Getting Down to FACTS

Who Benefits from Public PreK Expansions & Increased K-5 Spending? Dynamic Complementarity in California's Education Policies

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Who Benefits from Public PreK Expansions & Increased K-5 Spending? Dynamic Complementarity in California's Education Policies

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Abstract

From 2009-10 to 2018-19, three major public investments in educational opportunity reshaped California's preschool and elementary school landscape. First, in 2009, the state created the California State Preschool Program (CSPP) to consolidate fragmented funding streams into a single contract for publicly funded preschool serving three- and four-year-olds from lower- and middle-income families. Second, in 2012, the state began implementing the Kindergarten Readiness Act of 2010 (KRA), which (a) instituted a delay in the kindergarten eligibility start age for children turning age 5 late in the year (students who would otherwise be "young-for-grade") and (b) introduced Transitional Kindergarten (TK) for those children in the year before kindergarten. The policy was a direct response to the advantage of entering kindergarten later, an advantage that was out of reach for many families who could not afford to delay kindergarten entry due to the high costs of child care. Third, in 2013, the state began implementing the Local Control Funding Formula (LCFF), which overhauled California's K-12 finance system, increasing levels of funding and directing per-pupil funding to districts in proportion to their enrollment of English learners, low-income students, and foster youth. Each reform directed resources disproportionately to disadvantaged children and districts, reflecting a sustained state commitment to narrowing disparities in educational opportunity. Together, these three investments

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reached successive cohorts of children with varying degrees of intensity during sequential developmental stages of childhood.

We leverage the staggered implementation of these reforms to estimate the independent causal effects of CSPP per-pupil spending and TK attendance on math and reading achievement in grades 3 and 4, and to identify potential synergistic effects between TK attendance and LCFF-induced increases in elementary school spending. We use population student-level administrative longitudinal data for the full universe of California public school students, focusing on the period from 2009 through 2019. To facilitate causal inference, we exploit three sources of quasi-experimental variation: CSPP contractor-level funding shocks, birthdate cutoffs for TK eligibility, and the staggered rollout of LCFF-induced changes in per-pupil elementary school funding. Each source supports a distinct identification strategy, and we integrate these strategies within a unified framework to estimate each investment's independent causal effects on student achievement and to identify potential interactions across them. We supplement these findings with event study analysis of the KRA's overall effect on math and reading achievement.

California's investments in CSPP, TK, and elementary school spending delivered substantial, equity-enhancing gains in student achievement, and their effects reinforce one another across the preschool and early elementary grades. The results suggest that sequenced public investments in educational opportunity can produce developmental multiplier effects that exceed the sum of their independent effects.

Summary of Key Findings[2]

- $1,000 increase in CSPP per-pupil spending led to almost ¼ of a year greater learning gains in math and reading achievement three and four years after kindergarten.
- Among students whose parents are low-income and speak English, TK attendance led to 6 months more learning gains in 3rd grade reading and math achievement on average, relative to the typical student from the same school in the same kindergarten cohort who did not attend TK.
    - Average effects on students with parents who do not speak English were relatively modest. TK attendance led to about 1.8 months of learning gains in math achievement and 2.4 months in reading achievement.
- TK impacts on both math and reading achievement among socioeconomically disadvantaged students were significantly larger among those who first attended CSPP at age 3. TK impacts on reading and math scores were roughly 0.2-grade-levels higher for these early CSPP-goers.
    - TK led to roughly half a year of learning gains in math and reading for students with non-English-speaking parents, when preceded by CSPP attendance.
- Achievement gains were consistently and substantially larger in schools with higher levels of per-child funding.
- TK-induced gains in 3rd-grade math and reading achievement persisted into fourth grade
    - Gains in math were particularly persistent.
    - Persistence appears stronger where per-child funding was higher.

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- Gains induced by TK participation and increases in per-child funding were mutually reinforcing: TK attendance and LCFF-induced spending increases exhibit positive, synergistic impacts.
- Significantly smaller TK effects for non-poor children on average (likely due to greater access to high quality private preK in counterfactual care setting).
- Effects of TK attendance were found to be smaller for children whose parents are low-income and who do not speak English.

Introduction

Over the past four decades, preschool has become the norm for most children in the United States. Although the majority of children attend preschool, the cost can be exorbitant for many families. Public preschool programs seek to ensure that the potential benefits of early education are accessible to all, regardless of family income. From 1980 to 2021, the number of states funding preschool programs increased from seven to 44 states and Washington D.C. (Mitchell, 2001; Friedman-Krauss et al., 2022). States spent almost $10 billion on preschool programs in 2021, up from $2.4 billion in 2001 -- growth driven primarily by school-based public pre-k investments. Public investment in preschool reflects a societal interest in equality of educational opportunity, and publicly funded programs have served more than half of all preschool students since the turn of the century. In California, the 2020 Master Plan for Early Learning and Care established the intent to (1) "Phase-in Universal Preschool for all four-year-olds, starting with highest-need areas" and (2) "Phase-in access to preschool for all income-eligible three-year-olds, starting with highest-need areas." California's 2025-26 budget commitments of $4.1 billion for Transitional Kindergarten and $2.8 billion for State Preschool reflect the state's commitment to ensure Universal Preschool for all four-year-olds and preschool access for all income-eligible three-year-olds, as established in the 2020 Master Plan.[3]

Nationally, the expansion of public preschool has enjoyed some success, frequently in the form of one-year pre-kindergarten (pre-k) programs for four-year-olds. A consensus statement issued by a panel of national experts claimed, "Convincing evidence shows that children attending a diverse array of state and school district pre-k programs are more ready for school at the end of their pre-k year than children who do not attend pre-k." The effects of public spending on preschool appear to promote equality of educational opportunity, as preschool-induced learning gains are larger for socioeconomically disadvantaged children and multi-language learners who often exhibit lower levels of skill accumulation at kindergarten entry (Phillips et al., 2017; Elder and Lubotsky, 2009).

However, evaluations of public pre-k programs have revealed substantial heterogeneity in the

[3] Legislative Analyst's Office. (2025, November 14). *The 2025-26 California spending plan: Child care and state preschool*. https://lao.ca.gov/Publications/Report/5088

effects of preschool attendance on child outcomes, and initial learning gains evident at kindergarten entry typically fade away as children progress through school (Duncan and Magnuson, 2013; Gibbs et al., 2013; Elango et al., 2015; Abenavoli, 2019; Bailey et al., 2020). While several studies of contemporary, large-scale preschool programs have found positive short- and long-term effects on a variety of student outcomes, some have found null and even negative results (Durkin et al., 2022; Gray-Lobe et al., 2023; Berne, 2024). There are many unresolved puzzles in the empirical literature on the causal impacts of public pre-k investments.

The study of public pre-k investments is vexed by potential interactions with both antecedent and subsequent educational experiences. There is general consensus and substantial evidence that educational investments are most successful when they build on prior learning gains. Scholars have long hypothesized that preschool students' subsequent schooling environments may moderate the impacts of the early education programs they attend (Zigler, 1978). Substantial scholarship has investigated the extent to which interactions between early and later educational experiences explain scholastic success, but evidence remains limited (Bailey et al., 2020a; Bailey et al., 2020b; Burchinal et al., 2022; Currie and Thomas, 2000; Johnson and Jackson, 2019). The potential influence of antecedent preschool experiences on the impacts of pre-k investments is far less developed, though roughly half of all children attend center-based child care or preschool at age three before enrolling in one-year pre-k programs at age four (Friedman-Krauss et al., 2020). The identification of potential synergistic effects across sequenced educational investments has been hobbled by the scarcity of independent, sequenced sources of exogenous variation therein. This study aims to fill part of this void, and provides new causal evidence on the potential interactive effects of public preK and elementary school investments on student achievement trajectories.

Beginning in 2009, California enacted three education policy reforms that provide a unique opportunity to evaluate the independent effects of sequenced public investments in educational opportunity and to identify potential synergistic effects across them: the California State Preschool Program, Transitional Kindergarten, and the Local Control Funding Formula. A central impetus of each reform was to address educational opportunity gaps among children from lower-income families and children whose parents do not speak English that emerge early in childhood.

In 2009, the state consolidated fragmented funding streams for center-based care of preschool-age children into a single contract, the California State Preschool Program (CSPP). CSPP serves three- and four-year-olds from lower- and middle-income families, and the income eligibility threshold is comparably high relative to Head Start. We find that one in four children who entered California's public schools first attended a CSPP-funded preschool for an average of 13 months, and CSPP three-year-olds accounted for one in four children attending state preschool anywhere in the nation.

In 2012, the state began implementing the Kindergarten Readiness Act of 2010 (KRA), which delayed the kindergarten start age for children turning five late in the year and introduced Transitional Kindergarten (TK) for these children in the year before kindergarten. TK eligibility was universal for children whose birthdays fell within the eligible age range, and all school districts were required to provide access.

In 2013, the state began implementing the Local Control Funding Formula (LCFF), which overhauled California's K-12 finance system. In the decade preceding LCFF, California consistently ranked among the lowest state systems in per-pupil spending adjusted for cost of living, with persistently large educational opportunity gaps across socioeconomic divides. LCFF increased funding levels and directed per-pupil resources to districts in proportion to their enrollment of English learners, low-income students, and foster youth, with additional concentration grants for districts where these students make up a large share of enrollment.

Each reform directed resources disproportionately to disadvantaged children and districts, and together they reached successive cohorts of children with varying degrees of intensity during sequential developmental stages of childhood.

In this report, we investigate the independent causal effects of CSPP per-pupil spending and TK attendance on math and reading achievement in third and fourth grades and on gaps in achievement by parental socioeconomic status and language. Specifically, we ask: how did the KRA affect access to CSPP and TK, and subsequent achievement? What are the independent effects of CSPP spending and TK attendance on student achievement? Do the effects of TK attendance depend on antecedent CSPP

participation? Do TK attendance and LCFF-induced spending increases reinforce one another? And how do effects differ by parental socioeconomic status and language? We use population student-level administrative data for the full universe of California public school students, linking student- and provider-level preschool records to K-12 longitudinal data to trace children's experiences from preschool through the elementary grades. Our analysis focuses on 2009 through 2019, up to but not including the onset of the COVID-19 pandemic.

To facilitate causal inference, we exploit three distinct sources of quasi-experimental variation within a unified empirical framework. We identify the effects of CSPP spending using a shift-share instrumental variable that interacts contractor-level funding shocks with historical enrollment patterns linking CSPP contractors to the districts they serve. We identify the effects of TK attendance using a regression discontinuity design that exploits the December 2 birthday cutoff for TK eligibility. We identify the effects of elementary school spending using a simulated instrumental variable approach derived from the pre-post funding change induced by the 7-year staggered rollout of the LCFF funding formula. The unified framework estimates the independent effects of each investment and identifies synergistic effects between TK attendance and LCFF-induced spending increases. We supplement these strategies with a difference-in-differences event study design that leverages the staggered implementation of the KRA to estimate its effects on preschool enrollment and subsequent achievement.

We find that each investment produced substantial, equity-enhancing gains in student achievement. A $1,000 increase in CSPP per-pupil spending led to almost a quarter of a year of additional learning in math and reading three and four years after kindergarten, independent of TK attendance and elementary school spending. TK attendance led to roughly six months of additional learning in math and reading for low-income children whose parents speak English, with significantly smaller effects for non-poor children, whose families had greater access to high-quality alternatives in the absence of TK. Effects of TK attendance for children whose parents do not speak English were more modest on average, but roughly five times larger in math and three times larger in reading among those who first attended CSPP as three-year-olds. TK effects on students with low-income, English-speaking parents were also significantly larger among those that first attended CSPP. TK effects

were consistently and substantially larger in schools with higher levels of LCFF-induced per-pupil funding, and the gains induced by TK and elementary school spending were mutually reinforcing — evidence of dynamic complementarity across preschool and K-12 investments. Event study estimates indicate that the first cohorts affected by the KRA, who attended CSPP at age three and TK at age four at substantially higher rates than their predecessors, exhibited reading achievement nearly a full grade level above and math achievement roughly two-thirds of a grade level above comparable earlier cohorts.

The remainder of this report is organized as follows. Section II provides background on the three reforms and their policy context. Section III describes the data, including the linked preschool and K-12 administrative records. Section IV presents the research design and empirical strategy. Section V reports the main results. Section VI discusses implications and concludes. Supplementary analyses, including validation of the regression discontinuity design and robustness checks, are presented in the appendices.

Background

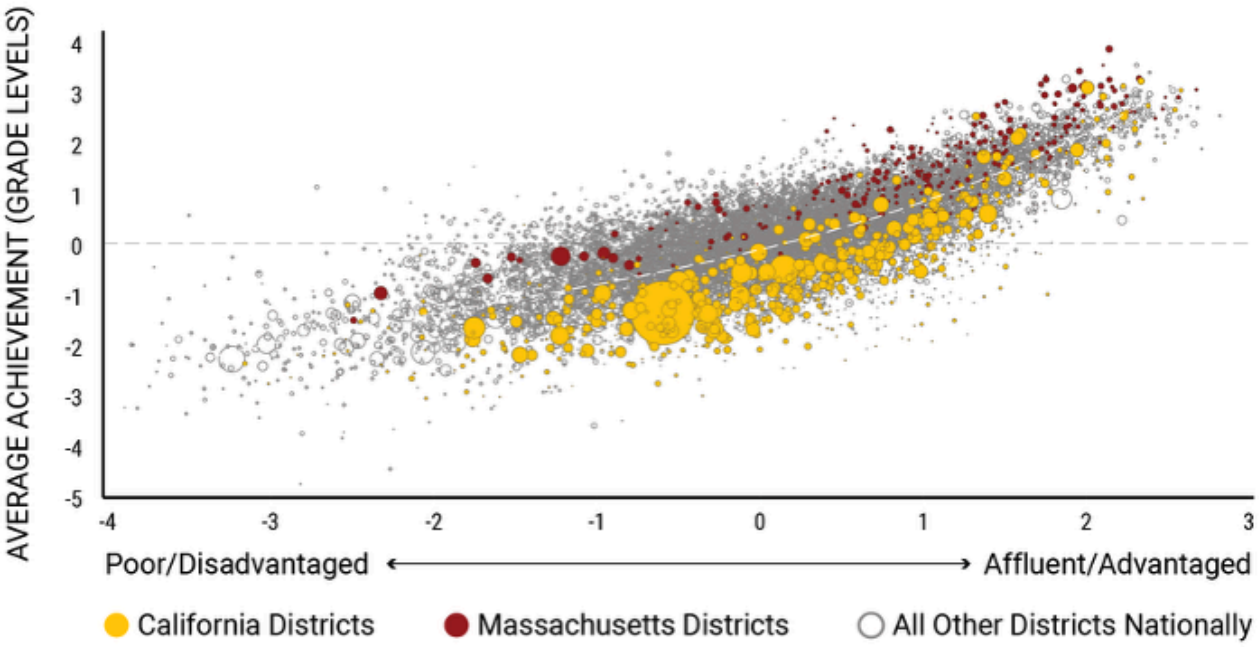
California's Policy Context

One in eight students in the United States is educated in California's public school system (NCES, 2023), the largest and most diverse state system in the country. The sheer size and diversity of the education system, and the timing of its reforms, enable unique insights to be gleaned from the diverse experiences of California's school children, which may also provide valuable lessons for other state systems.[4] Forty percent of California students speak a language other than English at home (California Department of Education, 2020). Much research has documented large and persistent achievement gaps by parental socioeconomic status and language (Reardon and Portilla, 2016; Novicoff, Reardon, and Johnson, 2025; Fry, 2007; Garcia, 2015; Rumberger and Gándara, 2004).

Figure 1 presents the socioeconomic gradient in student achievement across all U.S. school districts, with California and Massachusetts highlighted for comparison. Massachusetts had adopted a more progressive funding formula with higher 'adequacy' baseline funding levels. The gradient is staggering. In California, students in affluent districts outperformed those in disadvantaged districts on math and reading assessments in grades 3 through 8 by more than two grade levels on average from 2009 to 2013. Analysis of third-grade achievement gaps in California between students classified as English Learners at kindergarten entry and their never-EL peers finds disparities of similar magnitude (Novicoff, Reardon, and Johnson, 2025). Figure 2, reproduced from Novicoff, Reardon, and Johnson (2025), presents a timeline of major policy changes affecting California's English Learners across the study period, including changes in language of instruction, curriculum standards, school funding, and early childhood access.

[4] For example, nationwide, 13% of students have a special education disability (e.g., speech and language delays; dyslexia; autism) and an Individualized Education Plan (IEP). Children who gain access to high-quality ECE programs can enable both increased access to developmental screenings and reduced likelihood that a student is placed in special education (NYC universal preK study Hong, Dragan, Glied, 2019; Jacob and Weiland, 2023; Barnett, 1995; McCoy et al., 2017; Muschkin, Ladd, and Dodge, 2015). This may be because, for children with emergent developmental differences, earlier intervention through ECE programs can reduce the number of years they receive services (Bakken, Brown, and Downing, 2017), or may change students' developmental trajectories so that they are not placed in special education at later stages of their development and matriculation in K12 schools (Mann, McCartney, and Park, 2007).

Figure 1: Relationship between academic achievement and socioeconomic status in California and Massachusetts, 2009–2013



Note: Achievement is based on a composite of math and reading test scores (pooled across grades 3–8) that have been NAEP-normed, standardized, and converted into grade-level-equivalent units, following methods developed by Sean Reardon and colleagues, where 1 represents student achievement that is one grade level above the national average achievement for that grade, and -1 represents achievement that is one grade level below the national average for that particular grade. The size of the dot is proportional to district enrollment size. The definition of district socioeconomic status (SES) used along the x-axis is a district SES composite index computed as the first principal component factor score of the following measures: median income, percentage with a bachelor's degree or higher, poverty rate, Supplemental Nutrition Assistance Program (SNAP) rate, single mother–headed household rate, and unemployment rate.

Source: Author analysis of data from Reardon, S., Fahle, E., Ho, A., Shear, B., Kalogrides, D., Jang, H., Chavez, B., & Saliba, J. (2021). *Improving educational equity* [Data set]. Stanford Educational Data Archive.

Figure 2: Selected timeline of California's English Learner-related policies by kindergarten cohort

Policy Category						
	Language of instruction	1998-2016: Prop. 227 in effect, defaulting to English-only instruction for ELs				In 2016, voters pass Prop. 58, repealing Prop 227 and allowing bilingual education.
	Teacher preparation	Beginning in 2004, *Williams v. California* requires teacher preparation programs to train new teachers on EL instruction				
	Curriculum	California Content Standards outline knowledge and skills for each grade level and are not aligned to other states.	In 2010, the State Board of Education adopts the Common Core state standards.	In 2012, the State Board of Education revises the English Language Development standards to align with Common Core. These remain in effect.		
	Funding	California allocates money through "categorical" grant programs with specific purposes and ranked low among other states in funding levels for schools.		In 2013, the Governor signs the Local Control Funding Formula (LCFF), increasing state funding for educ. overall, providing additional money for ELs. Funding amounts are increased incrementally over time.		In 2018, LCFF reaches full funding.
	Early Childhood	Students with disabilities are able to attend limited early childhood options. Low-income three- and four-year-olds can attend federal Head Start program or California State Pre-Schol Program, depending on eligibility.		From 2012-2015, CA expands transitional kindergarten (TK) for all students turning five late in the calendar year.		

Source: figure reproduced from Novicoff, Reardon, Johnson (2025)

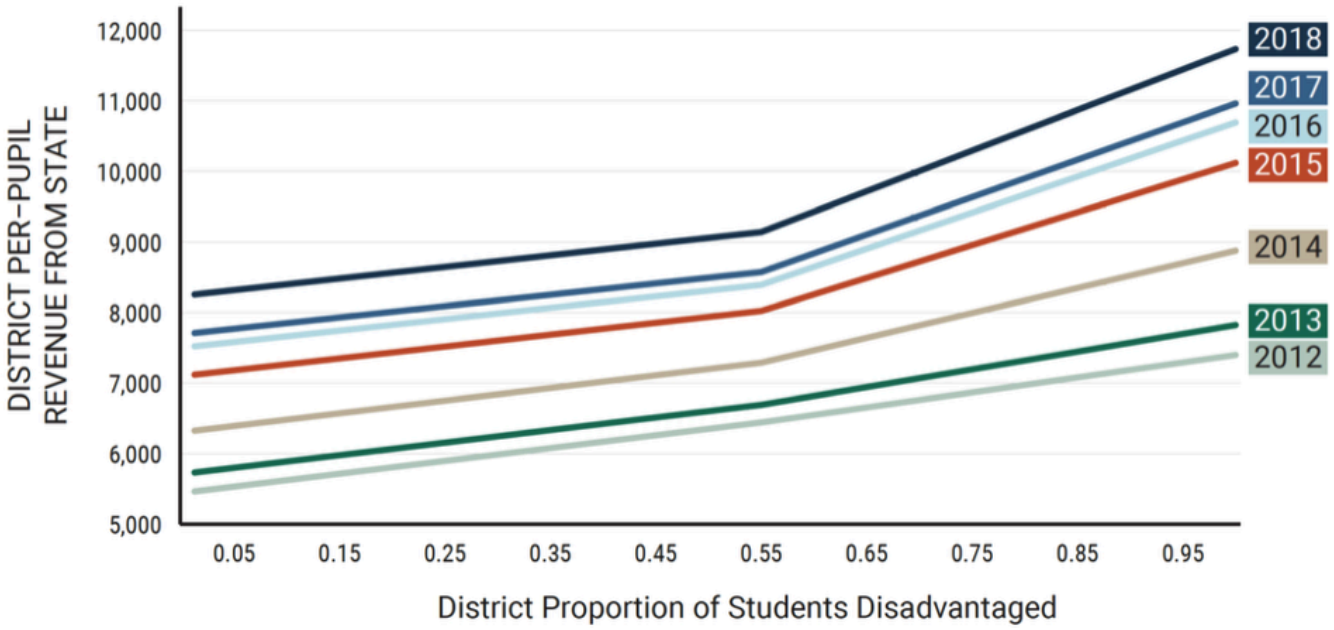
Prior nationally representative evidence from the Early Childhood Longitudinal Study (ECLS-K) has revealed that roughly half of the achievement gap observed in third grade was already evident at kindergarten entry (Loeb and Bassok, 2007), reflective of the significant footprint of early childhood experiences. These gaps are partly explained by disparities in access to early learning experiences between children from higher- and lower-income families (Magnuson and Duncan, 2016; Magnuson and Waldfogel, 2016; Johnson and Jackson, 2019; Chaudry et al., 2021).

In California, kindergarten entry patterns compounded these disparities. Prior to the KRA, California's December 2 kindergarten eligibility cutoff was among the latest in the nation, meaning children who turned five in the fall routinely entered kindergarten at age four — particularly children from lower-income families who more often could not afford to delay entry. Children from more affluent families who turned 5 in September-November were more likely to delay kindergarten entry, a practice known as redshirting, which afforded their children an additional year of development and, often, an additional year of preschool before entering kindergarten. The result was that the developmental advantage of entering kindergarten at an older age accrued disproportionately to children from higher-income families, exacerbating the achievement gap by parental SES and language observed at kindergarten entry in California.

In the decade leading up to LCFF passage, California consistently ranked among the lowest 15 state systems in average per-pupil spending adjusted for cost of living, with persistently large

educational opportunity gaps across socioeconomic divides left unaddressed. LCFF directs supplemental funding to districts based on their enrollment of English Learners, low-income students, and foster youth, with additional concentration grants for districts where these students make up a large share of enrollment. The reform also eliminated many state categorical programs, reduced restrictions on how funding could be utilized, and increased local control and autonomy over spending decisions. Figure 3 depicts the staggered rollout of LCFF from 2012 through 2018-19, illustrating the progressive structure of the formula and the magnitude of funding increases for higher-poverty districts. LCFF resulted in substantial increases in public education investments in TK-12, sustained in recent years. Funding for K-12 public schools in the 2022-23 state budget was nearly triple the funding received in 2011.

Figure 3: Funding formula amounts and the 2013-2018 rollout of LCFF



The California State Preschool Program

The California State Preschool Program (CSPP), originally established in 1965 and administered by the California Department of Education (CDE), is one of the largest public preschool programs in the United States. CSPP today serves three- and four-year-old children from families with incomes below the state median income, or about $127,000 per year for a family of four in the 2025-26 school year. About 63 percent of three- and four-year-old children are eligible for the program today, and eligibility was comparably broad during the study period.[5] Before the recent expansion of Universal Transitional Kindergarten, CSPP was the state's flagship preschool program. Using newly assembled longitudinal data linking student-level preschool and K-12 records, we find that roughly one in four students enrolling in kindergarten in California's public schools from 2009 to 2015 first attended CSPP for an average of 13 months. CSPP enrollment accounted for about one in four three-year-olds attending state preschool anywhere in the United States from 2010 to 2020 (Friedman-Krauss et al., 2021). During the introduction of the TK program from 2012-13 to 2014-15, among students who attended TK as four-year-olds, one in five attended CSPP as three-year-olds.

CSPP was originally modeled after Head Start, and many classrooms follow an instructional model typical of Head Start-aligned programs. A 2022 survey of 439 CSPP providers, conducted by CDE, found that nearly half used either Creative Curriculum or HighScope, consistent with national patterns in center-based preschool dating back to 2011 (NSECE, 2012). During the study period (2009-2019), state regulations required a staff-child ratio of 1:8, mirroring Head Start regulations, and lead teachers were required to hold a qualifying Child Development Permit.[6] CSPP offered both part- and full-day programming. The part-day program operated for a minimum of three hours per day and 175 days per year; the full-day program operated for a minimum of 6.5 hours per day and 246 days per year. In

[5] As of 2025-26, the income-eligibility threshold was set at 100 percent of state median income (SMI). The income-eligibility threshold was set at 75 percent of state median income from 1997 or earlier until 2011, when it was lowered to 70 percent of 2005 SMI potentially to protect income among lower-income families as the state implemented recession-era budget cuts. Children are eligible if they are income-eligible, receive cash assistance, are homeless, or are at risk of abuse, neglect, or exploitation.

[6] During the study period, teachers were required to hold a Child Development Permit issued by the Commission on Teacher Credentialing, with the lowest qualifying level — the Associate Teacher permit — requiring 12 semester units of ECE coursework (5 CCR §80111). This permit was renewable once, but holders were required to complete the full 24-unit Teacher Permit within ten years. The Education Code required that classroom aides be at least 18 years of age (EC §8360(a)(1)).

practice, many providers operated beyond these minimums. Eligibility for full-day programming required demonstration of parental participation in work, training, or educational activities, or incapacitation.[7] Four-year-olds and children with greater demonstrated need were prioritized for enrollment. In addition to classroom instruction, CSPP programs provide meals, parent education, and referrals to health and social services.

California state preschool providers, which include both local education agencies and community-based organizations, are funded via direct contract with CDE. Total contract funding for CSPP reached nearly $1 billion in 2009-10 (inflation adjusted, 2018 dollars). The average effective reimbursement was about $28 per child-day of enrollment,[8] or roughly $7,400 per year of enrollment, from 2009 to 2015.

***Enrollment in child care and development programs*.** Though California first established its state preschool program in 1965, the state operated several additional child care and development (CCD) programs before the study period.[9] These programs funded CCD vouchers ("alternative payments") and direct contracts for CCD providers.[10] Child care and development programs were all administered by CDE,[11] with the exception of the largely TANF-funded CalWORKs Stage 1 voucher program for families who had recently entered the CalWORKs program. In 2009, the state consolidated funding of center-based care for preschool-age children into a single contract: CSPP. The consolidation subsumed Part-Day State Preschool (CPRE), Full-Day State Preschool (CFDP), and the Prekindergarten and Family

[7] These need requirements were historically imposed for compliance with federal Child Care and Development Block Grant (CCDBG) funding, which partially supported full-day slots prior to 2014-15. The requirements remain in place, though there is no longer a federal funding justification for their imposition.

[8] The average effective reimbursement rate reflected an average over funded enrollment in part-day and full-day enrollment. The part-day standard reimbursement rate set by the legislature was about $21 per day while the full-day rate was about $34 per day (nominal dollars) from 2009 to 2014.

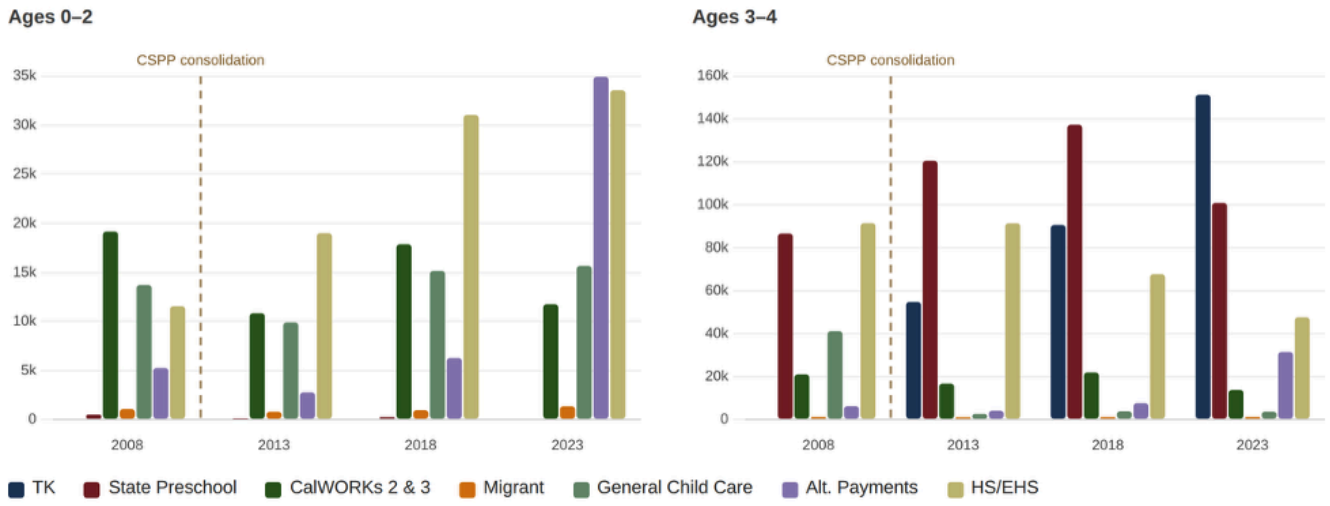
[9] Direct contract funding programs administered by CDE from 2006-07 to 2015-16 included the California State Preschool Program (CSPP), part-day state preschool (CPRE), full-day state preschool (CFDP), General Child Care Program (CCTR), the Family Child Care Education Home Networks Program (CFCC), the Severely Handicapped Program (CHAN), the Extended Day Child Care “Latchkey” program (CLTK), the Migrant Child Care and Development Program (CMIG), and the Prekindergarten Family Literacy Program (PKFLP). Alternative payment programs administered by CDE included CalWORKs Stage 2 (C2AP), CalWORKs Stage 3 (C3AP), the California Alternative Payments Program (CAPP), and the Migrant Alternative Payment Program (CMAP).

[10] Some programs made exceptions to age-eligibility restrictions, including but not limited to services for children with disabilities.

[11] With the exception of CSPP, child care and development programs are now administered by CDSS, pursuant to post-COVID restructuring.

Literacy Program (PKFLP) contracts, and carved out center-based care for preschool-age children from General Child Care (CCTR) contracts, which funded services for children from birth through age 12. Binding program regulations for center-based care of preschool-age children were effectively identical across these contracts.[12] We treat enrollment reported under these predecessor contracts before 2009 as CSPP enrollment for descriptive purposes. Appendix Figure A1 presents enrollment rates by program type and age relative to kindergarten entry, illustrating the role of other CCD programs alongside CSPP, particularly among younger children. Figure 4 presents enrollment trends for CSPP and other CCD programs, as well as Head Start and TK, from 2008 to 2023. The increase in state preschool from 2008 to 2013 reflects the 2009 consolidation.

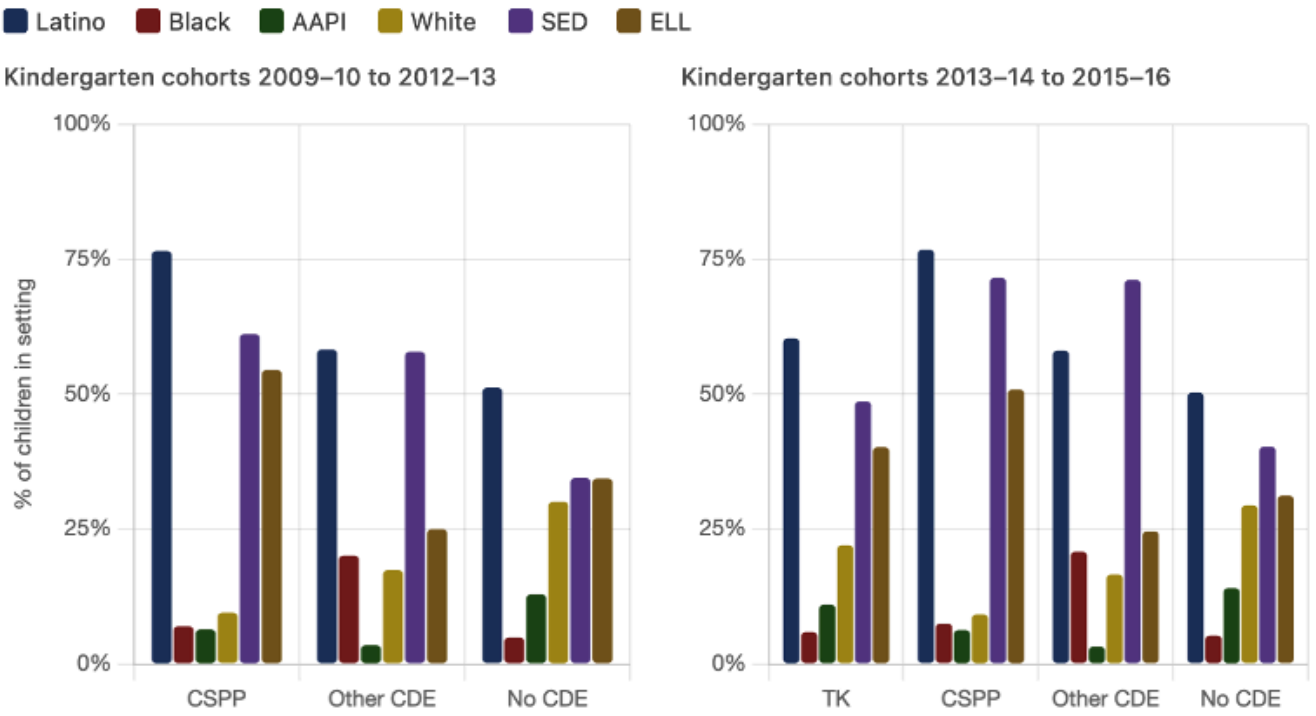
Figure 4: Enrollment in CSPP Programs, TK, & Head Start for Selected Years



Historically, over half of children attending CSPP lived in families with incomes below the federal poverty line. CSPP has long been a major component of California's investments in educational opportunities for English learners, who account for 50 percent of enrollment. Figure 5 presents the demographic composition of children who enrolled in CSPP, other CCD programs, TK, or none.

[12] All licensed child care in California is subject to health, safety, and licensing standards under Title 22 of the California Code of Regulations. CSPP and other CDE-contracted programs were additionally subject to Title 5, which imposed stricter staff-child ratios and teacher qualification standards.

Figure 5: Demographic composition of CA students by ECE program participation



Despite its scale, rigorous evidence on the effects of CSPP participation on child outcomes is limited. Barnett et al. (2018) use a regression discontinuity design to estimate effects of state preschool in eight states, including California. The CSPP sample included 1,630 children, 559 of whom attended CSPP in 2006. The authors found positive effects of CSPP attendance on a measure of early literacy at kindergarten entry (effect size = 1.4) but did not detect effects on language or mathematics assessments. Barnett et al. noted that the California sample exhibited a high rate of non-compliance with the study design, likely because many children in the control group had already enrolled in CSPP at age three. Sulik et al. (2022) use a Mahalanobis distance matching design to study the effects of CSPP attendance on kindergarten readiness among students in San Francisco Unified School District. The study found positive effects of CSPP attendance on literacy and cognitive skills at kindergarten entry across kindergarten cohorts from 2017-18 to 2019-20. The lack of rigorous evidence on the effects of CSPP on children's learning outcomes is striking given its scale and ongoing role in supporting access to early education.

We study the influence of CSPP spending on student outcomes. Public spending on preschool

may improve both the quality of and access to early education. On the quality margin, spending increases may support class size reductions, professional development and coaching, or wage increases that reduce teacher turnover and financial hardship among educators — any of which may foster more stimulating and positive interactions between educators and children. On the access margin, spending may facilitate the expansion of enrollment slots, program operating hours, or days of operation.

A small but growing literature examines the effects of preschool funding on child outcomes directly. Currie and Neidell (2007) match Head Start administrative data to child outcomes in the NLSY and find that programs with higher per-capita spending were associated with better outcomes. Thompson (2018) compares adult outcomes of NLSY79 cohorts with different levels of exposure to Head Start funding, exploiting within-county variation generated by the staggered introduction of local programs, and finds that better-funded programs produced systematically better long-run outcomes even among counties with active programs. Johnson and Jackson (2019) find that per-poor-child Head Start spending increased educational attainment and earnings and reduced the annual incidence of poverty and incarceration in adulthood, with effects amplified by subsequent exposure to higher K-12 school spending. A series of studies examine the more recent, staggered rollout of state funding for preschool in North Carolina. These studies employ a two-way fixed effects framework to estimate effects of per-child spending on scholastic outcomes across the full population of North Carolina public school students (Ladd, Muschkin, and Dodge, 2014; Dodge et al., 2017; Bai et al., 2020). These studies find positive effects on reading and math achievement as late as eighth grade with no evidence of fadeout, as well as reductions in grade retention and special education placement. Watts et al. (2023) draw on the same setting across 18 birth cohorts and find positive effects on fifth-grade achievement. This literature is largely confined to federal Head Start and a single state context. CSPP offers an important opportunity to extend it, as one of the largest state preschool programs in the nation operating in a diverse state with substantial funding variation over the period we study.

The Kindergarten Readiness Act and Transitional Kindergarten

California Legislature's 2010 Kindergarten Readiness Act (KRA) delayed the kindergarten start age and introduced Transitional Kindergarten (TK) for children affected by the delay. The KRA was designed to address the disadvantage faced by children who entered kindergarten young-for-grade, an

advantage of later entry that had been accessible primarily to families who could afford to delay. Just prior to the implementation of TK, California ranked 24th in access to state pre-k for four-year-olds and the costs of private ECE were among the highest in the nation (Barnett et al., 2014; Wood et al., 2014). TK was designed to act as a bridge from preschool to kindergarten by exposing children to curricular content they would be expected to master in kindergarten, supporting social-emotional development, and preparing English Learners for instruction in English. The creation of TK aimed to reduce the income-based gap in kindergarten readiness through universal provision of pre-kindergarten education.

For those age-eligible, TK represented the first universal prekindergarten program in California. Pre-k programs consistently produce larger effects for children from socioeconomically disadvantaged families, and universal eligibility removes the administrative barriers that means-tested programs impose on the families most likely to benefit.[13] Moreover, programs that serve exclusively low-income children may be of lower quality than universal programs like TK, which enjoy a broader political constituency[14] and may be better supported. Some evidence has shown that preschool classrooms are among the most segregated settings — both socioeconomically and racially — in all of care and education. Universal programs feature more integrated learning environments that may be more conducive to learning and socioemotional development.

The first TK cohort enrolled in the 2012-13 school year, restricted to children whose fifth birthdays fell between November 2 and December 2. Eligibility for the second cohort, enrolling in 2013-14, included children whose fifth birthdays fell between October 2 and December 2. Thereafter, all children whose fifth birthdays fell between September 2 and December 2 were eligible for TK. The California Department of Education described TK as the first year of a two-year kindergarten program. TK is operated exclusively in public schools (including charter schools), and per-child funding levels are set by the LCFF. All school districts were required to provide TK access for age-eligible children, unlike TK programs in other states where district participation is voluntary.

[13] Another advantage of universal programs is that there are no administrative hurdles to register for eligibility, whereas if a parent wants to enroll their child for CSPP or Head Start the family must demonstrate need, overcome a great deal of red tape, which can be prohibitive for many families.

[14] e.g., Cascio (2023) reports suggestive evidence that low-income children's outcomes improved more in universal preK programs (vs targeted).

When TK was introduced, it created a new preschool sequence for young-for-grade children. One in five TK-eligible students had already attended CSPP, and the KRA's delay of kindergarten entry conferred an additional year of CSPP eligibility at age four.[15] As a result, TK-eligible children were much more likely to have attended CSPP two years prior to kindergarten than children born in identical months in earlier cohorts. Figure 6 depicts the changes in birthdate eligibility rules for CSPP, TK, and kindergarten before and after the KRA went into effect.

Figure 6. Timing of TK introduction & eligibility policy changes

5th birthday

Birthyear	School Year	Aug	Sep	Oct	Nov	Dec
2006	2011-12	K	K	K	K	CSPP @ 4
2007	2012-13	K	K	K	TK / CSPP	CSPP @ 4
2008	2013-14	K	K	TK / CSPP	TK / CSPP	CSPP @ 4
2009	2014-15	K	TK / CSPP	TK / CSPP	TK / CSPP	CSPP @ 4
2010	2015-16	K	TK / CSPP	TK / CSPP	TK / CSPP	CSPP @ 4

4th birthday

Birthyear	School Year	Aug	Sep	Oct	Nov	Dec
2006	2010-11	CSPP @ 4	CSPP @ 4	CSPP @ 4	CSPP @ 4	CSPP @ 3
2007	2011-12	CSPP @ 4	CSPP @ 4	CSPP @ 4	CSPP @ 4	CSPP @ 3
2008	2012-13	CSPP @ 4	CSPP @ 4	CSPP @ 4	CSPP @ 3	CSPP @ 3
2009	2013-14	CSPP @ 4	CSPP @ 4	CSPP @ 3	CSPP @ 3	CSPP @ 3
2010	2014-15	CSPP @ 4	CSPP @ 3	CSPP @ 3	CSPP @ 3	CSPP @ 3

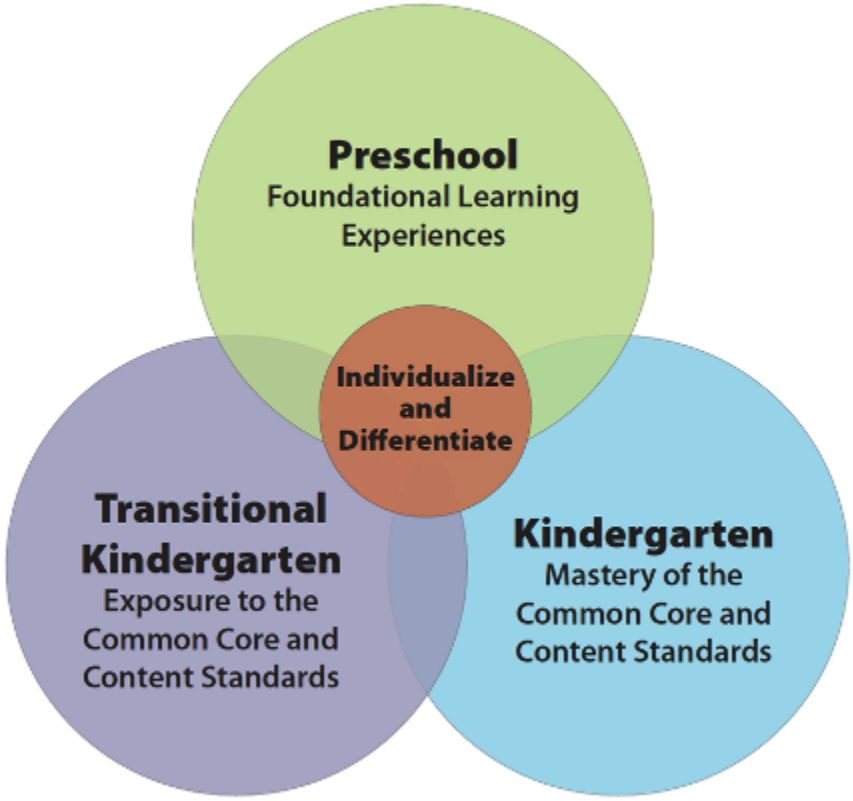
3rd birthday

Birthyear	School Year	Aug	Sep	Oct	Nov	Dec
2006	2009-10	CSPP @ 3	CSPP @ 3	CSPP @ 3	CSPP @ 3	*CSPP @ 3 10-11*
2007	2010-11	CSPP @ 3	CSPP @ 3	CSPP @ 3	CSPP @ 3	*CSPP @ 3 11-12*
2008	2011-12	CSPP @ 3	CSPP @ 3	CSPP @ 3	CSPP @ 3	*CSPP @ 3 12-13*
2009	2012-13	CSPP @ 3	CSPP @ 3	CSPP @ 3	*CSPP @ 3 13-14*	*CSPP @ 3 13-14*
2010	2013-14	CSPP @ 3	CSPP @ 3	*CSPP @ 3 14-15*	*CSPP @ 3 14-15*	*CSPP @ 3 14-15*

15

The CDE's TK Implementation Guide emphasized academically focused curricular content aligned with Common Core K-5 learning goals in English, math, science, and history-social science. CSPP providers used curricula more typical of Head Start-aligned programs, which are frequently whole-child oriented. TK's curricular emphasis on academic content aligned with elementary school learning goals distinguished it from CSPP and was central to its design as a bridge from preschool to kindergarten. Figure 7 from the TK Implementation Guide situates TK apart from preschool and emphasizes its academic focus.

Figure 7. Continuum of Development



Source: Transitional Kindergarten Implementation Guide: A Resource for California Public School District Administrators and Teachers, Child Development Division, CDE.

By regulation, TK classrooms could not exceed a 1:33 teacher-child ratio. Manship et al. (2017), in a study of 189 TK classrooms, found that the average effective ratio was 1:17, that one-quarter of TK classrooms were combined with kindergarten classrooms, and that only 23 percent of TK teachers had

previous experience teaching preschool. TK was not always full day (a minimum of three hours was required), but typically maintained smaller class sizes than kindergarten, and districts had autonomy to require teachers to hold an early childhood education credential. In contrast, CSPP required a staff-to-child ratio no greater than 8:1 and specialized early childhood credentials for lead teachers. By 2018-19, approximately one fifth of four-year-olds enrolled in TK, and the state budget allocated roughly $850 million for TK and $1.3 billion for CSPP.

Data Description

Among studies that have examined impacts of early childhood interventions, the present study is unprecedented in scale and resolution. The analyses draw on detailed school finance information and individual education records for every child who enrolled in public school in California, the largest and most diverse state system in the country, linked with student- and provider-level preschool records to trace children's experiences from preschool through the elementary grades. This student-level data included 6.2 million students across K-12 each year across 10,000 schools and 1,000 districts. Our analysis focuses on kindergarten cohorts from 2009 through 2015 and achievement outcomes observed through 2019.

In addition to student outcome data, student administrative records contain demographic characteristics, birthdate, and attendance information. Annual district financial data contains all general ledger financial records, both expenditures and revenues, for all public school districts in a given year. The analyses include both traditional public schools and charter schools

We convert test scores to national grade-level equivalent units following the methods described by Reardon et al. (2019). This approach accounts for differences in standardized testing regimes over time and converts test scores to a single national scale in three steps. First, we rank each student's scores in the statewide distribution for a given grade, year, and subject. Second, we use data from the National Assessment of Educational Progress (NAEP), administered to a sample of students by the US Department of Education, to convert state-year-grade-subject rankings to a national scale. Third, we

convert the standardized test scores to grade-level equivalents in both math and reading.[16] We benchmark achievement to the subject-specific national average for the 2005 kindergarten cohort, the last California cohort that would not have been exposed to the LCFF through 8th grade. This approach enables cross-cohort comparisons of absolute improvements or declines in learning outcomes relative to a pre-reform national standard. The analysis sample was restricted to students who were observed in a California public school in kindergarten or earlier and followed thereafter.

Because TK was a new program, districts did not always report TK enrollment consistently, and TK students were sometimes combined with kindergarten students for instruction and reporting purposes. We constructed TK attendance from multiple administrative data sources, including enrollment records, birthdates, eligibility rules, and grade progression sequences, and validated our measures against published CDE enrollment counts.[17]

School- and student-level records are assembled to analyze student trajectories from CSPP and TK through fourth grade. The full analysis sample includes 3,083,881 student-year achievement observations in math and reading three to four years after kindergarten, comprising 1,634,901 children from 6,127 public elementary schools across 935 school districts.

Student-level Preschool Records Data & Linking to K-12. This study focuses on birth cohorts eligible to enroll in kindergarten from 2011-12 to 2014-15, whose academic achievement outcomes can be observed three to five years after kindergarten (through 2019). We match some 32 million monthly child-level enrollment records from child care and development programs administered by CDE to longitudinal student-level records from CALPADS for the full universe of public school students in California. These enrollment records were stored in CDE's Child Development Management and Information System (CDMIS), which captures family income, dates of birth, demographic characteristics, county and ZIP code of family residence and service provider, care setting type, and

[16] We analyze student achievement in 2014-2019 in which California's Smarter Balanced test serves as the basis for students' academic achievement measures. The Smarter Balanced test is designed to evaluate students' progress toward mastery of the Common Core State Standards.

[17] As a validation check, we have compared our computed school-level TK enrollment counts over time with those reported in the publicly available numbers on the CDE webpage, and we have also compared our overall annual school and district enrollment numbers with those reported in the publicly available numbers on the CDE webpage.

funding program. We obtain approximately 36 million CDMIS records for fiscal years 2006-07 through 2015-16 and positively match 32 million to unique student records, for an overall match rate of 88 percent. For preschool-age children in the 2008 through 2015 kindergarten birth cohorts, the match rate exceeds 90 percent. The unmatched records likely include children who left California or were homeschooled. These records include roughly 450,000 students per kindergarten cohort, roughly 30 percent of whom were matched to child care and development program records.[18] Among socioeconomically disadvantaged students in California, 30 percent first enrolled in CSPP from 2006 to 2015. Among all children who ever enrolled in any CDE-administered child care and development program, 85 percent were enrolled in CSPP for at least two months. Some 5 percent of all preschool-age children who enrolled in a CDE-administered program never attended CSPP.

We link student CSPP enrollment records to contractor-level financial records detailing funding, earnings, and reported costs. CSPP services during the study period were delivered by a stable set of over 690 contractors, including both school districts and community-based organizations, each operating under an annual contract with CDE. We obtain contract-level fiscal records covering fiscal years 2009-10 through 2015-16. Four fields from these records are central to the analysis: the maximum reimbursable amount (MRA), the annual contract ceiling set at the start of each fiscal year; annual enrollment activity, which records child-days of service actually delivered; earnings, the reimbursements issued to the contractor; and reported eligible costs, the contractor's reported expenditure on CSPP services. The MRA is set before the fiscal year begins; enrollment activity, earnings, and reported eligible costs are realized over the course of the year. We treat reported eligible costs as our empirical measure of CSPP spending. Fiscal records report enrollment activity in child-days, a field not available in the monthly CDMIS enrollment records. Child-days reported in fiscal reimbursement claims closely track child-months observed in CDMIS at approximately 22 child-days per child-month, confirming that the two administrative data sources measure the same underlying enrollment activity.

We augment these data with geodemographic and district-level information from the Common

[18] Monthly child care and development enrollment records include data for all children, primarily ages 0-12, enrolled in the California State Preschool Program (CSPP), the General Child Care Program (CCTR), CalWORKS childcare programs stages 1 and 2, and others.

Core of Data, the Stanford Educational Data Archive (SEDA), the U.S. Census, licensed child care capacity from the California Department of Social Services, Housing Value Index data from Zillow, and ZIP-code level birth counts from the California Health and Human Services Agency. Our key outcome measures are math and reading achievement in third and fourth grades observed in 2014-2019.

Research Design

The Kindergarten Readiness Act

To estimate the causal effects of the KRA on preschool enrollment and subsequent achievement, we exploit the staggered implementation of the policy across the first three kindergarten cohorts for which TK was available (2012-13 through 2014-15). Although the Kindergarten Readiness Act did not itself address CSPP, it altered the timing and sequence of eligibility and prioritization. CSPP prioritizes enrollment of four-year-olds, and the KRA conferred a second year of statutory four-year-old status. In effect, TK-eligible children were much more likely to have enrolled in CSPP two years prior to kindergarten (as four-year-olds) relative to children with identical birthdates who entered kindergarten before the KRA was implemented.

These consequences are consistent with the KRA's goal of expanding preschool opportunities. The child-level enrollment records show that about 10 percent of students attended CSPP two years prior to kindergarten entry during the study period. The TK-eligibility window under the post-KRA schedule includes children turning 5 between September 2 and December 2. Within this window, the CSPP participation rate two years prior to kindergarten was 7 to 8 percent before the KRA and was 20 percent afterwards. Figure 8 illustrates CSPP enrollment rates two years before kindergarten by birth month and cohort as the KRA was implemented.

Figure 8. CSPP Enrollment Rates by Birth Month and Kindergarten Cohort

Cells shaded by event time. Orange = treatment onset (e=0). Blue = pre-treatment. Gray = base period (e=−1). Em dash = entry delayed by KRA.

	Control			Treated		
Kcohort	**Jun**	**Jul**	**Aug**	**Sep**	**Oct**	**Nov**
2009	.089	.085	.079	.070	.064	.060
2010	.096	.094	.084	.076	.069	.062
2011	.100	.098	.092	.083	.074	.068
2012	.098	.094	**.087**	.079	.071	—
2013	.091	**.086**	.081	.086	—	**.205**
2014	**.086**	.084	.078	—	**.199**	.174
2015	.095	.094	.090	**.206**	.182	.153

e = −4 e = −3 e = −2 e = −1 e = 0 e = +1 delayed

We implement a difference-in-differences event study design using birth-month-by-cohort as units of treatment assignment. Treated months are those the KRA shifted out of kindergarten eligibility and into TK eligibility: September, October, and November. Treated months are paired with control months that were unaffected by the KRA across all study cohorts: September is paired with June, October with July, and November with August. Each pair is assigned event times relative to the kindergarten cohort in which the KRA first shifted age eligibility for the treated month. The control month is assigned placebo event times that mirror the treated month's. The November-August pair is assigned an event time of 0 in 2012-13, the first year in which November was treated. Our models include school district and kindergarten cohort fixed effects. The pooled specification is

$$Y_{idmb} = \alpha Treated_m + \sum_{e \neq -1} \mu_e \cdot 1\left[b - b_m^0 = e\right] + \sum_{e \neq -1} \tau_e \cdot 1\left[b - b_m^0 = e\right] \cdot Treated_m + X'_{idmb}\gamma + \delta_d + \lambda_b + \varepsilon_{idmb}$$

where i indexes child, d indexes school district, m indexes birth-month pair, b indexes kindergarten cohort. $Treated_m$ is an indicator equal to one for birth months that were ever affected by the KRA (September, October, and November). The quantity $b - b_m^0$ measures event time for pair b as the number of cohorts separating a given kindergarten cohort from the pair's anchor cohort b_m^0, which we define as the first cohort in which the birth-month pair's treated month shifted out of kindergarten eligibility. The coefficients of interest are the τ_e, which estimate treated-versus-control differences in outcomes at each event time e. The μ_e coefficients absorb the control month's trajectory. X_{idmb} is a vector of child-level controls including race/ethnicity, sex/gender, socioeconomic status, and non-English parental language; δ_d are district fixed effects that absorb time-invariant differences across school districts; and λ_b are kindergarten cohort fixed effects that absorb cohort-wide shocks. Outcomes include CSPP enrollment at age three, TK enrollment at age four, and math and reading achievement measured three and four years after kindergarten. Robust standard errors are clustered at the school district level.[19]

The identifying assumption of this design is that, absent the KRA, outcomes would have evolved in parallel across treated and control birth months. We assess this assumption by examining pre-KRA trends in both enrollment and achievement outcomes. Appendix Table B1 reports the full set of event study coefficients.

[19] The results were similar using robust standard errors clustered at the birth-month-by-cohort level.

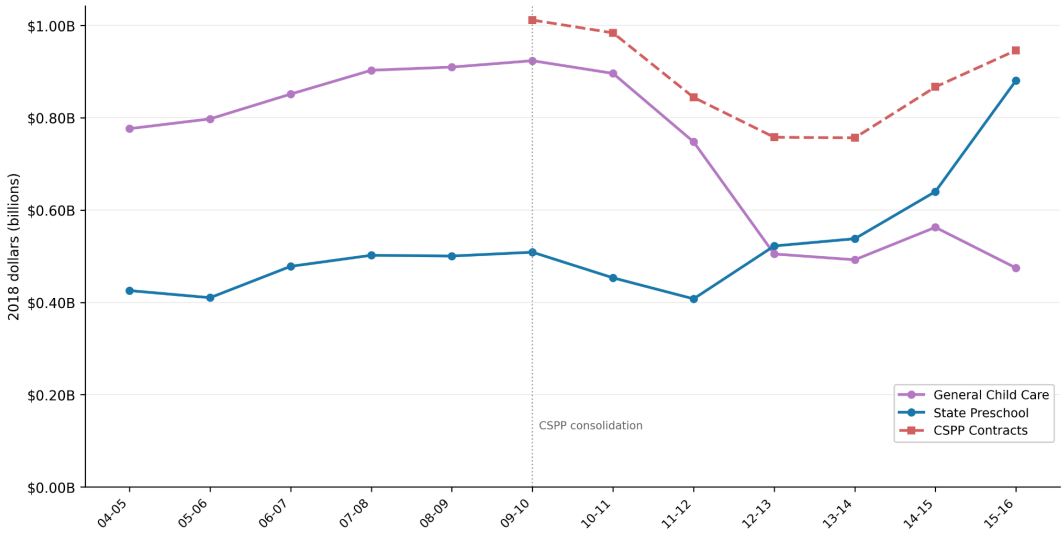
CSPP Spending

Spending shocks may affect CSPP access, preschool quality, or both. In the event of negative shocks, public preschool supply may contract along the extensive margin via enrollment reductions, whether through disenrollment or a decrease in new enrollments. Preschool supply may contract along the intensive margin via reductions in hours or days of operation. Negative funding shocks may also induce reductions in wages, staffing levels, or other investments in program quality that may, in turn, affect achievement.

To identify the independent effects of CSPP spending on student achievement, we exploit policy-induced changes in contractor-specific funding levels. The central intuition behind our empirical strategy is that policy-induced changes in CSPP funding translate into contract-specific MRA shocks, the timing and magnitude of which are plausibly exogenous to determinants of children's outcomes that operate through channels other than CSPP. Contract-specific shocks propagate to communities in which children live via historical enrollment patterns. We isolate plausibly exogenous variation in district-cohort CSPP spending using a shift-share instrument that interacts changes in contractor-level funding across 693 legacy CSPP contractors active in 2009 (shifts) with pre-recession contractor-district enrollment patterns (shares).

The CSPP contract drew on two program streams: Preschool Education and General Child Development. Preschool Education funded part-day CSPP enrollment exclusively. General Child Development funding supported both full-day and part-day CSPP enrollment, as well as other child care and development contracts such as General Child Care (CCTR). Figure 9 depicts changes in funding for the state budget items that funded CSPP contracts (and other programs) as well as the aggregate MRA across all contracts.

Figure 9: State Preschool Funding: Budget Items and the CSPP Contract

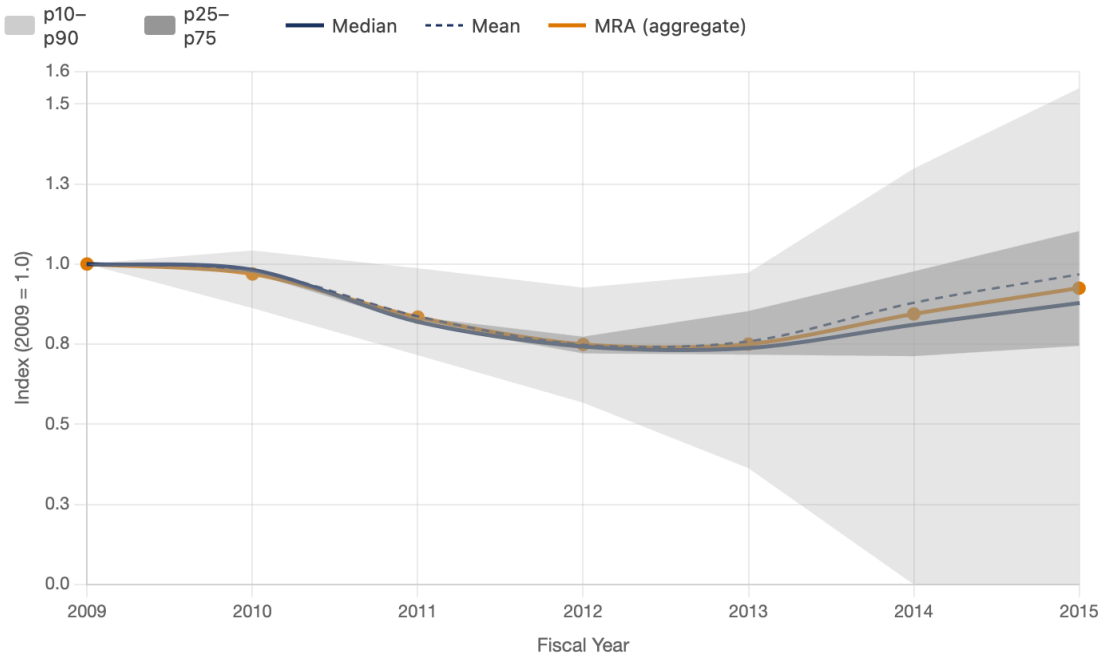


Each year, CDE and each contractor execute a contract that specifies a maximum reimbursable amount (the upper bound on what CDE will pay that contractor under that contract) along with a per-child-per-day reimbursement rate and requirements for days and hours of service. The contractor earns reimbursement against that ceiling by enrolling eligible children and documenting eligible costs. Contractor reimbursements equal the minimum of service earnings, documented costs, and the MRA.

An individual contract's MRA may change via any of four mechanisms: (1) legislative changes to per-child reimbursement rates and/or cost-of-living adjustments, (2) legislative changes in budget allocations for enrollment, (3) contractor-initiated reductions in service provision, or (4) CDE-initiated contract reductions for chronic under-enrollment, or other adjustments. During the study period, reimbursement rates and cost-of-living adjustments were frozen. Almost all contractors were fully enrolled and contractor performance was empirically unrelated to year-over-year MRA changes, inconsistent with CDE-initiated reductions for chronic under-enrollment. Absent legislative action, contract renewal can be characterized as an opt-out process; new contractor entry required either an explicit budget allocation for expansion funds or reallocation from a non-renewed contract. Contractor entry and exit was rare, accounting for less than two percent of total contracted funds from 2009-10 to 2014-15. Thus, year-to-year MRA variation during the study period reflects state budget actions transmitted to an otherwise stable set of contractors. Almost all contractors were fully enrolled and the

median contractor's service earnings as a share of MRA was 97 percent. State regulations of staff-child ratios and minimum hours and days of operation constrained contractors' ability to adjust on non-spending margins, limiting the scope for contractors to absorb funding shocks by reducing the structure of services. Figure 10 depicts the distribution of changes in CSPP contractor MRA over time.

Figure 10: Distribution of CSPP Contractor Funding Shocks, 2009–2015



Descriptively, contractors' enrollment activity is tightly correlated with contract MRA. At the agency-year level, each additional $1,000 in MRA is associated with 1.3 additional child-months of observed enrollment. Just over 70 percent of enrollment in a given year consisted of new program participants from 2009 to 2015, while students returning for a second year accounted for almost 30 percent. The number of child-days per child-month declined during the period, consistent with contractors reducing days of operation toward regulated minimums. From its peak enrollment at about 163,000 children in fiscal year 2010-11, enrollment declined by 13 percent to 142,000 in 2012. MRA totals declined from $969 million in 2009 to $726 million in 2012, or about 25 percent. Reported costs, our measure of spending, declined by 21 percent. Child-days and child-months of enrollment declined by 21 and 15 percent, respectively, from peak to trough.

Unlike K-12 schools, CSPP contractors do not operate within geographically defined attendance boundaries; even school districts with CSPP contracts enroll preschool-age children from outside their K-12 boundaries. This precludes the straightforward district-level treatment assignment available in K-12 finance studies. Geographic proximity nonetheless shapes enrollment: children attend preschools and elementary schools near their homes (Blumenberg et al., 2024; McGuckin, 2013), and the geographic overlap between preschool and elementary school attendance patterns allows us to approximate which CSPP contractors were accessible to each district's families. Most districts drew enrollees from multiple CSPP contractors, and most contractors served children who entered multiple kindergarten districts; our exposure measure aggregates across the contractor set serving each district.

We observe, at the child level, the contractor that provided a child's CSPP care as well as the district the child subsequently entered for kindergarten. Joining these records yields district-by-contractor *feeder patterns*: for each contractor, the distribution of its enrollees across the districts in which they later entered kindergarten. Feeder patterns identify the set of districts that lie within each contractor's effective catchment area and, equivalently, the set of contractors accessible to each district's families (those with a positive share of enrollment flowing to that district).

We measure district *d*'s exposure to contractor *c* as contractor *c*'s share of total child-months attributable to children who subsequently entered kindergarten in district *d*:

$$\omega_{dc} = \frac{CSPPmonths_{dc}}{CSPPmonths_{c}}$$

Exposure is fixed at its 2009 baseline to isolate historical feeder patterns from time-varying influences (local supply-and-demand conditions in child care, demographic change, contractor entry and exit) that might themselves influence child outcomes. Contractor entry and exit over the study period was minimal: 98 percent of CSPP funding flowed through legacy contractors holding CSPP contracts in 2009, and earlier child care and development contracts before that. The set of legacy CSPP contractors accessible to district *d*'s families are those with ω_{sc}^{2009} greater than zero.

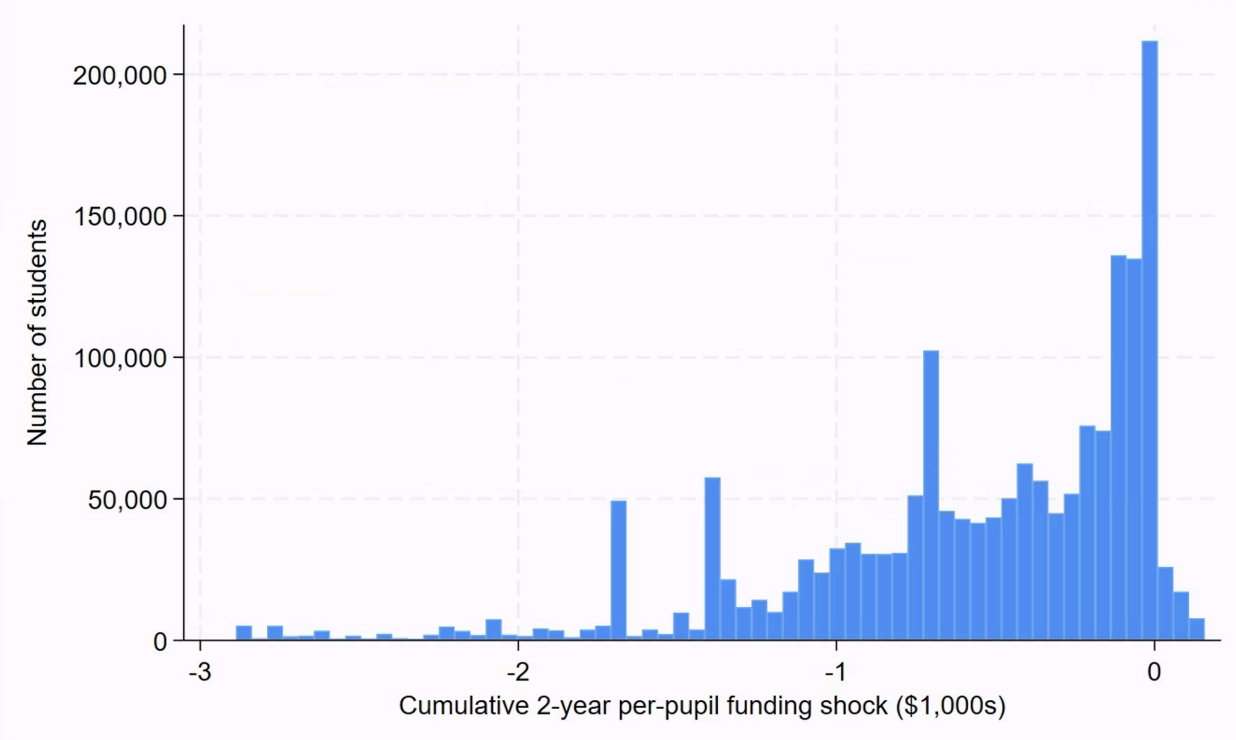
We measure district *d*'s total annual exposure to CSPP funding changes as the share-weighted sum of changes in each contractor's MRA:

$$\Delta MRA_{dt} = \sum_{c \in C} \omega_{dc}^{2009} \times \Delta MRA_{ct}^{2009},$$

where *t* indexes fiscal year, and $\Delta MRA_{ct}^{2009} = MRA_{ct} - MRA_{c}^{2009}$. To estimate the effects of district exposure to CSPP spending on student achievement, we express exposure over two years of a kindergarten birth cohort's eligibility for CSPP enrollment one and two years prior to expected kindergarten entry: $\Delta MRA_{db} = \Delta MRA_{b(t+1)} + \Delta MRA_{b(t+2)}$ where *b* indexes kindergarten birth cohort. For interpretation in per pupil terms, $\Delta MRApp_{db}$ is scaled so that a one-unit change corresponds to a $1,000 cumulative two-year shock to district-level aggregate CSPP funding capacity, per 2009 district-cohort pupil.

Figure 11 depicts the distribution of the instrument across students. The sample mean of −$564 per pupil (s.d. $566) reflects the mix of early cohorts, who experienced smaller shocks, and later cohorts, who experienced larger ones. Students in the 2013 and 2014 cohorts experienced average shocks exceeding −$900 per pupil, and students at the 10th percentile experienced shocks exceeding −$1,400. A $1,000 change in the instrument approximates the cumulative funding exposure of students who entered kindergarten during the depth of the contraction.

Figure 11: Distribution of Cumulative Two-Year Per-Pupil Funding Shocks



To interrogate its validity, we embed the instrumental variable in a two-way fixed effects framework that includes kindergarten birth cohort and school district fixed effects:

$$Y_{idb} = \beta MRApp_{db} + \gamma X_{idb} + \delta_d + \theta_b + \varepsilon_{idb}$$

where X_{idb} is a vector of child-level controls including race/ethnicity, sex/gender, socioeconomic status, and non-English parental language; δ_d are school district fixed effects; and θ_b are kindergarten birth cohort fixed effects.

District-level exposure to CSPP funding shocks, as measured by the shift-share instrument, has significant effects on CSPP enrollment, participation intensity, and funding flows, confirming first-stage relevance across multiple margins. A $1,000 per-pupil decline in cumulative MRA exposure reduced the probability of CSPP enrollment by 1.4 percentage points, reduced CSPP participation by 0.25 months per pupil, and reduced contractor spending by $840 per pupil. The apparently small effect on the

probability of enrollment is similar in magnitude to the observed changes in enrollment rates, which did not vary substantially at the state level amidst declining kindergarten enrollment. These and other findings are discussed in further detail in Appendix A.

Identification of the causal impact of changes in CSPP spending on child outcomes requires that ΔMRA_{db} be orthogonal to any unobserved determinants of achievement, conditional on observed district and cohort fixed effects:

$$E\left[\Delta MRApp_{db} \times \varepsilon_{isb} \mid X_{idb}, \delta_{d}, \theta_{b}\right] = 0.$$

The shift-share instrumental variables literature interrogates this central identifying assumption, which cannot be tested directly, from two distinct perspectives: quasi-randomness of shifts and quasi-randomness of shares. Our approach is best characterized by a framework developed by Borusyak, Hull, and Jaravel (2022) in which identification rests on the quasi-random assignment of shifts, or contractor-level funding shocks in this setting, which originate in statewide budget actions and are transmitted to a stable set of contractors. The shares enter as weights that allocate contractor-level funding changes to districts. Identifying variation comes from within-district timing and magnitude of funding shocks. All models include district fixed effects that preclude between-district comparisons, including differences in baseline exposure defined by historical enrollment patterns (shares).

We explore potential avenues through which the central identification assumption may be violated. The shifts (CSPP funding shocks, ΔMRA_{ct}^{2009}) may be correlated with community characteristics along many dimensions that independently predict children's outcomes over the study period. For example, contractor-level funding trends may trace recession recovery trajectories or contemporaneous public investments in the communities those contractors serve. Historical enrollment patterns represented by ω_{dc}^{2009} (the shares) encode the historical geography of CSPP contractor activity. CSPP enrollment was historically concentrated in low-income and otherwise disadvantaged communities. These communities were not static over the study period. Some were undergoing gentrification, others entrenched in persistent poverty, and either trajectory would generate within-district trends in outcomes that could be spuriously attributed to CSPP funding if

correlated with baseline exposure. Historical disadvantage encoded by shares may also correlate with contemporaneous trends in post-recession public investment in children and families during the study period.

We conduct a series of falsification tests on student, school, neighborhood (ZIP code), district, and contractor characteristics to assess the validity of the shift-share instrument. These tests are designed to detect the association of CSPP funding shocks (the shift-share instrument) with demographic and economic trends, as well as enrollment in other publicly funded ECE programs, that would threaten identification. Tests for associations with student characteristics include race/ethnicity, sex/gender, socioeconomic disadvantage, and English Learner classification. We find no evidence of compositional change: coefficients on demographic characteristics are uniformly small relative to their means, with the exception of a modest association with non-English parental language (0.009, against a mean of 0.376). Tests for associations with district demographic and economic conditions include annual birth counts, population size by age segment, housing values, and other measures of socioeconomic conditions. We find no association between CSPP funding shocks and birth rates, population growth, housing values, or an index measure of district socioeconomic status. Among economic indicators, we find small but statistically significant associations with SNAP receipt and unemployment. Further, year-over-year contract shocks are not associated with contractor performance as measured by enrollment as a share of funding, which is inconsistent with CDE-initiated contract reductions selectively reducing contracts, perhaps in response to perceived decline in demand or need for CSPP. Results from falsification tests are presented in Appendix Table A3.

Transitional Kindergarten

Public preschool investments and school policies that expand access to high-quality schooling often do not bear fruit immediately. Prior evidence has shown these investments pay off over years, through the course of people's school careers and beyond. On the other hand, benefits from high-quality pre-k access that manifest early on may fade out in subsequent years if high-quality learning environments are not sustained during the elementary school years. Furthermore, due to the cumulative nature of learning, early learning often begets and augments future learning capacities

(Heckman, 2007). Therefore, measuring success requires (a) analyses that use student-level longitudinal data that follow children and their learning outcomes from preschool and TK through their K-12 years; and (b) a research design that accounts for parental family background factors and can disentangle the relative roles of school resources at different child developmental stages (and potentially interactive roles—e.g., dynamic complementarity in early and later public education investments). A contribution of the study design is its ability to achieve both aims.

Because roughly 40 percent of California students speak a language other than English at home, and the dynamics of student achievement are known to differ across parental language backgrounds, all models include interaction terms of the school district fixed effects and kindergarten cohort fixed effects with an indicator for non-English parental language, and the models allow the main estimated effects of TK attendance to differ for students whose parents are English speakers and for those whose parents are non-English speakers. This analysis of heterogeneity proves critically important, as not only do the take-up rates of TK differ by both parental SES and language, but the impacts of TK on achievement also differ substantially along these dimensions.

A significant component underlying what appear as heterogeneous treatment effects (i.e., differences in the estimated causal impacts of TK attendance) is in fact substantial heterogeneity in the counterfactual care conditions children would have received absent the TK program. These counterfactual settings differ in the quality and affordability of local options (including home care), language modality (bilingual immersion), program funding and resources, and a host of other factors.

To address potential bias, the research design accounts for the following:

a) pre-existing (baseline) school-specific persistent differences in the level of achievement;

b) pre-existing (baseline) school-specific persistent differences in the average growth rate of achievement;

c) common statewide trends;

d) years since kindergarten (“grade”).

Building on Johnson (2023), the research design utilized to estimate causal impacts of LCFF-induced increases in per-pupil spending (experienced over multiple years) on student achievement employs a difference-in-differences (DiD) approach that facilitates comparisons of students from the same school and same kindergarten cohort across successive grades (exploits differences in the duration of exposure and intensity of reform-induced spending increases). Taken together, the RD comparisons for TK based on age-eligibility cutoffs combined with the DiD comparisons for school spending (i.e., "apples-to-apples" comparisons) ensure valid causal inferences can be gleaned from the study results.

While it is common practice in empirical research to aggregate data, group all students, and present the average treatment effects, this convention masks enormous differences in the effects of TK attendance due to substantial differences at the starting gate by parental SES and between students whose parents are English-speakers and those whose parents are not, as well as differences in the quality of counterfactual care conditions (in the absence of TK).[20] Moreover, there are several factors that are often considered exogenous and independent of TK, but are in fact endogenous outcomes that are directly influenced by the introduction of TK, including grade progression, the age and timing of academic assessments, including initial English Learner (EL) and English proficiency designations, and the subsequent rate of EL reclassifications. In light of this, and the fact that the starting gate and dynamics of learning vary significantly along these parental socioeconomic dimensions, we present results separately by parental SES and language, and include estimated impacts on math and reading achievement 3 and 4 years after kindergarten, and analyses of impacts on grade repetition. Furthermore, the study tests for differences in TK impacts between children who attended well-resourced elementary schools vs. those who attended less well-funded schools.

Methods. To identify the independent effects of TK, our research design exploits TK's age eligibility cutoffs within a regression discontinuity (RD) design 2SLS-IV approach to isolate the causal impacts of TK attendance on student achievement trajectories. Students from all public elementary schools in California are included in the analyses with interaction terms for Los Angeles Unified School District (LAUSD), which was an early adopter of 'Early TK' that would otherwise result in significant

[20] For example, an insignificant average treatment effect may be masking beneficial effects that are experienced for some sociodemographic groups and negative effects for others.

non-comparability issues.[21] Interaction terms for Basic Aid districts are also included as they were not eligible for state LCFF funding. During the analysis period, the age eligibility for TK across all districts analyzed (except LAUSD) required that children turn five on or before December 2. Taking advantage of this strict birthday cutoff for TK eligibility, we use an RD design to identify the causal impacts of TK. We focus on analyses of the second TK-eligible cohort of students who entered kindergarten in the 2014–2015 school year and can be followed up to four years after kindergarten—among this "intent to treat" group, students must have turned five between October 2 and December 2 of 2013 to be eligible to attend TK during the 2013–2014 school year. The control group (for TK) includes public school students whose fifth birthdays occurred between December 3 and January 31 who were observed in the 2013-2015 kindergarten cohort and have been followed in the years since. To facilitate valid causal inferences, the empirical approach and analyses are designed to ensure that the primary comparisons are among students from both the same kindergarten school and cohort who differed in age by only a few weeks but differed in TK access (in the year immediately preceding kindergarten entry) due to the strict birthday TK eligibility cutoff. The models include flexible controls for the number of days (on either side of) a student's (fifth) birthday relative to the December 2 eligibility cutoff and include controls for gender, race/ethnicity, parental SES, the number of years since kindergarten, and the number of years since kindergarten interacted with parental SES to account for the preexisting parental SES gradient in achievement growth. The primary analytic sample includes 662,111 student-year achievement observations of 340,310 students across 5,700 elementary schools and 806 school districts.

***Empirical Approach & Model Specification*.** As with other public pre-k programs across the country, California's TK program is voluntary for eligible children. This means that children enrolled in TK may differ from those whose parents choose other care setting options. An analysis that does not take into account the endogeneity of TK enrollment and the difference in children's age by design is likely to lead to biased results. The regression discontinuity design described above addresses this endogeneity by exploiting the strict birthday cutoff for TK eligibility.

[21] The main results are robust to excluding LAUSD.

Let Y_{itsc} represent the student-level end-of-year outcome *t* years after kindergarten of student *i* from school *s* and kindergarten cohort *c*, including math and reading achievement, where *s* indexes kindergarten school attended, *c* indexes kindergarten cohort, *t* indexes years since kindergarten, *i* indexes student. All models include dummy indicator variables for child family socioeconomic disadvantage (SES_i), child gender and race/ethnicity ($demog_i$), and years since kindergarten fixed effects (η_t), years since kindergarten fixed effects interacted with child family SES ($\eta_t * SES_i$), and a complete set of school-by-kindergarten cohort fixed effects (θ_{sc}). All models are estimated separately for students whose parents are English speakers and for those whose parents are non-English speakers.

One of the endogenous treatment measures, $ppe^{3yravg}_{d(s)ct}$, is average per-pupil spending during the three immediately preceding years (*t*-2 through *t*) in district *d* and kindergarten cohort *c*. (e.g., for 4th grade achievement, this corresponds with spending in grades 2-4 (ages 7-9)). Average district per-pupil spending during school years *t*-2 through *t* is inflation-adjusted using the CPI-U deflator (in real 2015 dollars) and then expressed in thousands (and centered around 12). This is done to facilitate interpretation of marginal effects, so that the estimated effects are in the range we observe LCFF-induced variation in per-pupil spending, and so that the average TK treatment effects are evaluated at the average school spending (~$12,000) experienced for exposed cohorts.

The simulated instrumental variables for school funding reform (based on the funding formula, pre- & post-LCFF) are represented by $Sim\hat{\Delta}LCFF_{d(s)ct(t-1)(t-2)(t-3)}$: LCFF-induced increase in funding from state (relative to pre-LCFF) for district *d*, kindergarten cohort *c*, at *t*, *t*-1, *t*-2, *t*-3 years since kindergarten (holding district socioeconomic disadvantage, property values, fixed at 2013 levels). The inclusion of school-by-kindergarten cohort fixed effects ensures that sources of identifying variation rely on comparisons of students from the same school and same kindergarten cohort across successive years after kindergarten, which exploits differences in the duration of school-age years of exposure and the intensity of dosage of reform-induced spending changes. The two-stage least squares instrumental variables (2SLS-IV) models isolate exogenous variation in LCFF-induced increases in per-pupil spending (experienced over multiple years) on student achievement in elementary school. The student outcomes include math and reading standardized achievement (NAEP-normed adjusted in grade-level equivalent

units), three and four years after kindergarten. The analysis is restricted to students observed in kindergarten or earlier, and followed thereafter.

Formally, using the student-level longitudinal data, we estimate the following system of equations by 2SLS (where the first three equations comprise the 1st stage, and the fourth equation below represents the 2nd stage):

(1) 1st stage (Prob(TK participation)):

$$\hat{TKattend}_{isc} = \alpha_{1,1}\left(TKelig_{isc'} * SES_i\right) + f\left(Birthdate_i\right) * SES_i + \pi_{1,1}\left(\hat{SimLCFF}_{d(s)ct'} * SES_i\right) + \pi_{2,1}\left(\hat{SimLCFF}_{d(s)ct'-1} * SES_i\right)$$

$$+ \pi_{3,1}\left(\hat{SimLCFF}_{d(s)ct'-2} * SES_i\right) + \pi_{4,1}\left(\hat{SimLCFF}_{d(s)ct'-3} * SES_i\right) + \pi_{5,1}\left(\hat{SimLCFF}_{d(s)ct'(t'-1)(t'-2)(t'-3)} * TKelig_{isc'} * SES_i\right)$$

$$+ \gamma_{1,1}SES_i + \gamma_{2,1}Demog_i + (\eta_{t,1} * SES_i) + \theta_{sc',1}$$

where $Birthdate_i$ represents the student's birthdate centered at the TK age eligibility cutoff (December 2nd), and $f\left(Birthdate_i\right)$ is a flexible non-parametric (semi-parametric, polynomial function) specification for the birthdate running variable centered around the TK eligibility cutoff; $TKelig_{isc}$ is a dummy indicator for whether the student is TK eligible. Let c' represent kindergarten cohort year based upon child's birth date and kindergarten state eligibility law that applied for that particular school year when the child turned 5 (which is used in constructing exogenous instruments; and thus t' = *year* – c'); while *c* represents the kindergarten cohort year child was observed actually entering kindergarten (which is used in the instrumented key measures; and thus *t* = *year* – *c*). Because the timing parents choose to have their child first enter kindergarten is partly endogenous and potentially an outcome of grade progression influenced by the TK treatment, it is important to use *c* in the construction of the instruments to ensure exogeneity, while accounting for this via 2SLS to address non-compliance issues. For example, it is found that more affluent parents are often more likely to "redshirt" their children on the margin. The indicator for child family socioeconomic disadvantage is fully interacted with all the TK and LCFF instrumental variables throughout (the models include the main effects but some of the terms are suppressed in the equations for notational ease), to enable tests of differential treatment effects for non-poor children vs children from low-income families.

(2) 1st stage (per-pupil spending):

$$\hat{ppe}^{3yravg}_{d(s)ct} = \alpha_{1,2}\left(TKelig_{isc}' * SES_i\right) + f\left(Birthdate_i\right) * SES_i + \pi_{1,2}\left(Sim\hat{\Delta}LCFF_{d(s)ct}' * SES_i\right)$$

$$+ \pi_{2,2}\left(Sim\hat{\Delta}LCFF_{d(s)ct-1}' * SES_i\right) + \pi_{3,2}\left(Sim\hat{\Delta}LCFF_{d(s)ct-2}' * SES_i\right) + \pi_{4,2}\left(Sim\hat{\Delta}LCFF_{d(s)ct-3}' * SES_i\right)$$

$$+ \pi_{5,2}\left(Sim\hat{\Delta}LCFF_{d(s)ct(t-1)(t-2)(t-3)}' * TKelig_{isc}' * SES_i\right) + \gamma_{1,2} \bullet SES_i + \gamma_{2,2}Demog_i + (\eta_{t,2} * SES_i) + \theta_{sc,2}'$$

(3) 1st stage (TK*spending):

$$\hat{TKattend}_{isc} * \hat{ppe}^{3yravg}_{d(s)ct} = \alpha_{1,3}\left(TKelig_{isc}' * SES_i\right) + f\left(Birthdate_i\right) * SES_i + \pi_{1,3}\left(Sim\hat{\Delta}LCFF_{d(s)ct}' * SES_i\right)$$

$$+ \pi_{2,3}\left(Sim\hat{\Delta}LCFF_{d(s)ct-1}' * SES_i\right) + \pi_{3,3}\left(Sim\hat{\Delta}LCFF_{d(s)ct-2}' * SES_i\right) + \pi_{4,3}\left(Sim\hat{\Delta}LCFF_{d(s)ct-3}' * SES_i\right)$$

$$+ \pi_{5,3}\left(Sim\hat{\Delta}LCFF_{d(s)ct(t-1)(t-2)(t-3)}' * TKelig_{isc}' * SES_i\right) + \gamma_{1,3}SES_i + \gamma_{2,3}Demog_i + (\eta_{t,3} * SES_i) + \theta_{sc,3}'$$

(4) 2nd stage (TOT):

$$Y_{itsc} = \beta_{1t}(\hat{TKattend}_{isc} * SES_i) + \beta_2\left(\hat{ppe}^{3yravg}_{dsct} * SES_i\right) + \beta_3(\hat{TKattend}_{isc} * \hat{ppe}^{3yravg}_{dsct} * SES_i)$$

$$+ f\left(Birthdate_i\right) * SES_i + \gamma_{1,4} \bullet SES_i + \gamma_{2,4}Demog_i + (\eta_{t,4} * SES_i) + \theta_{sc,4}' + \varepsilon_{itsc}$$

(4') 2nd stage (Reduced-Form "ITT" for TK eligibility):

$$Y_{itsc} = \beta_{1t,4}'(TKelig_{isc} * SES_i) + \beta_{2,4}'\left(\hat{ppe}^{3yravg}_{dsct} * SES_i\right) + \beta_{3,4}'(TKelig_{isc} * \hat{ppe}^{3yravg}_{dsct} * SES_i)$$

$$+ f\left(Birthdate_i\right) * SES_i + \gamma_{1,4}' \bullet SES_i + \gamma_{2,4}'Demog_i + (\eta_{t,4}' * SES_i) + \theta_{sc,4}' + \varepsilon_{itsc}$$

where ε_{itsc} is a stochastic error term. Standard errors are robust to heteroscedasticity and clustered at the district level. The effects of TK attendance are allowed to differ by years since kindergarten, while the effects of per-pupil spending are constrained to be equal across grades.[22]

The purpose of the 2SLS-RD models in (4') is to compare students who are eligible for TK with

[22] Johnson (2023) showed that the impacts of LCFF-induced changes in per-pupil spending on achievement do not differ significantly for third and fourth grades.

those who are not eligible for TK to estimate effects of offering the program—i.e., the intent-to-treat (ITT) effect. Ignoring noncompliance attenuates the estimated impact of TK attendance, because some of the control students might have attended TK and some parents of treatment eligible students might have chosen not to enroll their child in TK. Therefore, the 2SLS-IV-RD models in (4) aim to estimate the causal impacts of TK attendance (TOT), and in each, examine how the estimated TK effects differ by parental socioeconomic status, parental language, and school resources during subsequent elementary school years (induced by LCFF). The internal validity of the estimated TK impact in the RD design relies on the assumption that, in the absence of the TK program, there would be a smooth relationship (i.e., no discontinuity) between the student achievement outcome and the birthdate running variable. For this reason, any discontinuity observed in the student outcomes at the TK eligibility cutoff is attributable to TK. Therefore, to check the smoothness assumption, we test for a discontinuity at the cutoff in the birthdate running variable and student and family background characteristics, such as parental socioeconomic status, parental home language, race/ethnicity, among others. Visual inspection of the figures does not reveal any jump around the cutoff for any of these other control variables.Provided that the conditional mean function $E\left[Birthdate_i\right]$ is continuous at the TK eligibility cutoff, the causal impact of TK attendance on a student outcome is given by

$$\beta_1 = E\left[Birthdate_i\right] \quad - E\left[Birthdate_i\right]$$

The excluded instruments for per-pupil spending are the pre-post LCFF-induced change in a district's per-pupil funding in each year at ages *t*, *t*-1, *t*-2, *t*-3. The empirical strategy combines regression discontinuity and difference-in-differences designs to isolate the interactive effects of TK participation and LCFF-induced changes in per-child funding, which mitigate potential sources of bias.

Robustness checks. In the main analyses, we use a flexible non-parametric (semi-parametric) specification for the running variable with a 60-day bandwidth around the age eligibility cutoff, and present the figures showing the main findings for TK attendance and the impact estimates of both TK eligibility ITT (reduced-form), and TK attendance (TOT), on student achievement up to 4 years after kindergarten, respectively. We also check whether the results are robust to alternative bandwidths: 15 days, 30 days, 90 days, 120 days, outcome-specific IK optimal bandwidth (Imbens and Kalyanaraman, 2012) and CCT optimal bandwidth (Calonico et al., 2014). We also test whether the main results are

robust to different model specifications and conduct a series of sensitivity analyses that test alternative model specifications, including different functional forms for the birthdate running variable (relative to the age eligibility cutoff). Birthdate relative to the cutoff measured in days is the forcing variable that defines TK program eligibility. We choose 60 days on either side of the eligibility cutoff as the optimal bandwidth, which represents students born up to two months before the cutoff and students born up to two months after the cutoff. A formal statistical test for optimal bandwidth (cross-validation method proposed by Ludwig and Miller (2007)), supports this choice of bandwidth. The main results are not very sensitive to the bandwidth selection.

Results

The Kindergarten Readiness Act

Figure 12 depicts event study estimates of KRA effects on TK attendance and enrollment in CSPP two years before kindergarten. CSPP enrollment rates two years before kindergarten were roughly 12 to 13 percentage points, or 2.5 times, higher among students whose kindergarten eligibility timing was altered by the KRA than among children born in identical months in earlier cohorts.

Figure 12. KRA effects on enrollment in TK at age 4 and CSPP at age 3

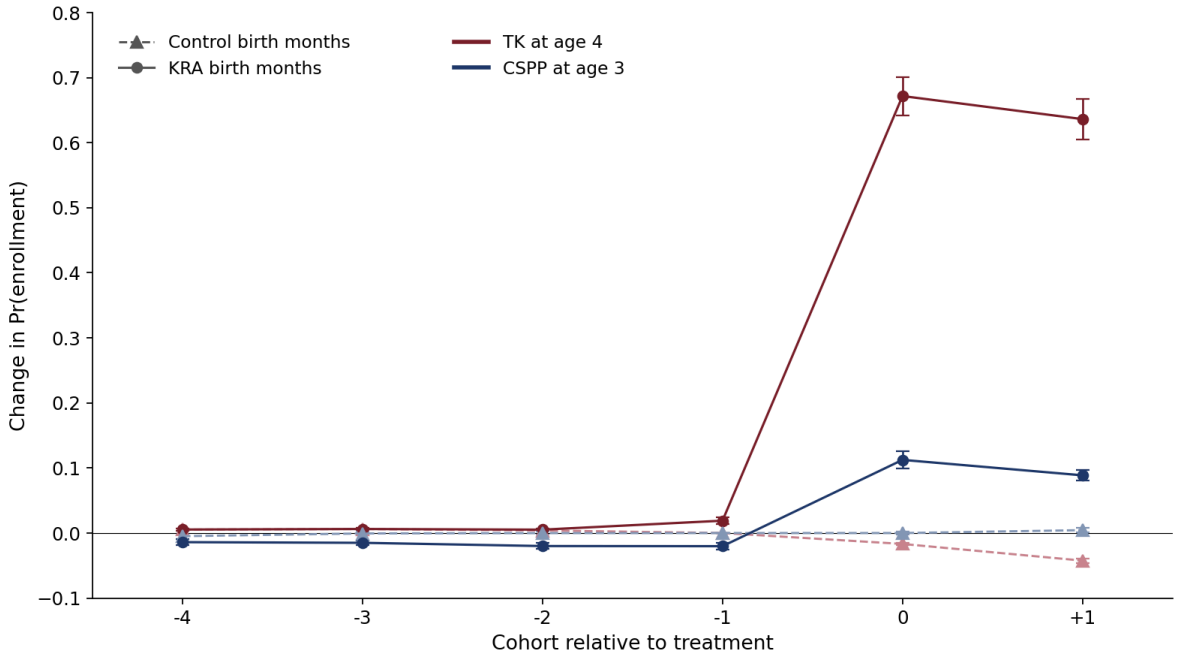
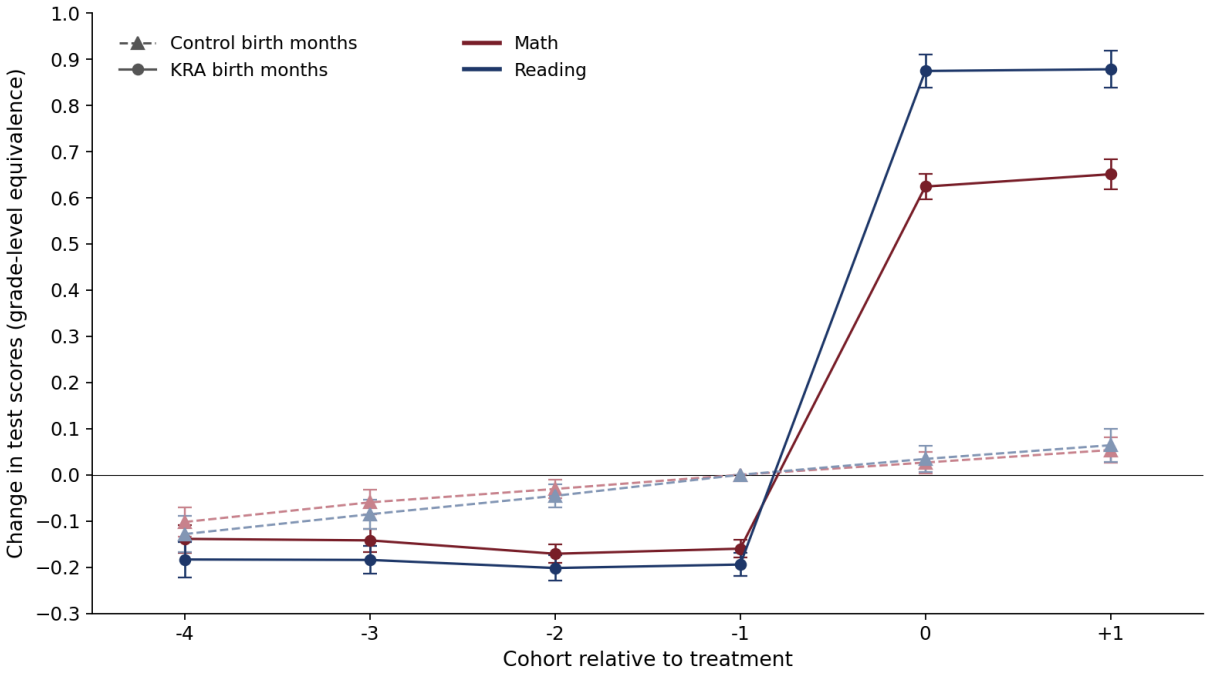


Figure 13 presents event study estimates of KRA effects on math and reading achievement three and four years after kindergarten. Students who turned five in November of 2012-13, October of 2013-14, or September of 2014-15 exhibited reading achievement nearly a full grade level above their peers born in identical months who turned five in earlier years, and math achievement approximately two-thirds of a grade level above the same comparison group. Both figures reveal parallel pre-KRA trends in enrollment and achievement between children born in treated and control months.

Figure 13. KRA effects on math and reading achievement, grades 3 & 4



The event study results yield three key insights. First, the KRA generated an 11 to 13 percentage-point increase in CSPP enrollment at age three with flat pre-trends, reflecting a new preschool sequence in which CSPP at around age three and a half preceded a year of TK. Second, the design identifies TK enrollment effects in the specific subpopulation for whom the KRA created access. Third, the achievement effects reflect the combined CSPP-at-age-three and TK-at-age-four sequence delivered by the policy change. Importantly, these estimates capture the full effect of delaying kindergarten entry by one year, not only the effects of TK and CSPP attendance. Children affected by the KRA also gained an additional year of developmental time before kindergarten, during which parental investments, informal learning experiences, and other developmental inputs may have increased. The event study therefore estimates the combined effects of the entire policy change. Isolating the independent effect of TK attendance from this broader package requires the within-cohort

regression discontinuity design, and understanding how antecedent CSPP participation conditions the effects of TK is a central focus of the analyses that follow.

Grade Repetition and Grade Progression

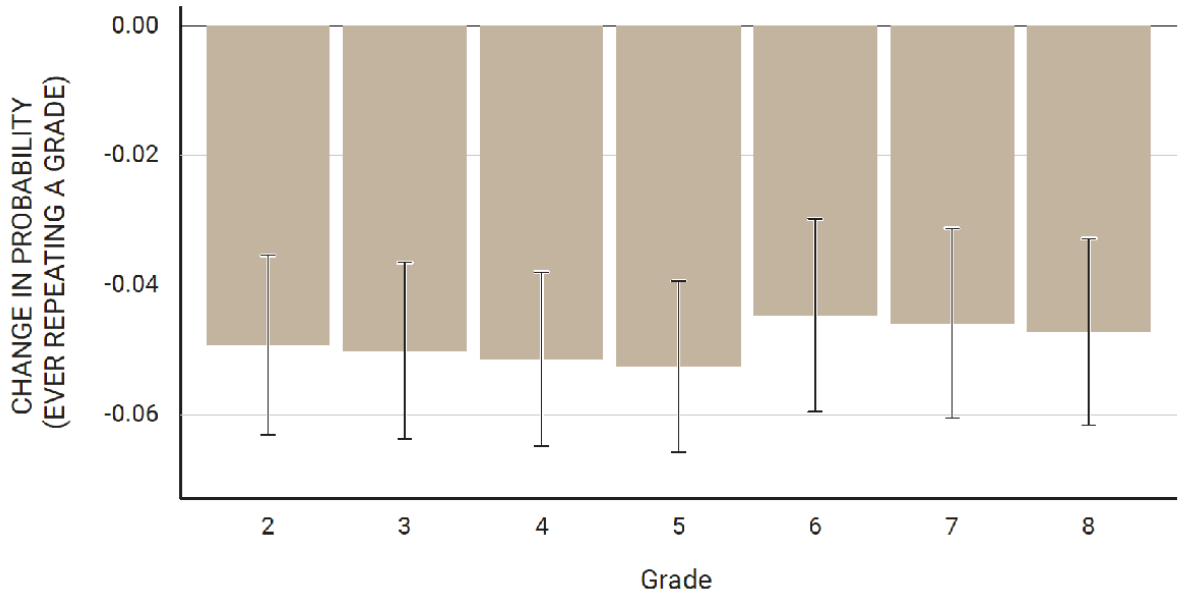
Grade progression and grade repetition are potential outcomes influenced by the quality of learning conditions in schools, and a student's observed grade may be endogenous to the school funding reforms enacted. We therefore measure achievement outcomes as a function of years since kindergarten, defined exogenously based on the student's birth date and the prevailing state laws governing age eligibility for TK and kindergarten in the year the child turned five, rather than treating the child's observed grade as exogenous.[23]

We investigated the effects of school funding on the likelihood of a student ever experiencing grade repetition since kindergarten (grades 2-8). As shown in Figure 14, the 2SLS-IV results indicate that LCFF-induced increases in school spending led to significant reductions in the probability of grade repetition, particularly during elementary school. A $1,000 increase in per-pupil spending experienced for three consecutive years resulted in a 5 percentage-point reduction in the probability of grade repetition by 3rd grade, a 5.1 percentage-point reduction by 4th grade, and a 5.3 percentage-point reduction by the end of elementary school (5th grade). These grade progression effects were enhanced by the coincident introduction of TK over this period. This finding would not have been uncovered without access to the student-level longitudinal data following the same children since kindergarten as they progress through California public schools, and is missed using aggregate school-level data. The

[23] For example, it has been widely documented in prior work for states and time periods that do not have universal pre-k programs, that more socioeconomically advantaged parents tend to “redshirt” their children (i.e., delay their kindergarten start year) if they turn 5 late in the year, while low-income parents tend to send their kids to school earlier (when they have birthdays on the margin) because they often cannot afford the additional childcare costs that would be incurred.

endogeneity of grade progression to the reforms studied here may contribute to the reason the estimated interactive pre-k and elementary school spending effects documented in this report are larger than prior estimates in the literature.

Figure 14. Estimated impacts of a $1,000 increase in per-pupil spending (for 3 years) on the likelihood of repeating a grade

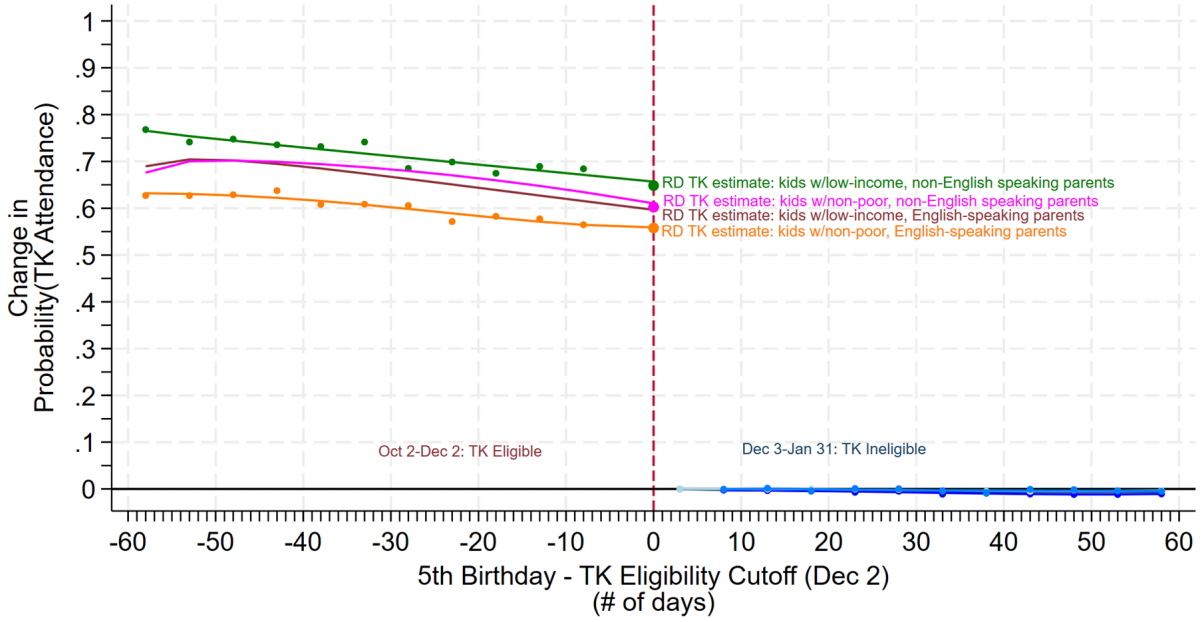


Sources: Author analysis of data from the California Department of Education for 2014–2018 in the Standardized Account Code Structure and California Longitudinal Pupil Achievement Data System.

Key Findings from the Unified Framework

***TK Attendance*.** The results indicate that the take-up rates of Transitional Kindergarten among newly eligible children was about 60 percent overall (based on the RD estimate), but we find significant differences by parental SES and language. In particular, children from low-income families had higher TK attendance than those from non-poor families (about a 5 percentage-point difference), and children who had non-English speaking parents had higher TK enrollment rates than those who had English-speaking parents (roughly 5 percentage-point difference as well).

Figure 15. Effect of TK Eligibility on the Likelihood of Attending Transitional Kindergarten, by Parental SES & Language



***Student Achievement*.** The 2SLS-IV models reveal several important findings. First, the results indicate that a $1,000 increase in CSPP spending per-pupil over two years (ages 3 and 4) leads to almost a quarter of a year of learning gains in math and reading achievement 3 and 4 years after kindergarten on average for children, wherein the 0.23 point estimate is statistically significant at the 0.01 level, independent of the effects of TK attendance and elementary school spending. This amount of CSPP spending change is within the range experienced over the analysis period and should be viewed as a relatively large funding-induced change in light of the fact that CSPP spending is targeted to children from lower-income families (not all three- and four-year olds) and we are measuring CSPP per-pupil spending (using that specific cohort's kindergarten cohort enrollment to put the CSPP spending measure in per-pupil terms, instead of using CSPP spending per program participant). Figure 16 presents impacts of CSPP spending, TK attendance (among socioeconomically disadvantaged students), and LCFF-induced school spending increases on student achievement in third and fourth grade.

Figure 16a. Impacts of CSPP spending, TK attendance, & school Spending on third grade achievement

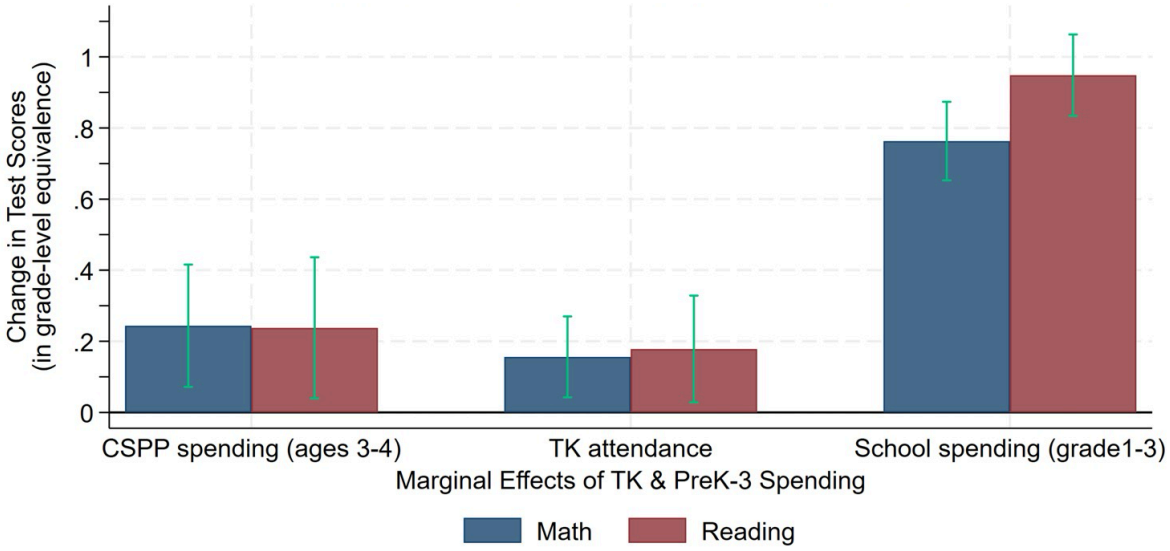
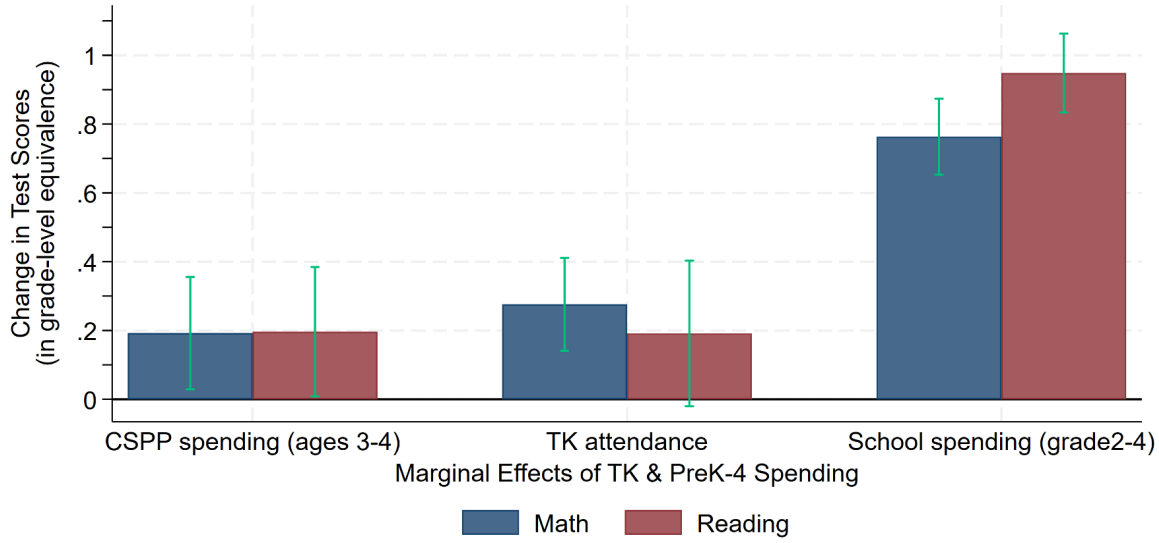


Figure 16b. Impacts of CSPP spending, TK attendance, & school Spending on fourth grade achievement



Second, the 2SLS-IV results indicate that a $1,000 increase in per-pupil spending for three consecutive years of elementary school results in between a 0.8 and a full grade-level improvement in reading and math achievement in 3rd and 4th grades, independent of CSPP spending and TK attendance (these results are in line with previous findings from Johnson (2023)).

Third, the 2SLS-IV results reveal significant positive effects of TK attendance on both math and reading achievement 3 and 4 years after kindergarten, with significant heterogeneity by parental SES and whether or not parents speak English. In particular, the beneficial effects of TK attendance are particularly pronounced for children from low-income families whose parents speak English. We find for those children TK attendance leads to about 6 months more learning gains in 3rd grade reading and math achievement on average (0.63 increase in grade-level equivalence for math; 0.44 increase in grade-level equivalence in reading), relative to those students from same school in same kindergarten cohort but did not attend TK.

Figure 17a. Impacts of TK attendance on third grade achievement by parental SES and language

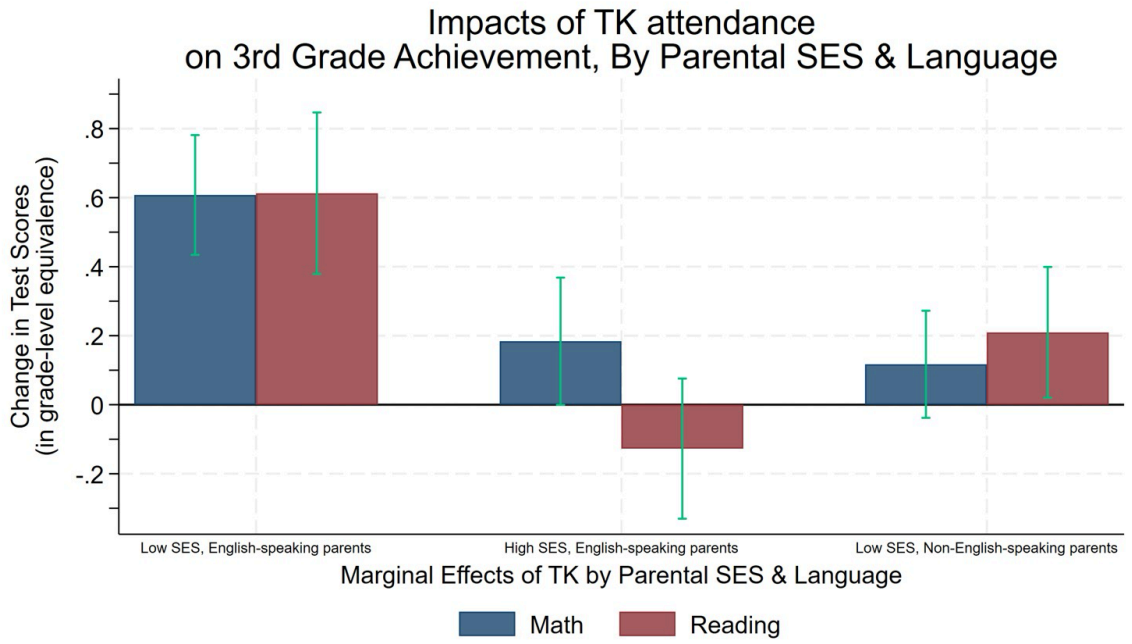
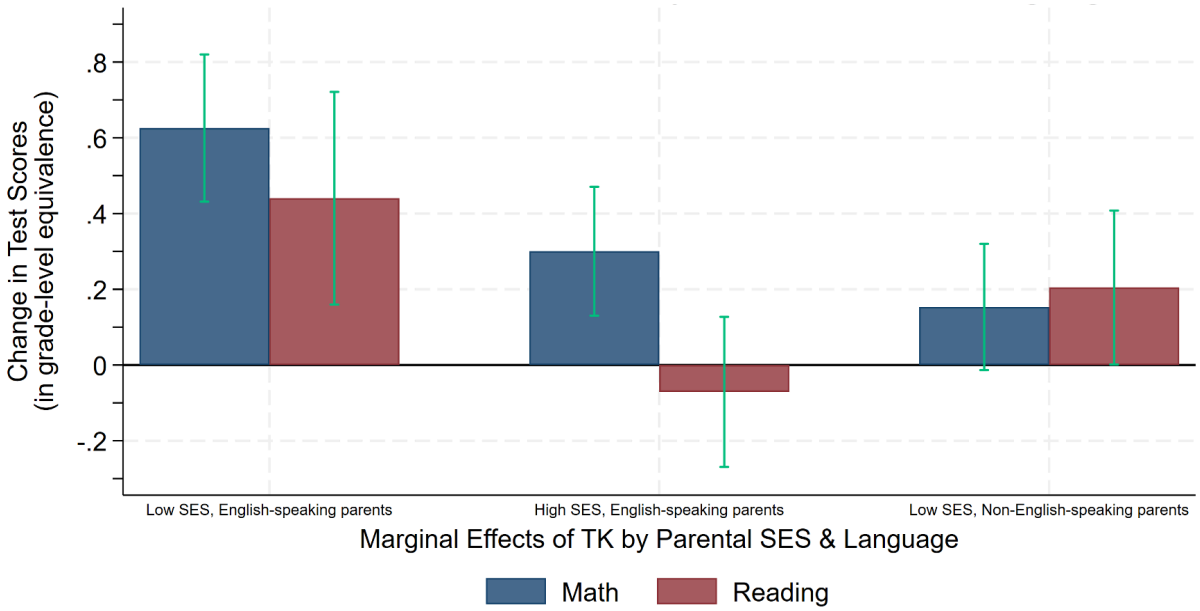


Figure 17b. Impacts of TK attendance on fourth grade achievement by parental SES and language



We highlight two key results that emerged for students who have English-speaking parents. The impacts of TK attendance were significantly greater for children from low-income families, relative to those from non-poor families. For example, among children from non-poor families whose parents speak English, we see negligible changes in reading achievement and a 0.3 improvement in math achievement. This pattern was expected and is likely due to the fact that non-poor families are much more able to afford and access high-quality private pre-k environments for their children in the absence of TK, while children from more socioeconomically disadvantaged families are far less likely to have access to high-quality early learning environments when it is not publicly provided.

Figure 18a. Impact of TK eligibility on math achievement 3 & 4 years after K among low-income students, by school spending

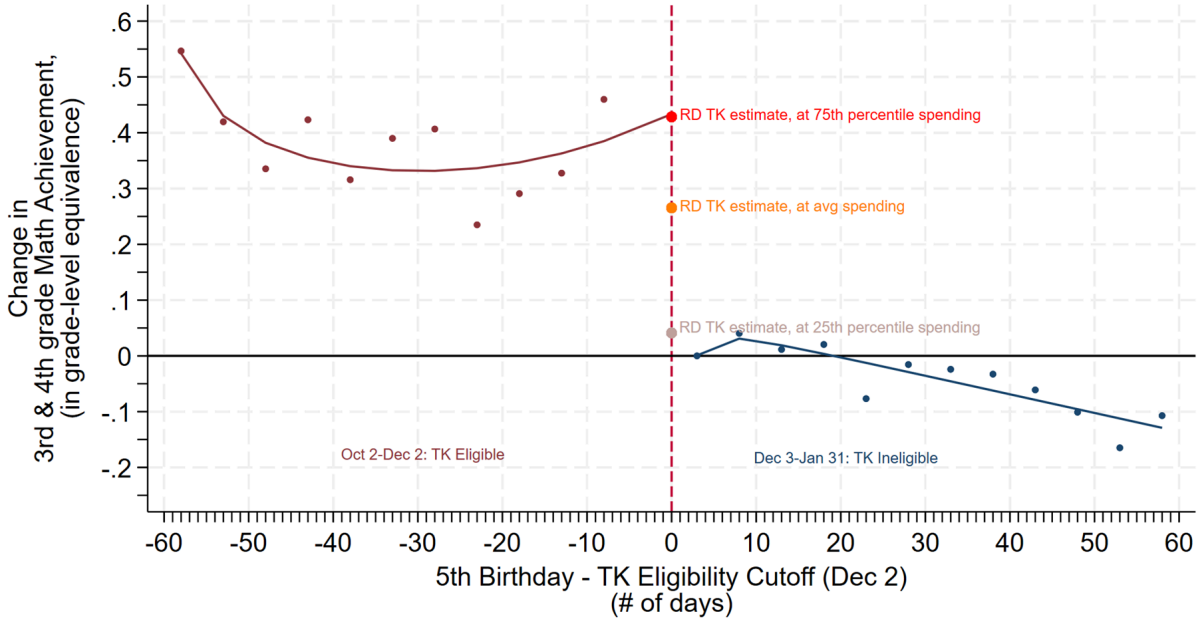
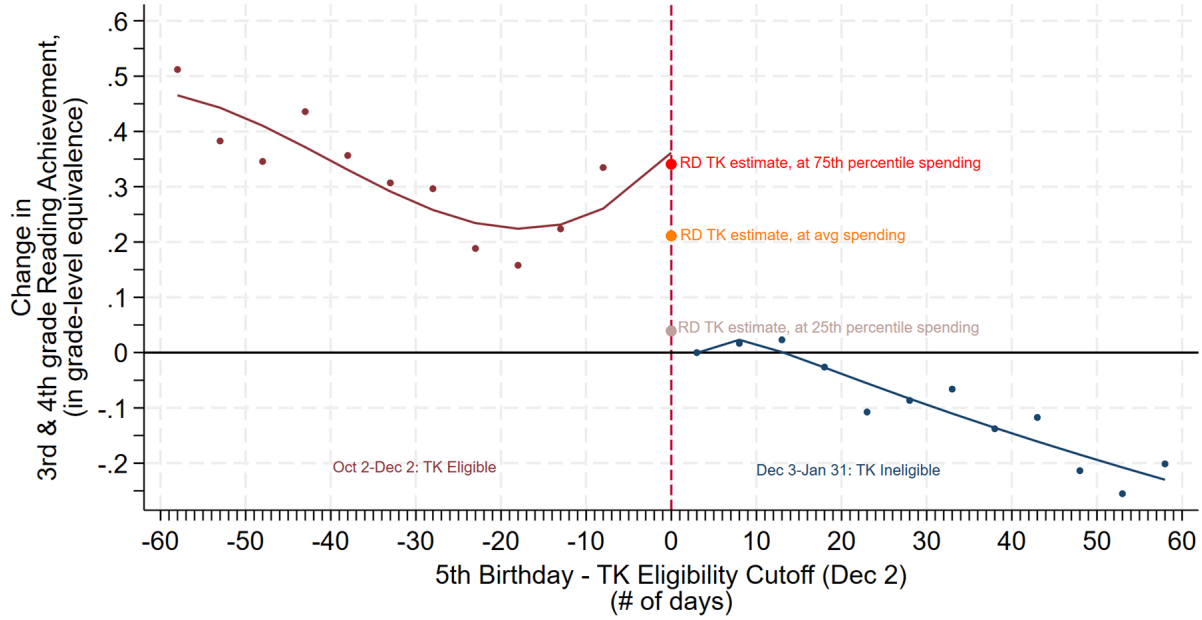


Figure 18b. Impact of TK eligibility on reading achievement 3 & 4 years afer K among low-income students, by school spending



Second, TK attendance and elementary school spending exhibit strong positive synergistic effects on both math and reading achievement. TK-induced improvements in achievement in 3rd and 4th grades were especially pronounced for low-income children who attended well-resourced elementary schools in their K-4 years.

Among children from low-income families who attended elementary schools with average per-pupil spending ($12,000), TK attendance led to half a grade-level improvement in reading and math achievement three years after kindergarten (i.e., 6 months of learning gains more than the typical student from the same school in the same kindergarten cohort experienced who did not attend TK). These effect sizes are remarkable, and largely persisted four years after kindergarten.

One of the most striking patterns is that among children from low-income families who attended elementary schools with per-pupil spending at the 75th percentile (i.e., $13,000, well-resourced schools), TK attendance led to a 0.8 grade-level improvement in reading and math achievement three years after kindergarten (equivalent to nearly 10 months of learning gains more than the typical student from the same school in the same kindergarten cohort experienced who did not attend TK). Equally noteworthy, the results indicate the effect sizes of these TK impacts persisted for math achievement 4 years after kindergarten and led to more than 8 months of learning gains in reading achievement 4 years after kindergarten.

Figure 19a. Impacts of attending TK on 3rd grade achievement, low-income students with English-speaking parents, by changes in per-pupil spending (1st-3rd grades)

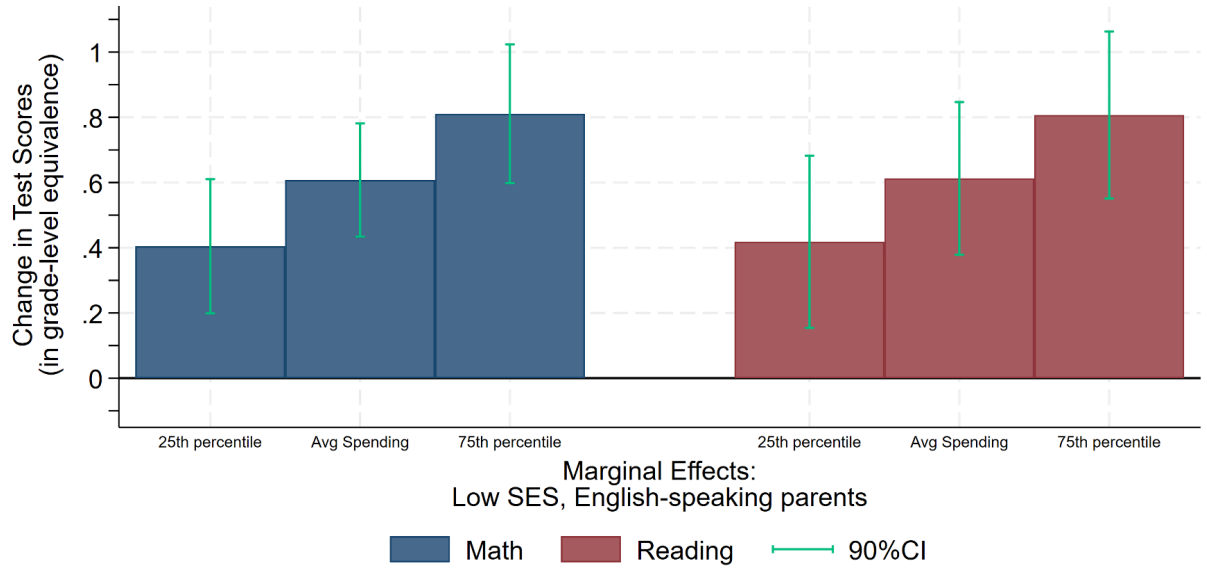


Figure 19b. Impacts of attending TK on 4th grade achievement, low-income students with English-speaking parents, by changes in per-pupil spending (2nd-4th grades)

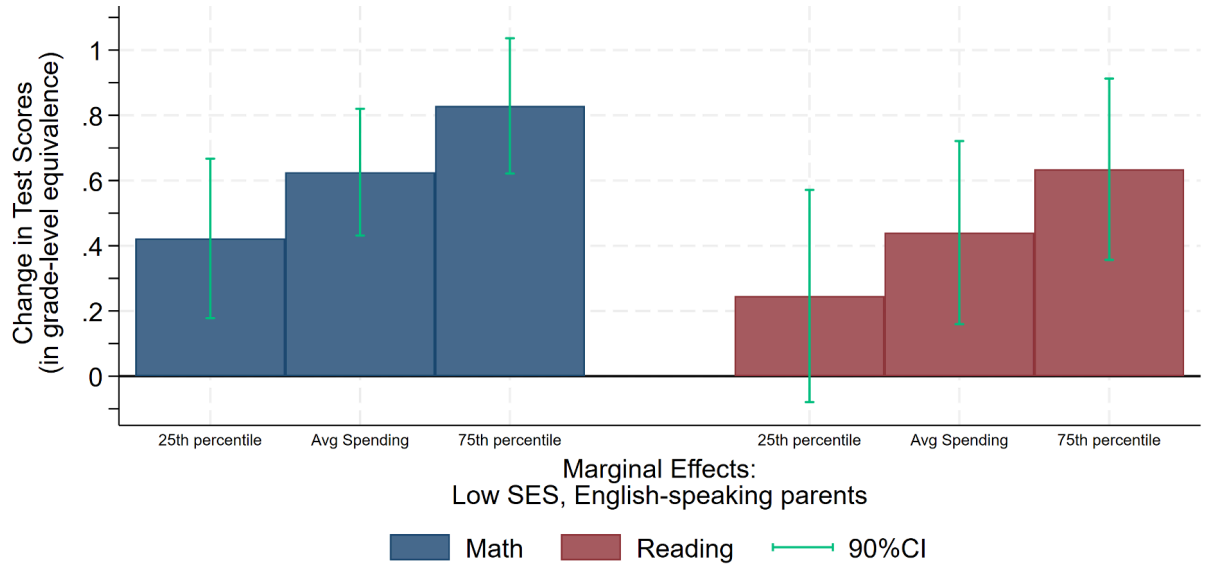


Figure 19c. Impacts of attending TK on 3rd grade achievement, low-income students with non-English speaking parents, by changes in per-pupil spending (1st-3rd grades)

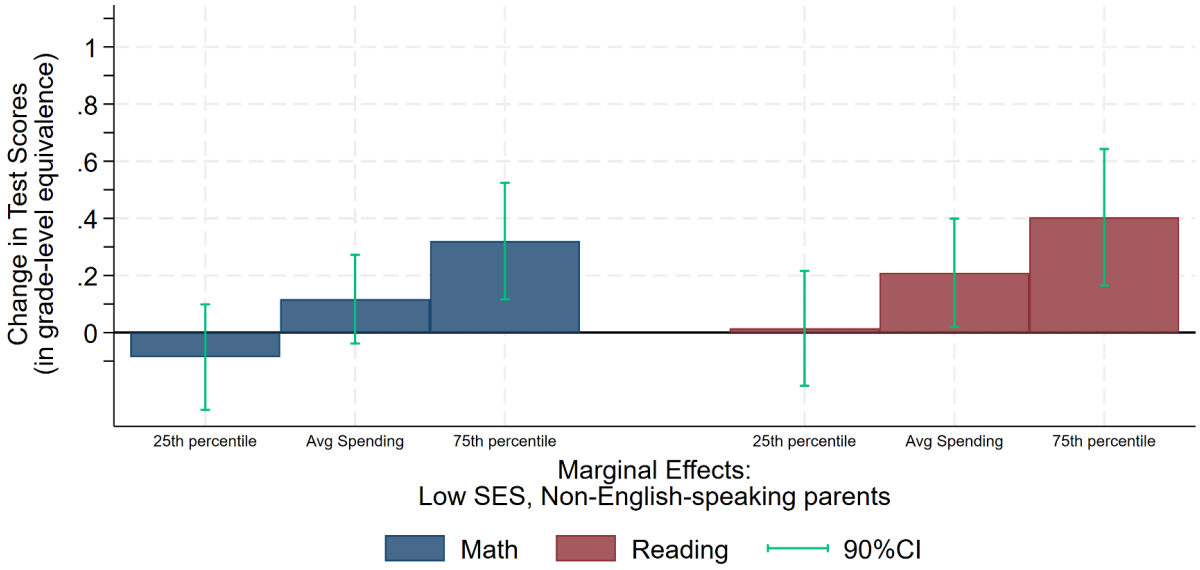
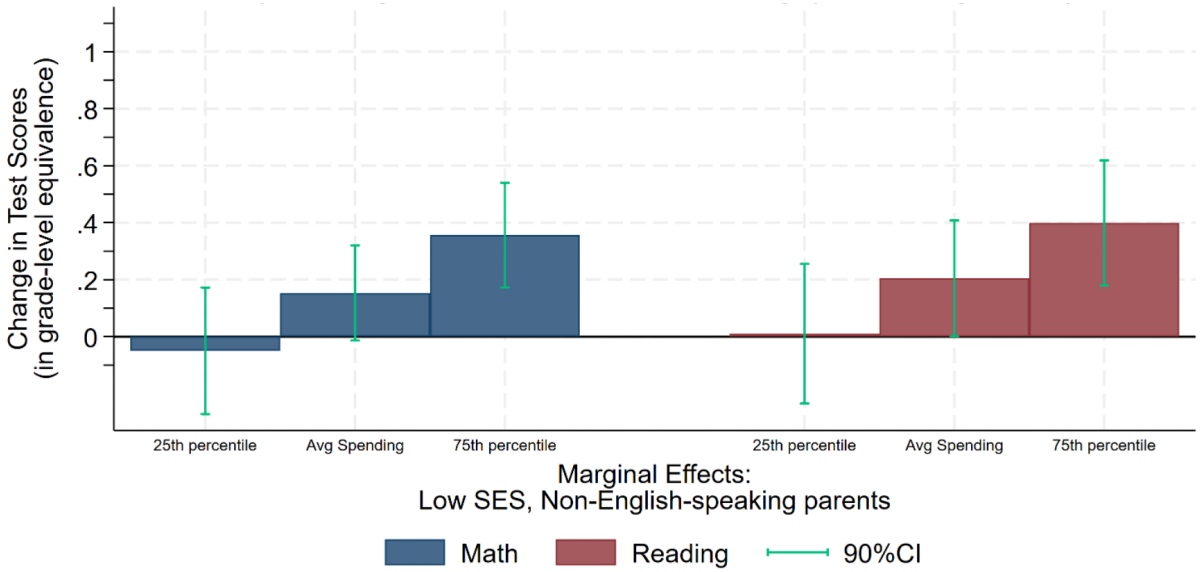


Figure 19d. Impacts of attending TK on 4th grade achievement, low-income students with non-English speaking parents, by changes in per-pupil spending (2nd-4th grades)



In stark contrast, among children from low-income families who attended elementary schools with per-pupil spending at the 25th percentile ($11,000), TK attendance led to a statistically significant but more modest 0.2 grade-level improvement in math achievement three years after kindergarten and a 0.25 grade-level improvement in reading achievement three years after kindergarten (equivalent to 2-3 months of learning gains more than the typical student from the same school in the same kindergarten cohort experienced who did not attend TK). These latter results are not trivial achievement gains, but highlight that the impacts of TK attendance depend heavily on the quality of resources experienced in the elementary school years that follow. And while the point estimates for the estimated effects of TK attendance remained positive four years after kindergarten for these students who attended less well-resourced elementary schools, those estimated TK effects became statistically insignificant over that time period (see Figure X). At the same time, the results indicate a $1,000 increase in school spending experienced for three consecutive years (induced by LCFF) led to a full grade-level improvement in math and reading achievement, three and four years after kindergarten; and those effects were most pronounced for low-income students who had attended TK.

Students with non-English speaking parents. While the take-up rates of TK were highest for students who have non-English speaking parents, the impacts on achievement were smaller than for students who have English-speaking parents. In particular, among low-income children whose parents do not speak English, we find the effects of TK attendance result in a statistically significant 0.15 increase in math achievement and a 0.2 increase in reading achievement (in grade-level equivalence). We hypothesize several important factors that may have contributed to the relatively smaller effects of TK attendance on achievement for students whose parents do not speak English. First, it may be linked with the fact that during this period (through 2016) when Proposition 227 was in effect, the default

language of instruction for English learners was English-only; as it was not until voters passed Proposition 58 repealing Proposition 227 that allowed bilingual education in California public schools. A second issue that may have undermined the relative effectiveness of TK for children whose parents do not speak English is that in these early years of the rollout of TK, these children had to take the English Language Assessment as four-year olds and would not have the opportunity to retake as five-year olds in order to be reclassified out of English Learner classification until third grade, which potentially may have undermined achievement growth for some students who would have otherwise been able to reclassify earlier if the English Language Assessment had been initially taken as five-year-olds. This policy and school practice was in fact changed due to these very concerns in subsequent years, but had not yet been changed for the cohorts analyzed here in the current study.

Students who attended CSPP at age 3. Our current findings are restricted to results from estimating a difference-in-discontinuity model of TK effects that allows us to estimate effects separately for students who did and did not attend CSPP two years prior to kindergarten entry. These findings are causal estimates of TK effects for two separate groups, but the difference in these effects is purely descriptive as we have not estimated models that isolate exogenous variation in CSPP *enrollment at age 3*.

Nevertheless, we find that TK impacts on both academic achievement among socioeconomically disadvantaged students were significantly larger among those who first attended CSPP at age 3. Specifically, TK impacts on reading and math achievement in third and fourth grade were roughly 0.2-grade-levels higher for these early CSPP-goers (among socioeconomically disadvantaged students).

The pattern is consistent with several channels through which antecedent CSPP participation could support positive TK effects on child development, generally. Children who develop secure attachments with early educators are more likely to form positive relationships with peers and future

educators, and to perform better in the classroom (National Scientific Council on the Developing Child, 2004; Pianta and Steinberg, 1992; Birch and Ladd, 1997; Tarlov, 2008). Children who attended CSPP and formed positive relationships with CSPP educators may be more likely to form positive relationships with TK teachers, and to benefit more from TK instruction. Higher quality and quantity of exposure to linguistic inputs can lead to improved early and later communication skill acquisition (Shonkoff and Phillips, 2000); children who enter the TK classroom with greater language proficiency may benefit more from TK instruction, particularly children from non-English-speaking households for whom CSPP may be an important setting for early language development.

The cultural and linguistic alignment between CSPP's workforce and children from non-English-speaking households is a further dimension of the CSPP-TK sequence that provides critical context for understanding variation in the effects of TK on achievement, especially among students with lower-income, non-English-speaking parents. For these students, TK led to roughly half a year of learning gains in math and reading when preceded by CSPP. Compared to their peers who attended TK but did not first attend CSPP, TK effects for these students were 5.75 times larger in math and 3.3 times larger in reading.

Figure 20a. Impacts of TK attendance on 4th grade math achievement, by parental language & CSPP at age 3

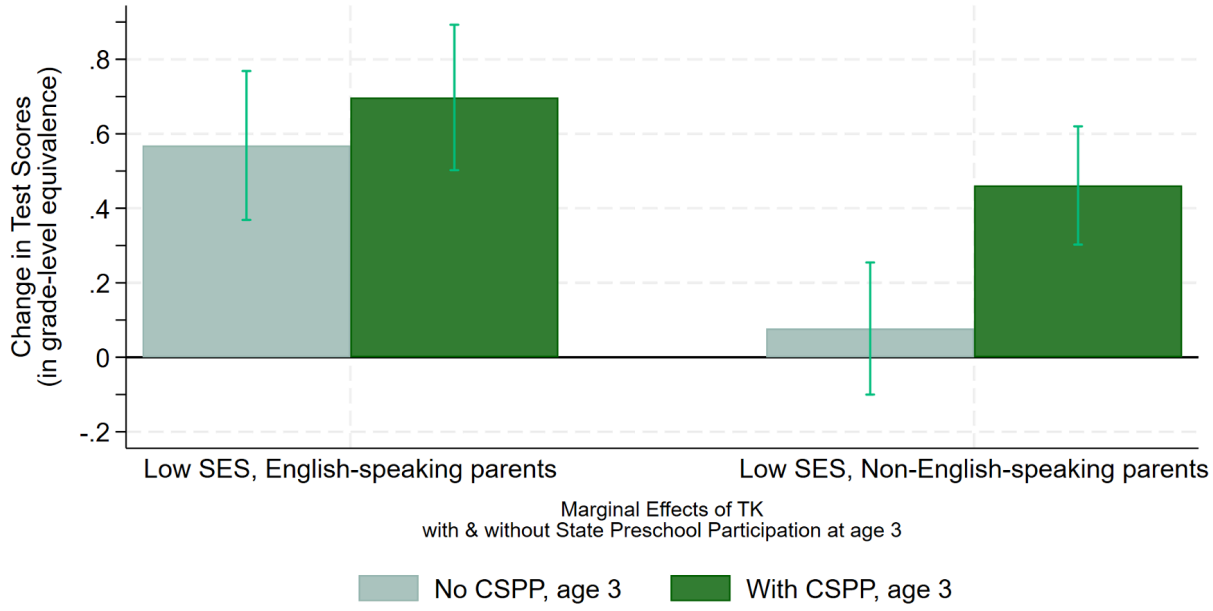
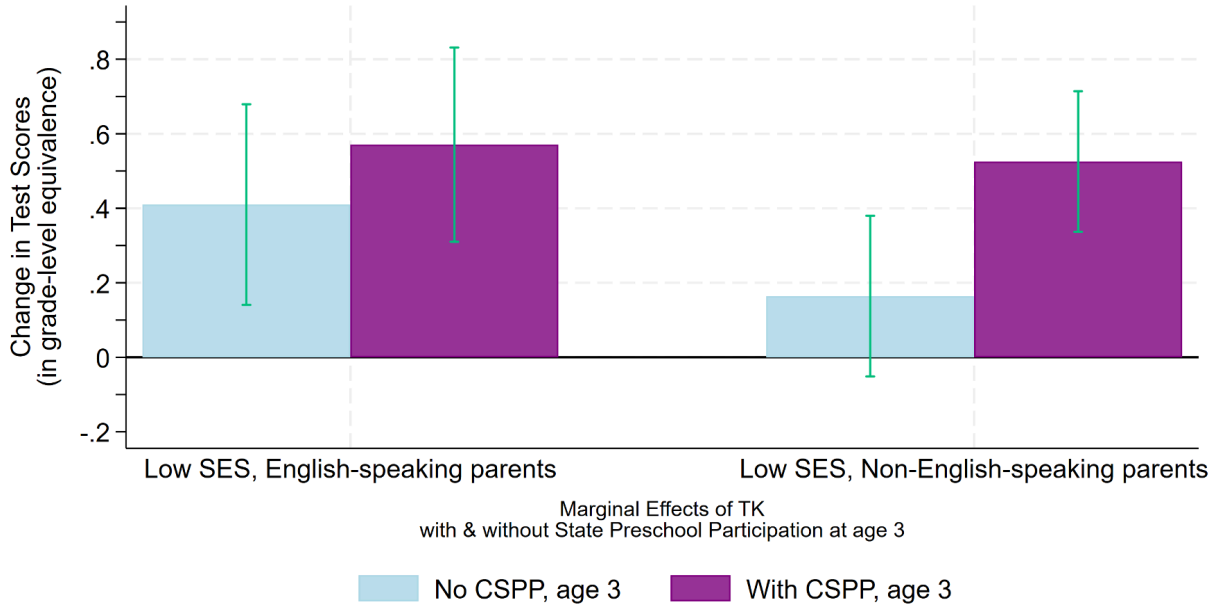
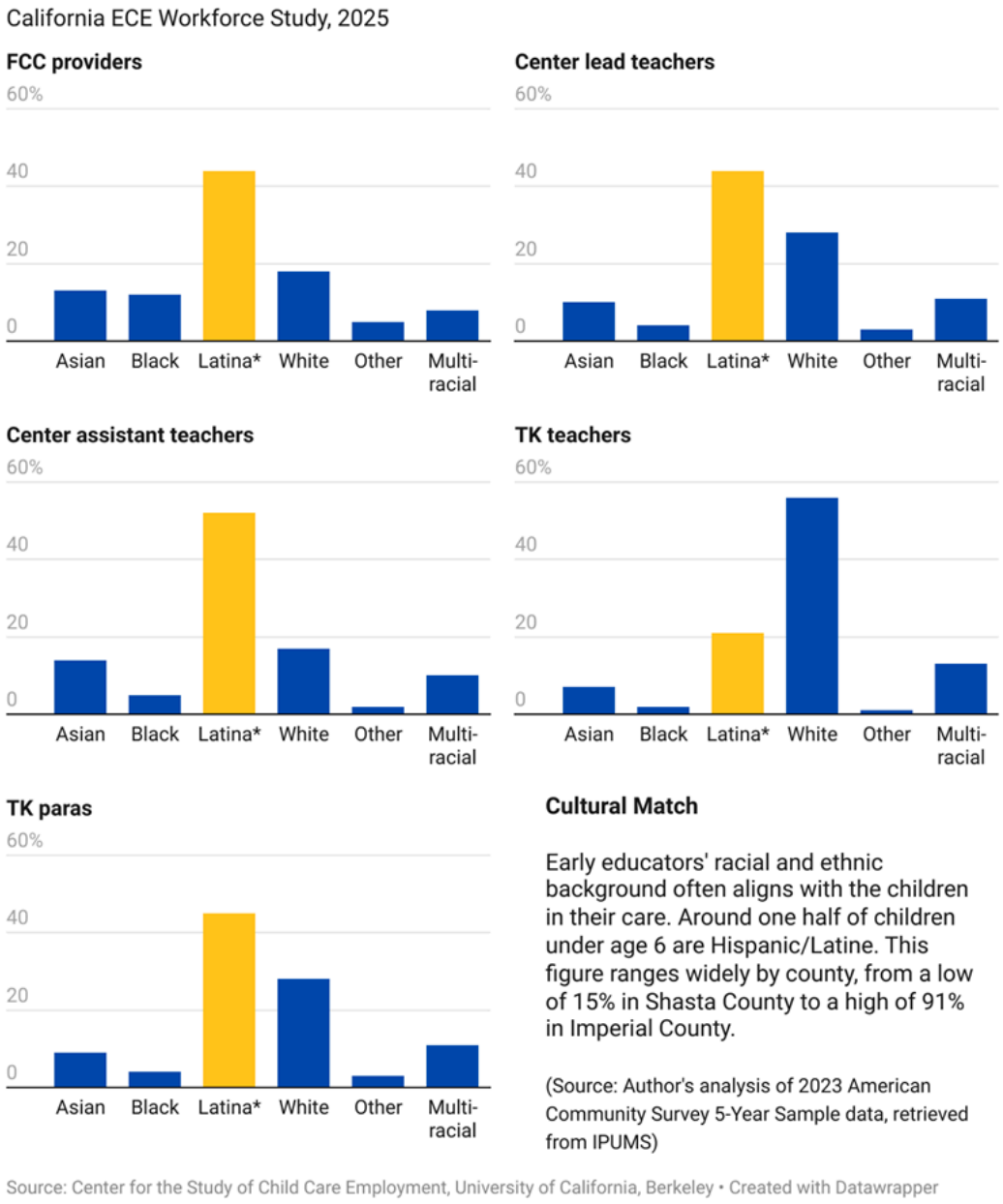


Figure 20b. Impacts of TK attendance on 4th grade reading achievement, by parental language & CSPP at age 3



The teaching workforce of CSPP has much greater linguistic diversity and bilingual capacity among its teachers, particularly Spanish, relative to TK teachers (Powell et al., 2025; reproduced in Figure 23 below). While the source of these differences in impacts of TK attendance by parental language background have not been fully explored or identified, one potential factor to consider is the availability (or lack thereof) of teachers with bilingual instructional skillsets that may be requisite to maximize student outcomes for classrooms with a significant proportion of English learners, and/or to be able to differentiate and provide more tailored instruction to students in classrooms with greater variation in school readiness. As highlighted in the work of Powell et al., in California, most early educators are women of color, and Latinas comprise the largest group of early educators in every role except credentialed TK teachers. Like other elementary school teachers, TK teachers are most often White.

Figure 21. Early Educator Race and Ethnicity



Recent analysis of CSPP preschools in 2024-25 provides additional evidence of children's language match with classroom staff. 59 (69) percent of Spanish-speaking children in community-based CSPP-funded preschools were in classrooms with lead teachers (classroom staff) spoke who also spoke Spanish (Darriet, 2026).

Validity of the Regression Discontinuity Design for TK impacts

Overall, it is clear that the introduction of TK and TK eligibility policy caused children who were TK eligible to attend TK at rates of roughly 60 percent (on average), whereas children whose 5th birthdays were only a few weeks later and just barely ineligible for TK did not have access to TK programs. Moreover, the rollout of TK and its age-eligibility cutoffs led to significant (relative) increases in pre-k attendance for low-income children. Furthermore, children whose 5th birthdays occurred just barely before the December 3rd cutoff for TK eligibility have significantly higher subsequent achievement in both math and reading that persists in third and fourth grade, relative to TK ineligible students whose 5th birthdays are just barely after the TK cutoff date. This pattern of improved student achievement around the TK eligibility cutoff is particularly pronounced among children from low-income families. To the extent that these two groups of children – TK eligible and ineligible children born just weeks apart – are similar in the other characteristics that determine achievement outcomes, then these results imply that TK attendance raises math and reading achievement through at least 4th grade (on average). In this section, we present several different pieces of evidence suggesting that this key identification condition is met.

It is important to recall in this context that California school districts were mandated to serve all TK eligible students in local elementary public schools. TK eligibility around the age cutoff should be randomized as long as children's birthdates cannot be perfectly manipulated by parents, students, teachers, school administrators (and/or selective enforcement of TK eligibility policy is prohibited) (Lee, 2005). For example, we might worry that parents most motivated to get pre-k access do not report children's birthdates honestly if their child's 5th birthday just missed the eligibility cutoff date or if school administrators select (presumably based on other unobserved indicators of school readiness)

some children just weeks beyond the age eligibility threshold and adjust their birthdates to be just eligible. This could lead to biased (upward) estimates of the effect of TK by leaving a relatively more disadvantaged group of children beyond the TK eligible cutoff (ineligible: Dec 3-).

The first source of evidence on the validity of an RD design concerns the "integrity" of the assignment variable (5th birthdate), and in the present case this seems very unlikely that it could be compromised in any way. The schools' use of child birthdates to determine TK eligibility are based on verification of birth certificates that are submitted. The institutional knowledge of this process (e.g., the use of a policy predetermined birthday cutoff for TK eligibility that prevailed for a given year) suggests that the child birthdate cutoff assignment variable could not be easily manipulated in ways that would confound the internal validity of the RD design. The room for parents or administrators to finely manipulate birthdates around the TK age-eligibility cutoffs would thus seem narrow. Further, if such manipulation were taking place, it would be observable in a discontinuity in the density of birthdates at the TK eligibility cutoff—in the example above, there would be "missing" birthdays just beyond the cutoff (Dec 3-) and a corresponding "hump" just before the cutoff. We test for a discontinuity in the density function of child birthdays using a variant of a test proposed by McCrary (2004), using the analysis sample within 60 days on either side of the Dec 2nd cutoff. Consistent with this assumption of no manipulability, we find that a density test does not reject the null hypothesis that the number of observations is smoothly distributed around the threshold (McCrary, 2008; Cattaneo, Jansson, Ma, 2020). Unrestrictive histograms are consistent with this assumption as well. Appendix Figure C1 presents the RD density plot for children's birthdates relative to the December 2nd TK eligibility cutoff (i.e., a histogram that shows the distributions of children by date of birth around the TK age-eligibility cutoff). We examined whether there was any evidence of manipulation by displaying the distribution of

child birthdates observed in the data. There is clearly no evidence of manipulation: the date of birth follows an approximately uniform distribution within the 120-day window (60-day bandwidth) around the cutoff (Appendix Figure C1). We conducted a formal density test and confirm that there are no statistically significant changes across cohorts in the degree of manipulation, which is measured as the difference in the density around the cutoff. Since the support of the birthdate distribution is discrete, we fit a linear term in the birthdate (centered around the TK eligibility cutoff), a dummy indicator for TK eligibility, and their interaction to the log of the fraction of students with each birthdate within 60 days of the TK age-eligibility cutoff, using weighted least squares regression. The results confirm there are no statistically significant discontinuities evident in the birth date densities.[24]

A second source of evidence on the validity of an RD design concerns testing for any differential attrition. Because we observe our key achievement outcome measures for the full sample irrespective of whether they remain in the same California public school, differential attrition is not an issue among those who attend public schools in California. Some of our exploratory outcome measures (i.e., achievement, attendance, grade progression, and credits earned) are only observed for students who remained enrolled in California public schools, but miss those who may have moved out of state or who attended private schools. However, we present RD evidence that the probability of being enrolled in a California public school is balanced around the ITT threshold (TK age-eligibility cutoff) for each of the years after entering kindergarten, which suggests that sample attrition is not differential (see Appendix Table C6). Furthermore, auxiliary RD regressions indicate that students with 5th birthdays on or before Dec 2nd (i.e., ITT=1) are not significantly more likely than students whose 5th birthdays are just after the age cutoff (ITT=0) to appear in the statewide records on entering kindergarten cohorts.

[24] The 60-day "bandwidth" is chosen so that a reasonable number of points of support are included on either of the cutoff; the results are robust to 30-day bandwidth.

An important, third source of evidence on the validity of an RD design concerns the balance of outcome-relevant, baseline covariates. If the variation in students' position around the RD threshold is "as good as randomized," we would expect that baseline student covariates do not exhibit discontinuities at the ITT threshold. An implication of the assumption that the TK eligibility policy creates local randomization of TK attendance at the birthdate eligibility cutoffs is that all preset characteristics (i.e., fixed at age 4) should be similar for the groups of children barely old enough for TK eligibility and barely too young for TK eligibility. While we can never be certain that the unobservable characteristics of children satisfy this condition, the validity of this assumption can be tested by ensuring that the conditional expectations of the observable characteristics do not vary discontinuously in the neighborhood of the TK age-eligibility cutoff. Appendix Tables C4a-C4b and Tables C5a-C5b, present the results of estimating models similar to the main 2SLS specification on selected covariates available in the data.

As seen in the Appendix Tables C4-C5, the estimated discontinuities for nearly all of the demographic and parental SES characteristics are vanishingly small (point estimates are small in magnitude in all cases and in all cases are not statistically significant). For example. the likelihood of parental socioeconomic disadvantage is nearly identical for children on either side of the TK age-eligibility cutoff. In the case of independent covariates, we would expect about 5 percent of the discontinuity estimates to be statistically significantly different from zero under the null hypothesis that all student characteristics are balanced, which is line with the findings. In the table, some of the instrumented per-pupil spending estimates appear statistically significant but their magnitude is extremely small and therefore practically insignificant (e.g., it indicates a $1,000 increase in per-pupil spending is associated with less than a percentage-point change in the portion of the relevant

socioeconomic or demographic background characteristic). Consistent with this, auxiliary RD regressions in which the baseline covariates used in this study are the dependent variables indicate that the covariate differences at the ITT threshold are small and statistically insignificant.

As an omnibus test of whether children near the TK age-eligibility cutoff differ from each other in terms of their observed characteristics, Appendix Tables C7 and C8 present the estimated discontinuities in the predicted 3rd and 4th grade achievement (math and reading, respectively) as a function of baseline child and family socioeconomic characteristics. The predicted values are generated from a regression of 3rd grade & 4th grade achievement on all of the available covariates, conditional on school-by-kindergarten cohort fixed effects and years since K; but excluding any functions of the child's birthdate (including D_i, the TK eligibility indicator) and whether a child attended TK. These predicted values represent all the information contained in the covariates that predict future achievement. As shown in Appendix Tables C7-C8, there is essentially no difference in students' predicted academic performance at the TK age-eligibility cutoff, demonstrating again that children above and below the TK age-eligibility threshold are nearly identical in terms of the characteristics that affect achievement. This is also found to be true for the interaction of TK and instrumented per-pupil spending. This provides further confidence in the assumption that the unobservable determinants of achievement may also be balanced. Additionally, this evidence supports the assumption that the contrast at this threshold does not imply outcome-relevant determinants other than TK attendance. We also find while some of the instrumented per-pupil spending estimates appear statistically significant, their magnitude is extremely small and therefore practically insignificant, lending further support to the validity of the research design.

A closely-related fourth source of evidence is to examine graphically the relationship between

the TK eligibility assignment variable and the student achievement outcomes. This graphical evidence provides both unrestrictive evidence on the possible effect of the ITT and an opportunity to assess whether any discontinuities are apparent at other thresholds that did not define a treatment contrast. Focusing on the 60-day bandwidth on either side of the TK age-eligibility cutoff, we find supportive evidence of the identification strategy, as reflected in the graphical evidence that shows no apparent discontinuities at other thresholds among those from the same kindergarten cohort.

We also present additional evidence consistent with the "exclusion restriction" (i.e., that the contrast at this threshold does not imply outcome-relevant determinants other than TK attendance). We find that this age eligibility cutoff (5th birthday on or before December 2nd) was associated with large and statistically significant negative effects on achievement outcomes among students at California public schools in years before TK had been introduced; and only became positive and statistically significant after TK implementation. This provides the fifth source of evidence supporting the validity of the research design. This is elaborated upon in the additional Difference-in-Regression Discontinuity (Diff-in-RD) design results presented in the penultimate section of Appendix C.

A sixth class of evidence on the validity of an RD design involves exploring the robustness of the results to alternative ways of modeling functional form (i.e.,f(G_{ist})). Our baseline specification conditions on linear splines of G_{ist}, allowing the TK eligibility assignment variable to have distinctive slopes above and below the ITT (TK eligibility) threshold. Appendix Tables C9-C12 present another important robustness check based on restricting the sample to observations in increasingly tight bandwidths around the age-eligibility cutoff for the reduced-form and 2SLS-IV-RD estimated effects. These results provide evidence about whether the estimates are biased due to functional-form assumptions or are unduly influenced by observations that are far from the December 2nd birthday

eligibility cutoff. We checked whether the results are robust to alternative bandwidths: 15 days, 30 days; and also checked whether the main results are robust to different model specifications. Overall, the main results are robust to the choice of bandwidth and model specification. The results in Appendix Table C9, C10, C11, and C12 indicate that the 2SLS-IV-RD estimates are robust as the sample shrinks with each of the progressively tighter bandwidths and with narrower splines, including splines within 14 days of the birthday eligibility cutoff and including bandwidths within 30 days of the cutoff. If anything, for low-income children whose parents speak English, the 2SLS-IV-RD estimates become larger with narrower splines (e.g., spline 0-20 days vs 0-30 days) and as the bandwidth tightens (e.g., 45-day bandwidth vs 60-day bandwidth).

In addition, placebo tests are performed by re-estimating the RD models but using placebo TK age-eligibility cutoffs in place of the actual December 2nd TK eligibility cutoff. As expected, the results show that the RD estimated effects of TK eligibility are always largest and with the most precision when using the correct TK age-eligibility cutoff, whereas the further the placebo cutoff is from the actual cutoff the greater the reduction in RD estimated effects with small, insignificant effects found when the placebo cutoff is greater than 8 days before the actual eligibility cutoff or greater than 6 days after the actual eligibility cutoff. Parallel patterns are found for RD “first-stage” effects of TK eligibility on the probability of TK attendance and RD "reduced-form" effects of TK eligibility on math and reading achievement 3 & 4 years after kindergarten (see Appendix Figures C5-C7). The placebo results lend strong evidence of the credibility of the TK impacts, and support the causal interpretation of the main results.

The analyses presented in Appendix Tables C4-C5, C7-C8 are convincing in demonstrating that children barely old enough for TK eligibility and barely too young for TK eligibility around the TK

age-eligibility cutoff are nearly identical to one another in terms of the observed characteristics that might affect achievement (including belonging to the same entering kindergarten cohort). As a result, it seems reasonable to believe that the regression discontinuity design (RDD) has effectively randomized TK eligibility near the age cutoff with respect to predetermined student characteristics. Taken together, the findings do not reject the hypothesis that TK-eligibility induced differences in the probability of TK attendance is effectively randomized near the TK age-eligibility cutoff. Thus, using the 2SLS-IV-RD framework, the findings represent credible causal evidence of the impacts of TK and demonstrate a significant positive role for TK attendance in boosting student achievement, particularly among students from low-income families. The effect size of TK attendance is comparable to the 0.22 standard deviation effect size found for reducing class size by one-third in the Project Star experiment (see, for example Krueger, 2002); and similar in the pattern of finding the largest effects for students from socioeconomically disadvantaged backgrounds (whose parents speak English).

Grade progression and grade repetition are outcomes of the TK policy, not a predetermined characteristic, and thus we measure achievement outcomes as a function of the number of years since kindergarten cohort year based on the California TK and kindergarten eligibility laws that prevailed when each child turned 4-5 (instead of treating the child's observed grade as exogenous).

More Background on TK

California TK policy treats TK largely as a new grade in the public education system. This is a unique pre-k design characteristic relative to California's State pre-k Program (CSPP) and other state universal pre-k programs in other states e.g., (Georgia, New York) that function akin to a voucher system wherein the state contracts their pre-k classrooms to outside providers. Thus, all TK students

attend school-based classrooms rather than independent pre-k/daycare centers. Importantly, TK students are included in the funding formula for allocating state funds to districts (i.e., LCFF TK-12) rather than pre-k centers applying for funds directly from the state (as in the case of CSPP and many other state universal pre-k initiatives). Institutions offering TK in California can be districts schools or charter schools. Furthermore, regulatory quality standards with regard to teacher credentials and class sizes are enforced similarly for TK and K-12 grades, including similar teacher salary structures as other elementary school teachers.

These features of TK programming may contribute to more standardization of curriculum and alignment with Common Core standards, but some early childhood policy leaders have expressed concerns about/that adopting a more academic focus for four-year olds in TK may come at the expense of more holistic (age-appropriate) play-based programming emphasis that cultivate socioemotional development and relational skills (as emphasized in Head Start and CSPP that occur in independent centers). Whether there is greater variation in quality provision in TK vs other public local pre-k options remains an open, unanswered question.

The primary RD design and this alternative Diff-in-RD design are complementary approaches, and identify potentially different local average treatment effects and help extend the external validity of the estimated causal impacts of TK attendance. The fact that these two different research designs (approaches) yield broadly similar patterns of results is noteworthy, and provides compelling evidence of the robustness of the findings and the sources driving heterogeneity of treatment effects. (e.g., these results provide suggestive evidence and additional support that the findings are representative and not narrowly applicable to the oldest (youngest) students relative to their cohort/grade).

Contributions. This study makes several contributions to the study of large-scale, pre-k expansions.

First, TK and CSPP offer a rare opportunity to study the influence of antecedent preschool attendance on pre-k impacts. This understudied, potential source of heterogeneity in preschool impacts has become increasingly important amid the proliferation of one-year pre-k programs as 51 percent of all three-year-olds were enrolled in public or private ECE (6% are in state pre-k programs as three year olds), nationally, in 2019 (Friedman-Krauss et al., 2020). In this context, one-year pre-k programs may be best understood as one part of multi-year learning sequences.

Second, our study of the CSPP-TK pre-k sequence may shed light on the broader "sustaining environments" hypothesis, which posits that the persistence of early learning gains depends on the quality of later learning environments. One explanation of this hypothesis focuses on instructional redundancy, typical of two-year preschool experiences, and is theorized to explain findings of diminishing marginal returns of the second year. In our study population, many children attend two years of CSPP, while others attend one year of CSPP followed by TK, allowing us to examine the role of instructional redundancy in explaining variation in the persistence of preschool impacts. TK was explicitly designed to act as a "bridge from preschool to kindergarten" and allow for the gradual introduction of more formal, academic curricular content aligned with elementary school learning goals. While TK used modified kindergarten curricula, CSPP delivered more conventional, whole-child preschool programming. Thus, the CSPP-TK sequence represents graduated programming exposure in which novel classroom experiences in TK may build on CSPP-induced developmental gains, a unique sequence in which theory suggests that synergistic effects may be expected.

Third, this study of the CSPP-TK sequence provides insight into an important innovation in pre-k expansion at scale. TK was implemented in a novel manner that avoided the potential for academic "push down"—moving content from higher grades to earlier ones, and potentially buoyed

by investments in CSPP. Some researchers and policymakers see curricular emphasis on academic skills as a promising avenue for pre-k reform aimed at better preparing students for today's more academic kindergarten. Others warn that the growing emphasis on academic content in kindergarten has extended to pre-k programming in a manner that may undermine pre-k efficacy (Miller and Almon, 2009; Zigler and Bishop-Josef, 2006; Kagan and Kauerz, 2007). The CSPP-TK sequence is a rare example of how states may pursue alignment in preschool and early grade programming, which may be a critical component of sustaining the benefits of early education programs (Weiland et al., 2023).

In sum, the study contributes to the evidence base that demonstrates the steep development trajectory that can be influenced by expanding learning opportunities in early childhood.

Summary Discussion & Conclusion

This paper investigated the impacts of funding for and participation in California's State Preschool Program (CSPP), Transitional Kindergarten (TK) program, and increases in elementary school spending induced by the Local Control Funding Formula (LCFF). The study examined the extent to which (1) CSPP funding per-pupil, (2) attending California's Transitional Kindergarten (TK program) and (3) increases in TK-5 funding affect (a) student learning trajectories and (b) achievement gaps by parental socioeconomic status (SES), race/ethnicity and language. Using population student-level longitudinal administrative data from the full universe of public school students in California, and linked with school-, provider-, and student-level pre-k records, the study employed a rigorous methodology to facilitate causal inference, combining regression discontinuity and difference-in-differences designs to isolate the interactive effects of TK participation and LCFF-induced changes in per-child funding.

The results indicate California's Transitional Kindergarten program delivered significant learning

gains to students. These gains are particularly pronounced among students from socioeconomically disadvantaged families—who often lack access to early educational opportunities as compared to their relatively advantaged peers—suggesting a reduction in the socioeconomic achievement gap. Among low-income children with English-speaking parents, TK attendance led to 6 months more learning gains in 3rd grade reading and math achievement on average, relative to the typical student from the same school in the same kindergarten cohort experienced who did not attend TK. These gains were consistently and substantially larger in schools with higher levels of per-child funding. TK-induced gains in third-grade math and reading achievement persisted into fourth grade. Gains in math achievement were particularly persistent. Persistence appeared stronger where per-child funding was higher. The achievement gains induced by TK participation and increases in per-child funding were mutually reinforcing: TK attendance and LCFF-induced spending increases exhibit positive, synergistic impacts.

Instead of thinking of individual policies in a vacuum (that is, in isolation), one should also consider the potential interactive effects of greater school resource equity and ways in which early childhood investments may augment and support the efficacy of later investments that are equity enhancing, which may lead to positive developmental multiplier effects that may not have been possible without the funding increases. The results from this study highlight that state and local education leaders can create and bolster school conditions (in the early years) that substantially boost learning outcomes and narrow racial/ethnic/SES achievement gaps for children, which may have lasting impacts into adolescence and adulthood. To achieve meaningful progress (and in partnership with parents), school communities need actionable metrics they can use to assess current conditions and monitor their progress.

A more holistic approach to the set of outcomes considered, including academic achievement,

socioemotional development, and health-related measures is important, and will be pursued in future research. These results are part of a larger project in which we investigate the causal impacts of pre-k-12 school resources on a suite of socioemotional development and academic, disciplinary, behavioral, school climate, and mental health outcomes throughout the K–12 school career. Our goal is to identify at what child development stages (early childhood/pre-k/elementary/middle/high school), and at what levels of the public education system (from federal, state, district, school, classroom, to student level), can educators and policymakers most productively address educational opportunity gaps that open in early childhood and have cascading impacts on student learning trajectories when left unaddressed. This work has promise to highlight the types of education policy interventions—particularly using the expansions of CSPP, and implementation of TK and LCFF—and opportunity-to-learn factors that hold the most promise for improving student success for all children, and simultaneously hold the greatest potential to reduce racial and socioeconomic achievement gaps across grades and over time.

Moreover, these expansions of public pre-k-TK-12 investments in education have been sustained over the past decade, as evidenced in the approved funding TK-12 public schools received in the 2022-23 state budget that is nearly triple the funding received in 2011, and the 2020 Master Plan that highlighted a prominent role for CSPP. California's 2025 historic budget has reshaped early learning policy in the state. In addition to making needed investments in expanding access and reforming rates in childcare, the budget makes major changes to preschool education with the expansion of transitional kindergarten (TK) to all four-year-olds. The expansion of transitional kindergarten and preschool is an enormous policy opportunity; and as districts begin to implement this new grade level, they will need support from the state, researchers, and advocates to maximize the fullness of its potential to positively

impact student success in early childhood education and beyond.

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APPENDIX A: The California State Preschool Program

CSPP by the Numbers

2025-26:

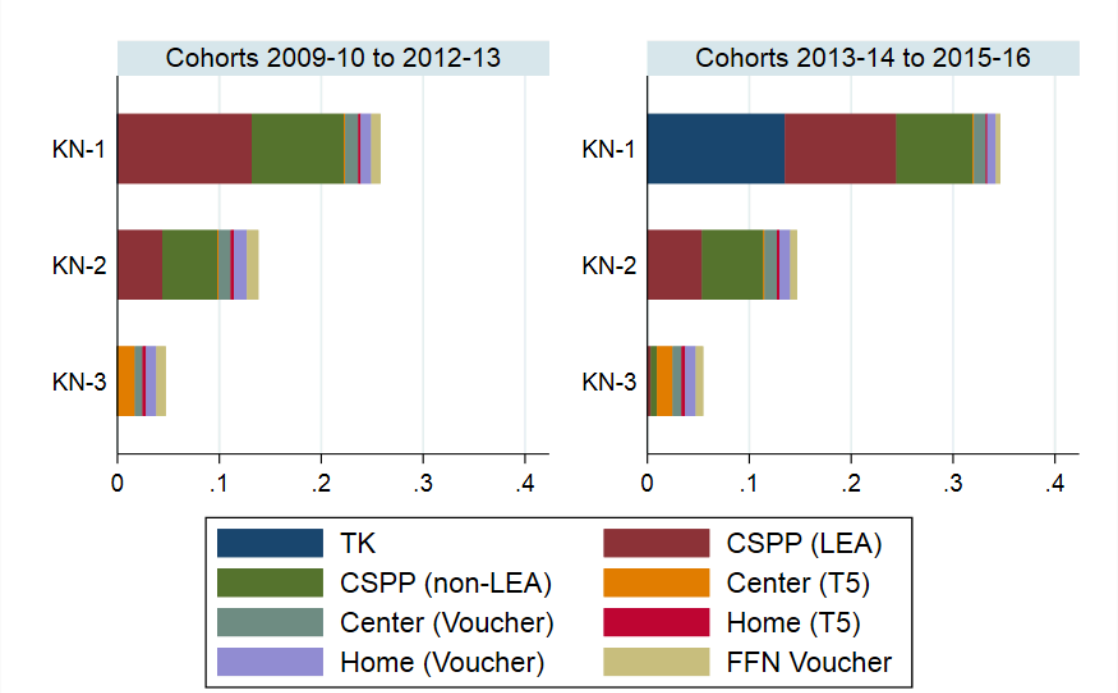
- $2.8B in funding ($4.2B for TK) – policy aim of pre-K access for all income-eligible three-year-olds
- 63% of preschool-age children eligible, from families with incomes below SMI ($127,000 per year for a family of four)

2009-10 to 2015-16:

- 1-in-4 public school students attended CSPP for an average of 13 months
- 1-in-4 three-year-old state preschool students in the U.S. were in CSPP
- 1-in-5 TK students first attended CSPP during the introduction of the program from 2012-13 to 2014-15
- 1-in-2 CSPP students were English language learners
- 1-in-2 CSPP students lived in families with incomes below the federal poverty line

Figure A1 presents enrollment in CDE-funded ECE programs one, two, and three years prior to kindergarten entry for California students who entered kindergarten before (cohorts 2009-10 to 2012-13) and after (cohorts 2013-14 to 2015-16) TK implementation. CSPP accounted for the vast majority (85 percent) of CDE-funded ECE enrollment among preschool-age children, and over half of that enrollment occurred in CSPP programs administered by public schools.

Figure A1: Enrollment rates in California early care and education programs

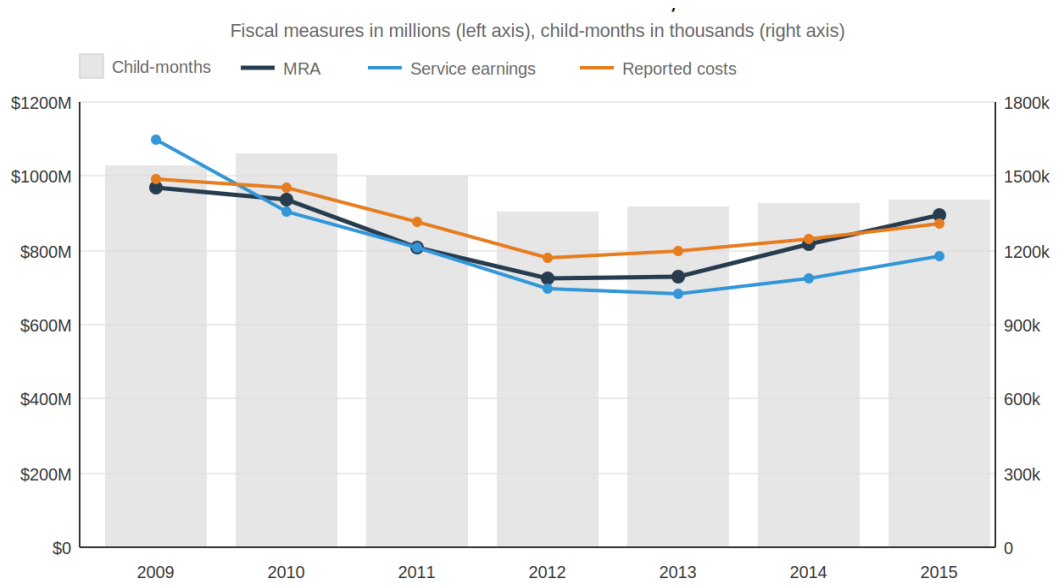


Notes: (T5) indicates the center or home was funded via direct contract with the state and subject to Title 5 regulations that impose more stringent requirements on staff-child ratios and teacher qualifications. (Voucher) indicates the center; home; or family, friend or neighbor (FFN), was funded via a voucher program subject to Title 22 regulations on that impose less stringent requirements on staff-child ratios and teacher qualifications. (LEA) indicates that CSPP services were provided by a public school (local education agency). We select the modal program in which a child was enrolled in that year to summarize aggregate enrollment. In the case of a tie, a single program was chosen in the following order: TK, CSPP (LEA), CSPP (non-LEA), Center (T5), Center (Voucher), Home (T5), Home (Voucher), FFN voucher.

CSPP Spending: Descriptive Evidence and Instrument Validity

Descriptive Evidence on Funding and Enrollment. Descriptively, contractors' enrollment activity is tightly correlated with contract MRA. Analysis of CSPP enrollment records linked to contractor fiscal records reveals that contractors reported approximately 22 child-days of enrollment in fiscal records for every child-month reported in enrollment records, consistent with a standard operating month. Child-months observed in enrollment records closely track the child-days reported in fiscal reimbursement claims, confirming that the two administrative data sources measure the same underlying enrollment activity.

Figure A2: CSPP Fiscal Measures and Enrollment, FY 2009-2015



At the agency-year level, each additional $1,000 in MRA is associated with 1.3 additional child-months of observed enrollment. At 22 days per month and a blended reimbursement rate of approximately $28 per day, this implies roughly $800 in enrollment-based reimbursements per $1,000 of contract allocation. Actual reimbursements track MRA at approximately $0.91 per dollar, indicating that contractors utilized most of their contract allocation.

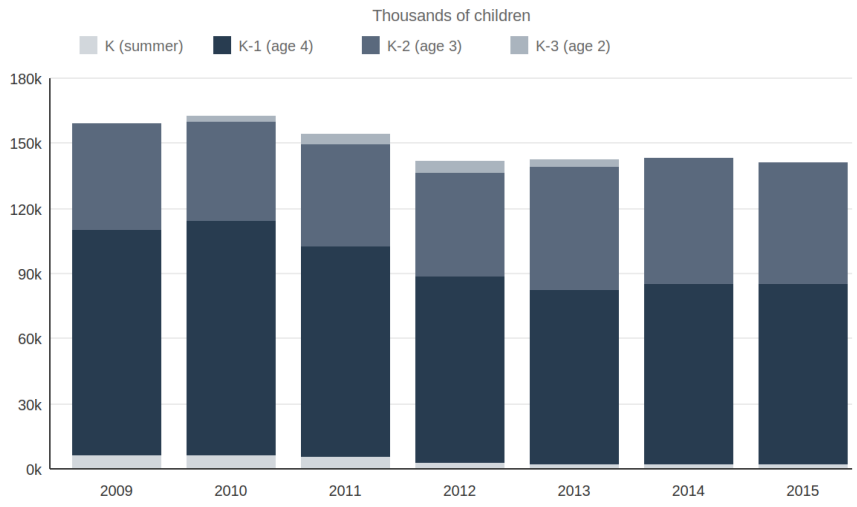
The relationship between days and months of enrollment declined over this period. In FY 2009-10, contractors reported approximately 22.5 days per child-month of enrollment. By FY 2014-15,

this had fallen to 19.3 days per month, a 14 percent reduction. This pattern is consistent with contractors adjusting to funding cuts in part by reducing days of operation, shortening the program year or closing on additional days, rather than reducing the number of enrolled children.

Enrollment Composition During Contraction. Despite the partial recovery in funding after FY 2012, enrollment did not return to pre-cut levels. Examination of enrollment composition by kindergarten cohort reveals where the adjustment occurred.

From 2009 to 2015, just over 70 percent of enrollment in a given year was comprised of new entrants, while children returning for a second year of CSPP at age four accounted for nearly 30 percent of annual enrollment. Age-3 enrollment remained stable at approximately 46,000 to 48,000 children annually throughout the funding cuts, and grew to 57,000 by the 2015 cohort as funding was restored. Children who enrolled at age 3 continued to age 4 at consistent rates. The funding reduction-induced enrollment adjustment fell on a single margin: new entrants at age 4. The apparent conflict with required prioritization of four-year-old enrollment may be partly explained by changes in age-eligibility definitions introduced by the Kindergarten Readiness Act of 2010.

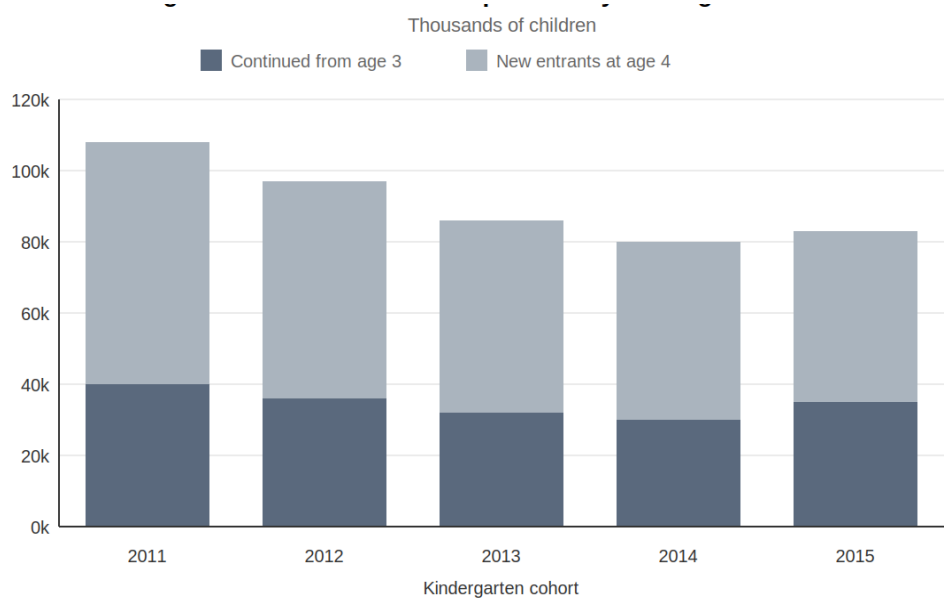
Figure A3: CSPP Enrollment by Age, FY 2009-2015



New age-4 entrants fell from 68,000 in the 2011 cohort to 48,000 in the 2015 cohort, a decline of 30 percent. The introduction of Transitional Kindergarten beginning in 2012-13 may provide an alternative public option for age-eligible children, and some of this decline may reflect substitution toward TK

rather than reduced CSPP access. However, substitution was minimal during the first few years of TK implementation, when the TK year occurred after the two-year CSPP age-eligibility period, which was extended to three-years for TK-eligible children. TK eligibility is limited to children with fall birthdays, and the instrument does not predict TK enrollment (Table A2 below), suggesting the funding-enrollment relationship we estimate is not confounded by TK expansion. The persistence of lower enrollment into the recovery period may reflect capacity reductions that were difficult to reverse, though the specific mechanisms cannot be identified in these data.

Figure A4: Age-4 CSPP Enrollment Composition by Kindergarten Cohort



Funding Shocks and Enrollment Rates by Cohort. Our primary enrollment measure is an indicator for whether a child was enrolled in CSPP for at least two months at any point during the eligibility window. A child contributes to the ever-enrolled count for their kindergarten cohort if they attended CSPP at age 3, age 4, or both. Only age-3 enrollment and new age-4 entry represent marginal additions to a cohort's ever-enrolled count; continuing children were already counted when they enrolled at age 3.

This distinction matters for understanding the relationship between fiscal-year enrollment and cohort enrollment rates. Total fiscal-year enrollment can decline substantially while the share of children ever enrolled in CSPP remains relatively stable. If age-3 enrollment is maintained and fewer children enroll for two years rather than one, fiscal-year counts fall but cohort coverage does not. Table

A1 presents enrollment and funding by kindergarten cohort. The funding shock is constructed as the sum of changes in statewide MRA from the 2009 baseline at ages 3 and 4, scaled by 2009 cohort size, a descriptive figure distinct from the instrument.

Table A1: Funding Shocks and Enrollment by Kindergarten Cohort

K Cohort	Avg. Two-Year Shock	Total Months	Ever Enrolled	Cohort Size	Rate
2011	−$66	1,499k	116,848	477,889	24.4%
2012	−$422	1,367k	107,269	444,244	24.1%
2013	−$893	1,259k	101,278	442,739	22.9%
2014	−$1,072	1,210k	97,458	427,015	22.8%
Mean	−$613	1,334k	105,713	447,972	23.6%

Notes: Avg. Two-Year Shock is the sum of changes in statewide MRA from 2009 baseline at ages 3 and 4, scaled by 2009 cohort size (452,250). Total Months sums K-2 (age 3) and K-1 (age 4) enrollment months. Ever Enrolled counts children enrolled at age 3 or as new entrants at age 4, distinct from fiscal year head counts. Rate is Ever Enrolled divided by Cohort Size.

From the 2011 to 2014 cohort, cumulative per-pupil funding exposure declined by approximately $1,000. Over the same period, total enrollment months fell 19 percent and ever-enrolled counts fell 17 percent, but cohort sizes also declined by 11 percent. The enrollment rate declined only 1.6 percentage points, from 24.4 to 22.8 percent.

The estimated 1.4 percentage point change in the probability of CSPP enrollment caused by a $1,000 two-year MRA shock is consistent with the aggregate 1.6 percentage point decline in enrollment rates. The apparently modest coefficient captures most of the observed variation in enrollment rates across cohorts.

First-Stage Results

Table A2 reports the first-stage relationship between the shift-share instrument and CSPP funding and enrollment outcomes.

Table A2: First-Stage Response of CSPP Funding and Enrollment to Instrument

	Mean (SD)	Coefficient (SE)
CSPP Funding & Enrollment		
Contractor Spending (per 1,000 pp)	−0.472	0.840***
	(0.838)	(0.063)
Service Earnings (per 1,000 pp)	−1.315	1.116***
	(5.665)	(0.424)
Contract Earnings (per 1,000 pp)	−0.485	0.828***
	(0.634)	(0.046)
Pr(CSPP), one to two years before K	0.236	0.014***
	(0.425)	(0.003)
Months of CSPP, one to two years before K	2.983	0.246***
	(6.069)	(0.047)
Observations		1,774,535

Notes: Each row reports the coefficient from a separate regression of the indicated outcome on the shift-share instrument (ΔMRA, per 1,000 pp), with district and cohort fixed effects. Sample restricted to cohorts 2011–2014 in non-small districts. Robust standard errors clustered at the district level in parentheses.

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

Falsification Tests. Table A3 reports coefficients from regressions of student, district, and neighborhood characteristics on the shift-share instrument with district and cohort fixed effects. These tests assess whether CSPP funding shocks are associated with demographic or economic trends that might independently affect child outcomes.

Student Characteristics. We find no evidence that funding shocks predict changes in student composition along most dimensions. Coefficients on gender and socioeconomic disadvantage are small and statistically insignificant. Among race/ethnicity indicators, we find modest associations with Asian/Pacific Islander (0.004) and White (−0.007) shares, though these are small relative to their means of 0.114 and 0.238, respectively. The coefficient on non-English parental language (0.009 against a mean of 0.376) is statistically significant but similarly modest in magnitude.

Other ECE Programs. If the instrument captured general trends in early childhood program availability rather than CSPP-specific funding, we would expect associations with enrollment in other CDE-administered programs or Transitional Kindergarten. We find no such associations. Coefficients on participation in other Child Development Division programs and TK enrollment are uniformly small and statistically insignificant.

District Locale and Demographics. The instrument is negatively associated with rural locale (−0.039), consistent with CSPP contractor activity concentrating in urban and suburban areas. Among district-level demographic measures, we find associations with Hispanic share (0.005), White share (−0.013), economic disadvantage (0.037), and English Learner share (−0.054). These patterns are consistent with CSPP contractors historically serving communities that were more Hispanic, more economically disadvantaged, and undergoing demographic change over the study period. District fixed effects absorb time-invariant differences in these characteristics; the coefficients reflect within-district trends correlated with baseline CSPP exposure.

District Economic Conditions. We find no association between funding shocks and the SES index, median income, educational attainment, poverty rate, housing values, or single-mother household share. Among economic indicators, we find small but statistically significant negative associations with unemployment (−0.002) and SNAP receipt (−0.004). These associations are modest relative to their means (0.113 and 0.087, respectively) and suggest that districts with greater CSPP exposure experienced slightly faster post-recession recovery, which would if anything bias estimates toward finding positive effects of CSPP funding on achievement.

Population and Child Care Capacity. We find no association between funding shocks and birth counts, population by age segment, Head Start capacity, or licensed child care capacity outside of CSPP. The absence of associations with these supply-side measures suggests the instrument is not capturing broader trends in early childhood infrastructure investment.

In sum, the falsification tests reveal modest associations between the instrument and within-district demographic trends, consistent with the historical geography of CSPP contractor placement in communities undergoing change. These associations are small in magnitude, and the patterns do not suggest confounding that would bias estimates of CSPP effects on enrollment or achievement.

Table A3: Validity Tests of Student and District Characteristics on CSPP Funding Shocks

	Mean (SD)	Coefficient (SE)
Student Characteristics		
Female	0.484	−0.001
	(0.500)	(0.001)
Socioeconomic Disadvantage	0.472	0.003
	(0.499)	(0.006)
Non-English Parental Language	0.376	0.009***
	(0.484)	(0.003)
Student Race/Ethnicity		
American Indian/Alaska Native	0.006	−0.000
	(0.074)	(0.000)
Asian/Pacific Islander	0.114	0.004***
	(0.317)	(0.001)
Black	0.062	−0.000
	(0.241)	(0.001)
Latino	0.579	0.003
	(0.494)	(0.002)
White	0.238	−0.007***
	(0.426)	(0.003)
Unknown	0.002	0.000
	(0.039)	(0.000)
Observations		1,769,841
Other CDE ECE Programs		
Pr(Other CDD program), one to two years before K	0.048	0.001
	(0.214)	(0.001)
Pr(Other CDD program), one to three years before K	0.070	0.001
	(0.256)	(0.001)
Pr(TK)	0.050	0.001
	(0.218)	(0.003)
Observations		1,774,535
District Locale		
Urban	0.452	0.030
	(0.420)	(0.019)
Suburban	0.409	0.008
	(0.400)	(0.019)
Town	0.060	0.001
	(0.214)	(0.007)
Rural	0.080	−0.039***
	(0.181)	(0.009)
Observations		1,772,595
District Student Population Demographics (all grades)		
Share Native American	0.006	−0.001***
	(0.019)	(0.000)

	Mean (SD)	Coefficient (SE)
Share Asian	0.109	0.001
	(0.124)	(0.001)
Share Black	0.062	0.002
	(0.060)	(0.001)
Share Hispanic	0.545	0.005***
	(0.241)	(0.002)
Share White	0.240	−0.013***
	(0.197)	(0.002)
Share Free Lunch	0.522	−0.004
	(0.219)	(0.006)
Share Econ. Disadvantaged	0.661	0.037***
	(0.243)	(0.012)
Share ELL	0.173	−0.054***
	(0.146)	(0.008)
Share Special Education	0.106	−0.001
	(0.022)	(0.001)
Observations		1,772,595
District SES & Economic Indicators		
SES Index	−0.004	0.021
	(0.898)	(0.014)
Log Median Income	10.984	−0.004
	(0.296)	(0.004)
Share BA+	0.274	0.000
	(0.144)	(0.001)
Poverty Rate	0.160	−0.002
	(0.068)	(0.001)
Unemployment Rate	0.113	−0.002**
	(0.030)	(0.001)
SNAP Rate	0.087	−0.004***
	(0.055)	(0.002)
Share Single Mother	0.201	0.000
	(0.054)	(0.001)
Zillow Home Value Index	380,868	6,738
	(239,945)	(9,192)
Observations		1,758,785
District Population		
Births	15,263	967
	(31,427)	(800)
Population Age 6–12	128,146	2,044
	(272,751)	(1,322)
Population Age 13–18	116,713	8,987
	(255,816)	(6,866)
District Child Care Center Licensed Capacity (excluding CSPP)		
Head Start Capacity	900	99

	Mean (SD)	**Coefficient** (SE)
	(1,701)	(97)
Other Center Capacity	1,730	−58
	(3,831)	(80)
Private Center Capacity	8,402	208
	(15,577)	(230)
Family Child Care Capacity	5,006	249
	(10,684)	(197)
Observations		1,757,236

Notes: Each row reports the coefficient from a separate regression of the indicated characteristic on the shift-share instrument (ΔMRA, per 1,000 pp), with district and cohort fixed effects. Student-level characteristics are regressed at the individual level. District-level characteristics are regressed using student-level observations, implicitly weighting districts by enrollment. Sample restricted to cohorts 2011–2014 in non-small districts. Robust standard errors clustered at the district level in parentheses. Zillow Home Value Index: N = 1,746,529. Population and capacity variables: N ranges from 1,746,529 to 1,757,236.

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

APPENDIX B: The Kindergarten Readiness Act

Table B1: KRA Event Study Estimates

	CSPP at age 3	TK at age 4	Math	Reading
Panel A: KRA Birth Months				
e = -4	-0.014	0.005	-0.139	-0.183
	(0.002)	(0.001)	(0.015)	(0.019)
e = -3	-0.015	0.006	-0.142	-0.184
	(0.002)	(0.001)	(0.013)	(0.015)
e = -2	-0.020	0.005	-0.171	-0.202
	(0.002)	(0.001)	(0.010)	(0.014)
e = -1 (base)	-0.020	0.019	-0.160	-0.194
	(0.003)	(0.003)	(0.010)	(0.013)
e = 0	**0.112**	**0.672**	**0.624**	**0.874**
	(0.007)	**(0.015)**	**(0.014)**	**(0.018)**
e = +1	**0.089**	**0.636**	**0.651**	**0.878**
	(0.004)	**(0.016)**	**(0.017)**	**(0.020)**
Panel B: Control Birth Months				
e = -4	-0.005	0.005	-0.102	-0.128
	(0.002)	(0.001)	(0.016)	(0.020)
e = -3	-0.001	0.006	-0.060	-0.086
	(0.002)	(0.001)	(0.014)	(0.016)
e = -2	-0.000	0.004	-0.031	-0.046
	(0.001)	(0.001)	(0.011)	(0.013)
e = −1 (base)	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
e = 0	0.000	-0.017	0.026	0.034
	(0.001)	(0.001)	(0.012)	(0.014)
e = +1	0.004	-0.042	0.054	0.064
	(0.002)	(0.002)	(0.014)	(0.018)
Observations	**1,639,207**	**1,639,207**	**1,628,048**	**1,629,624**

Notes: Panel A reports estimates of treated birth month outcomes at each event time, computed as the linear combination of the event-time main effect, the event-time-by-treated interaction, and the treated main effect (α). Panel B reports control birth month outcomes (event-time main effects), normalized to zero at e = −1. CSPP and TK columns report enrollment probabilities. Math and Reading columns report achievement in grade-level equivalents, estimated net of LCFF per-pupil expenditure (instrumented using simulated LCFF exposure measures). All specifications include district-by-language and cohort-by-language fixed effects and child-level demographic controls. Robust standard errors clustered at the school district level in parentheses.

Appendix C. TK Robustness Checks and Placebo/Falsification Tests

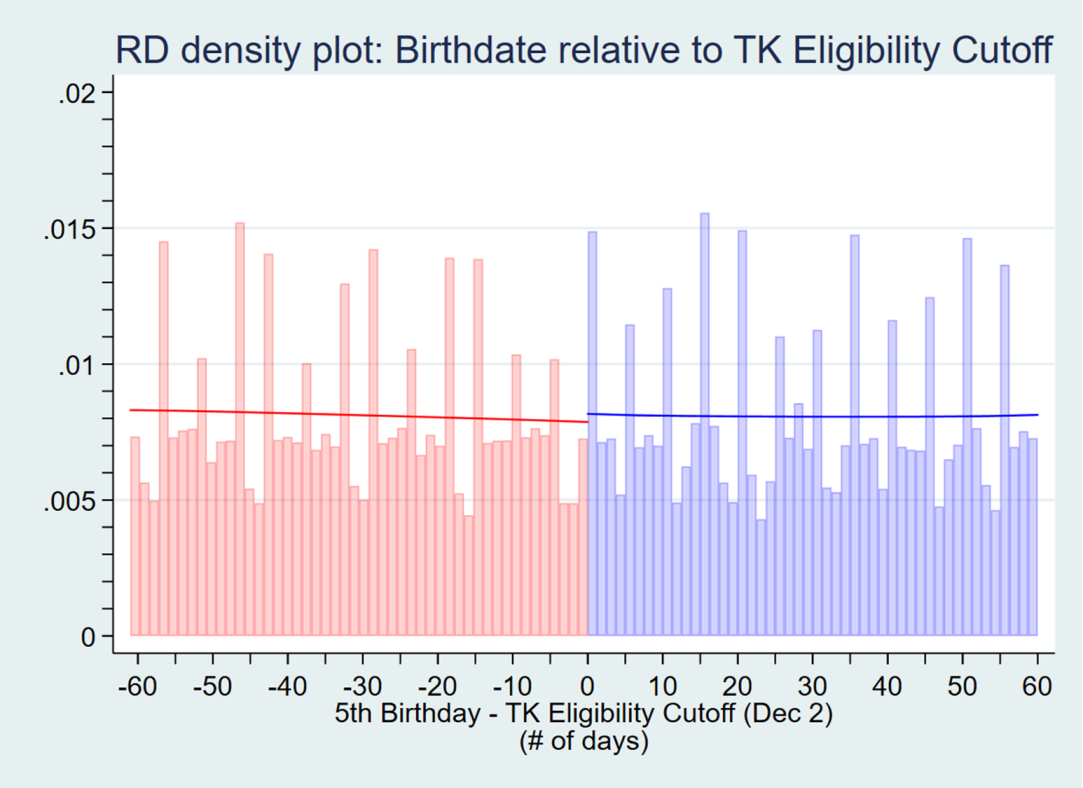
1.RD Density Tests

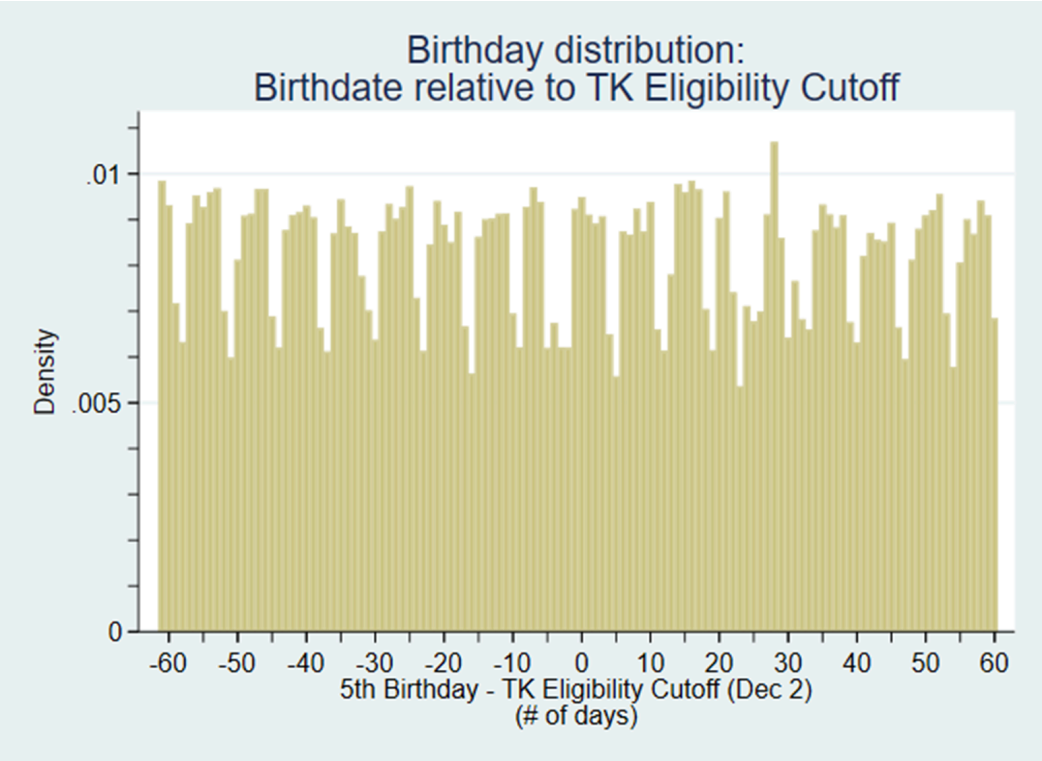
- implements manipulation testing procedures using the local polynomial density estimators proposed in Cattaneo, Jansson and Ma (2020); demonstrates there is no evidence of manipulation of birthdates, birthday distribution looks uniform

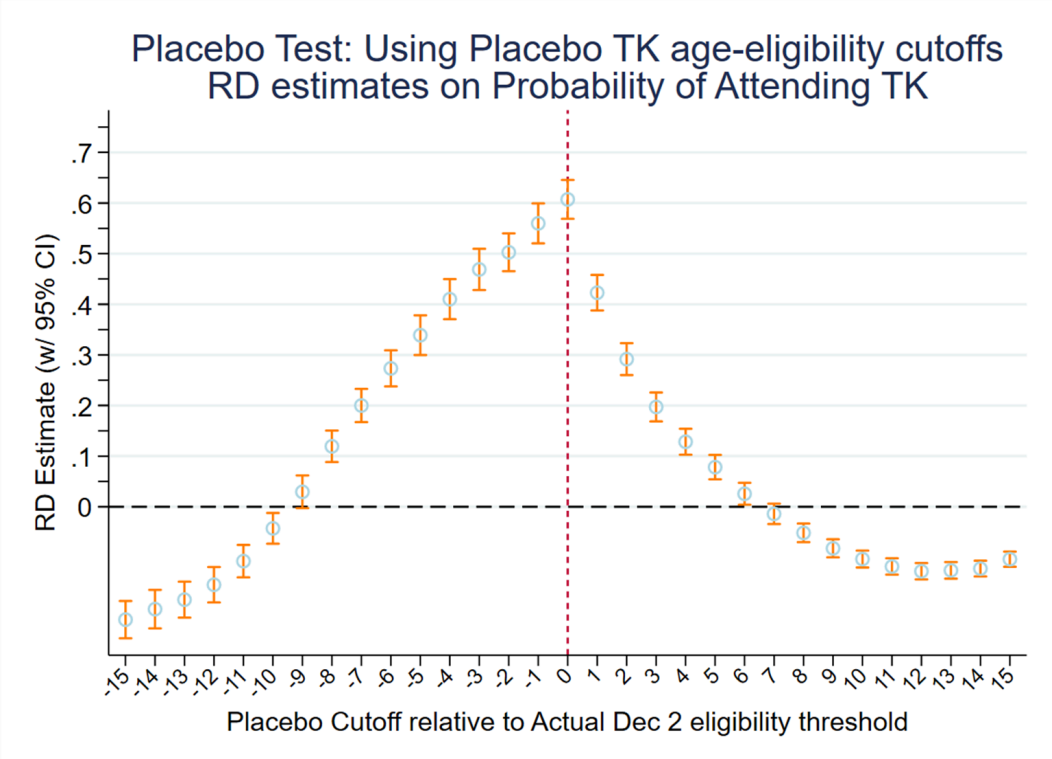
2.Tests using Placebo TK age-eligibility cutoffs
3.Robust to different bandwidths & functional form for running variable

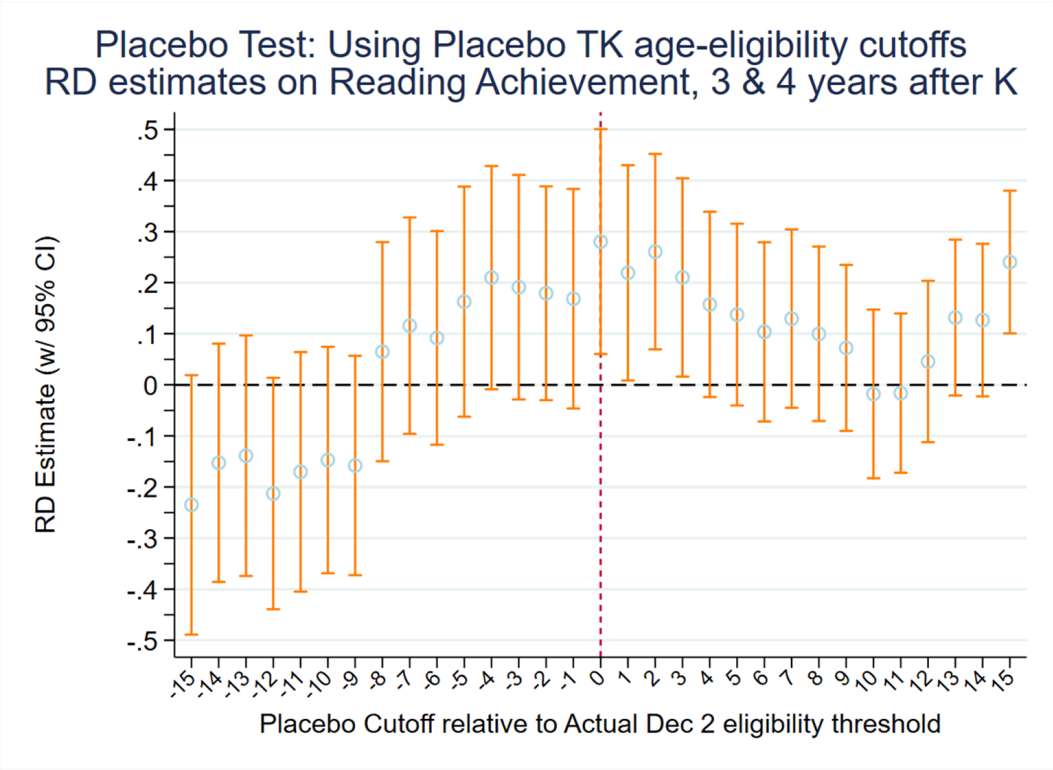
4.2SLS-IV-Regression Discontinuity (RD) Estimates, Covariate Balance Tests & Attrition Balance Tests

5.Difference-in-Regression Discontinuity design yield similar results




Birthday distribution:
Birthdate relative to TK Eligibility Cutoff
Density
.01
.005
0
-60 -50 -40 -30 -20 -10 0 10 20 30 40 50 60
5th Birthday - TK Eligibility Cutoff (Dec 2)
(# of days)


Placebo Test: Using Placebo TK age-eligibility cutoffs
RD estimates on Probability of Attending TK
RD Estimate (w/ 95% CI)
.7
.6
.5
.4
.3
.2
.1
0
-15 -14 -13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15
Placebo Cutoff relative to Actual Dec 2 eligibility threshold


Placebo Test: Using Placebo TK age-eligibility cutoffs
RD estimates on Reading Achievement, 3 & 4 years after K
RD Estimate (w/ 95% CI)
.5
.4
.3
.2
.1
0
-.1
-.2
-.3
-.4
-.5
-15 -14 -13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15
Placebo Cutoff relative to Actual Dec 2 eligibility threshold

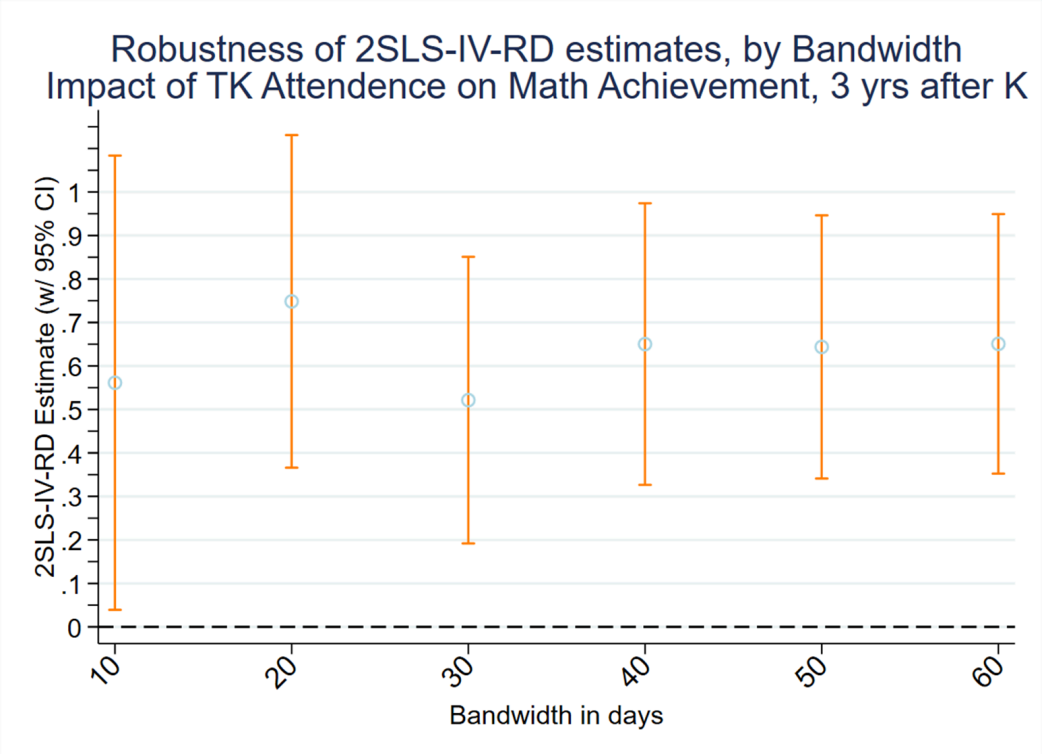

Robustness of 2SLS-IV-RD estimates, by Bandwidth
Impact of TK Attendence on Math Achievement, 3 yrs after K
2SLS-IV-RD Estimate (w/ 95% CI)
1
.9
.8
.7
.6
.5
.4
.3
.2
.1
0
10 20 30 40 50 60
Bandwidth in days

Table A4a: Auxiliary 2SLS-IV-Regression Discontinuity (RD) Estimates, Covariate Balance Tests

Sample: Children w/English-speaking parents	Dependent variable:				
	Female	White	Black	Hispanic	Non-Poor family
Independent variable (instrumented):	(1)	(2)	(3)	(4)	(5)
TK eligible	-0.0083246 (0.0110216)	0.0040869 (0.0089193)	0.004415 (0.0068167)	-0.0068325 (0.0098646)	-0.006892 (0.0109349)
TK eligible*3-yr avg per-pupil Spending$^{(t-2,t)}$	-0.0022173 (0.0043004)	-0.0020337 (0.0035584)	0.0000745 (0.0020821)	-0.0031361 (0.002984)	-0.0032191 (0.0046278)
3-yr avg per-pupil Spending$^{(t-2,t)}$	0.004348*** (0.0009215)	-0.0016689** (0.0007242)	-0.0003068 (0.0004849)	-0.0000294 (0.0006432)	0.0048337*** (0.0008149)
Student-level observations	346,187	346,187	346,187	346,187	346,187
Number of students	178,945	178,945	178,945	178,945	178,945
Number of schools	5,130	5,130	5,130	5,130	5,130
Number of school districts	812	812	812	812	812

Appendix C: *Difference-in-regression-discontinuities design (complementary alternate design)*

California's public elementary school districts were mandated to offer TK and it is a universal program for children who are age eligible, but it is voluntary for parents to choose to enroll their children. Thus, enrolled children in TK may differ from those whose parents choose other options. The endogeneity of TK enrollment and the differences in children's age by design must be taken into account or will likely lead to biased results. As an alternative, and complementary research design to the primary one reported in the paper, we also utilize a difference-in-regression discontinuities (Diff-in-RD) design. This Diff-in-RD design combines two distinct sources of variation to circumvent potential endogeneity challenges: the timing of the introduction of the TK program and the age eligibility rules (using earliest birthdate cutoff). The introduction of TK provides variation for before-and-after comparisons (as in difference designs or fixed effect models) and the age cutoff allows one to compare outcomes for children who are just a little too old to be TK eligible with those children who are just young enough to be eligible (as in a regression discontinuity design—e.g., for 2008 birth cohorts, the October 2nd cutoff (5th birthday) is the relevant TK age-eligibility cutoff exploited for this strategy). The panel data is leveraged by using a Diff-in-RD, pairing/embedding the age eligibility cutoff RDD within a difference-in-differences design. This alternate, complementary research design identifies the causal impacts of TK access as the difference between the estimated effects on subsequent student achievement of an RDD around the age eligibility cutoff in the year after TK was introduced relative to an RDD around the age cutoff before TK existed. This approach yields estimates of the causal effect of TK availability (or the intent-to-treat effect of TK access) compared to the prior mix of preschool services or homecare (i.e., the pre-existing mix includes CSPP, Head Start, private preschool, and homecare). We embed this within a 2SLS-IV framework, to account for the endogeneity of enrollment decisions, to identify the causal impacts of TK attendance.

In addition to the health and developmental risk levels of children that enrolled, the effect of TK will depend on the local availability and quality of preschool services that those enrolled children would have experienced if they had not attended TK.

Both empirical approaches (2SLS-RD; 2SLS-Diff-in-RD) leverage the treatment contrast between children just below and just above the TK birthdate eligibility cutoff in the second year of TK implementation.

Diff-in-RD Design

Table C13a - Treatment Contrasts at TK Birthdate Eligibility Cutoffs in Various School Years

	2010-11		**2011-12**		**2013-14**	
	Child A	**Child B**	**Child C**	**Child D**	**Child E**	**Child F**
Birthdate	*12/2/2005*	*12/3/2005*	*12/2/2006*	*12/3/2006*	*10/1/2008*	*10/2/2008*
Age Relative to Others in Grade	Youngest	Oldest	Youngest	Oldest	Youngest	Oldest
Grade at Age 4					-	-
at Age 5	K	-	K	-	K	TK
at Age 6	1	K	1	K	1	K
Student Achievement Observed in Sample (2014-2019):						
3 years after K	*no*	*yes*	*yes*	*yes*	*yes*	*yes*
4 years after K	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
					→ ***TK introduced***	
					2012-13	**2013-14 2014-15**

Primary RD Design

Table C13b - Treatment Contrasts at TK Birthdate Eligibility Cutoffs in Various School Years

	2011-12		**2012-13**		**2013-14**	
	Child A	**Child B**	**Child C**	**Child D**	**Child E**	**Child F**
Birthdate	*12/3/2005*	*1/31/2006*	*12/3/2006*	*1/31/2007*	*12/2/2008*	*12/3/2008*
Age Relative to Others in Grade	Oldest	Older	Oldest	Older	Oldest	Oldest
Grade at Age 4					-	-
at Age 5	-	-	-	-	TK	-
at Age 6	K	K	K	K	K	K
Student Achievement Observed in Sample (2014-2019):						
3 years after K	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
4 years after K	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
					→ ***TK introduced***	
					2012-13	**2013-14 2014-15**

Prior to the introduction of TK, students in California had to be 5 years old by December 2nd to attend Kindergarten in the public school system. This creates a birthdate cutoff where children born on or before December 2nd in a given year were assigned to a different kindergarten cohort than children born after December 2nd. Appendix Table C13a shows how the schooling experiences of these children differed in the years surrounding the introduction of TK. In the Appendix Table A13a, TK denotes that a child is *eligible* to attend TK. In the 2013-14 school year, the year after TK was first introduced, Child F in Appendix Table 13a (born October 2, 2008) just meets the birthdate cutoff and therefore is eligible to attend Transitional Kindergarten in 2013-14 and is part of the 2014-15 kindergarten cohort. On the other hand, Child E in Appendix Table A13a (born October 1, 2008) misses the birthdate cutoff and is therefore ineligible for TK and is part of the 2013-14 kindergarten cohort. This means that Child E and Child F differ in their access to TK. But they also differ in relative age to their classmates: Child E is the youngest in her cohort and Child F is the oldest in her cohort (even though they were born just a day apart).

This difference is problematic for a standard RD identification strategy to the extent that being young for grade affects later achievement and developmental outcomes. Dobkin and Ferreira (2010) find that students who are young relative to their peers attain slightly higher levels of education, but do not perform as well academically, but they find that these differences do not impact later labor market outcomes. Relatedly, Evans et al (2010) find causal evidence that being young for grade (relative immaturity) makes children more susceptible to being misdiagnosed for ADHD.

Except for the years after TK was introduced, there is no policy contrast at the Kindergarten birthdate cutoff. In 2011-12, prior to the introduction of TK, Child C in Appendix Table C13a (born December 2nd , 2006) meets the December 2nd cutoff and attends Kindergarten in 2011-12 as the youngest in her grade. Child D in Appendix Table C13a (born December 3rd, 2006) just misses the birthdate cutoff and so she does not attend Kindergarten in 2011-12, but attends in 2012-13 as the oldest in her grade.

Critically, Child C and D are nearly exactly the same age but differ in their relative ages with their classroom peers, but *neither* are eligible for TK because it is not implemented until they are too old. Similarly, children with the same birthday from earlier cohorts never experienced TK eligibility because it had not been available the prior years. Child A and B in Appendix Table C13a (born December 2nd and

3rd, 2005) differ in their relative ages of their classroom peers, but both are ineligible for TK. We leverage the lack of policy contrast in these others years that predate TK's introduction to test whether discontinuities in subsequent student achievement in elementary school at the Kindergarten birthdate cutoff in the year after TK was implemented are a result of TK or relative age differences to their classmates.

This design explicitly incorporates any student achievement outcome discontinuity at the eligibility threshold in the prior years, when there was no TK policy contrast, by measuring the outcome discontinuity in the year after TK was first introduced relative to the discontinuity in the years before TK was implemented. Therefore, this approach accounts for differences between the oldest and youngest students (relative to their grade) that are not related to TK eligibility.

The Diff-in-RD approach may address concerns of bias from other confounding policies that both use the same age-eligibility cutoff as TK, and potentially affect early childhood development. Graphical analyses and RD estimations using the student-level data in the years before TK was introduced (below) confirm that there are pre-existing discontinuities at the earliest kindergarten age cutoff in achievement outcomes before the TK expansion. Indeed, the conception of TK was in part motivated by a recognition that children who turned 5 late in the calendar year often entered kindergarten young for grade and their achievement outcomes often lagged behind their peers. When using the earliest age cutoff, the RDD does not allow one to disentangle the true effects of TK, impacts of child's age relative to classmates, and confounding policies. To address these endogeneity issues, we combine the RDD approaches in both pre- and post-TK rollout periods and utilize a difference-in-regression-discontinuities design (Diff-in-RD). The Diff-in-RD approach accounts for both the potential relative pre-existing differences at the cutoff of the standard RDD and for heterogeneous development trends by age.

The two key identifying assumption of this Diff-in-RD approach are 1) that potential outcomes are continuous in age at age-eligibility cutoffs; and 2) without the introduction TK, the effect of children's age relative to classmates or other coincident policies on children at the cutoffs are the same before and after the rollout of TK. Evidence supporting this assumption is represented by the fact that achievement outcomes show a similar trend around the cutoff in the years prior to the introduction of TK (i.e., for children on either side of the cutoff assigned to adjacent kindergarten cohorts). The

Diff-in-RD approach allows for the existence of differences before the TK rollout, and imposes a "common trend" assumption that such difference would remain the same if TK had not been introduced, but only for the children around the cutoffs.

The primary results are based on models using children born in a 120-day window (60-day bandwidth) around the age-eligibility cutoff.[25]

[25] As with the primary 2SLS-IV-RD design, another possible concern with the Diff-in-RD approach is the uncertainty in the choice of the underlying functional form. The running variable – children's date of birth relative to the age eligibility cutoff – is discrete. I checked whether the differences in socioeconomic demographic composition of our samples around the cutoff are continuous before and after the introduction of TK. We implement the same Diff-in-RD estimations but with socioeconomic demographic background variables such as low-income, gender and race as outcome variables. Overall, the difference patterns in socioeconomic and demographic backgrounds around the cut-off are quite stable during the TK rollout, suggesting that the results are not driven by changes in the characteristics of the population of students entering kindergarten in California public schools. I then control for the socioeconomic and demographic background variables in the Diff-in-RD estimation as covariates and get very similar results.