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Elizabeth U. Cascio
Dartmouth College, NBER and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Does Universal Preschool Hit the Target? Program Access and Preschool Impacts*

Despite substantial interest in preschool as a means of narrowing the achievement gap, little is known about how particular program attributes might influence the achievement gains of disadvantaged preschoolers. This paper uses survey data on a recent cohort to explore the mediating influence of one key program attribute – whether disadvantage itself is a criterion for preschool admission. Taking advantage of age-eligibility rules to construct an instrument for attendance, I find that universal state-funded prekindergarten (pre-K) programs generate substantial positive effects on the reading scores of low-income 4 year olds. State pre-K programs targeted toward disadvantaged children do not. Differences in other pre- K program requirements and population demographics cannot explain the larger positive impacts of universal programs. The alternatives to universal and targeted state pre-K programs also do not significantly differ. Together, these findings suggest that universal preschools offer a relatively high-quality learning experience for low-income children not reflected in typical quality metrics.

JEL Classification: H75, I24, I28, J13, J24

Keywords: early education, preschool, targeted, universal, access, quality

Corresponding author:
Elizabeth U. Cascio
Dartmouth College
6106 Rockefeller Hall
Hanover, NH 03755
elizabeth.u.cascio@dartmouth.edu

* I thank seminar and lecture participants at the Institute for Fiscal Studies, McMaster University, Montana State University, and Franklin and Marshall College for their comments. I also thank Diane Whitmore Schanzenbach for conversations that helped to motivate this research and Taryn Dinkelman, Ethan G. Lewis, and Na'ama Shenhav for helpful conversations and comments since then. All errors are my own.

I. Introduction

Over the past several decades, a consensus has emerged that the United States spends too little on preschool education. This view is supported by considerable experimental and quasi-experimental research pointing to large long-term social returns to preschool investments in disadvantaged children – returns manifested in adulthood as increases in earnings and educational attainment and reductions in violent crime and receipt of public assistance.[1] Yet, considerably less is known about the determinants of productive efficiency in preschool education – how to make the dollars spent on preschool more beneficial.

The great diversity in state pre-kindergarten (pre-K) programs may reflect the relative lack of research on the benefits associated with particular program characteristics. In 2014-15 – the most recent school year with data available – not all states funded pre-K, and among the 42 states that did, there was great variation in how funds were allocated (Barnett et al., 2016). Some state programs, like Florida's, serve all 4 year olds that meet age-eligibility requirements ("universal" programs) but otherwise have low standards; others, like Tennessee's, are means-tested or targeted on other risk factors ("targeted" programs), with stringent state requirements. Yet other states – most famously, Georgia and Oklahoma – operate high-standard, universal programs that have served as models for recent federal proposals to invest in early education.

In this paper, I take advantage of age-eligibility rules to estimate the short-term cognitive and socio-emotional benefits of pre-K attendance in survey data where the mediating influences of program characteristics can be directly assessed. My specific focus is on *access* – on the impacts of using disadvantage as a criterion for admission. Universal pre-K programs may deliver higher benefits than targeted pre-K programs for low-income children. For one, by being

[1] For recent reviews of this literature, see Almond and Currie (2011), Elango et al. (2016), and Almond, Currie, and Duque (2017).

open to all, universal programs reach low-income children who do not meet the means tests of targeted programs but would otherwise lack a preschool experience. In 2014, for instance, only 66.3% of 4 year olds were enrolled in any preprimary program (Snyder, de Brey, and Dillow, 2016), suggesting considerable scope to increase preschool attendance. Universal programs may also be higher quality given observed inputs or program standards. They may attract better teachers, set higher expectations, or be under more parental pressure to perform.

To my knowledge, no study to date has evaluated the efficacy of universal and targeted programs for low-income children on the same basis – using the same data and research design. Indeed, there is a mature body of research on targeted preschool programs, most famously the federal Head Start program (e.g., Currie and Thomas, 1995; Garces, Thomas, and Currie, 2002; Ludwig and Miller, 2007; Deming, 2009; Puma et al., 2010; Aizer and Cunha, 2012; Carneiro and Ginja, 2014; Bitler, Hoynes, and Domina, 2014; Kline and Walters, 2016) and the "model" preschool interventions of the distant past (e.g., Heckman et al., 2010; Schweinhart et al., 2005).[2] There is also emergent research on state-funded universal preschools, based largely but not entirely on the long-operating programs in Georgia and Oklahoma (e.g., Gormley and Gayer (2005), Fitzpatrick (2008), Cascio and Schanzenbach (2013)). Data from the Birth Cohort of the Early Childhood Longitudinal Study (ECLS-B) makes it possible to test formally whether program access yields different short-term achievement effects for a recent cohort, born in 2001.

My analysis using the ECLS-B faces two challenges. The first is selection of children into pre-K attendance. I address this challenge by taking advantage of the large differences in pre-K attendance rates among preschool-aged children with birthdays near pre-K entrance cutoff birthdates. While this source of variation is not new to the pre-K evaluation literature, the ECLS-

[2] There are also several stand-alone studies of targeted state pre-K programs, for example in North Carolina (Ladd, Muschkin, and Dodge, 2014) and Tennessee (Lipsey et al., 2013).

B allows me to harness it in a new way.[3] Instead of applying a regression discontinuity design (RDD), I take a difference-in-differences (DD) approach, exploiting the *relatively* large difference in pre-K attendance rates between 4 year olds in adjacent kindergarten cohorts in states with robust state-funded pre-K programs. That is, I use a comparison group of other states to remove the confounding effects of age and season of birth. This approach is more appropriate than the RDD in this setting, where exact day of birth is not observed. More generally, survey data allow me to overcome a number of limitations of previous RDD evaluations of pre-K, which have relied on school administrative data (Lipsey et al., 2014). For example, by observing outcomes for an entire cohort, I am in principle able to produce consistent estimates of the impact of pre-K attendance using age eligibility as an instrument; rich background characteristics, including pretests, also allow for new tests of internal validity.

The second challenge is that, just like the decision to operate a program at all, whether to operate a universal or targeted program is arguably not random, but rather a function of a state's population, budget, and preferences. The overall standards of universal and targeted programs nevertheless looked similar on average in the 2005-06 school year, when children born earlier in 2001 would have first been eligible to attend pre-K. For specific areas where there are differences – indeed for all observed standards – I explore whether those differences mediate the estimated effects in a regression framework. While variation in program standards is not random, the approach is similar in spirit to that previously taken to understand variation in the impacts of Head Start (Walters, 2015) and charter schools (Angrist, Pathak, and Walters, 2013; Dobbie and Fryer, 2013). I also examine whether differences in demographics across universal and targeted states, combined with heterogeneous treatment effects, can explain the findings.

[3] Indeed, the literature has relied heavily on its use since Gormley and Gayer's (2005) pioneering application of the RDD to Tulsa's pre-K program. See, for example, Wong et al. (2008) and Weiland and Yoshikawa (2013).

I find substantial positive effects of attending universal pre-K on the cognitive test scores of 4 year olds who qualify for free or reduced-price lunch (i.e., low-income children). By comparison, low-income children do not benefit from pre-K targeted on income or other risk factors. Universal pre-K attendance (eligibility) improves their early reading scores by a statistically significant 1 (0.2) standard deviation; by contrast, the impacts of targeted pre-K attendance on their early reading scores are statistically indistinguishable from zero and significantly lower than those for universal pre-K attendance. A similar pattern of findings emerges for the early math scores of low-income children, but the differences across universal and targeted programs are not statistically significant.[4] Though imprecise, achievement gains from universal pre-K for higher-income children do not appear as large, suggesting that universal pre-K diminishes early income achievement gaps.

The substantive findings hold up to a number of internal validity checks, and my exploration of mechanisms suggests that variation in program access *per se* is their driving force. Supporting a causal interpretation, the preferred specification does not predict developmental or socio-emotional scores when children are toddlers, and substantive conclusions are little changed after alterations to the control variables, the comparison group, and how the data are stratified. Regarding mechanisms, both universal and targeted programs displace enrollment in other center-based care for low-income children – and not differentially so – and differences in neither state pre-K program standards nor state population demographics can explain the higher impacts of universal programs for low-income children.

Taken together, these findings suggest that universal programs offer a relatively high-quality learning experience for low-income 4 year olds not reflected in the quality metrics frequently targeted by policymakers. To explore what this experience might look like, I conclude

[4] Findings for socio-emotional outcomes are unfortunately uninformative.

the paper with a descriptive analysis of pre-K teacher and school administrator interviews in the ECLS-B. These data echo the differences in reported program standards, but also provide insights into pre-K orientations and teacher attitudes. Targeted programs and universal programs serving low-income children look similar along a lot of dimensions, but there is suggestive evidence that they may have a stronger academic orientation. These findings are considerably more speculative, however, and warrant exploration in future research, as does the importance of direct peer effects in pre-K classrooms.

II. Program Landscape

There has been dramatic growth in public funding of preschool programs since the early 1980s. Figure 1 shows trends from 1968 through 2011 in the number of states funding preschool (left axis) and in the Head Start and overall public preschool enrollment rates of 3 and 4 year olds (right axis).[5] In the early 1980s, only four states funded pre-K programs; by 2011, this figure reached 40 states and the District of Columbia. Public preschool enrollment rates have risen as more states have established pre-K programs. This is particularly the case for 4 year olds, for whom enrollment in Head Start – the other primary provider of public preschool – has been stagnant if not declining since the early 1990s. For 3 year olds, by contrast, public preschool enrollment rates have risen alongside the funding expansions, but so too has Head Start enrollment. This pattern reflects the focus of state-funded pre-K programs on 4 year olds.[6]

The time period relevant for the present study is the 2005-06 school year, represented by the dashed vertical line in Figure 1. Thirty-eight states and the District of Columbia funded pre-K

[5] Data on public preschool enrollment rates by age are calculated from the October Current Population Survey (CPS) School Enrollment supplements. Head Start enrollment rates divide Head Start enrollments reported by the Head Start Bureau by cohort size estimates based on Vital Statistics data on live births. State funding dates were constructed from program narratives published by NIEER (Barnett et al., 2016).

[6] During the 2014-2015 school year, 29% of 4 year olds were enrolled in state-funded pre-K programs, compared to 5% of 3 year olds. These percentages have increased little since the 2009-2010 school year (Barnett et al., 2016).

programs at this time. In terms of access, some of these programs had no eligibility requirements beyond those establishing the age of prospective enrollees – universal programs – whereas others were means-tested or used risk factors other than poverty to determine eligibility – targeted programs.[7] Due to constraints imposed by my empirical approach, my analysis will focus on state programs that served 4 year olds nearly exclusively, for which there were state-established dates by which the youngest enrollees were to have been 4 years old, and for which those "cutoff birthdates" did not fall in the middle of a month, according to statistics and program narratives published by the National Institute for Early Education Research (NIEER) (Barnett et al., 2006, 2007).[8] Sixteen states met these criteria for 2005-06, but I eliminate two from consideration due to large divergence between NIEER-reported and ECLS-B-based enrollment figures.[9]

Though weighted toward states with larger programs due to these criteria, the remaining 14 states span the range of observed pre-K standards that existed in 2005-06. Figure 2 gives a scatterplot of NIEER's 10-point index of state program standards against its estimate of the fraction of 4 year olds enrolled in the state-funded pre-K program.[10] The dot sizes represent Census estimates of the state's 4-year-old population, and for comparison, I designate the 14 states of interest with their state postal codes. The (population-weighted) state-funded pre-K

[7] Such risk factors include (but are not limited to) low parental education, single parenthood, English language learner (ELL) status, homelessness, placement in foster care, and development delays.
[8] Specifically, I define "state programs that served 4 year olds nearly exclusively" as those for which the difference in NIEER-reported state pre-K enrollment rates for 4 year olds and 3 year olds was at least 8 percentage points. Twenty-two of the 38 states with state-funded programs in 2005-06 meet this criterion, Illinois being the last. Four of these 22 states (Vermont, Kentucky, Connecticut, and New Jersey) must be excluded due to local determination of their pre-K entry cutoff dates; two (Maine and North Carolina) must be excluded for having cutoff birthdates in the middle of the month. Of the remaining states with (smaller) state-funded pre-K programs, eight have age eligibility requirements that are locally determined.
[9] These states are Louisiana and Michigan. Unfortunately, Barnett et al. (2006) does not report on Washington, D.C., so I cannot include ECLS-B observations from D.C. in the analysis.
[10] One point on the index comes from comprehensive early learning standards; four come from teacher training and credentialing requirements (teacher has BA, specialized training in pre-K, assistant teacher has Child Development Associate (CDA) or equivalent, at least 15 hours of in-service training annually); two come from staffing ratios (maximum class size no larger than 20, staff-child ratio 1:10 or better); two come from comprehensive services (vision, hearing, health, and one support service, at least one meal provided); and one comes from a site visit requirement.

enrollment rate for 4 year olds for these 14 states was 34.5 percent in 2005-06, compared to only 10.3 percent for the remaining 24 states with programs.[11] Most notably, of the five states with the largest pre-K programs in terms of enrollment shares – Florida, Georgia, Oklahoma, Texas, and Vermont – only Vermont is excluded from the analysis. Yet, states with lower enrollment rates do appear in the analysis sample, and the distribution of the program standards index appears similar across states with higher and lower enrollment rates.

Table 1 provides more detail on the programs in operation in 2005-06 in the 14 states of interest, with states listed in descending order by 4-year-old enrollment rates and by whether the program is universal (Panel A) or targeted (Panel B). The highest-access programs are the ones in Georgia and Oklahoma, which as noted are the two longest-standing and most-studied universal pre-K programs (Gormley and Gayer, 2005; Wong et al., 2008; Fitzpatrick, 2008, 2010; Cascio and Schanzenbach, 2013). The targeted programs include Tennessee's, the only state-funded pre-K program to date subjected to randomized evaluation (Lipsey et al., 2013). Ten of the 14 states of interest require entering pre-kindergartners to be age 4 on August 31 or September 1 – roughly the start of the typical school year – whereas three of the remaining four states require pre-kindergartners to be age 4 one month later (September 30 or October 1).

The goal of this paper is to use the variation in age eligibility for pre-K associated with these pre-K cutoff birthdates to estimate the effects of pre-K enrollment on age 4 cognitive and socio-emotional test scores, and in particular to explore how those effects differ for low-income children across universal and targeted programs. For the universal-targeted comparison to cleanly reveal the additional benefits of universal access, it would ideally be the case that the two types of programs on average had similar standards in 2005-06. Column 3 shows a small (and

[11] In the analysis states, the population-weighted average (standard deviation) of the NIEER index is 5.94 (2.06). In the remaining states, these figures are 5.86 (2.11).

inconsistently signed) difference between the population- and enrollment-weighted values of the 10-point index for universal and targeted states, suggesting that this might be the case overall. However, the remaining columns of the table show some differences for specific components of the index. Most notably, targeted programs on average impose more teacher training and credentialing requirements than universal ones, but are on average less likely to require small classes and low staffing ratios. The difference in estimated effects between universal and targeted programs is therefore at risk of picking up these differences in standards if they independently affect the impacts of pre-K attendance. To address this possibility, I estimate heterogeneity in the effects of pre-K by both access and state standards in some specifications.

III. Research Design

A. Intuition and Empirical Model

The basic relationship of interest is between state-funded pre-K attendance and children's academic and socio-emotional outcomes at preschool age. The fundamental identification problem is that children who attend state-funded pre-K may have unobserved characteristics that influence these outcomes directly. In targeted and universal programs alike, attendance is not mandatory and programs themselves are often not fully-funded, allowing for potential selection into enrollment based on unobserved characteristics. Simply controlling for observables in a regression framework or pursuing other selection-on-observables methods, such as inverse propensity-score weighting, are thus unlikely to uncover the causal effects of pre-K enrollment.

The approach taken in this paper is similar in spirit – but not in implementation – to that taken first in the pre-K evaluation literature by Gormley and Gayer (2005).[12] At base, I wish to compare children with 4th birthdays near pre-K eligibility cutoff birthdates, the idea being that children with 4th birthdays right on or before the cutoff should have a much higher probability of

[12] There have been many subsequent applications, including Wong et al. (2008) and Weiland and Yoshikawa (2013).

being enrolled in pre-K the following school year despite being on average similar in all ways – observed and unobserved – to the children with 4th birthdays right after. If it were thus possible to compare the short-term outcomes of children born right on the cutoff date to those born the day after (e.g., September 1 versus September 2) – if it were possible to compare the preschool-age outcomes of the oldest child in one school (e.g., pre-K) entry cohort to the youngest child in the next – one could then recover the causal effect of pre-K eligibility and attendance.

In practice, the available data are insufficient to make such a sharp comparison: some datasets, including the ECLS-B, lack information on exact day of birth, and even when information on exact birthday is available, there are generally too few observations on a daily basis to generate informative estimates. As a result, it is necessary to widen the range of birthdates under consideration. Doing so requires acknowledging, however, that children with birthdays on opposite sides of the cutoff date no longer have the same potential on average. In fact, even if these children have similar unobserved characteristics, they differ along an observed dimension – age – that is strongly related to child development. The RDD solution is to assume that age effects on test scores are smooth, or can be modeled with a polynomial function in age that is continuous through the birthdate cutoff.

In the current application, information on exact day of birth is not available, but information on month of birth is, as are data on children across the U.S., not just in specific states or school districts. These data support an alternative approach – a comparison of 4 year olds in adjacent school entry cohorts in states with the pre-K programs of interest, as identified in Section II (the treatment states), versus other states (the comparison states). In the treatment states, cutoff birthdates for pre-K in fall 2005 were the same as cutoff birthdates for kindergarten in fall 2006, so the children in adjacent pre-K kindergarten entry cohorts are also in adjacent kindergarten entry cohorts. The comparison group then consists of the 22 other states that had a state-established kindergarten cutoff birthdate in fall 2006 that was not in the middle of the month.[13] Differences in preschool-age outcomes across adjacent kindergarten entry cohorts in the comparison group are intended to capture what would have happened for children in the treatment states in the absence of pre-K, due to aging or other factors.

This is a DD rather than an RD design. Ignoring for now the distinction between universal and targeted programs, the basic model of interest is

$$(1) \qquad y_{is} = \theta\, elig_{is} \times treat_s + \gamma\, elig_{is} + \alpha\, treat_s + v_{is},$$

where y_{is} is an age 4 ("preschool-age") outcome (e.g., test score) of child i in state s; $treat_s$ is a dummy equal to one if state s is a treatment state (Table 1); and $elig_{is}$ is a dummy equal to one if child i is in the earlier (older) school entry cohort, set to enter kindergarten in fall 2006 rather than fall 2007 (or pre-K in fall 2005 rather than fall 2006, if pre-K is offered). More specifically, $elig_{is} = 1[agek_i - agek_s^* \geq 0]$, where $agek_i$ is child i's age in months on September 1, 2006, and $agek_s^*$ is the required minimum age in months for kindergarten entry in state s on September 1, 2006.[14] So intuitively, if all states had September 1 cutoff birthdates for kindergarten entry, $elig_{is}$ would equal one for children born January through August 2001, and zero for children born September through December 2001.

The coefficient of interest in model 1 is θ, which captures how much more (or less) kindergarten entry cohort relates to the preschool-age outcomes of children in treatment states. I

[13] That is, comparison states were defined using all of the same criteria used to define treatment states except for the requirement of having a state pre-K program that served 4 year olds nearly exclusively. The comparison states thus include states without pre-K programs and states whose pre-K programs did *not* serve 4 year olds nearly exclusively (see footnote 8). For the second group of states, it would likely be impossible for me to detect a first stage using this research design. All comparison states are listed in the notes to Table 2.

[14] Lacking information on exact day of birth, I must include children who are actually eligible for kindergarten in fall 2006 (e.g., those turning 5 on September 1, 2006 in a state with a September 1 cutoff date) in the fall 2007 kindergarten cohort. Assuming that births are (close to) uniformly distributed across the month, this should lead to a small amount of attenuation bias in estimates of the eligibility impacts. Thus, it is to minimize attenuation bias that I exclude states with cutoff dates closer to the middle of the month.

have designed the study so that θ is positive and both educationally and statistically significant for pre-K attendance (the first stage). If, in turn, pre-K attendance improves test scores, it should then be the case that there are relatively large preschool-age test-score differences associated with being in the older kindergarten cohort in the treatment states. For estimates of θ to be unbiased estimates of the impacts of pre-K eligibility – of intent-to-treat (ITT) effects – it must be the case that differences in unobserved determinants of test scores across kindergarten cohorts do not systematically differ across the treatment and comparison states (the exclusion restriction).

To increase the chances that this is the case, I focus on a DD model with less restrictive effects of eligibility and state of residence:

$$(2) \qquad y_{is} = \theta\, elig_{is} \times treat_s + \sum_{m=-4}^{7} \gamma_m elig_{is}^m + \alpha_s + v_{is}\,,$$

where the $elig_{is}^m$ represent a series of dummies for age in months relative to the minimum age in months for kindergarten entry, or $elig_{is}^m = 1[agek_i - agek_s^* = m]$, where $-4 \leq m \leq 7$ (replacing γ $elig_{is}$ in model 1),[15] and α_s represents a vector of state fixed effects (replacing α $treat_s$ in model 1). Below, I provide informal evidence in favor of model 2's identifying assumption that $\mathrm{cov}[elig_{is} \times treat_s, v_{is}] = 0$ by showing that children in the earlier kindergarten entry cohort in treatment states generally do not have systematically different observed characteristics.

B. Distinctions from Existing Literature

Model 2 differs from the RDD models generally estimated in the pre-K evaluation literature in a number of important ways. The first difference is theoretical: compared to the RDD, model 2 does not require that age effects be continuous and smooth in the absence of pre-

[15] Reflecting the fact that September 1 is the modal cutoff date (Table 1), I restrict the sample to children born in the eight months before their state cutoff date ($0 \leq agek_i - agek_s^* \leq 7$) or in the four months after ($-4 \leq agek_i - agek_s^* \leq -1$). I assess the sensitivity of the estimates to the choice of sample window in Table 5.

K. The use of eligibility indicators allows for effects that are discontinuous – even at the eligibility threshold – which is helpful since when measured discretely, age is perfectly collinear with season of birth, and season of birth effects need not be smooth. It is also helpful because there could be increases in preschool enrollment at kindergarten eligibility thresholds even in the absence of state-funded pre-K if private programs used state regulations to ration preschool slots (Gelbach, 2002; Fitzpatrick, 2010).

Second, as it is generally implemented in this literature, the RDD does not cleanly identify either the impacts of pre-K eligibility (ITT effects) or the impacts of pre-K attendance (treatment-on-treated, or TOT effects). As described by Lipsey et al. (2014), the typical pre-K RDD application has employed samples of *pre-K students* in adjacent school entry cohorts, rather than the school entry cohorts in their entirety, because of a lack of outcomes data on children who do not attend pre-K. I overcome this missing-data problem by using survey data with outcomes on all individuals in a birth cohort, and I attempt to produce valid TOT estimates (i.e., for the population at large) by instrumenting for pre-K attendance ($prek_{is}$) in the model:

$$(3) \qquad y_{is} = \beta\, prek_{is} + \sum_{m=-4}^{7} \lambda_m elig_{is}^{m} + \delta_s + \varepsilon_{is}\,,$$

with $elig_{is}$ x $treat_s$, using two-stage least squares (TSLS). I also estimate model 3 using ordinary least squares (OLS), for comparison.

Model 2 and my estimation of it differ from the standard implementation of the RDD in this literature in other important ways.[16] Unlike previous studies, I have ample data on baseline characteristics with which to evaluate the internal validity of the research design, including birth weight and earlier (age 2) test scores. I also not only observe but, via the use of a comparison

[16] See Lipsey et al. (2014) for a description of the full range of potential problems inherent in previous applications of the RDD for pre-K evaluation.

group, can also evaluate impacts of pre-K on enrollment in alternative care and education options for the same cohort of children affected by pre-K. Furthermore, the test score outcomes, described below, are designed to be age-appropriate and are measured before children attending pre-K would have progressed to kindergarten.

Finally, the data allow me to compare effects across states with different program characteristics. The closest previous work in this vein is Wong et al. (2008), who use the age-eligibility RDD to estimate the short-term cognitive effects of pre-K attendance in 2004-05 in five states – Michigan, New Jersey, Oklahoma, South Carolina, and West Virginia. These states differ in program access and standards, but the authors neither perform a formal analysis of the influence of particular program characteristics, nor do they present estimates by family socio-economic status, as is done in the present study. The present study also incorporates data from more states that are also more variable in terms of program standards, as described in Section II.

IV. Data

A. Sample

The distinct approach taken in this paper is made possible by detailed survey data. These data come from the Birth Cohort sample of the Early Childhood Longitudinal Study (ECLS-B). The ECLS-B is a longitudinal survey of a stratified random sample of children born in the United States in 2001.[17] ECLS-B respondents were assessed and their parents and caregivers interviewed at roughly 9 months of age (wave 1), 2 years of age/toddler age (wave 2), 4 years of age/preschool age (wave 3), and kindergarten age (waves 4 and 5). My estimation sample consists of all children with non-missing preschool-age cognitive assessments and demographic and background characteristics residing at preschool age in one of the 14 treatment or 22

[17] The ECLS-B contains oversamples of some demographic groups (Chinese and other Asians, Pacific Islanders, Native Americans, and Alaskan Natives), twins, and low and very low birth weight children. I apply sampling weights to make the estimates population representative.

comparison states and 5 years old between 8 months before and 4 months after their state's kindergarten entry cutoff – a total of 5,500 observations.[18],[19] Reported sample sizes are rounded to the nearest 50, per IES rules to protect confidentiality of ECLS-B respondents.

Most pertinent for this study are the data from wave 3; this wave includes test scores on children who were of preschool age, but may or may not have been actually enrolled in (or eligible to enroll in) state-funded pre-K. More specifically, given the fall kindergarten entry cutoffs in Table 1, the 2001 birth cohort can be split into two school entry cohorts in the wave 3 data – children eligible to enter kindergarten in fall 2006 (and pre-K in fall 2005, if relevant) and children eligible to enter kindergarten in fall 2007 (and pre-K in fall 2006, if relevant). Children were then tested starting in August 2005 – when any exposure to pre-K would have been limited – through June 2006 – when a child enrolled would have had a full school year of exposure. The average exposure to pre-K in treated states was 2.3 months, assuming September 1 starts the school year.

B. *Key Variables*

The main outcomes of interest are cognitive test scores in early math and reading, as well as socio-emotional test scores. The preschool cognitive assessment was designed to test both for developmental (age-based) milestones and for knowledge and skills considered important for school readiness and early school success.[20] I work with reading and math scale scores from this assessment normalized to mean zero and variance one in the full sample. The ECLS-B's socio-

[18] Restricting attention to respondents with non-missing cognitive assessments at wave 3 and non-missing demographic and family background information leads to a small number of drops from the full sample. The missing data are not predicted by the instrument.
[19] Note that since the same comparison group is used to estimate the impact of targeted and universal programs, the number of observations may appear to be larger when summing across regression-specific sample sizes.
[20] The preschool-age assessment drew on the Peabody Picture Vocabulary Test (PPVT), the Preschool Comprehensive Test of Phonological and Print Processing (Pre-CTOPPP), the PreLAS® 2000, and the Test of Early Mathematics Ability-3 (TEMA-3), as well as the cognitive assessment given to the fall 1998 kindergarten cohort of the ECLS (ECLS-K).

emotional assessment is based on coding of videotaped parent-child interactions in the "Two Bags Task," where a parent is asked to guide her child's play with the contents of two bags – one containing a book and the other some toys – over a ten-minute period. At preschool age, child behavior on the task was assessed on three dimensions, each on a 7-point Likert scale – engagement, quality of play, and negativity. I flipped the negativity scale ("positivity"), added across the three scales, and standardized to mean zero and variance one in the full sample to arrive at the socio-emotional test score used in this paper.

The ECLS-B also provides detailed information on the care and education of respondents at preschool age. Following Bassok et al. (2016), I base my measures on provider reports of the type of program in which a child was enrolled – pre-K, Head Start, other center-based care, or informal non-parental care – where available, and parental reports where these data are missing; the final category is parental care. Importantly, *prek* includes both public school and private school programs; this is intentional, since many state-funded programs operate through private schools or child care centers (Barnett et al., 2006). However, the ECLS-B does not directly identify *state-funded* pre-K enrollment, and some centers or schools (or parents) might designate a program as "pre-kindergarten" when it is not in fact the state-funded program. Thus, *prek* is a misclassified measure of enrollment in the state-funded pre-K program. For this reason, the ITT (model 2) estimates presented below are arguably more informative than the TOT ones (TSLS estimates of model 3).[21]

The ECLS-B also contains rich family background information on respondents.[22] In addition to basic demographics (age at assessment and indicators for sex (female) and race (non-Hispanic black and Hispanic)), I construct indicators for low-income (family income at or below

[21] TSLS does not produce consistent estimates of coefficients on misclassified binary regressors (Bound, Brown, and Mathiowetz, 2001).
[22] All time- or age-varying family background variables are measured in wave 3, or at preschool age.

185% of the federal poverty line (FPL)), for low birth weight (birth weight < 2,500 grams), for low maternal education (at or below a high school degree), for a language other than English being spoken in the home, and for the presence of both biological parents in the household. The ECLS-B also assessed toddler's motor and mental development and socio-emotional development using the Bayley Short Form-Research Edition (based on the Bayley Scales of Infant Development, 2nd Edition) and the Two Bags Task, respectively.[23] I standardize these measures analogously to the preschool-age outcomes.

I use the low-income indicator to stratify the analysis, and the remaining background and demographic characteristics as baseline regression controls. The low-income indicator is ideal for stratification, since free or reduced-price lunch eligibility (family income ≤ 185% FPL) is the modal income eligibility criterion for the targeted states of interest.[24] In specification checks, I estimate the preferred model (with baseline regression controls) for the toddler-age "pretests," as well as include pretest scores as additional controls in the main test score model. If model 2 identifies the effect of age eligibility for pre-K, the inclusion of these variables as controls should improve the precision of the estimates without greatly affecting their magnitudes.

C. Preliminary Evidence on Identifying Assumptions

Recall that the validity of my empirical approach rests on two assumptions. The first is that $elig_{is}$ x $treat_s$ predicts pre-K enrollment (first stage). The second is that $elig_{is}$ x $treat_s$ does not predict unobserved correlates of the outcomes of interest (exclusion restriction). The remainder of this section provides some preliminary evidence on whether these assumptions are met.

[23] In wave 2, the child behavior scores are for engagement, negativity, and sustained attention.

[24] The eligibility criterion is relevant for four targeted states – Texas, Maryland, Colorado, and Tennessee – which together account for 61 (73) percent of 4-year old population (state-funded pre-K enrollment) in the states with targeted programs listed in Table 1. The remaining states have tighter income eligibility requirements (Kansas) or no explicit income requirements, but risk factor requirements that correlate strongly with income (Illinois, South Carolina, Virginia). I test the sensitivity of my conclusions to how the data are stratified in a specification check.

Figure 3 shows evidence on the first stage, focusing on low-income children.[25] To elucidate how the DD estimates more generally are constructed, Panel A shows raw means of pre-K enrollment at preschool age (in 2005-06) by state of residence and by age relative to the minimum age for kindergarten entry. I group state of residence into the three pertinent groups – universal states, targeted states, and comparison states – and to reduce noise, I group the age variable into two-month intervals. Age is increasing along the horizontal axis, with the first two points representing ages at which children in treatment states would not have been eligible to attend pre-K in fall 2005, i.e., children in the 2006 (2007) rather than 2005 (2006) pre-K (kindergarten) entry cohort. Panel B then shows the *difference* in means between each treatment group and the comparison group, relative to (or subtracting off) what that difference was for the children who just missed eligibility ($-2 \leq agek_i - agek_s^* \leq -1$). Aside from age being grouped into intervals, this is the generalized version of model 2, and thus matches the DD estimates.

As shown in Panel A, all three groups of states have fairly similar 2005-06 pre-K enrollment rates among low-income children who were not eligible to attend pre-K (kindergarten) until fall 2006 (2007). However, a substantial gap between the treatment and comparison states emerges for children eligible to start pre-K (kindergarten) in fall 2005 (2006). Panel B shows that the treatment-comparison gap in 2005-06 pre-K enrollment rates is between 10 and 25 percentage points larger among eligible children than among those who just missed eligibility. Estimates are larger – and more likely to be statistically significant – for universal states than for targeted ones. Consistent with Panel A, there is also some evidence of a treatment-comparison difference in 2005-06 pre-K enrollment rates among younger children (those missing pre-K eligibility by 3 or 4 months), but these differences are not statistically significant.

[25] Analogous plots for children who are not low-income are shown in Figure A1. There is evidence for this population of a first-stage relationship for universal states but not for targeted states, as expected.

Table 2, Panel A shows the corresponding DD estimates of this first-stage relationship, i.e., subgroup-specific coefficients (standard errors) on $elig_{is}$ x $treat_s$ from model 2.[26] Continuing with the intuition from Figure 3, these are regression-adjusted differences-in-differences in 2005-06 pre-K enrollment rates between the 2006 and 2007 kindergarten cohorts in the treatment (universal or targeted) states versus the comparison states. To match the specifications later estimated, the underlying regression models now include indicators for month of assessment in addition to the $elig_{is}^m$ indicators and state of residence fixed effects.[27]

Consistent with the graphical evidence, the coefficient on $elig_{is}$ x $treat_s$ is relatively large for low-income children in universal states (column 2). This coefficient estimate is 0.207, implying that the gap in 2005-06 pre-K enrollment rates for low-income children between the 2006 and 2007 kindergarten cohorts is 20.7 percentage points higher in states with universal pre-K programs than in the comparison states. For low-income children in targeted states, this gap amounts to 15.6 percentage points, and for higher-income children in states with universal programs (column 4), this gap is similar, at 15.7 percentage points. In each case, the coefficient estimate is statistically different from zero but arguably lower than might be expected, possibly because the ECLS-B measure of pre-K enrollment is a misclassified measure of enrollment in *state-funded* pre-K programs, as noted above.[28,29] For higher-income children in targeted states (column 8), the coefficient estimate is smaller and not significant.

[26] As in Figure 3, I cluster standard errors on state of residence-by-month of birth (the level of the treatment) and weight the analysis using sampling weights.

[27] Figures A7 and A8 show that the first-stage "event study" estimates look fairly similar with controls for month of assessment and for baseline background characteristics versus without.

[28] In the absence of misclassification but with perfect compliance (i.e., all children starting pre-K only when they are age eligible), the first-stage DD coefficient would be the 2005-06 academic-year DD in pre-K enrollment rates between 3 and 4 year olds in treatment versus comparison states. Across all states with universal pre-K (Table 1, Panel A), the (population-weighted) difference in age 3 and age 4 enrollment rates is 41.2 percentage points; for states with targeted pre-K programs, this figure is 24.1 percentage points; and for the 22 comparison states, this figure is 4.2 percentage points. With perfect compliance, we might therefore expect to see first-stage estimates of 37 (41.2-4.2) percentage points for universal programs and 19.9 (24.1-4.2) percentage points for targeted programs.

Panel B then shows the analogous estimates for the baseline demographic and background characteristics. As earlier described, these estimates provide an informal test of the exclusion restriction; if model 2 is identified (or model 3 identified when estimated using TSLS), $elig_{is}$ x $treat_s$ should have little predictive power with regard to these observed correlates of test scores. And indeed, these estimates suggest that age-eligibility rules provide for relatively clean identifying variation for low-income children. For low-income children in both groups of treatment states (columns 2 and 6), the coefficients are both individually and jointly statistically insignificant. With the exception of having higher Hispanic shares and higher likelihoods of speaking a language other than English, low-income children also look fairly similar to one another across the two groups of states (columns 1 and 5). Higher-income children also look better than low-income children on many observed dimensions (column 3), as expected. However, the balance test fails more often for higher-income children in universal states.[30]

Overall, Table 2 suggests that comparisons of impacts of universal versus targeted pre-K for low-income children should be fairly informative, but more caution may be in order when making comparisons of impacts of universal pre-K across low- and higher-income children. Further, even if not statistically significant, the sometimes large coefficient magnitudes in Table 2 suggest that estimates including background controls are preferable to those without.

V. Findings

A. Baseline Results

Perfect compliance is untenable, however; existing research on kindergarten entry (e.g., Cascio and Schanzenbach, 2016) suggests that non-compliance with age-eligibility rules is likely to reduce the gaps for universal programs by on average 20%, but by a greater extent for higher-income children. If, for example, there were a 30% non-compliance rate for higher-income children and a 10% one for low-income children, the first-stage DD coefficients would be 33.2 percentage points for low-income children in universal programs, 21.7 percentage points for low-income children in targeted programs, and 25.9 percentage points for higher-income children in universal programs, consistent with attenuation bias on the order of 30% to 40%.

[29] Suggestive of misclassification is the high pre-K enrollment rate of higher-income children in targeted programs.

[30] This seems an unfortunate consequence of sampling variation, since there is arguably little sorting around these cutoff dates (Dickert-Conlin and Elder, 2010).

Table 3 presents, separately for each of the four access-by-income subgroups and each of the three test scores, least squares estimates of the effects of age-eligibility for pre-K from model 2 and both TSLS and OLS estimates of the effect of pre-K attendance from model 3. All of the regressions include the eligibility indicators (the $elig_{is}^{m}$), state fixed effects, month of assessment dummies, and the demographic and background variables listed in Table 2, Panel B as controls.[31] Mean test scores for the ineligible sample in treatment states (row 1 of each panel) show the large gaps in test scores by SES that emerge even at an early age.

Against this backdrop, consider the estimates for preschool-age reading scores, shown in Panel A. For low-income children in states with universal pre-K programs (column 1), the estimated DD coefficient from model 2 is a statistically significant 0.229. This implies that the gap in 2005-06 school year reading test scores between low-income children in the 2006 and 2007 kindergarten cohorts is about 0.23 standard deviations higher in states with universal pre-K programs than in the comparison states. When this estimate is scaled by the corresponding first-stage DD coefficient for *prek* – or when I instrument for *prek* in model 3 with $elig_{is}$ x $treat_s$ using TSLS – I predict that universal pre-K attendance brings about a 1.109 standard deviation improvement in reading scores at preschool age. This figure is inflated over what would have been achieved if *prek* were not a misclassified measure of state-funded pre-K attendance, but even absent misclassification, the effect size would have been substantial.[32] By comparison, the OLS estimate of the pre-K attendance effect is much smaller and not statistically significant, suggesting that conclusions are very different assuming only selection on observables.[33]

[31] Estimates of these same models without the demographic and background controls are in Appendix Table A1. Throughout, standard errors are clustered on state-by-month of birth, and estimates weighted by sampling weights.
[32] For example, the back-of-the-envelope prediction of the first-stage coefficient in the absence of misclassification (footnote 28) would be consistent with a TSLS impact of 0.69 standard deviation.
[33] Magnuson, Ruhm, and Waldfogel (2007) also find evidence of much larger effects of pre-K attendance on cognitive test scores using TSLS over OLS in a study using data from the ECLS-K.

State-funded pre-K appears to be more effective for low-income children when programs are universal rather than targeted. Column 3 shows that the gap in 2005-06 school year reading test scores between low-income children in the 2006 and 2007 kindergarten cohorts was actually more positive in the comparison states than in the targeted states; the DD estimate is negative. Though statistically insignificant, this DD estimate is significantly different from the DD estimate for low-income children in universal pre-K, in column 1. The same general pattern (but not significance) of estimates emerges for math scores (Panel B), and though not individually significant, estimated effects of pre-K eligibility on the socio-emotional scores of low-income children are also marginally significantly higher for universal programs than for targeted ones (Panel C).

Figure 4 shows graphical evidence of the impacts of pre-K attendance on reading scores for low-income children like what was shown for their pre-K attendance in Figure 3. There is a fairly strong age gradient in scores among all groups of states (Panel A). However, a relatively large treatment-comparison test score gap among eligible children only emerges for universal states (Panel B). Indeed, targeted and comparison states have very similar reading scores for low-income children on average regardless of age, despite the significantly higher pre-K enrollment rates among eligible children in targeted states (Figure 3, Panel B). Figures A2 and A3 provide the analogous graphs for math and socio-emotional scores of low-income children.[34] Graphical evidence of impacts of universal pre-K here is weaker, consistent with the relative lack of statistical significance of the findings for these outcomes.

Universal pre-K also appears to boost the academic performance of low-income children more than higher-income children. The estimated effects of universal pre-K on the cognitive test scores of higher-income children are close to zero and not statistically significant (column 2,

[34] Figure A7 provides event-study coefficients for all outcomes from a model with the same controls as in Table 3.

Panels A and B). On the other hand, I cannot reject equality of the effects of universal pre-K (eligibility or attendance) across family income for any of the outcomes, and as earlier noted, the column 2 estimates may be less credible, making me hesitant to draw strong conclusions. Yet, such a finding would be consistent both with a framework where higher-income children have relatively high-quality care and education options in the absence of universal pre-K and with much existing evidence on universal preschools both in the U.S. and worldwide (Cascio, 2015; Elango et al., 2016).[35]

To summarize, the baseline differences in the impacts of targeted and universal pre-K for low-income children are statistically and economically meaningful. The small implied effects of targeted state pre-K programs are consistent with findings from the Head Start Impact Study (Puma et al., 2010; Kline and Walters, 2016) and from the experimental evaluation of the (targeted) Tennessee Voluntary Pre-K program (Lipsey, et al., 2013), both of which focus on similarly recent cohorts. In addition, the large implied effects of universal pre-K attendance on cognitive test scores are in line with recent estimates of the effects of school exposure at young ages.[36] This pattern of findings suggests that universal pre-K programs are more like "school" in their effects than targeted ones. The effect sizes also suggest that universal pre-K attendance can (more than) close test score gaps by income that exist prior to pre-K entry; eligibility for pre-K alone cuts reading test score gaps by income by nearly a third.

B. Robustness Checks

[35] See Figures A4, A5, and A6 for graphs akin to Figure 4 for the reading scores, math scores, and socio-emotional scores of higher-income children. Figure A8 then provides event-study coefficients from a model with the same controls as in Table 3.

[36] For example, taking advantage of variation in eligibility for school entry using the RDD in the ECLS-K, Anderson et al. (2011) estimate that children who have completed first grade have reading (math) test scores that are on average 1 (0.776) standard deviation higher than children of the same age who have only completed kindergarten. Relying on quasi-random variation in test dates in the ECLS-K, Fitzpatrick, Grissmer, and Hastedt (2011) also estimate that a year of kindergarten or first grade raises test scores by about one standard deviation.

For the estimates in Table 3 to identify the effects of pre-K, it must be the case that there are no other reasons to believe outcomes would have differed with eligibility (or across kindergarten entry cohorts) more in the treatment states than the comparison states. I have already provided some evidence consistent with this assumption, namely the tests for balance on observables in Table 2. In the remainder of this section, I provide further evidence and probe the sensitivity of the estimates to additional controls, the choice of comparison group, and alternative ways of stratifying the data.

One concern is that, though the interaction between treatment state and age eligibility for pre-K may not be strongly associated with *observables* that predict performance (Table 2), it could still be associated with *unobservables*: eligible children in treatment states might have greater unobserved potential conditional on their observables. If so, we might expect to see an "effect" of age eligibility for targeted pre-K on the test scores of higher-income children. But this is not the case (Table 3, column 4). If it existed, such a correlation between the instrument and unobservables would also arguably appear earlier in life, before children are preschool age. Table 4 explores this possibility, presenting estimates of the same models in Table 3 but for developmental and socio-emotional test scores at age 2. Regardless of the outcome variable, the DD coefficients are relatively close to zero (often negative), statistically insignificant, and not statistically different from one another. Relatedly, the second row of each panel of Table 5 shows that, when age 2 scores are included as controls in the models for reading scores (Panel A) and pre-K enrollment (Panel B), the DD estimates are similar to what they were at baseline.

The remainder of Table 5 shows the DD estimates for reading scores and pre-K enrollment under other modifications to the specification.[37] The first modification is to the

[37] To conserve on space, the corresponding estimates for math and socio-emotional scores are in Appendix Table A2.

comparison group: instead of including all 22 states, I limit attention to the 13 comparison states that have pre-K programs; by revealed preference, these states may be more similar to the treatment states, and thus may do a better job of representing the counterfactual.[38] This sample restriction increases the estimated impacts of universal pre-K eligibility on the reading test scores of low-income children without appreciably changing impacts on pre-K enrollment. Focusing on a narrower window of observations around the cutoff dates also reinforces the conclusion that universal pre-K programs outperform targeted ones for low-income children in terms of reading.

The next two rows consider different ways of stratifying the sample. Stratifying by low maternal education again strengthens the conclusion that disadvantaged children benefit relatively more in terms of reading scores from universal pre-K. On the other hand, stratification by family income at or below 130% FPL (the cutoff for free lunch eligibility) somewhat weakens the difference in pre-K eligibility effects on reading scores for low-income children in universal and targeted states. To explore the relationship between family background and the effects more non-parametrically, Figure 5 plots DD estimates of the impacts of pre-K eligibility on pre-K attendance and reading scores by quintiles of a socio-economic status (SES) index derived from factor analysis on parental education, parental occupation, and family income. Universal pre-K eligibility yields statistically significant attendance effects through the fourth quintile of the index, whereas first-stage estimates for targeted programs diminish monotonically in the index (Panel A). The corresponding DD estimates for reading scores (Panel B) suggest that the benefits of universal pre-K decline in SES. In general, these findings suggest that my baseline stratification of the sample by free- or reduced-price lunch eligibility captured the key elements of the effects in the data.

[38] Recall that these 13 states are not treatment states either because their pre-K programs are too small or because those programs serve a high share of 3 year olds relative to 4 year olds, based on external (NIEER) data. In both cases, it would likely be difficult to detect a first stage impact on pre-K enrollment.

VI. An Effect of Program Access?

Thus far, I have shown that universal pre-K delivers larger short-term test score benefits than targeted pre-K for disadvantaged children. The estimates for universal programs are substantively similar to the short-term effects of universal kindergarten, while the estimates for targeted programs are substantively similar to those for targeted early education (e.g., Head Start) in recent cohorts. And while point estimates (and thus effect sizes) are somewhat sensitive to specification, a larger impact of universal pre-K on disadvantaged children's test scores – reading scores in particular –has emerged across all of the robustness checks considered.

But does universal access *per se* cause universal programs to be more productive? Not necessarily. Even if eligibility for pre-K is "as good as random" at the individual level, program access at the state level may be related to other state-level factors that make pre-K attendance more or less productive. For example, Table 1 showed that the universal pre-K programs of interest in operation in 2005-06 were more likely than targeted ones to require class sizes at or below 20 students and student to staff ratios of 10:1 or better (combined under the "Staffing" column of Table 1). If lower class sizes increase program benefits, class size requirements might be the actual explanation for the universal-targeted impact differential, or may diminish that differential substantially.[39] Likewise, if there is sufficient demographic heterogeneity in the effects of pre-K, differences in the demographics of the low-income populations in targeted and universal states (Table 2) could be contributing to the estimates. Further, the early childhood care and education landscape might be different in states with universal pre-K programs.[40] If there

[39] Findings from Project STAR (Krueger, 1999; Chetty et al., 2011) suggest that assignment to a small class in kindergarten yields immediate cognitive test score benefits, particularly for disadvantaged children, though not as large as the effects of exposure to universal pre-K for low-income children estimated here.
[40] The establishment of universal programs may in fact change the child care and early education sectors. See, for example, Bassok, Fitzpatrick, and Loeb (2014) and Bassok, Miller, and Galdo (2016).

were fewer center-based care alternatives to universal pre-K, for instance, I would expect to find relatively large test score effects of universal pre-K, all else constant.[41]

Table 6 considers this last possibility, showing DD estimates of eligibility impacts from model 2 for "counterfactual" care – Head Start, other center-based care, informal non-parental care, and parental care. These variables are mutually exclusive and, along with pre-K, span the space of possible education and care options for preschool-age children; the DD coefficients therefore add up to zero across all categories. The DD coefficients for pre-K (Panel A) are the first-stage coefficients on the instrument for the TSLS estimates presented in Tables 3 and 4. With the exception of a marginally significant reduction in the likelihood of Head Start enrollment among children in targeted programs (column 6), none of the DD estimates for the alternative care options (Panel B) are statistically significant for low-income children (columns 2 and 6). While the implied effects of pre-K attendance on the Head Start enrollment of low-income children are larger in magnitude for targeted than for universal programs, I cannot reject equality of the DD coefficients for any of the variables, including pre-K attendance.[42] Overall, these findings provide at best weak evidence that cross-state differences in counterfactual care options explain the relative efficacy of universal programs.

To address the first possibility, I then estimated the effect of pre-K eligibility holding constant each of the NIEER program standards, one-by-one, using the model:[43]

$$(4) \quad \begin{aligned} y_{isp} = {} & \theta_{UT} elig_{is} \times treat_s \times uni_s + \theta_T elig_{is} \times treat_s + \theta_Q elig_{is} \times treat_s \times Q_s + \\ & \sum_{p=0}^{1} \sum_{m=-4}^{7} \gamma_{mp} elig_{is}^{m} \times P_p + \alpha_{sp} + \sum_{p=0}^{1} \left(x_{is} \times P_p \right)' \beta_p + \upsilon_{isp} , \end{aligned}$$

[41] In re-analyses of data from the Head Start Impact Study, Feller et al. (2015) and Kline and Walters (2016) find that Head Start has much smaller impacts on children who would have otherwise been in center-based care.
[42] Likewise, I cannot reject equality of effects on overall (formal) preschool enrollment, which sums across pre-K, Head Start, and other formal centers.
[43] Estimates are very unstable when attempting to include all 10 standards in a linear and additive way.

where p indexes the estimation sample, with p=1 for universal pre-K estimates and p=0 for targeted pre-K estimates, and so $P_j = 1[j=p]$. uni_s and $treat_s$ represent, respectively, a dummy for whether s is a universal (treated) state and a dummy for whether s is a universal *or* targeted (treatment) state, and Q_s a dummy set to one if a particular state pre-K program standard was met in s in 2005-06. Because all of the regression controls are interacted with an indicator for the estimation sample, in a version of the model excluding $elig_{is}$ x $treat_s$ x Q_s, the triple-difference coefficient, θ_{UT}, generates the difference in the DD estimates for universal and targeted pre-K programs presented in Table 3; the coefficient θ_T then reproduces the DD estimate for targeted programs.

The interest here is then in the model as written, with $elig_{is}$ x $treat_s$ x Q_s included. This model adjusts estimates of θ_{UT} for the correlation between program access and the program standard in question, Q_s. Analogous to the interpretation of θ_{UT}, the coefficient θ_Q gives the difference in DD coefficients for state pre-K programs with versus without standard Q_s. If this coefficient is positive – if a program with standard Q_s appears more productive – and if universal programs are more likely to have standard Q_s than targeted ones – estimates of θ_{UT} with $elig_{is}$ x $treat_s$ x Q_s included will be lower than at baseline.

Table 7 presents estimates of θ_{UT} and θ_Q for each standard; for comparison, baseline estimates, without the control for standards, are shown in column 1.[44] The table focuses on reading scores and pre-K attendance rates of low-income children; estimates for math and socio-emotional scores for this subpopulation are in Appendix Table A3. The largest erosion in the relative reading score benefits of universal programs comes from holding constant teacher BA requirements (column 3), staffing/class size requirements (column 6), and the site visit

[44] I pool the program standards for class size and staffing ratios into one dummy because there is such limited independent variation in these standards.

requirement (column 10). The final two cases are anticipated: universal programs are more likely to have these standards, and these standards are associated with higher (though not statistically significant) benefits from pre-K attendance. Even so, the universal-targeted difference in DD coefficients remains sizable and statistically significant in both cases. And for the specification including the interaction between eligibility and a teacher BA requirement, the estimate of θ_{UT} is nearly statistically significant (p-value=0.124). In addition, the (insignificant) estimate of θ_Q in this specification is *negative*, suggesting that low-income children in states where programs have teacher BA requirements gain *less* from pre-K. While a null effect of teacher education requirements might be expected given decades of related findings in the literature on K-12 education, a negative effect is not.[45]

A potential cause of this unexpected finding is that model 4 forces the same effect of each program standard on both universal and targeted programs. An alternative way to think about the relative importance of universal access and these standards is thus to ask how standards interact with access. For example, do universal programs *without* teacher training and credentialing requirements actually perform better than those with such requirements? If so, it would suggest that we might want to take seriously the findings shown in Table 7, column 3. Similarly, do targeted programs with class size requirements produce effects on reading scores as large as those found for universal programs? If so, it might suggest that it is indeed class size rather than access that is driving the relative efficacy of universal programs.

The scope for testing whether there are differences in the effects of standards by access is unfortunately limited by the available policy variation. But there is still insight into these questions to be had. In particular, among universal states, Florida lacks any teacher teaching or

[45] This finding could reflect negative selection of states that require pre-K teachers to have BAs. That program standards are not randomly assigned is of course a key limitation of this exercise.

credentialing requirements but has both NIEER staffing requirements. On the flip side, among states with targeted programs, Texas lacks the staffing requirements but mandates three of the four NIEER teacher training and credentialing requirements, including the teacher BA requirement (Table 1). These states are large and therefore influential on the average difference in standards across states; dropping them from the estimation sample diminishes these differences.[46]

I therefore re-estimated model 2 dropping Florida and Texas. As shown in the last row in each panel of Table 5, doing so increases estimates of pre-K eligibility effects on reading scores for low-income children, regardless of whether the program is universal or targeted. This suggests that both sets of requirements might actually improve test score outcomes. However, the gains are larger in universal states, and the universal-targeted difference remains statistically significant. Together with the results in Table 7, these findings suggest that universal pre-K is not outperforming targeted pre-K for low-income children because universal programs have some different observed standards.

Finally, just like cross-state variation in program standards was a threat to the access interpretation of the findings, so too is cross-state variation in the characteristics of the low-income population. For example, the Hispanic share in the low-income population is higher in targeted states than in universal states (Table 2); if Hispanic children see systematically lower benefits from pre-K, the universal-targeted differential in program effects could be overstated. Appendix Table A4 shows estimates from a model that substitutes individual demographic

[46] For example, dropping Florida and Texas from the list of treatment states, population-weighted averages for the number of teacher training and credentialing requirements met (out of four possible) are 1.9 and 2.8 for universal and targeted pre-K states, respectively. For the number of staffing requirements met (out of two possible), these averages are 1.8 and 1.9, respectively. Dropping children from Florida and Texas from the estimation sample therefore narrows gaps in average standards across the two groups of states, and indeed, makes the two groups of states nearly identical in terms of staffing requirements. It also narrows gaps in average demographic and background characteristics across universal and targeted states.

characteristics for Q_s in model 4.[47] Allowing for demographic heterogeneity affects the conclusions very little; for all outcomes, the magnitude and statistical significance of the universal-targeted differential in estimated program effects is little changed. Demographic heterogeneity in the estimates is also arguably in the expected direction, with low birth weight children and children whose mothers have no more than a high school degree appearing to gain more from pre-K.

VII. What Makes Universal Pre-K Different?

Taken together, the findings of this paper suggest that universal programs offer a relatively productive learning experience for low-income 4 year olds, and that what makes this learning experience different is not captured by observed program standards. But this begs the question: what makes universal pre-K different? Low-income children may of course benefit from direct interaction with their higher-income peers. If higher-income children enter pre-K more prepared, as suggested by the income achievement gaps of incoming students (Table 3), universal programs may attract and retain better teachers – teachers who have warmer, more positive interactions with students.[48] The presence of higher-income children in the classroom may also change expectations of what all children should learn. If prompted to focus on relatively advanced material, for instance, teachers may accelerate the learning gains of most students.[49] These, too, are peer effects.

The nature of sampling in the ECLS-B makes it impossible for me to estimate direct peer effects in pre-K classrooms. However, interviews of the pre-K teachers of low-income children

[47] To enable a triple difference interpretation of this coefficient, I also include $elig_{is}$ x X_i (where X_i represents the relevant characteristic) as a control.
[48] Sabol et al. (2013) find that scores on the Classroom Assessment Scoring System (CLASS) do a better job than inputs (staff qualifications and class size) and learning environment (as measured by the Early Childhood Education Rating System – Revised, or ECERS-R) in predicting test score gains over the pre-K year.
[49] Using data from the ECLS-K, Engel, Claessens, and Finch (2013) show the more time teachers spend on more advanced mathematics content, the more children gain in math scores over kindergarten year, regardless of demographics.

and school administrators in the ECLS-B may provide some insight into the more indirect peer effects. Table 8 summarizes selected characteristics of pre-K teachers of low-income children, separately by whether the program is universal or targeted. Reassuringly, the basic background statistics reflect NIEER reports of the variation in program standards: teachers in universal programs report smaller class sizes, while teachers in targeted programs report higher levels of completed education. Teachers in targeted programs also report more experience and more job satisfaction, and directors of those programs if anything report less teacher turnover and more use of direct assessments – all of which might appear to favor targeted programs. But there is the suggestion of a difference in teacher beliefs about their role: compared to only about half of targeted pre-K teachers, over 63% of universal pre-K teachers strongly agree with the statement that "Children who begin formal reading and math instruction in preschool will do better in elementary school."

Using my research design, I unfortunately cannot explore whether teacher beliefs such as these can explain the difference between the estimates for universal and targeted programs. More specifically, I only observe this information for the children who actually enroll – and the subset thereof for whom provider reports are non-missing – making these findings the most speculative of the paper. But this should be a topic for future research, as should credible investigation of direct peer-to-peer learning spillovers in pre-K classrooms.

VIII. Conclusion

Despite substantial interest in preschool as a means of narrowing the achievement gap, little is known about how particular program attributes affect the achievement gains of disadvantaged preschoolers. Using age eligibility rules and state policy variation, this paper has shown that universal state pre-K programs are significantly more beneficial than targeted state pre-K programs for the early reading scores of disadvantaged children. This finding is robust to a number of specification checks and falsification exercises and continues to hold even when observed pre-K program standards and state population demographics are held constant. Though imprecise, effect sizes for targeted pre-K are consistent with the short-term test score benefits found in recent studies randomizing access to targeted preschool (Puma et al., 2010; Kline and Walters, 2016; Lipsey et al., 2013). Likewise, effect sizes for universal pre-K echo those associated with exposure to the earliest years of universal elementary education (Anderson et al., 2011; Fitzpatrick, Grissmer, and Hastedt, 2011). The difference in estimated impacts of universal programs on low-income and higher-income children, though also imprecise, suggests that universal pre-K can substantially narrow early reading test score gaps by income.

A limitation of the findings is that they only pertain to short-term effects for a set of outcomes weighted toward the cognitive domain. Indeed, the research design employed in this paper is not capable of uncovering longer-term effects, as the children who are ineligible for pre-K one year become eligible the next. Non-cognitive outcomes may also be as important as cognitive ones for predicting longer-term gains in well-being (Chetty et al., 2011; Heckman et al., 2013). Future research should work toward developing ways to estimate directly the differences in longer-term effects of targeted and universal preschools. Data limitations have also meant that I have focused only on one cohort, which may not be representative of preschool-age children today. There is therefore potential value in estimating even short-term differences in the effects of universal and targeted pre-K in other data.

Until that time, it is worth noting that the immediate cognitive test score gains from educational intervention appear predictive both of later academic performance (Duncan et al. 2007) and of adult outcomes like earnings (Chetty et al., 2011), suggesting that the differences in early test score impacts of universal and targeted pre-K programs documented here could well manifest over the longer term. Moreover, that my focus has been on a recent cohort is valuable: if anything, the impacts of early childhood intervention appear to have declined over time (Duncan and Magnuson, 2013). This makes the relative size of the impacts for universal pre-K all the more striking and worthy of future investigation.

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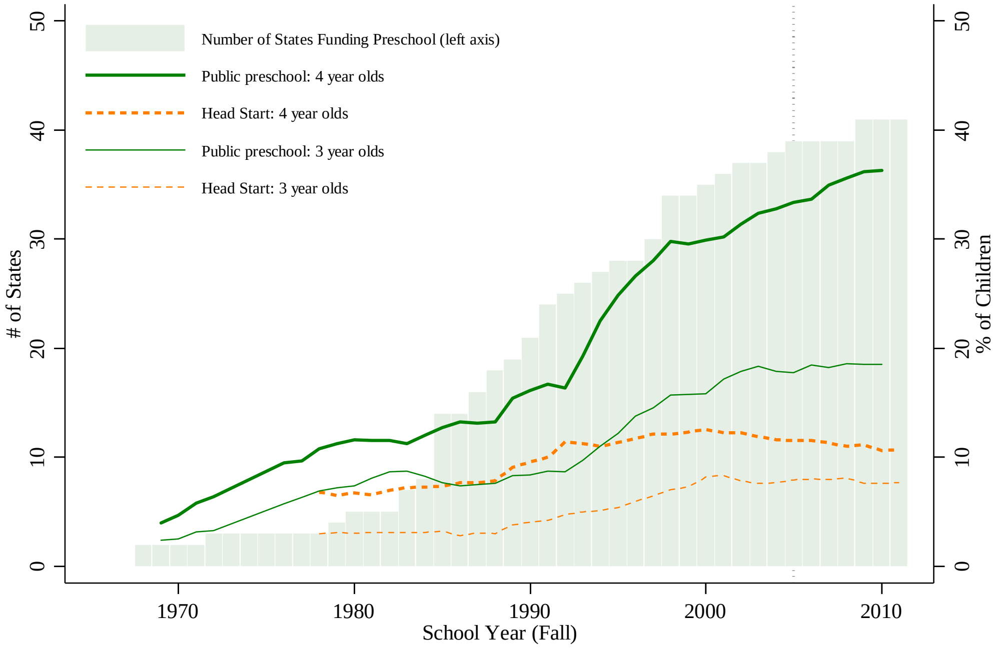
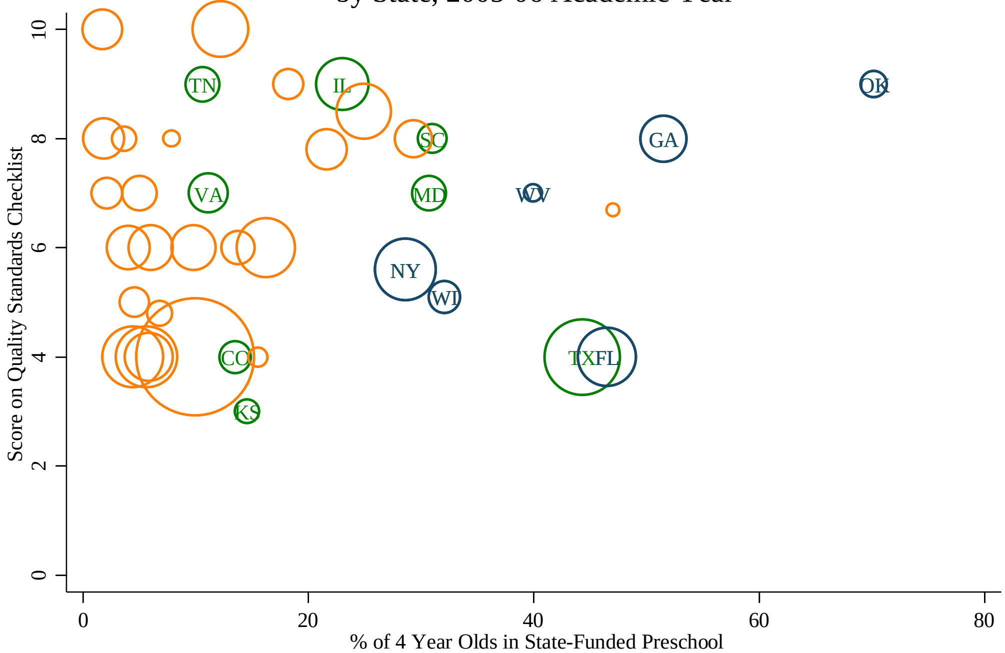
Figure 1. Trends in Public Preschool Enrollment Rates
and Prekindergarten Funding: 1968-2011



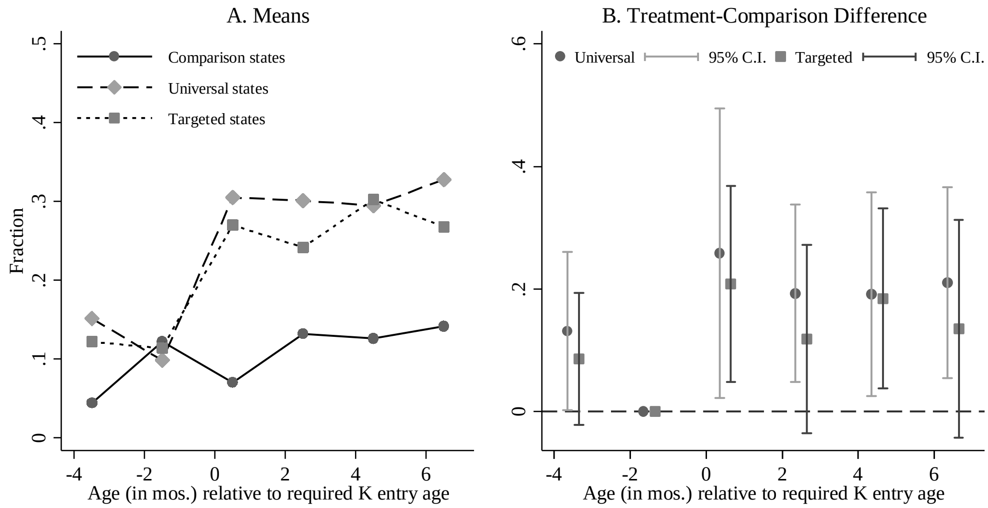
Notes: Data on public preschool enrollment rates by age are calculated from the October Current Population Survey (CPS) School Enrollment supplements. Head Start enrollment rates divide Head Start enrollments reported by the Head Start Bureau by cohort size estimates based on Vital Statistics data on live births. State funding dates were constructed from program narratives published by NIEER (Barnett et al., 2016).

Figure 2. Prekindergarten Access and Quality
by State, 2005-06 Academic Year



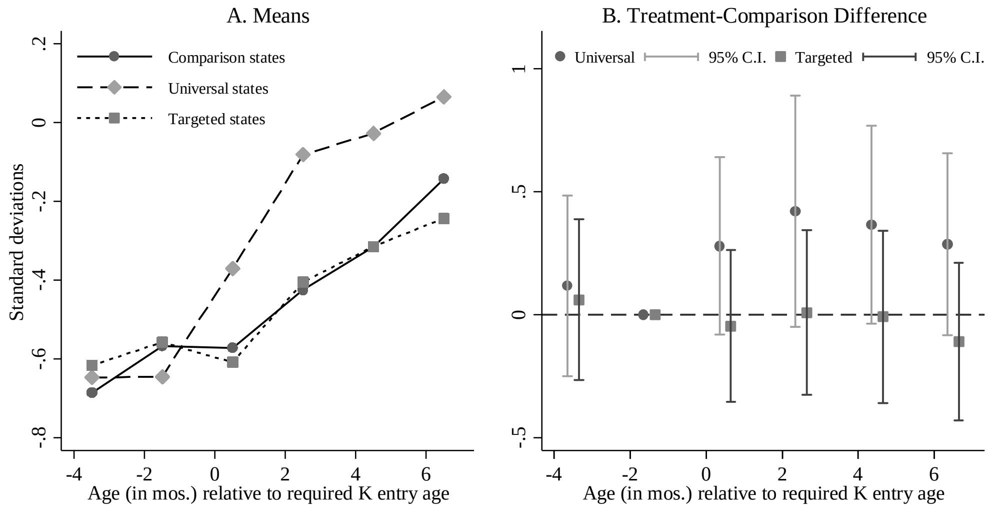
Notes: Data are from Barnett et al. (2006). Dot sizes represent the size of the state's 4-year-old population.

Figure 3. Prekindergarten Enrollment by Age and State Treatment Status:
Low-Income Children



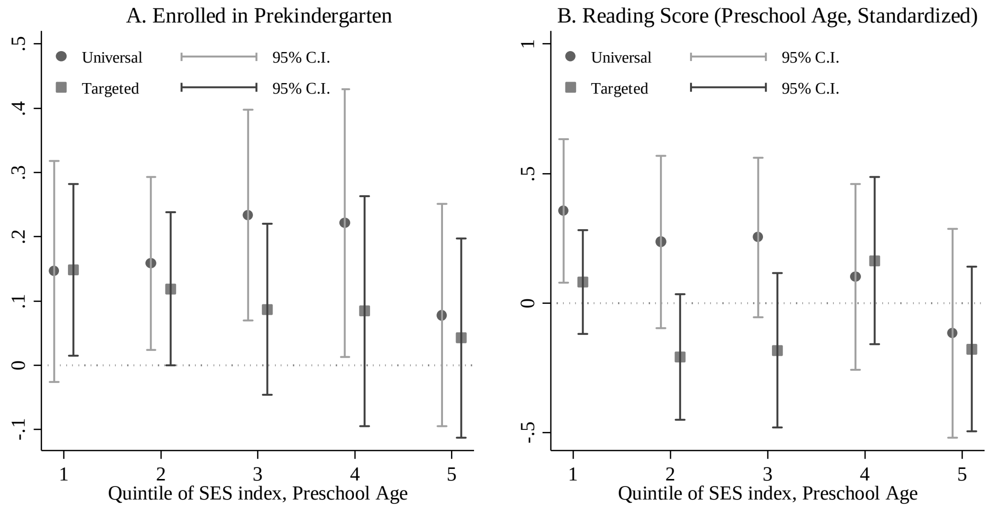
Notes: Data are from the ECLS-B and are restricted to children who, in 2005-06, were eligible for free or reduced-price lunch. Panel A plots average pre-K enrollment rates at preschool age (in 2005-06) by age relative to the minimum age for kindergarten entry (2-month bins) for treated states with universal pre-K programs, treated states with targeted pre-K programs, and comparison states; see notes to Table 2 for lists of states in each of these groups. The dots in Panel B represent, separately for treatment states with universal programs and treatment states with targeted programs, the coefficients on interactions between a treatment dummy and a series of dummies for age relative to the minimum age for kindergarten entry (2-month bins) from a regression that allows for direct effects of each of these (sets of) variables. The dependent variable is a dummy for pre-K enrollment at preschool age (in 2005-06), and the interaction with the dummy for missing eligibility by 1 to 2 months is omitted to identify the interaction coefficients. The capped vertical lines represent 95% confidence intervals, with standard errors clustered on state by month of birth.

Figure 4. Preschool-Age Reading Scores by Age and State Treatment Status: Low-Income Children



Notes: The dependent variable is standardized reading score at preschool age (in 2005-06). For further description, see notes to Figure 3.

Figure 5. Eligibility Effects on Pre-K Enrollment and Preschool-Age Reading Scores, by Program Type and SES Quartile



Notes: Each dot in each panel represents an estimate of θ in model 2 restricting attention to children in states with universal or targeted programs (in both cases relative to the same group of comparison states) in the designated quintile of the ECLS-B index for socio-economic status. The SES index is measured contemporaneously with outcomes (in 2005-06) and is derived from factor analysis on parental education, parental occupation, and family income. The underlying regression also includes indicators for month of assessment and the background characteristics listed in the Panel B of Table 2. The capped vertical lines represent 95% confidence intervals, with standard errors clustered on state by month of birth.

Table 1. Characteristics of State Pre-Kindergarten Programs Under Study

State	Birthday Cutoff for Pre-K	% of 4 y.o.'s enrolled	Quality Checklist (out of 10)	Quality Checklist Components: Comp. Learning Standards (1)	Tch. Training & Cred. Require-ments (4)	Staffing (Staffing Ratios & Class Size) (2)	Comp. Services (Health Svcs. & Meals) (2)	Required Site visits (1)
				A. Universal Programs				
Oklahoma	Sept. 1	70.2	9	1	3	2	2	1
Georgia	Sept. 1	51.5	8	1	2	2	2	1
Florida	Sept. 1	46.5	4	1	0	2	0	1
West Virginia	Sept. 1	39.9	7	1	2	2	1	1
Wisconsin	Sept. 1	32.1	5.1	1	2.9	0.1	0.1	0.9
New York	Dec. 1	28.6	5.6	0	1.4	2	1.2	1
pop-weighted avg.		*41.5*	*5.8*	*0.7*	*1.3*	*1.8*	*0.9*	*1.0*
enr-weighted avg.		*44.9*	*6.0*	*0.8*	*1.4*	*1.9*	*1.0*	*1.0*
				B. Targeted Programs				
Texas	Sept. 1	44.3	4	1	3	0	0	0
South Carolina	Sept. 1	31	8	0	3	2	2	1
Maryland	Sept. 1	30.7	7	1	3	2	1	0
Illinois	Sept. 1	23	9	1	4	2	1	1
Kansas	Aug. 31	14.5	3	0	2	0	1	0
Colorado	Oct. 1	13.5	4	0	1	2	0	1
Virginia	Sept. 30	11.1	7	0	2	2	2	1
Tennessee	Sept. 30	10.6	9	1	3	2	2	1
pop-weighted avg.		*29.0*	*6.1*	*0.7*	*2.9*	*1.2*	*0.8*	*0.5*
enr-weighted avg.		*35.3*	*5.5*	*0.8*	*3.0*	*0.8*	*0.5*	*0.3*

Notes: Source is Barnett, et al. (2006), and figures correspond to the 2005-06 academic year. One point on the quality checklist comes from comprehensive early learning standards; four come from teacher training and credentialing requirements (teacher has BA, specialized training in pre-K, assistant teacher has Child Development Associate (CDA) or equivalent, at least 15 hours of in-service training annually); two come from staffing ratios (maximum class size no larger than 20, staff-child ratio 1:10 or better); two come from comprehensive services (vision, hearing, health, and one support service, at least one meal provided); and one comes from a site visit requirement.

Table 2. Descriptive Statistics and Balance Tests on Key Variables, by Family Income and Program Type

States:	Universal states				Targeted states			
Children:	Low income		Not Low Income		Low income		Not Low Income	
	Mean	Coef. (se)	Mean	Coef. (se)	Mean	Coef. (se)	Mean	Coef. (se)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Treatment variable								
Pre-kindergarten[a]	0.253	0.206***	0.416	0.157***	0.223	0.156***	0.263	0.068
		(0.049)		(0.058)		(0.044)		(0.046)
B. Background characteristics								
Age in months[a]	52.385	0.053	52.300	-0.095*	52.226	-0.011	51.642	-0.032
		(0.066)		(0.053)		(0.075)		(0.041)
Female	0.516	0.073	0.467	0.083	0.476	-0.004	0.487	-0.003
		(0.079)		(0.052)		(0.064)		(0.052)
Black non-Hispanic	0.296	-0.006	0.096	0.061**	0.280	0.012	0.101	0.027
		(0.055)		(0.028)		(0.050)		(0.026)
Hispanic	0.207	0.005	0.134	-0.026	0.402	-0.071	0.177	0.042
		(0.054)		(0.044)		(0.049)		(0.041)
Low birth weight	0.086	0.035	0.075	0.016	0.103	0.011	0.067	0.020
		(0.022)		(0.021)		(0.026)		(0.017)
Non-English at home[a]	0.183	-0.031	0.091	0.008	0.254	-0.037	0.101	0.006
		(0.046)		(0.030)		(0.044)		(0.034)
Maternal education ≤ HS[a]	0.728	0.082	0.251	0.048	0.749	0.079	0.227	-0.065
		(0.060)		(0.059)		(0.061)		(0.048)
Both biological parents in HH [a]	0.506	-0.037	0.828	-0.086*	0.586	0.006	0.850	0.035
		(0.054)		(0.046)		(0.056)		(0.044)
p -value: joint test for background chars		0.43		0.06		0.55		0.19
Observations[b]	550	1,850	650	2,200	650	1,900	900	2,400

Notes: Coefficients in the even-numbered columns are on the interaction between a dummy for being eligible for kindergarten in 2006-07 (the same as a dummy for being eligible for pre-K in 2005-06 in a treatment state) and a dummy for being in a treatment state from a regression that also includes dummies for state of residence, for month of assessment, and for month age five relative to the state kindergarten entry cutoff birthdate in 2006-07 . Treatment states are those with state-funded pre-K programs focused much more on 4 year olds than 3 year olds and statewide minimum age at pre-K entry regulations; treatment states with universal programs are FL, GA, NY, OK, WI, WV, and treatment states with targeted programs are CO, IL, KS, MD, SC, TN, TX, VA. Comparison states have statewide age at kindergarten entry regulations; some comparison states have pre-K programs that serve a significant relative share of 3 year olds (AL, AZ, CA, CT, DE, KY, MN, MO, NM, NV, OH, OR, WA), while others lack pre-K programs (AK, HI, ID, IN, MS, ND, RI, SD, UT). A child is deemed eligible for K in 2006-07 if he /she turned age 5 in time to start K in fall 2006, given his/her date of birth and the kindergarten entry age regulations in effect in 2006-07 reported by Barnett, et al. (2007). Sample is limited to children who turn age 5 between 4 months after and 8 months before the cutoff date, and a child is considered low income if his (preschool age, or 2005-06) family income is at or below 185% FPL. Means and regressions are weighted by longitudinal sampling weights, and standard errors (in parentheses) are clustered on state x month of birth. [a] Measured at preschool age, or in 2005-06 (wave 3 interview). [b] rounded to the nearest 50. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3. Impacts on Cognitive Test and Socio-emotional Scores at Preschool Age

States:	Universal		Targeted	
Children:	Low income	Not low income	Low income	Not low income
	(1)	(2)	(3)	(4)
		A. Reading Scale Scores (Standardized)		
Ineligible mean	-0.645	0.056	-0.582	-0.102
DD Coef. on eligible x treated state	0.229**	0.026	-0.074	-0.084
	(0.105)	(0.107)	(0.090)	(0.090)
TSLS Coef. on pre-K enrollment	1.109**	0.153	-0.476	no f.s.
	(0.531)	(0.620)	(0.611)	
OLS Coef. on pre-K enrollment	0.085	0.212***	0.090	0.291***
	(0.061)	(0.066)	(0.073)	(0.069)
Observations[a]	1,850	2,200	1,900	2,400
		B. Math Scale Scores (Standardized)		
Ineligible mean	-0.680	0.089	-0.642	-0.132
DD Coef. on eligible x treated state	0.207	-0.004	0.017	-0.055
	(0.129)	(0.097)	(0.109)	(0.090)
TSLS Coef. on pre-K enrollment	1.002*	-0.025	0.107	no f.s.
	(0.589)	(0.572)	(0.675)	
OLS Coef. on pre-K enrollment	0.162**	0.141***	0.158**	0.251***
	(0.074)	(0.054)	(0.072)	(0.060)
Observations[a]	1,850	2,200	1,900	2,400
		C. Socio-emotional Scores (Standardized)		
Ineligible mean	-0.438	0.123	-0.235	0.200
DD Coef. on eligible x treated state	0.157	0.161	-0.119	-0.041
	(0.132)	(0.121)	(0.132)	(0.105)
TSLS Coef. on pre-K enrollment	0.798	1.115	-0.810	no f.s.
	(0.670)	(0.971)	(0.913)	
OLS Coef. on pre-K enrollment	-0.058	0.008	0.001	0.081
	(0.086)	(0.064)	(0.085)	(0.059)
Observations[a]	1,600	1,950	1,650	2,150
Additional controls? [b]	Y	Y	Y	Y

Notes: The DD coefficient is that on the interaction between a dummy for being eligible for kindergarten in 2006-07 (synonymous with a dummy for being eligible for pre-K in 2005-06 in a treatment state) and a dummy for being in a treated state from a regression that also includes dummies for state of residence, for month of assessment, and for month age five relative to the state kindergarten entry cutoff birthdate in 2006-07; see notes to Table 2 for description and listing of treatment and comparison states. The TSLS coefficient uses eligible x treated state as an instrument for pre-K enrollment in a specification with the same controls. Sample is limited to children who turn age 5 between 4 months after and 8 months before the kindergarten entry cutoff birthdate, and a child is considered low income if his (preschool-age, or 2005-06) family income is at or below 185% FPL. Means and regressions are weighted by longitudinal sampling weights, and standard errors (in parentheses) are clustered on state x month of birth.

[a] rounded to the nearest 50.

[b] Additional controls include dummies for female, Hispanic, black non-Hispanic, and low birth weight and (measured in the preschool wave) age at assessment and dummies for non-English at home, mom has high school degree or less, and both biological parents in household.

***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4. Falsification Test: Impacts on Cognitive and Socio-emotional Test Scores at Age 2

States:	Universal		Targeted	
Children:	Low income	Not low income	Low income	Not low income
	(1)	(2)	(3)	(4)
	A. Mental Scale (Standardized)			
Ineligible mean	-0.601	-0.036	-0.398	0.095
DD Coef. on eligible x treated state	0.043	-0.034	-0.080	-0.101
	(0.120)	(0.111)	(0.108)	(0.106)
TSLS Coef. on pre-K enrollment	0.203	-0.242	-0.553	no f.s.
	(0.546)	(0.749)	(0.759)	
OLS Coef. on pre-K enrollment	-0.045	0.060	-0.105	0.056
	(0.091)	(0.058)	(0.071)	(0.065)
Observations[a]	1,700	2,000	1,700	2,200
	B. Motor Scale Scores (Standardized)			
Ineligible mean	-0.096	-0.082	-0.162	0.033
DD Coef. on eligible x treated state	-0.030	-0.058	0.025	-0.096
	(0.154)	(0.131)	(0.120)	(0.122)
TSLS Coef. on pre-K enrollment	-0.126	-0.490	0.159	no f.s.
	(0.638)	(1.132)	(0.740)	
OLS Coef. on pre-K enrollment	-0.254***	0.027	-0.223**	-0.017
	(0.094)	(0.068)	(0.086)	(0.066)
Observations[a]	1,650	2,000	1,700	2,200
	C. Socio-emotional Scores (Standardized)			
Ineligible mean	-0.373	0.096	-0.316	0.123
DD Coef. on eligible x treated state	-0.080	0.064	0.015	-0.080
	(0.157)	(0.117)	(0.134)	(0.129)
TSLS Coef. on pre-K enrollment	-0.338	0.423	0.069	no f.s.
	(0.652)	(0.847)	(0.620)	
OLS Coef. on pre-K enrollment	-0.081	0.013	-0.115	-0.008
	(0.107)	(0.062)	(0.098)	(0.068)
Observations[a]	1,450	1,800	1,450	1,950
Additional controls? [b]	Y	Y	Y	Y

Notes: The DD coefficient is that on the interaction between a dummy for being eligible for kindergarten in 2006-07 (synonymous with a dummy for being eligible for pre-K in 2005-06 in a treatment state) and a dummy for being in a treated state from a regression that also includes dummies for state of residence, for month of assessment, and for month age five relative to the state kindergarten entry cutoff birthdate in 2006-07; see notes to Table 2 for description and listing of treatment and comparison states. The TSLS coefficient uses eligible x treated state as an instrument for pre-K enrollment in a specification with the same controls. Sample is limited to children who turn age 5 between 4 months after and 8 months before the kindergarten entry cutoff birthdate, and a child is considered low income if his (preschool-age, or 2005-06) family income is at or below 185% FPL. Regressions are weighted by longitudinal sampling weights, and standard errors (in parentheses) are clustered on state x month of birth.

[a] rounded to the nearest 50.

[b] Additional controls include dummies for female, Hispanic, black non-Hispanic, and low birth weight and (measured in the preschool wave) age at assessment and dummies for non-English at home, mom has high school degree or less, and both biological parents in household.

***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5. Additional Robustness Tests on Baseline Difference-in-Differences Estimates

States:	Universal		Targeted	
Children:	Low income	Not low income	Low income	Not low income
	(1)	(2)	(3)	(4)
	A. Reading Scale Scores (Standardized)			
Baseline[a]	0.229**	0.026	-0.074	-0.084
	(0.105)	(0.107)	(0.090)	(0.090)
Age 2 scores as controls[b]	0.231**	0.031	-0.050	-0.057
	(0.103)	(0.099)	(0.087)	(0.089)
Pre-K states only in comp. group[c]	0.295***	0.020	-0.027	-0.079
	(0.107)	(0.114)	(0.093)	(0.099)
+/- 4 months from cutoff [d]	0.253**	0.105	-0.022	-0.020
	(0.124)	(0.128)	(0.097)	(0.107)
Low income: Maternal ed ≤ HS	0.392***	-0.086	-0.003	-0.135
	(0.103)	(0.109)	(0.090)	(0.092)
Low income: ≤130% FPL	0.208	0.072	0.043	-0.143*
	(0.132)	(0.088)	(0.092)	(0.085)
Drop FL and TX	0.323**	-0.055	-0.001	-0.131
	(0.132)	(0.146)	(0.101)	(0.091)
	B. Pre-K Enrollment			
Baseline[a]	0.206***	0.168***	0.156***	0.062
	(0.047)	(0.060)	(0.044)	(0.045)
Age 2 scores as controls[b]	0.212***	0.167***	0.145***	0.071*
	(0.047)	(0.057)	(0.045)	(0.043)
Pre-K states only in comp. group[c]	0.202***	0.187***	0.160***	0.080*
	(0.052)	(0.062)	(0.049)	(0.048)
+/- 4 months from cutoff [d]	0.222***	0.185**	0.175***	0.042
	(0.064)	(0.072)	(0.053)	(0.050)
Low income: Maternal ed ≤ HS	0.195***	0.160***	0.091**	0.100**
	(0.054)	(0.061)	(0.044)	(0.049)
Low income: ≤130% FPL	0.170***	0.198***	0.175***	0.069
	(0.063)	(0.059)	(0.045)	(0.042)
Drop FL and TX	0.224***	0.234***	0.106**	0.057
	(0.063)	(0.070)	(0.051)	(0.052)

Notes: The DD coefficient is that on the interaction between a dummy for being eligible for kindergarten in 2006-07 (synonymous with a dummy for being eligible for pre-K in 2005-06 in a treatment state) and a dummy for being in a treated state from a regression that also includes dummies for state of residence, for month of assessment, and for month age five relative to the state kindergarten entry cutoff birthdate in 2006-07. See Table 2 for a definition of treatment and comparison states. Unless otherwise given, sample is limited to children who turn age 5 between 4 months after and 8 months before the kindergarten entry cutoff birthdate, a child is considered low income if his (preschool-age or 2005-06) family income is at or below 185% FPL, regressions are weighted by longitudinal sampling weights, and standard errors (in parentheses) are clustered on state x month of birth. [a] Additional controls include age at assessment, dummies for female, Hispanic, black non-Hispanic, low birth weight, non-English at home, mom has high school degree or less, and both biological parents in household. [b] Missing test scores imputed and indicated with dummy variables to maintain sample size. [c] Comparison states limited to those with state-funded pre-K programs in 2005-06 (AL, AZ, CA, CT, DE, KY, MN, MO, NM, NV, OH, OR, WA) [d] Sample is limited to children who turn age 5 between 4 months after and 4 months before the kindergarten entry cutoff birthdate. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6. The First Stage and Impacts on Alternative Care Arrangements, by Family Income and Program Type

States:	Universal states				Targeted states			
Children:	Low income		Not Low Income		Low income		Not Low Income	
	Ineligible Mean	Coef. (se)	Ineligible Mean	Coef. (se)	Ineligible Mean	Coef. (se)	Ineligible Mean	Coef. (se)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Treatment variable:								
Pre-kindergarten	0.122	0.206***	0.286	0.168***	0.132	0.156***	0.159	0.062
		(0.047)		(0.060)		(0.044)		(0.045)
B. Alternatives to Pre-K:								
Head Start	0.177	-0.067	0.019	0.007	0.197	-0.098*	0.024	0.031
		(0.068)		(0.021)		(0.058)		(0.023)
Other center-based care	0.174	-0.049	0.369	-0.121*	0.0804	0.000	0.388	-0.074
		(0.047)		(0.070)		(0.038)		(0.059)
Informal non-parental care	0.208	-0.032	0.122	0.043	0.203	-0.002	0.231	0.022
		(0.047)		(0.050)		(0.054)		(0.052)
Parental care	0.318	-0.058	0.204	-0.097	0.389	-0.056	0.197	-0.040
		(0.072)		(0.062)		(0.064)		(0.046)
Additional controls? [a]		Y		Y		Y		Y
Observations[b]	150	1,850	150	2,200	200	1,900	250	2,400

Notes: Coefficients in the even-numbered columns are on the interaction between a dummy for being eligible for kindergarten in 2006-07 (the same as a dummy for being eligible for pre-K in 2005-06 in a treatment state) and a dummy for being in a treatment state from a regression that also includes dummies for state of residence, for month of assessment, and for month age five relative to the state kindergarten entry cutoff birthdate in 2006-07; see notes to Table 2 for a description and listing of the treatment and comparison states. Sample is limited to children who turn age 5 between 4 months after and 8 months before the cutoff date, and a child is considered low income if his (preschool-age or 2005-06) family income is at or below 185% FPL. Means and regressions are weighted by longitudinal sampling weights, and standard errors (in parentheses) are clustered on state x month of birth.

[a] Additional controls include dummies for female, Hispanic, black non-Hispanic, and low birth weight and (measured in the preschool wave) age at assessment and dummies for non-English at home, mom has high school degree or less, and both biological parents in household.

[b] rounded to the nearest 50.

***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7. Do program standards mediate the universal-targeted reading score gap for low-income kids?

		Add interaction of eligibility with dummy for state program requiring:								
	Baseline (1)	Comp. learning stds. (2)	Teacher BA (3)	Specialized Pre-K Training (4)	15+ Hrs Inservice/Yr (5)	Asst. Teacher CDA+ (6)	Staffing Ratio 10:1 or better, Max Class Size ≤20 (7)	Health Services (8)	One Meal (9)	Site Visits (10)
					A. Reading Scale Scores (Standardized) (N=3,700)					
DDD Coef	0.303***	0.363***	0.188	0.315**	0.377***	0.292**	0.234**	0.309***	0.289**	0.232*
on elig x $treat_s$ x uni_s	(0.112)	(0.114)	(0.122)	(0.128)	(0.128)	(0.118)	(0.112)	(0.106)	(0.112)	(0.119)
p-value	*0.007*	*0.002*	*0.124*	*0.015*	*0.004*	*0.014*	*0.037*	*0.004*	*0.0102*	*0.052*
DDD Coef	-	-0.283**	-0.147	0.025	0.205	-0.050	0.206	0.226**	0.147	0.187
on elig x $treat_s$ x Q_s		(0.133)	(0.123)	(0.170)	(0.132)	(0.179)	(0.142)	(0.107)	(0.107)	(0.158)
					B. Pre-K Enrollment (N=3,700)					
DDD Coef	0.050	0.052	0.062	0.082	0.067	0.022	0.082	0.049	0.047	0.107*
on elig x $treat_s$ x uni_s	(0.053)	(0.054)	(0.067)	(0.073)	(0.063)	(0.056)	(0.056)	(0.053)	(0.051)	(0.059)
p-value	*0.344*	*0.334*	*0.355*	*0.261*	*0.292*	*0.693*	*0.146*	*0.356*	*0.362*	*0.070*
DDD Coef	-	-0.011	0.016	0.067	0.047	-0.127*	-0.096	-0.036	0.031	-0.151**
on elig x $treat_s$ x Q_s		(0.065)	(0.067)	(0.075)	(0.066)	(0.068)	(0.063)	(0.052)	(0.056)	(0.066)

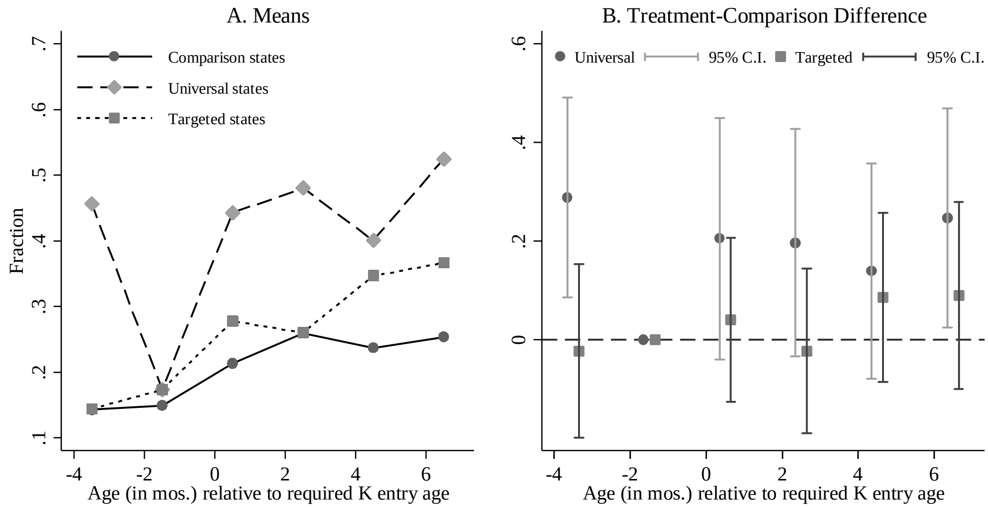
Notes: The table reports estimates of model 4, where Qs is defined as the program standard in the column header. Each column and panel presents the difference in DD estimates between universal and targeted programs; columns 2-10 also present the difference in DD estimates between programs with and without the program standard in question. Sample is limited to children with (preschool-age, or 2005-06) family income at or below 185% FPL. Regressions are weighted by longitudinal sampling weights, and standard errors (in parentheses) are clustered on state x month of birth. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8. Characteristics of Pre-K Programs Serving Low-Income Children in the ECLS-B

	Universal pre-K	Targeted pre-K
A. Teacher Turnover		
Hires in past 12 mos/total employment	0.260	0.273
Departures in past 12 mos/total employment	0.191	0.140
B. Teacher Credentials		
Years of experience in the profession	10.783	12.662
Years of experience at this school	4.880	5.781
4-year college degree +	0.670	0.882
2-year college degree +	0.756	0.950
C. Teacher Attitudes		
Enjoy job	0.728	0.804
Strongly agrees that:		
Pre-K is important for success in K	0.593	0.712
Reading/math instruction in preschool helps children do better in elem. school	0.632	0.498
Children should learn to read in K	0.307	0.366
D. Class size, Curriculum, and Assessment		
# Children	13.895	15.408
Direct assessment used	0.561	0.695
Written curriculum used	0.906	0.895
Observations	150	150

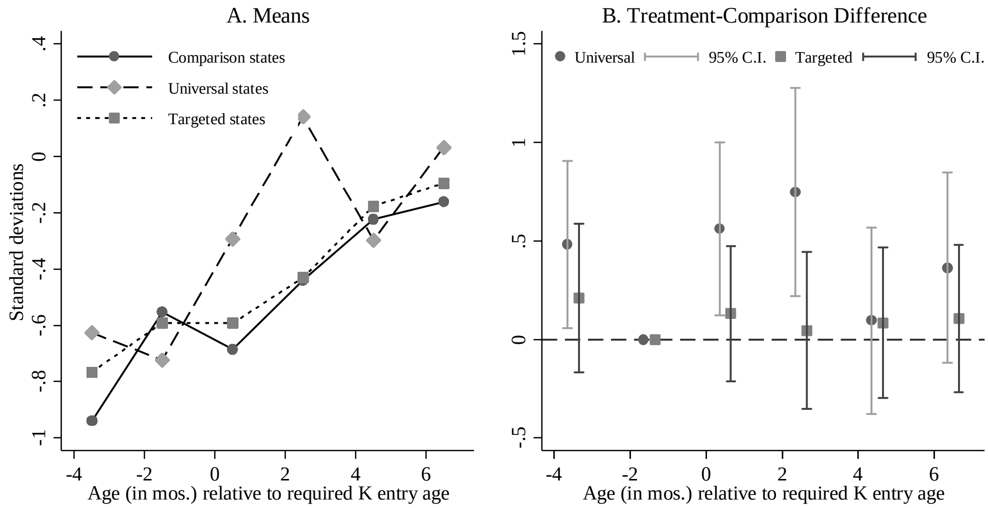
Notes: Samples are limited to low-income respondents in treatment states. Means are weighted by sampling weights.

Figure A1. Prekindergarten Enrollment by Age and State Treatment Status:
Not Low-Income Children



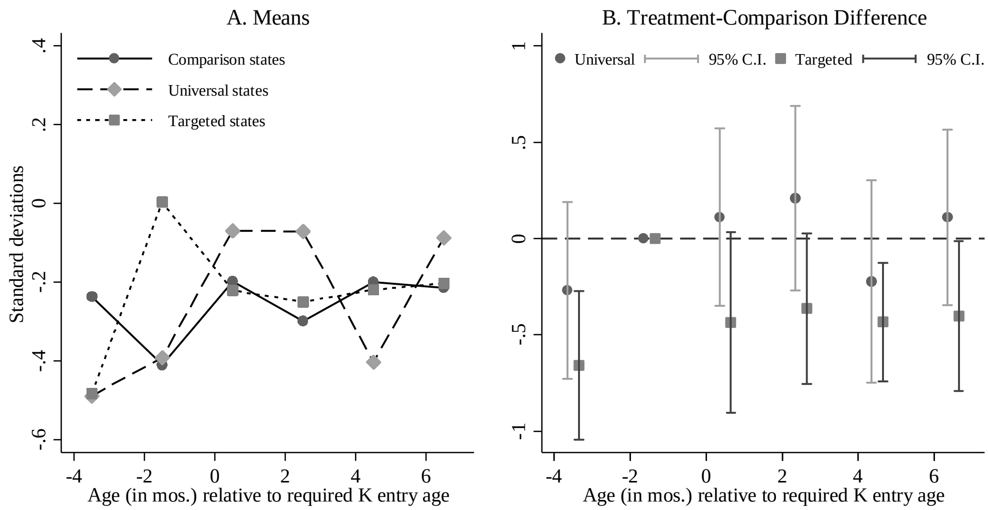
Notes: Data are from the ECLS-B and are restricted to children who, in 2005-06, were not eligible for free or reduced-price lunch. Panel A plots average pre-K enrollment rates at preschool age (in 2005-06) by age relative to the minimum age for kindergarten entry (2-month bins) for treated states with universal pre-K programs, treated states with targeted pre-K programs, and comparison states; see notes to Table 2 for lists of states in each of these groups. The dots in Panel B represent, separately for treatment states with universal programs and treatment states with targeted programs, the coefficients on interactions between a treatment dummy and a series of dummies for age relative to the minimum age for kindergarten entry (2-month bins) from a regression that allows for direct effects of each of these (sets of) variables. The dependent variable is a dummy for pre-K enrollment at preschool age (in 2005-06), and the interaction with the dummy for missing eligibility by 1 to 2 months is omitted to identify the interaction coefficients. The capped vertical lines represent 95% confidence intervals, with standard errors clustered on state by month of birth.

Figure A2. Preschool-Age Math Scores by Age and State Treatment Status:
Low-Income Children



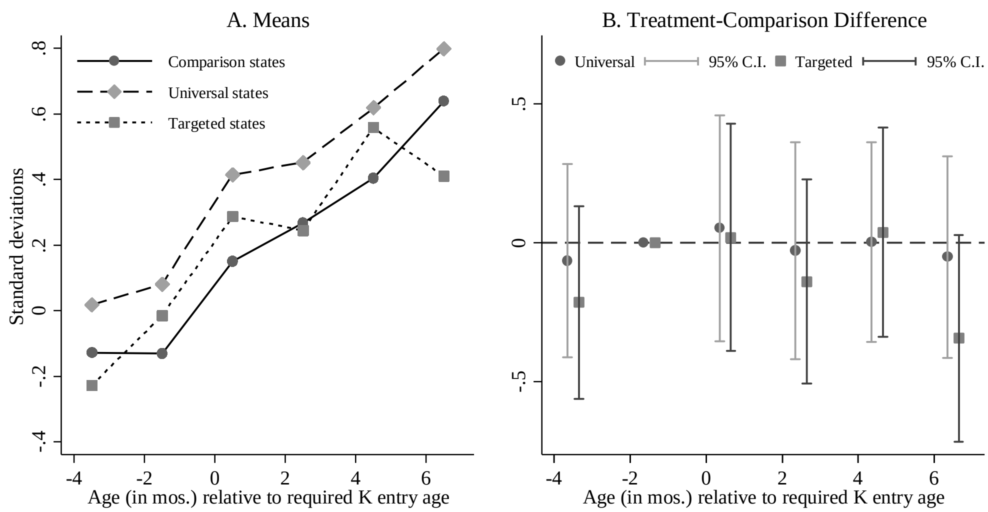
Notes: Data are from the ECLS-B and are restricted to children who, in 2005-06, were eligible for free or reduced-price lunch. Panel A plots average standardized math scores at preschool age (in 2005-06) by age relative to the minimum age for kindergarten entry (2-month bins) for treated states with universal pre-K programs, treated states with targeted pre-K programs, and comparison states; see notes to Table 2 for lists of states in each of these groups. The dots in Panel B represent, separately for treatment states with universal programs and treatment states with targeted programs, the coefficients on interactions between a treatment dummy and a series of dummies for age relative to the minimum age for kindergarten entry (2-month bins) from a regression that allows for direct effects of each of these (sets of) variables. The dependent variable is a standardized math score at preschool age (in 2005-06), and the interaction with the dummy for missing eligibility by 1 to 2 months is omitted to identify the interaction coefficients. The capped vertical lines represent 95% confidence intervals, with standard errors clustered on state by month of birth.

Figure A3. Preschool-Age Socio-emotional Scores by Age and State Treatment Status: Low-Income Children



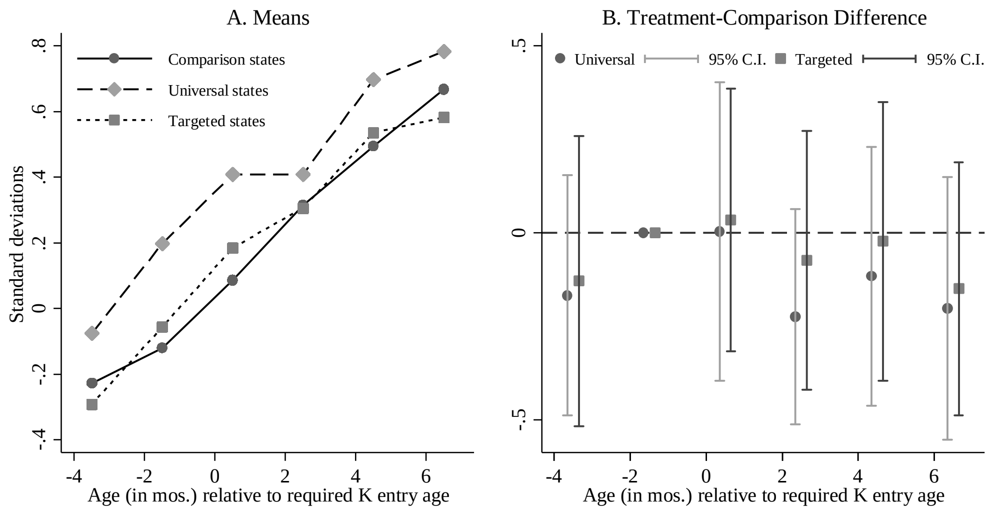
Notes: The dependent variable is standardized socio-emotional score at preschool age (in 2005-06). For further description, see notes to Figure A2.

Figure A4. Preschool-Age Reading Scores by Age and State Treatment Status:
Not Low-Income Children



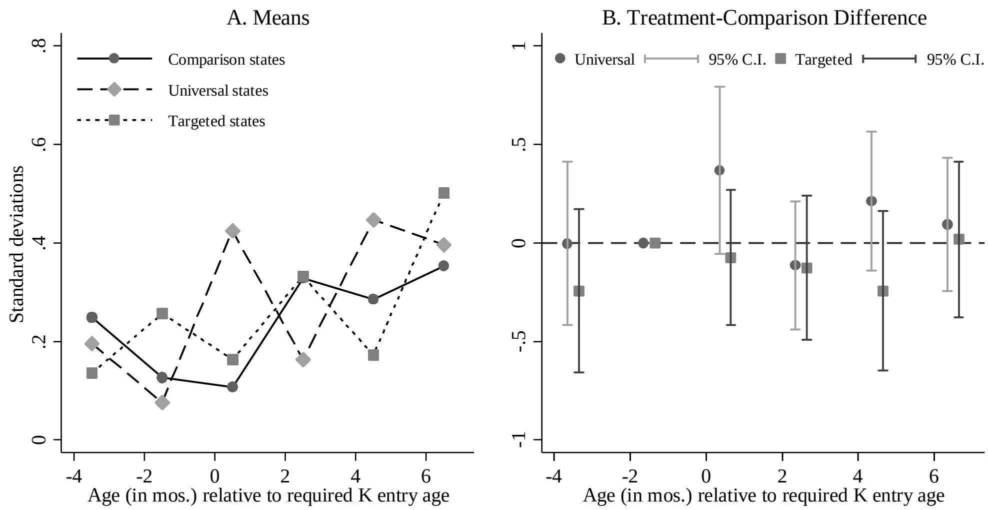
Notes: The dependent variable is standardized reading score at preschool age (in 2005-06), and the ECLS-B sample is restricted to children who, in 2005-06, were not eligible for free or reduced-price lunch. For further description, see notes to Figure A2.

Figure A5. Preschool-Age Math Scores by Age and State Treatment Status:
Not Low-Income Children



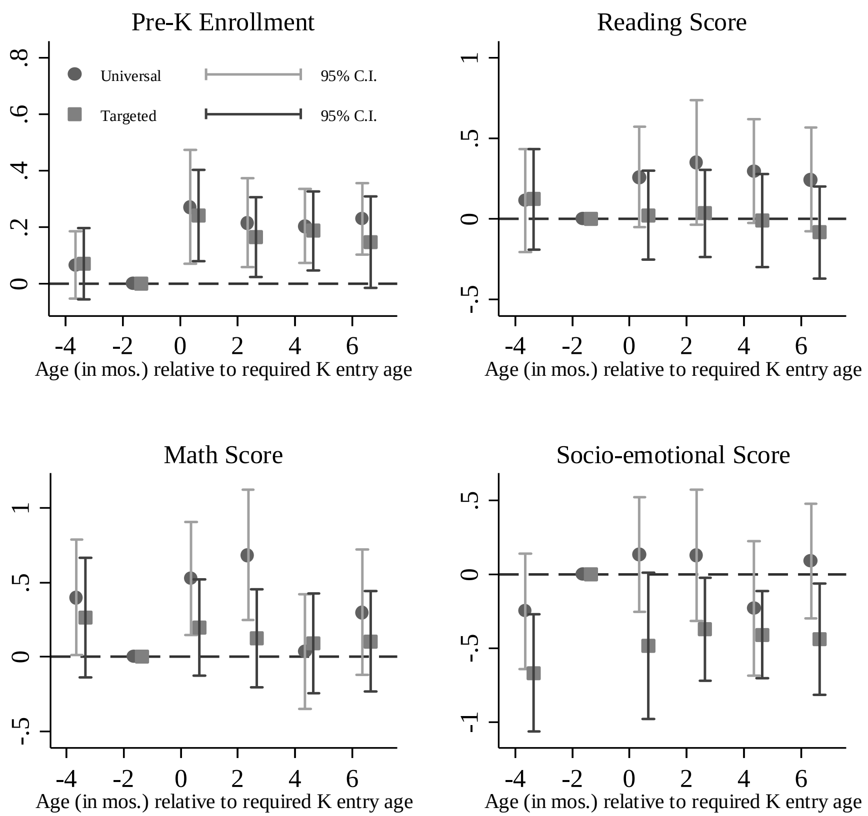
Notes: The ECLS-B sample is restricted to children who, in 2005-06, were not eligible for free or reduced-price lunch. For further description, see notes to Figure A2.

Figure A6. Preschool-Age Socio-emotional Scores by Age and State Treatment Status:
Not Low-Income Children



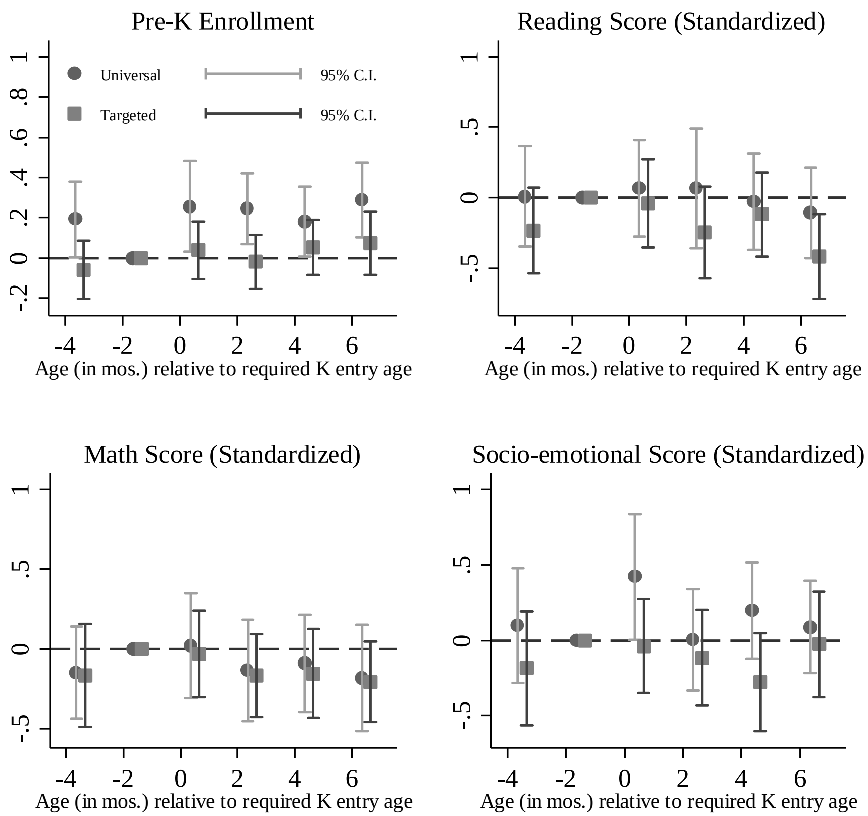
Notes: The dependent variable is standardized socio-emotional score at preschool age (in 2005-06), and the ECLS-B sample is restricted to children who, in 2005-06, were not eligible for free or reduced-price lunch. For further description, see notes to Figure A2.

Figure A7. Event Study Estimates with Controls:
Low-Income Children



Notes: Figures are constructed analogously to those in Panel B of Figures 3, 4, A2, and A3, but also include as controls a vector of state fixed effects, dummies for month of assessment, and the background controls listed in the Panel B of Table 2. See notes to figures referenced for further details.

Figure A8. Event Study Estimates with Controls:
Not Low-Income Children



Notes: Figures are constructed analogously to those in Panel B of Figures A1, A4, A5, and A6, but also include as controls a vector of state fixed effects, dummies for month of assessment, and the background controls listed in the Panel B of Table 2. See notes to figures referenced for further details.

Appendix Table A1. Impacts on Cognitive Test and Socio-emotional Scores at Preschool Age: No Additional Controls

States:	Universal		Targeted	
Children:	Low income (1)	Not low income (2)	Low income (3)	Not low income (4)
	A. Reading Scale Scores (Standardized)			
Ineligible mean	-0.645	0.056	-0.582	-0.102
DD Coef. on eligible x treated state	0.199*	-0.011	-0.071	-0.081
	(0.114)	(0.108)	(0.089)	(0.097)
TSLS Coef. on pre-K enrollment	0.965*	-0.070	-0.454	no f.s.
	(0.546)	(0.681)	(0.604)	
OLS Coef. on pre-K enrollment	0.087	0.294***	0.097	0.397***
	(0.064)	(0.076)	(0.073)	(0.075)
Observations[a]	1,850	2,200	1,900	2,400
	B. Math Scale Scores (Standardized)			
Ineligible mean	-0.680	0.089	-0.642	-0.132
DD Coef. on eligible x treated state	0.168	-0.056	-0.003	-0.057
	(0.140)	(0.096)	(0.106)	(0.095)
TSLS Coef. on pre-K enrollment	0.815	-0.357	-0.021	no f.s.
	(0.632)	(0.628)	(0.674)	
OLS Coef. on pre-K enrollment	0.175**	0.211***	0.166**	0.339***
	(0.078)	(0.059)	(0.074)	(0.062)
Observations[a]	1,850	2,200	1,900	2,400
	C. Socio-emotional Scores (Standardized)			
Ineligible mean	-0.438	0.123	-0.235	0.200
DD Coef. on eligible x treated state	0.181	0.119	-0.112	-0.028
	(0.140)	(0.126)	(0.133)	(0.109)
TSLS Coef. on pre-K enrollment	0.924	0.880	-0.775	no f.s.
	(0.710)	(1.012)	(0.921)	
OLS Coef. on pre-K enrollment	-0.043	0.038	-0.013	0.122*
	(0.094)	(0.066)	(0.089)	(0.062)
Observations[a]	1,600	1,950	1,650	2,150

Notes: The DD coefficient is that on the interaction between a dummy for being eligible for kindergarten in 2006-07 (synonymous with a dummy for being eligible for pre-K in 2005-06 in a treatment state) and a dummy for being in a treated state from a regression that also includes dummies for state of residence, for month of assessment, and for month age five relative to the state kindergarten entry cutoff birthdate in 2006-07; see notes to Table 2 for description and listing of treatment and comparison states. The TSLS coefficient uses eligible x treated state as an instrument for pre-K enrollment in a specification with the same controls. Sample is limited to children who turn age 5 between 4 months after and 8 months before the kindergarten entry cutoff birthdate, and a child is considered low income if his (preschool-age, or 2005-06) family income is at or below 185% FPL. Means and regressions are weighted by longitudinal sampling weights, and standard errors (in parentheses) are clustered on state x month of birth.
[a] rounded to the nearest 50.
***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A2. Additional Robustness Tests on Baseline Difference-in-Differences Estimates: Pre-K Enrollment and Socio-Emotional Test Scores

States:	Universal		Targeted	
Children:	Low income	Not low income	Low income	Not low income
	(1)	(2)	(3)	(4)
	A. Math Scale Scores (Standardized)			
Baseline[a]	0.207	-0.004	0.017	-0.055
	(0.129)	(0.097)	(0.109)	(0.090)
Age 2 scores as controls[b]	0.200*	0.014	0.037	-0.027
	(0.120)	(0.096)	(0.102)	(0.092)
Pre-K states only in comp. group[c]	0.286**	0.008	0.079	-0.035
	(0.137)	(0.101)	(0.117)	(0.095)
+/- 4 months from cutoff [d]	0.324**	0.080	0.016	-0.013
	(0.145)	(0.103)	(0.121)	(0.102)
Low income: Maternal ed ≤ HS	0.205	-0.015	0.032	-0.075
	(0.138)	(0.095)	(0.104)	(0.089)
Low income: ≤130% FPL	0.111	0.054	0.082	-0.092
	(0.146)	(0.080)	(0.111)	(0.081)
Drop FL and TX	0.191	-0.007	0.047	-0.015
	(0.174)	(0.129)	(0.130)	(0.094)
	B. Socio-emotional Scores (Standardized)			
Baseline[a]	0.157	0.161	-0.119	-0.041
	(0.132)	(0.121)	(0.132)	(0.105)
Age 2 scores as controls[b]	0.172	0.150	-0.074	-0.035
	(0.141)	(0.119)	(0.137)	(0.104)
Pre-K states only in comp. group[c]	0.201	0.284**	-0.093	0.062
	(0.148)	(0.124)	(0.144)	(0.115)
+/- 4 months from cutoff [d]	0.218	0.229	-0.156	-0.010
	(0.161)	(0.156)	(0.166)	(0.120)
Low income: Maternal ed ≤ HS	0.186	0.176	-0.098	-0.024
	(0.165)	(0.123)	(0.132)	(0.118)
Low income: ≤130% FPL	0.113	0.168	-0.039	-0.065
	(0.169)	(0.112)	(0.162)	(0.098)
Drop FL and TX	0.157	-0.000	-0.134	-0.061
	(0.168)	(0.159)	(0.160)	(0.113)

Notes: The DD coefficient is that on the interaction between a dummy for being eligible for kindergarten in 2006-07 (synonymous with a dummy for being eligible for pre-K in 2005-06 in a treatment state) and a dummy for being in a treated state from a regression that also includes dummies for state of residence, for month of assessment, and for month age five relative to the state kindergarten entry cutoff birthdate in 2006-07. See Table 2 for a definition of treatment and comparison states. Unless otherwise given, sample is limited to children who turn age 5 between 4 months after and 8 months before the kindergarten entry cutoff birthdate, a child is considered low income if his (preschool-age, or 2005-06) family income is at or below 185% FPL, regressions are weighted by longitudinal sampling weights, and standard errors (in parentheses) are clustered on state x month of birth. [a] Additional controls include age at assessment, dummies for female, Hispanic, black non-Hispanic, low birth weight, non-English at home, mom has high school degree or less, and both biological parents in household. [b] Missing test scores imputed and indicated with dummy variables to maintain sample size. [c] Comparison states limited to those with state-funded pre-K programs in 2005-06 (AL, AZ, CA, CT, DE, KY, MN, MO, NM, NV, OH, OR, WA) [d] Sample is limited to children who turn age 5 between 4 months after and 4 months before the kindergarten entry cutoff birthdate. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A3. Do program standards mediate the universal-targeted math and socio-emotional test score gaps for low-income kids?

		Add interaction of eligibility with dummy for state program requiring:								
	Baseline (1)	Comp. learning stds. (2)	Teacher BA (3)	Specialized Pre-K Training (4)	15+ Hrs Inservice/Yr (5)	Asst. Teacher CDA+ (6)	Staffing Ratio 10:1 or better, Max Class Size ≤20 (7)	Health Services (8)	One Meal (9)	Site Visits (10)
				A. Math Scale Scores (Standardized) (N=3,700)						
DDD Coef	0.190	0.203	0.234	0.161	0.167	0.181	0.152	0.191	0.187	0.168
on elig x $treat_s$ x uni_s	(0.144)	(0.139)	(0.191)	(0.213)	(0.175)	(0.155)	(0.147)	(0.145)	(0.141)	(0.156)
p-value	*0.189*	*0.146*	*0.223*	*0.450*	*0.339*	*0.244*	*0.304*	*0.190*	*0.185*	*0.282*
DDD Coef	-	-0.064	0.056	-0.059	-0.062	-0.040	0.114	0.039	0.030	0.058
on elig x $treat_s$ x Q_s		(0.166)	(0.193)	(0.222)	(0.182)	(0.170)	(0.180)	(0.140)	(0.147)	(0.197)
				B. Socio-emotional Scores (Standardized) (N=3,200)						
DDD Coef	0.276*	0.265	0.306*	0.419**	0.275	0.233	0.313*	0.276*	0.257*	0.320*
on elig x $treat_s$ x uni_s	(0.154)	(0.176)	(0.174)	(0.165)	(0.176)	(0.158)	(0.167)	(0.154)	(0.151)	(0.178)
p-value	*0.074*	*0.134*	*0.078*	*0.012*	*0.119*	*0.142*	*0.061*	*0.073*	*0.090*	*0.073*
DDD Coef	-	0.052	0.040	0.306	-0.004	-0.198	-0.105	-0.066	0.140	-0.109
on elig x $treat_s$ x Q_s		(0.226)	(0.171)	(0.202)	(0.169)	(0.291)	(0.207)	(0.152)	(0.151)	(0.230)

Notes: The table reports estimates of model 4, where Qs is defined as the program standard in the column header. Each column and panel presents the difference in DD estimates between universal and targeted programs; columns 2-10 also present the difference in DD estimates between programs with and without the program standard in question. Sample is limited to children with (preschool-age, or 2005-06) family income at or below 185% FPL. Regressions are weighted by longitudinal sampling weights, and standard errors (in parentheses) are clustered on state x month of birth. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A4. Do demographics mediate the universal-targeted test score gaps for low-income kids?

		Add interaction of eligibility with:							
	Baseline (1)	Age in Months (2)	Female (3)	Black non-Hispanic (4)	Hispanic (5)	Low Birth Weight (6)	Maternal Ed. ≤ HS (7)	Both Biological Parents in HH (8)	Non-English at Home (9)
				A. Reading Scale Scores (Standardized) (N=3,700)					
DDD Coef	0.303***	0.302***	0.300***	0.304***	0.281**	0.303***	0.303***	0.295***	0.294***
on elig x $treat_s$ x uni_s	(0.112)	(0.115)	(0.113)	(0.113)	(0.112)	(0.112)	(0.114)	(0.113)	(0.110)
DDD Coef	-	-0.001	0.053	0.143	0.014	0.165*	0.253**	-0.151	0.044
on elig x $treat_s$ x X_i		(0.013)	(0.098)	(0.106)	(0.105)	(0.090)	(0.103)	(0.094)	(0.115)
				B. Math Scale Scores (Standardized) (N=3,700)					
DDD Coef	0.190	0.190	0.183	0.191	0.188	0.192	0.190	0.188	0.197
on elig x $treat_s$ x uni_s	(0.144)	(0.143)	(0.144)	(0.145)	(0.149)	(0.145)	(0.145)	(0.144)	(0.145)
DDD Coef	-	-0.006	0.114	0.074	0.016	0.036	0.046	-0.036	0.127
on elig x $treat_s$ x X_i		(0.015)	(0.105)	(0.135)	(0.124)	(0.109)	(0.114)	(0.102)	(0.122)
				C. Socio-Emotional Scores (Standardized) (N=3,200)					
DDD Coef	0.276*	0.282*	0.276*	0.278*	0.224	0.275*	0.277*	0.276*	0.267*
on elig x $treat_s$ x uni_s	(0.154)	(0.154)	(0.153)	(0.153)	(0.161)	(0.155)	(0.153)	(0.155)	(0.156)
DDD Coef	-	-0.034	0.046	0.238	0.143	-0.010	0.059	-0.023	0.279*
on elig x $treat_s$ x X_i		(0.022)	(0.132)	(0.159)	(0.150)	(0.137)	(0.174)	(0.126)	(0.143)

Appendix Table A4 (cont'd). Do demographics mediate the universal-targeted test score gaps for low-income kids?

		Add interaction of eligibility with:							
	Baseline (1)	Age in Months (2)	Female (3)	Black non-Hispanic (4)	Hispanic (5)	Low Birth Weight (6)	Maternal Ed. ≤ HS (7)	Both Biological Parents in HH (8)	Non-English at Home (9)
					D. Pre-K Enrollment (N=3,700)				
DDD Coef	0.050	0.052	0.051	0.049	0.051	0.050	0.050	0.045	0.044
on elig x treat_s x uni_s	(0.053)	(0.053)	(0.052)	(0.052)	(0.053)	(0.053)	(0.052)	(0.052)	(0.053)
DDD Coef	-	-0.007	-0.027	0.085	-0.029	0.019	-0.076	-0.107**	-0.127**
on elig x treat_s x X_i		(0.007)	(0.048)	(0.052)	(0.057)	(0.056)	(0.054)	(0.049)	(0.057)

Notes: The table reports estimates of model 4 but substituting demographic and background characteristics for Qs; the relevant characteristic is given in the column header, and the specification also includes a a two-way interaction between the eligbility indicator and the demographic characteristic. Each column and panel presents the difference in DD estimates between universal and targeted programs; columns 3-9 also present the difference in DD estimates for individuals with and without the demographic characteristic (the column 2 interpretation is slightly different due to the continuous nature of the characteristic). Sample is limited to children with (preschool-age, or 2005-06) family income at or below 185% FPL. Regressions are weighted by longitudinal sampling weights, and standard errors (in parentheses) are clustered on state x month of birth. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.