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Author

Ruffini, Krista

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Universal Access to Free School Meals and Student Achievement: Evidence from the Community Eligibility Provision

Krista Ruffini [†]

October 25, 2018

Abstract

Improving nutritional intake can improve children's academic achievement and long-term health outcomes. This paper evaluates the extent to which, and for which groups of students, schoolwide meals affect reading and math performance. Leveraging cross-state variation in the timing of eligibility for schoolwide free meals under the Community Eligibility Provision, I find universal access to free meals substantially increases breakfast and lunch participation. Math performance increases approximately 0.02 standard deviations in districts with the largest shares of students becoming eligible for free meals. Scaling by the share of newly-eligible students, this effect implies gaining access to free meals increases math scores by 0.07 standard deviations. Test score improvements are concentrated among Hispanic and white students – groups with relatively low eligibility rates under the traditional school meals program.

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[†]Goldman School of Public Policy, University of California, Berkeley. Email: k.j.ruffini@berkeley.edu

1 Introduction

The National School Lunch Program (NSLP) is one of the nation's oldest and largest forms of nutritional assistance serving school-aged children. In combination with the related School Breakfast Program (SBP), the school meals programs provide a potentially large transfer both in terms of financial and nutritional resources, as students consume up to 50 percent of their daily calories at school (Gleason and Suitor, 2001), and a student receiving free breakfasts and lunch every school day pays up to $4.48 a day, or approximately $800 per school year, less than a student paying the full price.[1] In comparison to other forms of means-tested assistance, take-up of the school meals program is high: On a typical school day, more than 30 percent of 5-17 year-olds receive free lunches, and more than 20 percent receive free breakfasts (USDA, 2018b).[3]

Despite its importance in both the potential size of transfers and the number of students served, evaluating the causal effect of school-based nutritional assistance on child outcomes is difficult since the existing program provides little cross-school or cross-time variation: school lunches were implemented nationally within a short time period, guidelines are set at the federal level for all schools, and participation among schools is nearly universal. Moreover, since students' eligibility for free meals depends on family income, participating children are more disadvantaged than ineligible students, and comparisons of eligible and ineligible children will be biased towards finding negative program effects (Bitler, 2015; Currie, 2003). While the existing literature tends to find school meals increase food consumption and nutritional intake, evidence of the causal effect of school-based nutritional assistance on students' academic performance is more mixed (Schanzenbach, 2009; Gleason and Suitor, 2001; Gundersen et al., 2012; Imberman and Kugler, 2014; Dotter, 2013).

This paper examines a recent expansion in eligibility for free school meals under the Com-

[1] In comparison, the average daily per-person SNAP benefit for families with children is about $4.00 (USDA, 2018a). The size of the school meals program is also apparent by examining federal outlays. In Fiscal Year 2017, the federal government allocated about $16.8 billion to school nutrition programs, compared to $15.0 billion in Title I funding, the largest source of federal education funds to elementary and secondary schools, and more than half the estimated $28.0 billion in pro-rated SNAP benefits to children (USDA, 2017; US Department of Education, 2017; USDA, 2018a).[2]

[3] An additional five percent received meals at a deeply subsidized rate.

munity Eligibility Provision (CEP). CEP allows high-poverty schools and districts to provide free school meals for all students, removing the link between individual family resources and free school meals. Since eligible districts had relatively high school meal participation prior to reform and many participating districts were *de facto* approaching universal provision, *ex-ante* it is unclear whether universal assistance will affect meal participation or student test scores. Even among districts with a high baseline program participation rate, I find CEP led to a sizable increase in the number of breakfasts and lunches served. While universal assistance did not significantly improve test scores among the full set of participating districts, it did result in improved performance in districts with the largest gains in free meal participation, and among subpopulations that were less likely to participate in the free meals program before reform–specifically white and Hispanic students.

Under CEP, schools, or groups of schools within a district, where at least 40 percent of students are "categorically eligible" for free and reduced-price (FRP) meals - meaning their families receive another form of means-tested assistance - can opt to offer free meals for all students, and receive a federal reimbursement at the free meal rate equal to 1.6 times the share of categorically eligible students, up to full reimbursement.

Importantly, not all schools became eligible for CEP at the same time. The year in which a school became eligible depends on the state in which it is located: schools in Illinois, Kentucky, and Michigan became eligible in the 2012 school year; schools in the District of Columbia, New York, Ohio, and West Virginia became eligible in 2013, schools in Florida, Georgia, Maryland, and Massachusetts became eligible in 2014, and the remaining states entered the program in 2015 (Figure 2).[4] In addition, within eligible states, participation has increased over time. My empirical approach leverages the staggered rollout, as well as within-state variation in program participation, by comparing performance changes in districts that adopted CEP in the early-adopting states to districts that adopted CEP in later-adopting states. Under the assumption that achievement would have evolved similarly in CEP-adopting districts in the absence of the program, this approach will identify the

[4] Throughout, years refer to the final year of the academic year. For example, "2012" refers to the 2011-2012 school year.

causal effect of universal access to free meals on student performance. I present supporting analyses that baseline characteristics, such as district resources and economic well-being, did not systematically differ between CEP districts in early- and late-adopting states.

There are several channels by which schoolwide free meals may affect student performance. First, by eliminating enrollment paperwork, students who are otherwise income-eligible, but who did not previously receive free meals, gain access. Second, some students who are above the income cutoff also receive free meals. Both of these channels increase the amount of income available to families for other food expenditures and consumption goods, which may benefit children's outcomes. Third, if greater access to school meals improves behaviors or reduces distractions in the classroom, the program can increase teaching time and have spillover effects to students whose nutritional consumption does not change. Fourth, since all students receive free meals in CEP schools, family income may become less salient, which could reduce stigma associated with school meals. Fifth, if schools opt into CEP based on financial benefits, schools may allocate additional resources to educational activities and support that directly benefit student performance. These channels are not mutually exclusive, and I cannot fully disentagle amongst them. The results, however, do not meaningfully change after accounting for changes in district resources, indicating the findings are not solely due to concurrent changes in financial or instructional supports.

This paper contributes to a burgeoning literature that uses quasi-experimental variation in program eligibility and access to estimate the effects of nutritional assistance on health and economic outcomes. Much of the existing literature focuses on the Supplemental Nutrition Assistance Program (SNAP). For example, Hoynes et al. (2016) find access to SNAP in childhood improves health in adulthood and economic self-sufficiency for women, and Gassman-Pines and Bellows (2015) and Gennetian et al. (2015) find that greater SNAP resources improve short-term student performance and behaviors. The relationship between school meals and educational outcomes is more mixed. While Schanzenbach (2009) finds negative effects of school meals on health, Gleason and Suitor (2001), Schanzenbach and Zaki (2014), and Bhattacharya et al. (2006) find school meals improve nutritional intake, and in the long-term, Hinrichs (2010) finds that greater exposure to school lunches increases educational attainment. Examining the effects of these programs on short-term outcomes can shed light on channels for nutritional assistance to yield long-term educational and economic benefits. In particular, examining shorter-run effects can help disentangle whether these benefits arise through a direct academic achievement channel, or through latent improvements in health outcomes or non-cognitive skills.

This analysis makes several contributions to the existing literature. First, this paper examines variation in access to free meals that is unrelated to household characteristics (current income), but is related to the characteristics of the surrounding area, regardless of whether a particular student's family faces economic hardship. Accordingly, CEP may be viewed as a "place-based policy" – assistance targeted to high-poverty areas, rather than impoverished individuals. Importantly, this program examines the effect of eligibility for school meals that is not driven by family resources. Second, much of the recent literature on universal meals focuses on a single, urban school district (Dotter, 2013; Imberman and Kugler, 2014; Schwartz and Rothbart, 2017). In contrast, this paper examines how universal meals affect performance in both rural and urban districts in all fifty states and the District of Columbia. Finally, as the first districts to adopt CEP are treated for four years in my sample, I am able to explore whether the marginal benefits of nutritional assistance increase or decrease with additional years of program experience.

I find that CEP increased the number of breakfasts and lunches served by about 46 and 11 percent, respectively. Schools with relatively low baseline rates of free breakfast and lunch consumption experienced the largest increases in the number of free meals served. For the entire sample of CEP schools, I find no significant improvements in student test scores. However, many of these schools had high free meal participation rates under the traditional program and thus experienced little effective change under CEP. Focusing on the districts with the largest share of students becoming eligible for free meals under CEP – those with approximately less than 58 percent of students qualifying for free and reduced meals before CEP began – I find modest improvements in performance of 0.02 SD in math. In these districts, free meal eligibility increases approximately 27 percentage points under CEP. Scaling these effects by the share of students gaining free meal eligibility under CEP implies that school meals improve math performance by approximate 0.07 SD, while overall reading performance does not significantly improve. In both subjects, any benefits are concentrated among Hispanic and white students, two groups with relatively low free meal eligibility rates under the traditional program, and therefore likely to gain access to free meals through universal provision. In particular, universal meals improves math performance for Hispanic students by 0.031 standard deviations, and for white students by approximately 0.019 standard deviations. Accordingly, while white-black proficiency gaps slightly widened following CEP adoption, this growth is due to improvements in the absolute performance of white students; black students' performance did not meaningfully change following schoolwide provision. In addition, changes in performance do not significantly differ between elementary and middle school students. Further, it does not appear that these findings are driven by concurrent reductions in district resources: per-pupil expenditures and district federal revenue (excluding nutrition assistance), as well as student-teacher ratios did not significantly change following implementation. All of these qualitative findings are robust across specifications and alternative identifying assumptions.

While this paper finds that universal free meals improve short-term academic performance, several caveats should be kept in mind when interpreting these results. First, since CEP is a relatively new program, I am only able to examine short-term academic and consumption outcomes. Based on research examining the long-term effects of SNAP and WIC, improved nutritional access may have long-term benefits that are not immediately apparent. Second, these findings show *district* level average changes in academic achievement; it is possible that these results mask heterogeneous positive and negative responses across schools or students within a district. Finally, the recent expansion primarily affected near-poor students living in high-poverty communities. Since low-income students had access to free meals prior to CEP, the population of directly-affected students are more moderate-income individuals who may have lower benefits from school meal participation. Therefore, the results may not generalize to more affluent settings or to the most disadvantaged families.

This paper proceeds as follows. Section 2 reviews the relevant literature. Section 3 overviews the history of the school meals program, including CEP, and outlines the channels by which school-based assistance can affect student performance. Section 4 outlines the data and methodology. Section 5 presents results and Section 6 concludes.

2 Existing Literature

Adequate access to food and nutrition is summarized by food insecurity, a series of questions enquiring whether a household has an adequate quantity and quality of food. While the children living in food insecure households is low overall (9 percent), it is much higher (24 percent) among poor households (Coleman-Jensen et al., 2017).

Food insecurity is linked to negative health conditions including anemia, diabetes, heart disease and depression, and lower levels of social, emotional, and cognitive development for children (Howard, 2011). While lower-income families are more likely to be food insecure, the relationship between food scarcity and poorer development is not entirely driven by family socioeconomic characteristics; after controlling for family resources, lower food security is associated with poor academic performance and psycho-social behaviors, lower levels of educational attainment, and worse economic outcomes in adulthood (Alaimo et al., 2001; Case et al., 2005; Jyoti et al., 2005).

A growing body of research finds that nutritional assistance programs reduce children's food insecurity, and can therefore improve children's health, and cognitive and non-cognitive outcomes. For example, Ratcliffe, McKernan, and Zhang (2011) estimate the Supplemental Nutrition Assistance Program (SNAP) reduces food insecurity by 30 percent and Mabli and Worthington (2014) find that receiving SNAP for six months reduces the likelihood of food insecurity among children by about 33 percent. The benefits of SNAP translate into both short-and long-term gains. In the short term, greater access to SNAP improves test scores (Gassman-Pines and Bellows, 2015) and lowers disciplinary action (Gennetian et al., 2015), while in the long-term, childhood exposure to SNAP improves health outcomes and economic sufficiency for women in adulthood (Hoynes et al., 2016).

Even accounting for SNAP, however, some families continue to face food insecurity, as SNAP is often insufficient to cover families' food bills (Hoynes et al., 2015; Coleman-Jensen et al., 2017). Second, SNAP take-up is incomplete: in 2011, 88 percent of income-eligible households received benefits (Ganong and Liebman, 2013). Since family-based nutritional assistance does not eradicate food insecurity, there is scope for school-based assistance to improve health outcomes and reduce hunger.

A challenge for empirical work evaluating income assistance programs, including school meal programs, is that assistance is non-random, since eligibility depends on family income (Bitler, 2015; Bitler and Currie, 2005; Currie, 2003). Given the negative relationship between family income and health, recipients are likely to have poorer health outcomes without the program than ineligible individuals. Therefore, naive comparisons of participating and non-participating children tend to understate any benefits of nutritional assistance.

To overcome selection bias, previous research has used variation in free meal access from either income eligibility thresholds or seasonal patterns. In general, this work finds that access to school meals increases food consumption and nutritional intake. For example, comparing income-eligible students to those with slightly higher incomes, Schanzenbach (2009) finds that children who consume school lunches enter kindergarten with BMIs similar to those who do not eat school lunch, but by first grade, obesity rates are higher for those who consume school lunches. Arteaga and Heflin (2014) examine changes in access to meals stemming from kindergarten entry and likewise conclude that the school lunch program reduces food insecurity. Using food consumption data, Gleason and Suitor (2003) find that students who ate school lunches some, but not all, days did not consume more calories on days they ate a school lunch. Another source of variation comes from comparing children's health status when school is in session to the summer months (when school meals are less available). This work finds school meals lower food insecurity (Nord and Romig, 2006; Huang and Barnidge, 2016). Finally, Gundersen et al. (2012) document school meals are associated with a lower likelihood of poor health outcomes, including obesity and food insecurity.[5]

The empirical evidence examining school meals programs and student performance is small by comparison. Particularly for the NSLP, evaluation is hindered by the universal nature of the program and lack of program variation over time. This literature finds mixed results on the effect of NLSP on student achievement. Dunifon and Kowaleski-Jones (2003) compare outcomes between siblings where one sibling received a subsidized meal and the other did not and do not find that NSLP significantly affects positive behaviors or math or reading achievement. Closely related to this work, Hinrichs (2010) uses federal reimbursement rate reforms during the 1960s to look at longer-term effects of school meals. Using differential exposure to subsidized meals by birth cohort and state, Hinrichs (2010) finds that a 10 percentage point increase in a state's exposure to NSLP increases educational attainment by more than four months for females and nearly one year for males. He does not, however, find any effect of the NSLP on health outcomes in adulthood. One possible explanation for this finding is that access to school meals increased school attendance, but did not affect overall food consumption.

In contrast to the NSLP, school participation in the SBP has increased since the program's inception, particularly since the 1990s. Much of the work on the SBP uses this cross-school variation to compare observationally-similar students in participating and non-participating schools. This literature tends to find SBP improves nutritional intake during breakfast and reading test scores, but has mixed results on overall nutritional intake (Bhattacharya et al., 2006; Frisvold, 2015).

Closely related to the reform examined in this paper, in recent years some districts have chosen to provide universal free breakfasts outside the CEP program. Imberman and Kugler (2014) examine the staggered implementation of Breakfast in the Classroom in a single urban school district and find that serving breakfast during the school day increases reading and math scores, but does not affect grades or attendance. Dotter (2013) takes a similar

[5] It is important to note that all of the aforementioned studies examined the school meals programs prior to the implementation of revised nutritional standards. With the new standards focusing on nutritional content and lower calories, previous results may not generalize to the current program.

approach by examining a phased adoption of universal breakfast programs in San Diego and finds improvements in math and reading performance after adoption, particularly in high-poverty schools. In contrast, using data from the School Breakfast Pilot Project, Bartlett et al. (2014) find that universal breakfast programs increase the likelihood students consume breakfast at school or a nutritious breakfast, but do not significantly affect test scores or attendance. Likewise, Schanzenbach and Zaki (2014) find that each of the universal breakfast programs increased breakfast take-up and the likelihood of eating a "nutritionally substantive" breakfast, but did not affect nutritional intake, food insecurity, behavior, or academic achievement. As universal programs extend eligibility to higher-income students, but result in no change for students who qualified under the means-tested formula, any treatment effect is likely largest among students who previously did not qualify. Schwartz and Rothbart (2017) examine universal meals in New York City under Provision 2 and find schoolwide meals improve performance for all students, but the gains are much larger for students who were ineligible under the means-tested program. The present work complements these existing studies by examining changes in performance across, rather than within, districts.

3 Program Overview and Conceptual Framework

3.1 National School Lunch and School Breakfast Programs

The federal government directly provides to nutritional assistance to low-income households through SNAP and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). In addition, lower-income school-aged children are eligible for free and reduced-price (FRP) meals through the School Breakfast Program (SBP) and National School Lunch Program (NSLP). Children in families below 130 percent of the federal poverty level pay $0 for school breakfasts and lunches, while children in families up to 185 percent of the federal poverty level receive school meals at a heavily-subsidized rate. The federal government reimburses schools based on the number of free, reduced, and full-price meals served, with reimbursement rates shown in Table 1.

The original motivation for providing school-based nutritional assistance was to reduce childhood hunger. Federally-provided school-based nutritional assistance began during the Great Depression, when schools with high rates of malnourishment received excess food commodities (Hinrichs, 2010; Hoynes and Schanzenbach, 2015). A permanent NLSP began in 1946 after many men were disqualified from serving in World War II due to factors likely stemming from poor nutrition, (Gunderson, 2014). The SBP began in 1966 as a pilot program targeted in high-poverty areas with long travel distances, and was made permanent and expanded to all schools in 1975 (USDA, 2013).

Over the past 40 years, the number of school meals served has increased. More than half of 5-17 year olds received a school lunch on a typical day in 2017, and more than a quarter received a school breakfast. This represents a slight increase in the share of school-aged children receiving a school lunch (53 percent in 1990) and a larger share in the fraction of school-aged children receiving a breakfast (9 percent in 1990). This overall rise is driven by an increase in the number of free meals. Whereas about 15 percent of school lunches were FRP in 1969, this share had risen to 74 percent by 2017 (Figure 1).

Recent changes to the school meals programs have aimed to reduce administrative burdens by automatically enrolling students whose families receive SNAP or Medicaid, or by reducing the frequency that schools are required to report FRP numbers to USDA. The reform examined in this paper, the Community Eligibility Provision (CEP), is one such effort.[6] Under CEP, high-poverty schools that provide both breakfasts and lunches can opt to offer free meals to all students regardless of students' actual family incomes. To be eligible for CEP, at least 40 percent of students in a school, or combination of schools in a district, must have "categorically eligible" FRP status, meaning their families receive another form of

[6]Other funding changes include Provisions 1-3 which allow schools to offer schoolwide free meals to all students and receive reimbursement based on a base year (up to four years under Provision 2). As reimbursement depends on the share of FRP students, however, this option is most beneficial to schools where nearly all students are income-eligible. Relative to Provision 2, CEP doesn't change access to free meals, but provides an alternative federal reimbursement. If schools aim to maximize federal revenue, Provision 2 schools that take-up CEP should experience a (weak) increase in federal revenue. See USDA (2002). Importantly, these districts are included in my main analyses only if they adopted CEP by 2017 *and* had a baseline share of FRP eligibility below 57.8 percent between 2009 and 2011 (e.g.: were not operating universal programs prior to 2011).

assistance targeted to low-income families, such as SNAP, TANF, and the Food Distribution Program on Indian Reservations.

Importantly, not all schools gained access to the CEP option at the same time. CEP was incrementally rolled out across states beginning in the 2012 school year. The rollout order was determined by the Secretary of Agriculture to ensure "an adequate number and variety of schools and [districts] that could benefit from [CEP]". Schools in Illinois, Kentucky, and Michigan became eligible to participate in the 2012 school year; schools in the District of Columbia, New York, Ohio, and West Virginia were newly eligible in 2013; schools in Georgia, Florida, Maryland, and Massachusetts became eligible in 2014; and schools in the remaining states became eligible in 2015 (Figure 2).

Not all eligible schools, however, participate in the program. In the 2015 school year, about 45 percent of eligible schools participated in the program, ranging from 0 percent in New Hampshire to 81 percent in Montana (Neuberger et al., 2015). In general, CEP participation is higher among the highest-poverty schools, but even among schools where all meals would be reimbursed at the free rate under CEP, only about 63 percent participated in 2015 (Neuberger et al., 2015). Eligible schools that chose not to participate frequently mentioned financial concerns as a factor for not participating (Logan et al., 2014).[7] Over time, participation in CEP has increased within states. For example, in my sample, approximately 5 percent of districts in Kentucky, Illinois, and Michigan had at least one participating school in the 2012 school year (the first year of eligibility). By the fourth year of eligibility in 2015, this number had increased to 18 percent. However, starting in the second year of implementation, it does not appear that these early-adopting CEP schools are more disadvantaged than later-CEP adopting schools. For instance, 59 percent of students in the median school that adopted CEP in 2013 were eligible for free meals under the traditional program, the same share as in the median district that adopted CEP in 2015.

Both financial and meal access considerations affect a school's decision to participate.

[7] Even though the highest-poverty schools receive full federal reimbursement under CEP, in the first several years of the program, it was unclear how CEP would affect other funding such as Title I and state programs. The Department of Education issued guidance to clarify the interaction in spring 2015.

Based on historical income data of the share of students who are categorically-eligible, the federal government reimburses CEP schools at 1.6 times the number of categorically eligible students, up to a maximum of 100 percent. For example, a school where 40 percent of students are categorically eligible receives the full subsidy for 64 percent of the meals provided; the remaining 36 percent are subsidized at the paid price. The remaining gap must be offset by local sources. Schools where at least 62.5 percent of students are categorically eligible receive the full federal subsidy for all meals. Therefore, schools with a categorical eligible rate less than 62.5 percent (those that do not receive the full free-price federal reimbursement for all meals served) must contribute local and state sources to make up the funding shortfall, and have a muted incentive to participate in the program. Above 62.5 percent, however, federal reimbursement remains at 100 percent, but the gain relative to non-CEP status diminishes, as these schools were already receiving a high reimbursement rate from the federal government.

Schools may also decide to participate based on changes in free meal access. In contrast to the financial incentive structure, student participation benefits under CEP are greatest for schools with relatively low free- and reduced counts. For example, a school with a 64 percent FRP count would see access to free meals increase by 56 percent (36 percentage points). On the other hand, a school with a 100 percent FRP count would see no change in the number of students with access to free meals, regardless of the categorical-eligible share. A school where at least 62.5 percent of students are categorically eligible *and* all students are FRP-eligible under the traditional program experiences no change in revenues or student eligibility under CEP.

These incentives played a role in schools' participation decisions. A survey of administrators conducted the second year of CEP indicates that financial feasibility was a concern among both participating and non-participating districts. More than half of both participating and non-participating districts cited financial concerns or reimbursement rates as one of the three most important factors in deciding whether to participate, and approximately 80 percent stated that CEP would increase meal participation and increase access to healthy foods (Logan et al., 2014).

These considerations are also reflected in the school participation data. Figure 3 shows that the likelihood of participating in CEP is increasing with the baseline share of free-meal-eligible students. Within adopting districts, however, differences between districts that adopted before and after 2015 appear similar on most economic and demographic dimensions. Figures 4, 5, and 6 and Table 2 display area economic characteristics and school resource and performance measures prior to the reform for participating and non-participating schools. In each figure, the solid line shows the distribution of districts that adopted CEP prior to 2016; the dotted line shows the distribution of districts that adopted in 2016 or 2017; and the dashed line shows the distribution of schools that were not participating in CEP as of 2017. These figures and table show districts with at least one CEP-participating school are more disadvantaged than districts with no participating schools: prime-age labor force participation rates and median income are lower, and child poverty and unemployment rates are higher. Income inequality tends to be higher and baseline FRP eligibility is higher in CEP districts. In addition, student characteristics in CEP-participating districts are associated with lower levels of academic achievement: such districts have a greater share of black and ELL students, and lower baseline (2009-2011) test scores.

These patterns suggest the eligible schools that opt to adopt CEP are unlikely to be a random sample of eligible schools. In order to compare districts with similar incentives to participate, my main specifications restrict the sample to schools that participate in CEP at any point through 2017, and compare participating districts that adopted CEP earlier in this period to districts that adopt later.

3.2 Conceptual Framework

There are several non-mutually-exclusive channels through which increased access to free school meals may affect academic performance. First, by eliminating enrollment paperwork, students who are otherwise income-eligible, but who did not receive free meals prior to CEP, gain access to the program. This mechanism is potentially important given that SNAP is insufficient to cover many families' food bills (Hoynes et al., 2015). By providing a guaranteed source of breakfast and lunch throughout the month, universal free meals may help smooth the cycle of nutritional intake associated with the Food Stamp month. Second, students who are above the income cutoff are also able to receive free meals. For both of these groups, universal free meals increase the amount of family resources available for other food expenditures and consumption goods, which may benefit children.

Third, as discussed in Lazear (2001), many aspects of classroom education can be conceptualized as a public good. As with any public good, the benefits to students from being in a classroom depend not only on their own behavior, but also on the behaviors of their peers. Disruptions by others reduce the learning time of all students. The literature shows that food insecurity is linked to worsened non-cognitive skills, including externalizing behaviors (Alaimo et al., 2001). Therefore, if CEP lowers food insecurity, it may improve behaviors or reduce distractions in the classroom. Through this channel, CEP could increase effective teaching time and have spillover effects to students whose nutritional consumption does not change.

Fourth, since all students receive free meals in CEP schools, family income may become less salient and meal participation may increase for all students. Previous work examining the introduction of free school breakfasts in New York City finds universal breakfasts increased participation among all students regardless of a student's initial FRP eligibility, consistent with a role for reduced stigma (Leos-Urbel et al., 2013). Besides the direct effect of greater food consumption associated with school meal participation, reducing stigma around school meals may also create a more inclusive learning environment.

Fifth, if CEP lowers schools' administrative costs, schools may be able to allocate additional resources to educational services that directly improve student performance.

Early surveys of school administrators reported that adopting schools considered student socioeconomic and academic well-being when deciding whether to participate in CEP: more than 80 percent of participating schools responded that a "moderately" or "very" important factor was providing financial relief for families, more than half felt that the program would improve academic performance and reduce stigma for needy children (Logan et al., 2014).

Although I am unable to fully disentangle amongst these channels, the main specifications account for other changes in school resources and finances that coincided with CEP adoption in order to isolate the non-financial effect of CEP. In practice, these controls do not substantially alter the results, demonstrating that my findings are not solely due to changes in school resources.

4 Measuring CEP Participation and Achievement

To estimate the short-term effects of CEP, I combine information from several data sources. Data on CEP participationcomes from state educational agencies for the 2012 through 2014 school years and data reported to the USDA Food Research Action Center (FRAC) for the 2015 through 2017 school years.[8] I limit the sample to public and public charter schools and aggregate CEP participation at the district-grade-year level. In total, this includes approximately 64,000-67,000 district-cohort-year math and reading performance observations where at least one elementary or middle school participated in CEP at any point over the six year period. My main analyses focuson a subset of about 32,000-33,000 district-cohort-year observations that experienced increases in free meal eligibility under CEP larger than the baseline median (42.2 percentage points). Among these districts, CEP increased access to free meals by approximately 27 percentage points.[9]

Academic performance data comes from standardized achievement data harmonized for cross-state comparability from the Stanford Education Data Archive (SEDA). This novel

[8]Although states also report the categorically-eligible share to USDA, the main specifications do not include this figure. During the pilot years, some districts were permitted to stop counting the number of categorically eligible students once this number surpassed 62.5 percent. Beyond 62.5 percent, there is no change in federal reimbursement and no incentives for schools to report additional (or fewer) students. Second, districts were experimenting with how to count categorically-eligible students with minimal paperwork, which may also understate the share categorically-eligible. Third, and most importantly, categorical eligible shares are reported at the *school* level, whereas the achievement data is available at the *district* level. Aggregating to the district level is not feasible as only the subset of schools with a categorically-eligible share $>$ 40 percent were required to report this information.

[9]The average increase in free meal eligibility in this group is less than 42.2 percentage points because not all schools in these districts participated in CEP (e.g.: the average share of students in the *district* eligible under CEP is less than 100 percent).

dataset addresses several issues that have precluded comparisons of student achievement at the sub-state level across states and over time. In particular, data from the biennial National Assessment of Educational Progress (NAEP) does not include all schools and the universe of NAEP-tested schools changes each test wave, which limits the ability to make cross-district comparisons over time. An alternative data source is school-level proficiency data required of most states under the No Child Left Behind (NCLB) and the Every Student Succeeds Act (ESSA). While these data allow for comparisons within a state, each state can design its own test and its own proficiency metric. Further, states change their tests and proficiency measures over time, and reporting standards differ across states. Therefore, the raw state assessment data cannot compare student achievement across states over time.

The SEDA data uses information from these two data sources to facilitate comparable measures of proficiency across states the sub-state level. First, school-level data are aggregated to the district level. Under the methodology discussed in Reardon et al. (2016), these discrete district proficiency data are then transformed to a continuous, standardized distribution using the cross-state achievement distribution from the NAEP data. Since the NAEP data are collected biennially for fourth and eighth graders, the remaining grades and years are estimated by linear interpolation. Under the assumption that the state-grade-year distribution of NAEP scores matches the state test score distribution, these data provide district-grade-level achievement scores that are comparable over time and across states.

In addition to district-level achievement data for academic years 2009 through 2015, the SEDA data provides standardized math and reading performance gaps between white and Hispanic and between white and black students, as well as proficiency by race/ethnicity, for a subset of between about 12,000-30,000 district-cohort-year observations.[10]

The performance data is linked to a rich set of area economic and demographic characteristics from the American Community Survey (ACS). In addition, I augment the SEDA data with additional area and school characteristics that might affect student performance

[10]The SEDA data reports performance metrics for cells containing at least 20 assessment observations in each group. For example, white-black reading gaps are only available for districts in which there are at least 20 white and 20 black reading observations. Racial gaps are measured according to the standardized mean difference between the distributions for each race/ethnic group.

and CEP participation, such as county unemployment rates from the BLS Local Area Unemployment Statistics (LAUS), county per-capita income maintenance payments from the BEA Regional Economic Accounts (REIS) data, district school-aged poverty rates from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE), and detailed information about the types of school district expenditures (including federal reimbursement for nutritional assistance) from school finance data reported in the Department of Education's School Finance Survey.[11]

While the SEDA data contain more local measures of student performance than the NAEP data, one shortcoming of the SEDA database for this analysis is that it only provides district-level (rather than school- or student-level) achievement information. While entire districts could opt to participate in CEP, individual schools within a district could also participate, with the applicable categorical-eligible share determined either alone or in combination with other schools.[12] In practice, this distinction was muted in the early years of the program: between 2012 and 2015, in 71 percent of districts with any CEP participation, all students attended a CEP school (see also Figure 7). In light of this similarity, and for simplicity, by main specifications focus on a binary treatment measure.

In some specifications, I account for partial CEP participation by matching the CEP database to the US Department of Education Common Core of Data and estimating the fraction of students in cohort c in district d at time t who attend a CEP school. Results under this method are largely similar to those using a binary "any CEP" measure of participation.

4.1 Empirical Strategy

In order to examine how CEP affected student academic achievement, I estimate a panel differences-in-differences specification, comparing schools that adopted CEP at different points between 2012 and 2017. Districts where no school chose to participate in CEP are ex-

[11]Income maintenance payments include SSI, TANF, SNAP, and the EITC.

[12]For example, a school with a categorical-eligible share of 20 percent and 100 students could combine with a school in the same district with a categorical-eligible share of 80 percent and 100 students to receive a total federal reimbursement of ($\frac{20+80}{100+100} * 1.6 = 80\%$).

cluded from the analysis. Among the group of ever-participating districts, whether a district is treated in a given year depends on the state in which it is located and the year it adopted CEP. Therefore, a district can be treated for between zero years (districts that first adopted in 2017, regardless of state) and four years (schools in Illinois, Michigan, and Kentucky that adopted the first year of the pilot period).[13] For example, a district in Michigan that adopted CEP the first possible year would be "treated" in 2012, 2013, 2014, and 2015. In contrast, a school in California that adopted its first possible year would only be "treated" in 2015.[14] The base specification takes the following form:

$$y_{dsct} = \beta CEP_{dct} + X^{'}_{dct}\gamma + \theta_c + \theta_d + \theta_t + \epsilon_{dsct} \tag{1}$$

Where y_{dsct} is the normalized achievement score or proficiency gap in district d in cohort c at time t in subject $s \in \{math, reading\}$. θ_c, θ_d, and θ_t are vectors of cohort, district, and year fixed effects, respectively, accounting for factors that do not change within a district or cohort over time, and factors that change over time, but affect all states. For example, time fixed effects account for changes in school meal nutritional requirements that applied to all states in 2013. CEP_{dct} is the treatment variable and takes two potential forms. First, I consider a dichotomous treatment, equal to 1 if any school serving students in cohort c in district d participated in CEP in year t. Second, I consider a more nuanced version, where $CEP_{dct} \in [0, 1]$ is defined as the fraction of students in cohort c in district d at time t attending a school that participates in CEP. As the entire district participated in most districts with any CEP participation, these measures yield statistically similar results (Figure 7). The first treatment year for the initial round of early-adopting states is 2012; under both participation measures, $CEP_{dct} = 0$ for all districts between 2009-2011.

The coefficient of interest is β. Under the assumption that CEP timing is uncorrelated with a district's potential benefits from participating, β identifies the causal effect of CEP on

[13] Additional results reported in Appendix Table 1 show results do not significantly change when limiting the sample to districts that participated the first three years their state was eligible.

[14] An event study analysis verifies that there are not large systematic trends in achievement prior to CEP adoption among the group of ever-participating schools (Figure 11 and Appendix Figure 1).

student performance. This assumption is weakened under specifications that include X_{dct}, a vector of time-varying district-cohort level characteristics that may affect both CEP take-up and performance. X_{dct} includes the fraction of students who are Hispanic, black, or English-learners; the student-teacher ratio; per-pupil total and instructional expenditures; county unemployment rates; whether the school is located in a state that is CEP-eligible in year t; and district child poverty rates. With the inclusion of X_{dct}, a causal interpretation requires that the timing of CEP eligibility is orthogonal to potential outcomes, conditional on these observable characteristics.

Since the sample is limited to schools that participated in CEP at any point, a violation of this assumption requires districts that adopted CEP earlier had greater potential benefits than districts that adopted later. As participation timing partially depends on the state in which a district is located, this assumption requires that states with the greatest potential benefits of CEP adoption were not chosen as pilot states.[15]

Legislative details help inform the plausibility of this assumption. The legislation required that the Secretary of Agriculture "select states with an adequate number and variety of schools and [districts] that could benefit from [CEP]" (Public Law 111-296). The number of states was limited to three in the first award year (2012) and four states in the subsequent two years. In determining the first CEP states, USDA aimed to identify states with the greatest number of qualifying schools, using the share of school food authorities with at least 40 percent of students categorically eligible, as well as state SNAP participation rates and share of students receiving free school meals because of Medicaid enrollment. For the 2012 award year, Alaska, DC, Illinois, Kentucky, Louisiana, Michigan, Mississippi, Oklahoma, South Carolina, and Tennessee were eligible to apply (USDA 2011). In the following award years, all states were eligible to apply and states were chosen based on knowledge of CEP procedures, awareness of CEP among districts, the state's approaches to interagency communication around poverty data, and eligibility and likeliness of participation (USDA, 2012, 2013). Importantly, baseline academic performance was not a factor in selecting the pilot states,

[15]This assumption would be violated, for example, if pilot states were chosen based on previous academic performance, or in a "Race-to-the-Top"-style competitive granting process.

and of the seven candidate states not selected the 2012 school year, only DC became eligible to adopt before the national rollout.

In addition to introducing CEP, the 2010 legislation changed several other components of the school lunch and breakfast programs in an effort to increase access and improve the nutritional quality of school meals. For example, the legislation called for greater direct certification, whereby students receiving Medicaid automatically receive free meals. It also provided competitive grants to states that expanded SBP. In addition, updated nutritional guidelines became effective in the 2013 school year, and required school meals provide healthier foods than previous guidelines. In contrast to the previous requirements, which set calorie floors for each grade level, the 2010 legislation instituted both calorie floors and ceilings. The calorie changes were the largest for fourth through eighth graders for whom the current calorie ceiling is less than the previous calorie floor. While each of these other provisions might affect student outcomes, unlike CEP, these other changes affected all states at the same time and regardless of CEP participation. Moreover, Harkness et al. (2015) find that CEP did not have a significant impact on the quality of meals served. Therefore, while simple before-and-after analyses are likely confounded by these additional changes, year fixed effects will account for these concurrent changes and isolate the effect of CEP.

5 Results

5.1 CEP and School Meal Participation

Before turning to academic outcomes, I examine whether CEP affected school meal take-up. Previous work shows CEP increased school meal participation, particularly breakfast consumption, in CEP schools relative to non-CEP schools (Logan et al., 2014; Levin and Neuberger, 2013).

To verify districts in my sample experienced an increase in school meal consumption following CEP, I use school-level meal count data for breakfasts and lunches for six of the eleven states that adopted CEP before 2015: Georgia, Illinois, Kentucky, New York, Maryland, and

West Virginia. Data availability varies by state, and in total, the meal participation data cover approximately 18,500-20,100 school-year observations spanning 2009 through 2016. I merge the meal count data to school-level enrollment information from the Department of Education's Common Core of Data and school-level CEP participation. Following an approach similar to equation (1) for the full universe of schools in these states, Table 3 shows the annual number of breakfasts served increased by 24 meals a student (about 46 percent), and the number of lunches served increased by 12 per student (about 11 percent). An event study framework illustrated in Figure 8 shows similar results: while lunch participation appears to be slightly increasing prior to CEP adoption, both breakfast and lunch participation jumped after CEP participation. Consistent with meal count data, column (3) of Table 3 and Figure 9 show per-pupil federal nutritional assistance payments increased about 9 percent after CEP adoption for districts the main analytical sample.

These average effects mask heterogeneity across the FRP distribution. Consider the following thought experiment: a school where 40 percent of students received free meals prior to CEP could see the number of free meals increase up to 60 percentage points. In contrast, a school where all students received free meals at baseline is unable to increase the number of free meals, regardless of CEP takeup. Figure 10 divides the sample into four quartiles based on pre-CEP FRP participation. Although results are imprecise due to small sample sizes, these figures suggest the increase in free meals is negatively correlated with baseline provision: whereas schools in the first quartile (the schools with the lowest baseline FRP share that eventually participated in CEP) increased the number of free breakfasts (lunches) served by 25 (12) log points, the poorest quartile increased the number by about 17 (8) log points.

5.2 CEP and Academic Performance

To estimate the effect of CEP on academic achievement, I follow equation (1) with the sample of ever-participating schools. Table 4 displays the main achievement results for math and reading in panels (a) and (b), respectively. Column (1) estimates the base specification, including district, cohort, and year fixed effects, but without controlling for time-varying district resources or economic conditions. Column (1) suggests that CEP did not improve math performance, but this effect is imprecise and small in magnitude. Reading performance, on the other hand, appears to have slightly worsened following CEP adoption.

Column (2) adds a vector of time-varying district characteristics: student demographic information, per-public expenditures, student-teacher ratios, district poverty rates, per-capita transfers, and county unemployment rates. After accounting for these factors, there are no significant improvements in math or reading performance. Column (3) reports the results using a continuous measure of participation: the share of students in cohort c at time t attending a CEP school. Conditional on district participation, the share of students attending a CEP school is high (Figure 7); accordingly, results do not meaningfully change under this approach. For simplicity, the remaining results will focus on the discrete participation treatment effect and district resource controls in Column (2). Columns (4) through (6) replicate the specification in Column (2), separately examining the effects among black, Hispanic, and white students. Comparing districts that adopted CEP during the sample period to those that adopted later, the basic approach does not show improvements in reading or math performance following CEP adoption for any race or ethnic group.

As previously discussed, and shown from the meal participation results, the extent to which CEP marked a change from the status quo varied depending on the fraction of students that were eligible for free and reduced meals prior to reform. In particular, a school with a high baseline FRP share saw little change in access to free meals, as most students would be eligible to receive a free meal regardless of CEP adoption. In contrast, while CEP was not available to the lowest-poverty districts, districts on the eligibility cusp – those with a FRP just above the eligibility cutoff of 40 percent – saw a marked increase in eligibility of up to 60 percentage points.

The remainder of my analysis focuses on the subset of districts for which CEP offered a substantial change from the status quo. In particular, I define a "high-exposure" sample as districts that adopted CEP at any point between 2012 and 2017 and had relatively low fractions of students eligible for free meals between 2009 and 2011, defined as the baseline median of all districts that adopted CEP by 2017 (57.8 percent). In this group of schools, average eligibility for free meals increased about 27 percentage points under CEP relative to the 2009-2011 share. While this increase is larger than the change in the number of lunches served from the meal participation data, recall that the consumption data includes free, reduced, and paid meals. Under CEP, some students who previously purchased a school meal would continue to receive a school meal (e.g.: no change in the number of meals served), but would have no out-of-pocket costs. Further, although CEP extended free meal eligibility to all students in a school, some students may choose to bring a lunch from home.

Returning to the main regression results on the "high exposure" sample, Table 5 shows an improvement in math performance of about 0.02 SD. Among this group of schools, an average of 45.3 percent of students were free-meal eligible under the traditional program; scaling the ITT result by the fraction of students gaining access to free meals under CEP (26.9 percent) implies that school meals improve math performance by approximately 0.07 SD. Performance among black students, a group with high baseline participation rates and therefore less likely to gain access to free meals under CEP, did not significantly improve. Hispanic and white students' performance substantially improved, by 0.03 SD and 0.02 SD, respectively. Both of these groups are likely to gain access to free meals as part of CEP. First, white students have lower baseline eligibility rates than other groups, so universal access is most likely to reduce the prices these students pay. Second, take-up rates of other forms of nutritional assistance are lower among income-eligible Hispanics than other groups, suggesting the marginal benefits of universal meals may be greater for low-income Hispanic students (Chaparro et al., 2014; Goerge et al., 2009). While white-black proficiency gaps widened following reform, this gap is driven by absolute gains of white students, rather than worsened performance among their black peers. As both Hispanic and white performance improved, there is no significant change in the white-Hispanic proficiency gap.

An alternative way to evaluate heterogeneous treatment effects by changes in free meal eligibility is to instrument changes in meal consumption by CEP participation. The meal data are only available for schools in six states (not all of which include breakdowns by payment category), which limits the feasibility of this approach. As an alternative, I consider how the change in federal funding for school meals induced by CEP participation affects student performance. Recall that under CEP, schools are reimbursed at the free rate for the number of meals served multiplied by 1.6 times the share of categorically eligible students, up to a maximum of 100 percent. One shortcoming of this approach, however, is that federal financing is not disaggregated by grade and is only available at the district level. Further, the kink in the funding formula at 62.5 percent ISP presents non-linearities in the funding schedule. With these caveats in mind, Appendix Table 9 reports results from instrumenting per-student federal school meal spending on student performance, and shows that an additional $1,000 per student in school meals induced by CEP adoption increases math scores by 0.043 standard deviations (column 1) and reading scores by 0.015 standard deviations (column 3). As the average school in the full sample received approximately an additional $100 in federal school meals funding under CEP, this implies an improvement of 0.002 to 0.004 standard deviations, relatively similar to the results in Table 4. Also consistent with the main results in Table 5, Appendix Table 9 shows that each additional dollar in federal nutritional spending leads to larger test score gains in areas that had relatively low participation prior to CEP (columns 2 and 4). Interestingly, changes in federal funding are similar for both schools (about $100), suggesting that not only additional federal spending, but access to free meals, is important for student performance.[16]

To probe the robustness of these results to other measures of increased exposure to universal meals, Appendix Tables 4-7 return to the full sample of CEP-participating schools and split the sample by other baseline characteristics correlated with FRP status: SNAP participation, income assistance payments, and baseline shares of black and Hispanic students. Focusing on math performance, while most of results are not statistically different from my main findings, Appendix Table 5 suggest benefits are larger in areas with relatively

[16]Recall that federal reimbursement depends on the fraction of categorically-eligible students, whereas the baseline amount of federal funding depends on the share of students eligible under the traditional program. The increase in per-student revenue depends on the ratio between these groups and the distance from the 62.5 percent kink, as discussed in Section 3.

low participation in other forms of nutritional assistance (SNAP). In principle, SNAP recipients are automatically enrolled in free meals programs, so this measure should be highly correlated with the main results. Again, any benefits are concentrated among white and Hispanic students.

To provide a fuller picture of performance changes by baseline eligibility, Appendix Figure 2 plots coefficients from four separate regressions limiting the sample to each quartile of the baseline free meal distribution. Although results are somewhat imprecise, point estimates suggest gains through the 75th percentile of the eligibility distribution for Hispanic and white math performance (row 2, center and right figure). In contrast, schools with the highest free meal eligibility prior to CEP show no significant (and if anything, a slight *negative*) performance effect for white and Hispanic students.

One outstanding question is whether treatment effects vary by grade. Table 6 presents results for elementary (fourth and fifth) and middle school (seventh and eighth) graders and eighth Hispanic and white students by subject. Results are less precise due to relatively small samples, and cannot rule out equal treatment effects between younger and older students.

In a series of robustness checks, I confirm the results are not driven by a single grade or state. Appendix Table 8 shows that overall performance improvements are smaller when excluding districts in the South; however, in interpreting these results, two points are important. First, the results are not significantly different from the full national results. Second, six of the eleven pilot states (states that became eligible prior to 2015) are located in the south. Eliminating this region from the sample substantially reduces the amount of cross-year variation in CEP adoption.

Further robustness checks probe sensitivity to the sample definition. Appendix Table 1 shows results are robust to limiting the sample to districts that participate within three years of initial eligibility. Second, all of the results presented thus far have been weighted by district, indicating the effect of universal meals on the average *district's* test scores. Alternatively, Appendix Table 2 presents results weighing by the number of students to uncover the treatment effect on the average student. Finally, Appendix Table 3 limits the sample to districts-grade observations where all students in a grade attend a single school. In these districts, district CEP participation is synonymous with full school participation, and the reported district scores are school-level scores. While results are less precise due to small sample sizes, particularly for Hispanic and black students, these tables continue to show that math improvement is concentrated among Hispanic students, with smaller gains for white students. In sum, these tables show the main findings are not driven by especially large or small areas, or areas that immediately implemented CEP upon eligibility.

One possible explanation for the results is that the findings capture other changes occurring in the district at the same time of CEP adoption, or that schools choosing to adopt CEP were trending differently prior to implementation. To provide graphical evidence supporting the validity of my research design for this main sample, and investigate the extent to which early-adopting schools differ in pre-treatment performance trends to districts that adopted CEP later, I estimate an event study framework:

$$y_{dct} = \sum_{p=-5}^{3} [\beta_p (eventyear)_{dcpt}] + X'_{dcp}\gamma + \theta_d + \theta_c + \theta_t + \epsilon_{dct} \tag{2}$$

Where $p = -1$ is the year before a district adopted CEP. In addition to providing a visual verification of pre-participation differences in academic performance between early- and late-adopting CEP schools, the event study framework also addresses the extent to which districts gained expertise in implementing CEP over time by examining the difference between β_0 through β_3.

Figure 11 explores the extent to which the districts that adopted CEP had different math trends in the years leading up to adoption using an event study framework, described in equation (2). First, and most importantly for the validity of the research design, there does not appear to be strong differential trends in academic performance prior to CEP implementation. Second, these graphs no significant improvement in math for most subgroups, but slight improvements for white and Hispanic students. While there is no systematic pattern in white students' performance with each additional year of exposure to CEP, for

Hispanic students, Figure 11 suggests that the benefits of universal meals increase with each additional year of exposure to the program. Appendix Figure 1 shows comparable results for reading. Although it does not appear that reading performance was trending differently prior to CEP adoption, consistent with the findings of Table 5, these figures do not show a significant improvement in reading following CEP adoption.

One possibility is that CEP adoption coincided with other changes in school resources or the educational environment. While my preferred results control for changes in demographic composition and school resources, I can examine this hypothesis directly by omitting these controls and examining the effect of CEP on school resources. Table 7 shows that while total federal revenues increased by about 2 percent following CEP adoption, federal revenues other than nutritional assistance payments (Column 2) did not significantly change, nor did per-pupil total or instructional expenditures (columns 4 and 5). Per-capita transfers slightly fall following adoption, partly reflecting decreases in child poverty and the unemployment rate in adopting areas. Finally, student-teacher ratios did not significantly change.

Even if early and late-adopting districts were similar at baseline, the adoption of CEP changed the composition of the district. Such violations would arise, if for example, families with the greatest need for school meals moved into districts that adopted CEP or from private schools to public schools once CEP was implemented. To address this concern, I regress area demographic characteristics on CEP participation in Appendix Table 10. These results show no changes in the share of black or special education students enrolled in a district following CEP implementation. Although the fraction of Hispanic and ELL students significantly decreased, these results are very small in magnitude and do not suggest that district composition is a driving force in the observed results.

Another potential explanation stems from other provisions introduced at the same time as CEP. All schools, regardless of CEP participation, were required to implement the revised dietary guidelines for school meals beginning in 2013. While time fixed effects account for factors that affect all districts at the same time, it may have been the case that schools in pilot states were less able to implement these updated nutrition standards as they were simultaneously experimenting with CEP implementation. In this case, CEP would be associated with a higher quantity, but lower quality of school meals. Recent work by Anderson et al. (2017) shows that healthier school meals improve student performance, and if CEP schools were less able to implement the new guidelines, any benefits of the quantity of meals could be negated by poorer quality. ALthough I am unable to directly test this hypothesis with the available data, previous work does not find CEP pilot schools were better able to implement the new nutrition standards (Harkness et al., 2015), suggesting that the results are not driven by differences in school meal quality.

6 Conclusion

This paper finds that universal access to free school meals increases school breakfast and lunch participation, and improves math performance among groups of students that were relatively unlikely to receive free meals under the traditional, means-tested program, namely Hispanic and white students. Results are not driven by concurrent changes in school resources or observable features of the school environment.

In interpreting these results, it is important to note that district-level data may mask gains that are concentrated among subgroups of students. The existing literature concludes effects of nutritional assistance are concentrated among particularly disadvantaged students. When reconciling these findings, several points are relevant. First, scores by race and ethnicity can provide insights into this heterogeneity only if race/ethnicity is correlated with students' socioeconomic status. In addition, there are two types of students who previously did not qualify for free meals under the traditional program, and are therefore likely to gain access to free meals under CEP. One type is a student who lives in a high-poverty district, but whose family income is greater than the cutoff or free meals. The marginal benefit of additional nutritional assistance for these students is less than the benefit for more disadvantaged students. The second type is a student who is income-eligible for the traditional program, but who is not receiving other forms of assistance and whose families did not complete the necessary paperwork. For this student, the marginal benefit of universal meals is likely larger for these students. Given the aggregate nature of district-level data, I am unable to fully explore the extent to which *individual* benefits differ by receipt of other forms of assistance or family income.

Finally, while I do find significant improvements in math performance, it is important to stress that these findings are relatively small in magnitude. Directly related to CEP, Dotter (2013) found that the staggered introduction of another nutritional intervention, Breakfast in the Classroom (BIC) improved reading test scores by 0.10 standard deviations and math scores by 0.15 standard deviations, while Imberman and Kugler (2014) find reading (math) test scores increased 0.06 (0.09) standard deviations in a similar program. My results are small relative to both of these effects, even scaling by the participation gains. However, considering the size and generosity of the implied income transfer, the benefits provided through CEP are in line with other modest interventions that affect child health and family income. For example, an additional $1,000 in EITC refunds increases test scores for 3-8th graders by about 0.04 standard deviations (Dahl and Lochner, 2017). By these metrics, CEP delivered benefits on the order of a $500 income transfer (or 110 free breakfasts and lunches) to each student. Taken as a whole, these results suggest that school-based assistance can yield important benefits on an order of magnitude similar to other forms of income support.

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7 Tables

Table 1: Federal Reimbursement Rates for School Meals, 2017-2018

	Free	Reduced	Paid	Nutrition Quality
		Breakfast		
48 Contiguous States	\$1.66 (\$1.99)	\$1.36 (\$1.69)	\$0.29	
Alaska	\$2.66 (\$3.19)	\$2.36 (\$2.89)	\$0.43	
Hawaii	\$1.94 (\$2.32)	\$1.64 (\$2.02)	\$0.33	
		Lunch		
48 Contiguous States	\$3.23 (\$3.29)	\$2.83 (\$2.89)	\$0.31 (\$0.33)	\$0.06
Alaska	\$5.24 (\$5.26)	\$4.84 (\$4.86)	\$0.50 (\$0.52)	\$0.06
Hawaii	\$3.78 (\$3.80)	\$3.38 (\$3.40)	\$0.36 (\$0.38)	\$0.06

Source: USDA (2017). Left numbers show the base rate; right numbers show the rate for high-poverty schools (for lunch, those with at least 60 percent of students receiving free or reduced meals; for breakfast, those with at least 40 percent of students receiving free or reduced meals). In addition, schools may receive an additional 6 cents per lunch for serving fruits and vegetables.

Table 2: Baseline District Summary Statistics

	(1) Early Adopters	(2) Late Adopters	(3) Never Adopters
Pct FRL	0.590	0.546	0.331
	(0.163)	(0.160)	(0.183)
Pct ELL	0.0611	0.0589	0.0365
	(0.103)	(0.0956)	(0.0695)
Pct black	0.229	0.171	0.0595
	(0.292)	(0.237)	(0.126)
Pct Hispanic	0.159	0.171	0.115
	(0.248)	(0.257)	(0.185)
Pct special ed	0.145	0.144	0.135
	(0.0505)	(0.0632)	(0.0551)
Student-teacher ratio	15.53	17.80	15.07
	(4.696)	(77.76)	(4.487)
Per pupil nutrition asst (1000s)	0.401	0.371	0.240
	(0.148)	(0.134)	(0.136)
Total per pupil expend (1000s)	13.45	13.64	14.02
	(3.768)	(4.326)	(4.390)
Per pupil instruct expend (1000s)	6.965	7.063	7.284
	(2.052)	(2.347)	(2.389)
Pct single mom	0.336	0.300	0.208
	(0.129)	(0.112)	(0.0947)
Median hh income	46666.5	50801.3	73124.8
	(13709.4)	(15511.9)	(29644.7)
90-10 ratio	11.31	10.36	7.651
	(4.721)	(4.368)	(3.822)
Pct college ed	0.170	0.173	0.249
	(0.0874)	(0.0923)	(0.146)
Child pov rate	0.260	0.230	0.131
	(0.108)	(0.101)	(0.0911)
Pct receiving SNAP	0.288	0.248	0.134
	(0.115)	(0.108)	(0.0984)
PC income asst	1.152	1.067	0.818
	(0.419)	(0.350)	(0.298)
Unemployment rate	0.0600	0.0541	0.0435
	(0.0267)	(0.0244)	(0.0212)
Gini	0.410	0.396	0.348
	(0.0488)	(0.0514)	(0.0549)
Male LFPR	0.757	0.775	0.839
	(0.111)	(0.107)	(0.0970)
Female LFPR	0.672	0.681	0.725
	(0.0825)	(0.0795)	(0.0799)
Observations	1241	850	10075

mean coefficients; sd in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Early-adopting districts are districts in which at least one school adopted CEP by 2015. Late-adopting districts are districts in the remaining states in which at least one school adopted CEP by 2017. Never-adopting districts are districts that had not adopted CEP by 2017. All characteristics collapse to the district (rather than district-cohort) level. All dollars in 2017 constant thousand dollars. School and student characteristics are averaged over the 2009-2011 baseline period from the CCD as part of the SEDA database and BEA REIS; socioeconomic characteristics from pooled 2006-2010 ACS as part of the SEDA database.

Table 3: Breakfasts and Lunches Served

	(1) Per student bfast b/se/stats	(2) Per student lunch b/se/stats	(3) Log per student nutrit. asst b/se/stats
CEP	23.719***	11.757***	0.092***
	(3.463)	(1.992)	(0.012)
Observations	18530	20108	62290
R^2	0.081	0.008	0.163
Baseline cntl mean level	51.51	104.8	0.324
Level	School	School	District

Notes: Meal count data (columns 1 and 2) collected from state Department of Educations for six of the eleven states that adopted CEP before 2015: Georgia, Illinois, Kentucky, New York, Maryland, and West Virginia. Data availability varies by state, but spans 2009-2016. Column 3 presents federal nutritional assistance dollarts to districts from the Annual Survey of School System Finances. All specifications include year and school fixed effects. Robust standard errors clustered by district.

Table 4: Effect of CEP on Academic Performance: Full Sample

(a) Math

	(1) Performance b/se/stats	(2) Performance b/se/stats	(3) Performance b/se/stats	(4) Black perform b/se/stats	(5) Hispanic perform b/se/stats	(6) White perform b/se/stats
CEP	-0.008	0.002		-0.006	0.013	0.010
	(0.006)	(0.006)		(0.009)	(0.010)	(0.007)
CEP (pct)			-0.009			
			(0.007)			
Area and district controls	No	Yes	Yes	Yes	Yes	Yes
Observations	64315	64315	64278	31325	25427	51250
Sample	CEP districts	CEP districts	CEP districts	CEP districts	CEP districts	CEP districts
Baseline DV mean	-0.273	-0.273	-0.273	-0.583	-0.370	-0.0310
FRP gain	0.216	0.216	0.216	0.167	0.185	0.228
r2	0.0468	0.0564	0.0563	0.0736	0.0477	0.0588

(b) Reading

	(1) Performance b/se/stats	(2) Performance b/se/stats	(3) Performance b/se/stats	(4) Black perform b/se/stats	(5) Hispanic perform b/se/stats	(6) White perform b/se/stats
CEP	-0.014***	-0.000		-0.004	-0.007	0.005
	(0.005)	(0.005)		(0.007)	(0.008)	(0.005)
CEP (pct)			-0.009			
			(0.006)			
Area and district controls	No	Yes	Yes	Yes	Yes	Yes
Observations	66574	66574	66536	32316	26410	52975
Sample	CEP districts	CEP districts	CEP districts	CEP districts	CEP districts	CEP districts
Baseline DV mean	-0.262	-0.262	-0.262	-0.526	-0.454	0.00498
FRP gain	0.220	0.220	0.220	0.169	0.197	0.230
r2	0.0547	0.0677	0.0678	0.106	0.0671	0.0686

Notes: Robust standard errors clustered by district. Race/ethnic proficiency scores available for cells with at least 20 students. Sample limited to districts with at least one school participating in CEP by 2017. Treatment districts are districts in which at least one school adopts CEP by 2015. Columns 1-2, and 3-6 define a CEP as a binary treatment that equals one if any school in a district with students in cohort c participates in CEP. Column 3 defines CEP as the share of students attending a CEP school based on school-level CEP information and enrollment data from the Common Core of Data. “FRP gain” is the share of students gaining access to free meals under CEP relative to the baseline (2009-2011) period in CEP-adopting schools. All specifications include district, cohort, and year fixed effects. “Area and district controls” include student racial/ethnic composition, per-student expenditures, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. District covariates from the Common Core of Data, SEDA, Small Area Income and Poverty Estimates, and Local Area Unemployment Statistics.

Table 5: Effect of CEP on Academic Performance: High-Exposure Districts

(a) Math

	(1) Performance b/se/stats	(2) Black perform b/se/stats	(3) Hispanic perform b/se/stats	(4) White perform b/se/stats	(5) WB gap b/se/stats	(6) WH gap b/se/stats
CEP	0.019**	0.001	0.031**	0.019**	0.030***	0.008
	(0.008)	(0.014)	(0.013)	(0.009)	(0.011)	(0.012)
Area and district controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32090	12310	12994	29322	11969	11776
Baseline FRL below	.578	.578	.578	.578	.578	.578
Baseline DV mean	-0.122	-0.501	-0.309	0.0158	0.633	0.458
FRP gain	0.269	0.186	0.196	0.263	0.184	0.167
r2	0.0385	0.0534	0.0426	0.0420	0.0122	0.0189

(b) Reading

	(1) Performance b/se/stats	(2) Black perform b/se/stats	(3) Hispanic perform b/se/stats	(4) White perform b/se/stats	(5) WB gap b/se/stats	(6) WH gap b/se/stats
CEP	0.010	0.003	-0.003	0.015**	0.018*	0.014
	(0.007)	(0.011)	(0.011)	(0.007)	(0.010)	(0.012)
Area and district controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	33348	12741	13484	30349	12393	12055
Baseline FRL below	.578	.578	.578	.578	.578	.578
Baseline DV mean	-0.105	-0.440	-0.387	0.0556	0.600	0.537
FRP gain	0.275	0.188	0.205	0.265	0.186	0.166
r2	0.0620	0.0942	0.0883	0.0661	0.0165	0.0274

Notes: Robust standard errors clustered by district. Proficiency gaps and performance by race and ethnic group available for cells with at least 20 black or 20 Hispanic students. Sample limited to districts with at least one school participating in CEP by 2017 with a baseline FRP share of 57.8 percent or less. Treatment districts are districts in which at least one school adopts CEP by 2015. CEP is defined as a binary treatment that equals one for years in which any school in a district with students in cohort c participates in CEP. "FRP gain" is defined as the share of students gaining access to free meals under CEP relative to the baseline (2009-2011) period in CEP-adopting schools serving students in cohort c in district d at time t. All specifications include district, cohort, and year fixed effects. "Area and district controls" include student racial/ethnic composition, per-student expenditures, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. District covariates from the Department of Education Common Core of Data, the SEDA database, Census Small Area Income and Poverty Estimates, and BLS Local Area Unemployment Statistics.

Table 6: Effects of CEP on Academic Performance in High-Exposure Districts: By Grade and Race

	(1) Hispanic b/se/stats	(2) Hispanic b/se/stats	(3) Hispanic b/se/stats	(4) Hispanic b/se/stats	(5) White b/se/stats	(6) White b/se/stats	(7) White b/se/stats	(8) White b/se/stats
CEP	0.037**	0.003	0.008	-0.005	0.010	0.012	0.007	0.023**
	(0.019)	(0.022)	(0.015)	(0.020)	(0.012)	(0.014)	(0.010)	(0.011)
Area and district controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4951	3423	4991	3910	10594	8777	10807	9386
Baseline FRL below	.578	.578	.578	.578	.578	.578	.578	.578
Grade	4 and 5	7 and 8	4 and 5	7 and 8	4 and 5	7 and 8	4 and 5	7 and 8
Baseline DV mean	-0.275	-0.351	-0.349	-0.461	0.0456	-0.0582	0.0983	-0.0345
FRP gain	0.191	0.184	0.186	0.237	0.252	0.281	0.253	0.281
Subject	Math	Math	Reading	Reading	Math	Math	Reading	Reading
r2	0.0353	0.0473	0.0638	0.147	0.0304	0.0246	0.0279	0.0617

Notes: Robust standard errors clustered by district. Sample limited to districts in which at least one school participates in CEP by 2017 with a baseline FRP share of 57.8 percent or less. Treatment districts are districts in which at least one school adopts CEP by 2015. "FRP gain" is defined as the share of students gaining access to free meals under CEP relative to the baseline (2009-2011) period in CEP-adopting schools serving students in cohort c in district d at time t. All specifications include district, cohort, and year fixed effects. Baseline variables defined as 2009-2011 average. "Area and district" controls include student racial/ethnic composition, per-student expenditures, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. All dollars (baseline means in levels) in $1,000 of 2017 dollars. District covariates from the Department of Education Common Core of Data, the SEDA database, Census Small Area Income and Poverty Estimates, and BLS Local Area Unemployment Statistics.

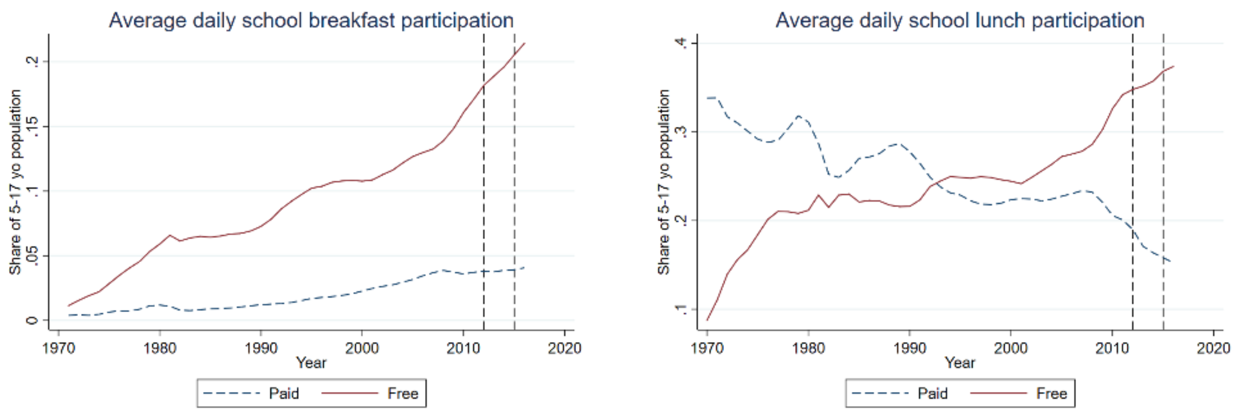
Table 7: Effect of CEP on School Resources, High Exposure Sample.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log fed revenue	Log revenue (- nutr. asst)	Log pc transfers	Log per-pupil expend	Log per-pupil instr.	Student-teacher ratio
	b/se/stats	b/se/stats	b/se/stats	b/se/stats	b/se/stats	b/se/stats
CEP	0.018	0.006	-0.009***	0.007	-0.020***	-0.193
	(0.012)	(0.006)	(0.003)	(0.011)	(0.007)	(0.126)
Area and district controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32200	32272	32023	27742	28008	32272
Baseline FRL below	.578	.578	.578	.578	.578	.578
Baseline DV mean (level)	1.745	13.18	0.990	13.09	6.892	15.47
CEP gain	0.273	0.273	0.273	0.275	0.275	0.273
r2	0.389	0.0855	0.414	0.100	0.222	0.432

Notes: Robust standard errors clustered by district. Sample limited to districts in which at least one school participates in CEP by 2017 with a baseline FRL share of 57.8 percent or less. Treatment districts are districts in which at least one school adopts CEP by 2015. All specifications include district, cohort, and year fixed effects. Baseline variables defined as per-student 2009-2011 average. "Area and district" controls include student racial/ethnic composition, per-student expenditures, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. All dollars (baseline means in levels) in $1,000 of 2017 dollars. District covariates from the Department of Education Common Core of Data, the SEDA database, Census Small Area Income and Poverty Estimates, and BLS Local Area Unemployment Statistics.

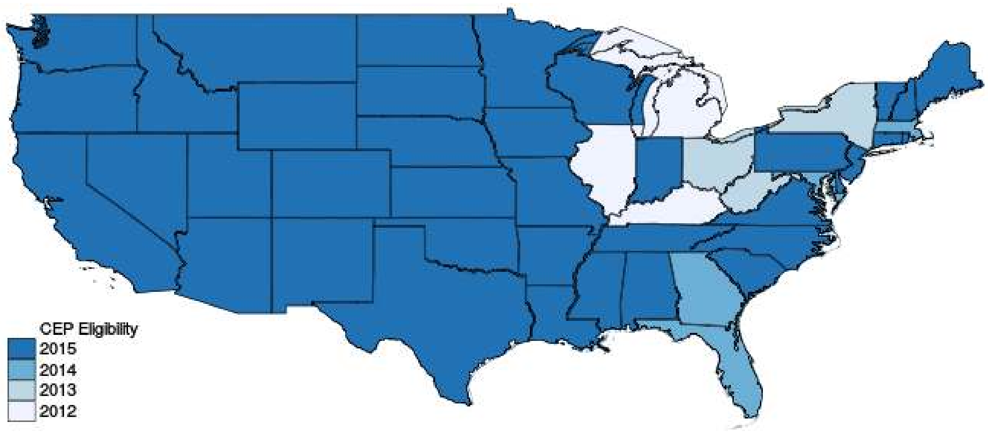
8 Figures

Figure 1: Fraction 5-17 Year-Olds Receiving School Meals by Payment Category



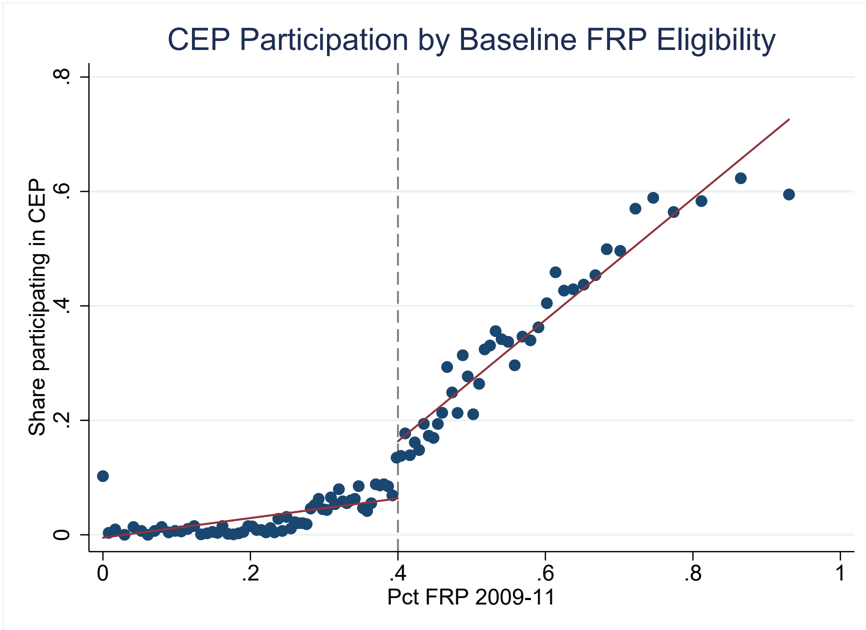
Source: School meal counts from USDA (2018). Population estimates from Census Bureau decennial census and intercensal estimates. Left dashed line denotes 2012, the year schools in the first states became eligible to adopt CEP. Right dashed line denotes 2015, the first year schools in all states were eligible to adopt CEP.

Figure 2: States by First Year of CEP Eligibility



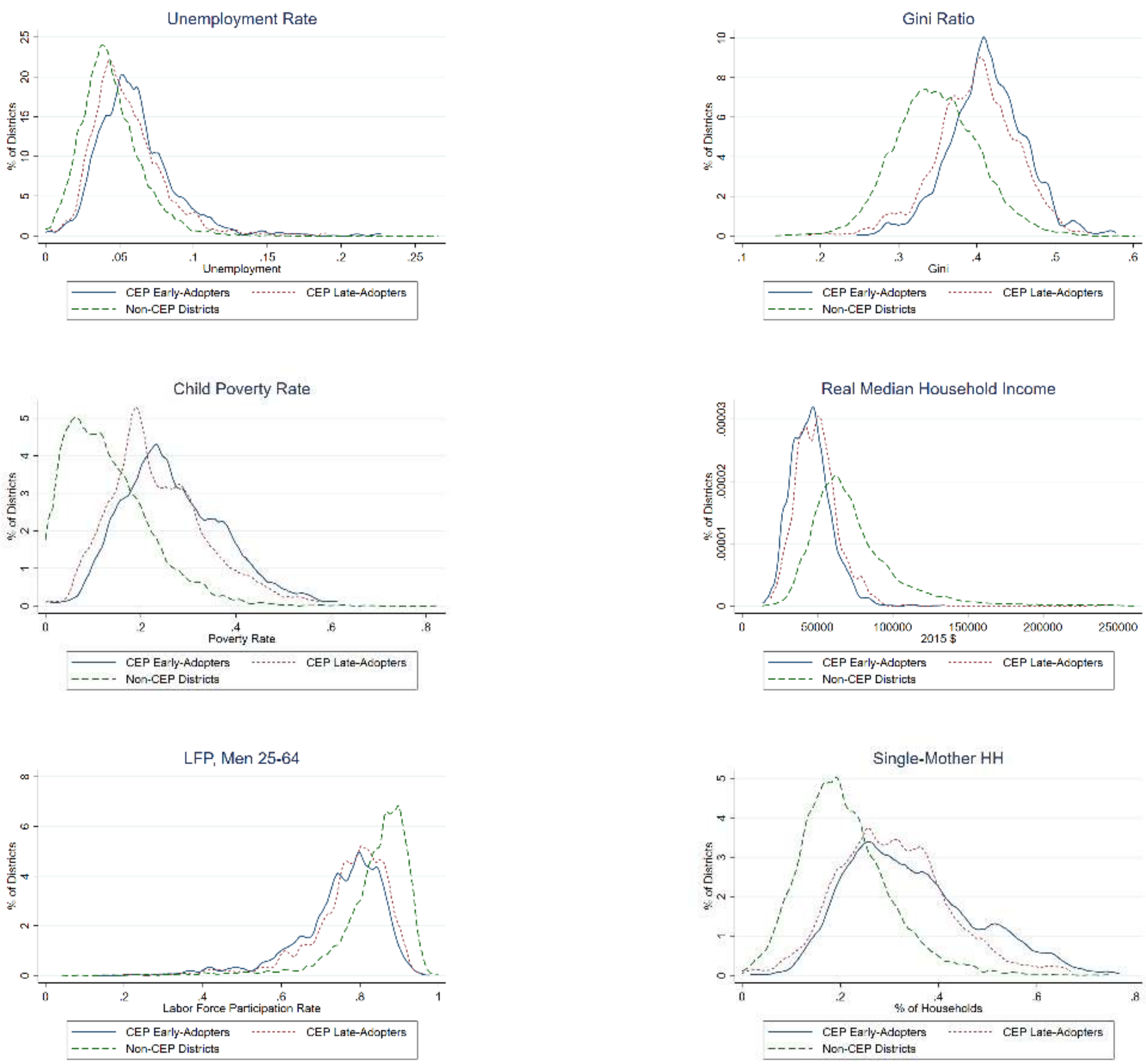
Source: USDA, 2011, 2012, 2013, 2014.

Figure 3: CEP Participation by Baseline FRP



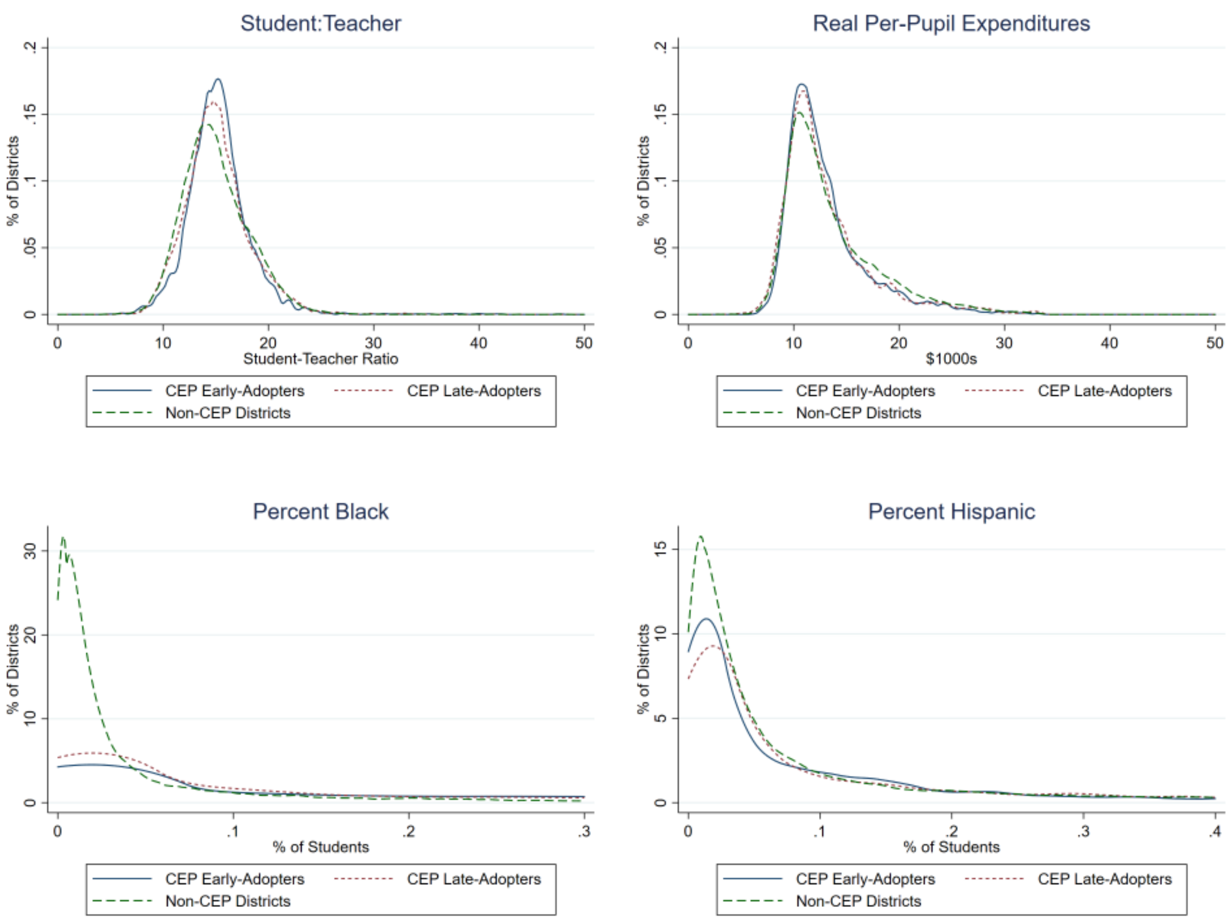
Notes: Figure plots relationship between baseline share of FRP students in a district in 2009-2011 (horizontal axis) and the probability a district participated in CEP by 2015 (vertical axis) from a binscatter of 100 equal-sized bins. The first vertical line at 40 percent shows the minimum share FRP students in a district in order to fully participate in CEP. Source: USDA FRAC/CBPP, Common Core of Data.

Figure 4: Measures of Economic-Well-being by CEP Adoption



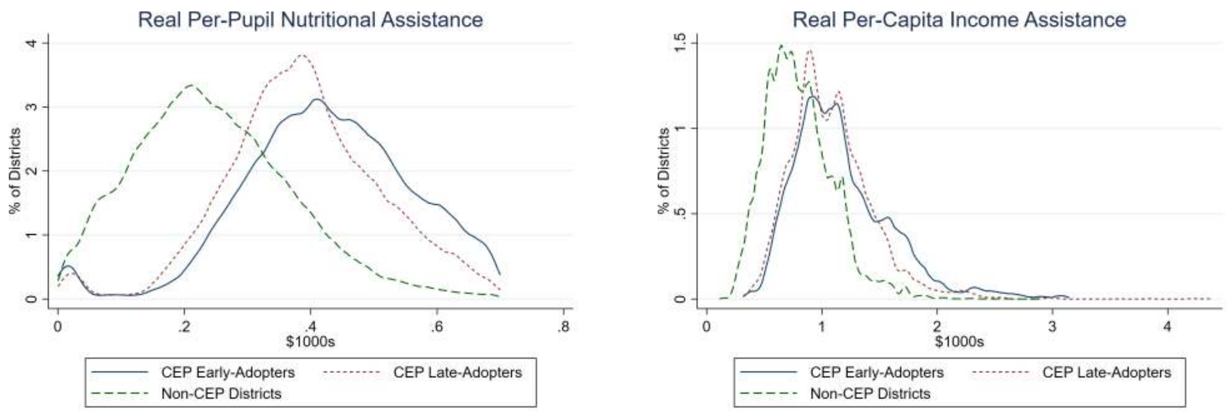
Note: Early-adopting districts are districts in which at least one school adopted CEP by 2015. Late-adopting districts are districts in which at least one school adopted CEP by 2017. Non-CEP districts are districts that did not adopt CEP. Student and district characteristics are estimated as the average from 2009-2011. All data available through SEDA.

Figure 5: Student Characteristics by CEP Adoption



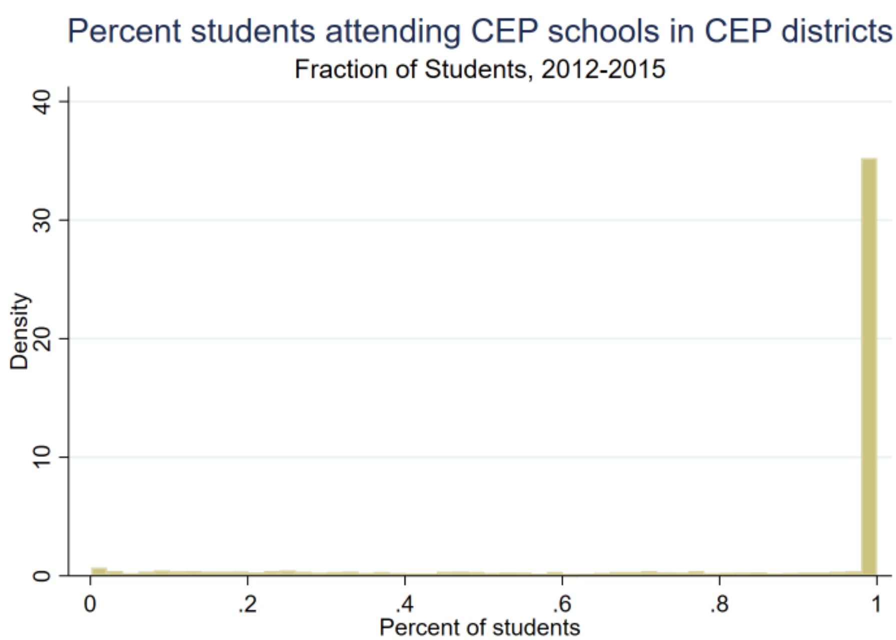
Note: Early-adopting districts are districts in which at least one school adopted CEP by 2015. Late-adopting districts are districts in which at least one school adopted CEP by 2017. Non-CEP districts are districts that did not adopt CEP. Student and district characteristics are estimated as the average from 2009-2011. All data available through SEDA.

Figure 6: Student Baseline Performance and Meal Participation by CEP Adoption



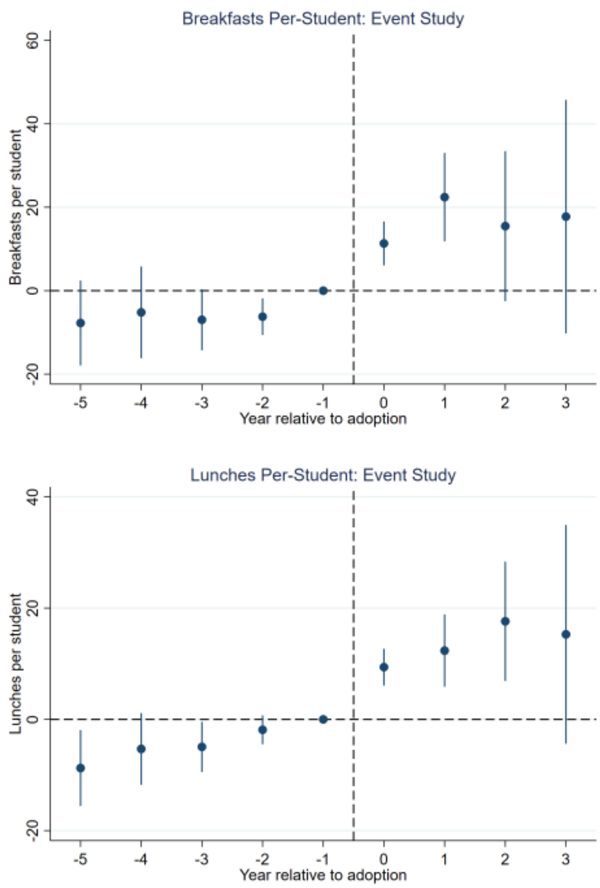
Note: Early-adopting districts are districts in which at least one school adopted CEP by 2015. Late-adopting districts are districts in which at least one school adopted CEP by 2017. Non-CEP districts are districts that did not adopt CEP. Student and district characteristics are estimated as the average from 2009-2011. Performance and meal data available through SEDA, nutritional assistance from CCD, per-capita transfers from REIS.

Figure 7: Fraction Students Attending CEP Schools in CEP-participating Districts



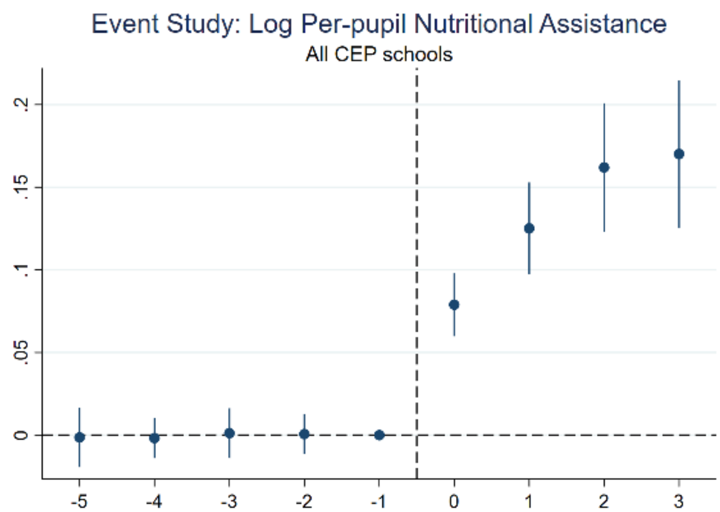
Note: Figure plots the share of students in each district-cohort dc attending a CEP school conditional on at least one school that covered cohort c participating in CEP for the 2012-2017 period. Data collected from state Departments of Education and FRAC/CBPP.

Figure 8: School Breakfasts and Lunches Served: Event Study



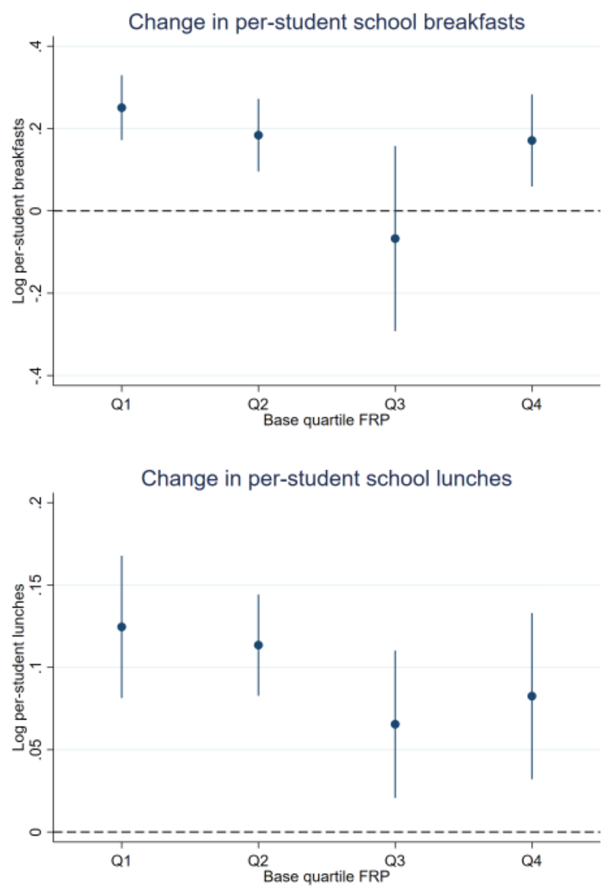
Notes: Event year 0 is first year of CEP implementation from an unbalanced panel (2012 for the first schools, 2013 for the second wave of schools, 2014 for the third wave of schools, etc.). Data collected from state Department of Educations for six of the eleven states that adopted CEP before 2015: Georgia, Illinois, Kentucky, New York, Maryland, and West Virginia. Data availability varies by state, but spans 2009-2016. All specifications include year and school fixed effects. Robust standard errors clustered by district.

Figure 9: Federal Nutritional Assistance Payments: Event Study



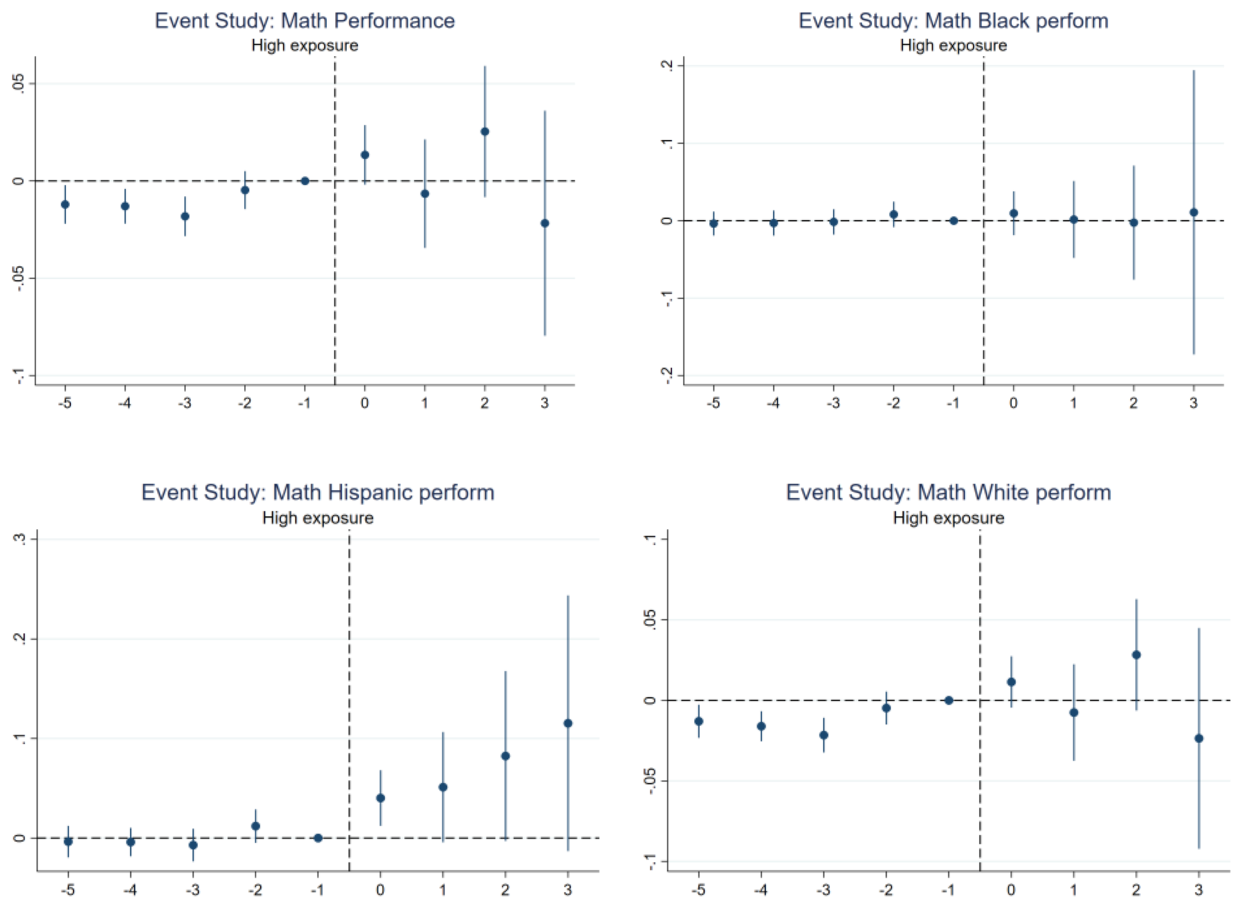
Notes: Notes: Event year 0 is first year of CEP implementation (2012 for the first schools, 2013 for the second wave of schools, 2014 for the third wave of schools, etc.). Specification includes year, district, and cohort fixed effects, as well as controls for annual racial/ethnic composition, percent of students attending a charter school, child poverty rates and county unemployment rates. Vertical bars are 95 percent confidence intervals with robust standard errors clustered by district. Black vertical dashed line separates the before- and after-CEP period. Sample limited to districts in which at least one school participated in CEP by 2017.

Figure 10: Change in School Meals, by Baseline FRP Quartile



Notes: Schools divided into quartiles based on pre-CEP FRP participation among schools adopting CEP by 2017. The first quartile has base participation up to 59 percent; the second quartile up to 71 percent; and the third quartile up to 84 percent.

Figure 11: Math Achievement and CEP



Notes: Event year 0 is first year of CEP implementation (2012-2015 depending on year of adoption and eligibility). Specification includes year, district, and cohort fixed effects, as well as controls for annual racial/ethnic composition, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Vertical bars are 95 percent confidence intervals with robust standard errors clustered by district. Black vertical dashed line separates the before- and after-CEP period. Sample limited to districts in which at least one school participated in CEP by 2017 and that had a baseline FRL share of 57.8 percent or lower.