias estimates of correlations among child skills. Our approach allowed us to approximate the extent to which stronger earlier cognitive and social-emotional skills causally shaped stronger later cognitive and social-emotional skills by relying on variation in skills generated by randomization to Head Start. In addition to building on non-experimental work that

has addressed similar questions, but is likely susceptible to omitted variables bias, our model builds on experimental work that has examined intervention impacts on non-targeted skill domains without adjustments for interventions' impacts on targeted skills (e.g., to deduce whether SEL interventions improve academic achievement through improved social-emotional functioning or any number of other factors). Although we did not have strong *a priori* hypotheses, we believe that our approach provides important new estimates that can advance our understanding of early childhood developmental processes. We also hope that this approach provides an applicable model for use in future work in other areas of developmental psychology, where experimental data can yield variation in the skills of interest.

Methods

Data

The current study used data from the Head Start Impact Study (HSIS). The HSIS was an RCT evaluation of Head Start services provided in the 2002-2003 school year. The federal government launched Head Start in the 1960s with the ambition of reducing socioeconomic disparities in development with a particular focus on school readiness (Puma et al., 2012). To test the effects of the program, Congress mandated a government-commissioned study to evaluate the primary Head Start function at the time: providing preschool services, as well as medical, dental, mental health supports, and parenting resources, to racially diverse three- and four-year-old children from families with low incomes. Here, we provide a brief overview of the HSIS data relevant to our study with information drawn from the original HSIS reports published by the U.S. Department of Health and Human Services, Administration for Children and Families

(Puma et al., 2010; 2012). The original reports provide additional details on all aspects of study design.

Children ($n = 4,667$) were randomly assigned to receive, or not receive, Head Start slots within 378 Head Start centers. The study investigators aimed to create a nationally representative sample of centers through an intensive multistep process. Only oversubscribed centers (i.e., centers where more families attempted to enroll than there were slots) were included. Within each oversubscribed center, a random sample of children was selected from the applicant pool. From this random sample, children were then randomized to the treatment condition (i.e., offer of a limited Head Start slot) or to the control condition (i.e., no Head Start slot). Random assignment occurred within two cohorts: (1) a three-year-old cohort, and (2) a four-year-old cohort. Both cohorts were randomly assigned to the offer of a single year of Head Start services, after which families with children in the three-year-old cohort were free to seek additional services (which could have included another year of Head Start). Randomization was successful, with few statistically significant differences in baseline characteristics between the treatment and control groups.

Relevant to the current study, centers were nested within 84 “grantee/delegate agencies” (henceforth referred to as “grantees”). Both public and private agencies (e.g., school districts, faith-based institutions, non- and for-profits) served as grantees. Grantees reported to the federal Office of Head Start and were responsible for ensuring that Head Start centers administered high-quality, comprehensive programming that was responsive to their community’s needs.

Thus far, the HSIS has collected six waves of data. In the current study, we used assessments collected on a consistent schedule for children in the three-year-old and four-year-old cohorts to maximize our analytic sample. The assessment waves included: (1) fall of 2002

pre-test assessment at baseline; (2) spring of 2003 posttest assessment at the end of the intervention; (3) spring of 2004 follow-up assessment collected a year after posttest; and (4) spring of 2005 follow-up assessment collected two years after posttest.

Participants

Table 1 presents baseline characteristics for children randomized to the treatment and control groups.² Recall that to be eligible for Head Start, a child's family income was required to fall below the federal poverty line.³ The sample was nearly equally split among Black, White, and Hispanic children. Approximately 25% of children spoke Spanish as their primary language, and close to 70% of families indicated Spanish as the primary language spoken in the home. More than 60% of mothers had a high school degree or higher. In about 50% of families, both parents lived in the home, and approximately 45% of mothers were married. Fewer than 20% of mothers gave birth as teenagers.

Measures

Cognitive and Social-Emotional Composites

The primary variables used in our analyses were social-emotional and cognitive composite scores calculated at pre-test (for use as a covariate), posttest, 1-year follow-up, and 2-year follow-up assessment waves. To form these composites, we first restricted our dataset to the measures that were collected consistently across the posttest and follow-up assessment waves.⁴ Several parent- or teacher-report measures capturing behavioral problems, social skills, learning skills, and child-adult relationship quality composed the social-emotional skill measures,

² We did not use the weights from the original HSIS reports as we were not interested in recovering nationally representative statistics.

³ Some centers allowed up to 10% of enrollees to have incomes above the poverty line (see Puma et al., 2010).

⁴ Other than *Adjustment Scales for Preschool Intervention*, *Parent-child Relationship Scale*, and *Teacher-child Relationship Scale* (only at posttest and follow-up) measures were collected at pre-test, posttest, and follow-up.

including the: (1) total problems score from the *Adapted Child Behavior Checklist*; (2) *Developing Skills Checklist*; (3) *Social Skills and Positive Approaches*; (4) *Adjustment Scales for Preschool Intervention*; (5) *Parent-child Relationship Scale*; and (6) *Teacher-child Relationship Scale*. Direct assessments of language and math performance, as well as parent reports of emerging literacy, composed the cognitive assessments including: (1) the *Peabody Picture Vocabulary Test* in both English and Spanish; (2) the oral comprehension, letter-word identification (in both English and Spanish)⁵, and applied problems subscales from the *Woodcock-Johnson*; and (3) the *Emergent Literacy Scale*. Puma et al. (2012) reports additional details on each assessment.

To generate composite scores, we first standardized each measure using the concurrent control-group standard deviation. We multiplied standardized negatively valenced measures (e.g., behavioral problems) by -1 so that higher scores were consistently indicative of more desirable functioning (e.g., stronger vocabulary, fewer behavioral problems). We then averaged the valence-adjusted standardized cognitive and social-emotional measures to form the composites. If children completed both the English and Spanish assessments, both scores were included in the composite. Across assessment waves, the Cronbach's Alpha ranged from .83 to .87 for the cognitive composites and from .73 to .77 for the social-emotional composites. Finally, we re-standardized the composite scores using the sample-wide control-group standard deviation. Composite scores were generated for participants if they had non-missing data for at least one measure collected at the assessment wave of focus.

(Quasi-)experimental Grantee-level Treatment Impacts

⁵ Both English and Spanish assessments were included in composites for children who completed both.

Rationale. We relied on meta-analytic techniques to examine the extent to which intervention-induced changes to social-emotional and cognitive skills were stable over time and associated with subsequent intervention effects in the opposite domain. In a traditional meta-analytic framework, one could test the associations among intervention impacts by compiling longitudinal intervention impacts from many studies with varied initial effects. In this paradigm, if auto-regressive and cross-domain skill building dynamics are at play, then the magnitude of effects on skill two should be a function of the magnitude of earlier effects on skill two and skill one; studies that generated larger effects on skill two and on skill one should produce larger effects on skill two. Although this kind of meta-analysis would provide useful insights regarding the extent to which intervention-induced changes in children's skills persist and transfer, it is challenging to accomplish in practice. Few interventions conduct follow-up assessments (Watts et al., 2019), and even fewer collect data on *both* social-emotional and cognitive outcomes. Among studies that do, longitudinal assessment schedules are rarely aligned across studies.

In the current study, we applied a similar meta-analytic framework using data from one intervention, the HSIS, instead of many. The cluster-based randomization of children (within centers and grantees) allowed us to compute grantee-level treatment effects on the same social-emotional and cognitive measures that we then analyzed using meta-analytic techniques demonstrated in Watts et al. (2024).⁶ We essentially treated each grantee as a small-scale experiment. Given heterogeneity in the initial effects of Head Start across grantees (also observed by Bloom & Weiland, 2015 across centers), we were then able to longitudinally examine the extent to which larger benefits in one skill domain corresponded with larger subsequent benefits in the same and the other skill domain. Critically, variation in treatment

⁶ In the supplement, we provide additional details on our decision to estimate effects at the grantee level as opposed to the center level.

impacts across grantees should be exogeneous and otherwise unrelated to the individual factors that might otherwise cause variation in child skills across time and, thus, our models should be guarded against omitted variables bias.

Estimation Method. First, we estimated treatment impacts on cognitive and social-emotional composites at the grantee level for each assessment wave as follows:

$$zComposite_{ct} = \beta_0 + \beta_1 TX_c + \alpha_g + \gamma_g TX_c + \delta' X_{ct} + \phi' X_c + \varepsilon_{ct} \quad (1)$$

where g indicated grantee, c indicated child, and t indicated assessment wave. In each model, we regressed the wave-specific composite of focus (social-emotional or cognitive) on a dummy variable indicator (TX_c) for the child's treatment status (0 = control; 1 = treatment), a fixed effect for the grantee the child was nested within (α_g), and the interaction between the grantee and the treatment status ($\gamma_g TX_c$). For each outcome wave, we also included time-specific controls for age at the assessment, and binary indicators for whether age was mean-imputed ($\delta' X_{ct}$).

Additionally, we included baseline child controls ($\phi' X_c$). Baseline child controls included pre-test cognitive and social-emotional composites, as well as a host of additional covariates included in the Puma et al. (2012) HSIS report: child sex, child race/ethnicity, child's primary language, primary language spoken at home, primary caregiver's age, whether both biological parents live with the child, whether the biological mother is a recent immigrant, maternal educational attainment, maternal marital status, and whether the mother gave birth as a teenager.⁷

After fitting each linear model, we used a marginal effects function to identify the treatment effect, and associated standard error, for each grantee. Recall that the social-emotional

⁷ We included child age at assessment in our models, and not number of weeks elapsed from 9/1/02 and testing/the parent intervention which Puma et al., (2012) used.

and cognitive composites were standardized using concurrent assessment-specific control-group standard deviations. As such, the grantee-specific impacts can be interpreted as effect sizes measuring the impact of receiving a randomly assigned slot to attend Head Start. Ultimately, this estimation process resulted in a treatment impact estimate for each grantee at each assessment wave for both the social-emotional and cognitive composites.

Assumptions. Importantly, this approach relies on the strong assumption that grantee-level treatment impacts are exogenously generated and not caused by pre-existing differences in achievement or social-emotional functioning between treatment and control children. If this exogeneity assumption were violated (i.e., random assignment did not produce groups with equal outcomes on expectation), associations between cognitive and social-emotional impacts could reflect underlying stability in child characteristics rather than causal transfer processes. We ran several sensitivity analyses to assess the degree to which this assumption held, and to reduce bias caused by any violations. Ultimately, we label our grantee-level impacts as “quasi-experimental” because we cannot entirely rule out baseline differences between the two groups at the grantee level.

Heterogeneity statistics suggested meaningful variation in posttest effects across grantees for both cognitive composites ($\tau = 0.12$) and social-emotional composites ($\tau = 0.11$; see Figure 1). Q-tests indicated that between-grantee variation in the posttest impacts was statistically significant for the cognitive composites ($Q(79) = 108.79, p = .01$), but not the social-emotional composites ($Q(79) = 89.73, p = .19$). At 1- and 2-year follow-ups, estimable heterogeneity dropped to $\tau = 0.07$ ($Q(79) = 83.19, p = .35$) and then $\tau = 0.03$ ($Q(78) = 90.78, p = .15$) for cognitive composites. For the social-emotional composites, estimable between-grantee variation

dropped to $\tau = 0.00$ at 1-year follow-up ($Q(80) = 80.84, p = .45$) and $\tau = 0.00$ at 2-year follow-up ($Q(80) = 74.86, p = .64$).

Data Coverage. Table 2 presents details on data coverage across assessment waves and outcomes. The HSIS originally randomized 4,667 children within 84 grantees. However, the publicly available data excludes children from Puerto Rico. As such, our base sample size included 4,442 children from 83 grantees. Within these 83 grantees, an average of 32 children were randomized to the treatment and 22 were randomized to control.

Of these 83 grantees, we were only able to compute impacts for 78 to 80 grantees (depending on the outcome and assessment wave) that had child-level data for at least one child in both the treatment and the control groups. Across cognitive and social-emotional impacts and assessment waves, 80% to 85% of participants contributed data to our grantee-level impacts ($n = 3,580$ to $3,820$). On average, each grantee-level impact was estimated using non-missing data from 28 to 30 treatment children and 17 to 18 control children, though sample size varied across grantees. Our analytic approach (detailed in the “Analysis” section) weighted grantee-level impacts according to their precision (a function of sample size) such that grantees with few children were down-weighted. In addition, we ran supplemental models that dropped grantees with less than 10 treatment or control children, and models that included only children who consistently provided data at each assessment wave.

Analysis

Using the grantee-level, wave-specific treatment impacts on the cognitive and social-emotional composites, we ran a series of regressions to identify the auto-regressive and cross-lagged associations between social-emotional and cognitive functioning over time. We set out to test the theory that stronger earlier skills support the development of stronger later skills in the

same, and opposite, domain. Table 3 outlines the six independent linear regression models we ran to test for auto-regressive and cross-lagged effects. Consider Figure 2, which depicts Model 1, as an example. In this model, we sought to examine the (1) auto-regressive association between posttest impacts on cognitive skills and 1-year follow-up effects on cognitive skills (represented by “1” in the figure), and (2) cross-lagged association between posttest impacts on social-emotional skills and 1-year follow-up impacts on cognitive skills (represented by “2”).

We ran the models in two steps. First, we fit a model in which we regressed grantee-level cognitive impacts at 1-year follow-up on grantee-level cognitive impacts at posttest. As explained in Hart et al., (2024), the slope from this model provides an estimate of the “conditional persistence rate” for an intervention impact over time. Next, we introduced social-emotional posttest impacts to the model, which enabled us to estimate the cross-lagged association between cognitive and social-emotional skills across time (depicted as “2” in Figure 2), while controlling for the initial impact on cognitive skills. In the context of this full cross-lagged model, the intercept captured the portion of the follow-up effect on cognitive skills that was unexplained by posttest effects on cognitive and social-emotional skills.

Model

We ran these full cross-lagged models with either posttest or 1-year follow-up impacts as the independent variable and 1- or 2-year follow-up effects as the dependent variable. Each model was executed in R using the *metafor* and *clubSandwich* packages as follows:

Level 1:

$$TX_Impact_{kgt} = \beta_{0g} + \beta_1 TX_Impact_{kg(t-x)} + \beta_2 TX_Impact_{-kg(t-x)} + \varepsilon_{kgt} \quad (2)$$

Level 2:

$$\beta_{0g} = \gamma_{00} + u_{0g} \quad (3)$$

where g indicated grantee, t represented assessment wave/time, x represented the time elapsed between assessment waves (one or two years), and k represented whether the outcome was social-emotional or cognitive (with $-k$ indicating the opposite of k). Thus, at Level 1 on the left-hand side of the equation, TX_Impact_{kgt} represented the effect size for grantee g at assessment t (1- or 2-year follow-up) on outcome k . On the right-hand side of the equation, TX_Impact_{-kgt-1} and TX_Impact_{kgt-1} indicated the effect size for the corresponding grantee at a prior assessment wave (posttest or 1-year follow-up) on the opposite skill domain ($-k$) and same skill domain (k), respectively. ε_{kgt} represented error in the estimation of TX_Impact_{kgt} .

We included random intercepts in the model, represented in Level 2. γ_{00} represented the mean intercept across grantees and u_{0g} represented the site-specific deviation in follow-up effects (with an assumed variance of τ^2). Each model employed random-effects weighting for the inverse variances of the dependent variable (TX_Impact_{kgt}).

In the context of the hypothetical model depicted in Figure 2, β_1 represented the autoregressive association between cognitive skills at posttest and follow-up. A larger β_1 would indicate greater persistence of posttest cognitive impacts. β_2 represented the extent to which grantees that generated larger treatment benefits on social-emotional functioning at posttest then observed larger treatment effects on cognitive functioning at follow-up, holding constant the initial impacts of the intervention on cognitive skills. A positive β_2 would indicate cross-lagged transfer. Finally, the cross-grantee average intercept, γ_{00} , represented the portion of the follow-up effect on cognitive skills that was not explained by posttest impacts on either social-emotional or cognitive skills. A larger γ_{00} would indicate that a significant portion of the benefit of an

intervention on cognitive skills at follow-up must have been generated through mechanisms other than initial intervention benefits on cognitive or social-emotional skills.

Transparency and Openness

The data needed to reproduce the analyses presented in this manuscript (United States Department of Health and Human Services, 2018) are available, with approval, from the Inter-university Consortium for Political and Social Research (ICPSR) at: <https://www.icpsr.umich.edu/web/ICPSR/studies/36968>. The analytic syntax necessary to replicate these findings will be publicly posted, and linked here, prior to publication. This study was not pre-registered. ChatGPT (OpenAI, 2026) and Perplexity (Perplexity AI, 2026) were used for troubleshooting statistical code and for minor editing of author-generated text.

Results

Longitudinal Descriptives for Quasi-experimental Impacts

Figure 1 depicts the trajectories of grantee-level social-emotional and cognitive impacts across assessment waves. The grantee-level impacts largely reflected the child-level estimates reported in Puma et al. (2010; 2012). As shown in Table S1, the meta-analytic average of social-emotional impacts was consistently near 0 *SD* at all assessment waves, though estimates ranged from approximately -1 *SD* to 2 *SD* across waves (the middle 80% of the distribution ranged from about $-.50$ *SD* to $.40$ *SD*) suggesting variation across grantees in the extent to which randomization to Head Start affected social-emotional outcomes.⁸ The meta-analytic average of cognitive impacts was $.18$ *SD* at posttest, but faded to approximately 0 *SD* at follow-up, with

⁸ Note that Figure 1 does not depict grantees with less than 10 participants and, hence, does not exactly align with the range of estimates for the full sample (including grantees with less than 10 participants) described here.

impacts ranging from about $-2\ SD$ to $1.50\ SD$ across assessment waves (middle 80% ranged from approximately $-.40\ SD$ to $.50\ SD$).

Table 4 presents the raw, unweighted correlations between social-emotional and cognitive impacts across assessment waves. The associations among social-emotional composite impacts ranged from $r = -.03$ to $.26$ ($p = .02$ to $.82$) across waves. Associations among cognitive composite impacts were larger, ranging from $r = .38$ to $.67$ ($p < .001$). At posttest, we observed little correspondence between cognitive and social-emotional impacts ($r = -.09$, $p = .42$), suggesting that the grantees that generated larger end-of-treatment effects on cognitive skills were not necessarily the same grantees that generated larger posttest impacts on social-emotional skills. Social-emotional and cognitive impacts at 1-year ($r = .22$, $p = .05$) and 2-year ($r = .14$, $p = .21$) follow-up demonstrated greater correspondence. The larger association at follow-up is consistent with the possibility of transfer across skills; however, we cannot determine the direction of transfer between socioemotional and cognitive skills based on these associations, nor can we rule out the possibility of unmeasured mediators affecting both cognitive and social-emotional skill impacts in the post-treatment period.

Within-Domain Associations among Quasi-Experimental Impacts

We first examined the auto-regressive associations among impacts over time, prior to entering cross-domain impacts to our model (see Table 5, columns 1 and 3). Overall, we observed that posttest impacts were predictive of 1- and 2-year follow-up effects. At the 1-year follow-up wave, we observed that posttest impacts persisted at a rate of 38% for cognitive skills ($p = .001$) and 20% for social-emotional skills ($p = .11$). At 2-year follow-up, conditional persistence further reduced to 27% for cognitive impacts ($p = .04$) and to 17% for social-emotional impacts ($p = .14$). 1-year follow-up impacts were very predictive of 2-year follow-up

impacts for cognitive skills (67% persistence of remaining 1-year impact; $p < .001$) and, to a lesser extent, for social-emotional skills (31% of 1-year impact; $p = .004$). Across waves, estimates for cognitive outcomes were more precise and statistically significant than estimates for social-emotional outcomes.

We also observed near-zero, negative intercept effects (on average, $\beta = -.02$) across the models that were statistically non-significant. The estimates indicated that no predictable portion of the observed variation in follow-up impacts was explained by factors other than earlier intervention impacts on cognitive or social-emotional skills; unmeasured mediators did not appear to drive the persistence of cognitive or socioemotional impacts in this study.

When we then introduced cross-domain posttest impacts to these models (Table 5, columns 2 and 4), the auto-regressive slope paths generally demonstrated a slight reduction (by .04 on average). The small decrease indicated that cognitive and social-emotional skill impacts did not share much common variance in predicting follow-up impacts. If anything, the intercepts became more negative, though remained small (on average, $\beta = -.04$) with the inclusion of the cross-domain effects.

Cross-Domain Associations among Quasi-Experimental Impacts

Figure 3 and Table 5 present the results from our analyses examining whether grantees that generated larger initial impacts on social-emotional skills observed larger subsequent follow-up impacts on cognitive skills, and vice versa. Overall, cross-lagged associations ranged from .01 to .24. Cross-lagged estimates were consistently imprecise, and only one path was statistically significant ($p < .05$). Considered together, we found more consistently positive and larger point estimates for cognitive-driven transfer effects (average $\beta_1 = .20$) than social-

emotional-driven transfer effects (average $\beta = .10$), although the precision of our estimates was likely limited by our sample size of ~80 Head Start grantees.

Larger cognitive impacts at posttest predicted larger social-emotional impacts at 1-year follow-up ($\beta = .18, p = .23$) and 2-year follow-up ($\beta = .21, p = .15$). Additionally, larger 1-year follow-up impacts predicted larger social-emotional impacts at 2-year follow-up ($\beta = .21, p = .14$). If taken at face value, our results imply that for every 1 *SD* increase in Head Start's impact on cognitive skills, one would expect a ~.20 *SD* increase in social-emotional performance a year or two later. In the context of the HSIS study, the magnitude of these effects is likely small. For example, compared with a grantee that observed a posttest cognitive impact at the meta-analytic average (.18 *SD*), a grantee that observed a posttest impact 1 standard deviation above the average (.30 *SD*, aligned with the observed $\tau = .12$) would observe a .02 *SD* larger follow-up effect on social-emotional functioning.

Social-emotional impacts were less consistently predictive of cognitive impacts. From posttest to 1-year follow-up, and 1-year to 2-year follow-up, social-emotional effects at time 1 were minimally predictive of cognitive effects at time 2 ($\beta = .01, p = .93$ and $\beta = .04, p = .70$, respectively). Interestingly, in contrast, larger social-emotional posttest impacts were predictive of larger cognitive 2-year follow-up effects ($\beta = .24, p = .03$).

Additional Analyses

Omnibus Model

To increase statistical power and probe the sensitivity of our estimates, we ran a pooled model in which we examined within-domain and cross-domain associations from posttest to 1-year follow-up, 1-year to 2-year follow-up, and posttest to 2-year follow-up simultaneously, with dummy variable controls for the timing of the earlier and later skills. Our results aligned with the

results from our primary models. We observed that later social-emotional skills were similarly predicted by earlier social-emotional skills ($\beta = .13, p = .11$) and cognitive skills ($\beta = .14, p = .15$). In predicting later cognitive skills, however, earlier cognitive skills ($\beta = .34, p < .001$) were more predictive than earlier social-emotional skills ($\beta = .08, p = .28$).

Exploratory Within-Study Comparison of Methods

Next, to probe whether the patterns observed in observational data converge with those drawn from more causally relevant methods, we performed a within-study comparison of the estimates generated through our quasi-experimental grantee-level impacts and those generated through a more traditional correlational analysis. The supplement provides more details on model specifications and Table S2 and Figure S1 depict the results.

Overall, we observed that the auto-regressive paths in the child-level correlational models were, on average, about 10% to 140% larger than those from the quasi-experimental models for both cognitive and social-emotional skills (average = 67% larger, 71% for social-emotional skills and 63% for cognitive skills). In contrast, the cross-lagged paths generated using the correlational methods were not consistently larger than those estimated using our quasi-experimental approach. For cognitive skills, which showed more evidence of cross-lagged transfer in the quasi-experimental data, the cross-lagged paths were about 30% to 60% *smaller* when relying on non-experimental variation (.08 to .15 *SD* as opposed to $\sim .20$ *SD*). For social-emotional skills, the non-experimental cross-lagged associations were consistently observed to be around .05 to .07 *SD*, marking small increases from the estimates observed in two of the three grantee-level estimates, and a decrease from the .24 *SD* cross-lagged coefficient observed at one wave. Consistent with the grantee-level estimates, cognitive skills were more strongly predictive of social-emotional skills in the child-level data than vice versa.

Sensitivity Analyses

We then ran a series of analyses to examine whether the quasi-experimental grantee-level impacts reflected treatment-control differences due to randomization versus endogenous child characteristics. The findings from each sensitivity analysis are summarized here, with additional details provided in the supplemental text and Tables S3 – S8. In brief, we first examined grantee-level baseline balance, and we observed that several grantees showed treatment-control-group imbalance. To probe whether our estimates were biased by grantee-level imbalances, we first ran a model that substituted posttest impacts for pre-test “impacts,” generated prior to random assignment. Had pre-test “impacts” shown similar patterns as the true posttest “impacts,” it would have suggested that our grantee-level impacts may have captured endogenous differences between children in the skill composite variables, as opposed to variation generated through random assignment. However, in support of our assumption that the grantee-level effects captured variation due to random assignment, the results using pre-test “impacts” did not resemble the estimates from our primary models that used posttest impacts. Nonetheless, we ran additional models that further probed the sensitivity of our findings to the: (1) removal of particularly small-sample grantees (for which imbalance is most likely), (2) removal of pre-test demographic and child performance covariates, (3) reliance on the same analytic sample across assessment waves, and (4) inclusion of pre-test “impacts” as covariates in our second-stage meta-analytic models. The auto-regressive associations were largely aligned with our primary estimates. Although the cross-lagged estimates varied across analyses and remained imprecise and not statistically significant, we generally found more compelling support for cognitive to social-emotional transfer than vice versa. Finally, we ran a common-effects model, as a

reasonable alternative to our random-effects approach. Our estimates were nearly identical under this specification.

Discussion

In the present study, we conducted a novel examination of cognitive and social-emotional skill development using data from the HSIS RCT. Whereas most studies of within- and cross-domain skill-building processes rely on individual-level skill variation that is prone to omitted-variable bias, in this study we examined variation in children's skills generated by randomization to the HSIS intervention. Meta-analytic models indicated that cognitive impacts persisted by approximately 40% one year after program end and 30% two years after program end, while social-emotional impacts showed statistically non-significant persistence of approximately 20% at both timepoints. Overall, we did not observe strong support for transfer effects from social-emotional skills to cognitive skills or the opposite; our sample of ~80 grantees likely precluded precise estimates of transfer effects. However, it is worth noting that we did observe that Head Start grantees that generated larger earlier impacts on cognitive skills consistently generated ~.15 to .20 *SD* larger follow-up impacts on social-emotional skills, above and beyond earlier impacts on social-emotional skills. Although not statistically significant, the consistency of this pattern warrants further investigation in future work. Interestingly, in our comparison of estimates from our new method to estimates from traditional correlational modeling techniques, we found more evidence of bias in the auto-regressive estimates from traditional methods than the cross-lagged estimates from traditional methods.

A key innovation of the present study was our examination of exogenously generated variation stemming from the *same intervention* on the *same outcomes* assessed longitudinally.

Certainly, although the same intervention, our model directly relies on variation in the effects of the intervention across grantees, likely generated through differences in implementation and emphasis. We observed that changes in cognitive skills persisted at a rate very similar to that observed in Hart et al.'s (2024) meta-analysis of highly diverse educational programs, as well as Watts et al.'s (2026) analysis of early math interventions. In comparison with Hart et al., social-emotional impact persistence was weaker at 1-year follow-up, but stronger at 2-year follow-up, in the present study. Across studies with different methodological strengths, we are beginning to see an accumulation of evidence suggesting approximately 40% stability of exogenous skill changes one year after intervention end and even less (~20-30%) at two years after intervention end, with more consistency for cognitive skills than social-emotional skills. In contrast with Hart et al. (2024), we observed consistently small intercept effects, suggesting no evidence of unmeasured mediational processes driving follow-up impacts.

Same-skill stability did not appear to be meaningfully explained by posttest impacts on the opposite skill domain. Indeed, within-domain associations among intervention impacts across time minimally changed with the introduction of cross-domain impacts. In other words, we did not find evidence to suggest that, for example, social-emotional impacts mediated persistent follow-up impacts on cognitive outcomes, as has been theorized in the case of prominent early childhood programs (Heckman & Kautz, 2012). Interestingly, we also observed that grantees that generated larger posttest impacts on social-emotional skills were generally not the same grantees that generated larger posttest impacts on cognitive skills ($r = -.09$; in line with Jackson, 2018 and Liu & Loeb, 2021).

For cross-domain effects, estimates were harder to interpret due to imprecision. Generally, we observed that grantees with larger earlier impacts on cognitive skills consistently

produced larger follow-up impacts on social-emotional skills. Importantly, these estimates were not statistically significant; as such, we do not interpret this finding as strong evidence of transfer effects. However, the consistency of the estimates in the context of our method—which should avoid confounding—suggests the possibility of cognitive-to-social-emotional transfer processes that future research should investigate further. Researchers may be surprised that the opposite was not observed. Especially in recent years, there has been enthusiasm for interventions targeting social-emotional skills (Cipriano et al., 2023), with the hope that such interventions may also boost cognitive development. If replicated, our findings indicate the field should not dismiss the important role that directly shaping cognitive skills may play in influencing broad developmental functioning.

Insofar as the consistency in the cross-lagged estimates between cognitive and social-emotional skills is suggestive of transfer dynamics, the findings of the present study may be compatible with the Large Interconnected Network Theory (LINT; Bailey et al., 2024), which articulates how programs might generate long-run effects despite short-term fadeout. LINT argues that initial intervention impacts ripple out to a network of correlated skills, contexts, and relationships that interact over time to form a network-level effect that ultimately shapes adult functioning despite fading effects on targeted child skills. Future examinations of the co-development of skills can provide clarity as to the specific transfer dynamics underlying long-run intervention effects.

Our findings also speak to methodological concerns about the use of correlational approaches to examine skill-building dynamics. We found that while observational autoregressive paths were predictably larger than our quasi-experimental estimates, cross-lagged estimates were not. In line with Orth et al. (2024), our findings indicate that correlational

methods may lead us astray in the expectation of same-domain skills-beget-skills processes, but are potentially less biased in the context of cross-domain transfer. Whether our method eliminated confounding hinges on whether the grantee-level impacts captured variation in child skills due to randomization versus endogenous variation. Our sample was small, and our cross-lagged estimates were noisy. Thus, replication is needed. Triangulating findings from the vast observational literature, growing longitudinal intervention impact literature, and within-study comparisons of the two methods, may help to move the field forward (Bailey et al., 2024).

It is worth considering how measurement differences may have influenced our results. Cognitive skills were measured using direct assessments, while social-emotional skills relied on parent- and teacher-reports, which could differ in reliability and validity. The social-emotional composites did have lower Cronbach's alphas, less heterogeneity, and weaker auto-regressive paths. However, we find it hard to imagine that the social-emotional assessments did not capture *any* meaningful heterogeneity. Indeed, the auto-regressive impacts were consistently greater than zero. Additionally, the social-emotional measures were much like those that other interventions have impacted (e.g., Hart et al., 2024) and those that have shown strong predictive validity in forecasting adult functioning (e.g., Koepp et al., 2023). Nonetheless, larger questions in the field surrounding how to best conceptualize and operationalize social-emotional functioning with theoretical specificity are certainly relevant (Inzlicht et al., 2020; McCoy & Sabol, 2025; Morrison & Grammer, 2016). Poor explication of theoretical constructs may not only limit instruments' reliability and validity (Shadish et al., 2002), but also the ability of an intervention like Head Start to directly target social-emotional development (see Inzlicht & Roberts, 2024). Indeed, we observed that Head Start generated near-zero posttest impacts. The social-emotional measures may have tapped constructs that are hard to define and to change. It is also possible

that social-emotional to cognitive skill transfer processes are real, but much smaller in magnitude than cognitive to social-emotional transfer processes, and, as such, the current study was underpowered to detect such effects.

Conclusion

Longitudinal intervention data may provide a new inroad for triangulating the answers to causal questions about skill stability and transfer. The use of cluster-level RCT data affords unique opportunities toward these ends. We hope future studies will replicate our approach to test dynamics across a wide range of psychological constructs.

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Table 1
Baseline Equivalence on Child and Parent Characteristics

	Control Group		Treatment Group		<i>p</i>
	<i>M (SD) / proportion</i>	<i>n</i>	<i>M (SD) / proportion</i>	<i>n</i>	
Child Characteristics					
Pre-test cognitive composite	-0.01 (1.00)	1796	0.01 (1.04)	2646	0.67
Pre-test social-emotional composite	-0.00 (1.00)	1796	0.00 (1.00)	2646	0.82
Female child	0.49	1796	0.50	2646	0.68
Black child	0.30	1796	0.31	2646	0.69
White child	0.33	1796	0.31	2646	0.28
Hispanic child	0.37	1796	0.38	2646	0.52
Child's primary language is Spanish	0.25	1796	0.26	2646	0.47
Parent Characteristics					
Primary caregivers age	28.65 (7.06)	1796	29.08 (7.52)	2646	0.05
Primary home language is Spanish	0.71	1796	0.70	2646	0.34
Both parents live in the home	0.49	1796	0.50	2646	0.92
Mother is a recent immigrant	0.19	1796	0.20	2646	0.50
Mother has less than a HS degree	0.39	1796	0.37	2646	0.29
Mother has more than a HS degree	0.28	1796	0.29	2646	0.49
Mother has a HS degree	0.33	1796	0.34	2646	0.66
Mother was never married	0.39	1796	0.39	2646	0.74
Mother is currently married	0.45	1796	0.44	2646	0.59
Mother has other relationship status	0.16	1796	0.16	2646	0.82
Missing mother's relationship status	0.00	1796	0.00	2646	0.50
Mother gave birth as a teen	0.18	1796	0.16	2646	0.04

Joint Test

$\chi^2(18) = 10.71, p = 0.83, n = 4,442.$

Notes: To generate the *p* values, each characteristic was regressed on an indicator for treatment using robust standard errors. To provide a joint test of orthogonality for overall treatment-control group differences, a probit model with robust standard errors was executed (with reference groups dropped in the case of maternal education, maternal marital status, and child race/ethnicity). Note that no weights were used in generating these estimates. Also note that there were very few mothers with missing relationship status at baseline, hence the means of zero.

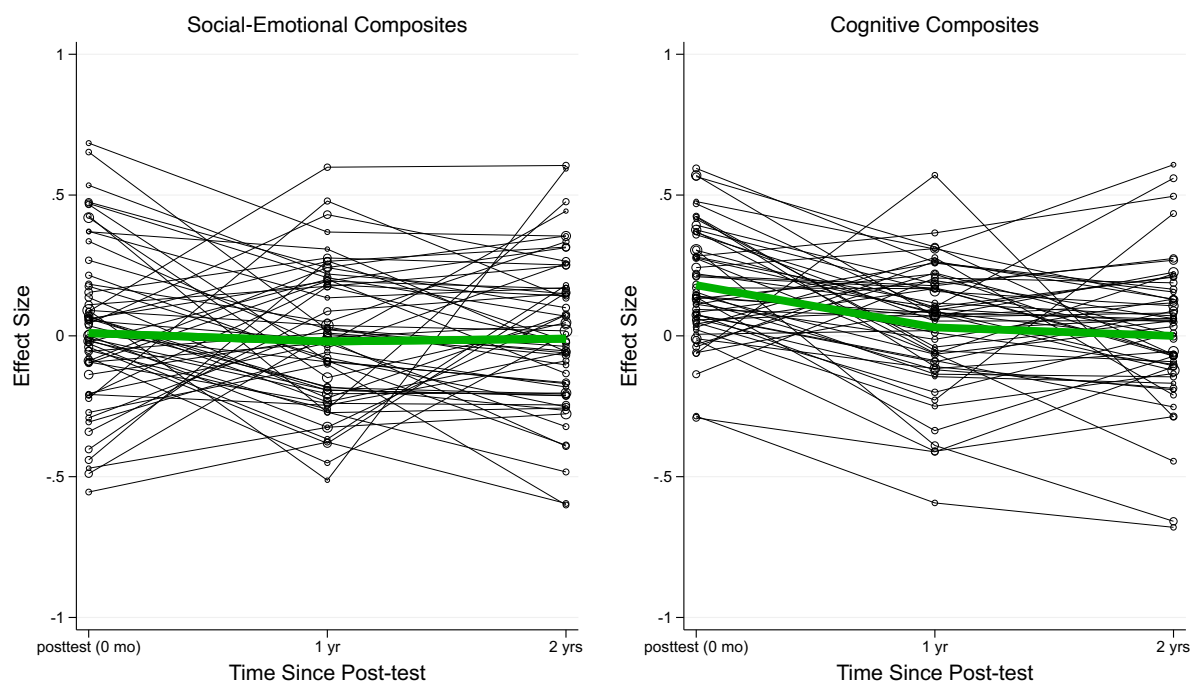
Table 2
Grantee and Child Level Sample Sizes

	Total		Treatment	Control
	<i>n</i>	<i>k</i>	Avg. <i>n</i> in <i>k</i>	Avg. <i>n</i> in <i>k</i>
Randomized Sample	4442	83	32	22
Analytic Sample				
Soc Posttest	3820	80	30	18
Soc 1-year Follow-up	3717	81	28	18
Soc 2-year Follow-up	3721	81	28	18
Cog Posttest	3783	80	29	18
Cog 1-year Follow-up	3668	80	28	18
Cog 2-year Follow-up	3580	79	28	17

Notes: “Soc” = social-emotional, “Cog” = cognitive. This table presents the child (*n*) and grantee (*k*) sample sizes in the original randomized sample and in the analytic sample of grantee-level effects.

Figure 1

Trajectory of Grantee-level Intervention Impacts for Social-emotional and Cognitive Measures

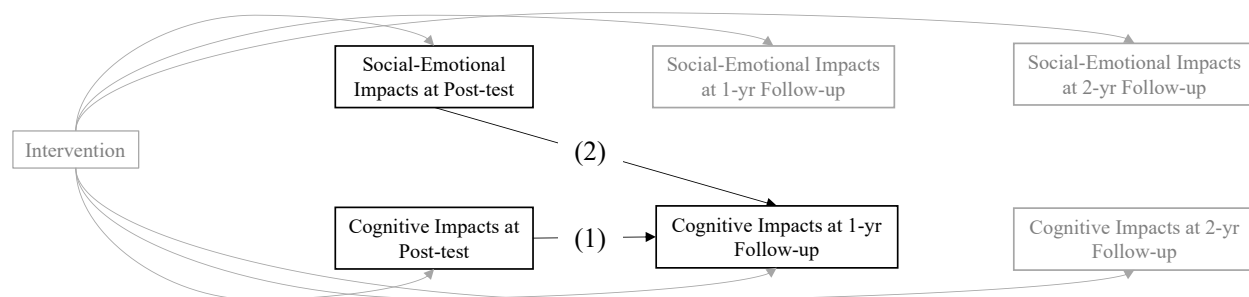


Notes: Each line indicates a grantee-specific trajectory of intervention impacts on the cognitive or social-emotional composite across posttest, 1-year follow-up, and 2-year follow-up. For each grantee, coordinates were weighted by the posttest inverse variances. Grantee-level intervention impacts were computed using a range of baseline child and family characteristics. Green lines indicate the meta-analytic average of grantee-level impacts, computed at each assessment wave in R using the metafor package. Models contained inverse variance weighting and a random intercept for grantee. Grantees with fewer than 10 participants were removed from this figure.

Table 3
Primary Linear Regression Models

Model	
1	$TX_Impact_{cog_d_1yr} = \beta_{0d} + \beta_1 TX_Impact_{cog_d_post} + \beta_2 TX_Impact_{soc_d_post} + \varepsilon_{dt}$
2	$TX_Impact_{cog_d_2yr} = \beta_{0d} + \beta_1 TX_Impact_{cog_d_post} + \beta_2 TX_Impact_{soc_d_post} + \varepsilon_{dt}$
3	$TX_Impact_{cog_d_2yr} = \beta_{0d} + \beta_1 TX_Impact_{cog_d_1yr} + \beta_2 TX_Impact_{soc_d_1yr} + \varepsilon_{dt}$
4	$TX_Impact_{soc_d_1yr} = \beta_{0d} + \beta_1 TX_Impact_{soc_d_post} + \beta_2 TX_Impact_{cog_d_post} + \varepsilon_{dt}$
5	$TX_Impact_{soc_d_2yr} = \beta_{0d} + \beta_1 TX_Impact_{soc_d_post} + \beta_2 TX_Impact_{cog_d_post} + \varepsilon_{dt}$
6	$TX_Impact_{soc_d_2yr} = \beta_{0d} + \beta_1 TX_Impact_{soc_d_1yr} + \beta_2 TX_Impact_{cog_d_1yr} + \varepsilon_{dt}$

Figure 2
Example Model



Notes: This model visualizes Model 1 in Table 3 which aimed to estimate auto-regressive associations among cognitive posttest impacts and cognitive 1-year follow-up impacts and cross-domain impacts between social-emotional posttest impacts and 1-year cognitive follow-up impacts.

Table 4
Correlation Matrix: Grantee-level Treatment Impacts

	Soc Post	Soc 1-year	Soc 2-year	Cog Post	Cog 1-year	Cog 2-year
Soc Post	1.00					
Soc 1-year	0.26*	1.00				
Soc 2-year	0.19+	-0.03	1.00			
Cog Post	-0.09	-0.15	0.06	1.00		
Cog 1-year	-0.09	0.22*	0.07	0.45***	1.00	
Cog 2-year	0.19	0.07	0.14	0.38***	0.67***	1.00

Notes: “Soc” = Social-emotional, “Cog” = Cognitive, “Post” = end of treatment impacts, “1-year” = 1-year follow-up impacts, “2-year” = 2-year follow-up impacts. This matrix reports the correlations between grantee-level treatment impacts.

* $p < .05$, ** $p < .01$, *** $p < .001$

Table 5
Auto-regressive and Cross-domain Dynamics in Grantee-Level Effects

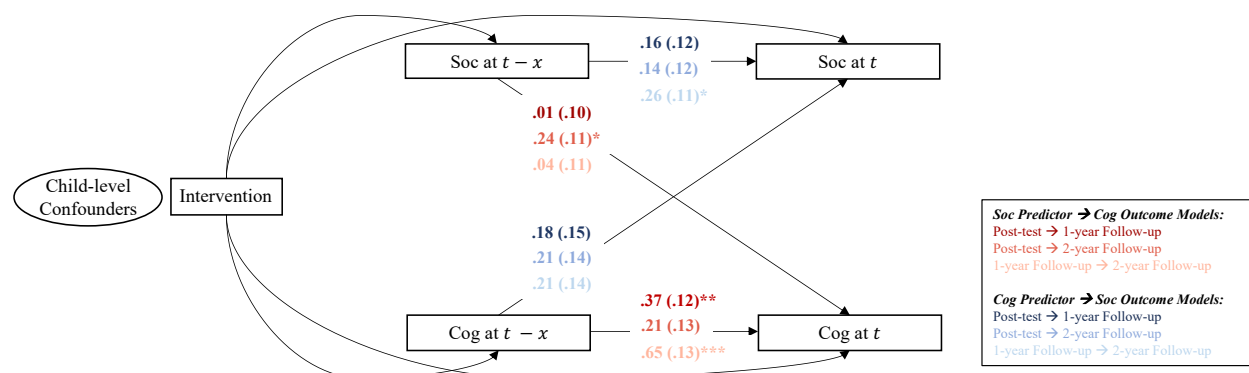
	Outcome: Social-Emotional Composite		Outcome: Cognitive Composite	
	Same-Domain Model (1)	Cross-Domain Model (2)	Same-Domain Model (3)	Cross-Domain Model (4)
Panel A: Posttest Predicting 1-year Follow-up Effects				
Intercept	-0.03 (0.03)	-0.06 (0.04)	-0.03 (0.03)	-0.03 (0.03)
Soc Posttest	0.20 (0.12)	0.16 (0.12)		0.01 (0.10)
Cog Posttest		0.18 (0.15)	0.38 (0.12)***	0.37 (0.12)***
$\tau_{intercept}$	0.04	0.02	0.00	0.00
Observations	80	80	80	80
Panel B: Posttest Predicting 2-year Follow-up Effects				
Intercept	-0.01 (0.03)	-0.05 (0.04)	-0.05 (0.04)	-0.04 (0.04)
Soc Posttest	0.17 (0.12)	0.14 (0.12)		0.24 (0.11)*
Cog Posttest		0.21 (0.14)	0.27 (0.13)*	0.21 (0.13)
$\tau_{intercept}$	0.02	0.00	0.00	0.00
Observations	80	80	79	79
Panel C: 1-year Follow-up Effects Predicting 2-year Follow-up Effects				
Intercept	0.00 (0.03)	-0.01 (0.03)	-0.02 (0.03)	-0.02 (0.03)
Soc 1-year Follow-up	0.31 (0.11)**	0.26 (0.11)*		0.04 (0.11)
Cog 1-year Follow-up		0.21 (0.14)	0.67 (0.13)***	0.65 (0.13)***
$\tau_{intercept}$	0.00	0.00	0.00	0.00
Observations	81	80	79	79

Notes: “Soc” = Social-emotional. “Cog” = Cognitive. Treatment impacts were estimated at the grantee level with controls for baseline child and family characteristics. Same-domain and cross-domain transfer estimates were computed through a series of independent meta-regression models (12 presented in this table) in which grantee-level impacts from a later assessment wave (1- or 2-year follow-up) were regressed on grantee-level impacts from an earlier assessment wave (1-year follow-up or posttest). Models included weighting by the inverse variances of follow-up effects and a random intercept for grantee. Controls used for computing the grantee-level effects included: center-level fixed effects, pre-test cognitive and social-emotional composites, cohort, age at concurrent testing (e.g., in computing posttest impacts, we controlled for age at posttest), child gender, child race, child primary language, primary caregiver’s age, primary language spoken at home, whether both biological parents live with the child, whether the child’s mother is a recent immigrant, mother’s educational attainment, mother’s marital status, and whether the mother gave birth as a teenager.

* $p < .05$, ** $p < .01$, *** $p < .001$

Figure 3

Auto-regressive and Cross-domain Dynamics among Grantee-level Impacts



Notes: Model-based estimates come from Table 5.

Supplemental file for:
Using experimental variation to examine the (co-)development of cognitive and social-emotional skills in early childhood

Grantee-Level Impacts

It is worth noting that concerns about exogeneity shaped our decision to compute effects at the grantee level. Indeed, the HSIS had two levels of clustering: centers ($k = 378$) and grantees ($k = 84$). In our initial conceptualization of these analyses, we had intended to compute center-level treatment impacts. However, we found that these impacts were highly imprecise (and standard errors were often uncorrelated with sample size) because centers often contained very few children (posttest $\bar{n} = 13$). For this reason, we did not proceed with running our analyses using the center-level estimates and, instead, we opted to estimate treatment impacts and run our analyses at the grantee level, for which we observed larger sample sizes and plausible standard error estimates.

Exploratory Within-Study Comparison

For our traditional correlational analysis of skill associations across time, we limited the sample to children from the control group and executed a series of models in which we regressed child-level composites in one domain at follow-up on child-level composites in the opposite skill domain at an earlier assessment wave. We included controls for functioning in the same-skill-domain at the earlier assessment wave, child and family characteristics, and pre-test social-emotional and cognitive composites.

Sensitivity Analyses

The exogeneity assumption is particularly tenuous in low-sample-size grantees. Fortunately, our primary meta-analytic approach inherently addressed this concern by down-weighting less-precise effect sizes from smaller samples via inverse-variance weighting. Additionally, we ran models in which we dropped grantees with treatment and/or control sample sizes smaller than 10 participants, and found substantive patterns that were generally similar to those observed from our primary models (see Table S3). Of note, the auto-regressive paths for social-emotional impacts were generally smaller, and the cross-lagged paths for cognitive impacts predicting social-emotional impacts were generally larger.

Importantly, there were some grantees that failed the joint test for baseline balance and did *not* have abnormally small sample sizes. To further address imbalance in baseline characteristics, we controlled for baseline child and family characteristics, including child social-emotional and cognitive functioning, when we computed our primary grantee-level treatment impacts. As expected, the inclusion of these controls attenuated the autoregressive associations among social-emotional and cognitive skills (see Table S4 for the models without controls). For cross-lagged associations, the inclusion of controls in some cases minimally affected the estimates and in other cases reduced the magnitude of the estimates.

As a further test, we ran models in which we substituted grantee-level posttest impacts for “impacts” estimated at pre-test. If it were the case that the grantee-level treatment impacts reflected nothing more than endogenous child-level differences in cognitive and social-emotional skills, then pre-test differences among the treatment and control groups should be just as predictive of follow-up effects as posttest differences. However, in support of the expectation that the quasi-experimental estimates captured exogenous differences due to the treatment itself, overall, the models using pre-test “impacts” produced auto-regressive and cross-lagged paths that did not resemble estimates from models using posttest impacts (see Table S5).

Given that the grantee-level pre-test “impacts” were not all zero, we then ran our primary cross-lagged models with additional controls for social-emotional and cognitive pre-test

“impacts.” Building from our primary model that used grantee-level effects, estimated with controls for baseline characteristics, we entered both cognitive and social-emotional pre-test “impacts” to test for additional bias due to treatment-control-group differences in skills observed prior to randomization (see Table S6). The auto-regressive and cross-lagged estimates were minimally affected with the inclusion of the pre-test “impacts,” indicating that above and beyond controls for child-level baseline characteristics, treatment-control *differences* in baseline social-emotional and cognitive functioning did not appear to bias our estimates.

Next, we examined extent to which issues of missing data biased our estimates (see Table S7). In our primary grantee-level quasi-experimental impacts, about 80% to 85% of participants contributed data across assessment waves and outcome types. To probe how changes in the contributing sample affected estimates, we ran models in which we limited the sample to participants who contributed cognitive and social-emotional data at each assessment wave ($n = 3,262$ children from 79 grantees). The auto-regressive and social-emotional to cognitive transfer paths were minimally affected, but the cognitive to social-emotional transfer paths were significantly attenuated in two of the three models.

Finally, we tested the consistency of our results when using a common-effects model, a reasonable alternative to our primary random-effects model. The estimates from this model were very similar to that of our primary model (Table S8).

Table S1

Meta-analytic Averages for Cognitive and Social-Emotional Outcomes (B(SE))

	Posttest (1)	1-year Follow-up (2)	2-year Follow-up (3)
Social-Emotional Composite			
Meta-analytic Average	0.01 (0.03)	-0.02 (0.03)	-0.01 (0.03)
$\tau_{intercept}$	0.11	0.00	0.00
Q	Q(79) = 89.73 $p = .19$	Q(80) = 80.84 $p = .45$	Q(80) = 74.86 $p = .64$
Observations	80	81	81
Cognitive Composite			
Meta-analytic Average	0.18 (0.03)***	0.03 (0.03)	0.00 (0.03)
$\tau_{intercept}$	0.12	0.07	0.03
Q	Q(79) = 108.79* $p = .01$	Q(79) = 83.19 $p = .35$	Q(78) = 90.78 $p = .15$
Observations	80	80	79

Notes: This table presents the meta-analytic average of grantee-level treatment impacts on the social-emotional and cognitive composites. Grantee-level intervention impacts were computed using a range of baseline child and family characteristics. Meta-analytic averages were computed at each assessment wave for the social-emotional and cognitive composites, respectively. Models included weighting by the inverse variances of follow-up effects and a random intercept for grantee.

* $p < .05$, ** $p < .01$, *** $p < .001$

Table S2

Auto-regressive and Cross-domain Associations in Child-level Observational Data

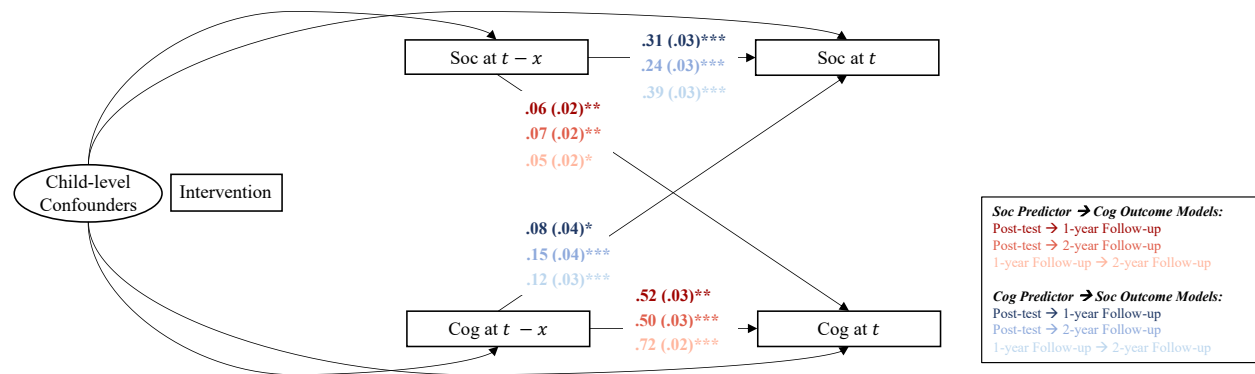
	Outcome: Social-Emotional Composite		Outcome: Cognitive Composite	
	Same-Domain Model	Cross-Domain Model	Same-Domain Model	Cross-Domain Model
	(1)	(2)	(3)	(4)
Panel A: Posttest Predicting 1-year Follow-up Effects				
Intercept	-0.06 (0.04)	-0.06 (0.05)	-0.05 (0.04)	-0.05 (0.04)
Soc Posttest	0.32 (0.03)***	0.31 (0.03)***		0.06 (0.02)**
Cog Posttest		0.08 (0.04)*	0.53 (0.03)***	0.52 (0.03)***
Observations	1329	1322	1308	1305
Panel B: Posttest Predicting 2-year Follow-up Effects				
Intercept	0.00 (0.04)	0.03 (0.04)	0.04 (0.05)	0.03 (0.05)
Soc Posttest	0.25 (0.03)***	0.24 (0.03)***		0.07 (0.02)**
Cog Posttest		0.15 (0.04)***	0.52 (0.03)***	0.50 (0.03)***
Observations	1317	1310	1268	1268
Panel C: 1-year Follow-up Effects Predicting 2-year Follow-up Effects				
Intercept	0.00 (0.03)	0.02 (0.04)	-0.07 (0.03)*	-0.08 (0.03)*
Soc 1-year Follow-up	0.41 (0.02)***	0.39 (0.03)***		0.05 (0.02)*
Cog 1-year Follow-up		0.12 (0.03)***	0.74 (0.02)***	0.72 (0.02)***
Observations	1352	1326	1287	1282

Notes: "Soc"= Social-emotional. "Cog"= Cognitive. Only children from the control group were included in these analyses. The analyses relied on child level data, not estimated treatment impacts. Same-domain and cross-domain transfer estimates were computed through a series of independent regression models (12 presented in this table) in which child-level functioning at a later assessment wave (1- or 2-year follow-up) was regressed on child level functioning at an earlier assessment wave (1-year follow up or posttest). Models included the following controls: pre-test cognitive and social-emotional composites, cohort, age at concurrent testing (for both the predictor and outcome), child gender, child race, child primary language, primary caregiver's age, primary language spoken at home, whether both biological parents live with the child, whether the child's mother is a recent immigrant, mother's educational attainment, mother's marital status, and whether the mother gave birth as a teenager.

* $p < .05$, ** $p < .01$, *** $p < .001$

Figure S1

Auto-regressive and Cross-domain Associations using Observational Data from the Control Group



Notes: Estimates are presented in Table S2.

Table S3

Auto-regressive and Cross-domain Dynamics in Grantee-level Effects with Small Grantees Dropped

	Outcome: Social-Emotional Composite		Outcome: Cognitive Composite	
	Same-Domain	Cross-Domain	Same-Domain	Cross-Domain
	Model (1)	Model (2)	Model (3)	Model (4)
Panel A: Posttest Predicting 1-year Follow-up Effects				
Intercept	-0.02 (0.03)	-0.07 (0.05)	-0.03 (0.04)	-0.03 (0.04)
Soc Posttest	0.09 (0.13)	0.02 (0.14)		-0.04 (0.11)
Cog Posttest		0.29 (0.17)	0.37 (0.13)**	0.38 (0.14)**
$\tau_{intercept}$	0.00	0.00	0.00	0.00
Observations	57	57	57	57
Panel B: Posttest Predicting 2-year Follow-up Effects				
Intercept	-0.01 (0.03)	-0.07 (0.05)	-0.05 (0.04)	-0.04 (0.04)
Soc Posttest	0.12 (0.13)	0.04 (0.13)		0.15 (0.13)
Cog Posttest		0.33 (0.17)	0.33 (0.15)*	0.26 (0.16)
$\tau_{intercept}$	0.03	0.00	0.00	0.00
Observations	58	58	55	55
Panel C: 1-year Follow-up Effects Predicting 2-year Follow-up Effects				
Intercept	0.00 (0.03)	-0.01 (0.03)	-0.01 (0.03)	-0.01 (0.03)
Soc 1-year Follow-up	0.53 (0.14)***	0.46 (0.14)***		0.10 (0.14)
Cog 1-year Follow-up		0.31 (0.16)	0.54 (0.15)***	0.51 (0.15)***
$\tau_{intercept}$	0.00	0.00	0.00	0.00
Observations	57	57	55	55

Notes: Treatment impacts were estimated at the grantee level with controls for baseline child and family characteristics. Grantees with fewer than 10 children in the control or treatment group were not included in these models. Same-domain and cross-domain transfer estimates were computed through a series of independent meta-regression models (12 presented in this table) in which grantee-level impacts from a later assessment wave (1- or 2-year follow-up) were regressed on grantee-level impacts from an earlier assessment wave (1-year follow-up or posttest). Models included weighting by the inverse variances of follow-up effects and a random intercept for grantee. Controls used for computing the grantee-level effects included: center-level fixed effects, pre-test cognitive and social-emotional composites, cohort, age at concurrent testing, child gender, child race, child primary language, primary caregiver's age, primary language spoken at home, whether both biological parents live with the child, whether the child's mother is a recent immigrant, mother's educational attainment, mother's marital status, and whether the mother gave birth as a teenager.

* $p < .05$, ** $p < .01$, *** $p < .001$

Table S4

Auto-regressive and Cross-domain Dynamics in Grantee-level Effects with Minimal Controls

	Outcome: Social-Emotional Composite		Outcome: Cognitive Composite	
	Same-Domain	Cross-Domain	Same-Domain	Cross-Domain
	Model (1)	Model (2)	Model (3)	Model (4)
Panel A: Posttest Predicting 1-year Follow-up Effects				
Intercept	-0.02 (0.04)	-0.03 (0.04)	-0.06 (0.04)	-0.05 (0.04)
Soc Posttest	0.30 (0.12)*	0.28 (0.12)*		0.08 (0.11)
Cog Posttest		0.07 (0.11)	0.51 (0.10)***	0.49 (0.10)***
$\tau_{intercept}$	0.00	0.00	0.00	0.00
Observations	80	80	80	80
Panel B: Posttest Predicting 2-year Follow-up Effects				
Intercept	0.00 (0.03)	-0.02 (0.04)	-0.07 (0.04)	-0.07 (0.04)
Soc Posttest	0.29 (0.11)*	0.26 (0.12)*		0.22 (0.12)
Cog Posttest		0.11 (0.11)	0.39 (0.10)***	0.34 (0.11)**
$\tau_{intercept}$	0.00	0.00	0.00	0.00
Observations	80	80	79	79
Panel C: 1-year Follow-up Effects Predicting 2-year Follow-up Effects				
Intercept	0.01 (0.03)	0.00 (0.03)	-0.03 (0.03)	-0.03 (0.03)
Soc 1-year Follow-up	0.40 (0.11)***	0.33 (0.12)**		0.01 (0.12)
Cog 1-year Follow-up		0.24 (0.13)	0.73 (0.12)***	0.73 (0.12)***
$\tau_{intercept}$	0.00	0.00	0.00	0.00
Observations	81	80	79	79

Notes: Treatment impacts were estimated at the grantee level with a reduced set of controls (cohort, child age at concurrent testing, center-level fixed effects). Same-domain and cross-domain transfer estimates were computed through a series of independent meta-regression models (12 presented in this table) in which grantee-level impacts from a later assessment wave (1- or 2-year follow-up) were regressed on grantee-level impacts from an earlier assessment wave (1-year follow-up or posttest). Models included weighting by the inverse variances of follow-up effects and a random intercept for grantee.

* $p < .05$, ** $p < .01$, *** $p < .001$

Table S5

Auto-regressive and Cross-domain Dynamics in Grantee-Level Effects using Pre-test Effects

	Outcome: Social-Emotional Composite		Outcome: Cognitive Composite	
	Same-Domain	Cross-Domain	Same-Domain	Cross-Domain
	Model (1)	Model (2)	Model (3)	Model (4)
Panel A: Pre-test Effects Predicting 1-year Follow-up Effects				
Intercept	-0.02 (0.03)	-0.02 (0.03)	0.04 (0.03)	0.04 (0.03)
Soc Pre-test	0.03 (0.13)	0.08 (0.13)		0.14 (0.11)
Cog Pre-test		-0.21 (0.12)	-0.15 (0.09)	-0.18 (0.09)
$\tau_{intercept}$	0.02	0.00	0.05	0.04
Observations	81	81	80	80
Panel B: Pre-test Effects Predicting 2-year Follow-up Effects				
Intercept	-0.01 (0.03)	-0.01 (0.03)	0.01 (0.03)	0.01 (0.03)
Soc Pre-test	-0.11 (0.13)	-0.10 (0.13)		0.03 (0.12)
Cog Pre-test		-0.04 (0.11)	-0.10 (0.10)	-0.11 (0.11)
$\tau_{intercept}$	0.02	0.03	0.03	0.04
Observations	81	81	79	79

Notes: Treatment impacts were estimated at the grantee level with controls for baseline child and family characteristics. Same-domain and cross-domain transfer estimates were computed through a series of independent meta-regression models (12 presented in this table) in which grantee-level impacts from a later assessment wave (1- or 2-year follow-up) were regressed on grantee-level impacts at pre-test. Models included weighting by the inverse variances of follow-up effects and a random intercept for grantee. Controls used for computing the grantee-level effects included: center-level fixed effects, pre-test cognitive and social-emotional composites (in the case of 1- and 2-year follow-up effects), cohort, age at concurrent testing, child gender, child race, child primary language, primary caregiver's age, primary language spoken at home, whether both biological parents live with the child, whether the child's mother is a recent immigrant, mother's educational attainment, mother's marital status, and whether the mother gave birth as a teenager.

* $p < .05$, ** $p < .01$, *** $p < .001$

Table S6

Auto-regressive and Cross-domain Dynamics in Grantee-level Effects controlling for Grantee-level Pre-test “Impacts”

	Outcome: Social-Emotional Composite		Outcome: Cognitive Composite	
	Same-Domain	Cross-Domain	Same-Domain	Cross-Domain
	Model (1)	Model (2)	Model (3)	Model (4)
Panel A: Posttest Predicting 1-year Follow-up Effects				
Intercept	-0.02 (0.03)	-0.06 (0.04)	-0.04 (0.03)	-0.04 (0.03)
Soc Posttest	0.20 (0.12)	0.16 (0.12)		0.00 (0.10)
Cog Posttest		0.23 (0.15)	0.43 (0.12)***	0.43 (0.12)***
$\tau_{intercept}$	0.00	0.00	0.00	0.00
Observations	80	80	80	80
Panel B: Posttest Predicting 2-year Follow-up Effects				
Intercept	-0.01 (0.03)	-0.05 (0.04)	-0.05 (0.04)	-0.04 (0.04)
Soc Posttest	0.19 (0.12)	0.15 (0.12)		0.24 (0.11)*
Cog Posttest		0.23 (0.15)	0.30 (0.13)*	0.25 (0.14)
$\tau_{intercept}$	0.03	0.00	0.00	0.00
Observations	80	80	79	79
Panel C: 1-year Follow-up Effects Predicting 2-year Follow-up Effects				
Intercept	0.00 (0.03)	-0.01 (0.03)	-0.02 (0.03)	-0.02 (0.03)
Soc 1-year Follow-up	0.32 (0.11)**	0.27 (0.11)*		0.04 (0.11)
Cog 1-year Follow-up		0.24 (0.14)	0.68 (0.13)***	0.66 (0.13)***
$\tau_{intercept}$	0.00	0.00	0.00	0.00
Observations	81	80	79	79

Notes: Treatment impacts were estimated at the grantee level with controls for baseline child and family characteristics (listed below). In these models, only data from participants that contributed data at each assessment wave on both cognitive and social-emotional composites were included when computing grantee-level treatment impacts. Same-domain and cross-domain transfer estimates were computed through a series of independent meta-regression models (12 presented in this table) in which grantee-level impacts from a later assessment wave (1- or 2-year follow-up) were regressed on grantee-level impacts from an earlier assessment wave (1-year follow-up or posttest) as well as pre-test “impacts” computed at the grantee level using the aforementioned approach. Models included weighting by the inverse variances of follow-up effects and a random intercept for grantee. Controls used for computing the grantee-level effects included: center-level fixed effects, pre-test cognitive and social-emotional composites (in the case of posttest, 1- and 2-year follow-up effects), cohort, age at concurrent testing, child gender, child race, child primary language, primary caregiver’s age, primary language spoken at home, whether both biological parents live with the child, whether the child’s mother is a recent immigrant, mother’s educational attainment, mother’s marital status, and whether the mother gave birth as a teenager.

* $p < .05$, ** $p < .01$, *** $p < .001$

Table S7

Auto-regressive and Cross-domain Dynamics in Grantee-level Effects with Consistent Sample Across Waves

	Outcome: Social-Emotional Composite		Outcome: Cognitive Composite	
	Same-Domain	Cross-Domain	Same-Domain	Cross-Domain
	Model (1)	Model (2)	Model (3)	Model (4)
Panel A: Posttest Predicting 1-year Follow-up Effects				
Intercept	-0.01 (0.03)	-0.01 (0.05)	-0.04 (0.04)	-0.03 (0.04)
Soc Posttest	0.17 (0.12)	0.17 (0.13)		0.06 (0.10)
Cog Posttest		0.00 (0.16)	0.41 (0.12)***	0.39 (0.13)**
$\tau_{intercept}$	0.00	0.00	0.00	0.01
Observations	79	79	79	79
Panel B: Posttest Predicting 2-year Follow-up Effects				
Intercept	-0.01 (0.03)	-0.02 (0.05)	-0.07 (0.04)	-0.06 (0.04)
Soc Posttest	0.18 (0.12)	0.18 (0.12)		0.19 (0.11)
Cog Posttest		0.04 (0.16)	0.40 (0.14)**	0.35 (0.14)*
$\tau_{intercept}$	0.00	0.00	0.05	0.06
Observations	79	79	79	79
Panel C: 1-year Follow-up Effects Predicting 2-year Follow-up Effects				
Intercept	0.00 (0.03)	-0.02 (0.03)	-0.02 (0.03)	-0.02 (0.03)
Soc 1-year Follow-up	0.36 (0.11)**	0.31 (0.12)**		0.01 (0.10)
Cog 1-year Follow-up		0.25 (0.14)	0.76 (0.12)***	0.76 (0.13)***
$\tau_{intercept}$	0.00	0.00	0.00	0.00
Observations	79	79	79	79

Notes: Treatment impacts were estimated at the grantee level with controls for baseline child and family characteristics. In these models, only data from participants that contributed data at each assessment wave on both cognitive and social-emotional composites were included when computing grantee-level treatment impacts. Same-domain and cross-domain transfer estimates were computed through a series of independent meta-regression models (12 presented in this table) in which grantee-level impacts from a later assessment wave (1- or 2-year follow-up) were regressed on grantee-level impacts from an earlier assessment wave (1-year follow-up or posttest). Models included weighting by the inverse variances of follow-up effects and a random intercept for grantee. Controls used for computing the grantee-level effects included: center-level fixed effects, pre-test cognitive and social-emotional composites, cohort, age at concurrent testing, child gender, child race, child primary language, primary caregiver's age, primary language spoken at home, whether both biological parents live with the child, whether the child's mother is a recent immigrant, mother's educational attainment, mother's marital status, and whether the mother gave birth as a teenager.

* $p < .05$, ** $p < .01$, *** $p < .001$

Table S8

Auto-regressive and Cross-domain Dynamics in Grantee-level Effects using a Common-Effects Approach (no Random Effect)

	Outcome: Social-Emotional Composite		Outcome: Cognitive Composite	
	Same-Domain Model	Cross-Domain Model	Same-Domain Model	Cross-Domain Model
	(1)	(2)	(3)	(4)
Panel A: Posttest Predicting 1-year Follow-up Effects				
Intercept	-0.03 (0.03)	-0.06 (0.04)	-0.03 (0.03)	-0.03 (0.03)
Soc Posttest	0.19 (0.12)	0.16 (0.12)		0.01 (0.10)
Cog Posttest		0.18 (0.15)	0.38 (0.12)***	0.37 (0.12)***
Observations	80	80	80	80
Panel B: Posttest Predicting 2-year Follow-up Effects				
Intercept	-0.01 (0.03)	-0.05 (0.04)	-0.05 (0.04)	-0.04 (0.04)
Soc Posttest	0.17 (0.12)	0.14 (0.12)		0.24 (0.11)*
Cog Posttest		0.21 (0.14)	0.27 (0.13)*	0.21 (0.13)
Observations	80	80	79	79
Panel C: 1-year Follow-up Effects Predicting 2-year Follow-up Effects				
Intercept	0.00 (0.03)	-0.01 (0.03)	-0.02 (0.03)	-0.02 (0.03)
Soc 1-year Follow-up	0.31 (0.11)**	0.26 (0.11)*		0.04 (0.11)
Cog 1-year Follow-up		0.21 (0.14)	0.67 (0.13)***	0.65 (0.13)***
Observations	81	80	79	79

Notes: Treatment impacts were estimated at the grantee level with controls for baseline child and family characteristics (listed below). In these models, only data from participants that contributed data at each assessment wave on both cognitive and social-emotional composites were included when computing grantee-level treatment impacts. Same-domain and cross-domain transfer estimates were computed through a series of independent meta-regression models (12 presented in this table) in which grantee-level impacts from a later assessment wave (1- or 2-year follow-up) were regressed on grantee-level impacts from an earlier assessment wave (1-year follow-up or posttest) as well as pre-test “impacts” computed at the grantee level using the aforementioned approach. Models included weighting by the inverse variances of the follow-up effects. Controls used for computing the grantee-level effects included: center-level fixed effects, pre-test cognitive and social-emotional composites (in the case of posttest, 1- and 2-year follow-up effects), cohort, age at concurrent testing, child gender, child race, child primary language, primary caregiver’s age, primary language spoken at home, whether both biological parents live with the child, whether the child’s mother is a recent immigrant, mother’s educational attainment, mother’s marital status, and whether the mother gave birth as a teenager.

* $p < .05$, ** $p < .01$, *** $p < .001$