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SCHOOL NUTRITION AND STUDENT DISCIPLINE: EFFECTS OF SCHOOLWIDE FREE MEALS

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ABSTRACT

Under the Community Eligibility Provision (CEP), schools serving sufficiently high-poverty populations may enroll their entire student bodies in free lunch and breakfast programs, extending free meals to some students who would not qualify individually and potentially decreasing the stigma associated with free meals. We examine whether CEP affects disciplinary outcomes, focusing on the use of suspensions. We use school discipline measures from the Civil Rights Data Collection and rely on the timing of pilot implementation of CEP across states to assess how disciplinary infractions evolve within a school as it adopts CEP. We find modest reductions in suspension rates among elementary and middle but not high school students. While we are unable to observe how the expansion of free school meals affects the dietary intake of students in our national sample, we do observe that for younger students, these reductions are concentrated in areas with higher levels of estimated child food insecurity. Our findings suggest that the impact of school-based child nutrition services extends beyond the academic gains identified in some of the existing literature.

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I. Introduction

The intuitive appeal of improving student outcomes through school nutrition programs is straightforward, and evidence to support the intuition is building. Beyond the benefits school nutrition programs may offer by helping to fulfill a basic physiological need (Maslow, 1943), a growing literature on the effects of offering schoolwide free meals suggests positive impacts on children's academic performance and healthy body weight. We add to this literature by asking whether the provision of schoolwide free meals through the Community Eligibility Provision (CEP) of the Healthy, Hunger-Free Kids Act of 2010 changed school suspension rates. We rely on the staggered rollout of the policy across a set of pilot states beginning in the 2012 school year through the national implementation in 2015; this allows us to compare schools serving similarly low-income student populations but with differential access to the program to estimate the causal impact of free school meals on discipline. We find modest reductions in the fraction of elementary and middle school students suspended. These reductions are particularly large for elementary school students in counties with estimated high levels of childhood food insecurity.

We investigate the potential for CEP to influence student behavior and disciplinary actions because of at least two plausible mechanisms, though we cannot distinguish between them: First, CEP may improve nutritional intake by increasing the share of students eating school meals. Second, CEP could potentially improve the social climate of the school by reducing the stigma associated with free meals, especially in schools where a significant fraction of students would not qualify individually for free

meals; we do not have data on school climate and are unable to investigate this mechanism here.

The potential for CEP to increase the share of students eating free meals comes both from students who would not be individually eligible, as well as students who would be but do not return the form for parental-reported family income needed to qualify. Furthermore, surveys and student interviews suggest that stigma deters free meal consumption conditional on eligibility (Poppendieck, 2010; Marples and Spillman, 1995); students who are eligible for free meals therefore may be more willing to consume those meals when offered schoolwide via CEP. Indeed, after New York City implemented free meals, lunch participation increased among all students, including those previously eligible (Leos-Urbel et al., 2013). Overall, CEP could plausibly improve nutritional intake for some students who experience economic hardship despite not participating in subsidized meals on an individual basis absent school-level participation in CEP.

The existing literature has documented a link between measures of hunger and food insecurity and worsened externalizing behaviors. While we observe disciplinary actions rather than behavior itself, these types of actions could result in suspensions. If this link contains any causal element, by expanding nutritional assistance we might expect that universal school meals could help reduce behaviors that lead to suspension.

Our goal is to gain insight into how the program influences the school environment and student attitudes and behaviors. Given the constraints of our research design, which exploits variation across demographically similar schools based on the timing of state-level participation in the CEP pilot program, we turn to the link between

provision of universal school meals and disciplinary outcomes, which are available nationally at the school level. We focus on the rate of students in a school who are suspended in a given year—a necessarily limited measure in that it reflects how school personnel respond to a student or incident, rather than a true measure of student behavior. To take an extreme example, if a district rules outlaws use of suspension, suspension rates will be zero but behavior will likely not be perfect. Our assumption is that within schools, changes in suspension rates are correlated with changes in student behavior, as perceived by staff; we do not assume that they are perfect correlates.

The bulk of the data we analyze comes from the biennial 2011-12 through 2015-6 academic years in order to take advantage of the CEP pilot timing and the timing of the nationwide suspension data collection. (Going forward we refer to academic years by the year of the spring semester.) This coincides with a period in which momentum was building around discipline policy reform; we assess the sensitivity of our results with this in mind. Our strategy and time period has the advantage that it precedes most independent policy action on school discipline reforms. But while we use a national sample in our estimation, a drawback of this strategy is that the identifying variation comes only from the pilot states. Further, CEP take-up increases with years of state-level program eligibility, so our estimates do not likely represent the program in steady state.

This paper proceeds as follows. Section II describes what we know about nutritional assistance and student outcomes, and Section III describes the CEP reform. Section IV provides background on school discipline. Section V describes our data, and Section VI presents our empirical framework and results. Section VII concludes.

II. Background on children's nutrition

In 2016, 8 percent of American households were food insecure, meaning that their food intake or eating patterns were reduced or disrupted due to a lack of household resources (Coleman-Jensen, et al., 2017). The existing literature links food insecurity to myriad negative outcomes for children, including physical and mental health, as well as social and interpersonal development (Howard, 2011; Alaimo, et al. 2001; Gundersen and Kreider, 2009; Kimbro and Denney, 2015). Poorer health and food insecurity in childhood is also linked to worse medium-and long-term outcomes, including poorer health in adolescents (Ryu and Bartfeld, 2012), and lower educational attainment and labor force attachment in adulthood (Case, et al., 2005). If the associations in these correlational studies reflect a causal impact of food insecurity, then nutritional assistance programs, such as school meals, could improve student outcomes and behaviors.

The federal government operates multiple programs that aim to reduce food insecurity and improve nutritional intake. Several of these programs, such as the Supplemental Nutritional Assistance Program (SNAP, formerly Food Stamps) and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) provide assistance targeted to low-income families.

All analyses of income assistance programs, including school meals, must overcome two challenges. First, particularly in the case of the school lunch program, these programs have remained relatively unchanged since national implementation, which limits the time and geographic variation available to study. Second, participation is non-random, since eligibility is limited to those with low family incomes (Bitler, 2015; Bitler and Currie 2005; Currie 2003). Given the negative relationship between family

income and health, students who consume school meals and other forms of nutritional assistance are likely to have poorer health outcomes without the program than ineligible individuals. Therefore, naïve comparisons of participating and non-participating children tend to understate any benefits of nutritional assistance.

Much of the existing literature examining the effect of nutritional assistance on health outcomes has focused on these family-based forms of assistance. This literature finds SNAP reduces food insecurity and hunger (Kreider, et al., 2012; Ratcliffe, McKernan, and Zhang, 2011; Schmidt, Shore-Sheppard, and Watson, 2013) and reduces out-of-pocket food expenditures (Hoynes and Schanzenbach, 2009). For school-aged children, greater access to SNAP improves test scores and lowers disciplinary action (Gassman-Pines and Bellows, 2015; Gennetian, et al. 2015). Greater access to nutritional assistance in childhood (both SNAP and WIC) provides long-term benefits, as measured by health outcomes, while also improving women's economic outcomes (Almond, Hoynes, and Schanzenbach, 2011; Hoynes, Schanzenbach, and Almond, 2016; Hoynes, Page, and Stevens, 2011; Rossin-Slater, 2013).

Family-based assistance does not eliminate hunger and food insecurity (USDA, 2013). To supplement these family assistance programs, lower-income children are also eligible for school-based feeding programs—the School Breakfast Program (SBP) and the National School Lunch Program (NSLP). The NSLP was implemented in 1946 in an effort to reduce malnutrition and childhood hunger. The SBP began as a pilot program in 1966, and was main permanent in 1975 (USDA, 2013). Historically, under both school meal programs, the amount a student pays depends on family income: children in families below 130 percent of the federal poverty level pay nothing for school meals, while

children in families up to 185 percent of the poverty level pay a heavily-subsidized rate. Higher-income children can purchase a school meal at the full "paid" rate.[1]

Over the past 50 years, the number of school meals served has increased, driven by the increase in the number of free meals. 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, compared to less than 40 percent and 1 percent in 1969. The growth in free- and reduced-price (FRP) meals is remarkable: About 15 percent of school lunches were FRP in 1969, and 74 percent in 2017. In other words, students may infer with reasonably high probability that peers consuming schools meals—even without observing anything about the transaction that would reveal the form of payment or benefit—are consuming a FRP meal and have low family incomes.[2]

Unlike programs such as SNAP and TANF that require meeting with a case manager and satisfying various asset tests and income verification, eligibility for subsidized meals is typically established by family self-reported income. Students receiving SNAP are "directly certified," or automatically enrolled in free meals, but other students can obtain free meals by having their parents complete a brief income questionnaire. Accordingly, not all students who are income-eligible participate in the school meals program. For example, Domina, et al. (2017) note that 13 percent of students who are not certified to receive free meals are income-eligible.

[1] In 2016, the average middle school price for a school breakfast was $1.47 and the average lunch cost was $2.54 (School Nutrition Association, 2016).

[2] In 2012, approximately 72 percent of school food authorities used a PIN system in at least one school, and 28 percent used debit-card technology. Other methods, such as student rosters or tickets/tokens were relatively rare (USDA 2014b). The 1970 amendments to the National School Lunch Act included prohibited "overt identification" of free meal receipt by "special tokens or tickets, announced or published lists of names, or other means" (P.L. 91-248, Section 6(d)). Nonetheless, particularly in schools allowing both cash and non-cash transactions, other students may ascertain payment status. See, for example, Kavanagh (2010) and Pogash (2008).

In general, the existing literature finds the school meals program increases food consumption and nutritional intake, with mixed effects on overall health. For example, Schanzenbach (2009) finds that kindergartners just below the income cutoff who consume free school lunches enter first grade with higher BMIs than students slightly above the income cutoff, suggesting worsened health outcomes. In contrast, other studies find that receiving free school meals is associated with a lower likelihood of poor health outcomes, including obesity and food insecurity (Gundersen, et al., 2012; Arteaga and Heflin 2014). 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, but did increase consumption of fat, protein, and several vitamins and minerals. Several papers compare students in SBP-participating schools to similar students in schools that did not offer breakfast. This literature tends to find SBP improves nutritional intake during breakfast, and increases reading test scores, but has mixed results on overall nutritional intake (Bhattacharya, et al., 2006; Frisvold, 2015).

Related work examines the effect of the school meals program on academic performance and educational attainment. Again, the literature finds mixed results of no effect to small benefits for most students, but some evidence of improvements for the most disadvantaged subpopulations. Using a sibling fixed-effect model, Dunifon and Kowaleski-Jones (2003) do not find that NSLP significantly affects positive behaviors or math or reading achievement. Hinrichs (2010) also finds no effect of school lunches on short-term performance, but does find school meals increase educational attainment in the longer-term. Examining universal breakfast programs, Imberman and Kugler (2014), Bartlett, et al. (2014), and Schanzenbach and Zaki (2014) find no effect of breakfast on

student test scores. Other studies, however, find school breakfasts improve reading and math performance (Frisvold, 2012; Dotter, 2013). Using discontinuities in state requirements to offer SBP, Frisvold (2012) finds school breakfasts improve math and reading performance.

CEP is not the first form of universal school meals; through Provisions 1-3 of the National School Lunch Act, some cities have opted to provide free meals (particularly breakfasts) to all students.[3] Schwartz and Rothbart (2017) find universal access to free meals in New York City increases school meal consumption, while improving academic performance, particularly among non-poor students who previously were unlikely to be eligible for free meals. Dotter (2013) finds universal breakfasts increase test scores by 0.15 and 0.10, respectively. If the same mechanisms that lead schoolwide meals to improve student performance also affect behavior—and subsequently discipline—the reform may have other benefits as well.

III. The Community Eligibility Provision

The reform we examine in this paper, the Community Eligibility Provision (CEP), eliminates the individual student-level link between family income and free meal eligibility by offering free meals to all students in qualifying schools that choose to participate. To be eligible for CEP, at least 40 percent of students in participating schools must have "categorically eligible" FRP status. Categorical eligibility means their families receive another form of assistance targeted to low-income families, such as SNAP,

[3] Compared to these pre-existing efforts, however, the Community Eligibility Provision provides potentially more generous federal reimbursement. For example, Provision 2 allows schools to provide universal free meals and obtain federal reimbursement based on a base year FRP share. In contrast, CEP reimburses districts at up to 1.6 times the low-income share.

TANF, or the Food Distribution Program on Indian Reservations.[4] States and districts must match their students to administrative records for these other assistance programs even if they do not wish to participate in the CEP. This match is also used to "directly certify," or automatically enroll, individual students to receive FRP meals in schools that do not participate in CEP.[5]

A subset of schools within a district with at least 40 percent students categorically eligible group-wide, or an entire district with at least 40 percent students categorically eligible, may also participate even if some single schools participating do not have 40 percent of students categorically eligible. Throughout the paper we model eligibility at the school level; the data support this simplifying assumption, as only two percent of schools below the 40 percent threshold in states eligible for CEP participation in 2012 were participating by 2017.[6]

Under CEP, the federal government reimburses schools at the free meals rate (in 2018, this was $3.23 for lunch and $1.75 for breakfast in the 48 contiguous states) for 1.6 times the categorically-eligible share, up to a maximum of 100 percent.[7] This 1.6 multiplier means that once a school has 62.5 percent of student categorically-eligible, the meals are fully subsidized by the federal government; if only 40 percent of students are

[4] In practice, most categorically eligible students qualify via participation in SNAP, the most far-reaching of these policies.

[5] In the 2015 school year, approximately 91 percent of SNAP recipients were automatically enrolled in the free meals programs through direct certification. This is a slightly improvement from 86 percent in 2012, the first year of CEP. Across states, the share of SNAP recipients automatically enrolled ranged from 100 percent in 23 states to 83 percent in South Dakota, California, and Arizona. See USDA (2016b) for more details.

[6] Through 2016, the majority of districts with some CEP participation had full district participation. Among districts with any participation, about 77 percent of schools participated on average.

[7] The 1.6 multiplier is based on historical data indicating that nationwide, $E\left(\frac{FRP\ eligible}{categorically\ eligible}\right) = 1.6$. For most schools, CEP provides more generous reimbursement than earlier universal meals provisions (Provisions 1-3).

categorically eligible, the federal subsidy covers 67 percent of costs, with the share growing up to 100 percent at 62.5 percent of students categorically eligible.[8]

Importantly for our strategy, CEP was incrementally rolled out across states beginning in the 2012 school year, shown in Table 1. Legislation determined the number of states adopting each year, and the Secretary of Agriculture selected the states eligible each year 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. Prior to 2015, CEP provides variation over time across and within states in potential access to free school meals that is unrelated to students' family resources. This policy-driven variation is particularly useful because among eligible schools, participation in CEP varies systematically with key observable characteristics.

Table 2 presents summary statistics for elementary and middle schools based on CEP eligibility and participation. Because the 2010 CRDC sample includes only about 20 percent of the schools in the sample in subsequent years, we report 2012 values as our baseline. Columns (1) and (4) summarize characteristics of schools that were never eligible for CEP because they had too few qualifying students; columns (2) and (5) describe schools that were eligible to participate by 2017 because they had enough qualifying students, but that chose not to participate; and columns (3) and (6) focus on

[8] A school where 40 percent of students are categorically eligible receives reimbursement for 64 percent of meals at the free lunch rate ($3.23 in 2018) and the remaining 36 percent of meals at the paid rate ($0.31).

schools that participated at any point by 2017. By 2017, about 32 percent of all public elementary schools and 28 percent of middle schools had a categorically-eligible share large enough to participate.

Among eligible schools, about half chose to participate in the program. Comparing column (2) with (3) for elementary schools, and column (5) with (6) for middle schools, reveals significant demographic differences between eligible schools that adopted CEP and those that did not. Nearly 70 percent of students in CEP schools qualified for free or reduced meals under the traditional program (that is, qualified based on parental-reported family income) and about 60 percent were automatically enrolled. CEP eligible, non-participating schools are slightly less disadvantaged on both measures and for both elementary and middle schools. Schools that take up CEP participation also are more likely to be in urban locations, and have significantly smaller white populations and a greater share of black and Hispanic students.

Important for this analysis, there are also marked differences in suspension activity across the school types. Suspensions are less common for elementary school students than for middle school students. For both grade levels, out-of-school suspension rates are significantly higher in CEP-participating schools than eligible, non-participating schools—about 45-50 percent higher for elementary schools and 15-25 percent higher for middle schools. These baseline differences suggest that simply comparing outcomes in participating schools to eligible schools that did not choose to participate—much less to the full universe of schools—would be problematic.

This is the first work we know of to examine the effect of universal provision on student discipline on a national basis, though an emerging literature looks at impacts on

health and educational outcomes within individual states. Comperatore and Fuller (2018) estimate a difference-in-differences model between eligible participating and non-participating schools using administrative student-level data for students in North Carolina schools eligible for CEP. This generates effects of treatment on the treated for students in schools that chose to participate. They find universal meals reduce absences, improve test scores, and do not affect disciplinary outcomes. Kho (2018) takes a similar strategy. He examines the effect of CEP on student suspensions, attendance, and expulsions in Tennessee, and finds CEP reduced suspensions about 10 percent. Finally, Davis and Musaddiq (2018) show positive effects of CEP on the share of students in a healthy weight range in Georgia. They estimate an intent-to-treat model showing the relationship between CEP eligibility within the state (that is, schools above and below the 40 percent categorically-eligible threshold) as well as instrumental variables models in which school-level eligibility instruments for school-level participation and the percentage of lunches provided at FRP. These studies focusing on single states are careful to examine pre-program trends to validate their stronger assumptions regarding selection into treatment. They also rely on richer, student-level data on a broader range of outcomes than we can address here. In contrast, while our approach allows us to leverage variation in CEP eligibility due to the timing of the national roll-out, it necessarily limits the set of outcomes we can examine.

Ruffini (2018) uses the same identification strategy as the present study to look to at district-level academic performance, using proficiency measures from the Stanford Educational Data Archive. Similar to the current work, she finds modest improvements in student outcomes for the full set of participating schools. She finds proficiency gains

concentrated in relatively low-poverty districts (which experienced the largest change in access under CEP). In results not shown, we attempt to examine heterogeneous impacts for high and low poverty school districts, but results are extremely imprecise.

IV. Student discipline background

In recent years, levels and race- and disability-based gaps in suspension rates have come under increased scrutiny. We briefly chronicle the policy timeline here to emphasize the potential for forces independent of school meals to influence recorded discipline rates, and how we take care to estimate a causal impact of school meals despite contemporaneous seemingly unrelated changes in policy and practice.

The Obama Administration made reducing disparities in suspension rates, both by race and ethnicity and by disability status, a central focus of its Department of Education's civil rights efforts. One prong in this attack was the collection and dissemination of the biennial data source we use in this study, the Civil Rights Data Collection (CRDC). In 2010, the CRDC survey was sent to a sample of 7,000 districts, covering approximately 72,000 schools (about 19,000 of which reported suspension information), but was made mandatory for all schools beginning in 2012. Starting in 2012, about 95,000-96,000 schools provide information on suspension activity each year, essentially all of reporting schools.

In November 2014, the Department of Education, together with the Department of Justice, issued a Dear Colleague letter –a guidance document—on racial disparities in school discipline.[9] The letter summarized disparities found in the CRDC, and explained

[9] Sec. Betsy DeVos has publicly stated that she is considering rescinding this guidance.

the relevant law, investigative process, and potential remedies districts might face.[10] Perhaps most notably, in an appendix, the guidance described "illustrative" examples of policies and practices that might prevent violations of civil rights law, encouraging districts to reduce use of suspension and other forms of exclusionary discipline, focusing instead on positive approaches.

While a number of districts changed their formal discipline policies after the release of the federal guidance, the guidance itself was the result of a building movement to reduce racial discrepancies in discipline, and high levels of suspensions generally. Some districts changed their policies before the federal guidance was issued; some did after. For example, Chicago ended a zero-tolerance policy that prohibited most out-of-school suspensions (including mandatory 10-day suspensions) in 2012, and Broward County (Florida) increased diversions as an alternative to suspensions in 2013. At the state level, the California state legislature banned willful defiance as a reason for expulsions or suspensions (for K-3 students), beginning in the 2015 school year. Our main specifications exclude schools in large school districts that issued changes in formal discipline policy at any point over the 2010-2016 period.[11] Much of the district-level activity in these years focused on elementary grades, so middle school results may be less sensitive to these sample restrictions than elementary ones.

[10] For example, the letter noted, "[O]ver 50% of students who were involved in school-related arrests or referred to law enforcement are Hispanic or African-American."

[11] We based our sample of districts for these searches on Appendix F of Eden (2017), which contains media citations. The excluded districts are Broward County, FL; Chicago, IL; Denver, CO; Los Angeles, CA; San Diego, CA; Mobile County AL; Oakland, CA; Prince George's County MD; Minneapolis, MN; St. Paul, MN.; Miami-Dade County, FL; Hillsborough County, FL; Palm Beach, FL; Portland, OR; Philadelphia, PA; Fairfax County, VA; and Madison, WI. While our main estimates retain California, both elementary and middle school findings are robust to excluding the entire state.

These policy changes may be linked to true or reported changes in the frequency of suspension; similarly, schools may change practices even if not in response to formal policy changes from their states or districts. Hashim, Strunk, and Dhaliwal (2018) review the literature on suspension bans and restorative justice and study their effects in Los Angeles.

V. Data

Exclusionary discipline

We use school-level discipline data for the 2010, 2012, 2014, and 2016 academic years from the Department of Education Civil Rights Data Collection.[12] These data include the number of students per year with one out-of-school suspension (OSS), more than one OSS, and any number of in-school suspensions (ISS) on a biennial basis. We use enrollment from the CRDC as the denominator to generate suspension rates per 100 students. Our main outcome of interest is a relatively extreme measure of discipline—the percent of students in a school with multiple out-of-school suspensions in the given year, though we do examine other measures as well. We drop schools with more than 25 multiple OSS per 100 students – the 99th percentile in our data. We also drop schools with fewer than 50 students, where our outcome, measured as a rate, fluctuates more dramatically from year to year. Our main analyses exclude high schools, preschool programs, ungraded schools, and juvenile detention facilities.

While discipline rates are comparable for middle and high school students, high school students are less likely to consume a school meal if income-eligible, suggesting

[12] While the 2012-2016 data include the universe of public schools, the 2010 sample included discipline information for a representative sample of approximately 19,000 schools. We use the 2012 report for baseline statistics. We include the 2010 data for those schools reporting in our analyses. Results are robust to dropping 2010.

that the *availability* of free meals through CEP has a muted effect on actual behavior than for younger students (Danielson, 2015; MassBudget, 2012; National Research Council, 2010). Appendix Table 1 shows our main results for high school students. None of the difference-in-differences specifications show a significant change in suspension rates.

We merge the CRDC data on school-level eligibility for CEP (discussed in the next section) and school level characteristics from the Common Core of Data (CCD). Of the 126,470 schools identified in the CCD in 2012, 16,947 are middle schools and 54,377 are elementary schools.[13] Of these schools, we match 15,889 middle schools and 51,136 elementary schools to the CEP data (match rate of 94 percent), and 15,150 middle and 49,074 elementary schools to the CRDC data, for an overall match rate of about 90 percent (Appendix Table 1). High schools have comparable match rates (90 percent). The match rate is similar in 2014 and 2016, but lower in 2010, as the CRDC was only administered to a sample of schools that year.

Over our period of analysis, suspension rates for middle schools were trending down. In our middle school sample, approximately 5.8 students out of every 100 received more than one out-of-school suspension in 2012. By 2014, this figure had fallen to 5.2, and by 2016 it had fallen further to 4.9. The decrease in suspension rates among elementary students is less consistent: On average, 1.9 elementary students were suspended in 2012. This rate fell to 1.7 in 2014, but slightly increased to 1.8 students per 100 in 2016. Our difference-in-differences strategy will control for these aggregate

[13] Following the Department of Education's classification scheme, we define elementary schools as those with a low grade between pre-school and third grade and a high grade up through eighth grade, middle schools as those with a low grade between fourth and seventh grade and a high grade up to ninth grade, and high schools as schools with a low grade of seventh through twelfth that also serve grade 12.

differences over time, but assumes that the changes over time would be similar across schools that became eligible for CEP earlier and those that became eligible later.[14]

In all years, there is substantial variation in suspension rates, particularly in middle schools. Even after excluding outlier values, our data exhibit a right-skewed distribution: While the typical school had relatively few suspensions, 2012 suspension rates exceeded 13.4 in 10 percent of middle schools (Figure 2). Table 2 shows the 25th, 50th, 75th, 90th and 95th percentiles of the multiple suspension distribution in 2012 by CEP eligibility and participation for middle and elementary schools in our main sample. At all points, CEP-eligible, and especially participating, schools had higher baseline suspension rates than ineligible schools.

The last two rows show level and percent changes between 2012 and 2016, demonstrating a reduction in suspension rates over our time period. The median change in middle schools was a reduction of about 0.37 percentage points (the median percent reduction was 25 percent). In contrast, half of elementary schools experienced an increase in suspensions and half experienced a decrease. Excluding schools without out-of-school suspensions in either year, the median change was a 34 percent reduction.

In many schools, there are also large year-to-year fluctuations in suspension rates. For our middle school sample, suspension rates changed by less than 1 percentage point in about 30 percent of our main sample, but changed by at least 5 percentage points in about 22 percent of middle schools. Consistent with the aggregate trends towards fewer suspensions, most schools with large changes had high baseline out-of-school

[14] Unfortunately, we cannot fully explore this assumption with pre-period data because the CRDC sample for 2010 was about 20 percent the size of the later collections.

suspensions: More than half had a suspension rate greater than 10.1 students in 2012 (the 81st percentile in our main middle school sample).

CEP participation and categorical eligibility

We collect information on school participation in CEP from state Departments of Education for years 2012 through 2014 and from the Food Research and Action Center (FRAC) for years 2015 through 2017. These data also provide the share of students who are categorically (automatically due to other program participation, mainly SNAP, as opposed to parental report of family income) eligible for FRP meals in an adopting group of schools.[15] We use the categorical eligibility information to identify schools that are eligible to participate in CEP, but that chose not to participate.[16] As previously discussed, Table 2 shows significant differences between schools that chose to participate in CEP and those that did not. In general, among eligible schools, schools that participate in CEP are significantly more disadvantaged on most observable characteristics. These differences between participating and non-participating eligible schools inform our preferred sample.

Economic and demographic information

[15] Information on categorical eligibility is only available for eligible schools; schools with a categorically eligible rate below 40 percent were not required to report. In addition, there are several reporting limitations with this variable, particularly during the pilot years: states were permitted to report a categorical eligibility rate of 62.5 percent if their actual rate exceeded 62.5 percent, as a rate of at least 62.5 percent guaranteed full federal reimbursement. Accordingly, while the reported categorical eligibility rate is informative about whether a school qualified to participate in CEP or receive the full federal reimbursement, the right- and left-censoring renders other empirical approaches relying on a smooth distribution, such as regression discontinuity, problematic.
[16] USDA and some state departments of education report categorical eligibility rates as the Identified Student Percentage or share of students directly certified. The three are equivalent.

The Common Core of Data provides demographic information on the racial/ethnic composition of schools, school enrollment, and whether a school is located in a rural or urban area. Information on county unemployment, which is correlated with poverty and the share of students eligible for free meals, comes from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS). Finally, estimated child food insecurity is the expected share of children experiencing food insecurity based on the relationship between county economic characteristics and food insecurity in the December Current Population Survey from 2000 to 2015.[17] We define areas of estimated high child food insecurity as those above 22 percent, the county median for all CEP-eligible schools.

VI. Methods and results

Our preferred estimation strategy compares changes in suspension rates within the group of ever-adopting schools as they become eligible. Schools where at least 40 percent of students were categorically eligible for free meals (recall the categorical eligibility rate is essentially the same as the share of enrolled students participating in SNAP) were allowed, but not required, to participate in CEP. We leverage the four-year implementation period, described in Table 1 to compare schools that became eligible for CEP earlier in our sample period, with those that became eligible later in the period. Importantly, the year in which a school became eligible depended on the state in which it was located. For example, a Michigan school could adopt CEP as early as 2012 and be "treated" in 2012 through 2016, while a California school could only adopt CEP as early as 2015, and thus be "treated" in the final year of our sample period. This cross-state variation in CEP eligibility allows us to estimate the effect of universal meals without

[17] For methodology details, see Feeding America's (2018) Mind the Meal Gap project methodology at: http://www.feedingamerica.org/research/map-the-meal-gap/how-we-got-the-map-data.html

confounding eligibility with the decision to participate. Our main analyses exclude schools that do not participate in CEP by 2017. These "never-adopters" include both low-poverty schools, as well as eligible schools that opt to not participate in CEP for unobserved reasons. As described in Table 2, both these groups tend to have both lower poverty and lower suspension rates.

Table 3 panel (a) presents the main results for elementary school students; panel (b) shows analogous results for middle school students. Our preferred specifications limit the sample to schools that ever participate in CEP. For comparative purposes, we begin in column (1) by estimating the raw correlation between CEP participation and suspension rates for school *s* after introducing CEP in year *t*, as in equation [1]:

$$suspendrate_{st} = \alpha + \beta CEP_{st} + \varepsilon_{st} \qquad [1]$$

Column (1) shows CEP participation is associated with higher rates of multiple out-of-school suspensions: This is more than double the baseline rate for elementary students and a 75 percent increase for middle school students (approximately 2.6 per 100 middle school students). This approach does not control for CEP eligibility. As shown in Figure 1, the rate of multiple suspensions is increasing approximately linearly in baseline free meal eligibility. Elementary schools show a relatively flat relationship between poverty and suspensions for the least poor schools. About one student is suspended for multiple days for schools with less than 50 percent of students FRL-eligible. Among high-poverty schools, however, suspension rates increase with poverty: Approximately 3.5 percent of students are suspended for multiple days in the highest-poverty schools. Middle schools show a more pronounced linear pattern. Whereas average suspension rates were approximately one per hundred students in the least poor middle schools, they

exceed six in schools with more than 80 percent of students FRP eligible. Accordingly, as CEP schools tend to have higher poverty rates than non-CEP schools, naïve OLS comparisons will be biased towards finding higher suspension rates associated with CEP. Indeed, when we account for time-invariant school characteristics and secular changes over time by including school and year fixed effects in Column (2), we find a statistically insignificant reduction for elementary students and a small and marginally significant 8 percent drop in suspensions for middle school students.

Because the participation in CEP conditional on eligibility is not random, as Table 2 showed, we proceed with a difference-in-differences framework to estimate the impact of school-level eligibility, as jointly determined by the share of students categorically eligible in the school and whether its state is eligible for CEP participation in that year. Specifically, we estimate equation [2] as:

$$suspendrate_{st} = \beta CEP_s * stateelig_{st} + \delta_s + \delta_t + \varepsilon_{st} \qquad [2]$$

for school *s* at time *t*. CEP_s is a non-time varying variable that equals one if a school participates in CEP by 2017. $stateelig_{st}$ equals one each year a school's state is eligible to participate in CEP in year *t*. Our measure of eligibility, $CEP_s * stateelig_{st}$, is the interaction of these terms, which equals one if a school that participates in CEP at any point through 2017 is eligible (based on its state) in year *t*.

We include school fixed effects, δ_s, to account for time-invariant school factors, such as neighborhood and resources. We include year fixed effects, δ_t, to account for secular trends in suspensions, and secular changes due to national advocacy, media, and policy affecting all schools at the same time.

Column (3) presents the main difference-in-differences specification, estimated on the sample of all CEP-eligible schools. This approach is most similar to that employed in papers that examine changes within a state stemming from CEP (Comperatore and Fuller, 2018; Kho, 2018), albeit with less refined pre-treatment controls than those papers are able to leverage. Here we find no significant change in suspension rates following CEP for either elementary or middle schools.

However, this approach includes a number of schools that not only never take up the program, but that would not be expected to do so based on systematic differences in take up documented in Table 2—or perhaps based on institutional or political differences that we are unable to observe. We therefore next limit the sample to schools that ultimately did participate in CEP at any point through 2017 (column 4). Here we find a marginally significant reduction in suspension rates for elementary students and no change for middle school students following CEP adoption.

As discussed in Section IV, a number of districts were changing their formal student discipline policies during our period of study. It is likely that many of these changes grew out of years of advocacy and political efforts that predated district participation in CEP, even if the policies were formally enacted during the sample period. Excluding schools in the 100 largest districts that changed formal discipline policies at any point during our period of study increases precision and implies suspensions fell by 0.32 elementary students per every 100 – a 15 percent reduction from the baseline mean. Results for middle schools show a marginally significant suspension rate reduction of 0.48 percentage points, or about 6 percent (column 5). Under this preferred specification, we can rule out decreases in suspension rates greater than 1.0 percentage points or

increases greater than 0.06 percentage points for middle school students, and decreases smaller than 0.12 students for elementary schools.[18] Column (6) shows that we obtain results that are somewhat larger, but not statistically different, when weighting by student enrollment. In contrast, we do not find a significant change for high school students in any specification once we account for school and year fixed effects (Appendix Table 1).

The difference-in-differences framework described above is our preferred empirical approach due to data limitations and sample considerations. Alternative approaches could leverage information on CEP eligibility and participation using the categorically-eligible share – the fraction of students automatically enrolled in the free meals program. However, in practice, approaches using more than the most basic categorical eligibility information have several limitations. In particular, during the pilot period, schools ineligible for CEP were not required to report their exact categorically-eligible share (other than that is was below 40 percent), nor were those eligible for full reimbursement (other than it was at least 62.5 percent). Appendix Figure 1 displays the observed distribution. This figure shows clear bunching around the 40 and 62.5 percent thresholds that limits our choice of methodology, and in particular excludes the possibility of a regression-discontinuity approach.

While one could account for selection into CEP by instrumenting participation with CEP eligibility, the highly-skewed suspension distribution in Table 2 and Figure 2 raises concerns about this approach. In particular, even in high-poverty areas, most schools have relatively few suspensions, but a small share of schools have very high

[18] It is possible (if improbable) that some policies were directly influenced by CEP eligibility. In this case, the specification in column 4 would be more appropriate, as the reforms to discipline policies would be the mechanism for changing the discipline rates as a result of CEP participation.

suspension rates. Young (2017) shows an instrumental variables approach will be sensitive to outlier observations; this is true in our data, as demonstrated in Appendix Tables 1 and 2. The first table shows first-stage results for several alternative instruments based on the 40 percent eligibility and 62.5 percent full-funding categorically-eligible thresholds. While each of the specifications yields a strong first stage, Appendix Table 2 demonstrates IV results are sensitive to the instrument choice: they vary as we select the 40 percent eligibility cutpoint (column 1), or the 62.5 percent cutpoint for full reimbursement (column 2), include the fraction of students in the district attending a CEP eligible school (column 3), or drop schools in Florida (column 4).

Our difference-in-difference results define treatment as whether a school was eligible to participate in year *t*. For example, a school in Michigan that became eligible in 2012, but chose not to participate until 2014 would be considered "treated" in 2012. This approach has the advantage of only leveraging treatment status based on immutable factors (the state the school is located in) and not relying on selection into treatment based on factors within the control of the school. Column 5 of Appendix Table 2 instruments the *actual* year of participation with our difference-in-differences treatment variable of state eligibility. Despite the relatively small sample size, this approach yields a strong first-stage. Column 5 shows suspensions rates fell by approximately 1.5 percentage points when a school was prompted to participate in CEP when its state became eligible (the local average treatment effect in this framework is weighted towards schools that participated immediately upon eligibility).

In robustness checks not shown, we have explored whether our results are driven by a single state, or by schools with very high suspension rates. We obtain similar results

dropping one pilot state at a time, and when we restrict our sample to the bottom 95 percent (rather than the bottom 99 percent) of the suspension distribution for both elementary and middle schools.

Other outcomes

Multiple out-of-school suspensions are a fairly severe and rare disciplinary outcome in most schools. Table 4 examines whether CEP led to any changes in less severe, but more commonly-used, forms of discipline. Column (3) shows a marginally significant reduction in the share of elementary students receiving any out-of-school suspensions, defined as either multiple or a single suspension. For middle school students, the decrease in multiple suspensions is offset by an (insignificant) increase in single suspensions so there is no overall change in the fraction of students with any suspension (column 5). In-school suspensions do not significantly change for elementary students, and slightly decrease for middle school students (columns 7 and 8).

Heterogeneity by predicted need

CEP adoption did not lead to the same increase in access to free meals for all participating schools. Students with family income below 130 percent of poverty are eligible to receive free meals under either the traditional program or CEP. Students from more moderate-income families become eligible under community eligibility. We do not have data on how many students who are formally eligible, via any mechanism, or actually eat the meals provided, but evidence from New York City suggests that meal participation increases more for non-poor students than for poor students when schools adopt universal free meals (Schwartz and Rothbart, 2018). We therefore restrict our attention to how CEP eligibility affects access to free meals, rather than actual

consumption of the meals. As with all our heterogeneity analyses, we cannot distinguish between the effect of CEP participation being stronger in some contexts versus differential likelihood of school level participation conditional on eligibility. We present these analyses in the spirit of generating hypotheses on either of these two dimensions. We expect any effect to be concentrated among students and schools who were likely to have lower free meal participation rates under the traditional program.

To explore whether any effect is concentrated in schools with the highest unmet need for nutritional assistance, we divide the sample at the median of county estimated child food insecurity in CEP-eligible schools—22 percent – in Table 6. For elementary students, the aggregate effect appears largely driven by schools in these food insecure areas. Suspension rates fell by 0.16 students per hundred with CEP eligibility in the less-food-insecure places, and by 0.54 students per hundred in the more food-insecure ones: the difference between the two is statistically significant. For middle school students, the magnitude of the point estimate for the difference is similar, but imprecise. In additional results, we do not find significant differences between high and low poverty schools, schools in urban and rural areas, or by school racial/ethnic composition. In interpreting these results, recall that CEP-eligible schools are a disadvantaged subset of all schools, so all differences between high- and low-poverty schools, or other measures of disadvantage, in this subsample are less marked than in the full national distribution.

Effects by student race and ethnicity

Table 5 explores the effect of CEP on suspension rates by race and ethnicity. As seen in Table 2, in CEP and non-CEP schools alike, black students are suspended more than twice as often as white students on average. Although higher suspension rates imply

there is greater scope for reducing black suspension rates, as a group, these students are also more likely to qualify for free meals absent CEP. Consistent with expanded eligibility reducing hunger for students who become eligible under universal provision, Table 5 shows CEP reduced suspension rates among middle school white students by about 0.47 percentage points (relative to a baseline mean of 4.02) and black students by 0.55 percentage points (relative to a baseline mean of 8.2), although neither estimate is significant. Although not statistically different from our main finding, column (5) shows larger point estimates for reductions among limited English proficiency students. For elementary students, columns (3) and (5) again show a similar percentage point reduction for white and black students, but a smaller percentage reduction for black students. Appendix Table 4 shows reductions for Hispanic and limited English proficiency students are not significantly different from the main results.

Relationship to other estimates in the literature

While our approach leverages cross-state variation in the timing of CEP, the other papers we know of studying the relationship between CEP and disciplinary outcomes do so using variation in program *participation*, conditional on eligibility, within single states. Kho (2018) studies Tennessee using a comparative interrupted time-series approach. He estimates a 1.2 percentage point reduction in the rate of students ever suspended or expelled in a given year; from a base level of 12 percent of students in all grades receiving these strong forms of discipline, this is a 10 percent reduction. While we cannot examine the identical metric in the CRDC data, we find a six percent reduction in the rate of students with multiple suspensions in middle school and a 15 percent reduction

for elementary school.[19] When we look to the rate of students with *any* number of suspensions, the coefficients are quite similar but less precisely estimated (Table 4).

Kho's largest reductions are in high schools and in the wealthiest schools, while under our preferred sample of participating schools in columns (1) through (3) of Appendix Table 5, we find a slight, marginally significant *increase* in the fraction of high school students with any suspension. In additional results, we find no differences between high and low-poverty schools. The difference in the high school effects may be due to Kho's discipline measure including expulsions, which, although rare, are more prevalent at the high school level than elementary or middle, and in Tennessee than nationally.[20]

Comperatore and Fuller (2018) study North Carolina using a difference-in-differences framework, comparing outcomes for individual students in CEP-participating schools with students in schools in the same state that were eligible but chose not to participate. They find positive effects on some student achievement measures, but no effects on the share of students suspended (any number of times) in a given year, for elementary, middle, or high school, in the aggregate or for subsets of high poverty or urban schools. We obtain similar results when we broaden the sample to include all eligible schools in columns (4) through (6) of Appendix Table 5.

VII. Conclusion

A consensus is emerging from the nascent literature that, through CEP, universal free school meals have positive effects on student achievement. This paper is the first to

[19] At the school-year level, the CRDC separately reports the counts of students: (1) with one out-of-school suspension; (2) with more than one out-of-school suspension; and (3) expelled. It is not possible to identify overlap between individual students suspended and expelled.

[20] In the CRDC data, the average high school expulsion rate was 0.81 in 2012, compared to 0.61 for middle school and 0.35 for elementary students. Expulsion rates in Tennessee high schools are more than double the national average.

look beyond a single state to show the benefits of CEP extend to modest reductions in suspension rates, as found by Kho (2018) in Tennessee and in contrast to the North Carolina experience as analyzed by Comperatore and Fuller (2018). The introduction of CEP occurred in a dynamic policy environment with substantial changes in both formal discipline policy and in disciplinary outcomes in schools without official discipline policy changes. Given this context, it is perhaps not surprising that these reductions, in a relatively severe form of discipline, were generally modest in the aggregate. We find significantly larger reductions for elementary students in areas with high estimated child food insecurity, on the order of 25 percent. These areas differ along multiple dimensions—indeed these dimensions are used to estimate the child food insecurity—so we do not take a strong causal stance on interpreting the pattern beyond noting it is consistent with universal meals having a greater effect in places with greater needs.

Overall, the body of early work on CEP suggests it has the potential to improve a range of student outcomes, extending beyond test scores. It also reveals that not all eligible schools are equally likely to participate in the program, at least through 2016-17, which has important implications for both research and policy. Researchers must be attuned to the school-level selection into CEP. The findings suggest that expanding access to the program would reduce the incidence of out-of-school suspensions.

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Table 1: CEP eligibility timing

School year	States eligible for CEP
2012	IL, KY, MI
2013	DC, IL, KY, MI, NY, OH, WV
2014	DC, FL, GA, IL, KY, MA, MD, MI, NY, OH, WV
2015	All

Notes: Table lists the school year (by spring semester) in which schools in each state became eligible to participate in CEP. In each year, a school's identified student percentage (ISP) must be at least 40% in order to participate.
Source: USDA, 2016a.

Table 2: Summary statistics by CEP eligibility and participation

	(1)	(2)	(3)	(4)	(5)	(6)
	Elementary			Middle		
	CEP ineligible	CEP eligible, not participating	CEP participating	CEP ineligible	CEP eligible, not participating	CEP participating
2012 poverty and school meal receipt						
% free and reduced price	29.90	63.11	69.51***	35.25	65.30	69.03***
	(24.47)	(28.38)	(29.09)	(21.57)	(21.91)	(24.55)
% free meal categorically eligible	34.72	52.20	62.23***	34.53	48.68	58.49***
	(2.869)	(10.89)	(10.70)	(2.853)	(8.444)	(9.335)
2012 average discipline actions (per 100 students)						
In-school suspensions	1.307	2.691	2.517***	6.844	13.22	12.58*
	(2.759)	(4.412)	-4.611	(7.718)	(11.12)	(11.19)
Any out-of-school suspensions	1.414	3.184	4.697***	5.054	10.81	13.12***
	(2.134)	(3.651)	(5.344)	(5.093)	(8.000)	(9.798)
Multiple out-of-school suspensions	0.429	1.219	1.856***	1.862	5.083	5.843***
	(1.019)	(2.120)	(2.993)	(2.764)	(5.863)	(5.843)
Single out-of-school suspensions	0.985	1.965	2.841***	3.192	5.788	7.280***
	(1.408)	(2.104)	(2.995)	(3.119)	(4.087)	(5.486)
2012 average multiple suspension rates by race						
White	0.420	1.147	1.445***	2.036	3.747	4.084
	(3.635)	(3.559)	(4.713)	(24.87)	(5.917)	(7.762)
Black	1.149	2.658	3.084***	4.586	8.680	8.757
	(5.689)	(7.271)	(6.292)	(10.58)	(11.62)	(12.18)
2012 multiple out-of-school suspension (OSS) distribution						
Multiple OSS 25th percentile	0.00	1.08	1.22	0.00	1.08	1.22
Multiple OSS 50th percentile	0.93	3.25	3.89	0.93	3.25	3.89
Multiple OSS 75th percentile	2.35	6.97	8.06	2.35	6.97	8.06
Multiple OSS 90th percentile	4.52	11.29	13.46	4.52	11.29	13.46
Δ multiple OSS (2012-6 median)	0.00	0.00	0.00	-0.04	-0.61	-0.37
% Δ multiple OSS (2012-6 median)	-0.73	-0.48	-0.34	-0.45	-0.35	-0.25
2012 school demographics						
Percent black	6.285	16.21	30.71***	7.724	19.14	29.35***
	(12.11)	(22.16)	(33.50)	(12.70)	(21.79)	(31.61)
Percent white	55.62	39.01	31.02***	68.45	42.22	35.83***
	(34.62)	(32.45)	(32.64)	(26.93)	(30.36)	(33.03)
Urban	0.158	0.225	0.451***	0.125	0.224	0.365***
	(0.365)	(0.418)	(0.498)	(0.331)	(0.417)	(0.482)
Rural	0.280	0.219	0.192***	0.293	0.225	0.224
	(0.449)	(0.414)	(0.394)	(0.455)	(0.418)	(0.417)
Enrollment	463.2	489.7	468.4***	618.0	598.0	563.1***

	(221.5)	(219.2)	(210.9)	(337.4)	(307.9)	(296.4)
County unemployment	7.616	8.499	8.866***	7.651	8.528	8.959***
	(2.211)	(2.541)	(2.254)	(2.127)	(2.604)	(2.401)
Percent high child food insecurity	0.138	0.381	0.446***	0.179	0.470	0.522***
	(0.344)	(0.486)	(0.497)	(0.384)	(0.499)	(0.500)
Observations	27176	9405	10840	10097	1953	2022

Notes: Sample limited to middle schools, as defined by Department of Education. Unless otherwise noted, mean coefficients, standard deviations in parentheses. Baseline discipline rates defined as 2010 values for schools surveyed in 2010; 2012 otherwise. Baseline demographic and economic characteristics defined as 2012 values. Column (1) presents statistics for schools with an ISP less than 40%; column (2) presents statistics for schools with an ISP of at least 40% that did not participate in CEP as of the 2017 school year and; column (3) presents statistics for schools with an ISP of at least 40% that participated in CEPby the 2017 school year. *** p<0.01, ** p<0.05, * p<0.1 indicates whether CEP participating schools are statistically different from CEP eligible schools.
Sources: Discipline information from the US Department of Education Civil Rights Data Collection (CRDC); school demographics from the Common Core of Data (CCD) for school year 2012; share categorically eligible for FRP meals from state departments of education, USDA, and CBPP; percent of schools experiencing high child food insecurity from Feeding America.

Table 3: Effect of CEP on multiple out-of-school suspensions

VARIABLES	(1) Multiple OSS	(2) Multiple OSS	(3) Multiple OSS	(4) Multiple OSS	(5) Multiple OSS	(6) Multiple OSS	(7) Log (Multiple OSS)
				Elementary			
CEP elig	1.128***	-0.0905	-0.0633	-0.221*	-0.321***	-0.327***	-0.0685**
	[0.0739]	[0.0642]	[0.0716]	[0.120]	[0.0894]	[0.101]	[0.0292]
Observations	158,486	158,486	67,046	37,247	34,437	34,259	34,259
R-squared	0.040	0.003	0.004	0.003	0.008	0.009	0.009
FE	None	School and yr	School and yr	School and yr	School and yr	School and yr	School and yr
Sample	All	All	CEP eligible	CEP partic	CEP partic, no change	CEP partic, no change	CEP partic, no change
Base DV mean	0.984	0.984	1.708	2.068	2.172	2.154	2.154
# of schools	52,247	52,247	21,915	12,344	11,430	11,254	11,254
Weight						Enrollment	
				Middle			
CEP elig	2.639***	-0.263*	0.0673	-0.434	-0.477*	-0.679**	-0.0945
	[0.160]	[0.157]	[0.200]	[0.268]	[0.274]	[0.331]	[0.0633]
Observations	47,649	47,649	13,833	7,399	7,138	7,093	7,093
R-squared	0.039	0.026	0.030	0.016	0.016	0.024	0.015
FE	None	School and yr	School and yr	School and yr	School and yr	School and yr	School and yr
Sample	All	All	CEP eligible	CEP partic	CEP partic, no change	CEP partic, no change	CEP partic, no change
Base DV mean	3.474	3.474	6.963	7.508	7.649	7.667	7.667
# of schools	16,316	16,316	4,957	2,715	2,625	2,580	2,580
Weight						Enrollment	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district in brackets. Sample limited to elementary (top panel) and middle (bottom panel) schools with at least 50 students and discipline rates below the 99th percentile for all schools. "CEP" equals one if a school participates in CEP in year t."State eligible" is an indicator equal to one if a school is in a state eligible to participate in year t. Baseline mean calculated as the mean of the dependent variable for 2010 for schools in the 2010 sample; 2012 otherwise. Columns (5-7) exclude schools in Broward County, FL; Chicago, IL; Denver, CO; Los Angeles, CA; San Diego, CA; Mobile County AL; Oakland, CA; Prince George's County MD; Minneapolis, MN; St. Paul, MN.; Miami-Dade County, FL; Hillsborough County, FL; Palm Beach, FL; Portland, OR; Philadelphia, PA; Fairfax County, VA; and Madison, WI -- districts that changed formal discipline policy over our study period. Log multiple OSS calculated as log(multoss + 1) in order to include schools reporting no instances of out-of-school suspensions.
Sources: Discipline information from the US Department of Education Civil Rights Data Collection (CRDC); CEP participation information from state departments of education, USDA, and CBPP.

Table 4: Effect of CEP on other discipline actions

VARIABLES	(1) Multiple OSS	(2) Multiple OSS	(3) Any OSS	(4) Any OSS	(5) Single OSS	(6) Single OSS	(7) ISS	(8) ISS
CEP elig	-0.321***	-0.477*	-0.252*	-0.366	0.0691	0.111	-0.157	-0.839*
	[0.0894]	[0.274]	[0.150]	[0.402]	[0.0848]	[0.195]	[0.131]	[0.499]
Observations	34,437	7,138	34,437	7,138	34,437	7,138	34,437	7,138
R-squared	0.008	0.016	0.004	0.014	0.001	0.007	0.002	0.015
FE	School and yr	School and yr	School and yr	School and yr	School and yr	School and yr	School and yr	School and yr
School	Elementary	Middle	Elementary	Middle	Elementary	Middle	Elementary	Middle
Sample	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no change
Base DV mean	2.172	7.649	5.176	15.46	3.004	7.880	2.759	14.67
Number of schools	11,430	2,625	11,430	2,625	11,430	2,625	11,430	2,625

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district in brackets. Sample limited to middle schools with at least 50 students and discipline rates below the 99th percentile for all schools. "CEP" equals one if a school participates in CEP in year t."State eligible" is an indicator equal to one if a school is in a state eligible to participate in year t. Baseline mean calculated as the mean of the dependent variable for 2010 for schools in the 2010 sample; 2012 otherwise.Schools in Broward County, FL; Chicago, IL; Denver, CO; Los Angeles, CA; San Diego, CA; Mobile County AL; Oakland, CA; Prince George's County MD; Minneapolis, MN; St. Paul, MN.; Miami-Dade County, FL; Hillsborough County, FL; Palm Beach, FL; Portland, OR; Philadelphia, PA; Fairfax County, VA; and Madison, WI -- districts that changed formal discipline policy over our study period -- are excluded.
Sources: Discipline information from the US Department of Education Civil Rights Data Collection (CRDC); CEP participation information from state departments of education, USDA, and CBPP.

Table 5: Effect of CEP on race-specific suspension rates

VARIABLES	(1) All	(2) All	(3) White	(4) White	(5) Black	(6) Black
CEP elig	-0.321***	-0.477*	-0.408***	-0.473	-0.409***	-0.553
	[0.0894]	[0.274]	[0.103]	[0.310]	[0.120]	[0.477]
Observations	34,437	7,138	33,005	6,877	31,495	6,542
R-squared	0.008	0.016	0.003	0.003	0.006	0.004
# of schools	11,430	2,625	11,207	2,553	10,915	2,489
FE	School and yr	School and yr	School and yr	School and yr	School and yr	School and yr
School	Elementary	Middle	Elementary	Middle	Elementary	Middle
Sample	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no change
Base DV mean	2.172	7.649	1.441	4.018	3.169	8.248

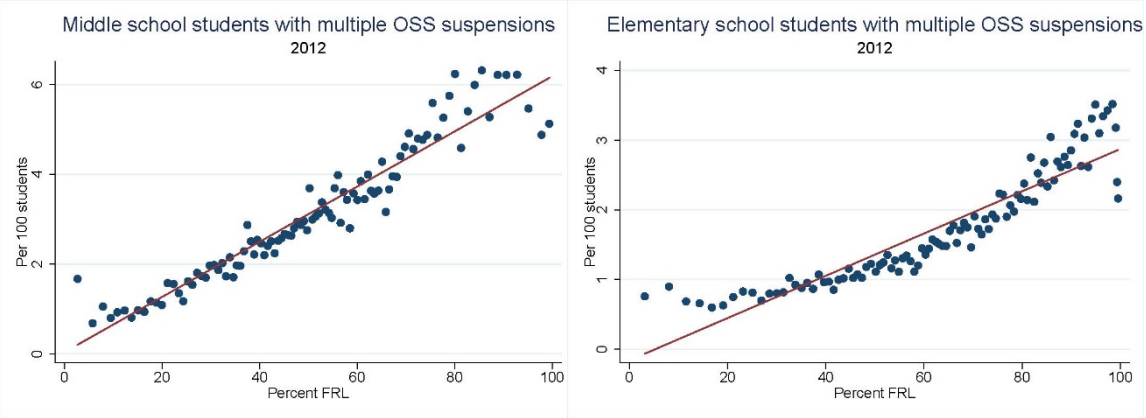
Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district in brackets. Sample limited to middle and elementary schools with at least 50 students and discipline rates below the 99th percentile for all schools. "CEP" equals one if a school participates in CEP in year t."State eligible" is an indicator equal to one if a school is in a state eligible to participate in year t. Baseline mean calculated as the mean of the dependent variable for 2010 for schools in the 2010 sample; 2012 otherwise. Schools in Broward County, FL; Chicago, IL; Denver, CO; Los Angeles, CA; San Diego, CA; Mobile County AL; Oakland, CA; Prince George's County MD; Minneapolis, MN; St. Paul, MN.; Miami-Dade County, FL; Hillsborough County, FL; Palm Beach, FL; Portland, OR; Philadelphia, PA; Fairfax County, VA; and Madison, WI -- districts that changed formal discipline policy over our study period -- are excluded.
Sources: Discipline information from the US Department of Education Civil Rights Data Collection (CRDC); CEP participation information from state departments of education, USDA, and CBPP.

Table 6: Effect on CEP in high- vs. low-predicted high food insecurity areas

	(1) Multiple OSS	(2) Multiple OSS
CEP elig	-0.161	-0.332
	[0.101]	[0.340]
High child FI X CEP elig	-0.379***	-0.332
	[0.141]	[0.340]
Observations	31,110	6,526
R-squared	0.012	0.015
Number of schools	10,280	2,390
FE	School and yr	School and yr
School	Elementary	Middle
Sample	CEP partic, no change	CEP partic, no change
Base DV mean	1.907	5.526
Ftest p-value	2.08e-05	0.0870

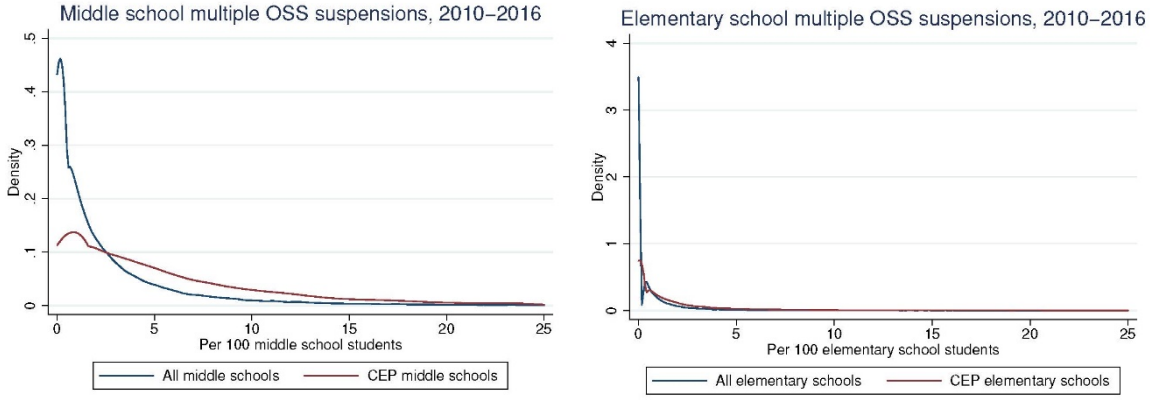
Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district in brackets. Sample limited to middle and elementary schools with at least 50 students and discipline rates below the 99th percentile for all schools. "CEP" equals one if a school participates in CEP in year t."State eligible" is an indicator equal to one if a school is in a state eligible to participate in year t. Baseline mean calculated as the mean of the dependent variable for 2010 for schools in the 2010 sample; 2012 otherwise. Schools in Broward County, FL; Chicago, IL; Denver, CO; Los Angeles, CA; San Diego, CA; Mobile County AL; Oakland, CA; Prince George's County MD; Minneapolis, MN; St. Paul, MN.; Miami-Dade County, FL; Hillsborough County, FL; Palm Beach, FL; Portland, OR; Philadelphia, PA; Fairfax County, VA; and Madison, WI -- districts that changed formal discipline policy over our study period -- are excluded. High food insecurity is defined as more than 22 percent of children predicted to experience food insecurity over the 2010-2015 period.
Sources: Discipline information from the US Department of Education Civil Rights Data Collection (CRDC); CEP participation information from state departments of education, USDA, and CBPP.

Figure 1: Share of students suspended for multiple days and share of students eligible for free meals, by grade level



Notes: Sample includes all middle (left panel) and elementary (right panel) schools with suspension rates below the 99th percentile.
Sources: Discipline data from CRDC and CEP participation data from CCD, fraction eligible for free or reduced-price lunch from the Department of Education CCD.

Figure 2: Suspension distribution, by grade level and CEP participation



Notes: Sample includes all middle (left panel) and elementary (right panel) schools with multiple out-of-school suspension rates below the 99th percentile. The blue line includes all schools, the red line shows that adopted CEP by 2017 and that are not in large districts that formally changed discipline policy over the 2010-2016 period.

Sources: Discipline data from CRDC and CEP participation data from CCD, state departments of education, and USDA FRAC and CBPP.

Appendix Table 1: Effect of CEP on high school suspensions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Multiple OSS	Multiple OSS	Multiple OSS	Multiple OSS	Multiple OSS	Multiple OSS	Log (Multiple OSS)
CEP elig	2.040***	-0.113	0.210	0.0853	0.231	0.143	0.0550
	[0.145]	[0.138]	[0.190]	[0.252]	[0.228]	[0.320]	[0.0539]
Observations	51,591	51,591	10,672	7,065	6,587	6,527	6,527
R-squared	0.023	0.033	0.029	0.022	0.028	0.052	0.033
FE	None	School and yr	School and yr	School and yr	School and yr	School and yr	School and yr
School	High	High	High	High	High	High	High
Sample	All	All	CEP eligible	CEP partic	CEP partic, no change	CEP partic, no change	CEP partic, no change
Base DV mean	3.473	3.473	6.796	6.531	6.763	6.830	6.830
Number of schools	18,795	18,795	4,158	2,696	2,509	2,453	2,453
Weight						Enrollment	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district in brackets. Sample limited to high schools with at least 50 students and discipline rates below the 99th percentile for all schools. "CEP" equals one if a school participates in CEP in year t."State eligible" is an indicator equal to one if a school is in a state eligible to participate in year t. Baseline mean calculated as the mean of the dependent variable for 2010 for schools in the 2010 sample; 2012 otherwise. Columns (5-7) exclude schools in Broward County, FL; Chicago, IL; Denver, CO; Los Angeles, CA; San Diego, CA; Mobile County AL; Oakland, CA; Prince George's County MD; Minneapolis, MN; St. Paul, MN.; Miami-Dade County, FL; Hillsborough County, FL; Palm Beach, FL; Portland, OR; Philadelphia, PA; Fairfax County, VA; and Madison, WI -- districts that changed formal discipline policy over our study period. Log multiple OSS calculated as log(multoss + 1) in order to include schools reporting no instances of out-of-school suspensions.
Sources: Discipline information from the US Department of Education Civil Rights Data Collection (CRDC); CEP participation information from state departments of education, USDA, and CBPP.

Appendix Table 2: First stage: CEP eligibility and participation

VARIABLES	(1) CEP	(2) CEP	(3) CEP	(4) CEP
Categorically-elig ≥40% X state elig	0.342***		0.180***	0.360***
	[0.00372]		[0.00623]	[0.00372]
Categorically-elig ≥62.5% X state elig		0.571***		
		[0.00838]		
% district ≥40% cat. elig X state elig			0.00250***	
			[7.79e-05]	
Number of schools	14,643	14,643	14,643	14,497
Sample	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no change
N	46505	46505	46505	46092
First Stage F	8438	4647	4877	9344
Overid p	0	0	0	0

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district in brackets. Sample limited to middle schools with at least 50 students and discipline rates below the 99th percentile for all schools. "CEP" equals one if a school participates in CEP in year t."State eligible" is an indicator equal to one if a school is in a state eligible to participate in year t. Baseline mean calculated as the mean of the dependent variable for 2010 for schools in the 2010 sample; 2012 otherwise. Schools in Broward County, FL; Chicago, IL; Denver, CO; Los Angeles, CA; San Diego, CA; Mobile County AL; Oakland, CA; Prince George's County MD; Minneapolis, MN; St. Paul, MN.; Miami-Dade County, FL; Hillsborough County, FL; Palm Beach, FL; Portland, OR; Philadelphia, PA; Fairfax County, VA; and Madison, WI -- districts that changed formal discipline policy over our study period -- are excluded.
Sources: Discipline information from the US Department of Education Civil Rights Data Collection (CRDC); CEP participation information from state departments of education, USDA, and CBPP.

Appendix Table 3: Instrumental Variables: Effect of CEP on multiple suspensions, middle schools

VARIABLES	(1) Multiple OSS	(2) Multiple OSS	(3) Multiple OSS	(4) Multiple OSS	(5) Multiple OSS
CEP elig	-1.358***	-0.394**	-1.400***	-0.426***	-1.536***
	[0.146]	[0.186]	[0.138]	[0.135]	[0.578]
R-squared	0.013	0.023	0.012	0.017	-0.000
Number of schools	14,643	14,643	14,643	14,497	2,297
IV	ISP ≥40% X stelig	ISP ≥62.5% X stelig	ISP ≥40% X stelig, % district elig X stelig	ISP ≥40% X stelig	ISP ≥40% X stelig
Sample	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no FL	CEP partic, no change
N	46505	46505	46505	46092	7138
Base DV mean	3.385	3.385	3.385	3.040	7.635
First Stage F	8438	4647	4877	9344	539.4
Overid p	0	0	0	0	0

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district in brackets. Sample limited to middle schools with at least 50 students and discipline rates below the 99th percentile for all schools. "CEP" equals one if a school participates in CEP in year t."State eligible" is an indicator equal to one if a school is in a state eligible to participate in year t. Baseline mean calculated as the mean of the dependent variable for 2010 for schools in the 2010 sample; 2012 otherwise. Schools in Broward County, FL; Chicago, IL; Denver, CO; Los Angeles, CA; San Diego, CA; Mobile County AL; Oakland, CA; Prince George's County MD; Minneapolis, MN; St. Paul, MN.; Miami-Dade County, FL; Hillsborough County, FL; Palm Beach, FL; Portland, OR; Philadelphia, PA; Fairfax County, VA; and Madison, WI -- districts that changed formal discipline policy over our study period -- are excluded.
Sources: Discipline information from the US Department of Education Civil Rights Data Collection (CRDC); CEP participation information from state departments of education, USDA, and CBPP.

Appendix Table 4: Effect of CEP on suspension rates by ethnicity and Limited English Proficiency

	(1)	(2)	(3)	(4)
VARIABLES	Hispanic	Hispanic	LEP	LEP
CEP elig	-0.169*	-0.352	-0.163*	-0.891*
	[0.101]	[0.382]	[0.0984]	[0.504]
Observations	32,012	6,689	27,685	5,843
R-squared	0.001	0.006	0.001	0.005
# of schools	11,053	2,540	9,873	2,288
FE	School and yr	School and yr	School and yr	School and yr
School	Elementary	Middle	Elementary	Middle
Sample	CEP partic, no change	CEP partic, no change	CEP partic, no change	CEP partic, no change
Base DV mean	1.144	3.975	0.698	4.065

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district in brackets. Sample limited to middle and elementary schools with at least 50 students and discipline rates below the 99th percentile for all schools. "CEP" equals one if a school participates in CEP in year t."State eligible" is an indicator equal to one if a school is in a state eligible to participate in year t. Baseline mean calculated as the mean of the dependent variable for 2010 for schools in the 2010 sample; 2012 otherwise. Schools in Broward County, FL; Chicago, IL; Denver, CO; Los Angeles, CA; San Diego, CA; Mobile County AL; Oakland, CA; Prince George's County MD; Minneapolis, MN; St. Paul, MN.; Miami-Dade County, FL; Hillsborough County, FL; Palm Beach, FL; Portland, OR; Philadelphia, PA; Fairfax County, VA; and Madison, WI -- districts that changed formal discipline policy over our study period -- are excluded.
Sources: Discipline information from the US Department of Education Civil Rights Data Collection (CRDC); CEP participation information from state departments of education, USDA, and CBPP.

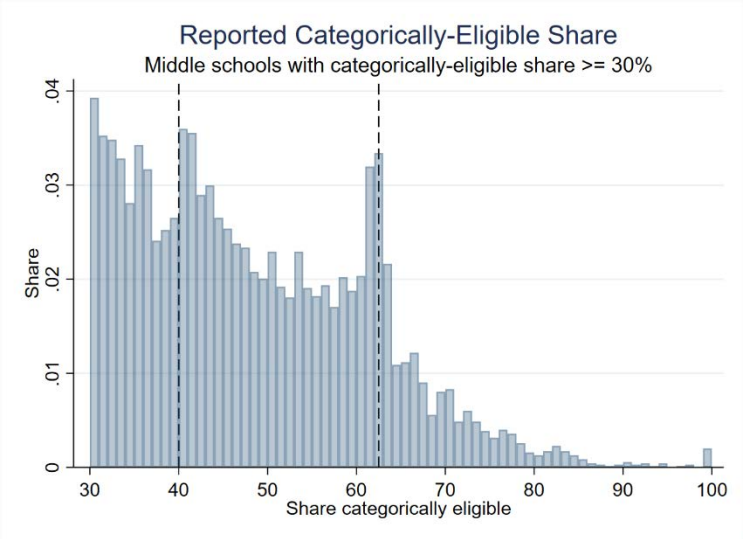
Appendix Table 5: Effect of CEP on any out-of-school suspension, by grade level. Preferred participation sample and expanded eligible sample

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Any OSS	Any OSS	Any OSS	Any OSS	Any OSS	Any OSS
CEP elig	-0.252*	-0.366	0.720*	0.106	0.209	0.479
	[0.150]	[0.402]	[0.399]	[0.119]	[0.294]	[0.301]
Observations	34,437	7,138	6,587	67,046	13,833	10,672
R-squared	0.004	0.014	0.027	0.004	0.026	0.030
FE	School and yr	School and yr	School and yr	School and yr	School and yr	School and yr
School	Elementary	Middle	High	Elementary	Middle	High
Sample	CEP elig	CEP elig	CEP elig	CEP partic, no change	CEP partic, no change	CEP partic, no change
Base DV mean	5.176	15.46	14.78	4.250	13.93	14.66
Number of schools	11,430	2,625	2,509	21,915	4,957	4,158

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district in brackets. Sample limited to schools with at least 50 students and discipline rates below the 99th percentile for all schools. "CEP" equals one if a school participates in CEP in year t."State eligible" is an indicator equal to one if a school is in a state eligible to participate in year t. Baseline mean calculated as the mean of the dependent variable for 2010 for schools in the 2010 sample; 2012 otherwise.
Sources: Discipline information from the US Department of Education Civil Rights Data Collection (CRDC); CEP participation information from state departments of education, USDA, and CBPP.

Figures

Appendix Figure 1: Distribution of categorically-eligible share



Notes: Figure shows the distribution of the reported share of students categorically eligible for free school meals. The left dashed line at 40 percent marks the minimum categorically-eligible share to be eligible for CEP. The right dashed line at 62.5 percent marks the point at which a school receives full federal funding at the free lunch rate for all meals served. Sources: State departments of education (2012-2014), CBPP (2015-2016), and FRAC (2017).
Sources: Categorically-eligible information from state departments of education, USDA, and CBPP.