SCHOOLWIDE FREE MEALS AND STUDENT DISCIPLINE: EFFECTS OF THE COMMUNITY ELIGIBILITY PROVISION

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Abstract

This paper examines whether schoolwide free meals affect disciplinary outcomes, focusing on the use of suspensions. 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 school meals. We leverage the staggered rollout of CEP across states and school discipline measures for the near-universe of public schools to assess how disciplinary infractions change within a school as it becomes eligible for CEP. We conclude that schoolwide free meals reduced suspensions statistically significantly by approximately 17 percent for white male elementary students. Point estimates for other subgroups in elementary schools, and overall, are negative but smaller in magnitude; while treatment effects for black students are statistically insignificant, we also cannot rule out equal treatment effects between black and white students. We lack statistical power to rule large positive or negative effects for middle and high school students. The reductions among white students are somewhat larger in areas with high baseline poverty rates, consistent with universal meals programs serving an unmet need.

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1. INTRODUCTION

The school meals programs—the National School Lunch Program and School Breakfast Program—are two of the nation's largest and oldest forms of nutritional assistance to school-aged children.[1] On a typical day, most students consume a meal at school, and more than one third receive a school lunch at no cost (USDA 2018). For much of their histories, these programs had undergone few reforms; however, recent years have shown a marked change with the rapid expansion of the Community Eligibility Provision (CEP). Schools that choose to participate in CEP offer federally subsidized meals to *all* students, regardless of individual student family income. In addition to expanding the number of children eligible to consume free school breakfasts and lunches, CEP's schoolwide nature could reduce the stigma of eating free meals and potentially enhance school climate. CEP participation has increased among eligible schools each year, and in 2016–17, about one in five public schools participated.[2] Therefore, understanding the nature of any benefits is increasingly important for informing policy parameters related to CEP's scale and scope.

This paper provides the first evidence of the effect of schoolwide free meals on disciplinary outcomes on a national scale. Our identification strategy compares the share of students in a school (or demographic group at the school) receiving out-of-school suspensions before and after the school becomes eligible for CEP, leveraging statutory variation in the timing of eligibility across states. This approach allows us to estimate the causal impact of eligibility for free school meals on discipline by comparing schools serving similarly low-income student populations with similar incentives and ability to adopt CEP but with differential access to the program based on the state in which they are located. Schools are eligible for CEP based on how many of their students qualify based on income; from 2012 to 2014, eligible schools within CEP pilot states became eligible to participate, and in 2015, all income-eligible schools nationally could participate. Through 2017, only about half of eligible schools chose to participate. We document that schools that choose to participate serve demonstrably more disadvantaged students than eligible schools that do not opt in, and therefore limit our sample to the group of schools that ever choose to participate by 2017. We estimate intent-to-treat effects with school-level fixed effects, in order to account for unobserved school- and state-level variation. This approach provides identifying variation coming from the state pilot schedule, rather than unobserved local characteristics. Importantly, all of our results are robust to limiting the sample to states participating in the pilot years (excluding the national rollout) or to schools that adopted CEP within three years of eligibility.

Schoolwide free meals could affect student behavior through some combination of directly improving students' nutritional status and improving school climate by reducing social stigma. The public health literature establishes a highly intuitive correlation between hunger and behavior (Alaimo, Olson, and Frongillo 2001; Jyoti, Frongillo, and Jones 2005; Howard 2011). While Ruffini (2018) finds that CEP yielded modest improvements in math performance for student groups with low participation under the traditional program, behavior may also be immediately affected by changes

1. We refer to the combined lunch and breakfast programs as the "school meals program" throughout.
2. Authors' calculations using CEP participation data from Food Research & Action Center (2017) and U.S. Department of Education (2017).

in *current* nutrition status and less dependent on accumulated skills or previous activity than achievement-based outcomes.

We estimate the impact of CEP on a measure of exclusionary discipline, the share of students receiving any out-of-school suspension in a year, as national data on behavior are not available. This measure is available at the school building level, with aggregate counts and gender-specific counts by race and ethnicity, for the entire United States through the U.S. Department of Education's Civil Rights Data Collection (CRDC). This measure is of independent policy interest, given concerns that the use of exclusionary discipline may have longer-term consequences by forming a "school-to-prison pipeline" that directs students from schools to the criminal justice system (e.g., Mallett 2016; Bacher-Hicks, Billings, and Deming 2019). While reported disciplinary incidents surely do not capture all elements of student behavior and measure others with error, within schools over time, this measure likely captures useful information about student conduct. In addition, this national data source allows us to overcome many concerns about selection into the CEP program that are inherent in analyses looking at effects within a single state.

We find that CEP eligibility reduced suspensions among white male elementary school students by about 17 percent, from a base rate of approximately 5.7 students per 100. The drop is slightly larger in areas with high poverty rates, consistent with schoolwide free meals meeting an unmet need for nutritional assistance. Statistically insignificant point estimates are consistent with more modest reductions for other elementary student subgroups—black and Hispanic students and female students—and estimates for black male students are estimated much less precisely. However, these subgroup coefficients lie in wide confidence intervals, preventing us from ruling out moderate impacts in either direction. Results for white and black students do not significantly differ. We lack statistical power to rule large positive or negative effects for middle and high school students.

Our findings are consistent with previous work documenting that greater nutritional assistance reduces student discipline for at-risk populations. Health and social service literatures have shown that hunger and food insecurity are associated with worsened externalizing behaviors (Slack and Yoo 2005; Slopen et al. 2011). A number of studies suggest that providing free school meals can improve individual outcomes ranging from academic achievement to nutritional status (Dunifon and Kowaleski-Jones 2003; Bhattacharya, Currie, and Haider 2006; Nord and Romig 2006; Hinrichs 2010; Arteaga and Heflin 2014). Directly documenting a link between nutritional assistance and discipline, Gennetian et al. (2016) find that as Supplemental Nutrition Assistance Program (SNAP) benefits are likely to run out near the end of the month, low-income Chicago students are more likely to be disciplined at school.

This paper also joins an emergent literature on how *universal* free school meals affect student outcomes. Like the current paper, this literature is unable to disentangle any differential effect through providing meals to more students versus reducing any stigma associated with the meals. Any reduction in stigma could independently improve school climate, which could in turn affect students' behavior and/or teachers' and administrators' propensity to use exclusionary discipline conditional on behavior. The existing research finds schoolwide free meals improve nutritional intake, with positive to null effects on academic performance, behavior, and discipline (Dotter 2013;

Imberman and Kugler 2014; Schanzenbach and Zaki 2014; Fuller and Comperatore 2018; Kho 2018; Ruffini 2018). Three papers look at state-level experiences with CEP specifically, comparing disciplinary outcomes in schools that chose to participate in CEP with otherwise similar schools that did not. Kho (2018) finds CEP participation led to sizeable reductions in suspensions and expulsions in Tennessee. Fuller and Comperatore (2018) find CEP improved math and reading performance for white and economically disadvantaged students but had no effect on the number of suspensions in North Carolina, both in the aggregate and across a range of subgroups. Finally, Gordanier et al. (2020) document CEP improved math performance among South Carolina elementary students, with smaller effects for older students and reading performance.

We contribute to this literature by providing the first nationwide analysis of how a universal meals program (CEP) affects discipline. This approach allows us to identify the effect of schoolwide free meals based on cross-state variation that is determined by federal regulation rather than local participation decisions. Our results show that schoolwide free meals cause a moderate, statistically significant reduction in exclusionary discipline for white male students, especially in high-poverty districts. Although we do not estimate statistically significant results for other subgroups, we cannot rule out modest reductions in suspensions.

This paper proceeds as follows. Section 2 describes CEP and the existing literature on school meals and student outcomes, and section 3 provides policy background on student discipline during this time period. Section 4 describes our data, and section 5 presents our empirical framework. Section 6 details results. Section 7 concludes.

2. BACKGROUND ON SCHOOL MEALS AND CHILDREN'S NUTRITION ASSISTANCE PROGRAMS

In 2017, 15 percent of American households with children were food insecure, meaning that a lack of household resources affected their food intake or eating patterns (Coleman-Jensen et al. 2018). The existing literature links food insecurity to myriad contemporaneous negative outcomes for children, including worsened physical and mental health and social and interpersonal development (Alaimo, Olson, and Frongillo 2001; Gundersen and Kreider 2009; Howard 2011; Kimbro and Denney 2015), as well as poorer health, lower educational attainment, and lower labor force attachment in the long term (Case, Fertig, and Paxton 2005; Ryu and Bartfeld 2012). If school meals improve nutrition, they help fulfill a basic physiological need (Maslow 1943) regardless of how they affect other outcomes. Nonetheless, understanding the impact of subsidized meals on health, educational, and behavioral outcomes is important for determining optimal public support for these programs. If these associations reflect a causal impact of food insecurity, then nutritional assistance, including universal school meals, could improve student outcomes and behaviors.

In addition to family-based assistance programs, low-income children are eligible for two school-based meals programs—the School Breakfast Program and the National School Lunch Program. Historically, under both 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 meal at the full "paid" rate.[3]

Unlike other means-tested programs that require meeting with a case manager and satisfying various asset tests and income verification, students receiving SNAP and other means-tested programs are categorically eligible, or "directly certified," and automatically enrolled in free meals, and other income-eligible students can obtain free meals by having their parents complete a brief income questionnaire. Not all students who are income-eligible participate in the school meals program; income-eligible participation rates are lower for white students than black students (Domina et al. 2017).

Participation in the school meals program, particularly the subsidized component, has increased over the past fifty years. Whereas about 15 percent of school lunches in 1969 were free or reduced-price (FRP), 74 percent were in 2017, with most of the increase driven by the share of free meals (USDA 2018). Given these patterns, students may infer with reasonably high probability that peers consuming school meals are consuming a FRP meal and have low family incomes, even without observing anything about the transaction that would reveal the form of payment or benefit.[4] Breakfast and lunch options available to students vary across grade level, with elementary students being more likely to receive a breakfast in the classroom (FRAC n.d.) and middle and high school students having greater access to vending machines and a la carte options that are not part of CEP or the traditional school meals program (USDA 2012b). Therefore, in elementary schools, the free meal is more likely to be the only option for students who do not bring a lunch from home, whereas such meals are less "universal" if on-site alternatives are available.

In general, the existing literature finds the traditional school meals programs—where eligibility is based on family income—increase food consumption and nutritional intake for some groups of students, especially low-income children (Gleason and Suitor 2003; Bhattacharya, Currie, and Haider 2006; Kreider et al. 2012; Arteaga and Heflin 2014; Frisvold 2015). These programs also improve academic performance among the most disadvantaged subpopulations (Bhattacharya, Currie, and Haider 2006; Frisvold 2015), with mixed effects overall (Dunifon and Kowaleski-Jones 2003; Hinrichs 2010). Of these papers, only Frisvold considers effects separately by student gender and finds larger academic improvement for male students.

In recent years, districts and schools have begun offering schoolwide free meals to all students, regardless of an individual student's family income. By expanding eligibility to all students, universal school meals programs are expected to increase participation among higher-income students, as well as lower-income students who are income-eligible but were not certified to receive free meals. Schoolwide free meal programs may also reduce stigma associated with free meals and increase consumption among students previously eligible.

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

4. 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 2014). The 1970 amendments to the National School Lunch Act 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 noncash transactions, other students may ascertain payment status. See, for example, Kavanagh (2010) and Pogash (2008).

A growing literature examines the effect of expanding free meal eligibility to all students.[5] The existing literature focuses on meal consumption and student achievement and tends to find universal access increases school meal participation, with mixed effects on student performance. Schwartz and Rothbart (2019) find universal access to free meals in New York City increases school meal consumption and improves academic performance, particularly for nonpoor students. Consistent with these programs reducing stigma, the New York City experience shows that universal school meals increase meal participation for both income-eligible and non-eligible students (Leos-Urbel et al. 2013; Schwartz and Rothbart 2019). While reduced stigma is a plausible channel for our findings, we are unable to fully explore this hypothesis, as data on which students actually receive school meals are not tracked in most CEP schools.

The CEP is the largest federal initiative to provide schoolwide meals. Like other universal meals programs, 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—that is, their families receive another form of means-tested income assistance, most commonly SNAP.[6] 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 individual schools in the group do not have 40 percent of students categorically eligible.[7]

Importantly for our strategy, schools became eligible for CEP at different points over a four-year period from 2011–12 through 2014–15. (Going forward we refer to academic years by the year of the spring semester.) Legislation determined the number of states that were able to adopt each year, and U.S. Department of Agriculture (USDA) selected the states eligible to ensure "an adequate number and variety of schools and [districts] that could benefit from [CEP]" (Public Law 111-296; USDA 2011, 2012a, 2013).[8] Under these regulations, 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

5. Early universal school meals programs include Provisions 1–3 of the National School Lunch Act, as well as district level reforms. Compared with these pre-existing efforts, however, the CEP 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.
6. States and districts match students to administrative records for other assistance programs in order to "directly certify," or automatically enroll, individual students to receive FRP meals, even if the school does not participate in CEP (regardless of a school's eligibility). Students receiving Temporary Assistance for Needy Families or the Food Distribution Program on Indian Reservations are also directly certified; these programs have more limited reach than SNAP.
7. We rely on state data that report the share of students categorically eligible aggregated to the grouping of schools used to determine eligibility.
8. Alaska, the District of Columbia, Illinois, Kentucky, Louisiana, Michigan, Mississippi, Oklahoma, South Carolina, and Tennessee were eligible to apply for CEP in 2012. USDA chose three of these states (Illinois, Kentucky, and Michigan) to participate that year. In the subsequent two years, USDA guidance decided which states would be chosen based on knowledge of CEP, the state's approaches to interagency communication around poverty data, and eligibility and likeliness of participation. Of the states not chosen to participate in 2012, only the District of Columbia was eventually chosen as a pilot state before the national implementation.

eligible in 2015. Over the 2012 through 2015 period, this pilot program design provides variation in access to free school meals over time and across states that is unrelated to school-level family resources *and* school decisions to opt into the program.

Within states, participation has also increased over time, and not all eligible schools participate. Several recent papers examine the effect of CEP on student performance by leveraging within-state variation, comparing participating and nonparticipating schools that are eligible for CEP. This approach generates effects of treatment on the treated for students in schools that chose to participate but does not fully account for selection into the program. For example, Fuller and Comperatore (2018) examine the North Carolina setting using administrative student-level data and find that universal meals reduce absences and improve test scores, while not significantly changing disciplinary outcomes, either overall or for any racial/ethnic subgroup. Kho (2018) takes a similar strategy and concludes that CEP reduced suspensions in Tennessee by about 10 percent.

Our paper complements the existing literature: Rather than using observed variation stemming from local participation decisions conditional on eligibility as in the state-level studies, our national sample allows us to leverage variation in the USDA-determined timing of state pilots. Importantly for our analysis, disciplinary actions were not criteria for selecting pilot states, and state-level direct certification rates (the share of SNAP recipients automatically enrolled in school meals) also are uncorrelated with when a state become eligible for CEP (USDA 2016). Moreover, a survey of participating and nonparticipating local education agencies during the first two years of the pilot program found that local rather than state actors were primarily responsible for the participation decision and that most districts received information about CEP from their states (Bartlett et al. 2014). Further, there were no systematic differences in how states participating in the pilot's first year communicated with their districts, relative to states who entered in year 2 (Bartlett et al. 2014).

CEP primarily expanded eligibility for free meals, but it may have also changed how meals were provided. Unfortunately, we lack systematic data reporting seemingly simple things, like cafeteria capacity, how lunch is served, or if students are permitted to leave campus for lunch. An early survey notes CEP schools were more likely to supplement traditional cafeteria breakfast lines with "grab-and-go" options or breakfast in the classroom, but to our knowledge, there is no comparable evidence about how lunches were served or whether CEP changed scheduling practices (Logan et al. 2014). Research on earlier universal free breakfast programs suggests that the delivery method may matter: breakfasts served during the school day increase participation and improve student performance (Dotter 2013; Imberman and Kugler 2014), while those served before the school day show less-positive results (Bartlett et al. 2014; Schanzenbach and Zaki 2014). With the available data, we are unable to document the extent to which CEP adoption coincided with changes in how meals were offered. Our estimates include any impact of CEP-induced moves to nontraditional offering techniques.

3. STUDENT DISCIPLINE BACKGROUND

In recent years, suspension rates (particularly race- and disability-based gaps) have come under increased scrutiny. We chronicle the timeline to emphasize how policies other than CEP may have influenced recorded discipline rates and explain how we

adjust our sample to account for observed contemporaneous changes in policy and practice.

The Obama Administration's Education Department was focused on documenting and reducing disparities in suspension rates by race, ethnicity, and disability status. Schools are required to submit biennial data on the number of suspensions by race and subgroup through CRDC, the data source we use. 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); the survey was made mandatory for all schools beginning in 2012. Starting in 2012, essentially all reporting schools (about 95,000) provide information on suspension activity.

In November 2014, the U.S. Department of Education and Department of Justice followed up on the CRDC expansion with a Dear Colleague letter on racial disparities in school discipline.[9] The letter summarized disparities found in the 2012 CRDC, noting, "Although African-American students represent 15% of students in the CRDC, they make up 35% of students suspended once, and 44% of students suspended more than once" (Lhamon and Samuels 2014, p. 3). It also explained the relevant law, investigative process, and potential remedies. Notably, the guidance encouraged districts to reduce the use of exclusionary discipline, including suspensions, and instead focus on positive approaches. It also provided illustrative examples of policies and practices that schools could adopt to avoid civil rights violations.

The guidance was the result of a building movement to reduce racial discrepancies in discipline and high suspension levels generally. A number of districts changed their formal discipline policies over our sample period. For example, Chicago ended a zero-tolerance policy that prohibited most out-of-school suspensions in 2012, and Broward County (Florida) increased diversions as an alternative to suspensions in 2013. At the state level, the California legislature banned willful defiance as a reason for expulsions or suspensions for K–3 students beginning in the 2015 school year. Our main specifications account for these contemporaneous changes by excluding schools in large school districts that issued changes in formal discipline policy at any point over the 2010–16 period, as these policy changes may be linked to true or reported changes in the frequency of suspension.[10] Similarly, schools may change practices even if formal policy does not change. As a robustness check, we show that our results are qualitatively similar when including all schools, including those in districts with formal discipline policy changes.

Policy and research discussions around these issues identify different issues at different developmental stages. Some advocacy and policy efforts have singled out the use of exclusionary discipline among very young students and students with disabilities and

9. Sec. Betsy DeVos rescinded this guidance in December 2018.

10. We based our sample of districts for these searches on Appendix F of Eden (2017), which contains media citations for discipline reform in the largest 100 school districts from year to year. 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. Schools in these districts account for about 5.5 percent of all schools, and 8.8 percent of all CEP elementary schools. While our main estimates retain California, findings for all grade levels are robust to excluding the entire state. Hashim, Strunk, and Dhaliwal (2018) review the literature on suspension bans and restorative justice and study their effects in Los Angeles.

race-based gaps within these groups. As Gilliam et al. (2016, p. 2) write, "Sex and race disparities in early expulsions and suspensions may be associated with several factors related to stress-tolerance and poor access to high-quality early learning environments and supports." Meanwhile, developmental psychologists have long flagged the gap between the developmental needs of young adolescents and many aspects of the middle school environment (see, e.g., Eccles et al. 2016).

4. DATA

Exposition by Grade Span

We present descriptive statistics and estimate our specifications separately by grade span because, in addition to serving students at different developmental stages, elementary and secondary schools have distinctive environments around both school meals and student discipline.[11]

There are more than twice as many elementary schools than middle and high schools combined, which allows us to detect more modest changes in suspension activity for elementary schools than middle and/or high schools. Indeed, we find no statistically significant results for the combined middle and high school sample. Going forward, we present all descriptive statistics and results for both samples (elementary schools, and combined middle and high schools) in the tables and restrict our discussion in the text to the elementary schools—not because they are more important, but because even the pooled middle and high school sample yields such wide confidence intervals that we cannot rule out large positive or negative impacts.

CEP Participation and Categorical Eligibility

We collect information on CEP participation from state Departments of Education for years 2012 through 2014 and from the Food Research & Action Center (FRAC) for years 2015 through 2017 and match these data to school and student demographic characteristics from the Common Core of Data (CCD) (USDOE 2017) and discipline data from the CRDC.

Table 1 presents in columns 1–3 summary statistics for elementary schools based on CEP eligibility and participation, and for the pooled middle and high school sample in columns 4–6. We determine CEP eligibility based on the reported share of students categorically eligible for free meals from the 2017 FRAC CEP participation data.[12] 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 low a share of qualifying students; columns 2 and 5 describe schools that were eligible to participate by 2017 because they had enough qualifying students but chose not to participate; and columns 3 and 6 focus on schools that participated at any point by 2017—about half of all eligible schools.

11. Pooling middle and high schools also enhances our statistical power. On average, we would be able to detect a 0.9 percentage point change in either middle or high schools separately, compared with 0.6 percentage points for the pooled sample.
12. CEP schools report categorically eligible shares at least once every five years. Among schools reporting multiple categorically eligible shares over the 2012–17 period, the average change is approximately 1 percentage point.

Table 1. School Demographic and Economic Characteristics by Community Eligibility Provision (CEP) Eligibility and Participation

	Elementary			Middle and High		
	Not CEP Eligible	CEP Eligible, Not Participating	CEP Participating	Not CEP Eligible	CEP Eligible, Not Participating	CEP Participating
	(1)	(2)	(3)	(4)	(5)	(6)
ISP	34.72	52.20	61.70	34.53	48.67	57.86
	(2.867)	(10.89)	(11.41)	(2.85)	(8.43)	(10.13)
% FRPL Eligible	29.89	63.12	68.99	35.25	65.30	68.71
	(24.46)	(28.38)	(29.24)	(21.57)	(21.91)	(24.63)
% Black	6.276	16.23	30.54	7.724	19.14	29.37
	(12.08)	(22.20)	(33.46)	(12.70)	(21.79)	(31.63)
% Hispanic	14.09	31.78	25.89	14.64	30.96	27.38
	(19.60)	(31.56)	(31.89)	(19.32)	(31.25)	(33.44)
% White	55.62	39.01	31.28	68.45	42.22	35.89
	(34.61)	(32.45)	(32.69)	(26.93)	(30.36)	(33.05)
Urban	0.158	0.225	0.454	0.125	0.224	0.369
	(0.365)	(0.418)	(0.498)	(0.331)	(0.417)	(0.483)
Rural	0.280	0.219	0.191	0.293	0.225	0.223
	(0.449)	(0.414)	(0.393)	(0.455)	(0.418)	(0.416)
Enrollment	463.3	489.6	467.4	618.0	598.0	557.8
	(221.6)	(219.2)	(211.2)	(337.4)	(307.9)	(297.6)
Observations	27,171	9,410	11,186	10,097	1,953	2,128

Notes: This table shows demographic and economic characteristics for elementary (columns 1–3) and middle and high (columns 4–6) schools based on CEP eligibility and participation. Columns 1 and 4 include all public schools with an Identified Student Percentage (ISP) less than 40 percent (ineligible for CEP); columns 2 and 5 include schools with an ISP of at least 40 percent that did not participate in CEP by 2017; columns 3 and 6 include schools with an ISP of at least 40 percent that participated in CEP by 2017. See text for details. FRPL = free or reduced-price lunch.

Sources: ISP and CEP participation from Food Resource & Action Center and Center on Budget and Policy Priorities; student demographics and urban/rural location from the Department of Education Common Core of Data.

The table shows stark differences between ineligible, eligible but nonparticipating, and participating schools. Unsurprisingly, schools eligible for CEP (regardless of eventual participation) are more disadvantaged than ineligible schools: the first row of table 1 reports a categorical-eligibility rate of 35 percent for CEP-ineligible schools (column 1 for elementary schools and column 4 for middle and high schools), 52 (49) percent for eligible, nonparticipating elementary (middle/high) schools (columns 2 and 5), and 62 (58) percent for participating schools (columns 3 and 6). The share of students eligible for FRP meals (either through direct certification or completed paperwork) shows a similar pattern. Nearly 70 percent of students in CEP schools qualified for FRP meals based on reported family income, compared with 63–65 percent in eligible, nonparticipating schools. These patterns are consistent with the incentives posed by the CEP reimbursement scheme.[13] They are also consistent with how local education officials in pilot states responded to a survey about CEP implementation. Just over half of survey respondents from eligible nonparticipating school districts reported a concern that CEP would not be "financially viable" as an important barrier to implementation.

13. Under CEP, the federal government reimburses schools at the free meals rate ($3.23 for lunch and $1.75 for breakfast in the forty-eight contiguous states in 2018) for 1.6 times the categorically-eligible share, up to a maximum of 100 percent. This 1.6 multiplier means that if only 40 percent of students are 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. 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.

Schools that take up CEP participation are also more likely to be in urban locations and have significantly smaller white populations and a greater share of black students. All of these differences suggest eligible schools that chose not to participate in CEP have different incentives to participate in CEP due to their share of categorically-eligible students and also serve markedly different student bodies. Given these systematic differences in suspension rates by racial and ethnic composition, as well as *within* race/ethnic groups by school poverty level (GAO 2018), we restrict our main sample to ever-participating schools, and report intent-to-treat effects.

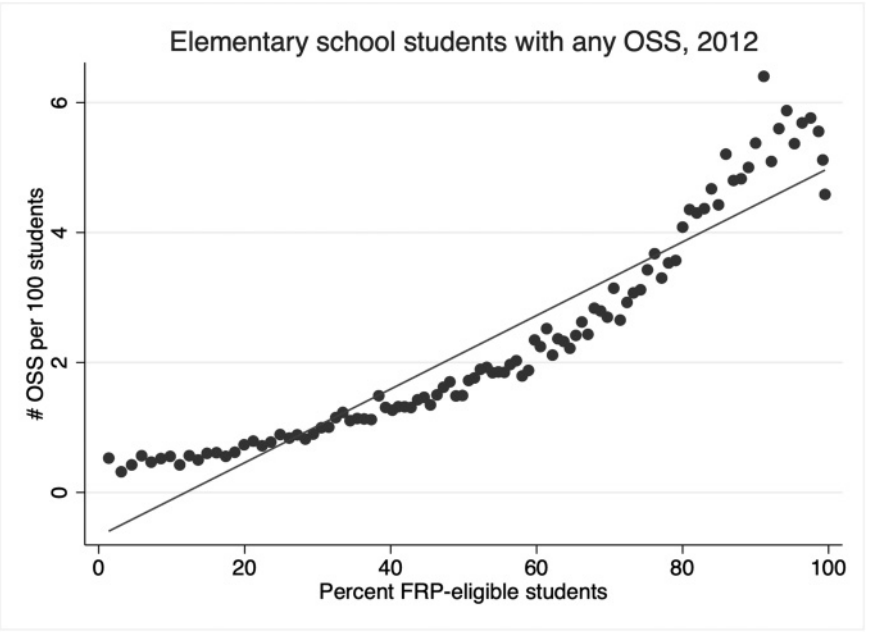
As discussed in section 3, the CRDC discipline measures are measured at the school level for the 2010, 2012, 2014, and 2016 academic years.[14] These data include the reported number of students per year with one out-of-school suspension (OSS), more than one OSS, and any number of in-school suspensions (ISS); there are no student-level data in the CRDC. We calculate the number of students with *any* out-of-school suspensions during the school year as the sum of students with one or more than one OSS, and we use enrollment from the CRDC as the denominator to generate suspension rates per 100 students.[15] Because our outcome is expressed as a rate, we drop schools with fewer than fifty students to avoid large fluctuations driven by small numbers of students. Finally, in order to avoid our results being driven by outlier observations, we drop schools with forty-seven or more OSS per one hundred students—the 99th percentile in our data.

We merge the CRDC data on school-level eligibility for CEP and school level characteristics from the CCD. Of the 126,470 schools identified in the CCD in 2012, 54,377 are elementary schools. Of these schools, we match 51,136 to the CEP data (match rate of 94 percent), and 49,074 to the CRDC data, for an overall match rate of about 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.[16]

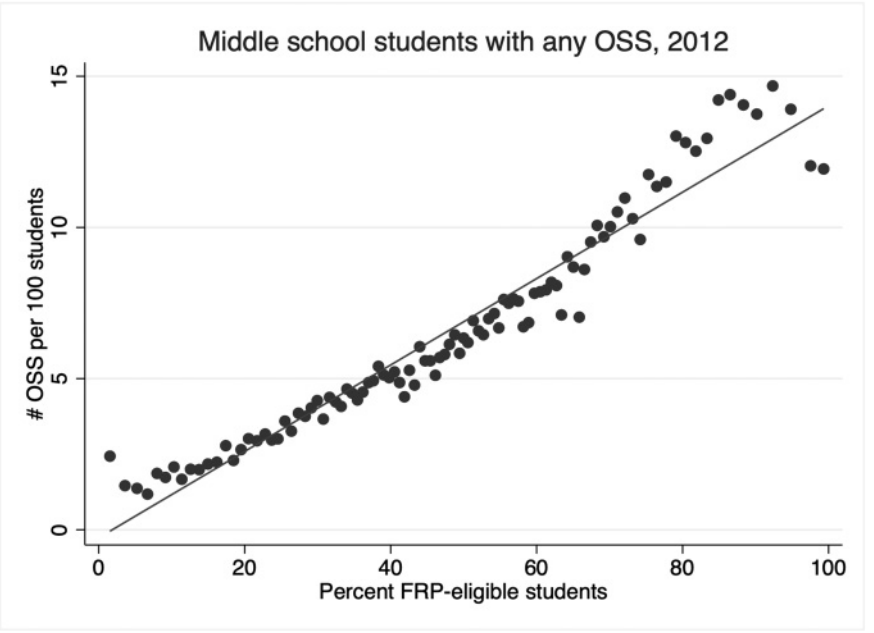
Figure 1 shows that in the 2012 baseline data, elementary schools where less than 20 percent of students qualified for FRP meals under the traditional program suspended about 0.5 students per one hundred each year. In schools with more economically disadvantaged students, suspension rates became positively and relatively linearly correlated with the fraction of students traditionally qualifying for FRP meals. In schools where at least 80 percent of students were traditionally eligible, about five students per one hundred were suspended each year. Patterns are similar for older students, albeit at a higher level.

As shown in table 1, CEP-participating schools tend to be more disadvantaged than eligible schools that opt not to participate, so it is not surprising that participating schools have higher baseline suspension rates than eligible, nonparticipating schools (table 2). In particular, the average suspension rate is 50 percent larger for participating

14. We use the 2012 report for baseline statistics but include the 2010 data for those schools reporting in our analyses. Results are robust to dropping 2010.
15. We use the share of students with any OSS rather than looking at the shares with one and multiple separately because it is easier to interpret absent student-level data. For example, if the share of students with exactly one OSS goes up, this might reflect a reduced likelihood of suspension, with students moving from multiple to one per year.
16. Following the Department of Education's classification scheme, we define elementary schools as those with a low grade between preschool and third grade and a high grade up through eighth grade. Match rates are nearly identical across grade levels.



(a)



(b)

Notes: Figure shows the relationship between the 2012 percent of students qualifying for free-and-reduced price meals under the traditional formula (e.g., family income below 180 percent of the federal poverty guidelines) and the number of students suspended per every 100 for elementary (a) and middle and high (b) students. OSS = out-of-school suspension.
Source: U.S. Department of Education (2014).

Figure 1. Suspension Rates and Percent Students Qualifying for Free or Reduced-Price (FRP) Meals

than nonparticipating elementary schools, and suspension rates are higher for each demographic group. These baseline differences suggest that simply comparing outcomes in participating schools to eligible schools that choose not to participate—much less to the full universe of schools—does not account for all factors determining participation and suspensions.

Table 2 also shows that in 2012, about 4.5 percent of elementary students in schools that ever adopted CEP were suspended, substantially lower than the 12 percent rate in middle schools and in high schools. It also shows considerable variation by race/ethnicity and gender within that 4.5 percent, with suspension rates ranging from 1.8 percent for Hispanic females to 10.3 percent for black males.

The first years of CEP coincide with heightened attention to the use of exclusionary discipline and active federal initiatives aimed at reducing the use of suspensions. Reflecting this policy environment, suspension rates in all elementary schools (regardless of CEP participation) slightly decreased over the 2012 through 2016 period, from 2.3 elementary students per 100 in 2012, to 2.2 in 2014, and 2.1 in 2016. Our difference-in-differences strategy controls for these aggregate time patterns and relies on the relatively weaker assumption that suspensions among participating schools in early- and late-eligible CEP states would have trended similarly in the absence of universal school meals. As the CRDC suspension data begin in 2010, with the full sample beginning only in 2012 (the first CEP pilot year), we are unable to fully examine baseline trends in suspension rates for the full sample of participating schools. However, Appendix table A.1 (which is available in a separate online appendix on *Education Finance and Policy*'s Web site at https://doi.org/10.1162/edfp_a_00307) shows that on several key baseline measures, participating schools are observationally similar across eligibility "cohorts." Specifically, participating schools that became eligible during the pilot years have similar baseline suspension rates and are located in similar geographical (urban/rural) locations as adopting schools that became eligible with the national rollout in 2015.[17]

5. METHODS

Any approach relying on the timing of school *participation* decisions, CEP_{st}, relies on the assumption that the year when a school decided to take up CEP is uncorrelated with potential discipline actions. This applies even to the most persuasive variant, such as an estimating equation (equation 1), incorporating school fixed effects, δ_s, to account for time-invariant school factors, such as neighborhoods and resources, as well as year fixed effects, ϑ_t, to account for secular trends in suspensions due to national advocacy, media, and policy affecting all schools at the same time.

$$suspendrate_{st} = \alpha CEP_{st} + \delta_s + \vartheta_t + \varepsilon_{st} \quad (1)$$

As illustrated in tables 1 and 2, participating schools are observationally different from both eligible nonparticipating and ineligible schools. These differences suggest that nonparticipating schools may differ on unobservable dimensions as well and in ways that may be correlated with discipline dynamics. Given the biennial nature of the CRDC

17. With the exceptions of Hispanic and white female students, baseline school suspension rates do not significantly vary by year of eligibility. We report results for Hispanic students but caution that they are underrepresented in the pilot states.

Table 2. Out-of-School Suspensions (OSS) by Community Eligibility Provision (CEP) Eligibility and Participation

		Elementary			Middle and High		
		Not CEP Eligible	CEP Eligible, Not Participating	CEP Participating	Not CEP Eligible	CEP Eligible, Not Participating	CEP Participating
		(1)	(2)	(3)	(4)	(5)	(6)
Any OSS, all students	Mean	1.24	3.04	4.45	5.39	10.51	11.85
	SD	1.97	3.64	5.31	5.24	7.96	9.03
	5th %ile	0.00	0.00	0.00	0.00	0.00	0.00
	10th %ile	0.00	0.00	0.00	0.00	0.95	1.05
	50th %ile	0.58	1.92	2.66	4.06	9.16	10.29
	90th %ile	3.30	7.42	11.26	11.88	21.66	24.54
	95th %ile	4.76	10.01	15.51	15.58	25.37	29.41
Any OSS, Black male	Mean	5.23	9.54	10.27	15.50	21.63	20.16
	SD	15.89	19.34	15.76	25.13	23.31	21.71
Any OSS, Hispanic male	Mean	2.13	3.55	4.89	9.53	13.25	14.01
	SD	7.80	8.27	12.74	17.51	19.25	22.47
Any OSS, White male	Mean	1.92	4.71	5.53	8.42	11.48	12.29
	SD	7.19	10.32	12.51	79.34	13.40	17.14
Any OSS, Black female	Mean	1.75	3.65	4.56	8.70	14.09	14.79
	SD	8.75	10.32	9.08	18.23	18.54	17.79
Any OSS, Hispanic female	Mean	0.54	1.12	1.81	4.78	7.69	8.71
	SD	3.90	5.24	7.97	12.26	14.31	17.69
Any OSS, White female	Mean	0.53	1.49	2.06	3.49	6.55	7.10
	SD	3.20	5.54	7.70	28.77	11.57	13.45
Absolute change in suspensions: 2012–14	Mean	0.99	1.96	2.78	2.90	5.02	5.56
	SD	1.60	2.52	3.66	3.57	5.07	5.68
	5th %ile	0.00	0.00	0.00	0.01	0.14	0.20
	10th %ile	0.00	0.01	0.04	0.15	0.51	0.50
	50th %ile	0.52	1.16	1.52	1.75	3.48	3.83
	90th %ile	2.44	4.68	6.97	6.91	11.53	12.81
	95th %ile	3.54	6.76	10.05	9.85	15.14	17.33
Absolute change in suspensions: 2014–16	Mean	0.89	1.72	2.50	2.29	4.02	4.52
	SD	1.43	2.29	3.39	3.02	4.41	4.88
	5th %ile	0.00	0.00	0.00	0.00	0.07	0.08
	10th %ile	0.00	0.00	0.02	0.10	0.32	0.34
	50th %ile	0.47	1.00	1.36	1.34	2.65	2.94
	90th %ile	2.20	4.12	6.17	5.42	9.56	11.11
	95th %ile	3.20	5.81	9.05	7.86	12.92	14.39
Observations		27,171	9,410	11,186	22,165	3,118	3,905

Notes: This table shows the distribution of baseline suspension rates for elementary (columns 1–3) and middle and high (columns 4–6) schools based on CEP eligibility and participation. Columns 1 and 4 include all public schools with an Identified Student Percentage (ISP) less than 40 percent (ineligible for CEP); columns 2 and 5 include schools with an ISP of at least 40 percent that did not participate in CEP by 2017; columns 3 and 6 include schools with an ISP of at least 40 percent that participated in CEP by 2017. See text for details. SD = standard deviation.

Sources: CEP participation from Food Research & Action Center and Center on Budget and Policy Priorities; suspension rates from USDOE 2014.

data and a lack of information on pre-CEP suspension trends, we are unable to test this hypothesis.

Therefore, our preferred estimation strategy avoids confounding the effect of universal meals with other concurrent factors—both observable and unobservable—that

affect participation timing by comparing changes within the group of schools that *choose* to adopt CEP by 2017 but become *eligible* in different years, depending on the incremental rollout across states. We take two steps to implement this approach. First, we limit the sample to schools in our sample that (by 2017) opt to participate in CEP, and therefore have similar incentives to implement universal free meals.[18] Second, we estimate equation 2 with the treatment parameter $stateelig_{st}$, an indicator equal to one once a school's state is eligible to participate. By using $stateelig_{st}$, rather than CEP_{st}, we isolate variation in participation that is determined by regulation and independent of school decisions *when* to opt in. As variation in CEP occurs at the state level, we report standard errors clustered by state.[19]

$$suspendrate_{st} = \beta stateelig_{st} + \delta_s + \vartheta_t + \varepsilon_{st} \quad (2)$$

This means, for example, that an income-eligible Michigan school could choose to adopt CEP as early as 2012, with an intent-to-treat effect from 2012 through 2016, while a demographically similar California school could only adopt CEP as early as 2015, and thus be potentially exposed to treatment only in the 2016 CRDC data (the CRDC data were not collected in 2015). It is key to interpret these results as the impact of being eligible for CEP, rather than of participating in CEP; this likely understates any benefits of participating in the universal free meals program.[20]

To align with existing state-level estimates, we also estimate a version of equation 1 that broadens the sample to include all eligible schools and defines $CEP_{st} = 1$ for each year a school actually participates in CEP.

We estimate results for aggregate suspension rates in the sample of all elementary schools and a combined middle and high school sample, in the aggregate, and by race/ethnicity and gender subgroups. We consider these subgroups on their own for several reasons: Their baseline suspension rates differ considerably (table 2), they may be differentially "treated" by CEP to the extent that some racial/ethnic groups were more likely to have received free meals prior to CEP, and the same treatment could matter differently for these groups. Students themselves could respond differently, or teachers could respond differently to similar behavioral changes for different student groups. Although we have many reasons to suspect a nonuniform response across types of students, we are unable to distinguish among these potential mechanisms.

18. The sample of "ever-adopting" schools excludes both ineligible, low-poverty schools, and eligible schools that opt to not participate in CEP for unobserved reasons. When we broaden the sample from ever-adopting to all eligible schools, our results are smaller in magnitude but point in the same direction as the main results, consistent with CEP schools' being more disadvantaged than eligible schools that choose not to participate.
19. In nearly all specifications, this approach yields slightly more conservative standard errors than clustering at the district level.
20. An earlier version of this paper explored the feasibility of instrumenting CEP participation with year of eligibility and the share of students eligible for free meals under the traditional program. While an instrumental variable approach has the advantage of isolating variation in participation due to statutory changes in eligibility and financial incentives to participate, the nature of the outcome variable presents problems for these analyses. As discussed in Young (2017), with a skewed distribution in the dependent variable, instrumental variable results are sensitive to outlier observations and the chosen instrument. In results available upon request, although we find statutory eligibility strongly predicts actual participation in the first stage, outcome results are sensitive to the precise instrument used, the inclusion of Florida, and the point in the baseline suspension distribution where we winsorize the data.

Table 3. Effect of Community Eligibility Provision (CEP) on Out-of-School Suspensions (OSS), CEP-Participating Schools

Variables	All Students (1)	Black Male (2)	Hispanic Male (3)	White Male (4)	Black Female (5)	Hispanic Female (6)	White Female (7)
Panel A: Elementary Schools							
State eligible	−0.235 [0.269]	−0.332 [0.708]	−0.145 [0.354]	−0.976** [0.456]	−0.319 [0.344]	−0.121 [0.160]	−0.348 [0.245]
Observations	34,125	30,197	30,793	32,022	29,857	30,592	31,721
R^2	0.004	0.003	0.002	0.002	0.005	0.001	0.003
Number of schools	11,320	10,571	10,751	10,955	10,512	10,703	10,897
Base DV mean	4.503	9.966	4.622	5.651	4.720	1.788	2.177
Panel B: Middle and High Schools							
State eligible	0.227 [0.441]	−0.370 [1.050]	1.309 [0.883]	−0.053 [0.670]	0.397 [0.778]	0.550 [0.687]	−0.182 [0.427]
Observations	13,635	11,905	12,061	12,844	11,772	12,010	12,714
R^2	0.018	0.001	0.005	0.002	0.002	0.004	0.001
Number of schools	5,091	4,659	4,717	4,863	4,624	4,709	4,831
Base DV mean	11.74	20.18	13.93	12.09	14.32	8.560	7.016
School fixed effects	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y
Districts with known changes?	N	N	N	N	N	N	N

Notes: Robust standard errors clustered by state in brackets. Sample limited to elementary (panel A) and middle and high (panel B) schools that participate in CEP by 2017 and have at least 50 students and discipline rates below the 99th percentile for all schools (47). "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 2012. All specifications exclude districts with formal changes in discipline policy between 2010 and 2016: 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.

Sources: Discipline information from the U.S. Department of Education Civil Rights Data Collection; CEP participation information from state departments of education, U.S. Department of Agriculture, and Center on Budget and Policy Priorities.

**$p < 0.05$.

6. RESULTS

We present results for elementary schools (panel A of table 3) separately from the pooled sample of middle and high schools (panel B of table 3) because of their different norms around school meals and school discipline.

Elementary Results

Table 3 presents the main results, with elementary schools in panel A. Column 1 shows school-level CEP participation had a statistically insignificant effect on overall suspension rates in elementary schools, with a point estimate suggesting a 0.24 percentage point (5 percent) reduction. The subsequent columns look at race/ethnicity and gender subgroups and show that white male students experienced particularly large reductions—approximately 1 percentage point, or 17 percent. For other groups (black and Hispanic male and female students, and white female students) there is substantial overlap in the confidence intervals, and point estimates are consistent with slight reductions comparable to the aggregate estimate, but these findings are not statistically significant. For example, we are unable to rule out reductions less than 1.22 percentage points (12 percent) among black male students at 95 percent confidence.

The statistical power for black male students in these estimates is particularly limited due to large variation in suspension rates (table 2), and we cannot reject the hypothesis that CEP reduced black and white suspension rates to a similar extent. In results not shown, we cannot reject the null of unchanged black/white suspension gaps for both male and female students.

Middle and High School Results

Even after combining middle and high schools, we are only able to detect much larger changes for older students.[21] Panel B of table 3 shows no statistically significant changes in suspension rates for older students, and point estimates do not reflect a consistent pattern of increases or decreases across subgroups. As we cannot rule out reductions on the magnitude found among elementary students—or modest increases in suspensions following CEP—we limit our discussion going forward to elementary schools. We emphasize, however, that these findings do not imply that CEP "didn't work" in middle or high schools; rather, we cannot conclude anything about how it affected discipline in those schools.

Pooled Results

Online appendix table A.2 replicates table 3, pooling estimates for all grade levels and yields qualitatively similar results to the findings for elementary schools. These patterns are unsurprising as elementary schools constitute approximately 70 percent of the full sample. We caution against interpreting these pooled results as an "average" effect across types of schools, given developmentally driven differences in social pressures for students and how schools handle discipline and meals. In addition, from a statistical perspective, we gain relatively little precision in pooling all grade levels.

Heterogeneity of CEP

We next explore whether CEP eligibility had differential impact on discipline by school context. Given our limited statistical power, we consider a few characteristics sequentially in isolation, rather than how any combination of factors might affect how CEP works. In table 4, we explore whether the effects varied by whether a school was in the top or bottom half of our (relatively disadvantaged) sample's distribution of baseline FRP-eligibility rates. A priori, it is unclear whether changes in discipline are concentrated in relatively low- or high-poverty schools. As CEP expanded access to students who previously did not qualify for free meals, the effective gain in access (and largest program change) would be larger in schools with relatively *low* baseline FRP eligibility (if eligible students are equally likely to participate in free meals under either regime). If some FRP-eligible students chose not to take up the meals before CEP but did after, perhaps due to reductions in stigma, we would expect to see larger reductions in high-poverty schools. Consistent with this second hypothesis, we find across nearly all (excluding Hispanic female) demographic groups that point estimates suggest CEP eligibility reduced suspensions more in higher poverty elementary schools than lower

21. For example, for an alpha of 0.2 and beta 0.8, we would be able to detect a 0.8-1.1 percentage point (0.06 standard deviation) change for middle and high school students, compared to 0.2-0.4 percentage points (0.02 standard deviations) for elementary schools with 90 percent power.

Table 4. Heterogeneous Effects of Community Eligibility Provision (CEP), by Baseline Poverty

Variables	Black Male (1)	Hispanic Male (2)	White Male (3)	Black Female (4)	Hispanic Female (5)	White Female (6)
Panel A: Elementary Schools						
High-poverty schools	−0.456	−0.291	−1.267**	−0.419	−0.102	−0.521
	[0.754]	[0.425]	[0.572]	[0.372]	[0.177]	[0.315]
Low-poverty schools	−0.152	0.0521	−0.615*	−0.172	−0.146	−0.134
	[0.724]	[0.353]	[0.345]	[0.361]	[0.207]	[0.211]
Observations	30,197	30,793	32,022	29,857	30,592	31,721
R^2	0.003	0.002	0.002	0.005	0.001	0.004
Number of schools	10,571	10,751	10,955	10,512	10,703	10,897
Base DV mean	10.49	4.970	5.626	4.629	1.798	2.099
F-test p	0.496	0.244	0.0654	0.359	0.781	0.0820
Panel B: Middle and High Schools						
High-poverty schools	−0.614	1.563	0.369	0.590	0.737	0.148
	[1.092]	[0.990]	[0.767]	[0.931]	[0.799]	[0.616]
Low-poverty schools	−0.172	1.118	−0.360	0.242	0.408	−0.419
	[1.151]	[1.030]	[0.723]	[0.829]	[0.713]	[0.431]
Observations	11,905	12,061	12,844	11,772	12,010	12,714
R^2	0.001	0.005	0.002	0.002	0.004	0.001
Number of schools	4,659	4,717	4,863	4,624	4,709	4,831
Base DV mean	20.16	14.01	12.29	14.79	8.711	7.102
F-test p	0.580	0.661	0.285	0.673	0.597	0.337
School Fixed Effects	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y
Districts with known changes?	N	N	N	N	N	N

Notes: Robust standard errors clustered by state in brackets. Sample limited to elementary (panel A) and middle and high (panel B) schools that participate in CEP by 2017 and have at least 50 students and discipline rates below the 99th percentile for all schools (47). The variable listed in each row equals one if a school is above or below the CEP-median poverty or child food insecurity rate and is eligible to participate in year t. Baseline mean calculated as the mean of the dependent variable for 2012. All specifications exclude districts with formal changes in discipline policy between 2010 and 2016: 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.

Sources: Discipline information from the U.S. Department of Education Civil Rights Data Collection; CEP participation information from state departments of education, U.S. Department of Agriculture, and Center on Budget and Policy Priorities.

*$p < 0.10$; **$p < 0.05$.

poverty schools. The difference between these impacts is statistically significant only for white male students, at the significance level of $p = 0.07$. Estimates for the middle/high school sample are very imprecisely estimated; the point estimates do not exhibit similar patterns to those in the elementary sample, but the wide confidence intervals do not support any meaningful inference in this sample.

We explore additional dimensions of heterogeneity, focusing on the elementary sample, in online appendix table A.3. We find no statistically significant differences in effects based on school-level racial/ethnic composition (panels A and B). Panels C and D of table A.3 show that although suspensions fell slightly more in rural and non-urban schools, these differences are not significantly different from changes in urban areas.[22]

22. In additional results, we do not find any significant differences across these dimensions for older students.

Table 5. Sensitivity Analyses, White Elementary Male Students

	Robustness White Male Students			
	(1)	(2)	(3)	(4)
State eligible	−0.028 [0.324]	−0.843** [0.435]	−1.035** [0.515]	-0.939** [0.421]
Observations	64,667	34,517	31,886	32,022
R^2	0.002	0.002	0.003	0.002
Number of schools	21,788	11,869	10,819	10,955
Base DV mean	5.030	5.526	5.651	5.651
School fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Student composition	N	N	N	Y
Grade level	Elementary	Elementary	Elementary	Elementary
Sample	Eligible	Participating	Participating	Participating
Districts with known changes?	No	Yes	No	No
Weights	None	None	Enrollment	None

Notes: Robust standard errors clustered by state in brackets. Sample limited to elementary schools that participate in Community Eligibility Provision (CEP) by 2017 and have at least 50 students and discipline rates below the 99th percentile for all schools (47). "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 2012. "Student composition" includes time-varying controls for the share of students identifying as white, black, or Hispanic in a school. All specifications exclude districts with formal changes in discipline policy between 2010 and 2016: 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.

Sources: Discipline information from the U.S. Department of Education Civil Rights Data Collection; CEP participation information from state departments of education, U.S. Department of Agriculture, and Center on Budget and Policy Priorities.

**$p < 0.05$.

Sensitivity Analyses

Table 5 shows that the main results for elementary white male students in table 3 column 4 are robust to a range of minor modifications; online appendix table A.4 presents results for other elementary subgroups. Column 1 broadens the sample to include all eligible schools, regardless of actual CEP participation. Results are sharply attenuated under this approach, which more closely matches the methods of single-state studies (Fuller and Comperatore 2018; Kho 2018), and there is no longer a significant impact on white male students. This is unsurprising as eligible nonparticipating schools are advantageously selected, measured by lower baseline poverty and suspension rates (table 2). Column 2 of table A.4 excludes eligible, nonparticipating schools, but includes schools in large districts with changes in formal discipline policy in the 2010–16 period. Including these schools does not significantly change the main findings. Column 3 weights each observation by the number of students in order to obtain an estimate how CEP affected suspension activity at the student level. Results are qualitatively similar under this approach, indicating that the main results in table 4 are not driven by particularly large or small schools.

All of our results include school fixed effects to control for school characteristics and features that do not change over time. It is possible, however, that CEP coincided with changes in the school environment, or types of students attending these schools.

Table 5 column 4 shows nearly identical findings when we include time-varying controls for school racial/ethnic composition. In addition, online appendix table A.5 uses school racial composition as an independent variable and shows no significant change in the fraction of white, black, or Hispanic students at any grade level once a school became eligible to adopt CEP. Although these analyses are unable to measure all unobservable school and student characteristics, they that suggest our findings are not driven by changes in student composition.

Finally, our identification strategy leverages state-based variation in school eligibility over a four-year implementation period. Ideally, we would test the assumption that within-state discipline trends are uncorrelated with the timing of state participation in the CEP pilot by examining pre-eligibility trends across states; however, the limited CRDC sample prior to 2012, its biennial collection cycle, and the condensed timeframe of the CEP pilot, preclude such an analysis. We can, however, implement several sample modifications to inform whether our results are driven by such within-state heterogeneity.

First, online appendix table A.6 provides results for the subset of schools in the eleven pilot states. Although results are less precise, point estimates for each group of male students are larger in magnitude (more negative) under this approach, suggesting that our findings are not driven by factors coinciding with the national implementation.

Second, as our analysis period spans the 2010 through 2016 school years, our main sample includes schools that adopted up to five years after first becoming eligible (in Illinois, Kentucky, and Michigan), but only up to three years after becoming eligible in non-pilot states. In online appendix figure A.1 we show the cumulative density curves of take-up among the ever-takers (by 2017) in our sample, by years since first eligible. The figure shows that among these schools, most schools were already participating by 2015 within each wave of state eligibility. Even in states that only became eligible with the national expansion, 63 percent of the newly eligible schools in our sample opted into CEP in 2015. This is relatively close to the 72 percent of schools in our sample from the first wave of pilot states that became eligible in 2012 and were participating by 2015. Online appendix table A.7 limits the sample to schools that participated in CEP within three years of eligibility, dropping later-adopting schools within early eligibility pilot states to make the samples across states more comparable. Results under this approach are very similar to our main findings in table 3.

Relationship to Other Estimates in the Literature

Our approach leverages cross-state variation in the timing of CEP eligibility in order to avoid confounding the effect of universal meals with other observable and unobservable factors that may affect a school's decision *when* to adopt CEP. In contrast, other work evaluates how CEP affected discipline within a single state using variation in program *participation* among eligible schools. In general, our findings are consistent with these other studies, despite different empirical approaches and slightly different discipline definitions. For example, Kho (2018) estimates that CEP reduced the fraction of Tennessee students suspended or expelled in a given year by 10 percent (1.2 percentage points) across all grade levels. While we cannot examine the identical metric in the CRDC data, we find a statistically insignificant reduction of 5 percent across all elementary students (table 3 column 1), and cannot rule out reductions of the magnitude

found in Tennessee.[23] Second, Fuller and Comperatore (2018) compare outcomes in North Carolina for individual students in CEP-participating schools with students in schools that were eligible but chose not to participate. They find some improvements in achievement measures but no effects on the number of suspensions for elementary, middle, or high schoolers, 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 column 1 of table 5 and columns 1, 5, and 9 of online appendix table A.5. As discussed earlier, these patterns are consistent with the most disadvantaged schools opting into CEP.

7. CONCLUSION

The CEP dramatically expanded access to universal free meals. In 2017, approximately 20 percent of students attended a CEP school, a 45 percent increase from the first year of national implementation (Hewins, Rosso, and Maurice 2017). A consensus is emerging from the nascent literature that universal free school meals improve student achievement and other outcomes. This paper is the first to look beyond a single state to show that the benefits of CEP extend to modest reductions in suspension rates at the national level. As such, our findings complement single-state studies by comparing schools that were able to adopt CEP sooner versus later due to federal regulation rather than local decisions.

Research on CEP, and many education policies, face a tradeoff between using student-level administrative data in single-state studies and aggregated data in national studies. The single-state studies predominate in the literature, but whereas these works can incorporate a relatively rich set of control and outcome variables, they are unable to eliminate unobserved local variation in what makes some eligible schools ultimately participate in CEP. The national approach we use in this paper, like Ruffini (2018) uses to study academic impacts of CEP, has the benefit of the staggered state pilot timing and more convincing causal inference but is unable to precisely inform which students incur the greatest benefits or costs, apart from aggregated measures. Going forward, the identification the national strategy provides is useful only during the pilot period and is less applicable for studying medium- or long-term effects over a longer horizon. We view these two lines of research as important complements. From a federal policy perspective, it is useful to understand the national scope of any benefits. However, the more local research projects are critical for state and local policy makers seeking to understand and improve the impact of federal programs in their specific contexts.

We find that CEP led to a sizable and robust reduction in suspensions for white, male elementary students but lack the statistical precision to reject moderate impacts for many other groups and grade spans. We are reluctant to speculate as to why this is the case, given our inability to statistically distinguish across estimates. The kinds of conditions and mechanisms that could contribute to differences in effects across demographics and grade spans are ripe for future research: the role of baseline suspension rates (relatively higher for black male students), initial free-meals participation rates,

23. The CRDC separately reports the counts of students: (1) with one OSS; (2) with more than one OSS; and (3) expelled. It is not possible to identify overlap between individual students suspended and expelled.

school-level differences (likely to vary across grade span as well as local context) in how meals are served, and school-level differences in disciplinary practice, including how changes in behavior translate to changes in disciplinary outcomes across different demographic groups.

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