# Food Assistance Timing and School Discipline: Evidence from SNAP and Universal Free Meals in Oregon

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Liliane Nienstedt
Wayne State University

Leah Clark[1]
U.S. Census Bureau

**Abstract**

Does cyclical food insecurity caused by the timing of nutritional support payments influence student educational outcomes, and does the provision of free meals at school offer a protective buffer to these cyclical effects? In this paper, we use administrative education data from Oregon linked to records of SNAP enrollment housed at the U.S. Census Bureau to examine the impact of the timing of SNAP receipt on student discipline, as well as the impact of the rollout of universal free meals on this relationship. We employ a difference-in-differences model with student fixed effects to examine whether suspension rates vary systematically over the SNAP benefits cycle. We find that middle- and high-school students on SNAP experience 0.15 additional daily suspensions per 1,000 students later in the SNAP cycle than earlier in the cycle, compared with students not on SNAP. These findings are statistically significant and hold across demographic and income subgroups. Finally, using triple- and quadruple-difference designs that exploit variation in school-level adoption of the Community Eligibility Program, we show that universal free meals reduce suspensions for students on SNAP, but do not attenuate the impact of the SNAP benefits cycle.

*JEL Classification: I2, I3*

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# I. Introduction

The U.S.'s largest anti-hunger policies—the Supplemental Nutrition Assistance Program (SNAP) and the National School Lunch Program (NSLP)—improve child outcomes, from test scores and school attendance to later-life health and economic self-sufficiency (Bailey et al. 2024; Bitler 2015; Hinrichs 2010; Hoynes et al. 2016). However, families on SNAP experience intra-monthly cycles in food expenditures, dietary quality, and food insecurity as a majority spend down their monthly benefits within two weeks (Schanzenbach 2023; USDA 2020). For families, these cycles have been shown to affect incidences of domestic violence and crime, increase ER visits, and worsen disease management (Carr and Packham 2019, Carr and Packham 2021, Cotti et al. 2020; Ojinnaka and Heflin 2018, Heflin et al. 2019; Kinsey et al. 2019). At school, nutritional supports may supplement families' food shortages at home. Students on SNAP with access to free meals bear less of the consumption-cycle burden than adults in their household, and their take-up of school lunch and breakfast increases toward the end of the SNAP month (Kuhn 2018; Laurito and Schwartz 2019). Yet, school meals provide an incomplete cushion: late in the benefits month, students on SNAP receive increased disciplinary infractions and score worse on standardized tests (Gennetian et al. 2016; Bond et al. 2022; Cotti et al. 2018; Gassman-Pines and Bellows 2015).

Can the expansion of nutritional supports in schools mediate the negative consequences of the SNAP benefits cycle? In recent years, the Community Eligibility Provision (CEP) of the NSLP has expanded access to and take-up of free meals in schools. Under CEP, eligible schools offer free breakfast and lunch to all students, regardless of family resources. This has been shown to increase meal uptake (Ruffini, 2022; Schwartz and Rothbart, 2020; Leos-Urbel et al., 2013) and improve educational outcomes, even among students previously eligible for free meals (Domina et al., 2024; Gordanier et al., 2020). Studies across different states show CEP reduces suspensions (Kho and Hunter, 2026; Gordon and Ruffini, 2021), a particularly important outcome for students on SNAP who comprise the majority of students receiving suspensions. In this paper, we conduct the first analysis of the combined impact of CEP and the benefits cycle on suspensions for students on SNAP.

While the educational impacts of SNAP and school meals are well-documented, our study benefits from a first-of-its-kind data linkage bringing student-level monthly indicators of SNAP receipt together with student-level K-12 administrative data for the state of Oregon over a full decade. We further link these records to family income derived from annual tax returns, allowing us to compare SNAP recipients with other low-income students. With data on student suspensions on a daily basis, and a panel that covers the five years before and five years after the introduction of CEP in Oregon, we are uniquely positioned to document the separate and combined impacts of the SNAP benefits cycle and universal free meals on suspensions.

We first investigate whether students on SNAP experience increases in suspensions late in the benefit month as family resources dwindle. We use a difference-in-differences (DiD) design with student-by-month fixed effects to compare students' outcomes late in their benefits cycle to their own outcomes earlier in the same month. This analysis builds on Gennetian et al. (2016) to estimate the impact of the SNAP cycle on suspension with the addition of student-by-month fixed effects and in a larger setting over many years. In Oregon, the first nine days of the month represent the period during which households receive SNAP benefits, while the remaining calendar days reflect periods when families are increasingly spending down their benefits. We do not observe the exact day of payment, so we compare outcomes on school days towards the end of the calendar month to those in the reference week spanning days 10-16—just after benefits receipt. The DiD approach allows us to account for time-varying factors within-month that are associated with discipline receipt, as long as they are not separately correlated with SNAP receipt.

Second, we estimate the impact of universal free meal expansion on annual suspension rates for students on SNAP compared to non-SNAP students using a triple-difference specification. We compare students—before and after the introduction of CEP—in schools that started CEP in the program's first year to those in eligible schools that never took up CEP in our analysis period. In the first year, more than one-half of Oregon schools were eligible for CEP, and over one-third of eligible schools took up the program (Center on Budget Priorities, 2015). We can identify the effect of CEP on SNAP students in take-up schools under the assumption that their suspension outcomes relative to non-SNAP students would have evolved in parallel to their counterparts in never-take-up schools if not for CEP.

Finally, we combine these two approaches to investigate whether CEP attenuates the impact of the SNAP benefits cycle on suspension. In a quadruple-difference design, we compare students who do and do not receive SNAP, in eligible schools that do and do not take up CEP, before and after the roll-out of CEP, and early and late in the monthly SNAP benefits cycle. To build intuition for our result, we separately run the initial DiD model for our treatment and control schools in the pre- and post-treatment periods.

Students on SNAP comprise approximately two-fifths of the student population, and have more than double the rate of suspension compared to non-SNAP students. Notably, we find that low-income students not on SNAP have substantially lower rates of suspension than students on SNAP. Students on SNAP are categorically enrolled in free meals under direct certification rules, and evidence suggests CEP's impact might be largest for low-income students not receiving other benefits (Gordanier et al. 2020). For CEP to plausibly mitigate the effects of the benefits cycle, it should drive down suspensions for students on SNAP broadly.

We find that students on SNAP experience increases in suspensions rates late in the month, consistent with the timing of families' benefit depletion. In the five years prior to CEP roll-out,

middle and high school students on SNAP experience 0.15 additional daily suspensions per 1,000 students in the second half of the month relative to non-recipients. This effect is statistically significant, and represents an increase of 7% relative to their reference period suspension rate. These effects are similar for in-school and out-of-school suspension outcomes, and are robust to restricting the control group to low-income students not enrolled in SNAP. Both female and male students experience the SNAP benefits cycle; the effect is larger for male students, but proportionally so relative to baseline differences in suspension rates. Similarly, effects are smaller in magnitude on lower baseline suspension rates for elementary school students, but still statistically significant.

Turning to CEP, we find that students on SNAP in schools that adopt universal free lunch see their suspension rates decline considerably following adoption. Declines in suspension for students on SNAP under CEP are nearly double the decline for students not on SNAP. After CEP adoption, students on SNAP experience a 2.5 percentage point reduction in the annual probability of being suspended. However, we do not find that CEP reduces the within-month disciplinary fluctuations associated with the SNAP benefit cycle. Our quadruple-difference model yields a null effect. SNAP benefits cycles are persistent across subgroups and over time, and even persist following a policy change that brings suspension rates down considerably for SNAP students.

This paper contributes to the literature in three ways. First, we provide the first analysis of how both SNAP benefit cycles and expanded in-school nutritional supports shape cyclical patterns in student discipline. Second, we extend the existing literature on the suspension effects of the SNAP benefits cycle and CEP expansion separately. Gennetian et al. (2016) is the only paper we identified that documents the effect of the benefits cycle on student discipline, and our expansive data allow us to build on that study—which focuses on a single district for a single year—with stronger identification and a larger geographic and temporal reach. Third, we are the first paper to document the effect of CEP on suspensions for students on SNAP. Gordanier et al. (2020) uses K-12 administrative records with SNAP indicators to document the effects of CEP but does not observe suspension outcomes. Domina et al. (2024) use a linked dataset for Oregon similar to ours, but does not isolate effects for students on SNAP.

It is so unusual to link SNAP enrollment records to K-12 administrative data that we are one of the first, to our knowledge, to document disproportionate rates of suspension for students on SNAP. Many prior studies document disproportionate suspension rates for FRPL-eligible students, but FRPL is an imprecise proxy for SNAP participation, in part because the eligibility threshold is higher. Our finding that low-income students not enrolled in SNAP have much lower suspension rates suggests that existing studies that use proxies for SNAP participation might conflate more advantaged students with students on SNAP.

The rest of the paper is organized as follows: Section 2 reviews the prior literature; Section 3 describes the data, and Section 4 details the analytic approach; Section 5 presents the main results,

followed by Section 6, which discusses limitations, and Section 7 concludes with a discussion of the study's contributions to the literature.

## II. Prior Literature

### The Importance of SNAP

There is a large body of evidence that shows that SNAP receipt increases food spending and has long-run health and economic consequences for recipients. It is challenging to establish a causal relationship between SNAP receipt and child outcomes because many unobservable factors are associated with both SNAP receipt and participants' health and economic circumstances. To address selection bias, many studies exploit variation in the date of the county-level original implementation of the Food Stamp Program in the 1960s and 1970s to provide quasi-experimental evidence of the program's causal impact on recipients' food expenditure and well-being. Hoynes and Schanzenbach (2009) found that the introduction of food stamps led to an increase in overall food expenditures, thus showing the efficacy of the program on increasing food consumption for recipients. Hoynes et al. (2016) find that food stamp access in childhood led to a significant reduction in obesity, high blood pressure, and diabetes. In a separate paper, the same authors find that pregnancies exposed to the Food Stamp Program roll-out yielded deliveries with significantly higher birth weights and reduced neonatal mortality (Almond et al. 2011). The likely role of SNAP in reducing food insecurity and improving health is replicated in modern studies (Gregory and Deb 2015; Swann 2017).

There is a much smaller body of work that examines how, and to what extent, SNAP receipt improves educational outcomes. Hoynes et al. (2016) find that food stamp access in childhood leads to an increase in economic self-sufficiency (defined as increases in educational attainment, earnings, income, and decreases in welfare participation) for exposed females; their findings indicated that SNAP participation increases the chances of graduating from high school by as much as 18 percentage points. Similarly, Bailey et al. (2024) find that children who had access to the program before age 5 had, among other positive impacts, higher adult human capital, were more likely to live in higher-quality neighborhoods, lived longer, and were less likely to be incarcerated.

A handful of non-causal studies have examined the relationships among SNAP participation, educational attainment, and later-life economic outcomes. Frongillo et al. (2006) find that children who started participating in SNAP between kindergarten and third grade have a 3-point greater improvement in reading and mathematics scores than those who stopped program participation during that period. Importantly, this research uses a fixed-effects model to minimize bias but provides no plausibly exogenous change to program participation. Bronchetti et al. (2019) use variation in benefit amounts and purchasing power across states to estimate how SNAP benefit

levels affect children and find that a 10% increase in purchasing power reduces missed school days by 22% relative to the mean.

### The Importance of SNAP Recency

There is growing evidence that the monthly nature of SNAP benefit receipt creates food hardship toward the end of the benefit month. Research suggests that there are large SNAP-induced cycles in food expenditures, dietary quality, and food insecurity (Schanzenbach 2023; Gassman-Pines and Schenck-Fontaine 2019; Gregory and Smith 2019; Hamrick and Andrews 2016; Lee et al. 2022; Whiteman et al. 2018). Gregory and Smith (2019) uses data from the 2012-2013 National Household Food Acquisition and Purchase Survey to show that the probability of being classified as food insecure increased by 11 percentage points near the end of the benefit month for surveyed SNAP recipients. Similarly, Hamrick and Andrews (2016) uses data from the 2006-08 American Time Use Survey to show that SNAP participants were more likely than nonparticipants to report a day without eating at the end of the benefit issuance cycle.

Cyclical food consumption and increased food insecurity throughout the month have been shown to negatively affect children's test scores. Work by Cotti et al. (2018) using state administrative data from South Carolina shows that students are more likely to perform worse on standardized math tests if the test dates fall later in the benefit month. Using data from North Carolina, Gassman-Pines and Bellows (2015) find that the relationship between student end-of-grade achievement test scores and SNAP receipt appears to be curvilinear, with test scores peaking by 0.021 to 0.022 standard deviations 17 to 19 days after benefit issuance, which they interpreted as the delayed effect of reduced household stress and improved nutrition. Bond et al. (2022) use national data from a college admission exam to show that students taking the exam later in the benefit month have lower test scores and a lower probability of attending a four-year college. Specifically, they make use of the fact that seven states and Washington, DC use the first letter of a family's surname to determine the monthly disbursement date. Using individual-level data on test scores and college attendance, they find that low-income students who took the test two weeks after their SNAP issuance date scored 0.06 standard deviations lower than those who took the test in the two weeks following disbursement.

The cyclical nature of food insecurity induced by SNAP benefit depletion may also impact children's behavior at school due to both increases in family stress and increases in child hunger. Alongside evidence that the SNAP cycle increases food insecurity, it has been documented that reductions in net family income at the end of benefit cycles contribute to family stress and disruption of routines (Edin et al. 2013). Only one paper has examined the impact of SNAP timing on non-test score outcomes. Gennetian et al. (2016) use Chicago Public Schools administrative data from the 2005-06 school year to show that the within-month difference in disciplinary referrals for

students in SNAP families was 7 percentage points larger than for nonrecipients.

### The Protective Effect of Free School Lunches and the Community Eligibility Provision

It is possible that access to free school meals may serve as a buffer to cyclical food insecurity for children. Federal school breakfast and lunch programs provided free and low-cost meals to forty percent of US school children in 2014 (Hoynes and Schanzenbach 2016). After SNAP, the NSLP is the largest antihunger program in the nation (Kinsey et al. 2020). Work that focuses on access to school meals indicates that the provision of school meals is associated with decreased food insecurity (Bartfeld and Kim 2010; Bhattacharya et al. 2006; Fletcher and Frisvold 2017; Huang and Barnidge 2016; Schanzenbach and Zaki 2014), improved academic outcomes (Andreyeva and Sun 2021; Gordon and Ruffini 2021; Hecht 2020; Hinrichs 2010; Murphy et al. 1998; Ruffini 2022), and physical health outcomes (Davis and Musaddiq 2019; Gundersen et al. 2012; Murphy et al. 1998).

In the last decade, the federal government has significantly expanded the reach of the NSLP through the Community Eligibility Provision (CEP), which was implemented nationally in 2014 as part of the Healthy Hunger-Free Kids Act.[1] Under the provision, schools with an Identified Student Percentage (ISP)[2] of 40 percent or more are eligible to provide free lunch and breakfast to all students. Districts can also enroll district-wide if the average ISP across all schools in the district exceeds 40 percent (Davis and Musaddiq 2019). There is a large body of literature investigating the impact of CEP on educational outcomes (Andreyeva and Sun 2021; Gordanier et al. 2020; Ruffini 2022). Focusing on disciplinary outcomes, Gordon and Ruffini (2021) find limited declines in suspensions for white male elementary students using differential timing in state eligibility for their identifying variation. Subsequent studies using DiD models exploiting variation in take-up across schools and state-wide student-level administrative data find broader reductions in suspensions and expulsions (Domina et al. 2024; Kho 2018).

Only Laurito and Schwartz (2019) and Kuhn (2018) have examined the impact of school meals on cyclical food insecurity over the SNAP benefit month. Kuhn finds that during the school year, students with access to free school meals bear less of the burden of the consumption cycle than adults in their household, while Laurito and Schwartz find that SNAP households rely more on school lunch and breakfast programs toward the end of the SNAP month. To date, no research has examined the impact of free school meals on students' educational outcomes across the benefit

[1] CEP was piloted prior to 2014 in 11 states: Illinois, Kentucky, and Michigan piloted the program in 2011; New York, Ohio, West Virginia, and the District of Columbia started in 2012; Florida, Georgia, Maryland and Massachusetts were added in 2013.

[2] Identified students are students whose families automatically qualify for free school meals through participation in government assistance programs, including Medicaid, Head Start, SNAP or other special criteria. Districts can also enroll district-wide if the average ISP of all schools in the district falls above 40 percent (Davis and Musaddiq 2019).

cycle; we aim to fill this gap with our study.

## III. Data

We make use of a unique dataset: student-level educational administrative data from the Oregon Department of Education (ODE) linked with confidential microdata stored at the Census Bureau, which includes monthly SNAP records from the state of Oregon and annual tax return records from the IRS. These data include all students enrolled in public K-12 schools in Oregon from the 2009-2010 school year to the 2018-2019 school year. For each student, we observe suspension incidences by date, whether a student's household receives SNAP in each month, and the income for the tax unit that claims them as a dependent (if claimed). To facilitate linkage, we rely on Protected Identification Keys (PIKs) that are assigned to each data source using the Census Bureau's enterprise linkage approach, the Person Identification Validation System (for more information, see Wagner and Layne (2014)).

### SNAP receipt and family income

Oregon SNAP records include PIKs for all household members, enabling us to link student PIKs to their household's SNAP records. These records are on a monthly basis, and we designate students as receiving SNAP only in months their household receives non-zero benefits. We construct student-by-month-of-year SNAP participation flags, but note there is little within-school-year variation in SNAP enrollment. The vast majority (87.2%) of students enrolled in SNAP at any point in the school year are enrolled for all months of the school year.

To characterize family income independent of SNAP participation, we link students to IRS Form 1040 records in which they are claimed as dependents. We construct an income-to-poverty ratio for each student using adjusted gross income and a proxy for household size based on the numbers of filers and dependents in the corresponding tax unit. We align this calendar-year based measure with the school year beginning in that calendar year (e.g., income reported on 2014 tax returns—filed in spring 2015—is used for the 2014–15 school year).

SNAP eligibility can vary month to month while IRS income is measured annually. IRS records do not capture all resources or deductions that are part of SNAP eligibility determinations, and not all households file taxes—including a nontrivial share of SNAP recipient households (Clark and Bhaskar, 2025). Thus, the Form 1040-derived income-to-poverty ratio should be interpreted as an alternative measure of underlying economic disadvantage instead of an exact measure of SNAP eligibility.

### Student-day dataset

To examine variability in daily outcomes, we construct a student-day dataset in which each student has an observation for each school day. This relies on school enrollment spell start and end dates available in the ODE data. We allow students to be enrolled in different schools in different months of the same school year, but we exclude the calendar months in which students switch schools. We make this exclusion because SNAP benefits and their corresponding cycles occur monthly, and a student switching mid-month into a new school would potentially confound the benefits cycle effect (e.g., the new school may have different disciplinary practices, there may be a corresponding family income shock). However, we do not completely drop these students from the data, since students who are suspended are more likely to switch schools or face expulsion, and we do not want to condition our sample on a factor correlated with our key outcome (Welsh 2017; Welsh 2018).

Using date of suspension incidents, we derive an indicator variable for suspension at the student-day level. Some K-12 administrative data sources report absences in a similar style, but ours does not, and, thus, suspension is the only regularly-occurring daily outcome we can measure. We use this dataset for all analyses examining the monthly benefits cycle.

### Student-year dataset

When we examine whether the suspension-reducing effects of CEP extend to the SNAP recipient population, we aggregate the student-day dataset to the annual level. Here, we retain student-year records only for students enrolled in the same school for at least 90 school days (to ensure students spend the majority of the year in the associated school) and exclude suspensions received at other schools. This enables us to designate students as enrolled in CEP schools on an annual basis and avoids conflating suspensions received at other schools, which may have different CEP eligibility and take-up.

### Outcomes

Suspensions are defined as in- and out-of-school suspensions. We employ two approaches to measuring suspension rates. Our primary approach is to define the suspension rate as the average number of suspensions per 1,000 students per day. As shown in Table 1, middle and high schoolers average 1.09 suspensions per 1,000 students each day, and elementary schoolers average 0.28. We additionally construct an indicator for whether a student receives one or more suspensions in a school year. In our sample, 9% of middle and high schoolers are suspended at some point each year, and 3% of elementary schoolers (see Table 1). The annual 'ever suspended' measure is more commonly used in the literature so we use it to facilitate comparison between the baselines sus-

pension rates we document and in extending existing findings on the suspension-reducing effects of CEP. When we analyze the monthly benefits cycle, we require a more fine-grained measure that can accommodate aggregation across varying lengths of time.

In Table 1, we document baseline suspension rates across four mutually exclusive groups: (1) students on SNAP; (2) students not on SNAP who are low income (defined as those whose 1040-derived household income-to-poverty ratio is less than 1.85, indicating likely eligibility for free or reduced-price meals); (3) students not on SNAP who are middle- or high-income (defined as those whose income-to-poverty ratio is greater than or equal to 1.85); and (4) students not on SNAP and not claimed on income tax returns. As shown in the first column of Table 1, 36 percent of students in our sample receive SNAP, while an additional 11 percent are low-income according to tax return records but do not receive SNAP. We do not observe SNAP receipt or tax records for 7 percent of students. These students are excluded from our subsequent analyses.[3]

## IV. Methodology

To investigate whether expanded nutritional supports in schools can moderate the impact of dwindling supports at home, we separately analyze the impact on suspensions of (a) the SNAP benefits cycle and (b) the introduction of universal free lunch via CEP, before combining these two approaches. For clarity, we articulate each of these three steps as separate research questions below and outline the methodology used to answer each question.

### RQ 1: What is the effect of the SNAP benefits cycle on suspension rates?

SNAP benefit cycles provide a natural experiment for studying the effects of food insecurity on disciplinary outcomes for welfare recipients, since we know when households receive their benefits and when they are likely running low. In Oregon, SNAP participating households receive electronic food benefits on a staggered monthly schedule determined by the last digit of the primary recipient's social security number. Those with numbers ending with zero or one receive benefits on the first day of the month, those with numbers ending with two receive benefits on the second day of the month, and so on, until the ninth day of the month.[4] Benefits are delivered via electronic benefit transfer cards, which can be used at authorized SNAP retailers, including most large grocery store chains. This predictable benefit schedule provides a natural experiment for studying the

[3]This may introduce bias into our results, though prior research suggests these students are not particularly economically disadvantaged (Clark and Bhaskar, 2025). Additionally, suspension rates for this group are similar to those of low-income students who do not receive SNAP. This suggests that these students do not appear to be systematically more disadvantaged in terms of observed outcomes.

[4]Parents without SSNs receiving benefits on behalf of their children also receive benefits on the first day of the month.

**Table 1.** Suspension Rates in Analytic Data, 2010–2019

| | | Average Daily Suspensions per 1000 Students | | | Proportion Suspended Annually | | |
|---|---|---|---|---|---|---|---|
| | Share, All Years | All Years | 2010-2014 | 2015-2019 | All Years | 2010-2014 | 2015-2019 |
| **Grades 6-12** | | 1.09 | 1.28 | 0.92 | 0.09 | 0.10 | 0.08 |
| Female | 0.48 | 0.60 | 0.71 | 0.49 | 0.05 | 0.06 | 0.05 |
| Male | 0.52 | 1.56 | 1.81 | 1.32 | 0.12 | 0.14 | 0.10 |
| Non-SNAP, all Incomes | 0.64 | 0.68 | 0.81 | 0.56 | 0.06 | 0.07 | 0.05 |
| Non-SNAP, Middle/High Income[a] | 0.46 | 0.54 | 0.64 | 0.45 | 0.05 | 0.06 | 0.04 |
| Non-SNAP, Low Income[b] | 0.11 | 1.05 | 1.20 | 0.89 | 0.08 | 0.09 | 0.07 |
| Non-SNAP, Income Unknown[c] | 0.07 | 1.05 | 1.22 | 0.87 | 0.09 | 0.10 | 0.07 |
| On SNAP | 0.36 | 1.81 | 2.10 | 1.53 | 0.13 | 0.15 | 0.12 |
| *N* | 3,019,000 | | | | | | |
| **Grades K-5** | | 0.28 | 0.30 | 0.27 | 0.03 | 0.03 | 0.02 |
| Female | 0.49 | 0.08 | 0.09 | 0.08 | 0.01 | 0.01 | 0.01 |
| Male | 0.51 | 0.47 | 0.50 | 0.45 | 0.04 | 0.05 | 0.04 |
| Non-SNAP, all Incomes | 0.57 | 0.16 | 0.17 | 0.16 | 0.02 | 0.02 | 0.01 |
| Non-SNAP, Middle/High Income | 0.42 | 0.13 | 0.14 | 0.12 | 0.01 | 0.01 | 0.01 |
| Non-SNAP, Low Income | 0.11 | 0.24 | 0.24 | 0.24 | 0.02 | 0.02 | 0.02 |
| Non-SNAP, Income Unknown | 0.05 | 0.28 | 0.29 | 0.28 | 0.03 | 0.03 | 0.02 |
| On SNAP | 0.43 | 0.44 | 0.45 | 0.43 | 0.04 | 0.04 | 0.04 |
| *N* | 2,497,500 | | | | | | |

*Note:* Observation counts (*N*s) rounded according to the Census Bureau's rounding rules. Summary statistics for the analytic student-year file, using all grades K-12. Student-year observations are retained only for students enrolled in the same school for $\geq$90 days. Suspensions from other schools are excluded. Suspensions in June and July are excluded.
[a] Middle/high income students, defined as student's whose household income-to-poverty ratio is greater than or equal to 1.85.
[b] Low income is defined as student's whose household 1040-derived ratio of income-to-poverty is less than 1.85.
[c] Students not claimed as dependents.

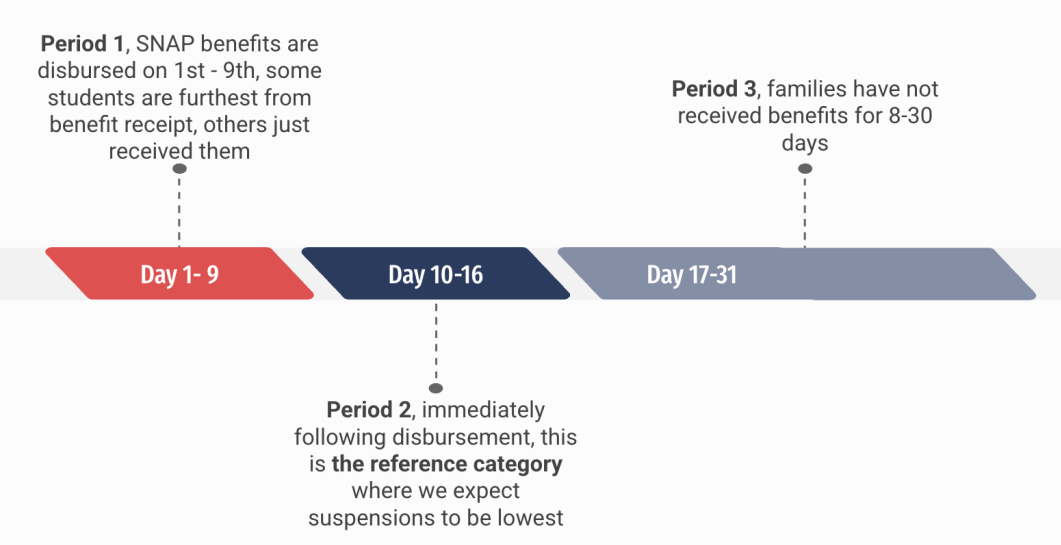



**Figure 1.** Oregon SNAP Disbursement Schedule and Identification Strategy

effects of food insecurity on student disciplinary outcomes because it offers exogenous variation in household food access over the course of the month.

To estimate the effect of the SNAP benefits cycle on suspension, we exploit the fact that students in SNAP families receive benefits in the first 9 days of the month. Although we do not directly observe each family's SNAP benefit disbursement date, Oregon's predictable disbursement schedule allows us to approximate families' relative positions within the SNAP benefits cycle (see Figure 1). By calendar day 10 of each month, all participating families have received their benefits. By the end of the month, families are between 19 and 30 days away from receiving benefits, depending on the month's length and their disbursement day. During the first nine days of the month, families are either at the furthest point from receiving benefits or have very recently received them. Since more families receive benefits on the first of the month relative to subsequent days, the median SNAP recipient likely has their benefits by calendar day 4.

We hypothesize that as families spend down their SNAP benefits throughout the month, students' externalizing behaviors increase, thereby increasing the likelihood of school suspension. To test this, we split the calendar days of each month into three periods:

- **Period 1 (calendar days 1–9):** when families receive SNAP benefits.
- **Period 2 (calendar days 10–16):** when all families have received SNAP benefits and are relatively close to disbursement.
- **Period 3 (calendar days 17–31):** when families are likely to be spending down remaining benefits.

Each period contains at least seven days, ensuring we cover a full school week. Our primary hypothesis is that suspension rates will increase in Period 3 relative to Period 2 for students on SNAP compared with those not on SNAP. We cannot offer a directional hypothesis for Period 1, as it spans both the period immediately before and the period immediately after benefit receipt. Period 2 roughly corresponds to the second week of the benefits cycle, while Period 3 corresponds to weeks three and four. Existing literature shows that behavioral responses to the SNAP benefits cycle typically appear in week three and persist into week four (Bond et al. 2022; Cotti et al. 2018; Gassman-Pines and Bellows 2015; Gassman-Pines and Schenck-Fontaine 2019; Whiteman et al. 2018).

To identify the causal effect of SNAP benefit timing on suspensions, we estimate models with student-by-month-of-year fixed effects. This means that we compare each student's Period 3 suspension probability to their own Period 2 suspension probability in the same month of the year. This allows us to test whether students receiving SNAP experience larger within-month increases in suspension propensity than students not on SNAP.

We estimate the following difference-in-differences model:

$$\begin{aligned} susp_{idm(y)} = \beta_0 + \beta_1 PERIOD3_d SNAP_{im(y)} + \beta_2 PERIOD1_d SNAP_{im(y)} \\ + \beta_3 PERIOD3_d + \beta_4 PERIOD1_d + \theta_{im(y)} + \varepsilon_{idm(y)} \end{aligned} \tag{1}$$

where $susp_{idm(y)}$ is an indicator of whether student $i$ is newly suspended on day $d$ of month-of-year $m(y)$.[5] $PERIOD3_d$ and $PERIOD1_d$ are indicators for whether day $d$ falls in Period 3 or Period 1, respectively, with Period 2 serving as the omitted reference category. $SNAP_{im(y)}$ is an indicator of whether student $i$ receives SNAP benefits in month-of-year $m(y)$. Student-by-month-by-year fixed effects, $\theta_{im(y)}$, absorb all student characteristics that do not vary within month and month-specific differences in suspension propensity (e.g., suspension is rarer in September than October). Since, by construction, there is no variation in schools attended within the month, it subsumes school fixed effects; likewise for the main effect of $SNAP_{im(y)}$.

Our outcome is an indicator for whether a student is suspended on a given day. For ease of interpretation, we multiply the suspension indicator by 1000. This results in estimates that can be interpreted as the average daily suspension rates per 1,000 students as our primary outcome. Using the daily suspension rate allows us to avoid bias due to the variation in the number of school days in each period (in contrast to measures based on whether a student is ever suspended in each period).

Students cannot be suspended during school breaks, so we exclude months with major school breaks from our analysis. These breaks—in November (Thanksgiving), December (winter break),

[5]The binary variable is set to 1 on the first day of a suspension. Suspensions may be for multiple days, but subsequent days are not set to 1. This ensures we capture new suspensions.

and March (spring break)—all fall during Period 3, meaning students are out of school when their families' SNAP balances are more likely to be low. Additionally, most schools are out of session for most of June, July, and August. Our primary analysis, therefore, focuses on September, October, January, February, April, and May. As a robustness check, we run the Model 1 separately for each calendar month.

The coefficient of interest, $\beta_1$, measures the difference in suspension rates between Period 3 and Period 2 for students on SNAP, net of the same difference for students not on SNAP. The coefficient $\beta_2$ on $PERIOD1_d * SNAP_{im(y)}$ has an analogous interpretation, but should not be thought of as occurring prior to treatment, because Period 1 contains both pre- and post-benefit receipt days for SNAP recipients, depending on their disbursement date.

Identifying variation comes from within-month changes in suspension rates for students receiving SNAP relative to those not receiving SNAP. For these estimates to be interpreted causally, there must be no omitted variables that vary within a student-by-month-of-year that are correlated with both SNAP receipt and a student's suspension likelihood. Such bias would arise from student- or school-level factors correlated with SNAP receipt that vary over the course of a month and affect a student's likelihood of being suspended. One potential concern is that lower-income workers, such as the parents of students on SNAP, might be more likely to be paid only once a month, leading to economic constraints at the end of the month unrelated to SNAP benefits. To address this concern, we omit middle- and high-income students and re-run our analysis with our comparison group comprised of other low-income students whose families may face similar non-SNAP-based cashflow pressures.

**RQ 2: Do the established benefits of expanded free lunch on low-income students extend to students on SNAP?**

Next, we extend the literature on the effects of CEP on suspension by examining whether CEP specifically reduces suspensions for students receiving SNAP. Domina et al. (2024) find that low-income students in Oregon experience large reductions in suspensions following the introduction of CEP. This low-income group includes students on SNAP, as well as students not receiving SNAP who are deemed likely eligible for free or reduced-price lunch based on tax-reported household income. Here, we investigate whether these effects extend to the SNAP-recipient majority of low-income students who are categorically eligible for free meals prior to the roll-out of CEP under direct certification rules. Gordanier et al. 2020—studying non-suspension educational outcomes—find the benefits of CEP concentrate on low-income students not receiving SNAP, so it is possible the effect shown in Domina et al. (2024) is driven by students not on SNAP.

To estimate the effect of CEP for students on SNAP, we use a triple-difference design that compares suspension rates for (i) students on SNAP to those not receiving SNAP, (ii) across schools

that adopted CEP in 2015 versus eligible schools that did not adopt CEP, and (iii) before and after CEP implementation. We restrict the sample to schools deemed eligible for CEP when the program began in 2015, and compare schools that took up CEP immediately to eligible schools who never take up CEP during our analysis period. We estimate the following model:

$$\begin{aligned} susp_{isy} = &\alpha_0 + \alpha_1 POST_y CEP_s SNAP_{iy} + \alpha_2 POST_y CEP_s + \alpha_3 CEP_s SNAP_{iy} \\ &+ \alpha_4 POST_y SNAP_{iy} + \alpha_5 SNAP_{iy} + \alpha_6 POST_y + \gamma_s + \varepsilon_{isy} \end{aligned} \tag{2}$$

Because CEP varies at the school-year level, Model 2 is estimated using annual suspension rates, whereas Model 1 use daily suspension rates. Here $susp_{isy}$ is a binary indicating whether student $i$ at school $s$ receives a suspension in year $y$. $POST_y$ is an indicator for the school years 2015-2019, corresponding to the years CEP is available. $CEP_s$ indicates whether school $s$ enrolled in CEP in 2015, and $SNAP_{iy}$ indicates whether student $i$ received SNAP benefits at any point in school year $y$. $\gamma_s$ denotes school fixed effects, which account for time-invariant differences across schools in suspension propensities.

The main coefficient of interest is $\alpha_1$, which captures the effect of CEP on suspensions rates for students on SNAP relative to those not on SNAP. Our identifying assumption requires that, in the absence of CEP, differences in suspension rates for students on SNAP and non-recipient students would have evolved similarly across schools that adopted CEP and those that did not.

We use a similar approach to that of Domina et al. (2024) but restrict the sample to schools that were eligible for CEP in 2015 and either adopted CEP in 2015 or never adopted CEP between 2015 and 2019. We restrict our sample in this way to keep our specification simple, since we next combine it with the benefits cycle approach and we want to avoid the complexity of staggered adoption timing. The resulting sample includes about 150 middle and high schools that adopted CEP in 2015, as well as approximately 200 middle and high schools that never adopted CEP despite being eligible. We exclude schools that were not eligible for CEP in 2015, as well as schools that were eligible but adopted CEP between 2016 and 2019.

We estimate an intent-to-treat effect, categorizing all schools as treated from 2015 to 2019 if they adopted CEP in 2015, even though some schools that adopted CEP in 2015 exit the program later. While our primary specification pools across pre- and post-implementation years, we also run an event-study specification, replacing the $POST_y$ indicator with a vector of indicators for each school year (with 2014 as the omitted reference year). This event-study approach allows us to assess whether the parallel trends assumption appears to hold in our data prior to CEP roll-out.

Finally, to evaluate whether students on SNAP experience a reduction in discipline following CEP implementation that is distinct from other low-income students, we re-estimate Model 2, replacing $SNAP_{iy}$ with a $LOWINC_{iy}$ indicator. Low-income students are defined as those with tax-

reported income below 185 percent of the poverty line (i.e., likely eligible for free or reduced-price lunch) and students on SNAP. To directly test for a difference in CEP effect between these two groups, we further run Model 2 using the $SNAP_{iy}$ indicator but restrict the sample to low-income students only.

**RQ 3: Does expanded free lunch alter the impact of the SNAP benefits cycle?**

Our final research question examines whether CEP attenuates the effect of the SNAP benefits cycle on student suspension outcomes. To investigate this, we combine the within-month within-student design from RQ 1 with the school-level CEP adoption design from RQ 2. We thus exploit variation both within students across the month and between schools across CEP adoption through a quadruple-difference design. Specifically, we compare: (i) students receiving SNAP to students not on SNAP, (ii) late in the cycle versus earlier (Period 3 versus Period 2), (iii) years before and after CEP implementation, and (iv) schools that adopted CEP in 2015 versus CEP-eligible schools that never adopted CEP. To do this, we estimate the following model:

$$\begin{aligned} susp_{idm(y)} = {} & \delta_0 + \delta_1 PERIOD3_d POST_y SNAP_{im(y)} CEP_{s(im(y))} + \delta_2 PERIOD3_d SNAP_{im(y)} CEP_{s(im(y))} \\ & + \delta_3 PERIOD3_d POST_y CEP_{s(im(y))} + \delta_4 PERIOD3_d POST_y SNAP_{im(y)} \\ & + \delta_5 PERIOD3_d POST_y + \delta_6 PERIOD3_d SNAP_{im(y)} + \delta_7 PERIOD3_d CEP_{s(im(y))} \\ & + \delta_8 PERIOD3_d + \ldots + \theta_{im(y)} + \varepsilon_{idm(y)} \end{aligned} \tag{3}$$

Period 2 serves as the omitted reference category. The ellipses indicate that each of the above terms is repeated with *PERIOD1* in place of *PERIOD3*.

Here $\delta_1$ is our parameter of interest. This coefficient captures whether the increase in suspension rates in Period 3 relative to Period 2 for students receiving SNAP changes after CEP is implemented. If $\delta_1$ is negative, then CEP diminishes the benefits cycle spike. Our identifying variation comes from within-student, within-month variation that differs by SNAP receipt and CEP adoption status.

To provide a more intuitive set of results, we also estimate the within-month SNAP-cycle specification (Model 1) separately for four subgroups: CEP-eligible take-up schools and never-take-up schools, before and after CEP implementation. While Model 3 offers an empirical test of the differences between these four estimates, we find comparing these main effects directly clarifies our results.

## V. Results

At baseline, students on SNAP are disproportionately suspended. Table 1 reports average daily suspensions per 1,000 students and annual suspension rates by grade level. We find that students on SNAP experience substantially higher suspension rates than their peers. Middle and high school students on SNAP receive an estimated 1.81 suspensions per 1,000 students per day, compared to 0.68 per 1,000 students not on SNAP. Students on SNAP also receive more suspensions than their low-income peers. Students in grades 6-12 from low-income families who are not receiving SNAP have an average of 1.05 suspensions per 1,000 students. Annually, 13 percent of students on SNAP receive suspensions, compared to 6 percent of non-recipient students. Gennetian et al. (2016) also find a 7 percentage point disparity in suspension rates between SNAP and non-SNAP students.

These disparities are particularly high in middle and high schools, where suspensions are more common overall. Across our data, middle and high schoolers receive about 1.09 suspensions per day per 1,000 students compared to 0.28 for elementary students. Although we focus on middle and high school students in our discussion of results, we also run analyses for elementary school students. Our results provide clear evidence that discipline disparities also arise for SNAP participants in elementary schools.

We next descriptively document changes in suspension probability within a calendar month. Figure 2 shows average daily suspensions per 1,000 students across all years and analysis months. As shown here, students on SNAP consistently exhibit a higher suspension rate, beginning around 1.6 suspensions per day per 1,000 students and peaking at 2.3 around day 24, before dropping to just above 2.0 at the end of the month. In contrast, the non-recipient group starts just under 0.7 suspensions per day per 1,000 students, rises gradually, and levels near 1 suspension per day per 1,000 students by the end.

*Notes:* Sample restricted to grades 6-12. Calculations are from data for September, October, January, February, April, and May 2010-2019.

Both groups follow an upward trend across most of the 31 days. Still, students on SNAP have a suspension rate roughly double that of the non-recipient group throughout, and the non-recipient group shows less variability. Studies using daily discipline data find that student discipline rates vary over the course of the school year, rising from the beginning of the school year to the Thanksgiving break, falling before major breaks, and climbing again after classes resume, indicating that discipline is dynamic (Darling-Hammond et al. 2023). As such, it is not surprising that, on average, we see within-month variation in suspensions among all students.

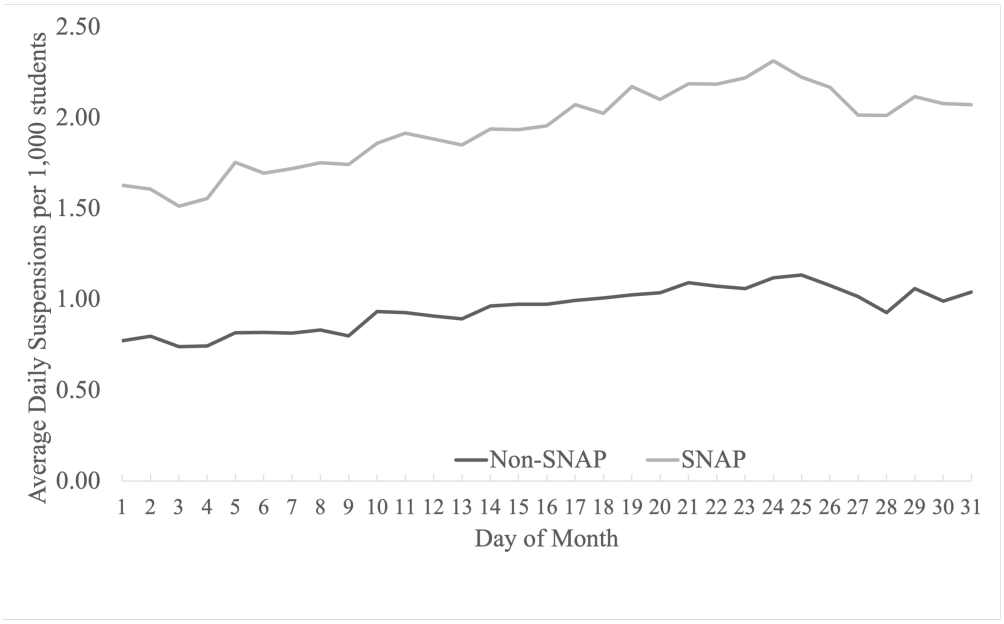



**Figure 2.** Average Daily Suspensions per 1,000 Students, 2010–2019

**RQ 1: What is the effect of the SNAP benefits cycle on suspension rates?**

We estimate Model 1 to find the causal effect of the SNAP benefits cycle on suspension rates. We compare within-student changes in suspension likelihood for students receiving SNAP in Period 3 versus Period 2 relative to students not receiving SNAP. Table 2 shows estimates from our DiD specification, with column 3 presenting our preferred model, which uses student-by-month-by-year fixed effects. We find that students on SNAP experience large jumps in suspension probabilities in Period 3 relative to Period 2, compared to students not on SNAP. We estimate that 6-12$^{th}$ graders on SNAP have 0.146 additional suspensions per thousand students in Period 3 compared to non-recipients. The average daily suspension rate for students on SNAP is 1.81 (see Table 1). Using this base rate, the estimate implies that, at the end of the month, these students have an 8% higher daily suspension rate than the monthly mean for students receiving SNAP. Our estimates are quite stable to relaxing the fixed effects, as shown in columns 1 and 2.

We divide our sample into two time periods for two reasons: first, CEP was introduced in 2015, and we analyze its effects in subsequent analyses; second, there is a general statewide decline in suspensions over this period. Despite these forces, the benefits cycle persists in the latter half of the decade (see column 4): 6-12$^{th}$ graders on SNAP have 0.136 more suspensions in Period 3 than in Period 2 during the 2015-2019 school years.

As previously noted, suspension rates are much lower in elementary schools. However, columns 5 and 6 show that the benefits cycle spike appears in elementary grades as well and remains statistically significant and persistent over time. The magnitude of the effect is smaller at 0.023

**Table 2.** Effects of SNAP Timing on Suspension Outcomes

| | Grades 6-12 | | | | Grades K-5 | |
|---|---|---|---|---|---|---|
| | 2010-2014 | | | 2015-2019 | 2010-2014 | 2015-2019 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Period 3 × SNAP | 0.139*** | 0.140*** | 0.146*** | 0.136*** | 0.023** | 0.035*** |
| | (0.0164) | (0.0200) | (0.0163) | (0.0134) | (0.00792) | (0.00760) |
| Period 1 × SNAP | -0.159*** | -0.167*** | -0.186*** | -0.080*** | -0.053*** | -0.037*** |
| | (0.0173) | (0.0195) | (0.0173) | (0.0141) | (0.00828) | (0.00791) |
| Period 3 | 0.084*** | 0.089*** | 0.088*** | 0.059*** | 0.021*** | 0.017*** |
| | (0.00655) | (0.0101) | (0.00655) | (0.00567) | (0.00370) | (0.00326) |
| Period 1 | -0.113*** | -0.138*** | -0.132*** | -0.079*** | -0.025*** | -0.018*** |
| | (0.00686) | (0.0102) | (0.00686) | (0.00567) | (0.00385) | (0.00340) |
| SNAP | 1.178*** | 0.974*** | | | | |
| | (0.0132) | (0.0432) | | | | |
| Constant | 0.769*** | 0.840*** | 1.155*** | 0.815*** | 0.290*** | 0.252*** |
| | (0.00526) | (0.0159) | (0.00458) | (0.00381) | (0.00253) | (0.00229) |
| Observations | 173,100,000 | 173,100,000 | 173,100,000 | 181,300,000 | 144,700,000 | 153,300,000 |
| $R^2$ | 0.000 | 0.002 | 0.070 | 0.069 | 0.067 | 0.072 |
| School-by-Month FEs | N | Y | – | – | – | – |
| Student-by-Month FEs | N | N | Y | Y | Y | Y |
| Standard Error Type | Robust | School | Student | Student | Student | Student |

*Note:* This table presents estimates from Model 1, a difference-in-differences specification examining the effect of SNAP benefit timing on daily suspension rates per 1,000 students. The dependent variable is an indicator for whether a student is newly suspended on a given day, multiplied by 1,000. Period 1 refers to days 1-19 of the month; Period 3 refers to days 17-31; Period 2 (the omitted reference category) refers to days 10-16. SNAP is an indicator for whether the student receives SNAP benefits in a given month-year. Observation counts are rounded according to Census Bureau rounding rules. Standard errors in parentheses. *p $<$ 0.05, **p $<$ 0.01, ***p $<$ 0.001.

suspensions per thousand students in 2010-14 and 0.035 suspensions in 2015-19, but these still correspond to a spike above 5% relative to the elementary suspension rate typical for students on SNAP.

The main effects of Period 1 and Period 3 capture within-month cyclicality in suspension rates common to all students and are identified off students not on SNAP. Consistent with prior work, suspension rates are, on average, lower early in the month and higher later in the month for all students, reflecting increases in suspensions over the school year, and after major breaks (Darling-Hammond et al. 2023). Our coefficient of interest on Period 3 * SNAP tells us whether there are late month increases in suspensions for students on SNAP, above and beyond those not on SNAP.

Table 3 shows additional subgroup analyses. In our main analysis, we compare students on SNAP to all students who were not receiving SNAP in each month. There might be concern that students on SNAP have other unobservable characteristics that coincide with the propensity to be suspended toward the end of the month that many students not on SNAP do not experience. One concern is that low-income children may face income constraints at the end of the month due to the timing of paychecks or large, regular expenses, such as rent, which usually fall at the beginning of the month. To mitigate this threat, we additionally compare students on SNAP to other low-income students who are not in Oregon's SNAP rolls. Specifically, we run our analysis limited to students whose family income-to-poverty ratio is below 1.85 and again to those whose income-to-poverty ratio is below 1.3. In Oregon, families with incomes below 130% of the poverty level are eligible for SNAP, but not every family applies. We find similar results (Oregon Department of Human Services 2023). Between 2010 and 2014, low-income middle- and high-school students whose families receive SNAP have, on average, 0.11 additional daily suspensions per 1,000 students than low-income students not on SNAP.

We also run models separately for male and female students, because male students have higher average suspension rates. Again, we find similar results, though male students see larger effects. Between 2010 and 2014, male middle- and high-school students whose families received SNAP had, on average, 0.195 additional daily suspensions per 1,000 students late in the benefits cycle compared to male students not on SNAP. We find similar results for male students in the second half of our analysis period. Female students experience a spike of less than half the size, but statistically significant and relative to a baseline suspension rate less than half that of male students. We also split outcomes into in-school and out-of-school suspensions and estimates effects separately. Our effects appear to be equally divided across these two suspension types, indicating that our findings are not driven by one or the other.

We additionally run all subgroup analyses shown in Table 3 for elementary students. These results are shown in Appendix Table Appendix Table A2. We find directionally similar results, though of smaller magnitude.

**Table 3.** Heterogeneity in Effects of the SNAP Benefits Cycle on Suspension Outcomes

| | Grades 6-12 | |
|---|---|---|
| | Pre-CEP (2010-2014) | Post-CEP (2015-2019) |
| | (1) | (2) |
| Main Effects | 0.146*** | 0.136*** |
| | (0.0163) | (0.0134) |
| Income-to-poverty ratio <1.85 | 0.110*** | 0.0627** |
| | (0.0234) | (0.0205) |
| Income-to-poverty ratio <1.3 | 0.107*** | 0.0669* |
| | (0.0298) | (0.0269) |
| Female | 0.0953*** | 0.0645*** |
| | (0.0175) | (0.0144) |
| Male | 0.195*** | 0.206*** |
| | (0.0271) | (0.0224) |
| Out-of-school suspension outcome | 0.0794*** | 0.0729*** |
| | (0.0112) | (0.00937) |
| In-school suspension outcome | 0.0662*** | 0.0631*** |
| | (0.0119) | (0.00963) |

*Note:* Each coefficient reports the estimate on $PERIOD3_d \times SNAP_{im(y)}$ from separate regressions estimated for each indicated subgroup. The outcome is an indicator for whether a student is suspended on a given day, scaled to represent suspensions per 1,000 students. All specifications include student-by-month fixed effects. Standard errors are clustered at the student level and reported in parentheses. *p $<$ 0.05, **p $<$ 0.01, ***p $<$ 0.001.

**RQ 2: Do the established benefits of expanded free lunch on low-income students extend to students on SNAP?**

Table 4 presents the results of Model 2, which estimates the effects of CEP adoption by SNAP and low-income status using a triple-difference approach. We estimate effects using annual suspension rates in addition to daily suspension rates because CEP varies at the school-year level and related studies focus on annual rates. Column 1 shows that students on SNAP in CEP schools experience a 2.5 percentage point decline in their average annual probability of suspension after the introduction of CEP. This effect is similar for low-income students, defined as those likely eligible for free/reduced-price meals based on either their tax-derived income or their SNAP enrollment status. Effects on average daily suspension rate are similar but not as precise. We find that students on SNAP in CEP take-up schools experience a decline of 0.38 average daily suspensions per 1,000 students, which is very similar to the 0.36 decline for low-income students generally.

The above estimates pool across pre- and post-CEP implementation years. We also run an event-study specification, replacing the $POST_y$ indicator with a set of school-year indicators and omitting the 2014 school year as the reference period. This event-study approach allows us to visually and statistically assess the identification assumption that the difference in suspension rates between students receiving SNAP and those not receiving SNAP would have followed similar trends across CEP and non-CEP schools in the absence of CEP. Figure 3 presents our event study results. Visually, our assumption of parallel trends holds. The pre-implementation coefficients are generally close to zero and not statistically significant, providing no evidence of pre-trends. In the years post-CEP implementation, point estimates indicate a decline in suspensions, although the year-specific estimates are not statistically significant at the 95% confidence level. The lagged response to CEP we show in the event study is consistent with Kho and Hunter (2026) results from Tennessee.

**RQ 3: Does expanded free lunch alter the impact of the SNAP benefits cycle?**

Next, we turn our attention to whether CEP roll out impacts the cyclical discipline patterns we describe above. Figure 4 summarizes average daily suspension patterns for students in CEP-eligible schools before and after CEP roll-out, by take-up status. We see that students consistently exhibit higher suspension rates in the pre-period compared to the post-period in both take-up and non-take-up schools. In the post-period, students on SNAP at CEP schools see a much larger decline in suspensions than those in non-CEP schools, consistent with the results of Model 2. All groups continue to show an upward trend over the 31 days, providing an ambiguous picture of the potential impact of CEP on the benefits cycle.

Table 5 reports estimates of Model 1 separately for the pre-CEP period (2010-2014) and the

**Table 4.** Effects of CEP on Suspension Outcomes by SNAP and Low-Income Status

| | (1) Ever suspended | (2) Avg. daily suspensions per 1,000 students |
|---|---|---|
| *A. DD model* | | |
| All students | -0.0185** | -0.274$^{+}$ |
| | (0.00665) | (0.170) |
| *B. DDD model for SNAP students* | | |
| Students not on SNAP | -0.0132* | -0.182 |
| | (0.00594) | (0.115) |
| Students on SNAP | -0.0245** | -0.376$^{+}$ |
| | (0.00789) | (0.193) |
| *C. DDD model for low-income students* | | |
| Non-low-income students | -0.0107$^{+}$ | -0.147 |
| | (0.00559) | (0.103) |
| Low-income students | -0.0241*** | -0.362* |
| | (0.00733) | (0.179) |

*Note:* Sample restricted to grades 6-12. The coefficients reported in panel A correspond to difference-in-differences estimates of the effect of CEP on suspension for all students. Panel B reports estimated effects of CEP from the triple-difference model by student's SNAP status: the effect for students on SNAP is derived as the linear combination of the triple-difference coefficient plus the coefficient for non-SNAP students. Panel C reports estimated effects of CEP from the triple-difference model by student's low-income status: the effect for low-income students is derived as the linear combination of the triple-difference coefficient plus the coefficient for non-low-income students. Low income is defined as students in households with tax-reported income below 185 percent of the poverty line or who receive SNAP. Column (1) reports the change in the probability a treated student is ever suspended in a school year; Column (2) reports average daily suspensions per 1,000 students. All specifications include school fixed effects, and standard errors are clustered at the school level. The difference between effects for students on SNAP and low-income students (which includes all students on SNAP) is not statistically significant. Standard errors in parentheses. $^{+}$p<0.10, *p $<$ 0.05, **p $<$ 0.01, ***p $<$ 0.001.

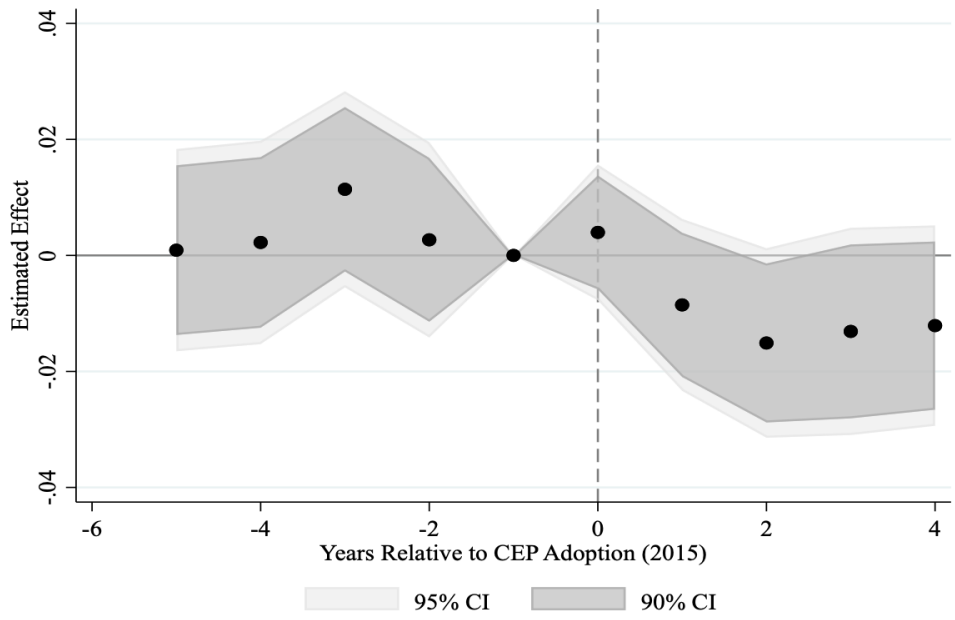



**Figure 3.** Event Study

*Note:* Event-study estimates of the differential change in suspensions for students on SNAP relative to non-recipient students, in CEP schools relative to non-CEP schools, by year. Coefficients are reported relative to the omitted year, 2014. Shaded areas show 95% confidence intervals.

post-CEP period (2015-2019) for CEP-eligible schools, stratified by CEP eligibility and take-up status. We first estimate the effects of the SNAP benefits cycle for students in CEP-eligible schools, which have high proportions of low-income and SNAP-receiving students, and in CEP-ineligible schools, which generally have below-average proportions of low-income and SNAP-receiving students. Importantly, the benefits cycle exists in both groups, both before and after CEP roll-out. SNAP benefit cycles do not appear to occur only in high- or low-poverty schools.

Next, we look across CEP-eligible take-up schools (defined as those that enroll in the program in 2015) and non-take-up schools (defined as eligible schools that do not enroll between 2015 and 2019). There is virtually no change in the Period 3 effect for students on SNAP in non-take-up schools following the introduction of CEP: both before and after CEP implementation, students on SNAP in non-take-up schools had about 0.13 more suspensions per day per 1,000 students in Period 3 compared to Period 2. For take-up schools, we find a weaker SNAP benefit-cycle effect in the pre-period, which appears to increase somewhat after the implementation of the universal free lunch program. This suggests that CEP does not mitigate the benefits cycle's impact even as it reduces overall suspension rates for students on SNAP.

When we formally test the quadruple difference specification, we find no evidence that CEP alters the impact of SNAP benefit cycles on treated students. Appendix Table A3 presents the results

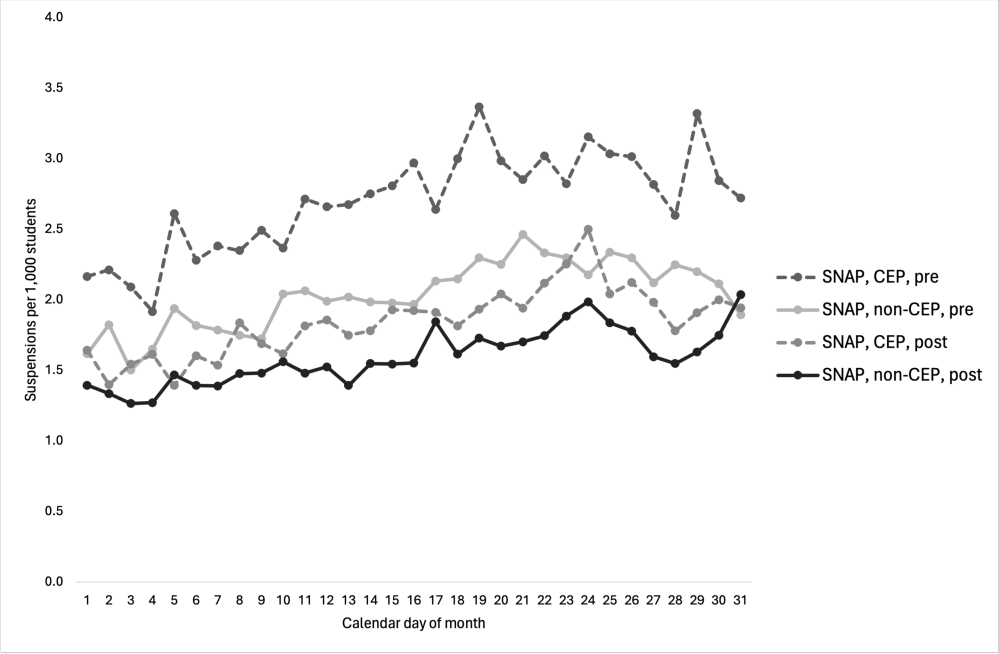



**Figure 4.** Average Daily Suspensions for Students on SNAP, Pre and Post CEP Roll-Out, in CEP-Eligible Take-Up and Non-Take-Up Schools

*Note:* Sample restricted to students on SNAP in grades 6-12. "Pre" results are for the years prior to CEP roll-out (2010-2014), while "post" results are for 2015-2019. Student-year observations are retained only for students enrolled in the same school for $\geq$90 days. Suspensions from other schools are excluded. Suspensions in June and July are excluded.

**Table 5.** Effects of the SNAP Benefits Cycle on Suspension Outcomes by CEP Take-Up and Eligibility

| | Grades 6-12 | |
|---|---|---|
| | Pre-CEP (2010-2014) | Post-CEP (2015-2019) |
| | (1) | (2) |
| Main Effects Grades 6-12 | 0.146*** | 0.136*** |
| | (0.0163) | (0.0134) |
| CEP eligible schools | 0.142*** | 0.131*** |
| | (0.0233) | (0.0193) |
| CEP ineligible schools | 0.100*** | 0.0988*** |
| | (0.0231) | (0.0189) |
| CEP eligible take-up schools | 0.0731 | 0.105* |
| | (0.0541) | (0.0440) |
| CEP eligible non-take-up schools | 0.128*** | 0.132*** |
| | (0.0287) | (0.0251) |

*Notes:* This table presents estimates of the effect of the SNAP benefits cycle on daily suspension rates per 1,000 students, estimated separately for the pre-CEP period (2010-2014) and post-CEP period (2015-2019) using the within-month within-student specification from Model 1. The dependent variable is an indicator for whether a student is newly suspended on a given day, multiplied by 1,000. Each coefficient reports the estimate on $PERIOD3_d \times SNAP_{im(y)}$ from separate regressions estimated for each indicated subgroup. All specifications include student-by-month-by-year fixed effects. The top panel presents estimates for the full sample. Subsequent panels stratify by CEP eligibility and, among eligible schools, by whether the school adopted CEP ("take-up schools") or did not adopt CEP despite eligibility ("non-take-up schools"). Standard errors are clustered by student-by-month and shown in parentheses. $^{+}$p<0.10, *p $<$ 0.05, **p $<$ 0.01, ***p $<$ 0.001.

of Model 3 across grades 6-12, along with subgroup analyses for grades 6-8 and 9-12. Across all three grade levels, the coefficient $\delta_1$ is positive and not statistically significant, indicating that the implementation of CEP does not reduce the effect of the SNAP cycle on discipline for students on SNAP, and may, in fact, make it more salient. Importantly, this does not undermine the finding that CEP reduces suspensions for students in households receiving SNAP: the decline is dramatic in the raw average daily suspension rates at CEP schools pre- and post-CEP implementation (see Figure 4). Interestingly, the coefficient $\delta_1$—which measures the effect of Period 3 on students generally—is insignificant but negative. This suggests CEP may be impacting cyclical suspensions writ large, but not specifically bringing down the Period 3 spike proportionally for students on SNAP.

## VI. Discussion and Conclusion

In this study, we provide some of the first evidence that students receiving SNAP experience increases in disciplinary outcomes over the course of the benefit month, and the first evidence that the introduction of the CEP does not attenuate these cyclical patterns. While there is ample evidence that low-income students are disproportionately suspended, there is no research to date investigating suspension disproportionality for students on SNAP relative to other low-income students. Additionally, prior research has considered the impact of the SNAP benefit cycle on student food security and academic outcomes, but little is known about its connection to school discipline. Moreover, despite evidence that CEP reduced disciplinary incidents among low-income students, no studies that we know of have examined how both SNAP participation and the rollout of CEP shape disciplinary outcomes across the benefit cycle.

We find that students receiving SNAP have suspension rates nearly twice as high as those of students not on SNAP. Surprisingly, students on SNAP also have significantly higher suspension rates than low-income students not enrolled in SNAP. Because Black, Latinx, and American Indian and Alaska Native children already experience disproportionate discipline at school (Gregory et al. 2010; Skiba et al. 2014; Welsh and Little 2018), they may be especially vulnerable to increased exclusionary discipline in response to food insecurity. Future work should investigate the intersection of race/ethnicity, SNAP receipt, and discipline to further unpack the role of food insecurity in shaping educational outcomes among marginalized students.

We find that suspension rates among students on SNAP follow an intra-monthly cycle, rising as families' benefits are depleted. This finding aligns with Gennetian et al. (2016), who document a similar pattern in referrals among public school students in Chicago. Although this is the only other study we know of to focus directly on discipline, related work shows that students perform worse on tests at the end of the benefit month (Bond et al. 2022; Cotti et al. 2018; Gassman-Pines and Bellows 2015). Given that SNAP serves more than 12% of U.S. residents and nearly one-third

of participants are school-aged children (Jones 2025), future research should attend to the impacts of SNAP on other educational outcomes, such as attendance.

We show that students on SNAP experience reductions in suspensions in schools that adopted CEP in 2015, compared with eligible non-take-up schools, consistent with Domina et al. (2024), which finds that students already receiving free or subsidized lunch exhibit some of the largest declines in suspension receipt following the introduction of universal free lunch. Given that SNAP recipients were already eligible for free school meals before CEP roll-out, the finding that CEP reduces discipline outcomes for these students may, at first glance, be surprising. However, Domina et al. (2024) posit that this decline may be due to reduced stigma surrounding the receipt of free school meals. Students may be more likely to take up these meals when their peers are also accessing them. CEP may also open up resources for schools in other ways that permit them to drive suspension rates down, such as freeing up staff from monitoring the lunch line during lunch periods.

Despite these important benefits of CEP for low-income students, we do not find that CEP buffers against the cyclical relationship between SNAP and discipline. It is possible that the benefits cycle spikes reflect the hazards of resource depletion and associated stresses at home, and that CEP, especially if its mechanism relates to the school environment more than improved food access, may not address these hazards. The mechanisms underlying the cyclical suspension pattern we observe remain unclear. Kinsey et al. (2019) conducted a prospective cohort study of mothers receiving SNAP benefits in Philadelphia. They found that, toward the end of the benefit month, mothers were more likely to skip meals and consume cheaper, lower-quality foods. They also found that mothers utilized a variety of coping strategies to manage the SNAP cycle, including adjustments to shopping and eating patterns, mental accounting, emotional resilience, and social support. Future qualitative research in this area can help illuminate whether increased discipline reflects student hunger, family stress, or other related factors.

This study is unique in that we have access to 10 years of administrative data for the entire state of Oregon, linked to SNAP and IRS records. This is the first paper we know of to study the impact of nutritional supports on suspension at the state level. Our work is unique in that it moves beyond free- and reduced-price lunch proxies for economic hardship to more nuanced administrative measures.

These results have implications for educational inequality between lower- and higher-income students. Our findings highlight the disparities that students on SNAP face in exclusionary discipline, the consequences of SNAP-induced food insecurity, and the limitations of school meal programs as a buffer against these factors. Educators and administrators are frequently tasked with responding to behaviors in school that may reflect unmet basic needs originating outside of school. The cyclical patterns documented here suggest that student behaviors may fluctuate in ways that

schools respond to with exclusionary discipline. It is unlikely, however, that suspensions will address the root causes of these behavioral changes. While we hypothesize that family stress and food insecurity may be affecting student behavior, further research is warranted to more directly assess the impact of income variability on student behavior.

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# Appendix

**Table A1.** Effects of SNAP Timing on Suspension Outcomes Across Multiple Specifications

| | 2010-2014 | | | | | 2015-2019 | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Period 1 | -0.113*** | -0.138*** | -0.138*** | -0.132*** | -0.132*** | -0.0694*** | -0.0824*** | -0.0824*** | -0.0791*** |
| | (0.00686) | (0.0102) | (0.00876) | (0.00686) | (0.00682) | (0.00564) | (0.00784) | (0.00694) | (0.00567) |
| Period 3 | 0.0837*** | 0.0893*** | 0.0893*** | 0.0877*** | 0.0877*** | 0.0564*** | 0.0598*** | 0.0598*** | 0.0588*** |
| | (0.00655) | (0.0101) | (0.00900) | (0.00655) | (0.00650) | (0.00538) | (0.00759) | (0.00676) | (0.00538) |
| SNAP | 1.178*** | 0.974*** | 0.974*** | | | 0.845*** | 0.647*** | 0.647*** | |
| | (0.0132) | (0.0432) | (0.0196) | | | (0.0108) | (0.0314) | (0.0146) | |
| Period 1 × SNAP | -0.159*** | -0.167*** | -0.167*** | -0.186*** | -0.186*** | -0.0651*** | -0.0698*** | -0.0698*** | -0.0799*** |
| | (0.0173) | (0.0195) | (0.0197) | (0.0173) | (0.0171) | (0.0142) | (0.0165) | (0.0161) | (0.0141) |
| Period 3 × SNAP | 0.139*** | 0.140*** | 0.140*** | 0.146*** | 0.146*** | 0.131*** | 0.133*** | 0.133*** | 0.136*** |
| | (0.0164) | (0.0200) | (0.0197) | (0.0163) | (0.0163) | (0.0135) | (0.0181) | (0.0156) | (0.0134) |
| Constant | 0.769*** | 0.840*** | 0.840*** | 1.155*** | 1.155*** | 0.535*** | 0.602*** | 0.602*** | 0.815*** |
| | (0.00526) | (0.0159) | (0.00741) | (0.00458) | (0.00458) | (0.00432) | (0.0118) | (0.00572) | (0.00381) |
| Observations | | | 173,100,000 | | | | | 181,300,000 | |
| $R^2$ | 0.000287 | 0.00216 | 0.00216 | 0.0696 | 0.0696 | 0.000219 | 0.00152 | 0.00152 | 0.0693 |
| School-by-month FEs | N | Y | Y | – | – | N | Y | Y | – |
| Student-by-month FEs | N | N | N | Y | Y | N | N | N | Y |
| SE Type | Robust | School | School-by-month | Student | Student-by-month | Robust | School | School-by-month | Student |

*Note:* This table presents estimates from Model 1, a difference-in-differences specification examining the effect of SNAP benefit timing on daily suspension rates per 1,000 students. The dependent variable is an indicator for whether a student is newly suspended on a given day, multiplied by 1,000. Period 1 refers to days 1-9 of the month; Period 3 refers to days 17-31; Period 2 (the omitted reference category) refers to days 10-16. SNAP is an indicator for whether the student receives SNAP benefits in a given month-year. Standard errors are clustered as indicated and shown in parentheses. *p $<$ 0.05, **p $<$ 0.01, ***p $<$ 0.001.

**Table A2.** Effects of SNAP Timing on Suspension Outcomes by Sex, Income and School Community Eligibility Program Status, 2010–2019

| | Grades K-5 | |
|---|---|---|
| | 2010-2014 | 2015-2019 |
| Female | 0.00683 | 0.0227*** |
| | (0.00620) | (0.00599) |
| Male | 0.0396** | 0.0468*** |
| | (0.0143) | (0.0137) |
| Income-to-poverty ratio <1.85 | 0.0224 | 0.0232* |
| | (0.0122) | (0.0113) |
| Income-to-poverty ratio <1.3 | 0.0504** | 0.0192 |
| | (0.0167) | (0.0148) |
| Out-of-school suspension outcome | 0.0171** | 0.0225*** |
| | (0.00609) | (0.00573) |
| In-school suspension outcome | 0.00636 | 0.0124* |
| | (0.00503) | (0.00500) |
| CEP eligible schools | 0.0211* | 0.0335*** |
| | (0.00989) | (0.00944) |
| CEP ineligible schools | 0.0287* | 0.0292* |
| | (0.0140) | (0.0139) |
| CEP eligible take-up schools | 0.0240 | 0.0401** |
| | (0.0167) | (0.0155) |
| CEP eligible non-take-up schools | 0.0175 | 0.0114 |
| | (0.0139) | (0.0135) |

*Note:* This table presents subgroup estimates from Model 1, a difference-in-differences specification examining the effect of SNAP benefit timing on daily suspension rates per 1,000 students. The dependent variable is an indicator for whether a student is newly suspended on a given day, multiplied by 1,000. Period 1 refers to days 1-9 of the month; Period 3 refers to days 17-31; Period 2 (the omitted reference category) refers to days 10-16. Each coefficient reports the estimate on $PERIOD3_d \times SNAP_{im(y)}$ from separate regressions estimated for each indicated subgroup. All specifications include school fixed effects. Standard errors are clustered at the school level and reported in parentheses. *p < 0.05, **p < 0.01, ***p < 0.001.

**Table A3.** Quadruple Difference Results: Effects of CEP Uptake and SNAP Timing on Suspension Outcomes Across Multiple Grade Levels, 2010–2019

| | Grades 6-12 | Grades 6-8 | Grades 9-12 |
|---|---|---|---|
| Period 3 | 0.108*** | 0.119*** | 0.0981*** |
| | (0.0143) | (0.0215) | (0.0192) |
| Period 3 × SNAP | 0.128*** | 0.177*** | 0.0807* |
| | (0.0287) | (0.0426) | (0.0388) |
| Period 3 × CEP | 0.105** | 0.127** | 0.0644 |
| | (0.0346) | (0.0481) | (0.0487) |
| Period 3 × SNAP × CEP | -0.0550 | -0.0233 | -0.186* |
| | (0.0612) | (0.0832) | (0.0890) |
| Period 3 × Post | -0.00818 | -0.00531 | -0.0106 |
| | (0.0192) | (0.0294) | (0.0252) |
| Period 3 × SNAP × Post | 0.00399 | -0.0743 | 0.0770 |
| | (0.0381) | (0.0578) | (0.0503) |
| Period 3 × CEP × Post | -0.0831 | -0.101 | -0.0542 |
| | (0.0470) | (0.0682) | (0.0617) |
| **Period 3 × SNAP × CEP × Post** | **0.0283** | **0.0831** | **0.0312** |
| | **(0.0795)** | **(0.112)** | **(0.109)** |
| Observations | 139,500,000 | 71,070,000 | 68,410,000 |
| Grade range | 6-12 | 6-8 | 9-12 |

*Note:* This table presents estimates from Model 3, a quadruple-difference specification examining whether CEP adoption attenuates the effect of the SNAP benefits cycle on daily suspension rates. The dependent variable is an indicator for whether a student is newly suspended on a given day, multiplied by 1,000. The sample includes all students at CEP-eligible schools that adopted CEP in 2015 and CEP-eligible schools that never adopted CEP; students at schools that became eligible after 2015 or adopted CEP in years other than 2015 are excluded. POST is an indicator equal to one for school years 2015-2019. CEP is an indicator for whether the school adopted CEP. SNAP is an indicator for whether the student receives SNAP benefits in a given month-year. All specifications include student-by-month-by-year fixed effects, Column 1 presents estimates for grades 6-12; column 2 for grades 6-8; column 3 for grades 9-12. Standard errors are clustered at the school level. Standard errors in parentheses. *p $<$ 0.05, **p $<$ 0.01, ***p $<$ 0.001.