The Long-Term Effects of Universal Free School Meal Policies: Evidence from the Community Eligibility Provision*

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Abstract

School meal policies in the United States are at a crossroads: half of public K–12 schools provide universal free meals to all students, regardless of income, while the other half offer free and reduced-price (FRP) meals based on income eligibility. Do universal policies improve student outcomes compared to targeted policies for low-income children? I evaluate the short- and long-term effects of universal free school meal policies using linked administrative data from Texas. I exploit the staggered rollout of the Community Eligibility Provision (CEP), a federal policy that allows high-poverty schools to offer meals free to all students. I find that CEP increases meal take-up by 6.2 percentage points (10 percent). Higher-income students previously ineligible for FRP are more responsive to CEP than low-income students already eligible under FRP. Despite the increase in take-up, I detect no improvements in academic, behavioral, or economic outcomes overall or across most subgroups, in either the short or long run. Applying the marginal value of public funds framework, I show that the costs of CEP exceed its benefits, suggesting limited efficiency gains from universal provision relative to targeted support.

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1 Introduction

School meals are a key source of child nutrition in the United States. In 2023, federally subsidized meals reached 30 million children with a $28 billion budget (USDA 2025). This paper evaluates the effects of the Community Eligibility Provision (CEP), a federal universal free school meal policy introduced over a decade ago to reduce the stigma associated with school meals and child food insecurity.[1] Unlike traditional means-tested free and reduced-price (FRP) meal programs that impose income eligibility on students, schools adopting CEP automatically enroll all students for free breakfast and lunch regardless of income. To date, more than half of public schools have adopted CEP (Hysom and Bylander 2025). Nine states have passed legislation to expand CEP statewide and over 20 states are considering similar proposals.

Despite its popularity, whether CEP improves student outcomes compared to FRP remains an open question. Proponents argue that CEP, relative to FRP, increases take-up by reducing stigma and administrative burdens, and improves academic outcomes as a result.[2] Critics, however, raise concerns about fiscal sustainability and targeting efficiency. Much of this controversy is due to lack of convincing evidence of the benefits and costs of CEP.

To fill this gap, I study the short- and long-term effects of CEP relative to FRP on student outcomes using staggered difference-in-difference (DiD) designs that exploit the rollout of CEP. I leverage linked administrative data from Texas that track public school students through college and into the workforce. My estimates show that the availability of universal free school meals increases meal take-up among both low- and higher-income students. Despite the increase in take-up, CEP has little effect on test scores, absences, suspensions, SAT scores, high school graduation, or college enrollment, either overall or by subgroup. These findings suggest that expanding free school meals to all students relative to FRP yields limited measurable benefits relative to FRP.

The key contribution of this paper is to quantify the effects of CEP using rich administrative data in both the short and long run. Prior literature focuses almost exclusively on grade 3–8 test scores and suspensions, finding that CEP increases take-up, raises test scores, and reduces suspension rates. My findings suggest a similar

1. According to the USDA, 7.2 million children were food insecure in 2023—meaning they lacked consistent access to sufficient, nutritious, and safe food.

2. As Governor Kathy Hochul of New York stated "good food in the lunchroom creates good grades in the classroom." Similarly, Governor Tim Walz of Minnesota said that universal free meals "make a huge difference in the moment for those students, and we know in the long run it'll make a difference in achievement and the well-being of those students."

increase in take-up. In contrast to prior studies, I find no robust positive effects of CEP, on average or across most subgroups, in either the short or long run.

I begin by estimating the reduced-form effects of CEP on student short-term outcomes using a DiD design based on the program's rollout to 1,800 schools between 2012 and 2018 (Callaway and Sant'Anna 2021). I use two complementary methods to find valid control schools. One specification compares schools that adopted CEP earlier and those that adopted it later. The second specification creates a control group with similar pretreatment characteristics using a doubly robust estimator that reweights never treated schools by their inverse propensity to adopt CEP and models outcome differences. Both approaches yield similar results.

My findings suggest that CEP increases take-up but has little effect on test scores, attendance, or suspension rates. CEP increases take-up by 6.2 percentage points (10 percent). It increases participation among both low-income students who were previously eligible for FRP meals and higher-income students who were not. However, I find no evidence of effects on short-term academic or behavioral outcomes, with the exception of pre-K students. CEP improves pre-K children's readiness scores by 2–4 percent but has no effects on older students. My intent-to-treat (ITT) estimate for grade 3–8 test score index is statistically insignificant (estimate = 0.001, SE = 0.008) and economically small, equivalent to ±$30 on earnings at age 25.[3] To explore the possibility that the full sample masks the subset of students responding to CEP, subgroup analyses by various student and school characteristics show no statistically significant positive effects.

I next estimate CEP's long-term effects on student educational and economic outcomes. To the best of my knowledge, no prior work has examined these long-term effects, largely because of two key challenges. First, U.S. school meal programs saw few reforms prior to the introduction of CEP. Second, few datasets track students beyond the K–12 system. I address these challenges by exploiting the rollout of CEP across Texas school districts between 2012 and 2023 and by linking administrative records from Texas K–12 education to college and labor market outcomes.

I use event study designs to estimate the effects of CEP exposure on adult outcomes across birth cohorts within school districts. Specifically, I compare outcomes between "exposed" cohorts (students who were young enough to be in school during or

3. Assuming CEP affects test scores only through meal take-up, scaling by the take-up rate suggests that $1,000 per student per year in school meal spending is associated with an insignificant 0.001 standard deviation increase in the test score index. This magnitude is economically small: based on Chetty, Friedman, and Rockoff (2014), the 95% confidence interval implies an effect on age-25 earnings of about ± $30.

after CEP adoption), and "unexposed cohorts" (students who had already graduated at the time of CEP adoption) within the same district. Furthermore, I use districts that adopted CEP later to account for any underlying differences across birth cohorts. The students were between the ages of 8 and 17 and were followed through 2023.

Mirroring the largely null short-term effects on students, I find that CEP has little impact on a broad set of long-term outcomes, including SAT scores, high school dropout rates, high school graduation, and college enrollment. I can also rule out improvements in earnings at age 21 greater than roughly 4–6 percent for cohorts exposed during middle or high school. The absence of statistically significant positive long-term effects is unsurprising, given that most children in need are already covered by FRP meals, and participation rates are high among low-income students. The availability of universal free school meals, on top of CEP, induces only a modest increase in meal take-up. This is also consistent with experimental evidence showing that the availability of universal breakfast primarily shifts meal consumption from home to school without improving short-term nutrition, achievement, or behavior (Bernstein et al. 2004; Schanzenbach and Zaki 2014).

A key identifying assumption of the long-term outcome research design is that CEP is the sole factor influencing the relationship between a student's exposure to CEP and her adult outcomes. The event study supports the validity of this approach by showing no pre-trends, but concerns remain that contemporaneous shocks can affect treatment and control groups differently. A threat to identification is that Texas lowered high school graduation requirements for all districts in 2014. To address this, I estimate specifications with matched control groups and restrict the sample to cohorts fully exposed to the reform, all of which point to no robust positive effects.

This paper makes three main contributions. First, it advances the literature on the long-term effects of school meal programs by leveraging rich administrative data. It builds on two prior studies of school meal programs introduced in the mid-20th century (Lundborg, Rooth, and Alex-Petersen 2022; Hinrichs 2010), both of which find positive long-term effects. In contrast, I do not detect statistically significant long-term impacts of CEP on students. This could be due to substantial differences between the historical programs and CEP in terms of eligibility criteria, nutritional standards, and counterfactual policies. Specifically, those studies estimate the impact of introducing government-funded meals in Sweden and the US, where no such provision previously existed, whereas this paper analyzes the effect of expanding access from low-income students to all students.

Second, this paper examines the effects of CEP on previously unstudied short-term outcomes. I examine academic and behavioral outcomes from pre-K through high school, while the prior literature has heavily focused on grades 3 to 8 test scores and finds mixed results. My null findings on test scores are consistent with evidence from a large-scale, federally sponsored randomized controlled trial (Bernstein et al. 2004; Schanzenbach and Zaki 2014). My confidence interval can rule out the large positive effects reported in Gordanier et al. (2020), while estimates from Ruffini (2022) and Schwartz and Rothbart (2020) remain within the upper bound of my confidence interval. Differences in specifications, treatment definitions, data sources, and lengths of follow-up periods could have contributed to my differences with prior studies. The test score estimates in the prior literature are sensitive to specifications. The magnitudes of the positive effects in the existing literature are at least 10 to 30 times larger than Page (2024) finds for the effects of common income transfer and education policies on test scores.[4]

Third, this paper adds to the literature on the long-run effects of childhood access to safety net programs. A substantial body of research shows that exposure to such programs in early childhood has large, positive effects on adult outcomes, but much smaller or no effects for children exposed at older ages. This includes evidence on SNAP (Hoynes, Schanzenbach, and Almond 2016; Bailey et al. 2024), Medicaid (Goodman-Bacon 2021b), and housing assistance (Chetty, Hendren, and Katz 2016).[5] Relative to these means-tested programs, school meal benefits provided through CEP are smaller in magnitude but universal in eligibility. Nevertheless, my results broadly align with this earlier literature, showing that access to safety net programs for children aged 8–17 has little discernible long-term effects.

Findings in this paper inform debates over universal free school meal policies. Expanding access to free meals increases take-up across income groups. In particular, the increase in take-up among low-income students previously eligible for FRP suggests that CEP reduces stigma and administrative burdens. Yet these benefits come at a cost: CEP increases program costs without significant improvements in measurable academic or behavioral outcomes. To evaluate the cost-effectiveness of CEP, I estimate the marginal value of public funds (MVPF; Hendren and Sprung-Keyser (2020)). The MVPF measures the "bang for the buck" of government spending. It is calculated as

4. See more discussions of magnitudes and differences between this paper and other test score estimates in Section 6.

5. While this general pattern holds, some studies detect positive effects among older children. For example, Bastian and Michelmore (2018) find that EITC exposure between ages 13 and 18 has large positive effects on educational attainment and earnings. Similarly, Pollakowski et al. (2022) find that participation in public housing during the teenage years (ages 13–18) increases earnings at age 26 by 3 to 6 percent.

the share of recipients' willingness to pay to its net cost to government. CEP's MVPF is around 0.4, which is lower than other federal nutrition assistance programs and K-12 spending policy.[6] My results quantify the trade-offs between modest benefits and higher program costs. Policymakers should carefully weigh these trade-offs when considering the expansion of universal free school meal policies.

The paper proceeds as follows. Section 2 provides background of school meal policies and a conceptual framework to evaluate the effects of CEP on student outcomes. In Section 3, I describe the data and sample construction. Section 4 outlines the empirical strategies. Section 5 presents the empirical results. Section 6 discusses magnitude and its relationship to existing literature. Section 7 conducts a welfare analysis within the MVPF framework and Section 8 concludes.

2 Background on CEP and Expected Effects

2.1 Background of Means-tested and Universal Free School Meal Policies

The National School Lunch Program (NSLP) and School Breakfast Program (SBP) provide FRP meals to low-income children in the United States. These programs are administered by the USDA. In 2024, about 30 million children participated in NSLP and 15 million in SBP, with federal spending totaling $24 billion (USDA 2025).

Eligibility for FRP is based on family income. Children whose family income is at or below 130 percent of the federal poverty line qualify for free meals, while those with income between 130 and 185 percent of the poverty line are eligible for reduced-price meals and pay a small fee (130 percent is $40,000 for a family of four; 185 percent is $58,000). About half of schoolchildren qualify for FRP based on family income. The cash value of free meals for a school year is about $1,200 per student. Students from families receiving public assistance such as SNAP are automatically enrolled in FRP, while others need to apply annually.

CEP was created by the 2010 Healthy, Hunger-Free Kids Act. It started in a few states in 2011 and went nationwide (including Texas) in 2014. Students enrolled in schools that have adopted CEP are automatically enrolled in free breakfast and lunch, regardless of income. As indicated in Appendix Figure A1a, meal prices remain largely

6. Depending on assumptions about how families value universal meals, the MVPF ranges from 0.2 to 1.1. CEP affects all students, and its MVPF likely varies by student family income. Hendren and Sprung-Keyser (2020) find that direct investments in the health and education of low-income children have high, if not infinite, MVPFs. However, high MVPFs should not set the benchmark for whether a policy is a good or bad idea. It is also not particularly meaningful to compare MVPFs when the target population differs. Nevertheless, the MVPF quantifies the tradeoffs associated with spending in different programs. It is up to policymakers to decide which expenditures are worthwhile.

unchanged for students eligible for FRP under CEP. However, for higher-income students who were not previously eligible, CEP reduces meal prices by roughly $3.00 per lunch and $1.40 per breakfast. In addition to CEP, there are some earlier universal free school meal policies that allow schools to offer universal meals, such as Provisions 2. Provision 2 operates similarly to CEP, except that these schools collect meal applications in a base year, and track meal take up by FRP eligibility in the first and only first year. In this paper, I do not distinguish between Provision 2 and CEP since these policies operate almost the same way for students.

School meal participation rates in Texas in vary by income, meal type, and school level. Appendix Figure A1b shows that in school year 2011-2019 (throughout the paper, I label school years by their start year), students eligible for FRP had higher lunch participation (over 70 percent) than ineligible students (46 percent). Participation in breakfast was lower compared to lunch. The take-up in Texas is similar to the rest of the US (USDA 2019).

CEP targets higher poverty schools. Eligibility is based on a measure called the Identified Student Percentage (ISP), defined as the share of students from families participating in public assistance programs. Schools with an ISP of at least 40% are eligible for CEP, but participation is also available for groups of schools within a district and entire school districts, provided that the aggregated ISP meets the eligibility threshold. The USDA lowered the threshold to 25% in October 2023. Appendix figure A2 indicate that nine states have expanded CEP statewide with state funding. All schools are eligible to participate in these states. In Texas, about 6,000 schools (60%) are eligible for CEP.

2.2 Rollout of CEP in Texas

I focus on the rollout of universal school meal policies between 2012 and 2023 in Texas. Prior to 2012, approximately 600 schools had already adopted earlier federal universal meal provisions, primarily Provision 2 during the late 1990s and early 2000s. Because the precise adoption years for these schools are unavailable, they are excluded from the analysis. In 2012, 52 schools adopted Provision 2, followed by more than 200 additional schools in 2013. When CEP was first introduced in Texas in 2014, over 700 schools adopted it. In cases where schools implemented universal breakfast prior to universal lunch, I define the event time based on the year of universal lunch adoption. By 2023, about 60% of eligible schools had adopted CEP. CEP participation decisions are made at the district level, although a subset of schools within a district can participate individually or as a group. Most districts in Texas participate district-wide. Appendix

Figures A3a and A3b show the cumulative number and share of schools adopting CEP from 2014 to 2024 in the United States and Texas.

What determines the timing of CEP adoption across schools? A USDA survey found that concerns about CEP's financial impacts were the largest barriers to participation among otherwise eligible schools and districts (Logan et al. 2014). The federal reimbursement formula provides strong incentives for higher-poverty schools to participate, and early adopters were generally poorer than later adopters. However, schools with ISPs of at least 62.5% receive the same reimbursement rate, so differences in timing cannot be attributed to financial incentives alone.[7] Adoption is also shaped by idiosyncratic factors, such as how effectively a district matches students to SNAP/TANF databases, which directly influences its ISP and reimbursement rates. When federal subsidies do not fully cover program costs, districts must cover the remaining expenses.

2.3 Potential Impact on Short- and Long-Term Outcomes

CEP unambiguously increases meal take-up by eliminating price, paperwork, and stigma.[8] CEP and related universal programs increase participation by about 5 to 18 percentage points, with larger gains for breakfast than for lunch and larger increases in lower-poverty schools and among higher-income students. The increase in take-up among higher-income students is about twice as much as among low-income students (Bernstein et al. 2004; Schwartz and Rothbart 2020).

However, whether higher participation translates into improvements in student nutrition, academic outcomes, and behavior is ex ante ambiguous, as it depends on the relevant counterfactuals. Because universal free school meals affect a diverse set of students, these counterfactuals likely vary by family income. Low-income students already have access to free meals through FRP, while higher-income students typically have access to quality meals at home or can afford to purchase school meals. Thus, CEP is likely to have the largest effect on students just above the FRP income eligibility cutoff. Ultimately, this is an empirical question.

7. The federal government sets a per-meal reimbursement rate by free, reduced, and paid categories. For example, in school year 2025–2026, the per-lunch subsidy is \$4.77, \$4.37, and \$0.52 for the free, reduced, and paid category. The ISP is multiplied by a factor of 1.6 to determine the total percentage of meals reimbursed at the federal free rate; the remaining meals (up to 100 percent) are reimbursed at the federal paid rate. No reduced-price meals are claimed by CEP schools. For example, if a school's ISP is 50%, then $50\% \times 1.6 = 80\%$ of meals are reimbursed at the federal free rate, and the remaining 20% are reimbursed at the federal paid rate. For schools with an ISP of at least 62.5%, all meals are reimbursed at the federal FREE rate.

8. CEP raises take-up among low-income students (by removing application frictions, reducing stigma, and ensuring consistent access) and higher-income students (by eliminating price).

CEP may influence student outcomes through both nutritional and non-nutritional channels. For the nutrition channel, despite higher participation, estimated effects on nutrition are modest or null. A randomized evaluation shows treated students are about 4 percent more likely to consume a nutritionally substantive breakfast, but there is no detectable impact on 24-hour nutrient intake; instead, students substitute breakfast at school for breakfast at home (Bernstein et al. 2004; Schanzenbach and Zaki 2014). Similarly, Au et al. (2025) find that access to FRP meals reduces caloric intake but does not affect food insecurity.

CEP may also operate through non-nutritional mechanisms such as reduced stigma, parental savings of time and money, and district budget changes via federal reimbursements. CEP lowers household expenditures by about $214 per year for families with children (Marcus and Yewell 2022; Handbury and Moshary 2021) and increases annual federal subsidies by roughly $72 per student (Rothbart, Schwartz, and Gutierrez 2023). No study yet finds positive attendance effects, though some report reductions in suspensions (Gordon and Ruffini 2021; Domina et al. 2024). Evidence on body mass is mixed: Davis, Kreisman, and Musaddiq (2024) report increases in BMI, whereas Rothbart, Schwartz, and Gutierrez (2023) find no effects overall (apart from secondary students).

The effect of CEP on student achievement is of particular interest. Several studies examine test scores up to three years after schools adopt universal school meal policies and find mixed results. Bernstein et al. (2004) and Schanzenbach and Zaki (2014) find no effects of universal breakfast on elementary school test scores, either overall or within subgroups with experimental data. Leos-Urbel et al. (2013) also find null effects of universal free breakfast on test scores in New York City school districts. Frisvold (2015) finds availability of the program increases student achievement. Several recent quasi-experimental studies estimate that CEP increases test scores by 0.02–0.08 standard deviations for some subgroups (Gordanier et al. 2020; Schwartz and Rothbart 2020; Ruffini 2022). However, there is no consistent pattern of significant positive effects by any subgroup.

Long-Run Effects

The expected long-run effects of CEP on student outcomes are ambiguous. A large literature shows that investments made before age five yield sizable long-run returns, whereas the effects for older children are more mixed (see (Currie and Almond 2011)). For example, Hoynes, Schanzenbach, and Almond (2016) and Bailey et al. (2024) find that access to Food Stamps before age five significantly improves adult health, educational attainment, and economic outcomes, with much smaller—and often statistically insignificant—effects for older children.

Food Stamps and school meals are both federal nutrition assistance programs but they differ in three important ways: (i) school meals serve school-age children (approximately ages 5–18); (ii) the value of school meal benefits is about half that of Food Stamps; and (iii) CEP provides relatively greater benefits to higher-income students, since low-income students were already covered by means-tested FRP meals.[9] Hence, the effects of CEP on child outcomes, if anything, are expected to be smaller than those found for Food Stamps.

Two prior studies of school meal programs find positive effects on student long-term outcomes. Lundborg, Rooth, and Alex-Petersen (2022) find that Swedish universal free school meals in the 1950s-1960s increase lifetime earnings by three percent, and have positive effects on education attainment and health. Hinrichs (2010) shows that expansions of NSLP in the 1960s have large positive effects on schooling but not on health with survey data. Unlike those settings, which introduced publicly funded school meals where none existed, CEP expands access from low-income students to all students. Eligibility rules and nutritional standards under CEP also differ significantly.[10]

Taken together, prior evidence suggests that CEP increases school meal take-up but is likely to yield at most modest improvements in nutritional and academic outcomes, particularly relative to larger transfers such as Food Stamps and to historical settings where universal school meals were newly introduced.

3 Data and Sample

I draw information from two main data sources: the Texas Department of Agriculture (TDA) and the University of Texas at Dallas Education Research Center (ERC). I first describe the two data sources and outcome measures, followed by the construction of the analysis samples for short-term and long-term outcomes. Finally, I provide descriptive statistics for the analysis samples.

9. About half of K–12 students were income-eligible for FRP meals in 2012, according to the NCES.

10. See detailed discussion of the differences betwen Swedish and US school meal programs in (Lundborg, Rooth, and Alex-Petersen 2022) page 879.

3.1 CEP Participation and School Meal Take-Up

The TDA provides data on school meals at the school-month level from 2011 through 2024. The dataset includes detailed information on FRP eligibility, CEP and Provision 2 participation, meal take-up rates, federal subsidies (inflated to 2023 dollars using the CPI-U), and meal prices—all disaggregated by meal type (breakfast and lunch) and student FRP eligibility category (free, reduced, and paid).

Meal take-up is measured as the average daily participation rate, calculated as the total number of meals served during the year divided by the product of school enrollment and the number of school days. This rate reflects the share of students in the entire school who eat school meals on a typical school day. In the subset of Provision 2 schools where I observe take-up by FRP eligibility based on student income, I calculate take-up rates separately by student eligibility category.

In addition, I use district-level school meal application data from the USDA Food and Nutrition Service. These data report the number of students eligible for FRP through direct certification and through applications each year at the district level. I use this to measure administrative burden.

3.1.1 Short-Term Student Outcomes

The short-term student outcomes come from administrative PK–12 records and include early childhood assessment scores, end-of-grade test scores, attendance, coursework, disciplinary records, and demographic information. The data are collected by the Texas Education Agency (TEA) and hosted at the ERC. I have access to records spanning 1995 to 2023, although much of my analysis of short-term outcomes focuses on the period from 2011-2018. Early childhood assessment data is only available from 2018-2023. For ease of computation, I collapse the data to school-year-grade level.

Early childhood assessment: Early childhood assessment scores are available for pre-kindergarten (4-year-olds) and kindergarten (5-year-olds). Pre-K assessments are administered at both the beginning and end of the school year and measure proficiency across five domains: emergent literacy reading, emergent literacy writing, mathematics, language and communication, and health and wellness. Kindergarten assessments include all of the above except health and wellness. Districts may select from TEA-approved assessment instruments, and each tool includes a readiness or proficiency flag per domain to indicate whether a student meets benchmark criteria. I use the flags as the primary outcomes.

Grade 3–8 Test Scores: Students in grades 3 through 8 are assessed annually in reading and mathematics. Test scores are standardized within grade and subject to have a mean of zero and a standard deviation of one. I also create a test score index that is an equally weighted average of math and reading standardized scores.[11]

High school academic outcomes: Course grades are available for most high school students in English I, English II (both of which include reading and writing), Algebra I, Biology, and U.S. History. These five courses are required for all students and correspond to the five STAAR End-of-Course exams that must be passed for graduation. Course credits reflect cumulative credits earned across all high school coursework.

Absence: Attendance is taken from the six 6-week attendance files collected for all grades throughout the school year. Absence is measured as the total number of days absent during a school year.

Suspension rate: The disciplinary record includes both in-school and out-of-school suspensions for all grade levels. I measure the suspension rate as the number of any suspensions per 100 students.

Demographics: The dataset contains demographic information on age, gender, race, ethnicity, FRP eligibility, and indicators for receiving special education or being an English language learner.[12]

In addition to the above data, I use nationwide CEP participation data from the National Center for Education Statistics (NCES) and the Census Bureau's Annual Survey of School System Finances for the 2013–2014 through 2018–2019 school years to conduct internal and external validity checks.

3.1.2 Long-Term Student Outcomes

I link the PK-12 records to college enrollment data from the Texas Higher Education Coordinating Board (THECB) and the National Student Clearinghouse (NSC), as well as quarterly wage data from the Texas Workforce Commission (TWC) using a unique person identifier.

SAT scores: These are raw scores obtained from the Texas college application file, which includes students who submitted applications to Texas public four-year institutions. Therefore, I only observe SAT scores for students who applied to at least one four-year public college in Texas. Students typically take the SAT during the

11. Texas implemented the State of Texas Assessments of Academic Readiness (STAAR) beginning in school year 2011–2012. Prior to STAAR, Texas administered the Texas Assessment of Knowledge and Skills (TAKS), which was used from 2003 to 2011 and covered grades 3 through 11.

12. Texas continues to collect student socioeconomic status (SES) information corresponding to FRP eligiiblity for accountability purposes after CEP implementation, but this data is collected outside the child nutrition department.

spring of their junior year (11th grade) or the fall of their senior year (12th grade) in high school.[13]

High school dropout: It is an indicator of whether a student ever drops out of high school.

On-time high school graduation: The primary outcome is an on-time measure of high school graduation, based on the year a student first enrolled in grade. For example, a student who entered first grade in fall 2010 would be expected to graduate from 12th grade on time in spring 2022. As a robustness check, I also construct an ever-high school graduation measure which does not impose a time frame when the outcome is observed.

On-time college enrollment: The primary outcome is an on-time enrollment indicator for attendance at any college. I also construct additional outcomes based on the type of college attended (two-year or four-year, public or private). The THECB dataset contains enrollment and completion details for students at two- or four-year Texas institutions, while the NSC covers out-of-state enrollment and completion for Texas high school graduates.

Earnings and employment: I construct annual earnings from quarterly earnings records in Texas. I do not observe earnings outside the state. If no earnings are observed, I impute the individual has zero earnings. Employment is defined as an indicator equal to one if annual earnings are positive. All earnings are adjusted to 2023 dollars using the CPI-U.

3.2 Analysis Sample

3.2.1 Sample for Short-Term Outcomes

My main sample period for short-term outcomes is 2011 to 2018 for several reasons. (i) 2011 is the first year with available meal participation data. Prior to 2011, around 800 schools had adopted earlier universal free school meal provisions. I reached out to USDA, TDA, and school districts but could not identify the exact adoption years for these schools. These always-treated schools are excluded from the sample. (ii) Data for school years 2019–2020 through 2021–2022 are affected by COVID, during which school meal operations changed significantly. (iii) I also exclude schools that adopted CEP in 2023 or 2024, as the USDA significantly lowered the CEP eligibility threshold in 2023, and 2023 is the last year with data for most outcomes. In robustness checks,

13. For students applied between 2020 and 2023, SAT scores were not required at most Texas public universities.

I extend the data period to 2023.

CEP creates financial incentives for high-poverty schools to adopt the policy. As a result, ever-treated and never-treated schools differ substantially. In light of this, I construct two complementary samples for analysis. My main sample for short-term outcomes includes schools that adopted CEP at some point between 2012 and 2018. These ever-treated schools are similar in observable school characteristics. I also construct an alternative sample in which treated schools are compared to eligible but untreated schools, reweighted by their estimated propensity scores. This sample includes all CEP-eligible schools (with a school- or district-level ISP of at least 40%). For both samples, I restrict the analysis to schools that remained open continuously during the data period and have no missing baseline (2011) school-level covariates.

3.2.2 Sample for Long-Term Outcomes

My main sample includes students born between 1986 and 2006. These children are old enough for me to observe their high school graduation and college enrollment by 2023, the last year for which I have data. I further restrict the sample to districts with at least 40% FRP students. Texas has 1,147 districts that remained open continuously from 2012 to 2023. Of these, 990 have a districtwide ISP at least 40%. I drop 95 districts that were treated before 2011.

Students may change school districts throughout their K–12 years. I assign each student to the district in which they were enrolled in first grade. If a first-grade enrollment record is not observed (e.g., for students who transferred into Texas later), I use the earliest observed district.

Table A2 shows that in school districts that adopted CEP, approximately 1 million students born between 1986 to 2006 were ever exposed to CEP. The number of years of exposure ranges from 1 to 10.

3.3 Summary Statistics

Table 1 presents summary statistics for schools in Texas that adopted CEP and for the full set of schools that were eligible for CEP. Compared to CEP-eligible schools, those that adopted CEP tend to be more disadvantaged, with a larger share of students eligible for free or reduced-price meals, higher shares of Hispanic students and English language learners, and a greater likelihood of being elementary and urban schools. Meal participation rates are substantially higher in CEP-adopting schools, with 47 percent of students eating breakfast and 76 percent eating lunch, compared to 35 percent and 66 percent, respectively, among all CEP-eligible schools. CEP-adopting schools also have lower test scores, higher absenteeism, and higher suspension rates.

Table 2 presents summary statistics for the analysis sample for long-term outcomes. Districts that adopted the Community Eligibility Provision (CEP) and those that were eligible but did not adopt it are generally similar in student characteristics. About 22 percent of students in districts that adopted CEP were never low-income, defined as not eligible for free or reduced-price (FRP) meals. Approximately 32 percent of students were eligible for FRP meals in every year, while 46 percent were eligible in some but not all years. Districts eligible for CEP have a larger share of students who were never eligible for FRP meals (28 percent). Among students who submitted the FAFSA, the average parents' adjusted gross income is approximately $53,016 in districts that adopted CEP, compared to $59,316 in CEP-eligible districts—slightly above the income eligibility threshold for FRP meals for a family of four (about $58,000).

4 Empirical Strategy

4.1 Strategy for Short-Term Outcomes

My goal is to estimate the reduced-form effects of CEP on student outcomes. Table 3 shows the distribution of event time, defined as the year in which a school first adopts CEP or Provision 2. CEP financially incentivizes high-poverty schools to provide free meals. As a result, high-poverty schools dominate the treatment group. Ever-CEP schools had 80 percent of their students eligible for FRP, compared to 49 percent in never-CEP schools. In response to this imbalance, my main DiD design compares average student outcomes in earlier treated schools to later-treated schools. This design exploits variation in the timing of adopting CEP. The key identifying assumption is that the average outcome among the earlier and later treated schools would have followed parallel trends in the absence of CEP.

To avoid potential negative weights in two-way fixed effects (TWFE) regression specification in staggered DiD settings, I use the estimators proposed by Callaway and Sant'Anna (2021).[14] This approach first estimates cohort- and time-specific average

14. This estimator has at least two advantages over a TWFE regression. First, it is not biased by time-varying treatment effects, because it uses only not-yet-treated comparison groups. The TWFE regression specification DiDs between treated and not-yet-treated units ("good comparisons") and DiDs between two sets of already-treated units (who began treatment at different times, "bad comparisons"). Second, it aggregates the cohort-specific treatment effect parameters by the share of treated units, while a TWFE regression weights subgroup parameters by treatment variances as well.

treatment effects on the treated schools ATT(g,t) for group g at time t, where a group is defined by the year when schools are first treated. It uses two-period two-group DiD estimators, and then aggregates them, weighting by the size of each treatment cohort to produce the overall treatment effect estimates.

I collapse individual student data to school-grade-year level and weight the estimator by the number of students in each school-year cell. For subgroup analysis by student characteristics (e.g. race, gender, FRP eligibility), I collapse the data to group level and keep a balanced school-grade-year panel. In robustness checks, I also use the estimator without weights.

Let $G_j = g \in \{2012, \ldots, 2018\}$ represent the time period when a school first adopted CEP. I use schools that eventually became treated but have not yet adopted CEP by time t as a comparison group. The cohort-time average treatment effect on the treated (ATT) is defined as, for t $\geq g$

$$ATT_{unc}^{notyet}(g,t) = \mathbb{E}[Y_t - Y_{g-1} \mid G_g = 1] - \mathbb{E}[Y_t - Y_{g-1} \mid D_t = 0,\, G_g = 0] \tag{1}$$

where g indexes the first year of treatment, t is the evaluation year, $G_g = 1$ denotes units first treated in year g, and $D_t = 0, G_g = 0$ denotes units that have not yet been treated by year t. The key identification assumption is that for each treatment cohort g and for each period $t \geq g$, the untreated potential outcomes would have evolved in parallel to those of the not-yet-treated groups.

Although I use the estimator from Callaway and Sant'Anna (2021), I present the analogous TWFE regression specification to illustrate the familiar fixed effects structure and facilitate comparison with prior studies.[15] I estimate the following specification using the pooled sample of all grades, as well as separately for elementary schools (grades K–5), middle schools (grades 6–8), and high schools (grades 9–12).[16] This

See De Chaisemartin and d'Haultfoeuille (2020), Goodman-Bacon (2021a), and Sun and Abraham (2021) for a fuller discussion. In particular, negative weights are more of a concern for long-term outcomes, since "longer-run treatment effects often receive negative weights" (Roth et al. 2023).

15. The estimator proposed in Callaway and Sant'Anna (2021) is not regression-based and therefore does not literally include group and time fixed effects in the way a TWFE regression does. By construction, however, it differences out unit and time heterogeneity in the same way that fixed effects would. It constructs group-time average treatment effects by comparing each treated cohort only with not-yet-treated or never-treated groups in the same period and then aggregates these effects to form overall estimates. Although fixed-effects terms are not part of an estimating equation, the estimator implicitly plays the same role: by design, comparisons within cohorts difference out all time-invariant group characteristics (analogous to group fixed effects), while comparisons against untreated groups in the same period difference out common time shocks (analogous to time fixed effects). In this sense, the estimator achieves the same identification goals as including fixed effects but avoids the biases that arise in TWFE regressions with staggered adoption.

16. Attendance and suspension outcomes are available for all grades from K through 12. Standardized

specification estimates contemporaneous (end-of-year) effects on student outcomes.

$$Y_{gst} = \beta CEP_{gst} + \theta_g + \delta_s + \alpha_t + \varepsilon_{gst} \tag{2}$$

where Y_{gst} is the outcome (test scores, absences, or suspension rates) for grade g in school s during school year t. CEP_{gst} is a binary treatment variable equal to 1 if the school has a CEP policy in place. θ_g, δ_s, and α_t denote grade, school, and year fixed effects, respectively. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications, clustered at the school level. All estimates are weighted by the number of students in the grade-school-year cell. If the identifying assumptions are satisfied, the coefficient β identifies the causal effect of CEP on student outcomes.

I estimate the following event study specification to assess the parallel trend assumption:

$$Y_{gst} = \sum_{k=-4}^{3} \beta_k D_{gs,t-k} + \theta_g + \delta_s + \alpha_t + \varepsilon_{gst} \tag{3}$$

where $D_{gs,t-k}$ is a series of dummy variables for event time k, indicating that CEP took place k periods before year t in school s, grade g. The other terms are defined as in equation (2). I aggregate the treatment effects over event times 0 through 3 to obtain the overall treatment effect.

Compared to existing quasi-experimental studies on student short-term outcomes, especially test scores, my specification differs in several respects: (i) Treatment definition: I define treatment at the school level since the policy is implemented at the school level, whereas Schwartz and Rothbart (2020) and Gordanier et al. (2020) define it at the student level and Ruffini (2022) define it at the district level, treating a district as treated if any school adopts CEP. (ii) Specifications: I use the estimator from Callaway and Sant'Anna (2021), while the prior papers all rely on a TWFE specification with different fixed effects—student fixed effects in Schwartz and Rothbart (2020), school fixed effects in Gordanier et al. (2020), and district fixed effects in Ruffini (2022). They all include time-varying school- or district-level controls. (iii) Samples and years: I use data from Texas with a longer follow-up period, while other papers use smaller samples and typically examine only one to three years after CEP adoption. Specifically, Gordanier et al. (2020) use South Carolina data (2014–2016),

test scores are available for grades 3–8. Pre-K, kindergarten, and high school academic outcomes follow a different structure and are analyzed separately by grade. Meal participation data are at the school-year level, and participation by grade is analyzed using broad grade bands (standalone pre-K, elementary, middle, and high schools).

Schwartz and Rothbart (2020) use NYC district data (2010–2013), and Ruffini (2022) use nationwide district-grade data (2009–2017).

My second strategy uses the doubly robust DiD estimator developed by (Sant'Anna and Zhao 2020; Callaway and Sant'Anna 2021), which combines inverse-propensity score weighting with outcome regression.[17] This approach first estimates a propensity score via a logistic model for being in a treatment cohort, based on observed pre-treatment covariates in Table 1. These scores are then used to reweight untreated units, creating a synthetic control group comparable to the treated group in terms of observed characteristics. In parallel, an outcome regression model is estimated for untreated units.

The reduced-form estimates identify the causal impact of being offered free school meals. Since not all students in schools that offer free school meals actually take them up, these estimates understate the causal effects of actually consuming school meals. The data do not include student-level take-up, which limits the ability to directly test effects on compliers. To explore whether CEP has a stronger effect in schools where it induces higher take-up, I split the sample by the baseline median share of students eligible for FRP. One might worry that a median split is too coarse to capture the subset of students who are actually responding to CEP. To address this concern, I also examine the results by splitting the sample into quartiles.

To assess the validity of the research design for short-term outcomes, I conduct several robustness checks. First, to address concerns about differential exposure and sample composition over time, I restrict the sample to schools observed for at least three pre- and post-treatment periods in order to balance event time. Second, while the primary analysis focuses on the 2011–2018 period, I re-estimate the models using alternative time windows (2007–2018 and 2011–2023) to test sensitivity to the sample period. Third, I examine whether the results are sensitive to the inclusion of student-count weights.

4.2 Strategy for Long-Term Outcomes

After documenting CEP's null short-term effects, I turn to estimating its long-term effects on students. The lack of short-term effects does not necessarily rule out the possibility of long-term impacts. It is possible that program effects, such as reducing

17. A key advantage of Callaway and Sant'Anna (2021), compared to other recently developed DiD estimators De Chaisemartin and d'Haultfoeuille (2020) and Sun and Abraham (2021), in my setting is that it can flexibly incorporate covariates—a main robustness check I use in this paper.

stigma, take more time to materialize.[18]

Recall that short-term specifications estimate the contemporaneous (end-of-year) effects of CEP. Both the outcome and treatment are naturally measured at the school-year level. However, for long-term outcomes such as college enrollment, the effects depend on students' entire history of CEP exposure throughout childhood. College enrollment is not observed until after high school graduation, so there is no single school-year observation to which the outcome can be attached. Therefore, I define treatment at the district-by-cohort level.[19]

I illustrate how treatment is assigned at district by cohort level in Table A3. Suppose a district adopted CEP in 2014. On the left are the birth cohorts from 1986 to 2006 in my sample; across the top are school grades. The last column indicates cumulative exposure. The 1997 cohort is the key reference cohort since they were in 12th grade when the district adopted CEP. Older cohorts (birth cohorts 1986-1996) were not exposed because they had already left. Younger cohorts (birth cohorts 1997-2006) were still in school and were exposed. The younger the cohort at adoption, the more years of exposure they had, as indicated by the red cell. When CEP was implemented between 2012 and 2023, the students were first exposed between the ages of 8 and 17 and were followed through 2023.

Two features of the CEP policy complicate treatment assignment for long-term outcomes. First, some districts implemented CEP district-wide but in a staggered fashion, or adopted it only in a subset of schools. As shown in Table A4, about 30% of the roughly 500 treated districts have more than one event time. For these districts, I define the event time as the one associated with the largest enrollment within the district.

Second, some districts implemented CEP only in a subset of schools. Among the roughly 500 treated districts, about 439 are district-wide (over 90% of students are enrolled in CEP schools), while the remaining 13% are only partially treated. In these partially treated districts, treatment typically covers elementary schools but not middle or high schools, creating cases where treatment turns on for some cohorts

18. The connection between short-term and long-term outcomes is not without debate. There is no consensus on which short-term indicators (e.g., test scores, non-cognitive skills) are reliable predictors of longer-term outcomes. See Gray-Lobe, Pathak, and Walters (2023), p. 367, for a detailed discussion.

19. For example, if treatment is defined at the district–year level, all students in a given calendar year would be assigned the same treatment status, regardless of grade level, thereby conflating cohorts with very different levels of exposure. By contrast, defining treatment at the district–cohort level correctly aligns exposure with students' schooling histories and ensures that the treatment measure reflects variation in cumulative CEP exposure across cohorts within the same district.

in elementary school and then turns off in later grades. This pattern violates the assumption in Callaway and Sant'Anna (2021) that once treatment is on, it stays on.

To assign treatment accurately at the district–cohort level, I tabulate the actual number of years of exposure during the seven years following CEP adoption, by the share of district enrollment in CEP schools in Appendix Table A5. The table shows a clear jump in exposure once the share of students in CEP schools exceeds 50%. Based on this pattern, I classify partially treated districts with fewer than 50% of students in CEP schools as untreated, and those with at least 50% as treated.

I use event-study designs to compare adult outcomes across birth cohorts within school districts. Specifically, I compare outcomes between "exposed" cohorts (students who were young enough to be in school during or after CEP adoption), and "unexposed cohorts" (students who had already graduated at the time of CEP adoption) within the same district. Furthermore, I use districts that later adopted CEP to account for any underlying differences across birth cohorts. Table 4 show the staggered event time across Texas Districts. A key identifying assumption of my research design is that CEP is the sole factor influencing the relationship between a student's exposure to CEP and her adult outcomes.

My baseline specification uses the DiD estimator from Callaway and Sant'Anna (2021), but I use the analogous TWFE specification for ease of illustration. This specification is similar to that in Bailey et al. (2024), which examines the long-term effects of the introduction of Food Stamps on adult outcomes by exploiting the program's rollout across counties. Under the event study framework, the coefficients can be understood as measuring the effect of each additional year of CEP exposure, conditional on having been exposed earlier.

$$Y_{db} = \sum_{k=-9}^{9} \beta_k \, 1[b - OldestCEPcohort_d = k] + \lambda_d + \theta_b + \varepsilon_{db} \tag{4}$$

where Y_{db} is a long-term outcome (SAT scores, high school graduation, college enrollment, and earnings) for birth cohort b attending district d. $1[b - OldestCEPcohort_d = k]$ is an indicator for event time k, equal to 1 if birth cohort b in district d is k years younger than the cohort that was in Grade 12 at the time of CEP adoption. λ_d and θ_b denote district and birth cohort fixed effects. District fixed effects absorb time-invariant differences across districts (e.g., baseline outcome levels). Cohort fixed effects absorb shocks common to all districts for a given birth cohort (e.g., statewide policy changes that affect the same cohorts across districts).Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications, clustered at the district level.

Following Bailey, Sun, and Timpe (2021) and Bailey et al. (2024), I estimate a spline model that allows for different linear slopes for exposure to CEP at different ages: grades 9–12 (event times 0 to 3), grades 6–8 (event times 4 to 6), and during grades 3–5 (event times 7 to 9).

$$\begin{aligned} Y_{db} = {} & \rho_1\, 1[0 \leq b - OldestCEPcohort_d \leq 3] + \rho_2\, 1[4 \leq b - OldestCEPcohort_d \leq 6] \\ & + \rho_3\, 1[7 \leq b - OldestCEPcohort_d \leq 9] + \lambda_d + \theta_b + \varepsilon_{db} \end{aligned} \tag{5}$$

Similar to the short-term outcomes, I use an alternative strategy that uses a doubly robust DiD estimator.

4.3 Tests of Identifying Assumptions

The parallel trends assumption is crucial to the validity of my research design. That is, the outcomes for treated cohorts would have evolved similarly to those of untreated cohorts in the absence of CEP. To evaluate it, I rely on both event-study models to visually and statistically test for the presence of pre-trends.

In addition to the parallel trends assumption, the research design also requires that no other shocks or policy interventions coincided with, or immediately followed, the rollout of CEP. Beginning with students entering grade 9 in school year 2014–2015, Texas implemented the Foundation High School Program through House Bill 5, which lowered the minimum graduation requirement from 26 to 22 credits and removed Algebra II as a universal requirement.

The high school graduation reform presents a threat to identification for districts adopted CEP in 2014. In such cases, CEP is perfectly coninside with the reform and both happened in event time 0. There is no variation to separately identify CEP from the high school graduation reform.

In addition, the high school reform will confound CEP if treated and control districts respond to the reform differently. If all districts are affected by the reform equally, then the cohort fixed effect will absorb the common shock. However, the statewide reform is not fully absorbed by cohort fixed effects if its impact varies across districts. Cohort fixed effects remove only the common statewide component for each cohort; any district-specific deviation arising from heterogeneous responses appears as a district-by-cohort shock. If those shocks are systematically related to CEP timing—for example, if poorer districts both react more strongly to the reform and tend to adopt CEP earlier—then the district-by-cohort shocks are correlated with event time. This will bias the event-study coefficients.

To address this, I take two approaches. First, I estimate specifications using matched control districts. If matched treated and control districts respond similarly to common shocks, this reduces the risk that heterogeneous responses to the reform bias the estimates. Second, I restrict the sample to birth cohorts fully exposed to the reform. In my data, cohorts born 1986–1999 graduated under the old requirements, whereas cohorts born 2000–2006 graduated under the new requirements. As a robustness check, I re-estimate the models using only the 2000–2006 cohorts to isolate CEP effects.

Another identification threat is that the treatment changed during COVID. From March 2020 to June 2022, the USDA issued Pandemic Electronic Benefit Transfer (P-EBT) for students eligible for FRP and those enrolled in CEP schools if schools were closed. The amount was up to $1,200 per student per year. In-person meals were free for all students. COVID affects both the treated and control districts. If it affects the treated and control districts equally, then it won't be a problem. Since families got the cash equivalent of missed free school meals, it was full take-up. The COVID period should be viewed as an upper bound.

5 Results

5.1 Short-Term Outcomes

5.1.1 School Meal Participation and Applications

Meal Participation

The availability of universal free school meals significantly increases participation. Figure 1a shows that treated and control schools track each other closely prior to CEP adoption, indicating parallel pre-trends. Following CEP adoption, there is a clear and immediate jump in the school meal participation rate. Figures 1b and 1c break down participation by breakfast and lunch. Consistent with higher take-up, Figure 1d shows that federal meal subsidies per student increase once a school adopts CEP, and this translates into a net increase in food revenue as shown in Figure 1e.

Table 5 reports the DiD estimates of CEP's effects on meal participation and federal subsidies. CEP increases meal participation by 6.2 percentage points (a 10 percent increase). This effect is equivalent to about 22 additional meals per student per school year, or approximately 1.5 months of meals. The effect is stronger for breakfast (7.7 percentage points) than for lunch (4.6 percentage points). In addition to participation, CEP adoption raised annual federal subsidies by $70 per student and increased net school food revenues by about $30 per student. These estimates are intention-to-treat (ITT) effects because the sample includes all students in treated schools, regardless of whether they actually ate school meals.

School-Meal Take-Up Heterogeneity

Table 6 presents heterogeneity in effects on take-up by student FRP eligibility, school grade level, and school poverty. Column (1) shows that CEP raises participation among both low-income students previously eligible for FRP meals and higher-income students who were not eligible. CEP increases participation by 7.9 percentage points (13 percent) among FRP-eligible students and by 20.2 percentage points (52 percent) among FRP-ineligible students, based on data from 244 schools with participation records by FRP eligibility. These data are available only for the 244 schools for one year after they adopted Provision 2, which still requires meal applications in the first year even though meals are free for all. Schools adopting CEP do not collect meal applications and do not track participation by FRP eligibility. While higher-income students appear more responsive to CEP, the majority of the overall increase still comes from low-income students, who comprise more than 80 percent of the student population in these schools.

The increase in take-up among FRP-eligible students suggests that CEP reduces the perceived stigma associated with subsidized meals. Stigma is difficult to measure directly, and under CEP reductions in stigma and administrative burdens are bundled together. The subsample of Provision 2 schools, however, still collected meal applications, which allows me to separate the role of stigma from administrative burdens. My results rely on a revealed preference approach, which assumes that adopting universal free school meals does not substantially change other aspects of meal provision, such as meal quality or serving logistics. Under these assumptions, the observed increase in take-up among FRP-eligible students suggests a reduction in stigma

Columns (3) through (6) of Table 6 show that CEP increases take-up similarly across elementary, middle, and high school levels. Columns (7) through (10) report effects by school poverty quartile, defined by the share of students eligible for FRP at baseline. The first quartile (least poor schools) experienced the largest increase in participation—8.5 percentage points, or a 17 percent increase. The effects for the second through fourth quartiles are smaller in percentage-point terms but represent relatively larger proportional increases given their lower baseline participation rates. These patterns are expected: lower-poverty schools are more responsive to CEP because fewer students were previously income-eligible for FRP meals, whereas in higher-poverty schools many students already had access to free meals based on income eligibility. These results are also in Figure 2.

Meal Applications

Figure 1f shows that the share of students eligible by submitting an application declines by 20 percentage points. Before CEP, about 34% of students were eligible via application. This indicates that CEP reduces administrative burdens for students.[20]

5.1.2 Pre-K Readiness Assessments, Grades 3-8 Test Scores, Absenteeism, Suspensions, and High School Academic Outcomes

Pre-K Readiness Assessments

Figure 3a present event study estimates for a summary index of pre-K readiness assessment, while Appendix Figure A4 shows results for its five components. The graphs support the parallel trends assumption. Because the Pre-K and Kindergarten data span 2018–2023 and overlap with the COVID-19 period, when school meal operations changed substantially, these effects should be interpreted with caution and are likely to represent an upper bound.

Table 7 reports the estimated effects of CEP on Pre-K and Kindergarten readiness scores. Panel A of Table 7 shows a statistically insignificant 0.014–standard-deviation increase in the index, but modest improvements in several Pre-K domains. CEP increases reading readiness by 0.02 percentage points (6 percent), communication and language by 0.02 percentage points (3.6 percent), and physical and mental health by 0.03 percentage points (3 percent), while effects on writing and math are small and insignificant. Overall, these findings suggest that CEP does not statistically improve the pre-K outcome, although there are suggestive positive effects in some pre-K domains.

Grades 3–8 Test Scores

Figure 3e shows the event study for grades 3–8 reading scores, and Figure 3f shows the event study for math. Both generally support the parallel trends assumption.

Table 8 reports the estimated effects of CEP on grades 3–8 test scores. Across reading, math, and the composite test score index, the estimates are small and statistically insignificant. For example, the full-sample effects are 0.008 (SE = 0.007) for reading, –0.006 (SE = 0.010) for math, and 0.001 (SE = 0.008) for the index, with

20. Because the data are limited to application-based certifications (i.e., students who would otherwise submit an application), declines in applications provide a lower-bound measure of reductions in administrative burden for families.

similarly null results across elementary and middle schools.

Appendix Tables A10 and A11 split the sample by baseline FRPL share, above and below the median. Results are uniformly insignificant, with many point estimates slightly negative.

High School Academic Outcomes

Appendix Figure A5 presents the event study of Grade 9 course scores in Reading, Algebra I, English, and Biology. The estimated event study graphs are essentially flat. Appendix Table A1 shows that the point estimates for the grade 9 course index and its components are slightly negative and not statistically significant.

Absenteeism

Figure 3e shows the event study of the number of days absent. Panel D of Table 8 presents the estimates of CEP on the number of days absent for the full sample. Column 1 of Table 8 shows the estimated effect is small and statistically insignificant (0.060, SE = 0.077), suggesting no detectable change in absenteeism.

When disaggregated by school level, the effect remains insignificant for elementary schools (column 2; 0.060, SE = 0.039) and high schools (column 4; –0.062, SE = 0.280). In contrast, for middle schools (column 3), CEP is associated with a statistically significant increase of 0.210 days absent (SE = 0.114), significant at the 10 percent level. It is about a 3 percent increase compared to its baseline mean. Taken together, these results indicate that CEP does not systematically reduce absenteeism and may modestly increase absences among middle school students.

Suspension Rates

Figure 3f shows the event study of suspension rates. Panel E of Table 8 reports the ITT estimates of CEP on student suspension rates. Column 1 shows that CEP is associated with a small but statistically significant increase of 0.017 percentage points (SE = 0.009), relative to a mean suspension rate of 0.39 percent per 100 students. Disaggregating by school level, the effect is statistically insignificant for both elementary schools (0.005, SE = 0.007) and middle schools (0.009, SE = 0.017). By contrast, in high schools (column 4), CEP significantly increases suspension rates by 0.064 percentage points (SE = 0.035), relative to a mean of 0.89 percent. These results suggest that CEP participation does not reduce suspensions.

5.1.3 Robustness Checks for Short-Term Outcomes

For the robustness checks, I use different years of data, weighting schemes, balanced samples, and control groups. Appendix Figure A6 presents the event studies of meal participation and revenue outcomes using the matching specification. Appendix Table A7 shows that the estimated effects on meal take-up are robust.

I conduct similar checks for the academic outcomes. The pre-K results are also robust, as shown in Appendix Table A8. Appendix Table A9 shows that the test score index is generally robust, although it can be sensitive to weighting in the matching specification.

5.1.4 Subgroup Analysis for Short-Term Outcomes

I begin by estimating the participation results separately by student FRP eligibility status, school grade, and school poverty quintile. For the main short-term outcomes—test scores, absences, and suspensions—I conduct subgroup analyses along several dimensions. Specifically, I divide schools according to whether their baseline share of students eligible for FRP meals is above or below the median, and I further disaggregate by school type (elementary, middle, and high schools). In addition, I examine heterogeneity in treatment effects by student race/ethnicity, gender, and low-income status. These subgroup analyses by student characteristics are conducted both for the full sample and within the above- and below-median school poverty categories.

Test Scores, Absenteeism, and Suspension Heterogeneity

Tables 9 examine heterogeneity by race, gender, and income status for different samples. Appendix Table A12 reports results for the same subgroups in the elementary school, and Appendix Table A13 reports results for middle schools. Appendix Table A14 presents subgroup results for schools with baseline FRP shares below the median, and Appendix Table A15 reports results for schools with baseline FRP shares above the median.

Across these subgroup analyses, the results are generally null, with the one exception being small, negative, and statistically significant effects for Black students. CEP does not affect standardized test performance in grades 3–8, either overall or across most student subgroups. The results are mostly insignificant, except for Black students, where the estimates are statistically significant but slightly negative.

5.2 Long-Term Outcomes

After documenting the large null short-term effects, I turn to CEP's long-term effects. It is possible that the program's impacts take longer to materialize.

For the long-term outcomes, I focus on two complementary samples. The first sample consists of birth cohorts 2000 to 2006 in ever-treated districts. Because all students in this sample were exposed to the high school graduation reform in 2014, it allows me to isolate the effects of CEP. However, these students are still relatively young, and I can follow them for only up to six years after CEP adoption. The second sample includes birth cohorts 1986 to 2006 in districts eligible for CEP. This sample allows me to follow students for up to ten years, but for districts that adopted CEP in 2014, I cannot fully separate the effects of CEP from those of the high school graduation reform.

For both samples, I use two specifications. The "within district specification" compares exposed cohorts to unexposed cohorts within the same districts and uses cohorts in later-treated districts to difference out time trends. The "matching specification" finds a synthetic control group comparable to the treated group based on pre-treatment characteristics. I first present the event studies in Figure 4 for birth cohorts 2000 to 2006 using the within-district specification. The results for the same sample based on the matching specification are shown in Figure A7, which are very similar. Figure 5 presents the results from the matching specification for birth cohorts 1986 to 2006, and Appendix Figure A8 shows the within-district specification for the same sample. These figures are also generally similar, except for high school graduation. This suggests that the high school graduation reform could potentially upward bias the estimated effects. Based on these event study results, my preferred sample is birth cohorts birth cohorts 2000 to 2006.

CEP Exposure

Figure 4a shows the event study of years of CEP exposure. It shows that treated students have more CEP exposure compared to untreated students. The coefficient would be mechanically one had students not moved school districts. However, since students move throughout childhood, the coefficient here is around 0.5. The event studies based on alternative specifications and samples in Figure 5a, A7a, and A8a, show similar results.

Table 10, column 1, Panel A reports the estimated effects using the within-district specification. Event times are grouped into exposure windows corresponding to high school, middle school, and upper elementary grades. The results indicate that cohorts first exposed in grades 9–12 experienced an average of 0.68 years of CEP exposure. Cohorts first exposed in grades 6–8 averaged 2.1 years, while those first exposed in grades 3–5 averaged 3.8 years. Column 1, Panel B presents the results from the matching specification, with point estimates that are slightly larger than those from the within-district specification. Table 11, column 1, shows similar results for birth cohorts from 2000 through 2006.

The key question is whether exposure to CEP during childhood affects students' long-term outcomes. In the next section, I present results on on-time high school graduation, high school dropout, SAT participation, SAT performance, on-time college enrollment, and earnings at age 21.

On-Time High School Graduation

Figures 4b and A7b indicate that CEP does not increase high school graduation for birth cohorts from 2000 through 2006, who were fully exposed to the graduation reform. Similarly, Figure 5b presents a flat event study using the matching specification for the full sample. However, Figure A8b shows an increase in high school graduation using the within-district specification, which can capture the effects of the statewide high school graduation reform.

Table 10, column 2, Panel A shows positive effects of CEP exposure on on-time high school graduation. For students first exposed in high school, CEP exposure is associated with a 0.008 percentage-point increase in graduation, or about 1.3% of the dependent variable mean. This estimate is statistically significant at the 1 percent level. Dividing the effect by the average exposure of 0.684 years implies that each additional year of CEP exposure is associated with a 0.01 percentage-point increase in graduation. For students first exposed in middle school, CEP exposure is associated with a 0.017 percentage-point increase, also significant at the 1 percent level. Extrapolating this effect to 13 years of exposure suggests an effect of roughly 0.15, a large magnitude for students exposed throughout K–12. For students first exposed in grades 3–5, CEP exposure is associated with a 0.020 percentage-point increase in graduation, or 3.2% of the dependent variable mean. These magnitudes are very large and may, at least in part, capture the contemporaneous high school graduation reform. Column 2, Panel B presents the results using the matching specification. The point estimates are similar to those from the within-district specification, but most lose statistical significance. In column 2 of Table 11, I restrict the sample to birth cohorts from 2000 through 2006, who were fully exposed to the high school graduation reform. The point estimates are smaller in magnitude, and none are statistically significant in either the within-district specification (Panel A) or the matching specification (Panel B).

Taken together, these findings indicate that CEP does not increase high school graduation rates. The positive effects in the within-district specification for the full sample are likely driven by the statewide graduation reform rather than by CEP exposure.

High School Dropout

Figures 4c and A7c show that CEP does not affect high school dropout rates for birth cohorts from 2000 through 2006. Figure 5c presents a flat event study using the matching specification. Figure A8c shows pre-trends in the high school dropout rate using the within-district specification. Overall, these indicate that CEP has no effects on high school dropout.

Consistent with the event study graphs, column 3, Panel A of Table 10 shows that CEP is associated with a 0.003 percentage-point decrease in the dropout rate, equivalent to about 6% of the dependent variable mean. This effect is statistically significant at the 5 percent level. However, the estimates for students first exposed in grades 6–8 and grades 3–5 are positive, and given the pre-trends, these results are not reliable. Column 3, Panel B mostly shows decreases in the dropout rate between 0.002 and 0.006, though most estimates are not statistically significant. For the 2000–2006 birth cohorts in column 3 of Table 11, nearly all of the point estimates are statistically insignificant.

SAT Taking and SAT Scores

The event studies of SAT scores in Figures 4d, 5d, and A7d, and A8d show little evidence of statistically significant effects.

Column 4 of Tables 10 and 11 reports the effects on the probability of taking the SAT. Most estimates are small and statistically insignificant, suggesting that CEP does not affect SAT taking. Column 5 of the same tables shows the estimated effects on SAT scores. Across specifications, there is some evidence of small positive effects for students first exposed in grades 9–12, statistically significant at the 10 percent level. However, the point estimates are modest in magnitude, ranging from 1 to 9 points.

On-Time College Enrollment

The event studies of on-time college enrollment in Figures 4e, 5e, A7e, and A8e, show little evidence of statistically significant effects.

Column 6 of Table 10 reports the effects on on-time college enrollment. The point estimates are 0 for students first exposed in grades 9–12 and in grades 6–8. For students first exposed in grades 3–5, the point estimate is 0.006 (1.4% of the dependent variable mean), but it is not statistically significant. The matching specification in Panel B yields similar results. Column 6 of Table 11 also indicates null effects on college enrollment.

Earnings

I am able to observe earnings at age 21 for birth cohorts from 1986 through 2002, all of whom reached age 21 by 2023. The event studies in Figures 5f and A8f show little evidence of statistically significant effects on earnings at age 21, and the confidence intervals are wide. Column 7, Panel A of Table 10 reports the effects on earnings at age 21. The point estimate is –$24 for students first exposed in grades 9–12, which is not statistically significant, with a wide confidence interval. For those first exposed in grades 6–8 or 3–5, the point estimates are more negative and even less precise. The matching specification in Panel B yields similarly negative and imprecise estimates. Among the 2000–2006 birth cohorts, only those born between 2000 and 2003 had reached age 23. The sample is too small to reliably estimate earnings effects.

5.2.1 Subgroup Analysis for Long-Term Outcomes

I conduct subgroup analysis by student low-income status, race/ethnicity, and gender. The results for students first exposed in middle school is in Appendix Table A16. Appendix Table A17 show the results for students first exposed in high school. The CEP exposure is significant for all subgroups. However, CEP exposure does not seem to have a consistent positive effects for any of the subgroups.

5.3 Additional Internal and External Validity Checks

This paper uses data from Texas only. Differences across states in demographics, equity in school finance, and available outcome measures can limit the generalizability of the findings. In addition to school meal–related funding, CEP can affect broader school district finances. Traditionally, states have used the share of students eligible for FRP meals as a key indicator of student poverty. This measure is used for K–12 funding allocations and for accountability purposes. However, since CEP eliminates school meal applications, FRP data have become a less reliable proxy for student poverty. States have adopted different approaches to address this.[21] Many states, including

21. The Urban Institute has a tool indicating how each state uses alternative measures following changes to FRP data.

Texas, continue to rely on FRP eligibility, supplemented with direct certification of students participating in programs such as SNAP, TANF, and Medicaid. Some states have moved away from FRP-based measures altogether, instead using direct certification data or other proxies such as census-based poverty estimates.

To assess internal and external validity, I link nationwide CEP participation data from the NCES with school district finance data from the Census Bureau's Annual Survey of School System Finances for school years 2013–2014 through 2018–2019. Because the finance data are available only at the district level, I define a district as treated if at least one school within it adopts CEP. My DiD estimates show that CEP increases federal food revenues and state revenues by $63.4 and $1 per pupil, respectively, while local revenue decreases by $18.5 per pupil. These results are consistent with those reported in Table 5 of this paper and in New York State (Rothbart, Schwartz, and Gutierrez 2023).

To assess how CEP affects district finances in Texas and nationwide, I plot per-pupil expenditures by district CEP status in Appendix Figure A9. Between 2013 and 2018, CEP districts in Texas and nationwide have nearly identical expenditure levels and track each other closely. Texas CEP and non-CEP districts have lower instructional and total expenditures than the nationwide averages, but they follow national trends closely. Overall, these figures suggest that CEP does not affect school district finances differently in Texas compared to the national pattern, and that CEP did not substantially change school finances during this period. After COVID, however, nine states implemented statewide universal free school meals in addition to CEP, so trends may differ in those states. I plan to update these graphs as more recent data become available.

6 Magnitudes and Relationship to the Existing Literature

Compare Effects of Universal Free School Meals Across Studies

Table 12 presents a summary of recent studies examining the effects of universal free school meals in the United States. These studies differ in research design, context, treatment definition, sample composition, and years of data used.

The strongest evidence on short-term outcomes comes from a large-scale, federally sponsored randomized controlled trial (RCT) conducted in 150 elementary schools Bernstein et al. (2004) and Schanzenbach and Zaki (2014). They find null effects on all outcomes, both overall and by subgroup, and the results in this paper are consistent with theirs.[22] Other quasi-experimental studies, including this paper, exploit the staggered adoption of provision 2 or CEP. These studies differ in their specifications, treatment definitions, sample characteristics, and follow-up periods.

Treatment definitions vary across studies. I define treatment at the school level, since CEP is implemented at that level. In contrast, some papers define treatment at the student or district level—for example, classifying a district as treated if any school within it adopts CEP (Ruffini 2022).

The model specifications also vary. I employ the estimator developed by Callaway and Sant'Anna, which is equivalent to a model with school and year fixed effects. Other studies instead use student-, school-, or district-level fixed effects. Test score estimates in this literature are highly sensitive to modeling choices. In papers reporting positive effects, those findings are typically not robust across alternative specifications within the same study. For example, Schwartz and Rothbart (2020) report significant effects with student fixed effects but not with school fixed effects.

The samples and years of follow-up also differ across studies. I use administrative data from Texas covering 2011–2018, while many other papers have shorter follow-up windows. Gordanier et al. (2020) study South Carolina data from 2014–2016, Schwartz and Rothbart (2020) use New York City data from 2010–2013, and Ruffini (2022) analyze nationwide district–grade data from 2009–2017. The sample characteristics also vary. Studies based on New York City schools (Leos-Urbel et al. 2013; Schwartz and Rothbart 2020) have the most disadvantaged populations, with over 85 percent of students eligible for free or reduced-price (FRP) meals and over 80 percent identifying as non-White. The population in this paper is similarly disadvantaged, with over 80 percent of students eligible for FRP meals and non-White. The full sample in Ruffini (2022) is comparable to that in (Schanzenbach and Zaki 2014), and both are less disadvantaged than the sample analyzed by Gordanier et al. (2020).

Despite these differences across studies, the estimated effects on the number of meals served and federal subsidies are broadly similar. Across studies, federal subsidies increase by approximately \$70 to \$100 per student per year.

Compare Grades 3-8 Test Score Effects Across Studies

Figure 6a compares the estimated effects on grades 3–8 test scores across studies.

22. Bernstein et al. (2004) conducted a federally sponsored randomized study of 151 elementary schools across six districts to examine the effects of universal breakfast during 1999–2001. The authors conclude that "there was no consistent pattern of significant effects by subgroup or school district" on any outcomes, including nutrition, suspensions, test scores, and health. Schanzenbach and Zaki (2014) reanalyze the experimental data by breakfast location (classroom versus cafeteria) and reach the same conclusion.

I do not scale the ITT estimates by the first-stage since participation rates are similar across most studies. The estimate from Schanzenbach and Zaki (2014), based on a federally sponsored RCT, finds no effect. Gordanier et al. (2020) and Ruffini (2022) report that CEP increases math but not reading test scores. My confidence interval rules out the effect size found in Gordanier et al. (2020), but the estimate from Ruffini (2022) overlaps with my upper bound.

Figure 6b reports effects on grades 6–8 test scores. (Schwartz and Rothbart 2020) finds positive effects in both math and reading. No other studies, including this paper, find positive effects, and the confidence intervals all overlap. Across studies, there is no robust evidence that CEP increases achievement.

Figure 6b reports the effects on Grades 6–8 test scores. (Schwartz and Rothbart 2020) find positive effects in both math and reading. However, no other studies, including this paper, detect positive effects, and the confidence intervals all overlap. Taken together, the evidence across studies does not provide robust support that CEP increases achievement.

Magnitudes of Grades 3-8 Test Score Estimates

My reduced-form estimates show that offering universal free school meals through CEP has a statistically insignificant effect on the student test score index (ITT $= -0.001$, standard error $= 0.008$). This is an ITT estimate because it captures the effect of offering free meals regardless of actual participation; I do not observe which students participate. The 95% confidence interval is $[-0.0167, , 0.0147]$. The magnitude is small and not economically significant.

Chetty, Friedman, and Rockoff (2014) suggest that a one–standard-deviation increase in test scores is associated with a gain of about \$2,000 (a 12% increase) in age-25 earnings. This implies an earnings effect ranging from approximately $-\$33$ to $+\$29$. Similarly, Kline and Walters (2016) find that a one–standard-deviation increase in test scores leads to a 10% increase in earnings, implying an effect ranging from about a 0.17% decrease to a 0.16% increase in earnings.

To facilitate comparison with other studies on the effects of transfer or education policies on test scores, I scale the estimates using the first stage. CEP increases federal subsidies by \$69.968 (standard error $= 3.738$).[23] This implies that every additional

23. For the Wald estimator to be interpreted as a Local Average Treatment Effect (LATE)—the causal effect of school meal participation on test scores among compliers—the exclusion restriction must hold. This assumption requires that CEP affects test scores only through its impact on school meal take-up, and not through any other channels. However, the assumption could be violated if CEP influences test scores via mechanisms beyond meal participation. For instance, CEP might reduce the stigma associated with school meals and shift social norms or peer behaviors, or it could

$1,000 spent on school meals is associated with a 0.014 standard deviation increase in composite test scores. Page (2024) review income transfers and find that a $1,000 cash transfer typically yields a 0.03–0.04 standard deviation increase in composite test scores. Common education policies (e.g., class-size reduction, Head Start) tend to deliver 0.01–0.02 standard deviations per $1,000.

Ruffini (2022) finds that an additional $1,000 per student in school meals induced by CEP adoption increases math scores by an insignificant 0.16 standard deviations for the full sample and by 0.51 standard deviations (significant at the 10% level) for the exposed subsample. Assuming the test score gains in Gordanier et al. (2020) are associated with a $100 government cost, this implies that $1,000 of government spending corresponds to a 0.6 standard-deviation increase in math test scores. These magnitudes are much larger than those reported in Page (2024) for other government transfer and education policies.

7 Empirical Welfare Analysis

I provide a welfare analysis using the MVPF framework, as described in Hendren and Sprung-Keyser (2020). The MVPF is defined as the ratio of recipients' willingness to pay for a policy change to the government's net cost. The general MVPF formula for in-kind transfer like school meals is

$$\text{MVPF} = \frac{\text{Willingness To Pay}}{\text{Net Government Cost}}$$

where the willingness to pay is each infra-marginal recipient's willingness to pay for the increased spending on school meals. The program cost includes the mechanical expenditure for each infra-marginal beneficiary and the fiscal externality.

Net Cost to the Government

The total cost of the program includes both the mechanical program expenditures and the fiscal externalities resulting from CEP's impact on children's future earnings. To calculate the mechanical program cost, I scale the ITT estimate of $70 per student

increase federal reimbursements, which in turn may improve meal quality. Such mechanisms could generate spillover effects for students who already participated in school meals (i.e., always-takers), not just for students induced to participate because of CEP (i.e., compliers). If such indirect effects exist, scaling by the first stage would yield biased estimates. Ultimately, whether the Wald estimator is biased in the CEP setting is an empirical question. In my analysis, I do not find statistically significant effects on test scores or other outcomes for students who were always eligible for FRP (likely always-takers), or for those occasionally or never eligible (likely compliers). This suggests that spillover effects are limited.

per year by the first-stage meal take-up rate (ITT = 0.062). This implies a cost of $1,129 per marginal student. This calculation relies on the exclusion restriction, which assumes that CEP affects student outcomes only through increased school meal take-up.[24] In addition, I assume that the only behavioral channel through which CEP affects tax revenue is changes in children's future taxable labor income.

To estimate the fiscal externalities associated with CEP, I use the program's impact on test scores to project its effect on lifetime earnings and, in turn, future tax revenue. I find that CEP has small, statistically insignificant positive effects on test scores for pre-K students and similarly small, statistically insignificant effects for other grades. I average the ITT estimates of the test score index across pre-K, kindergarten, grades 3–8, and high school, weighting by each group's share in the sample. The resulting average test score impact is -0.002 standard deviations. Scaling by the first-stage take-up rate, the effects on test score is -0.03 SD . Following the assumption that a 1 SD increase in test scores corresponds to a 10% increase in earnings, a -0.03 SD effect implies a 0.3% reduction in lifetime earnings. This corresponds to a present discounted lifetime earnings loss of approximately $1,650 per student. Assuming a 20% average tax rate, this earnings loss translates into a tax revenue reduction of about $329 per student. Spread over 13 years of CEP exposure (K–12), this amounts to an annual tax revenue loss of $25 per student.

Combining the program cost and fiscal externalities, the total net cost to the government is approximately $1,154 per marginal student, as shown in Figure 7a.

Willingness To Pay

Next, I estimate the willingness to pay. Because school meals are an in-kind transfer, students and parents might not value them in the same way as cash. I provide a range of estimates based on reduced grocery costs, out-of-pocket school meal spending, and the value of saved time, as shown in Figure 7b.

CEP reduces household grocery costs. Handbury and Moshary (2021) show that CEP reduces household grocery spending when children receive free meals at school. They find that household grocery costs for families with children decline by 7 percent following the adoption of CEP. Given average monthly grocery spending of $200, this corresponds to a $14 per month reduction in 2016 dollars.[25] This adds to $168 per year in 2016 dollars. After adjusting for inflation to 2023, this translates into an

24. This is a strong assumption. It may not hold if CEP affects test scores through channels other than meal participation.

25. Similarly, Marcus and Yewell (2022) find that CEP reduces grocery spending for households with children by about $11 per month (approximately 5 percent, also likely in 2016 dollars).

annual grocery savings of $214 per household.

CEP also reduces out-of-pocket spending on school meals. Unlike SNAP and WIC, school meals are partially traded in a well-functioning market—meal prices and take-up are fully observed for students who are ineligible for FRP meals.[26] Prior to CEP, 36% of FRP-ineligible students in Texas ate school breakfast and 57% ate lunch daily. The average price was about $2.0 for breakfast and $3.2 for lunch. Assuming a 180-day school year, this amounts to approximately $458 in annual meal costs. This is my baseline estimate.[27] A limitation of this approach is that it does not take account into the effects of stigma.

In addition to reducing grocery costs and out-of-pocket school meal spending, CEP also saves parents time. Guthrie and McClelland (2009) find that having children eat meals prepared at school or day care saves parents 24 minutes per day that would otherwise be spent preparing and packing meals at home, based on the American Time Use Survey.[28] This adds to roughly 72 hours per year. In addition, I include one hour of time savings from not having to submit meal applications. Assuming the parent earns an hourly wage of $13.34 (corresponding to 185% of the poverty level, which is the FRP income eligibility threshold), this translates to annual savings of about $974. These savings reflect recovered labor time, which could be used for paid work, rest, or household tasks.

Figure 7c shows that CEP's MVPF ranges from 0.19 to 1.03, depending on the underlying assumptions. In most cases, the MVPF is below 1, indicating that the willingness to pay is lower than the net cost to the government. Next, I compare CEP's MVPF with those of other federal nutrition assistance programs, such as WIC and SNAP, as well as with K–12 spending policies, in Figure 7d. Because CEP and these policies often target different populations, it is not straightforward to compare their MVPFs directly. Formally, Hendren and Sprung-Keyser (2020) show that one must use the incidence-weighted average social welfare weight when comparing MVPFs across policies.

Since CEP is a universal policy that affects all students, its MVPF likely varies

26. Although the federal government heavily regulates school meal prices, districts have some flexibility in setting them.

27. The fact that the increase in take-up induced by CEP is relatively small implies that the average school meal participant is similar to the marginal participant. Because CEP only modestly increases school meal take-up, the spending of the average participant is close to that of the individual who is just indifferent between participating and not participating (the marginal participant).

28. Only the 2006–2008 Eating & Health Module of the American Time Use Survey (ATUS) includes questions on time spent preparing meals for children. Later years do not, based on the data documentation.

across the income distribution. The MVPF constructed above applies to the full analysis sample. As noted in Finkelstein and Hendren (2020), when a policy affects a heterogeneous group of beneficiaries, evaluating its MVPF becomes more challenging. In such cases, it is important to account for societal preferences toward the different recipients within the policy's beneficiary group.

8 Discussion and Conclusion

This paper evaluates the short- and long-term effects of universal free school meal policies using administrative data from Texas. I find that CEP modestly increases meal participation, as well as reduces stigma and administrative burdens. However, I detect no improvements in student academic or behavioral outcomes in either the short or long run. These null results are robust to specifications and sample choices and hold across most subgroups.

These results are not without limitations. I do not have student-level data on meal participation. This limits my ability to precisely capture students who are induced to participate in school meals due to CEP. Although the administrative data include a wide range of outcomes, I do not observe students' nutritional intake or physical and mental health outcomes. Finally, this paper uses data from Texas only. State differences in demographics, school finance, and outcome measures may limit the generalizability of the findings.

The lack of positive effects from CEP could be due to two scenarios. The first possibility is that it does not influence outcomes through nutrition, behavior, or family resource channels. Universal free school meals may not affect nutrition because they replace what students would otherwise consume at home (Bernstein et al. 2004; Schanzenbach and Zaki 2014). In high-poverty schools, most students are already eligible for free or reduced-price meals, so stigma is likely to be less salient. Finally, CEP reduces family grocery costs by about \$200 per year (Handbury and Moshary 2021; Marcus and Yewell 2022), or roughly \$3,000 over 13 years of K–12 education—an amount that may be too small to move the needle.

The second scenario is that CEP did generate positive effects, but these effects are too small to detect, even with the large sample size used in this analysis. While I can rule out modest improvements in test scores, the confidence intervals for some long-term outcomes are wide, and their width varies across specifications, samples, and subgroups. Some health, educational, or labor market benefits may emerge later in life.

My findings do not imply that school meal programs are not worth supporting, nor that eliminating CEP would have no consequences for students. The estimates in this paper speak to the effects of expanding universal free school meals on top of existing means-tested meal programs for which over half of schoolchildren qualify based on family income. It is important to note that, while I do not detect improvements in the academic, behavioral, or economic outcomes observed in administrative records, CEP does fulfill the goals established by Congress at its inception (Billings and Carter 2020): increasing access to and participation in free school meals, reducing paperwork for families by eliminating meal applications. The increase in take-up among low-income students already eligible for free meals suggests a reduction in administrative burden and stigma. However, these goals have since broadened to include improving student health and achievement, as states continued to expand CEP.

My results suggest that expanding free meals beyond high-poverty schools may not be the most efficient way to improve student academic outcomes. Universal free school meals involve a trade-off between broader access and higher program costs. Making meals free for all American children would require an additional $15 billion annually, on top of the existing $24 billion in federal funding.[29] This expansion would increase total K–12 spending nationwide by about 1.6 percent. Such an increase would primarily subsidize children from higher-income families and is not well targeted to address nutrition or achievement gaps. Therefore, it is unlikely to yield significant short- or long-term benefits.

29. California began providing statewide universal free school meals in the 2022–2023 school year. For school year 2024–2025, federal funding amounts to $2.7 billion, while state spending totals $1.8 billion. California enrolls about 12% of all K–12 students in the United States. Scaling these costs to the national level suggests it would require approximately $15 billion in state funds, in addition to $24 billion in federal funding, to operate a universal free school meal program nationwide. This would increase the nation's total K–12 spending by about 1.6 percent.

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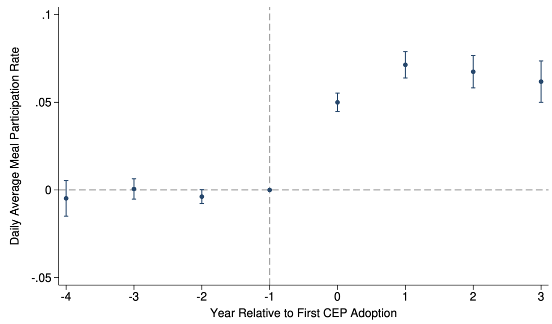
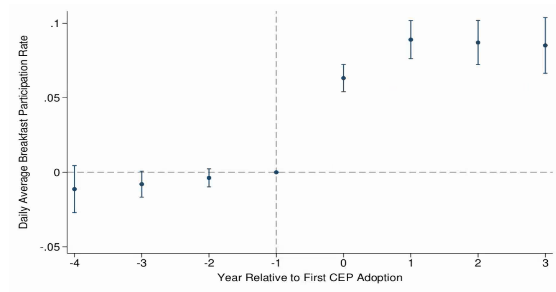
Figure 1: School Meal Participation, Subsidies, and Applications

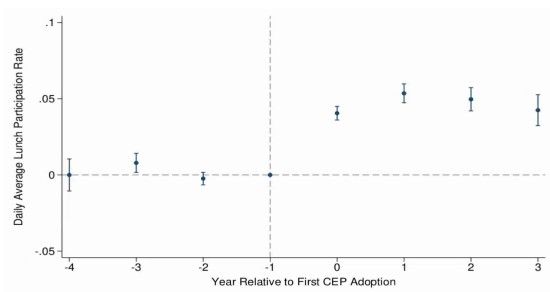
(a) Meal Participation (Breakfast + Lunch)



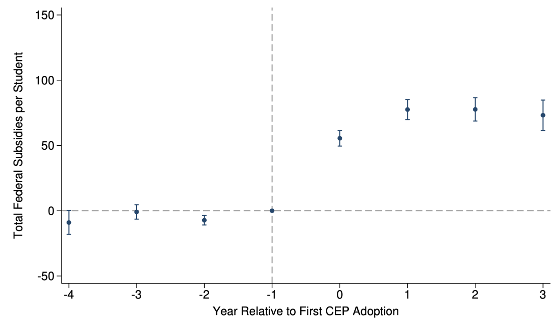
(b) Breakfast Participation



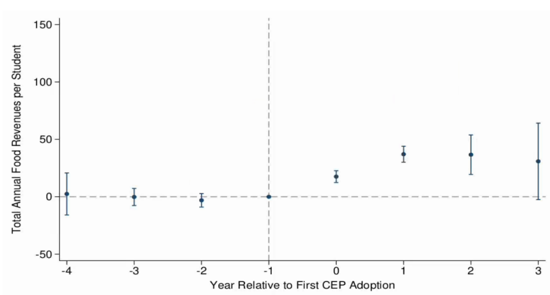
(c) Lunch Participation



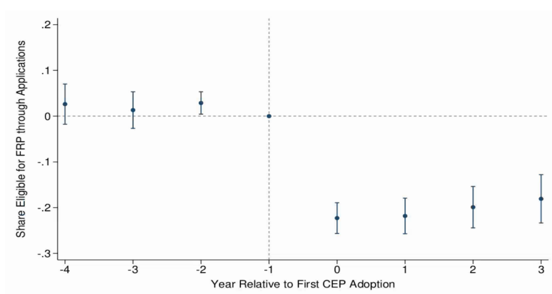
(d) Federal Subsidies per Student



(e) Net Food Revenues per Student

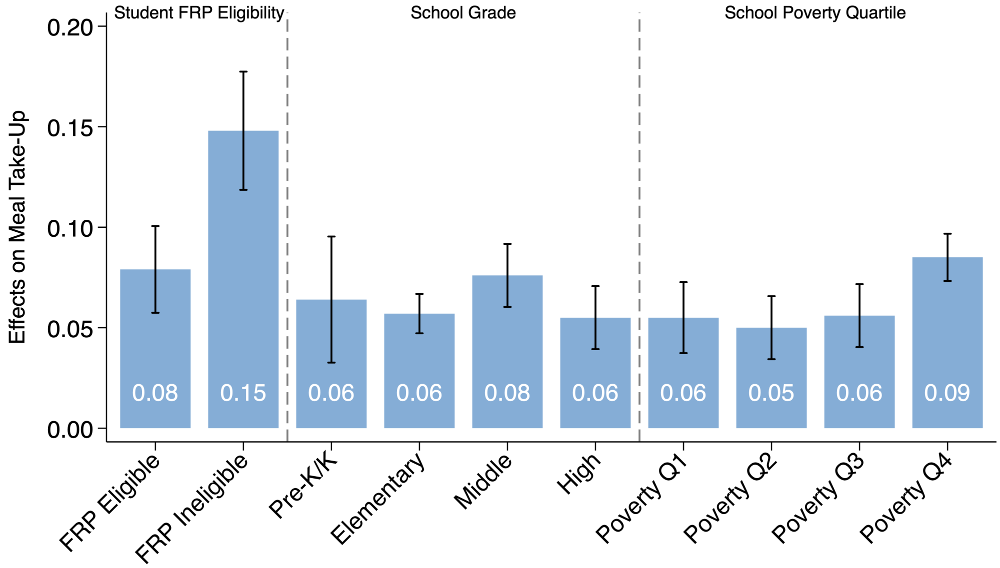


(f) Meal Applications



Notes: These graphs plot event-study estimates using the specifications in equation (2) for each of the first five main outcomes, employing the estimator developed by Callaway and Sant'Anna (2021). The sample includes schools in Texas that adopted CEP by 2018. All estimates are based on data collapsed into school × year and weighted by the number of observations per cell. For panel f, the sample includes districts in Texas that adopted CEP by 2018. The application data is only available at the district level. All estimates are based on data collapsed into district × year cells and weighted by the number of students per cell. The bars indicate 95 percent confidence intervals, based on standard errors obtained using a multiplier bootstrap procedure and clustered at the school level.

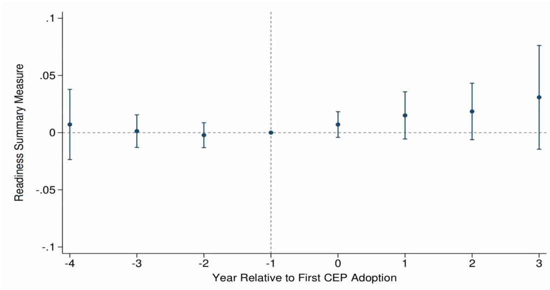
Figure 2: Meal Participation Rate by Subgroup



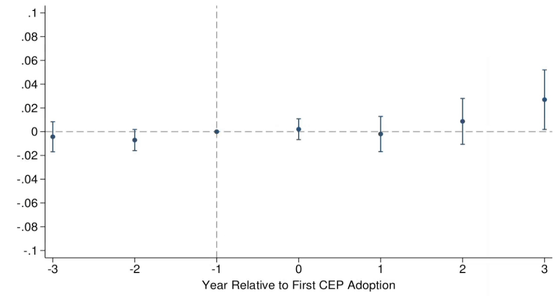
Notes: Free and reduced-price (FRP) meal eligibility is defined as $\leq$ 185% of the federal poverty line (about $60,000 for a family of four). The subgroup analysis by student FRP eligibility is based on school $\times$ year $\times$ FRP eligibility group data from a sample of 244 schools for which such data are available. The subgroup analyses by school grade and by school quartile are based on separate estimates using the estimator developed by Callaway and Sant'Anna (2021). Poverty Q1 indicates the lowest income quartile, and Poverty Q4 indicates the highest income quartile. All estimates are weighted by the number of observations per cell. The bars indicate 95 percent confidence intervals, based on standard errors obtained using a multiplier bootstrap procedure and clustered at the school level.

Figure 3: Short-Term Academic and Behavioral Outcomes

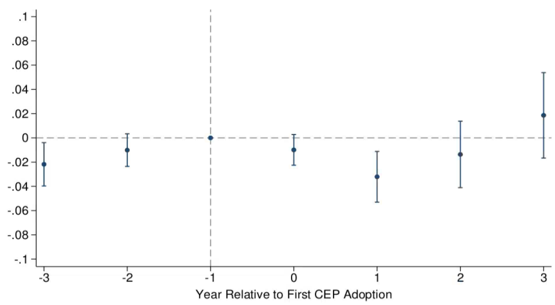
(a) Pre-K Assessment Index



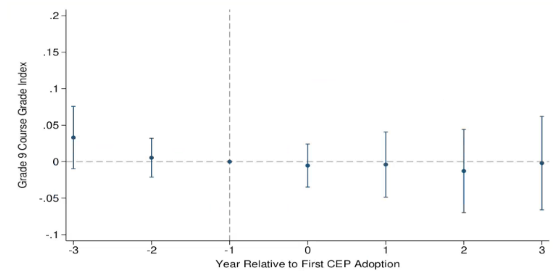
(b) Grade 3-8 Reading Test Scores



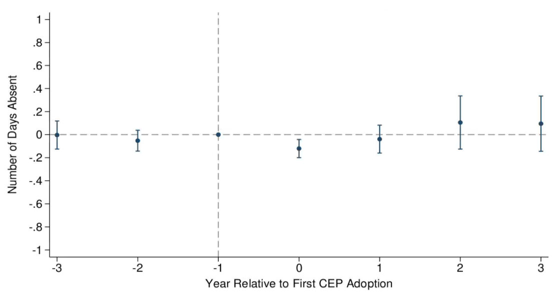
(c) Grade 3-8 Math Test Scores



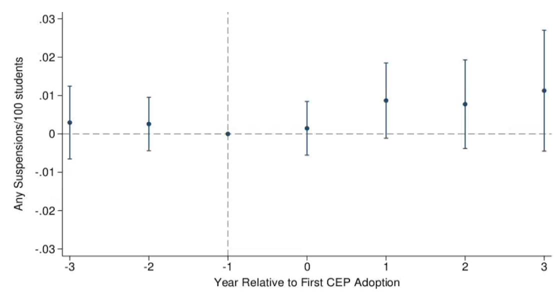
(d) High School Course Grades Index



(e) Number of Days Absent



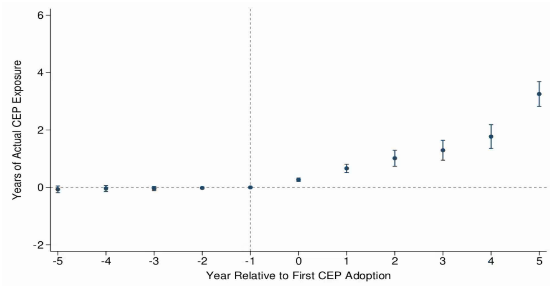
(f) Suspension Rate



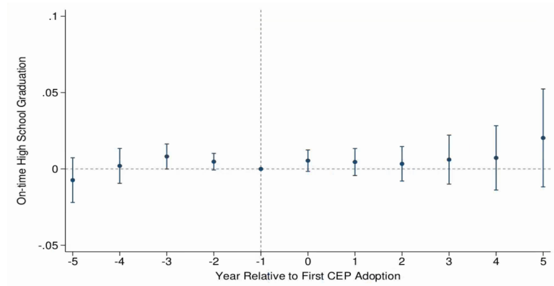
Notes: These graphs plot event-study estimates using the specifications in equation (2) for each of the eight main outcomes, employing the estimator developed by Callaway and Sant'Anna (2021). The sample includes schools in Texas that adopted CEP by 2018. All estimates are based on data collapsed into school × grade × year cells (school × year for meal participation and federal subsidies) and weighted by the number of observations per cell. The bars indicate 95 percent confidence intervals, based on standard errors obtained using a multiplier bootstrap procedure and clustered at the school level.

Figure 4: Event-Study Estimates of the Effects of CEP Exposure in School Districts That Ever Adopted CEP, Birth Cohorts 2000–2006

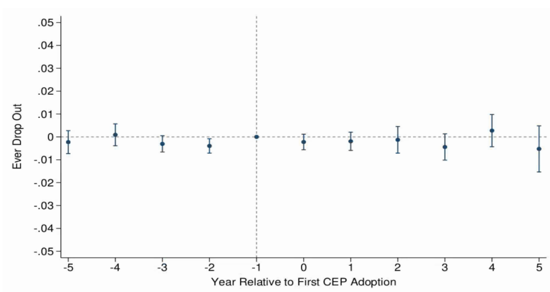
(a) Years of CEP Exposure



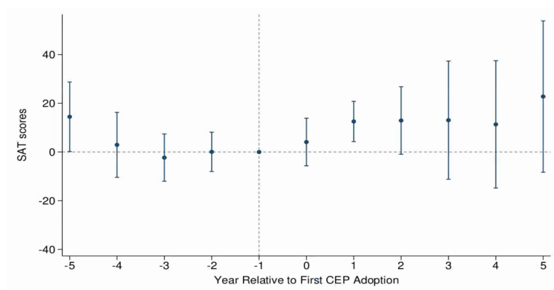
(b) High School Graduation



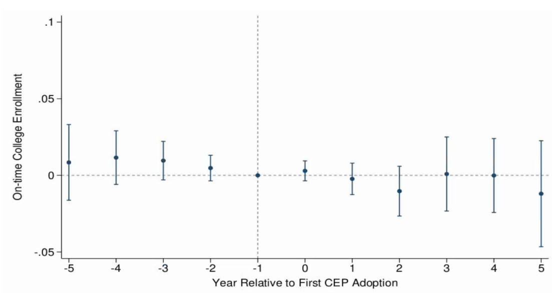
(c) High School Drop Out



(d) SAT Score



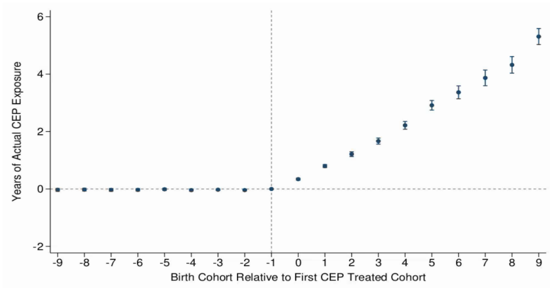
(e) College Enrollment



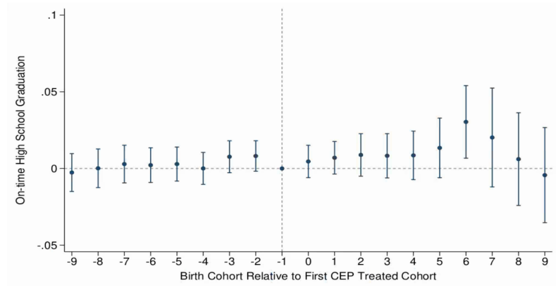
Notes: The figures plot event-study estimates using the specifications in equation (4) for each of the six main outcomes, using the estimator developed by Callaway and Sant'Anna (2021). For all outcomes except SAT scores and earnings, the sample includes children born between 2000 and 2006 in school districts in Texas that adopted CEP by 2024. The SAT score sample includes only students who took the SAT. For earnings at age 21, the sample includes only children who had reached age 21 by 2023. All estimates are based on data collapsed into district $\times$ cohort cells and weighted by the number of students per cell. The bars indicate 95 percent confidence intervals, based on standard errors obtained using a multiplier bootstrap procedure and clustered at the district level.

Figure 5: Event-Study Estimates of the Effects of CEP Exposure in School Districts That Were Eligible for CEP, Birth Cohorts 1986–2006

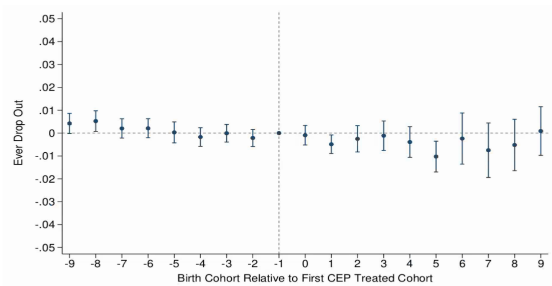
(a) Years of CEP Exposure



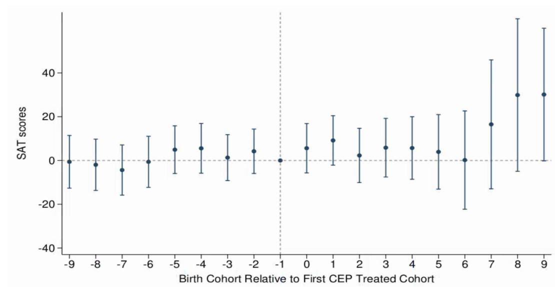
(b) High School Graduation



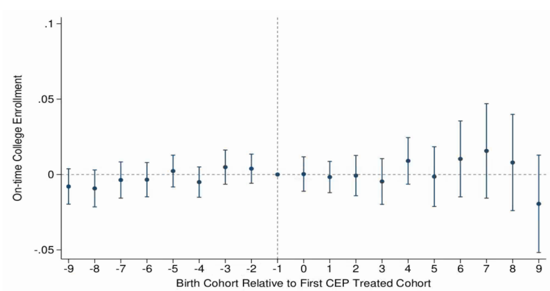
(c) High School Drop Out



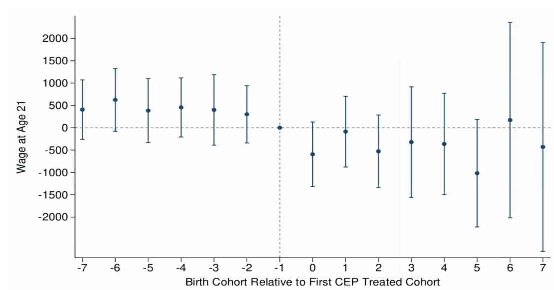
(d) SAT Score



(e) College Enrollment



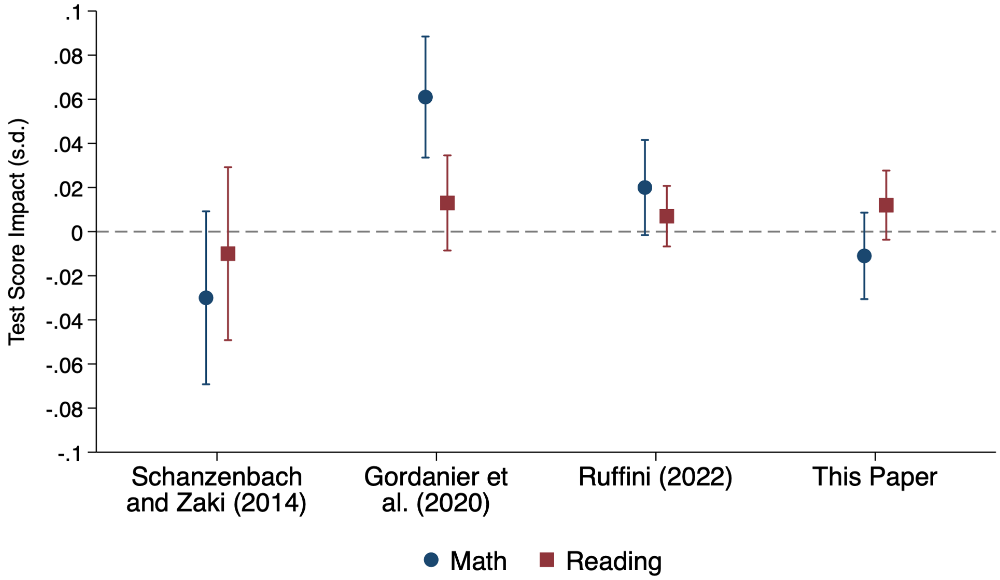
(f) Earnings at Age 21



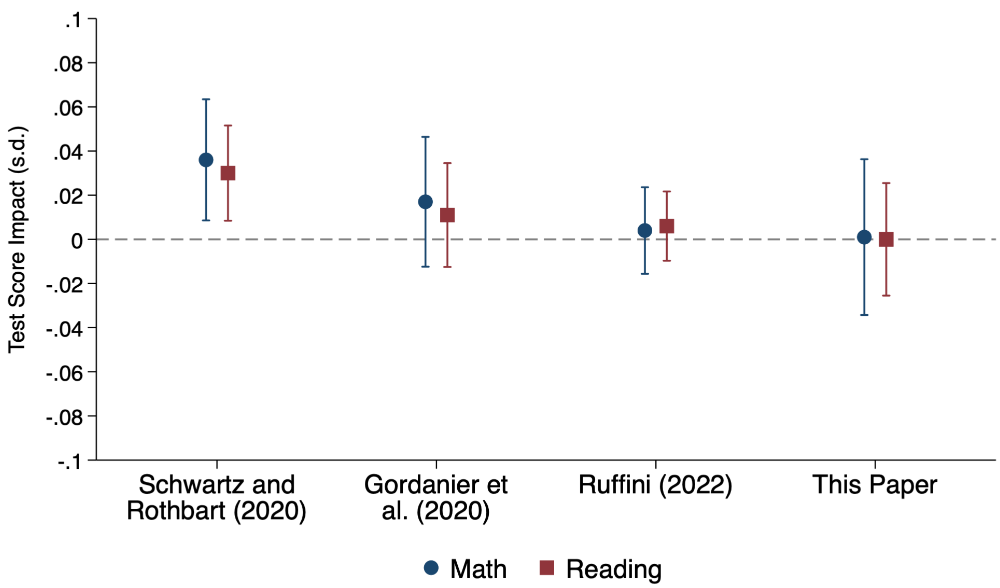
Notes: The figures plot event-study estimates using the specifications in equation (4) for each of the six main outcomes, using the estimator developed by Callaway and Sant'Anna (2021). For all outcomes except SAT scores and earnings, the sample includes children born between 1986 and 2006 in school districts in Texas that were eligible for CEP, with at least 40% of students eligible for free and reduced price meals. The SAT score sample includes only students who took the SAT. For earnings at age 21, the sample includes only children who had reached age 21 by 2023. All estimates are based on data collapsed into district × cohort cells and weighted by the number of students per cell. The bars indicate 95 percent confidence intervals, based on standard errors obtained using a multiplier bootstrap procedure and clustered at the district level.

Figure 6: The Magnitude of Universal Free School Meal's Effects on Test Scores

(a) Panel A: Effects on Grades 3-5 Test Scores

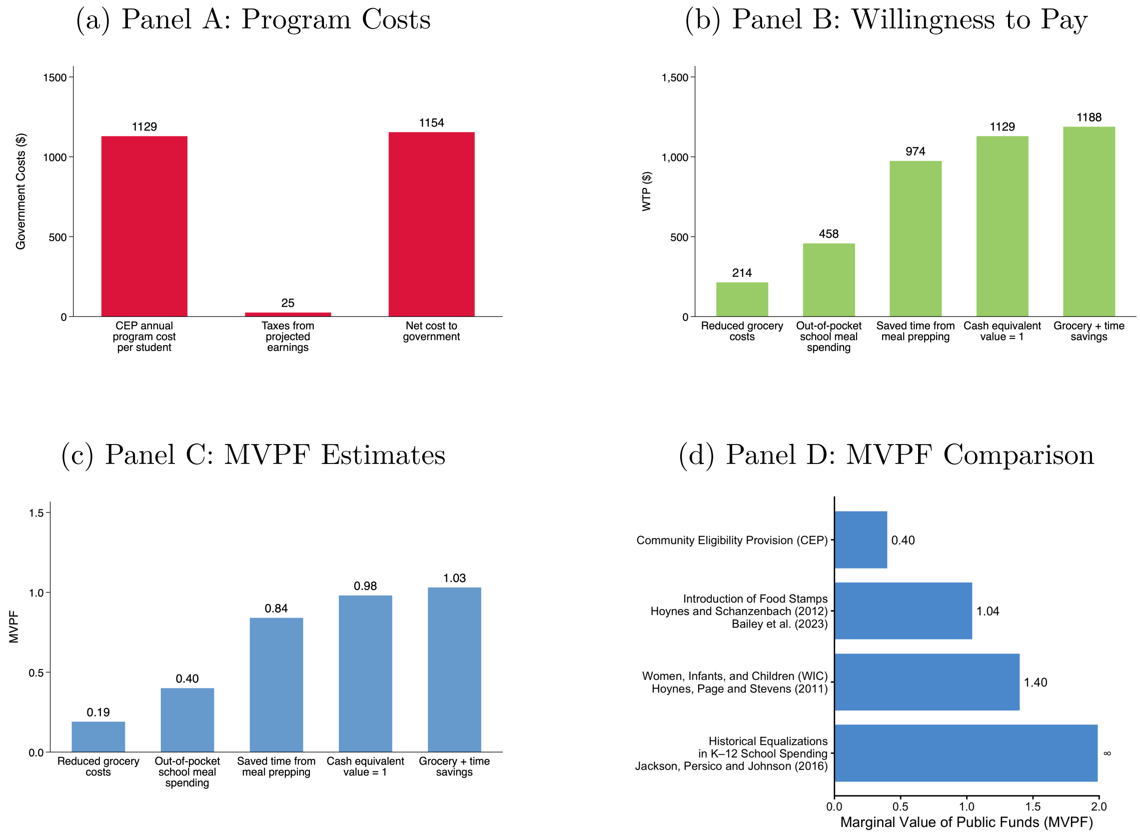


(b) Panel B: Effects on Grades 6-8 Test Scores



Notes: These studies examine the effects of universal free school meals in the United States. Schanzenbach and Zaki (2014) reanalyze a federally sponsored randomized study of 151 elementary schools in Bernstein et al. (2004) by breakfast location (classroom versus cafeteria). Other studies exploit the staggered adoption of Provision 2 or CEP. These studies differ in their specifications, treatment definitions, sample characteristics, and follow-up periods. See Table 12 for a summary of these studies.

Figure 7: Marginal Value of Public Funds



Notes: Program costs are measured as per-pupil annual spending. To calculate program cost, I scale the ITT estimate of \$70 per student per year by the first-stage meal take-up rate (ITT = 0.062). Willingness to pay (WTP) is measured as the estimated household valuation of school meals. I provide a range of estimates based on different assumptions. The grocery cost estimate is based on Handbury and Moshary (2021), adjusted to 2023 dollars. Out-of-pocket meal spending estimates are derived from Texas administrative data. The time-saving estimate is drawn from Guthrie and McClelland (2009). MVPF estimates for other programs are taken from Hendren and Sprung-Keyser (2020).

Table 1: Summary Statistics of Short-Term Sample

	Schools Adopted CEP			Schools Eligible for CEP		
	Mean	SD	N	Mean	SD	N
School characteristics						
Share eligible for free and reduced-price meals	0.80	0.16	1871	0.59	0.26	5574
Enrollment	636.31	422.98	1871	662.55	483.65	5574
Share of White	0.17	0.21	1871	0.38	0.28	5574
Share of Black	0.17	0.21	1871	0.13	0.16	5574
Share of Hispanic	0.63	0.29	1871	0.44	0.28	5574
Share of English language learner	0.28	0.23	1871	0.18	0.19	5574
Share enrolled in special education	0.09	0.07	1871	0.09	0.05	5574
Stand Alone Pre-K/Kindergarten	0.03	0.18	1871	0.02	0.13	5574
Elementary school	0.56	0.50	1871	0.49	0.50	5574
Middle school	0.24	0.43	1871	0.28	0.45	5574
High school	0.17	0.38	1871	0.20	0.40	5574
Urban	0.74	0.44	1871	0.64	0.48	5574
Meal Outcomes						
Breakfast participation rate	0.47	0.24	1871	0.35	0.24	5530
Lunch participation rate	0.76	0.18	1871	0.66	0.19	5568
Avg. breakfast and lunch participation rate	0.62	0.18	1871	0.50	0.20	5530
Test Scores						
Standardized Math Test Scores	-0.20	0.32	1496	0.02	0.38	4635
Standardized Reading Test Scores	-0.23	0.29	1496	0.02	0.37	4635
Test Scores Index	-0.21	0.29	1496	0.02	0.36	4635
Absenteeism						
Number of Days Absent	6.73	2.33	1816	6.38	1.90	5808
Share of Days Absent	0.04	0.01	1816	0.04	0.01	5808
Suspension rates						
In-school suspensions per 100 students	0.23	0.67	1817	0.21	0.50	5812
Out-of-school suspensions per 100 students	0.11	1.13	1817	0.06	0.53	5812
Any suspensions per 100 students	0.35	1.37	1817	0.27	0.85	5812

Notes: This table presents summary statistics for schools in Texas that adopted CEP by 2018 and for schools that were eligible for CEP. Schools that adopted CEP are a strict subset of those eligible for CEP. Eligibility is defined as having at least 40% of students eligible for free meals at either the school or district level. All outcomes are measured in 2011.

Table 2: Summary Statistics of Long-Term Sample

	Districts Adopted CEP			Districts Eligible for CEP		
	Mean	SD	N	Mean	SD	N
Student Characteristics						
Age in 2023	26.81	6.00	4,681,728	26.67	6.00	7,011,310
Male	0.51	0.50	4,681,728	0.51	0.50	7,011,310
White	0.30	0.46	4,681,728	0.36	0.48	7,011,310
Black	0.17	0.38	4,681,728	0.17	0.37	7,011,310
Hispanic	0.49	0.50	4,681,728	0.43	0.49	7,011,310
Asian	0.03	0.16	4,681,728	0.04	0.20	7,011,310
Home language not English	0.32	0.19	4,681,728	0.30	0.18	7,011,310
Never low-income	0.22	0.41	4,681,728	0.28	0.45	7,011,310
Transitorily low-income	0.46	0.50	4,681,728	0.44	0.50	7,011,310
Persistently low-income	0.32	0.47	4,681,728	0.28	0.45	7,011,310
Parents' Adjusted Gross Income	53,016	86,430	1,649,690	59,316	94,271	2,497,686
Educational Outcomes						
Taking SAT	0.16	0.37	4,681,728	0.17	0.37	7,011,310
SAT score	1,020.10	187.18	742,709	1,045.69	189.12	1,178,358
High school graduation ever	0.66	0.47	4,681,728	0.68	0.47	7,011,310
High school graduation on-time	0.58	0.49	4,681,728	0.60	0.49	7,011,310
High school drop out ever	0.08	0.28	4,681,728	0.07	0.26	7,011,310
College enrollment ever, any	0.46	0.50	4,681,728	0.47	0.50	7,011,310
College enrollment on-time, any	0.37	0.48	4,681,728	0.38	0.49	7,011,310
College enrollment on-time, 4-year public	0.13	0.34	4,681,728	0.14	0.35	7,011,310
College enrollment on-time, 2-year public	0.29	0.45	4,681,728	0.30	0.46	7,011,310
College enrollment on-time, 2-year private	0.03	0.16	4,681,728	0.03	0.16	7,011,310
Labor Market Outcomes						
Wage at age 20	8,302	22,849	3,995,134	8,282	20,969	5,946,173
Wage at age 24	14,086	29,723	3,070,444	14,704	29,557	4,524,513
Employment at age 20	0.46	0.50	3,995,134	0.46	0.50	5,946,173
Employment at age 24	0.42	0.49	3,070,444	0.42	0.49	4,524,513

Notes: This table presents summary statistics for school districts in Texas that adopted CEP and for districts that were eligible but did not adopt CEP by 2024. Districts that adopted CEP are a strict subset of those eligible for CEP. Eligibility is defined as having at least 40% of students eligible for free meals at the district level. Low-income is defined as eligible for free and reduced-price meals based on family income.

Table 3: Distribution of CEP Treatment Status Across Schools in Texas

Event time	Number of Schools	Number of Students	Percent of Schools	Percent of Students
2012	52	23,710	0.9	0.7
2013	244	162,790	4.2	4.5
2014	764	487,065	13.1	13.6
2015	150	88,622	2.6	2.5
2016	61	30,240	1.0	0.8
2017	287	172,083	4.9	4.8
2018	443	285,736	7.6	8.0
2019	364	223,609	6.3	6.2
2020	309	186,828	5.3	5.2
2021	150	109,325	2.6	3.1
2022	375	224,245	6.4	6.3
2023	348	240,927	6.0	6.7
Never treated	2,273	1,348,427	39.1	37.6
Total	5,820	3,583,607	100.0	100.0

Notes: The table reports the number of schools, students, and the shares of schools and enrollment eligible for the Community Eligibility Provision (CEP) in Texas. Eligibility is defined as having at least 40% of students who are income-eligible for free meals at either the school or district level. Event time refers to the first year a school adopted a universal meal policy, with years indicating the start of the corresponding school year. Universal meal policies include Provision 2 (before 2014) and the Community Eligibility Provision (CEP, since 2014). For schools that implemented universal breakfast before universal lunch, event time is based on the year of universal lunch adoption. Schools that implemented universal meals before 2012 are excluded because their exact adoption year is unavailable.

Table 4: Distribution of CEP Treatment Status Across School Districts in Texas

Event Time	Number of Districts	Number of Students	Percent of Districts	Percent of Students
2012	12	16,286	1.4	0.4
2013	7	162,865	0.8	4.4
2014	58	708,055	7.0	19.2
2015	16	172,926	1.9	4.7
2016	10	25,443	1.2	0.7
2017	40	122,127	4.8	3.3
2018	62	200,794	7.5	5.5
2019	53	298,463	6.4	8.1
2020	49	129,035	5.9	3.5
2021	25	108,993	3.0	3.0
2022	66	140,782	8.0	3.8
2023	52	213,280	6.3	5.8
2024	47	76,106	5.7	2.1
Never treated	331	1,309,452	40.0	35.5
Total	828	3,684,607	100.0	100.0

Notes: The table presents the number of school districts, the number of students, and the percentage of school districts and enrollment for schools that first adopted universal school meals in different years. Universal school meal policies include Provision 2 (before 2014) and the Community Eligibility Provision (CEP, since 2014). The table includes districts in which at least 40% of students are income-eligible for free meals. About 30% of treated districts have more than one event time. For these districts, I define the event time as the one associated with the largest enrollment within the district. In about 70 school districts, only a subset of schools (primarily elementary schools) are treated. I classify partially treated districts with fewer than 50% of students in CEP schools as untreated, and those with at least 50% as treated.

Table 5: Estimated Effects of CEP on Meal Take-Up and Federal Subsidies

	Meal Take-Up	Lunch Take-Up	Breakfast Take-Up	Annual Federal Subsidies per Student	Annual Food Revenues per Student
	(1)	(2)	(3)	(4)	(5)
CEP	0.062***	0.046***	0.077***	69.968***	29.732***
	(0.004)	(0.003)	(0.006)	(3.738)	(6.790)
Mean of Dependent Variable	0.62	0.76	0.47	598.26	478.54
Number of Observations	13,086	13,086	13,089	13,090	12,807
Number of Schools	1,871	1,871	1,871	1,871	1,795

Notes: This table presents estimates from a specification that compares earlier-treated schools to later-treated schools. Take-up is defined as the share of students eating school meals on a typical school day. The coefficients are averaged over event times 0 to 3. The sample is based on schools that adopted CEP between 2012 and 2018. The data are at the school level, and the specifications are weighted by the number of students in each school. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications, clustered at the school level.

Table 6: Estimated Effects of CEP on Meal Take-Up by School Grade, School Poverty, and Student FRP Eligibility

	Student FRP Eligibility		School Grade				School Poverty Quantile			
	Students Eligible for FRP	Students Ineligible for FRP	Pre-K/ Kindergarten	Elementary School	Middle School	High School	First Quantile	Second Quantile	Third Quantile	Fourth Quantile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CEP	0.079***	0.148***	0.064***	0.057***	0.076***	0.055***	0.055***	0.050***	0.056***	0.085***
	(0.011)	(0.015)	(0.016)	(0.005)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.006)
Mean of the Dependent Variable	.60	.48	.66	.68	.60	.43	.73	.66	.59	.49
Number of Observations	748	748	418	7275	3178	2215	3267	3272	3275	3272
Number of Schools	244	244	60	1040	454	317	467	468	468	468

Notes: Free and reduced-price (FRP) meal eligibility is defined as $\leq$ 185% of the federal poverty line (about $60,000 for a family of four). The subgroup analysis by student FRP eligibility is based on school $\times$ year $\times$ FRP eligibility group data from a sample of 244 schools for which such data are available. The subgroup analyses by school grade and by school quartile are based on separate estimates using the estimator developed by Callaway and Sant'Anna (2021). All estimates are weighted by the number of observations per cell. The bars indicate 95 percent confidence intervals, based on standard errors obtained using a multiplier bootstrap procedure and clustered at the school level.

Table 7: Estimated Effects of CEP on Pre-K Assessments

	Summary Index	Reading	Writing	Math	Communication & Language	Physical & Mental Health
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pre-K						
CEP	0.014	0.021*	0.007	-0.004	0.021*	0.031**
	(0.010)	(0.013)	(0.016)	(0.015)	(0.011)	(0.013)
Mean of the Dependent Variable	.69	.35	.84	.74	.55	.95
Number of Observations	1945	2630	2083	2424	2612	2160
Number of Students	27593	33837	25852	31200	33948	28670
Number of Schools	482	613	504	565	611	529

Notes: This table presents estimates from a specification that compares earlier-treated schools to later-treated schools. The coefficients are averaged over event times 0 to 3. The sample is based on schools that adopted CEP between 2018 and 2023. The Pre-K readiness score is the end-of-grade score for 4-year-olds. The Pre-K data include only public Pre-K programs, and about 90 percent of children in the sample are income-eligible for free and reduced-price meals. The data are collapsed into cells at the school–grade–subject level, and the specifications are weighted by the number of students in each cell. Not all subjects are administered by all schools, so sample sizes vary by subject. The summary index for Pre-K includes five subjects, while the summary index for kindergarten includes only reading and communication. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications, clustered at the school level.

Table 8: Grades 3-8 Test Scores, Absenteeism, and Suspensions

	Full Sample (1)	Elementary School (2)	Middle School (3)	High School (4)
Panel A: Grade 3–8 Reading				
CEP	0.008	0.012	-0.000	
	(0.007)	(0.008)	(0.013)	
Mean of Dependent Variable	-0.23	-0.22	-0.29	
Panel B: Grade 3–8 Math				
CEP	-0.006	-0.011	0.001	
	(0.010)	(0.010)	(0.018)	
Mean of Dependent Variable	-0.21	-0.18	-0.28	
Panel C: Grade 3–8 Test Score Index				
CEP	0.001	0.000	0.000	
	(0.008)	(0.008)	(0.015)	
Mean of Dependent Variable	-0.22	-0.20	-0.28	
Sample size for Panels A–C				
Number of Observations	31,395	23,394	8,001	
Number of Students	157,960	91,123	74,114	
Number of Schools	1,521	1,180	460	
Panel D: Number of Days Absent				
CEP	0.060	0.060	0.210*	-0.062
	(0.077)	(0.039)	(0.114)	(0.280)
Mean of the Dependent Variable	6.45	5.86	6.8	9.55
Number of Observations	64,323	47,733	8,477	8,113
Number of Students	285,150	129,280	94,150	75,886
Number of Schools	1,878	1,280	484	302
Panel E: Suspension Rates				
CEP	0.017*	0.005	-0.009	0.064*
	(0.009)	(0.007)	(0.017)	(0.035)
Mean of the Dependent Variable	0.39	0.20	1.03	0.89
Number of Observations	64,414	47,775	8,498	8,141
Number of Students	285,541	129,430	94,208	76,089
Number of Schools	1,879	1,281	485	303

Notes: This table presents estimates from a specification that compares earlier-treated schools with later-treated schools. The coefficients are averaged over event times 0 to 3. The sample includes schools that adopted CEP between 2012 and 2018. The data are collapsed into cells at the school–grade level, and the specifications are weighted by the number of students in each cell. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications and are clustered at the school level.

Table 9: Short-Term Outcomes by Student Characteristics

	Race				Gender		Low-income Status		
	White	Black	Hispanic	Other	Girls	Boys	Never Low-income	Transitorily Low-income	Persistently Low-income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Grade 3-8 Reading									
CEP	-0.004	-0.038***	0.006	0.010	0.008	0.009	0.007	0.001	-0.000
	(0.011)	(0.012)	(0.008)	(0.023)	(0.007)	(0.008)	(0.016)	(0.007)	(0.008)
Mean of the Dependent Variable	.1	-.39	-.26	.08	-.15	-.32	.43	-.2	-.38
Panel B: Grade 3-8 Math									
CEP	-0.013	-0.040***	-0.002	-0.030	-0.004	-0.007	-0.013	-0.011	-0.009
	(0.014)	(0.014)	(0.012)	(0.024)	(0.011)	(0.010)	(0.022)	(0.010)	(0.011)
Mean of the Dependent Variable	.05	-.47	-.2	.18	-.2	-.22	.37	-.2	-.31
Panel C: Grade 3-8 Test Score Index									
CEP	-0.008	-0.039***	0.002	-0.010	0.002	0.001	-0.003	-0.005	-0.005
	(0.011)	(0.012)	(0.009)	(0.022)	(0.008)	(0.008)	(0.018)	(0.008)	(0.009)
Mean of the Dependent Variable	.07	-.43	-.23	.13	-.17	-.27	.4	-.2	-.34
Sample size for Panels A–C									
Number of Observations	17969	19873	30289	9002	31185	31241	11389	31311	30611
Number of Students	51520	68493	278639	8618	201663	211956	25296	218347	166151
Number of Schools	990	1060	1494	570	1517	1519	652	1519	1510
Panel E: Number of Days Absent									
CEP	0.099	0.128	0.018	-0.003	0.050	0.070	-0.080	0.068	0.066
	(0.074)	(0.196)	(0.074)	(0.146)	(0.077)	(0.081)	(0.093)	(0.089)	(0.075)
Mean of the Dependent Variable	7.42	6.84	6.23	6.03	6.42	6.44	5.32	6.53	6.66
Number of Observations	41727	43162	62342	23128	63917	64078	28462	64113	63049
Number of Students	144303	189133	734152	27904	539688	568582	82221	569133	449468
Number of Schools	1468	1437	1858	1009	1874	1875	1113	1875	1874
Panel F: Suspension Rates									
CEP	0.334***	-0.071	0.050***	-0.118	0.037**	0.031	0.155	0.058***	0.085***
	(0.116)	(0.087)	(0.013)	(0.343)	(0.016)	(0.022)	(0.122)	(0.022)	(0.024)
Mean of the Dependent Variable	3.55	4.78	.78	7.56	.42	1.04	1.91	.75	1.35
Number of Observations	41811	43225	62391	23142	63980	64169	28616	64190	63056
Number of Students	144578	189702	736038	27961	541082	570011	82482	571045	450123
Number of Schools	1469	1438	1859	1010	1875	1876	1116	1876	1875

Notes: This table presents estimates from a specification that compares earlier-treated schools with later-treated schools. The coefficients are averaged over event times 0 to 3. The sample includes schools that adopted CEP between 2012 and 2018. The data are collapsed into cells at the school–grade–race/gender/low-income status level, and the specifications are weighted by the number of students in each cell. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications and are clustered at the school level.

Table 10: Estimated Effects on Long-Term Outcomes, Birth Cohorts 1986 – 2006

	Years of Actual CEP Exposure (1)	On-time High School Graduation (2)	Ever Drop Out (3)	Taking SAT (4)	SAT scores (5)	On-time College Enrollment (6)	Wage at Age 21 (7)	Employment at Age 21 (8)
Panel A Within Specification								
Exposed in Grades 9-12	0.684***	0.008***	-0.003**	0.005	6.234**	-0.000	-24.242	-0.004
	(0.078)	(0.003)	(0.001)	(0.004)	(3.174)	(0.003)	(271.538)	(0.009)
Exposed in Grades 6-8	2.128***	0.017**	0.001	0.023**	8.047	-0.000	-214.810	-0.017
	(0.190)	(0.008)	(0.003)	(0.010)	(0.956)	(0.007)	(468.279)	(0.019)
Exposed in Grades 3-5	3.774***	0.020	0.010*	0.009	9.856	0.006	-761.464	-0.081
	(0.328)	(0.014)	(0.006)	(0.015)	(2.223)	(0.013)	(1,343.917)	(0.061)
Mean of the Dependent Variable	.67	.63	.05	.11	1021.39	.42	11210.69	.48
Number of Observations	10341	10349	10349	10349	9099	10349	9202	9202
Number of Students	4681713	4681728	4681728	4681728	742310	4681728	3762617	3762617
Number of Districts	497	497	497	497	492	497	497	497
Panel B Matching Specification								
Exposed in Grades 9-12	0.932***	0.007	-0.002	-0.002	5.870	-0.001	-391.157	-0.011
	(0.030)	(0.005)	(0.002)	(0.004)	(4.577)	(0.005)	(293.336)	(0.008)
Exposed in Grades 6-8	2.724***	0.015*	-0.006*	-0.004	3.838	0.006	-449.129	-0.026*
	(0.078)	(0.008)	(0.003)	(0.006)	(0.861)	(0.008)	(553.741)	(0.015)
Exposed in Grades 3-5	4.430***	0.008	-0.004	-0.006	24.793**	0.003	-1,068.861	-0.024
	(0.129)	(0.014)	(0.005)	(0.011)	(12.632)	(0.014)	(1,100.560)	(0.039)
Mean of the Dependent Variable	.48	.64	.05	.11	1030.6	.43	10977.88	.47
Number of Observations	17111	17140	17140	17140	14542	17140	12869	12869
Number of Students	7011239	7011310	7011310	7011310	1176744	7011310	5381593	5381593
Number of Districts	828	828	828	828	778	828	698	698

Notes: The table presents event-study estimates using the specifications in equation (5) for each of the main outcomes, using the estimator developed by Callaway and Sant'Anna (2021). For all outcomes except SAT scores and earnings, the sample includes children born between 1986 and 2006 in school districts in Texas that adopted CEP by 2024. The SAT score sample includes only students who took the SAT. For earnings at age 21, the sample includes only children who had reached age 21 by 2023. All estimates are based on data collapsed into district $\times$ cohort cells and weighted by the number of students per cell. The standard errors obtained using a multiplier bootstrap procedure and clustered at the district level.

Table 11: Estimated Effects on Long-Term Outcomes, Birth Cohorts 2000-2006

	Years of Actual CEP Exposure	On-time High School Graduation	Ever Drop Out	Taking SAT	SAT scores	On-time College Enrollment
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A Within Specification						
Exposed in Grades 9-12	0.732***	0.005	-0.002	-0.005	0.952*	-0.002
	(0.084)	(0.005)	(0.002)	(0.005)	(0.914)	(0.006)
Exposed in Grades 6-8	2.180***	0.011	0.001	-0.018**	4.174	-0.003
	(0.239)	(0.011)	(0.004)	(0.009)	(1.961)	(0.012)
Mean of the Dependent Variable	.95	.66	.04	.12	1072.7	.44
Number of Observations	2478	2478	2478	2478	2221	2478
Number of Students	767886	767886	767886	767886	117970	767886
Number of Districts	354	354	354	354	344	354
Panel B Matching Specification						
Exposed in Grades 9-12	0.879***	0.006	-0.004*	-0.002	8.869*	-0.001
	(0.038)	(0.006)	(0.002)	(0.004)	(5.372)	(0.006)
Exposed in Grades 6-8	2.559***	0.004	-0.004	-0.016*	5.477	0.004
	(0.104)	(0.011)	(0.004)	(0.008)	(2.975)	(0.011)
Mean of the Dependent Variable	.70	.66	.04	.12	1078.31	.44
Number of Observations	4791	4792	4792	4179	4179	4792
Number of Students	1656889	1656890	1656890	282232	1656890	681597
Number of Districts	685	685	685	685	642	685

Notes: The table presents event-study estimates using the specifications in equation (5) for each of the main outcomes, using the estimator developed by Callaway and Sant'Anna (2021). For all outcomes except SAT scores and earnings, the sample includes children born between 2000 and 2006 in school districts in Texas that adopted CEP by 2024. The SAT score sample includes only students who took the SAT. For earnings at age 21, the sample includes only children who had reached age 21 by 2023. All estimates are based on data collapsed into district × cohort cells and weighted by the number of students per cell. The standard errors obtained using a multiplier bootstrap procedure and clustered at the district level.

Table 12: Estimates of Universal Free School Meals on Take-Up and Test Scores Across Studies

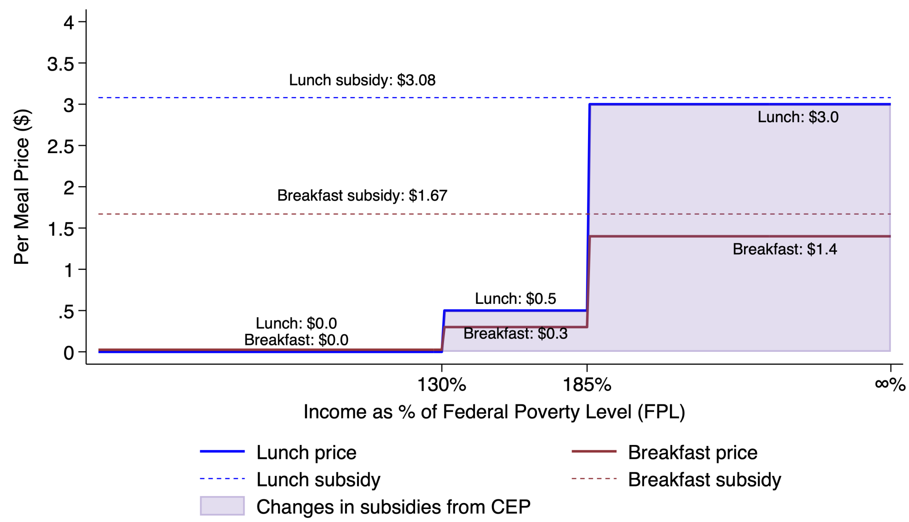
Paper	Context	Years	Grades	Policy	Treatment definition	Specification	Sample Characteristics	Effects on meals per student per year	Effects on federal subsidies per student per year	Math	Reading
Schanzenbach and Zaki (2014)	6 school districts in 6 states	1999–2003	2 to 6	breakfast	student	RCT	54% FRP, 38% non-White	34 breakfasts	\$75	−0.03 (0.02)	−0.01 (0.02)
Leos-Urbel et al. (2013)	New York City	2002–2003	3 to 8	breakfast	student	TWFE, school FE	85% FRP 80% non-White	6 breakfasts	L: \$13 H: \$13	L: 0.01 (0.01) H: 0.01 (0.01)	L: 0.02 (0.01) H: −0.01 (0.02)
Schwartz and Rothbart (2020)	New York City	2010–2013	6 to 8	lunch	student	TWFE, student FE	92% FRP 88% non-White	L: 10 lunches H: 20 lunches	L: \$35 H: \$70	L: 0.032 (0.014) H: 0.083 (0.025)	L: 0.027 (0.011) H: 0.059 (0.021)
Ruffini (2022)	Nationwide/9 states	2009–2017	3 to 8	CEP	district	TWFE, district FE	H: 45% FRP, 24% non-White F: 49% FRP, 40% non-White	20 breakfasts 13 lunches	\$89.50	H: 0.016 (0.009) F: 0.002 (0.01)	H: 0.007 (0.006) F: -0.012 (0.005)
Gordanier et al. (2020)	South Carolina	2014–2016	3 to 5	CEP	student	TWFE, school FE	62.3% FRP 46.5% non-White	29 lunches	\$101.50	0.061 (0.014)	0.015 (0.011)
This paper	Texas	2011–2018	3 to 8	CEP	school	Callaway and Sant'Anna	80% FRP 83% non-White	14 breakfasts and 8 lunches	\$70	−0.006 (0.01)	0.008 (0.007)

Notes: RCT = "randomized controlled trial." CEP = "Community Eligibility Provision." TWFE = "two-way fixed effects." FRP = "free and reduced-price" meals. In Leos-Urbel et al. (2013) and Schwartz and Rothbart (2020), "L" indicates the sample of low-income students who are income-eligible for free and reduced-price (FRP) meals, while "H" indicates the sample of higher-income students who are not income-eligible for FRP. In Ruffini (2022), "F" refers to the full sample of districts with at least one school that adopted CEP, and "H" refers to the subsample of CEP-participating districts with a share of students eligible for FRP below the median of the full sample (58 percent). Schanzenbach and Zaki (2014) and Schwartz and Rothbart (2020) do not report estimated effects on the number of meals served but instead report the average daily participation rate (ADP). I convert the ADP to the number of meals by multiplying it by 180 school days. The estimated federal subsidies are calculated using per-meal subsidy rates for the 2019–2020 school year: \$3.50 per lunch and \$2.20 per breakfast. I use 2019 rates because most of the cited studies use data from before the COVID-19 pandemic, and subsidy levels increased substantially afterward.

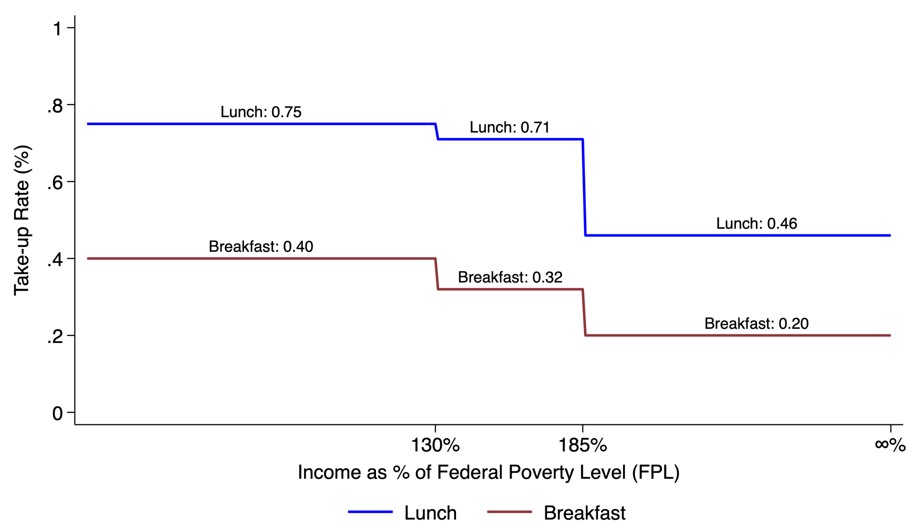
A Appendix Figures and Tables

Figure A1: School Meal Prices, Subsidies, and Take-Up in Texas

(a) Panel A: School Meal Prices and Subsidies

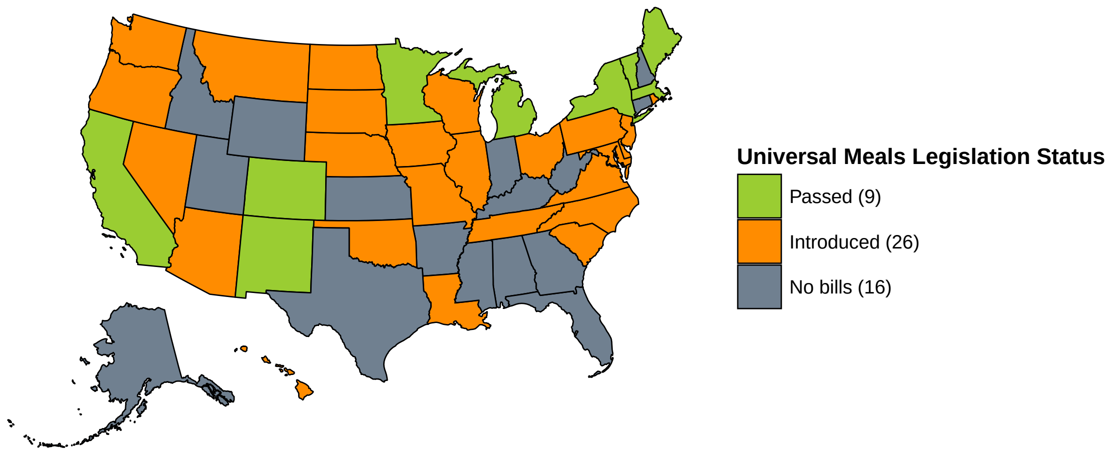


(b) Panel B: School Meal Take-Up Prior to CEP



Notes: Panel A presents average school meal prices and subsidies in Texas schools from 2011–2019 by student family income. Panel B reports the average daily participation rate in 2011 by student family income among Texas schools that were eligible for CEP. Students with family incomes at or below 130 percent of the federal poverty line (FPL) qualify for free meals, while those with incomes between 130 and 185 percent of the FPL are eligible for reduced-price meals. Students with family incomes above 185 percent of the FPL pay full price. Maximum subsidies for breakfast and lunch are set at the federal level and adjusted annually for cost of living.

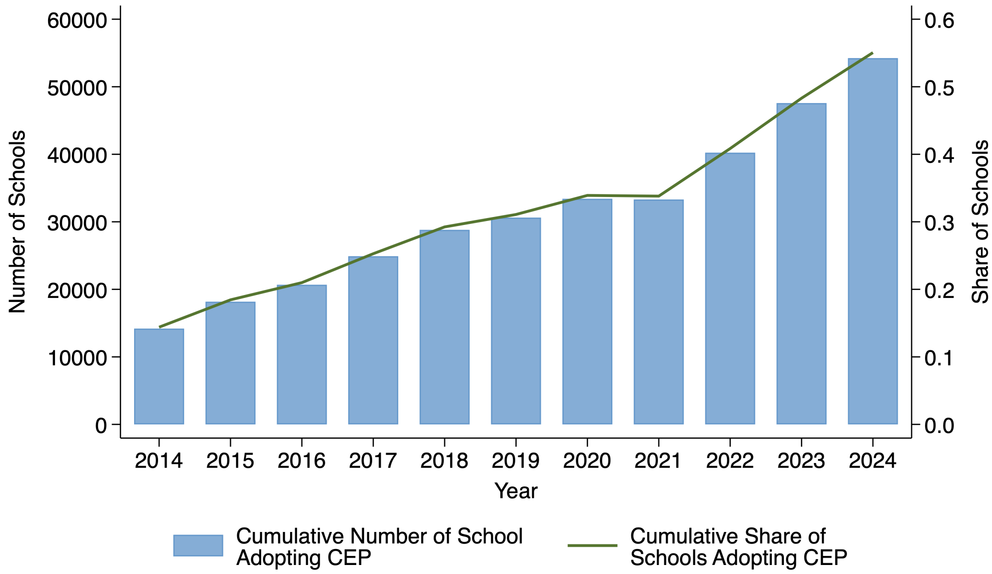
Figure A2: State Universal Free School Meal Legislation Status



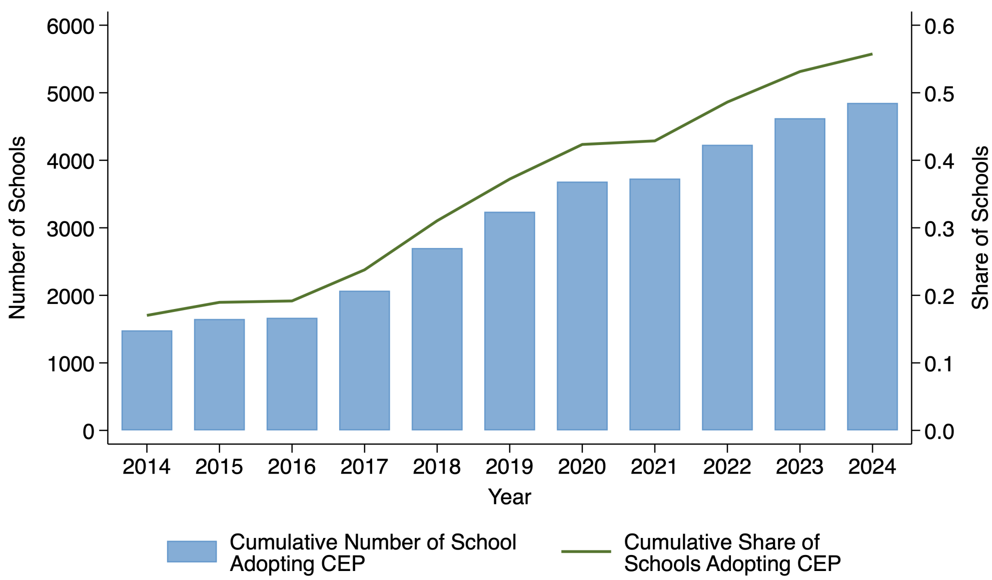
Notes: This graph builds on data collected by the Food Research and Action Center and updated through 2025.

Figure A3: Cumulative Number and Share of Schools Adopting CEP

(a) Panel A: United States, 2014-2024



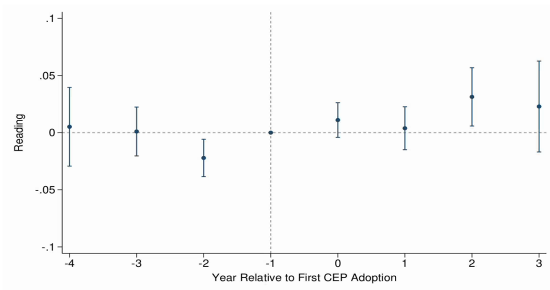
(b) Panel B: Texas, 2014-2024



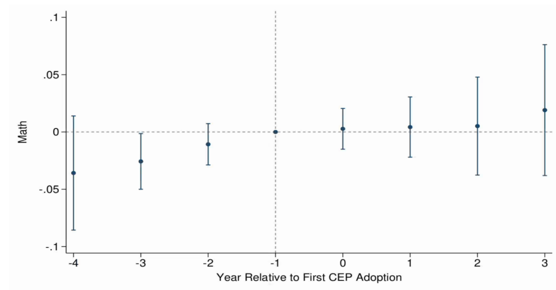
Notes: Panel A displays the cumulative number and percentage of U.S. public schools adopting CEP from 2014–2024, using data from Hysom and Bylander (2025). Panel B reports the corresponding figures for Texas, using data from the Texas Department of Agriculture.

Figure A4: Pre-K Outcome Event Study

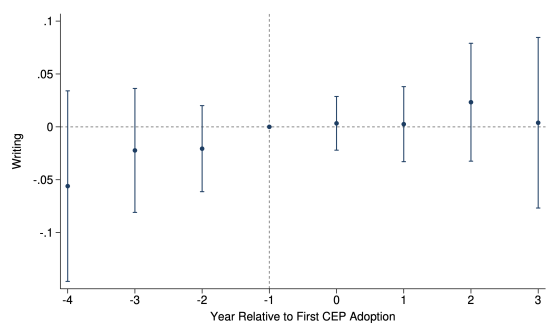
(a) Reading



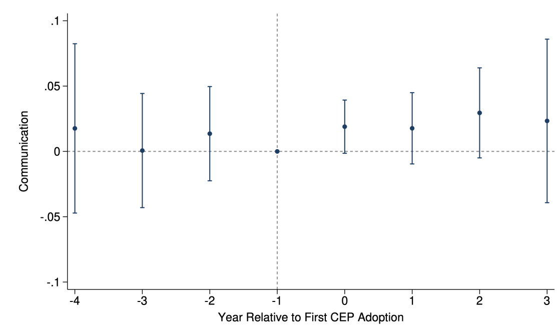
(b) Math



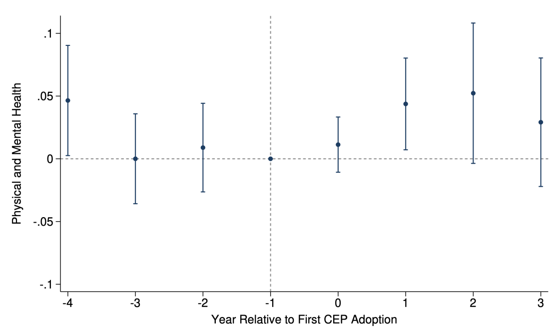
(c) Writing



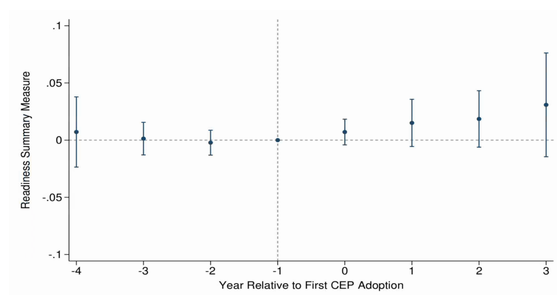
(d) Communication and Language



(e) Physical and Mental Health



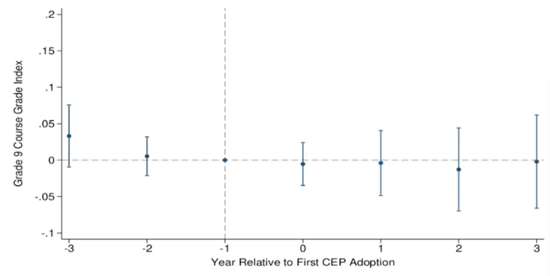
(f) Summary Index



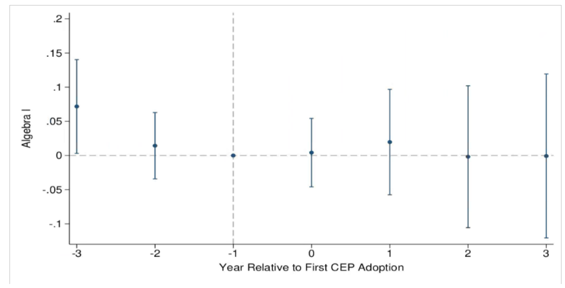
Notes: These graphs plot event-study estimates using the specifications in equation (2) for each of the main outcomes, employing the estimator developed by Callaway and Sant'Anna (2021) and a specification that compares earlier-treated schools with later-treated schools. The sample includes schools that adopted CEP between 2018 and 2023. The Pre-K readiness score represents the end-of-year assessment for four-year-olds. The Pre-K data include only public Pre-K programs, and approximately 90 percent of children in the sample are income-eligible for free and reduced-price meals. The data are collapsed into cells at the school–grade–subject level, and the specifications are weighted by the number of students in each cell. The summary index for Pre-K includes five subjects. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications and are clustered at the school level.

Figure A5: Course Grades for Grade 9 Students

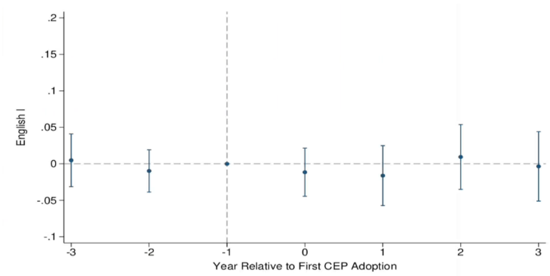
(a) Reading



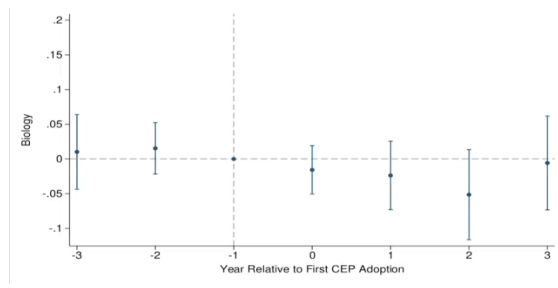
(b) Algebra I



(c) English



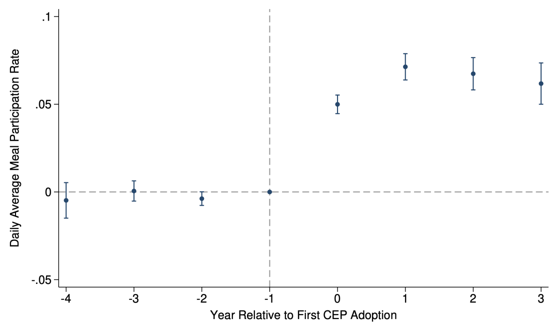
(d) Biology



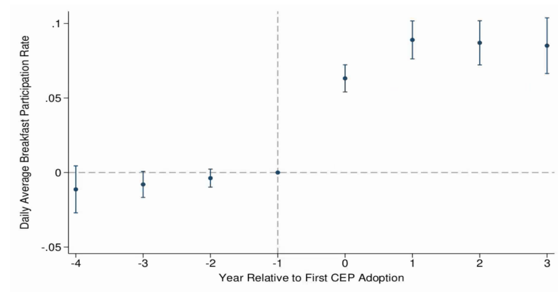
Notes: These graphs plot event-study estimates using the specifications in equation (2) for each of the four outcomes, employing the estimator developed by Callaway and Sant'Anna (2021). The sample includes high schools in Texas that adopted CEP by 2018. Regressions are estimated on data collapsed into school × grade × year cells and weighted by the number of observations per cell. The bars indicate 95 percent confidence intervals, based on standard errors obtained using a multiplier bootstrap procedure and clustered at the school level. All scores are standardized by grade, year, and subject.

Figure A6: Robustness Checks for School Meal Participation and Subsidies

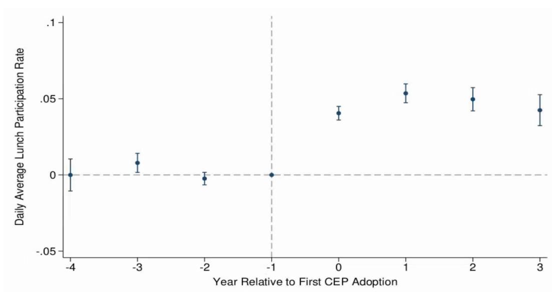
(a) Meal Participation (Breakfast + Lunch)



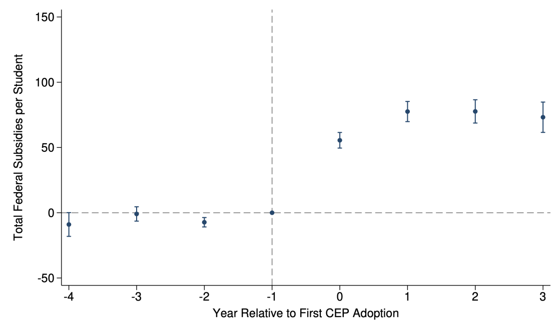
(b) Breakfast Participation



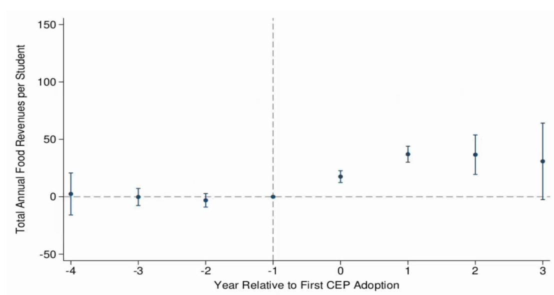
(c) Lunch Participation



(d) Federal Subsidies per Student



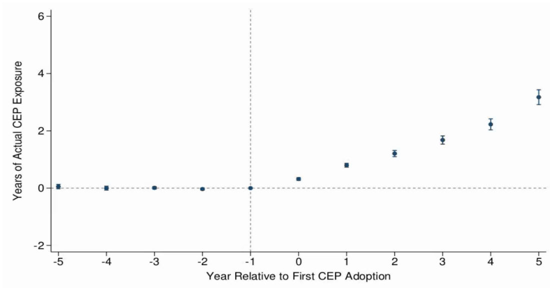
(e) Net Food Revenues per Student



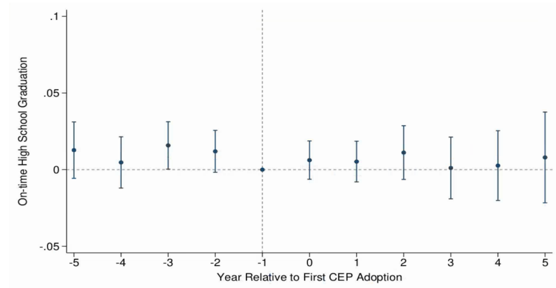
Notes: These graphs plot event-study estimates using the matching specifications for each of the first five main outcomes, employing the estimator developed by Callaway and Sant'Anna (2021). The sample includes schools in Texas that adopted CEP by 2018. All estimates are based on data collapsed into school × year and weighted by the number of observations per cell. All estimates are based on data collapsed into district × year cells and weighted by the number of students per cell. The bars indicate 95 percent confidence intervals, based on standard errors obtained using a multiplier bootstrap procedure and clustered at the school level.

Figure A7: Event-Study Estimates of the Effects of CEP Exposure in School Districts That Were Eligible for CEP, Birth Cohorts 2000–2006

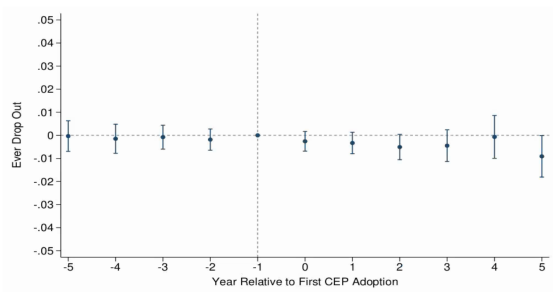
(a) Years of CEP Exposure



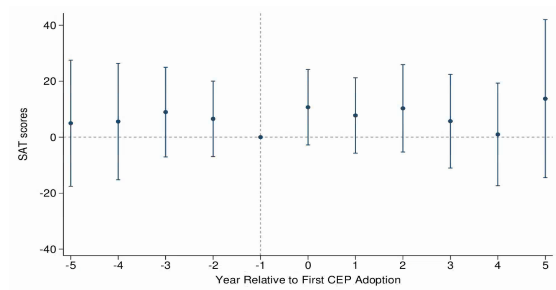
(b) High School Graduation



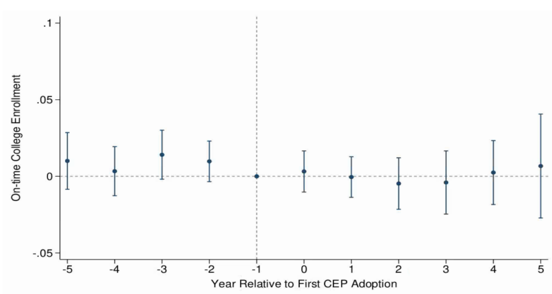
(c) High School Drop Out



(d) SAT Score



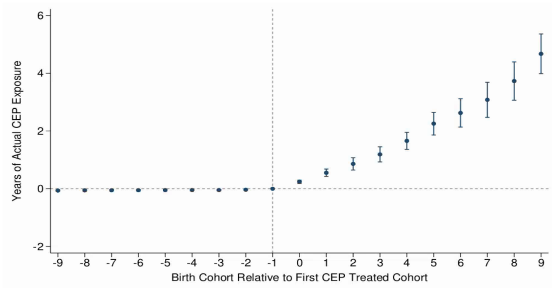
(e) College Enrollment



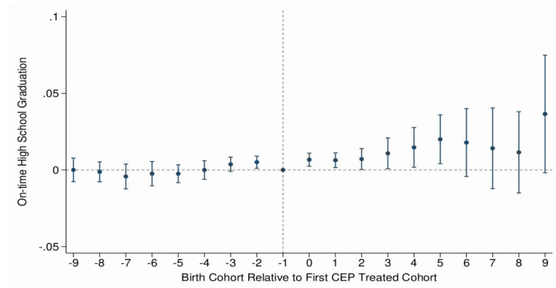
Notes: The figures plot event-study estimates using the specifications in equation (4) for each of the six main outcomes, using the estimator developed by Callaway and Sant'Anna (2021). For all outcomes except SAT scores and earnings, the sample includes children born between 2006 and 2006 in school districts in Texas that were eligible for CEP, with at least 40% of students eligible for free and reduced price meals. The SAT score sample includes only students who took the SAT. For earnings at age 21, the sample includes only children who had reached age 21 by 2023. All estimates are based on data collapsed into district $\times$ cohort cells and weighted by the number of students per cell. The bars indicate 95 percent confidence intervals, based on standard errors obtained using a multiplier bootstrap procedure and clustered at the district level.

Figure A8: Event-Study Estimates of the Effects of CEP Exposure in School Districts That Ever Adopted CEP, Birth Cohorts 1986–2006

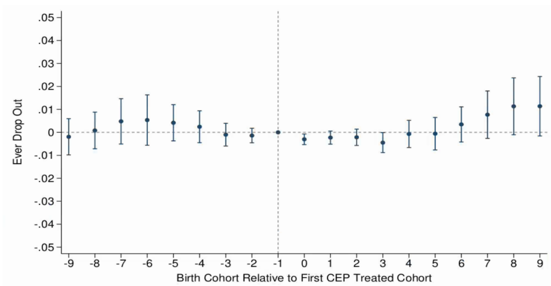
(a) Years of CEP Exposure



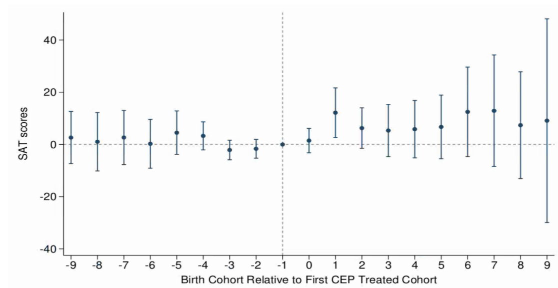
(b) High School Graduation



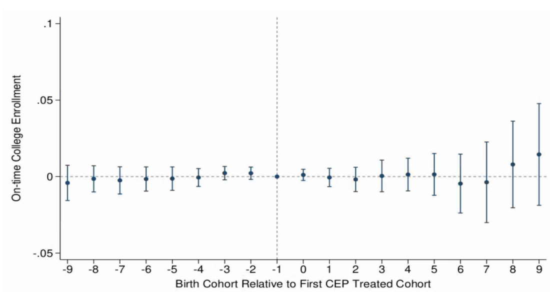
(c) High School Drop Out



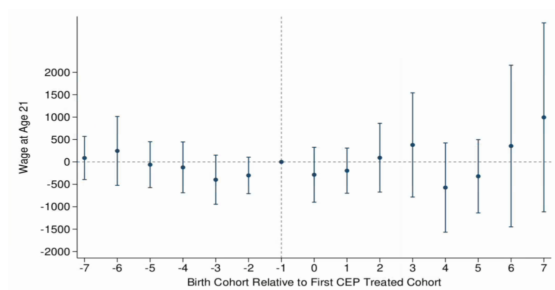
(d) SAT Score



(e) College Enrollment



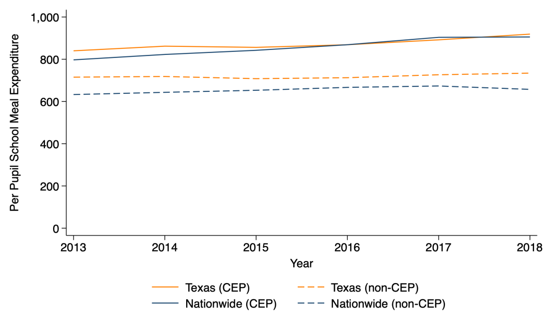
(f) Earnings at Age 21



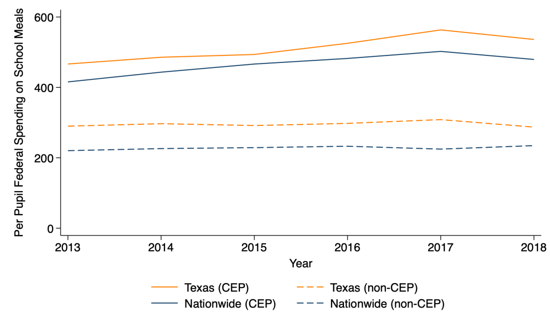
Notes: The figures plot event-study estimates using the specifications in equation (4) for each of the six main outcomes, using the estimator developed by Callaway and Sant'Anna (2021). For all outcomes except SAT scores and earnings, the sample includes children born between 1986 and 2006 in school districts in Texas that adopted CEP by 2024. The SAT score sample includes only students who took the SAT. For earnings at age 21, the sample includes only children who had reached age 21 by 2023. All estimates are based on data collapsed into district $\times$ cohort cells and weighted by the number of students per cell. The bars indicate 95 percent confidence intervals, based on standard errors obtained using a multiplier bootstrap procedure and clustered at the district level.

Figure A9: District-Level Expenditures by CEP Status: Texas vs. Nationwide, 2013–2018

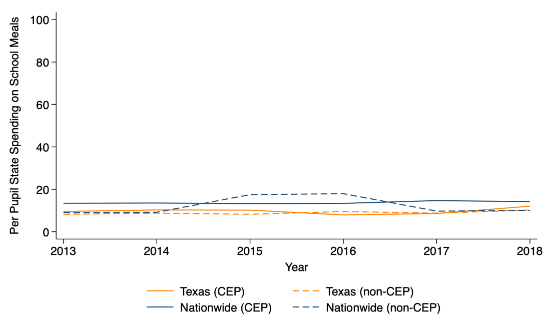
(a) Total School Meal Expenditure



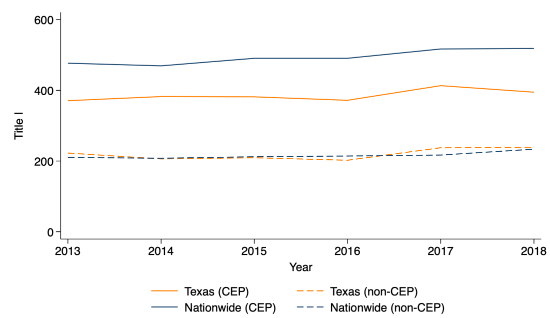
(b) Federal School Meal Expenditure



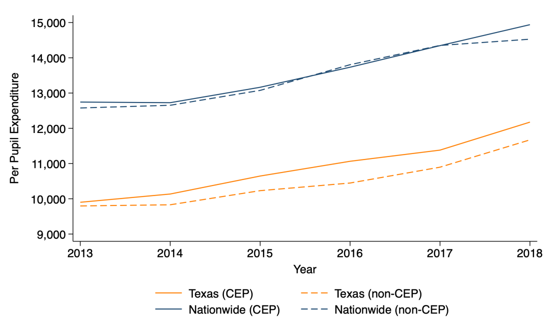
(c) State School Meal Expenditure



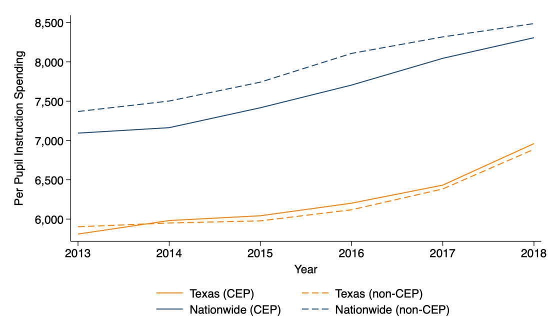
(d) Tital I Funding



(e) Total Expenditure



(f) Instruction Expenditure



Notes: These figures plot per-pupil expenditures in Texas and nationwide by school district CEP status. I link nationwide CEP participation data from the NCES with school district finance data from the Census Bureau's Annual Survey of School System Finances for school years 2013–2014 through 2018–2019. Because the finance data are available only at the district level, I define a district as treated if at least one school within it adopts CEP.

Table A1: Estimated Effects on High School Grades

	Summary Index (1)	Algebra I (2)	English I (3)	Biology (4)
CEP	-0.007	-0.033	-0.010	-0.006
	(0.023)	(0.042)	(0.022)	(0.025)
Mean of the Dependent Variable	.04	.06	.04	.01
Number of Observations	7840	8560	8632	7928
Number of Students	160559	162690	218787	214237
Number of Schools	980	1070	1079	991

Notes: This table presents estimates from a matching specification. The coefficients are averaged over event times 0 to 3. The sample includes schools that adopted CEP between 2012 and 2018. The data are collapsed into cells at the school–grade level, and the specifications are weighted by the number of students in each cell. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications and are clustered at the school level.

Table A2: Distribution of Years of CEP Exposure in School Districts Adopted CEP in Texas, Birth Cohorts 1986 to 2006

Years of CEP Exposure	Number of Students	Percent
0	3,667,446	78.00
1	236,227	5.00
2	171,924	4.00
3	137,971	3.00
4	116,837	2.00
5	89,369	2.00
6	74,282	2.00
7	58,477	1.00
8	45,480	1.00
9	36,988	1.00
10	28,490	1.00
Over 10	18,237	0.00
Total	4,681,728	100.00

Notes: The table presents the distribution of years of exposure to the Community Eligibility Provision (CEP) among students in districts that adopted CEP by 2024 in Texas. The sample includes students born between 1986 and 2006 who were enrolled in these districts.

Table A3: Treatment Assignment for Long-Term Outcomes for a District That Adopted CEP in 2014

Birth Cohort	Elementary School						Middle School			High School				Cumulative Years of Exposure
	K	1	2	3	4	5	6	7	8	9	10	11	12	
1986														0
1987														0
1988														0
1989														0
1990														0
1991														0
1992														0
1993														0
1994														0
1995														0
1996														0
1997														**1**
1998														2
1999														3
2000														4
2001														5
2002														6
2003														7
2004														8
2005														9
2006														10

1997 Birth Cohort = Oldest CEP Cohort   Grades exposed to CEP

Notes: This table illustrates how treatment is assigned in a district that adopted CEP in 2014. On the left are the birth cohorts from 1986 to 2006 in my sample, and across the top are school grades. The last column indicates cumulative exposure. The 1997 cohort is the key reference group: they were in 12th grade when the district adopted CEP. Older cohorts were not exposed because they had already left school, while younger cohorts were still enrolled and thus exposed. The younger the cohort at adoption, the more years of exposure they experienced, as indicated by the red cell.

Table A4: Distribution of Number of District Event Times

Number of Event Times in District	Number of District	District Enrollment	Share of Districts	Share of Enrollment
0	331	1,309,452	0.40	0.36
1	352	724,873	0.43	0.20
2	84	244,059	0.10	0.07
3	31	271,122	0.04	0.07
4	13	270,860	0.02	0.07
5	6	324,463	0.01	0.09
6	4	214,228	0.00	0.06
7	5	256,465	0.01	0.07
8	2	69,085	0.00	0.02
Total	828	3,684,607	1.00	1.00

Notes: The table presents the number of school districts, the number of students, and the percentages of school districts and enrollment by the number of times a school district adopted CEP. For example, 0 events indicate that a school district never adopted CEP; 1 event indicates that a school district adopted CEP districtwide in a single year; and 2 events indicate that a school district adopted CEP districtwide in two different years for two different subsets of schools within the district. The table includes districts in which at least 40% of students are eligible for free meals.

Table A5: Distribution of Districts by CEP Exposure in Seven Years Post CEP

Share of District Enrollment in CEP Schools	Number of Districts	Average Exposure in 7 Years Post CEP Adoption
0.0–0.1	267	0.0
0.11–0.2	8	0.2
0.21–0.3	6	0.3
0.31–0.4	14	0.4
0.41–0.5	22	0.3
0.51–0.6	17	1.3
0.61–0.7	15	1.0
0.71–0.8	24	1.2
0.81–0.9	25	1.1
0.91–1.0	430	1.6
Total	828	1

Notes: The table tabulates the actual number of years of exposure during the seven years following CEP adoption, by the share of district enrollment in CEP schools. The first column indicates the share of district enrollment in CEP schools, divided into ten bins. For example, 0–0.1 indicates that 0–10% of students in the districts are enrolled in CEP schools, while 0.91–1 indicates that over 91% of students are enrolled in CEP schools. The second column reports the number of districts in each bin. The third column presents the actual number of years of CEP exposure for birth cohorts 1986–2006 in these school districts.

Table A6: CEP Exposure by Birth Cohort

Birth Cohort	Number of Students	Years of Intended CEP Exposure	Years of Actual CEP Exposure
1986	154,824	0.00	0.10
1987	203,397	0.00	0.10
1988	215,242	0.00	0.10
1989	230,760	0.00	0.10
1990	222,102	0.00	0.10
1991	226,619	0.00	0.10
1992	227,937	0.00	0.10
1993	227,129	0.00	0.10
1994	225,603	0.00	0.10
1995	223,441	0.00	0.10
1996	220,879	0.00	0.20
1997	220,751	0.10	0.30
1998	221,619	0.50	0.40
1999	223,946	1.00	0.60
2000	227,861	1.40	0.80
2001	233,483	2.00	1.10
2002	233,132	2.60	1.50
2003	235,214	3.30	1.90
2004	234,055	4.10	2.40
2005	236,293	4.90	3.00
2006	237,441	5.80	3.60

Notes: This table reports the average number of years of CEP exposure and intended years of exposure by birth cohort in ever treated district by 2023.

Table A7: Estimated Effects of CEP on Meal Take-Up Across Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CEP	0.062***	0.075***	0.065***	0.068***	0.055***	0.062***	0.075***
	(0.004)	(0.005)	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)
Mean of Dependent Variable	0.62	0.65	0.62	0.50	0.58	0.61	0.50
Number of Observations	13,086	4,451	13,086	44,302	25,500	16,470	57,093
Number of Schools	1,871	1,113	1,871	5,529	2,834	2,060	4,744
Years of Data	2011–2018	2011–2018	2011–2018	2011–2018 and 2022–2024	2011–2018 and 2022–2024	2011–2018 and 2022–2024	2011–2018 and 2022–2024
Weights	Weighted	Weighted	Not weighted	Not weighted	Weighted	Weighted	Weighted
Control Schools	Later treated	Later treated w/ balanced group	Later treated	Matched	Later treated	Later treated w/ balanced group	Matched

Notes: This table presents estimates from different specifications and samples. "2011–2018" indicates that the data cover school years 2011–2012 through 2018–2019. "2011–2018 and 2022–2024" indicates that the data cover school years 2011–2012 through 2023–2024, but exclude 2019–2020, 2020–2021, and 2021–2022 because these years were affected by the COVID-19 pandemic. Some schools were closed during this period, and the data are not comparable to other years. "Weighted" indicates that the estimates are weighted by student enrollment in each school, while "Not weighted" indicates that no weights are used. "Later treated" indicates that the estimates are from a specification comparing earlier-treated to later-treated schools. "Matched" means the specification uses matched schools based on covariates from the short-term summary statistics table. "Balanced group" means the estimates include only schools that had CEP in place for at least three years.

Table A8: Estimated Effects of CEP on Pre-K and Kindergarten Readiness Scores

	Summary Index (1)	Reading (2)	Writing (3)	Math (4)	Communication (5)	Physical & Mental Health (6)
Panel A: Ever Adopted Sample, without Weights						
Pre-K Readiness Score	0.013*	0.012	-0.013	-0.005	0.034***	0.014
	(0.008)	(0.012)	(0.015)	(0.014)	(0.012)	(0.010)
Mean of the Dependent Variable	.69	.35	.84	.74	.55	.95
Number of Observations	1945	2630	2083	2424	2612	2160
Number of Students	27593	33837	25852	31200	33948	28670
Number of Schools	482	613	504	565	611	529
Kindergarten Readiness Score	-0.002	0.009	0.069*	-0.041*	-0.012	-0.012
	(0.017)	(0.013)	(0.040)	(0.023)	(0.021)	(0.014)
Mean of the Dependent Variable	.39	.5	.74	.52	.35	.
Number of Observations	1105	1924	220	531	1108	865
Number of Students	36595.5	60864	4996	19074	36328	26605
Number of Schools	469	800	76	237	469	326
Panel B: Not Yet Adopted Sample, with Weights						
Pre-K Readiness Score	0.015*	0.015*	0.008	0.006	0.017**	0.024**
	(0.008)	(0.008)	(0.013)	(0.012)	(0.008)	(0.011)
Mean of the Dependent Variable	.69	.35	.84	.74	.55	.95
Number of Observations	6302	7900	6736	7553	7851	6780
Number of Students	77704.2	88290	74214	85566	88022	78809
Number of Schools	1585	1834	1654	1777	1832	1665
Kindergarten Readiness Score	0.008	0.005	-0.004	-0.034*	0.013	0.011
	(0.015)	(0.010)	(0.026)	(0.018)	(0.019)	(0.010)
Mean of the Dependent Variable	.39	.5	.74	.52	.35	.
Number of Observations	3419	6185	968	1736	3425	5192
Number of Students	114270	193127	26782	67022	114077	164456
Number of Schools	1407	2445	372	774	1408	2033

Notes: Panel A of this table presents estimates from a specification that compares earlier-treated schools to later-treated schools , and the specifications are not weighted by the number of students in each school–grade–subject cell. Panel B of this table presents estimates from a specification that compares earlier-treated schools to later-treated and eligible but never treated schools , and the specifications are weighted by the number of students in each cell. The coefficients are averaged over event times 0 to 3. The sample is based on schools that adopted CEP between 2018 and 2023. The Pre-K readiness score is the end-of-grade score for 4-year-olds, and the kindergarten readiness score is the end-of-grade score for 5-year-olds. The Pre-K data include only public Pre-K programs, and about 90 percent of children in the sample are income-eligible for free and reduced-price meals. The data are collapsed into cells at the school–grade–subject level. Not all subjects are administered by all schools, so sample sizes vary by subject. The summary index for Pre-K includes five subjects, while the summary index for kindergarten includes only reading and communication. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications, clustered at the school level.

Table A9: Estimated Effects of CEP on Grades 3 to 8 Test Score Index Across Specifications

	(1)	(2)	(3)	(4)
CEP	0.001	-0.021**	0.009	0.003
	(0.008)	(0.010)	(0.006)	(0.008)
Mean of the dependent variable	-0.22	-0.07	-0.16	-0.22
Number of observations	31,395	141,648	91,643	45,828
Number of students	157,960	312,759	211,205	126,550
Number of schools	1,521	3,122	2,170	1,350
Years of data	2011–2018	2011–2018	2011–2018 & 2020–2022	2011–2018 & 2022–2024
Weights	Weighted	Not weighted	Weighted	Weighted
Control schools	Later treated	Matching	Later treated	Later treated w/ balanced group

Notes: This table presents estimates from different specifications and samples. "2011–2018" indicates that the data cover school years 2011–2012 through 2018–2019. "2011–2018 and 2022–2024" indicates that the data cover school years 2011–2012 through 2023–2024, but exclude 2019–2020, 2020–2021, and 2021–2022 because these years were affected by the COVID-19 pandemic. Some schools were closed during this period, and the data are not comparable to other years. "Weighted" indicates that the estimates are weighted by student enrollment in each school, while "Not weighted" indicates that no weights are used. "Later treated" indicates that the estimates are from a specification comparing earlier-treated to later-treated schools. "Matched" means the specification uses matched schools based on covariates from the short-term summary statistics table. "Balanced group" means the estimates include only schools that had CEP in place for at least three years.

Table A10: Estimated Effects of CEP on Test Scores, Absence, and Suspension for Schools with FRP below the Medium

	Full Sample (1)	Elementary School (2)	Middle School (3)	High School (4)
Panel A: Grade 3–8 Reading				
CEP	0.010	0.008	0.011	
	(0.010)	(0.011)	(0.015)	
Mean of Dependent Variable	-0.10	-0.06	-0.19	
Panel B: Grade 3–8 Math				
CEP	-0.013	-0.018	-0.006	
	(0.014)	(0.014)	(0.023)	
Mean of Dependent Variable	-0.13	-0.09	-0.21	
Panel C: Grade 3–8 Test Score Index				
CEP	-0.002	-0.005	0.002	
	(0.011)	(0.011)	(0.018)	
Mean of Dependent Variable	-0.12	-0.07	-0.20	
Sample size information for Panels A–C				
Number of Observations	14,357	9,303	5,054	
Number of Students	77,823	38,523	44,605	
Number of Schools	691	484	293	
Panel D: Number of Days Absent				
CEP	-0.016	0.047	0.364**	-0.221
	(0.129)	(0.062)	(0.157)	(0.309)
Mean of Dependent Variable	6.88	6.05	6.55	9.36
Number of Observations	30,947	18,634	5,299	7,014
Number of Students	159,619	53,919	52,583	60,691
Number of Schools	957	531	302	259
Panel E: Suspension Rates				
CEP	0.026	0.006	-0.029	0.080*
	(0.019)	(0.009)	(0.022)	(0.045)
Mean of Dependent Variable	0.61	0.25	1.35	0.99
Number of Observations	31,038	18,676	5,320	7,042
Number of Students	159,864	53,990	52,616	60,848
Number of Schools	958	532	303	260

Notes: This table presents estimates from a specification that compares earlier-treated schools with later-treated schools. The coefficients are averaged over event times 0 to 3. The sample includes schools that adopted CEP between 2012 and 2018. The data are collapsed into cells at the school–grade level, and the specifications are weighted by the number of students in each cell. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications and are clustered at the school level.

Table A11: Estimated Effects of CEP on Test Scores, Absence, and Suspension for Schools with FRP Above the Medium

	Full Sample (1)	Elementary School (2)	Middle School (3)	High School (4)
Panel A: Grade 3–8 Reading				
CEP	-0.017	-0.008	-0.033	
	(0.011)	(0.012)	(0.023)	
Mean of Dependent Variable	-0.34	-0.32	-0.46	
Panel B: Grade 3–8 Math				
CEP	-0.008	-0.018	0.016	
	(0.016)	(0.015)	(0.039)	
Mean of Dependent Variable	-0.27	-0.25	-0.40	
Panel C: Test Score Index				
CEP	-0.013	-0.013	-0.009	
	(0.013)	(0.012)	(0.030)	
Mean of Dependent Variable	-0.31	-0.28	-0.43	
Sample size information for Panels A–C				
Number of Observations	17,038	14,091	2,947	
Number of Students	80,137	52,600	29,509	
Number of Schools	830	696	167	
Panel D: Number of Days Absent				
CEP	0.003	-0.042	-0.146	0.599
	(0.069)	(0.053)	(0.154)	(0.456)
Mean of Dependent Variable	6.04	5.74	7.21	10.74
Number of Observations	33,376	29,099	3,178	1,099
Number of Students	125,531	75,361	41,567	15,195
Number of Schools	921	749	182	43
Panel E: Suspension Rates				
CEP	0.014	0.002	0.044	0.196
	(0.012)	(0.012)	(0.035)	(0.180)
Mean of Dependent Variable	0.20	0.16	0.51	0.23
Number of Observations	33,376	29,099	3,178	1,099
Number of Students	125,677	75,440	41,592	15,241
Number of Schools	921	749	182	43

Notes: This table presents estimates from a specification that compares earlier-treated schools with later-treated schools. The coefficients are averaged over event times 0 to 3. The sample includes schools that adopted CEP between 2012 and 2018. The data are collapsed into cells at the school–grade level, and the specifications are weighted by the number of students in each cell. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications and are clustered at the school level.

Table A12: Short-Term Outcomes by Student Characteristics, Elementary Schools

	Race				Gender		Low-income Status		
	White	Black	Hispanic	Other	Girls	Boys	Never Low-income	Transitorily Low-income	Persistently Low-income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Grade 3–8 Reading									
CEP	0.003	-0.028*	0.009	-0.004	0.013	0.012	0.021	-0.004	0.011
	(0.013)	(0.017)	(0.009)	(0.030)	(0.009)	(0.009)	(0.019)	(0.009)	(0.010)
Mean of the Dependent Variable	.15	-.37	-.24	.15	-.13	-.29	.48	-.18	-.35
Panel B: Grade 3–8 Math									
CEP	-0.013	-0.034*	-0.009	-0.050*	-0.006	-0.016	-0.005	-0.021*	-0.011
	(0.016)	(0.019)	(0.012)	(0.030)	(0.011)	(0.011)	(0.020)	(0.011)	(0.012)
Mean of the Dependent Variable	.08	-.46	-.16	.23	-.17	-.2	.41	-.17	-.28
Panel C: Grade 3–8 Test Score Index									
CEP	-0.005	-0.031*	-0.000	-0.027	0.004	-0.002	0.008	-0.012	-0.000
	(0.013)	(0.016)	(0.009)	(0.028)	(0.009)	(0.009)	(0.018)	(0.009)	(0.010)
Mean of the Dependent Variable	.12	-.41	-.2	.19	-.15	-.25	.44	-.18	-.32
Sample size for Panels A–C									
Number of Observations	12082	14651	22771	5873	23296	23331	7770	23352	23002
Number of Students	28868	38154	166731	4947	119071	124850	14181	12842	98634
Number of Schools	709	802	1163	382	1177	1179	461	1178	1169
Panel D: Number of Days Absent									
CEP	0.235***	0.238***	-0.054	0.100	0.055	0.059	-0.006	0.045	0.022
	(0.088)	(0.079)	(0.044)	(0.111)	(0.043)	(0.041)	(0.079)	(0.040)	(0.049)
Mean of the Dependent Variable	6.86	6.25	5.63	5.51	5.81	5.9	4.97	5.89	6.02
Number of Observations	28231	31752	46788	15134	47642	47677	18046	47621	47243
Number of Students	75439	99975	420493	14245	301293	319191	35643	323332	256071
Number of Schools	938	956	1269	616	1278	1280	654	1277	1278
Panel E: Suspension Rates									
CEP	0.358***	-0.034	0.005	-0.440	0.011	0.001	-0.078	0.005	0.060***
	(0.137)	(0.104)	(0.008)	(0.503)	(0.008)	(0.019)	(0.192)	(0.015)	(0.020)
Mean of the Dependent Variable	2.54	3.26	.32	5.06	.17	.59	1.18	.4	.67
Number of Observations	28280	31773	46802	15134	47684	47719	18172	47642	47243
Number of Students	75560	100074	420784	14256	301536	319506	35793	323587	256237
Number of Schools	939	957	1270	616	1279	1281	657	1278	1278

Notes: This table presents estimates from a specification that compares earlier-treated schools with later-treated schools. The coefficients are averaged over event times 0 to 3. The sample includes schools that adopted CEP between 2012 and 2018. The data are collapsed into cells at the school–grade–race/gender/low-income status level, and the specifications are weighted by the number of students in each cell. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications and are clustered at the school level.

Table A13: Short-Term Outcomes by Student Characteristics, Middle Schools

	Race				Gender		Low-income Status		
	White	Black	Hispanic	Other	Girls	Boys	Never Low-income	Transitorily Low-income	Persistently Low-income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Grade 3–8 Reading									
CEP	-0.013	-0.052***	-0.001	-0.029	0.001	0.000	-0.012	0.003	-0.019
	(0.018)	(0.015)	(0.015)	(0.037)	(0.013)	(0.014)	(0.028)	(0.013)	(0.015)
Mean of the Dependent Variable	0.00	-0.43	-0.34	-0.06	-0.19	-0.38	0.33	-0.26	-0.46
Panel B: Grade 3–8 Math									
CEP	-0.015	-0.049**	0.006	-0.000	0.002	0.001	-0.021	0.002	-0.006
	(0.025)	(0.022)	(0.022)	(0.039)	(0.019)	(0.019)	(0.044)	(0.018)	(0.021)
Mean of the Dependent Variable	-0.03	-0.52	-0.29	0.08	-0.28	-0.28	0.28	-0.29	-0.39
Panel C: Grade 3–8 Test Score Index									
CEP	-0.014	-0.051**	0.002	0.014	0.001	0.001	-0.017	0.002	-0.012
	(0.019)	(0.017)	(0.017)	(0.035)	(0.015)	(0.015)	(0.034)	(0.015)	(0.017)
Mean of the Dependent Variable	-0.02	-0.48	-0.32	0.01	-0.24	-0.33	0.31	-0.27	-0.42
Sample size for Panels A–C									
Number of Observations	5887	5222	7518	3129	7889	7910	3619	7959	7609
Number of Students	22652	30339	111908	3671	82592	87106	11115	89865	67517
Number of Schools	363	307	432	212	456	457	238	457	447
Panel D: Number of Days Absent									
CEP	-0.013	0.618**	0.103	0.415**	0.176	0.243**	-0.021	0.266**	0.110
	(0.140)	(0.248)	(0.113)	(0.209)	(0.112)	(0.123)	(0.146)	(0.125)	(0.133)
Mean of the Dependent Variable	7.85	7.11	6.56	5.89	6.72	6.86	5.10	6.86	7.24
Number of Observations	6748	5775	8001	4144	8316	8372	4732	8428	8141
Number of Students	30913	41013	156304	6429	114961	120730	18040	122102	94777
Number of Schools	409	336	460	266	479	480	298	483	472
Panel E: Suspension Rates									
CEP	0.480	-0.339**	0.048*	0.394	0.001	-0.018	0.400	0.017	0.036
	(0.350)	(0.163)	(0.027)	(0.895)	(0.028)	(0.042)	(0.318)	(0.033)	(0.057)
Mean of the Dependent Variable	8.06	10.84	1.89	12.19	1.26	2.42	2.50	1.79	3.88
Number of Observations	6755	5789	8008	4144	8330	8393	4739	8449	8141
Number of Students	30933	41045	156381	6436	115021	120817	18056	122184	94817
Number of Schools	410	337	461	266	480	481	299	484	472

Notes: This table presents estimates from a specification that compares earlier-treated schools with later-treated schools. The coefficients are averaged over event times 0 to 3. The sample includes schools that adopted CEP between 2012 and 2018. The data are collapsed into cells at the school–grade–race/gender/low-income status level, and the specifications are weighted by the number of students in each cell. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications and are clustered at the school level.

Table A14: Short-Term Outcomes by Student Characteristics, Lower-Poverty Schools

	Race				Gender		Low-income Status		
	White	Black	Hispanic	Other	Girls	Boys	Never Low-income	Transitorily Low-income	Persistently Low-income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Grade 3–8 Reading									
CEP	-0.003	-0.028**	0.012	-0.008	0.007	0.012	0.012	0.008	-0.001
	(0.012)	(0.014)	(0.012)	(0.024)	(0.010)	(0.011)	(0.010)	(0.012)	(0.012)
Mean of the Dependent Variable	0.18	-0.29	-0.18	0.17	-0.02	-0.18	0.44	-0.10	-0.34
Panel B: Grade 3–8 Math									
CEP	-0.007	-0.037**	-0.013	-0.033	-0.010	-0.015	-0.008	-0.016	-0.016
	(0.016)	(0.019)	(0.017)	(0.024)	(0.015)	(0.014)	(0.022)	(0.014)	(0.016)
Mean of the Dependent Variable	0.12	-0.39	-0.17	0.24	-0.12	-0.14	0.38	-0.15	-0.31
Panel C: Grade 3–8 Test Score Index									
CEP	-0.005	-0.032**	-0.000	-0.020	-0.001	-0.002	0.002	-0.004	-0.008
	(0.012)	(0.015)	(0.013)	(0.022)	(0.011)	(0.012)	(0.018)	(0.011)	(0.013)
Mean of the Dependent Variable	0.15	-0.34	-0.18	0.21	-0.07	-0.16	0.41	-0.12	-0.33
Sample size for Panels A–C									
Number of Observations	12789	9156	13482	6461	14252	14238	10206	14322	13629
Number of Students	45884	32211	112657	6747	98035	101990	24598	115715	57570
Number of Schools	650	494	667	398	690	559	691	691	681
Panel D: Number of Days Absent									
CEP	0.042	0.021	-0.047	-0.186	-0.012	-0.017	-0.089	0.006	0.008
	(0.078)	(0.305)	(0.134)	(0.184)	(0.128)	(0.130)	(0.096)	(0.138)	(0.135)
Mean of the Dependent Variable	7.11	6.73	6.84	6.11	6.90	6.86	5.31	7.03	7.36
Number of Observations	30268	22337	31976	16919	33348	33754	24878	33712	32207
Number of Students	132270	103725	332796	22623	291893	304966	80365	327710	184584
Number of Schools	920	757	949	681	965	963	870	966	963
Panel E: Suspension Rates									
CEP	0.100	-0.027	0.080***	-0.401	0.062*	0.047	0.266***	0.092**	0.131**
	(0.071)	(0.108)	(0.030)	(0.368)	(0.035)	(0.042)	(0.093)	(0.042)	(0.051)
Mean of the Dependent Variable	1.92	5.91	1.36	6.57	0.66	1.55	1.38	1.08	2.37
Number of Observations	28196	21021	29295	16030	30765	30814	24094	30884	29715
Number of Students	127679	100988	321608	21691	282188	294606	79471	316378	177268
Number of Schools	912	750	940	670	957	955	865	958	955

Notes: This table presents estimates from a specification that compares earlier-treated schools with later-treated schools. The coefficients are averaged over event times 0 to 3. The sample includes schools that adopted CEP between 2012 and 2018. The data are collapsed into cells at the school–grade–race/gender/low-income status level, and the specifications are weighted by the number of students in each cell. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications and are clustered at the school level.

Table A15: Short-Term Outcomes by Student Characteristics, Higher-Poverty Schools

	Race				Gender		Low-income Status		
	White	Black	Hispanic	Other	Girls	Boys	Never Low-income	Transitorily Low-income	Persistently Low-income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Grade 3–8 Reading									
CEP	-0.029	-0.050**	-0.009	-0.017	-0.015	-0.018	-0.142*	-0.025**	-0.013
	(0.031)	(0.020)	(0.011)	(0.060)	(0.011)	(0.012)	(0.085)	(0.012)	(0.012)
Mean of the Dependent Variable	0.00	-0.47	-0.33	-0.15	-0.26	-0.43	0.37	-0.29	-0.41
Panel B: Grade 3–8 Math									
CEP	-0.084**	-0.048**	0.006	-0.046	-0.005	-0.010	-0.124	-0.009	-0.008
	(0.035)	(0.024)	(0.017)	(0.063)	(0.017)	(0.017)	(0.114)	(0.018)	(0.017)
Mean of the Dependent Variable	-0.13	-0.55	-0.22	0.01	-0.26	-0.28	0.28	-0.25	-0.30
Panel C: Grade 3–8 Test Score Index									
CEP	-0.056*	-0.049**	-0.001	-0.015	-0.010	-0.014	-0.133	-0.017	-0.010
	(0.030)	(0.020)	(0.013)	(0.056)	(0.013)	(0.014)	(0.094)	(0.014)	(0.013)
Mean of the Dependent Variable	-0.11	-0.51	-0.27	-0.07	-0.26	-0.36	0.32	-0.27	-0.36
Sample size for Panels A–C									
Number of Observations	5180	10717	16807	2541	16933	17003	1183	16989	16982
Number of Students	5636	36282	165982	1871	103628	109966	698	102632	108581
Number of Schools	340	566	827	172	827	829	93	828	829
Panel D: Number of Days Absent									
CEP	0.340*	0.142	-0.034	0.131	0.009	0.042	0.040	0.026	0.009
	(0.181)	(0.116)	(0.067)	(0.196)	(0.062)	(0.071)	(0.285)	(0.067)	(0.068)
Mean of the Dependent Variable	8.10	7.10	5.88	6.06	6.06	6.21	5.54	6.18	6.17
Number of Observations	14840	24941	38115	7875	37800	38535	4697	38192	38087
Number of Students	19320	99963	458659	7175	287986	305503	3131	28749	303393
Number of Schools	600	730	960	380	960	962	269	960	962
Panel E: Suspension Rates									
CEP	1.160**	-0.098	0.017	-0.003	0.026	0.022	-5.360	0.012	0.036
	(0.550)	(0.148)	(0.012)	(1.021)	(0.017)	(0.032)	(3.882)	(0.028)	(0.024)
Mean of the Dependent Variable	6.94	3.71	0.27	9.77	0.20	0.58	4.78	0.44	0.44
Number of Observations	13615	22204	33096	7112	33215	33355	4522	33306	33341
Number of Students	168998	88714	414430	6270	258894	275405	3011	254667	272855
Number of Schools	557	688	919	340	918	923	251	918	920

Notes: This table presents estimates from a specification that compares earlier-treated schools with later-treated schools. The coefficients are averaged over event times 0 to 3. The sample includes schools that adopted CEP between 2012 and 2018. The data are collapsed into cells at the school–grade–race/gender/low-income status level, and the specifications are weighted by the number of students in each cell. Standard errors are obtained using a multiplier bootstrap procedure with 1,000 replications and are clustered at the school level.

Table A16: Estimated Effects on Long-Term Outcomes by Student Characteristics Among Students First Exposed to CEP in Middle School, Birth Cohorts 2000–2006

	Years of CEP Exposure (1)	On-time High School Graduation (2)	Ever Drop Out (3)	Taking SAT (4)	On-time College Enrollment (5)
Never low-income	1.231***	-0.012**	0.009**	-0.019	-0.035*
	(0.318)	(0.014)	(0.004)	(0.015)	(0.020)
Mean of the Dependent Variable	0.67	0.74	0.01	0.20	0.63
Number of Observations	2377	2377	2377	2377	2377
Number of Students	161353	161353	161353	161353	161353
Number of Districts	347	347	347	347	347
Transitorily low-income	2.313***	0.026*	-0.007	-0.021**	0.015
	(0.229)	(0.014)	(0.005)	(0.010)	(0.013)
Mean of the Dependent Variable	1.03	0.70	0.04	0.11	0.44
Number of Observations	2477	2477	2477	2477	2477
Number of Students	372771	372771	372771	372771	372771
Number of Districts	354	354	354	354	354
Persistently low-income	2.430***	-0.023*	0.005	-0.021**	-0.032**
	(0.254)	(0.012)	(0.007)	(0.010)	(0.013)
Mean of the Dependent Variable	0.95	0.53	0.05	0.06	0.28
Number of Observations	2460	2460	2460	2460	2460
Number of Students	233701	233701	233701	233701	233701
Number of Districts	354	354	354	354	354
White	1.689***	0.000	0.003	0.021**	-0.010
	(0.261)	(0.012)	(0.004)	(0.010)	(0.015)
Mean of the Dependent Variable	0.76	0.65	0.03	0.13	0.46
Number of Observations	2464	2464	2464	2464	2464
Number of Students	253453	253453	253453	253453	253453
Number of Districts	352	352	352	352	352
Black	2.140***	-0.013	-0.010	-0.013	0.017
	(0.241)	(0.025)	(0.013)	(0.014)	(0.021)
Mean of the Dependent Variable	1.07	0.63	0.03	0.12	0.41
Number of Observations	1816	1816	1816	1816	1816
Number of Students	102823	102823	102823	102823	102823
Number of Districts	296	296	296	296	296
Hispanic	2.433***	0.000	0.003	-0.020	-0.026*
	(0.312)	(0.013)	(0.005)	(0.016)	(0.013)
Mean of the Dependent Variable	1.08	0.67	0.04	0.09	0.40
Number of Observations	2381	2381	2381	2381	2381
Number of Students	376471	376471	376471	376471	376471
Number of Districts	348	348	348	348	348
Female	2.183***	0.009	-0.002	-0.026**	0.005
	(0.241)	(0.014)	(0.004)	(0.011)	(0.015)
Mean of the Dependent Variable	0.94	0.69	0.03	0.14	0.50
Number of Observations	2478	2478	2478	2478	2478
Number of Students	375005	375005	375005	375005	375005
Number of Districts	354	354	354	354	354
Male	2.179***	0.014	0.003	-0.010	-0.008
	(0.238)	(0.012)	(0.005)	(0.011)	(0.013)
Mean of the Dependent Variable	0.96	0.64	0.05	0.09	0.38
Number of Observations	2478	2478	2478	2478	2478
Number of Students	392881	392881	392881	392881	392881
Number of Districts	354	354	354	354	354

Notes: The table presents event-study estimates using the specifications in equation (5) for each of the main outcomes, using the estimator developed by Callaway and Sant'Anna (2021). For all outcomes except SAT scores and earnings, the sample includes children born between 2000 and 2006 in school districts in Texas that adopted CEP by 2024. The SAT score sample includes only students who took the SAT. For earnings at age 21, the sample includes only children who had reached age 21 by 2023. All estimates are based on data collapsed into district × cohort × low-income status/race/gender cells and weighted by the number of students per cell. The standard errors obtained using a multiplier bootstrap procedure and clustered at the district level.

Table A17: Estimated Effects on Long-Term Outcomes by Student Characteristics Among Students First Exposed to CEP in High School, Birth Cohorts 2000–2006

	Years of CEP Exposure (1)	On-time High School Graduation (2)	Ever Drop Out (3)	Taking SAT (4)	On-time College Enrollment (5)
Never low-income	0.610***	-0.006	0.001	-0.003	-0.008
	(0.119)	(0.006)	(0.001)	(0.007)	(0.007)
Mean of the Dependent Variable	0.67	0.74	0.01	0.20	0.63
Number of Observations	2377	2377	2377	2377	2377
Number of Students	161353	161353	161353	161353	161353
Number of Districts	347	347	347	347	347
Transitorily low-income	0.748***	0.007	-0.005**	-0.005	0.001
	(0.091)	(0.005)	(0.002)	(0.005)	(0.006)
Mean of the Dependent Variable	1.03	0.70	0.04	0.11	0.44
Number of Observations	2477	2477	2477	2477	2477
Number of Students	372771	372771	372771	372771	372771
Number of Districts	354	354	354	354	354
Persistently low-income	0.775***	0.004	-0.001	-0.003	-0.004
	(0.088)	(0.007)	(0.003)	(0.006)	(0.006)
Mean of the Dependent Variable	0.95	0.53	0.05	0.06	0.28
Number of Observations	2460	2460	2460	2460	2460
Number of Students	233701	233701	233701	233701	233701
Number of Districts	354	354	354	354	354
White	0.640***	0.007	-0.002	-0.003	0.001
	(0.078)	(0.006)	(0.002)	(0.005)	(0.005)
Mean of the Dependent Variable	0.76	0.65	0.03	0.13	0.46
Number of Observations	2464	2464	2464	2464	2464
Number of Students	253453	253453	253453	253453	253453
Number of Districts	352	352	352	352	352
Black	0.687***	-0.004	-0.001	-0.016**	0.006
	(0.095)	(0.008)	(0.005)	(0.007)	(0.008)
Mean of the Dependent Variable	1.07	0.63	0.07	0.12	0.37
Number of Observations	1816	1816	1816	1816	1816
Number of Students	102823	102823	102823	102823	102823
Number of Districts	296	296	296	296	296
Hispanic	0.801***	0.001	0.003	-0.002	-0.011
	(0.119)	(0.005)	(0.003)	(0.008)	(0.008)
Mean of the Dependent Variable	1.08	0.67	0.04	0.09	0.40
Number of Observations	2381	2381	2381	2381	2381
Number of Students	376471	376471	376471	376471	376471
Number of Districts	348	348	348	348	348
Female	0.729***	0.004	-0.003	-0.008	-0.003
	(0.082)	(0.006)	(0.002)	(0.006)	(0.006)
Mean of the Dependent Variable	0.94	0.69	0.03	0.14	0.50
Number of Observations	2478	2478	2478	2478	2478
Number of Students	375005	375005	375005	375005	375005
Number of Districts	354	354	354	354	354
Male	0.735***	0.007	-0.002	0.000	0.001
	(0.086)	(0.005)	(0.002)	(0.005)	(0.007)
Mean of the Dependent Variable	0.96	0.64	0.05	0.09	0.38
Number of Observations	2478	2478	2478	2478	2478
Number of Students	392881	392881	392881	392881	392881
Number of Districts	354	354	354	354	354

Notes: The table presents event-study estimates using the specifications in equation (5) for each of the main outcomes, using the estimator developed by Callaway and Sant'Anna (2021). For all outcomes except SAT scores and earnings, the sample includes children born between 2000 and 2006 in school districts in Texas that adopted CEP by 2024. The SAT score sample includes only students who took the SAT. For earnings at age 21, the sample includes only children who had reached age 21 by 2023. All estimates are based on data collapsed into district × cohort × low-income status/race/gender cells and weighted by the number of students per cell. The standard errors obtained using a multiplier bootstrap procedure and clustered at the district level.