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

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Abstract

Does cyclical food insecurity caused by the timing of food stamp benefit receipt influence student educational outcomes, and does the provision of free meals at school offer a protective buffer to these cyclical effects? It is well established that low-income families are vulnerable to increased food insecurity as they spend down their benefits over the course of the month. This within-month variation in benefit access may have important effects on family and child wellbeing, potentially impacting a child's behavior at school in ways that schools react to punitively.

In this paper, we use administrative education data from an Oregon linked to records of SNAP enrollment housed at the U.S. Census Bureau to examine the impact of the timing of Supplemental Nutrition Assistance Program (SNAP) receipt on student disciplinary punishments, and the impact of the school-level roll-out of universal free meals through the Community Eligibility Provision (CEP) on this relationship.

We first document that students receiving SNAP are suspended at significantly higher rates than non-recipients across the income distribution, including low-income students not on SNAP. In our sample, middle- and high-school students on SNAP receive 1.81 suspensions per 1,000 students per day, compared with 1.05 for low-income students not on SNAP and 0.54 for middle- and high-income students not on SNAP.

Next, 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 staggered school-level CEP adoption, we show that universal free lunch reduces suspensions for students on SNAP. However, we do not find that CEP attenuates the cyclical increase in suspensions associated with the SNAP benefits cycle.

Together, these findings suggest that universal free meals, while important for reducing baseline disparities in school discipline among low-income students, do not fully buffer students against the impacts of cyclical food insecurity caused by the SNAP benefit cycle.

1. Introduction

It is well established that poverty affects students' educational outcomes (Bradley & Corwyn, 2002; Brooks-Gunn & Duncan, 1997; Duncan & Murnane, 2011; V. E. Lee & Burkam, 2002; Sirin, 2005). One potential mechanism is through child hunger and food insufficiency, which have cognitive and non-cognitive impacts on children (Howard, 2011b, 2011a; Jackson, 2016). Research has found that the US's largest anti-hunger policies, the Supplemental Nutrition Assistance Program (SNAP) and the federal school breakfast and lunch programs improve child outcomes, from test scores and school attendance to later-life health and economic self-sufficiency (Bailey et al., 2024; Beharie et al., 2017; Bitler, 2015; Domina et al., 2024; Gordon & Ruffini, 2021; Hoynes et al., 2016). However, there is a large body of evidence that families on SNAP experience intra-monthly cycles in food expenditures, dietary quality, and food insecurity because benefits do not last the entire month (Schanzenbach, 2023; Gassman-Pines & Schenck-Fontaine, 2019; Goldin et al., 2022; C. A. Gregory & Smith, 2019; Hamrick & Andrews, 2016; Kuhn, 2018; J. Y. Lee et al., 2022; Todd & Gregory, 2018; Whiteman et al., 2018). These fluctuations have been shown to affect educational outcomes, including standardized test performance (Bond et al., 2022; C. D. Cotti et al., 2020; Gassman-Pines & Bellows, 2015) and school referral receipt (Gennetian et al., 2016).

This paper investigates whether students who receive SNAP experience increases in punitive disciplinary outcomes late in the benefit month as family resources dwindle, and whether expanding the free school meals programs buffers against these impacts. This study leverages a unique student-level data linkage between Oregon K-12 administrative data and monthly SNAP enrollment data. We provide some of the first evidence we know of of suspension disproportionality among students on SNAP, as well as one of the first analysis of how both SNAP benefit cycles and expanded in-school nutritional supports shape cyclical patterns in student discipline. Specifically, this paper asks:

1. Do students on SNAP experience disproportionate rates of suspension?
2. Do students on SNAP experience a relative jump in suspension rates as their families' benefits dwindle? In other words, do they experience cyclicality in suspension rates with the SNAP benefits cycle?

3. Do the established benefits of expanded free lunch on low-income students extend to students on SNAP?
4. Does expanded free lunch alter the impact of the SNAP benefits cycle?

We are uniquely positioned to answer these questions because our data allow us to observe student-level, monthly SNAP participation records linked to K-12 education records. We link these to family income in annual tax return records, allowing us to compare SNAP recipients with other low-income students. Most prior studies rely on free or reduced-price lunch (FRPL) status as a proxy for economic disadvantage or SNAP eligibility. However, FRPL is an imprecise proxy for SNAP participation, in part because the eligibility threshold is higher. As such, FRPL pools together a much larger group of students. We find that students on SNAP are significantly more likely to be suspended, even when compared to low-income peers.

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

We use a difference-in-differences (DiD) design to compare outcomes for students late in their benefits cycle to their own outcomes earlier in the same month. Specifically, 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 compare within-student changes in suspension likelihood for SNAP students

[1] Parents without SSNs receiving benefits on behalf of their children also receive benefits on the first day of the month. We cannot observe SSNs or disbursement dates, so we cannot use the specific disbursement date, but leverage the general timing.

across these periods to those for students not on SNAP. This allows us to account for time-varying factors associated with discipline receipt, as long as they are not separately correlated with SNAP receipt. For example, many families may be preparing to pay rent or mortgages on the first of the month, meaning they will face income constraints by the end of the month. However, because this timing likely affects students on SNAP and students not on SNAP in similar ways, our DiD specification accounts for this variation. Our identifying assumption requires that, absent SNAP benefits, no other factors affect suspension probabilities over the course of the calendar month in ways that differ between students on SNAP and those not on SNAP.

We next ask whether expanding the school meals program protects students on SNAP from cyclical food insecurity. The Community Eligibility Program (CEP) allows schools to offer free meals to all students, regardless of household income, and Oregon schools first became eligible for CEP in the 2014-15 school year. To examine the impact of CEP on students on SNAP, we build on prior work that exploits staggered CEP rollouts (e.g. Domina et al., 2024; Gordanier et al., 2020; Gordon & Ruffini, 2021; Kho, 2018; Rothbart et al., 2023). In the first year, 675 of the state's 1,239 schools were eligible for CEP, and 262 schools took up the program (Center on Budget Priorities, 2015). We exploit differential adoption of the program to estimate its impact on suspensions among students receiving SNAP. Specifically, we compare students in eligible schools before and after the program's introduction, in schools that did and did not adopt the program. We employ a triple-difference approach to extend the existing findings on the impact of CEP to students on SNAP specifically (Domina et al., 2024; Gordanier et al., 2020; Kho, 2018; Rothbart et al., 2023).

Finally, we combine the above two approaches to estimate the combined impact of CEP and the SNAP benefits cycle. We compare students who do and do not receive SNAP, in schools that do or do not take up CEP, before and after the roll-out of CEP, and early and late in the SNAP benefits cycle. There is limited scholarship on the impact of the SNAP benefit cycle on punitive disciplinary outcomes for students (for one of the few papers we could find see Gennetian et al., 2016), and no prior work has examined these SNAP impacts in combination with expanded school meals.

We find clear evidence that students on SNAP face disproportionate rates of suspension compared to their peers. We find that suspension rates for students on SNAP rise over the course of the month, consistent with the timing of families' benefit depletion. We also show that students in CEP schools experience lower suspension rates overall. We do not find that CEP attenuates the end-of-month spike in disciplinary incidents associated with the SNAP benefit cycle.

These findings translate into meaningful difference: Middle and high school students on SNAP experience 0.15 additional suspensions per 1,000 students in the second half of the month relative to non-recipients. 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.

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.

2. Prior Literature

The Importance of SNAP

In 2024, SNAP served 1 in 8 Americans (Center on Budget Priorities, 2024). 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.

Other papers have used the initial roll-out of the SNAP program to investigate the relationship between exposure and well-being. In their seminal paper on the long-run impacts of access to the social safety net, Hoynes et al. (2016) use the Panel Study of Income Dynamics to link early childhood county of residence to adult health. Again, using variation in county-level program roll-out, the authors find that exposure to the program had significant positive effects, even decades after exposure. They 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).

Other studies have used exogenous variations in SNAP participation to estimate the impact of SNAP on health and food insecurity. Gregory and Deb (2015) use variation in state SNAP eligibility to model the relationship between SNAP and health outcomes and find that SNAP increases the probability of reporting excellent or good health by 6 and 2 percentage points, respectively. Swann (2017) uses a similar approach, using variation in state-level policies that differentially impose administrative burdens on SNAP participation to show that SNAP reduces the likelihood of food insecurity by 7 percentage points.

There is a much smaller body of work that examines how, and to what extent, SNAP receipt improves educational outcomes. Two papers use variation in county-level roll-out of the Food Stamp Program to investigate educational and long-term economic 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. Two studies have investigated the relationship between SNAP and absenteeism: 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. In their investigation of SNAP and children's outcomes, Beharie et al. (2017) find that SNAP moderates the relationship between grade retention and poverty. Importantly, this study is associational and lacks a quasi-experimental design.

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 & Schenck-Fontaine, 2019; C. A. Gregory & Smith, 2019; Hamrick & Andrews, 2016; J. Y. Lee et al., 2022; Whiteman et al., 2018). Gregory and Smith (2019) used 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) used 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.

This cyclical experience of food insecurity may have significant negative health impacts on individuals. Indeed, research has shown that SNAP recipients have increased ER visits (C. D. Cotti et al., 2020; Ojinnaka & Heflin, 2018) and are less likely to adequately manage chronic diseases such as asthma (Heflin et al., 2019; Kinsey et al., 2019) at the end of the benefit month.

Cyclical food consumption and increased food insecurity throughout the month have been shown to negatively affect school-aged 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. While Cotti et al. (2018) and Gassman-Pines and Bellows (2015) focus on accountability tests that are generally high-stakes for schools but not for students, this study focuses on a test that is high-stakes for students themselves. Additionally, while schools may offer breakfast and lunch to students taking accountability tests, college entrance exams are generally taken on weekends, when schools are unable to fill students' nutritional gaps.

The cyclical nature of food insecurity induced by SNAP benefit depletion may also impact children's behavior at school. 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). Both increases in family stress and increases in child hunger may influence children's behavior in school. Food insecurity is associated with worse chronic and acute health problems (Thomas et al., 2019) and is associated with worse mental health outcomes for children, both immediately and later in life (Pourmotabbed et al., 2020). A study using National Health and Nutrition Examination Survey II data found that food-insecure 6- to 11-year-old children were more likely to have seen a psychologist and to have difficulty getting along with other children (Alaimo et al., 2001). 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. Importantly, this paper uses only one year of data from one school district. Our study expands on this by using multiple years of data from the entire state of Oregon and focusing on suspensions rather than referrals.

To date, only three studies we could find investigating SNAP cycles and educational outcomes have used actual SNAP enrollment data. Genettian et al. (2016) used one year of SNAP data linked to Chicago Public Schools' administrative data, Gassman-Pines and Bellows (2015) used three months of SNAP data linked to North Carolina public school test score data and Cotti et al. (2018) used four years of South Carolina standardized state testing data for SNAP recipients. Our study is unique in that we have access to ten years of administrative data for the entire state of Oregon, linked to IRS tax records and SNAP records.

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. The federal school breakfast and lunch programs (SBP and NSLP, respectively) provided free and low-cost meals to forty percent of US school children in 2014 (Hoynes & 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 & Kim, 2010; Bhattacharya et al., 2006; Fletcher & Frisvold, 2017; Huang & Barnidge, 2016; Schanzenbach & Zaki, 2014) improved academic outcomes (Andreyeva & Sun, 2021; Gordon & Ruffini, 2021; Hecht, 2020; Hinrichs, 2010; Murphy et al., 1998; Ruffini, 2022), and physical health outcomes (Davis & 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.[2] Under the provision, schools with an Identified Student Percentage (ISP)[3] 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 & Musaddiq, 2019). There is a large body of literature investigating the impact

[2] 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.

[3] 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 (W. Davis and Musaddiq 2019).

of CEP on educational outcomes (Andreyeva & Sun, 2021; Gordanier et al., 2020; Ruffini, 2022), including reductions in discipline outcomes (Domina et al., 2024; Gordon & Ruffini, 2021; Kho, 2018).

In investigating the impact of CEP, some researchers have leveraged the staggered state-level roll-out to investigate the relationship between CEP and outcomes. For example, Gordon and Ruffini (2021) restrict their analysis to ever-participating schools and report an intent-to-treat effect, using differential timing in when states became eligible for CEP to investigate the effects of CEP implementation on student discipline outcomes. They find that schoolwide free meals statistically significantly reduced suspensions by 17 percent among white male elementary students, with smaller, insignificant point estimates for other subgroups.

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

3. Data

We make use of a unique dataset: student-level educational administrative data from the Oregon Department of Education (ODE) is linked with confidential microdata stored at the Census Bureau, which includes monthly SNAP records from the state of Oregon and annual income tax records from the IRS. These data include individual level data on all students enrolled in public K-12 schools in Oregon from the 2009-2010 school year to the 2018-2019 school year. Our analysis dataset allows us to observe whether a student’s household receives SNAP in each month, as well as daily suspension incidences and annual household income records. This dataset is formed by linking across several sources of student-level microdata housed at the U.S. Census Bureau. 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)).

Central to our study are K-12 administrative records from the Oregon Department of Education. These data critically include dates of individual student suspensions and school enrollment, allowing us to construct daily measures of disciplinary outcomes for each student. These administrative data allow us to see within-month variation in suspensions, rather than relying solely on annual or school-year aggregates.

SNAP receipt and family income

To identify SNAP participation, we link students to Oregon SNAP administrative records using PIKs. These records report program receipt monthly, allowing us to construct student–month–year indicators of SNAP enrollment.

To characterize family income, 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 household size reported for the tax unit. We align this annual income measure to the relevant school year using income from the adjacent prior calendar year (e.g., income reported on 2014 tax returns for the 2014–15 school year, typically filed in spring 2015).

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 thus should be interpreted as a coarse measure of underlying economic disadvantage instead of an accurate measure of household advantage or program participation.

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. 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. It is important to retain students who enter or exit schools during the academic year, since students who are suspended are more likely to switch schools or face expulsion (Welsh, 2017, 2018), but our identification approach requires analyzing only the months in which students are enrolled in one school for the entire month. We

allow students' SNAP status to vary across months, though 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.

Student-year dataset

To 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 used in our descriptive analysis and our CEP analysis (RQ 1 and RQ 3), while daily suspension rates are used in analyses of SNAP benefit timing (RQ 2 and RQ 4).

4. Analytic Approach

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. We compare the likelihood of receiving a suspension in the days immediately following benefit disbursement with that in later periods in the month, for students on SNAP compared to students not on SNAP. To estimate the effect of CEP on suspension for students on SNAP, we employ a triple-difference approach, comparing SNAP recipients to non-recipients before and after CEP

implementation, at take-up and non-take-up schools. Lastly, we combine these two approaches to investigate if access to CEP attenuates the impact of the SNAP benefits cycle on discipline. The following section describes the analytic approaches for each research question in more detail.

RQ1: Do students on SNAP experience disproportionate rates of suspension?

To answer this question, we document suspension rates across four mutually exclusive groups: (1) students on SNAP; (2) students not on SNAP who are low income[4]; (3) middle- and high-income students not on SNAP, 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. We estimate annual and daily suspension rates for these groups by school level. Importantly, these results should not be interpreted causally.

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 observe do not observe SNAP receipt or tax records for 7 percent of students. These students are excluded from our subsequent analyses.[5]

[4] 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

[5] This may introduce bias into our results, though prior research suggests these students are not particularly economically disadvantaged (Clark & Bhaskar, 2025). Addityionally, 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.

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	.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	3019000						
Grades K-5		0.28	0.30	0.27	0.03	0.03	0.02
Female	0.49	0.08	0.14	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	0.29	0.12	0.01	0.01	0.01
Non-SNAP, Low Income	0.11	0.24	0.45	0.24	0.02	0.02	0.02
Non-SNAP, Income Unknown	0.05	0.28	0.09	0.28	0.03	0.03	0.02
On SNAP	0.43	0.44	0.24	0.43	0.04	0.04	0.04
N	2497500						

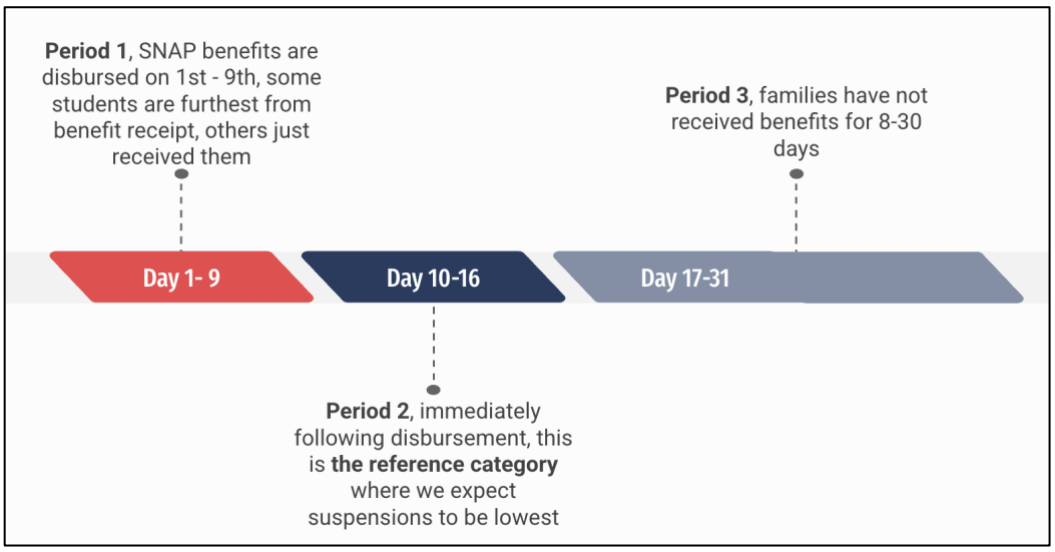
Note: Observation counts (Ns) rounded according to the Census Bureau's rounding rules. Summary statistics for the analytic student-year file, using all grades K-12. Student-year observations are retained only for students enrolled in the same school for ≥90 days. Suspensions from other schools are excluded. 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.

RQ2: What is the effect of the SNAP benefits cycle on suspension rates?

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.

Figure 1 Oregon SNAP Disbursement Schedule and Identification Strategy



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 (days 1–9):** when families receive SNAP benefits.
- **Period 2 (days 10–16):** when all families have received SNAP benefits and are relatively close to disbursement.
- **Period 3 (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; C. Cotti et al., 2018; Gassman-Pines & Bellows, 2015; Gassman-Pines & Schenck-Fontaine, 2019; Whiteman et al., 2018). Accordingly, Period 3 begins after the first week of the cycle and,

for most recipients, includes the weeks in which prior studies have observed SNAP-cycle effects on other outcomes.

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.

Building on Gennetian et al. (2016), we estimate the following difference-in-differences model:

$$susp_{idm(y)} = \beta_0 + \beta_1(PERIOD3_d * SNAP_{im(y)}) + \beta_2(PERIOD1_d * SNAP_{im(y)}) + \beta_3 PERIOD3_d + \beta_4 PERIOD1_d + \theta_{im(y)} + \epsilon_{idm(y)} \quad \text{(Model 1)}$$

where $susp_{idm(y)}$ is an indicator of whether student *i* is suspended on day *d* of month-of-year *m(y)*. $PERIOD3_d$ 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)*. The term $\theta_{im(y)}$ denotes the student-by-month-by-year fixed effects, which absorb all time-invariant student characteristics and month-year-specific differences in suspension propensity. The main effect of $SNAP_{im(y)}$ is subsumed by the fixed effects.

We use the average daily suspension rate per 1,000 students as our primary outcome. This allows us to account for the variation in the number of school days in each period without biasing our result (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[6] from our analysis. Additionally, most schools are not in session in June, July, and August. Our primary analysis, therefore, focuses on September, October, January, February, April, and May.

The coefficient of interest, β_1, measures the difference in suspension rates between students on SNAP in Period 3 and Period 2, net of the same difference for students not on SNAP.

[6] November (Thanksgiving), December (winter break), and March (spring break).

As robustness checks, we re-estimate our models, relaxing our fixed effects to school-by-month-by-year and removing them altogether. In these models, the main effect $SNAP_{im(y)}$ is not subsumed.

The coefficient β_2 on PERIOD1 × SNAP should not be thought of as a pre-trend, because Period 1 does not represent a pre-treatment period but instead contains both pre- and post-benefit receipt days for SNAP recipients, depending on their disbursement date.

By including student-by-month-by-year fixed effects, we compare each student's suspension likelihood in Period 3 with their own suspension likelihood earlier in the month (Period 2), differencing out all time-invariant student characteristics and any time-specific trends in discipline. This means that identification 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-by-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. However, we do not think this is a major concern. Section 8 provides further discussion of potential biases related to the timing of family income.

For additional robustness, we re-run the model using alternative clustering approaches, clustering standard errors by student and by school, as well as using heteroskedasticity-robust standard errors.

RQ3: 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, even though many of these students were likely already eligible for free

lunch. The authors argue that reductions in stigma around lunch take-up may explain these effects.

The low-income students identified in Domina et al. include students receiving 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. We next use a triple-difference design to establish whether the benefits of CEP extend specifically to students receiving SNAP. Our identifying assumption requires that, in the absence of CEP, suspension rates for students on SNAP and non-recipient students would have evolved similarly across schools that adopted CEP and those that did not.

Specifically, our triple-difference design identifies the effect of CEP on suspension outcomes for students receiving SNAP by comparing suspension rates for (i) students on SNAP and 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 estimate the following model:

$$susp_{isy} = \alpha_0 + \alpha_1(POST_y * CEP_s * SNAP_{iy}) + \alpha_2(POST_y * CEP_s) + \alpha_3(CEP_s * SNAP_{iy}) + \alpha_4(POST_y * SNAP_{iy}) + \alpha_5 SNAP_{iy} + \alpha_6 POST_y + \gamma_s + \epsilon_{isy} \quad \text{(Model 2)}$$

Because CEP varies at the school-year level, Model 2 is estimated using annual suspension rates, whereas Models 1 and 3 use daily suspension rates. Here $susp_{isy}$ is a binary indicating whether student i at school s received a suspension in year y. $POST_y$ is an indicator for the school years 2015-2019, corresponding to the post-CEP implementation period. 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*. γ_s denotes school fixed effects, which account for time-invariant differences across schools.

The main coefficient of interest is α_1, which captures the effect of CEP on students' suspension rates relative to those not on SNAP.

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. By restricting our sample in this way, we avoid potential methodological concerns related to staggered adoption timing. The resulting sample includes

about 150 middle and high schools and 200 elementary schools that adopted CEP in 2015, as well as approximately 200 middle and high schools and 250 elementary 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.[7]

We estimate an intent-to-treat effect, treating all schools as treated from 2015 to 2019 if they adopted CEP in 2015, even though some schools that adopted CEP in 2015 exited 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 holds in our data.

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.

RQ4: Does expanded free lunch alter the impact of the SNAP benefits cycle?

If students receiving SNAP experience more suspensions later in the benefit month relative to students not receiving SNAP due to food insecurity, providing school meals through CEP may buffer students against these effects. 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 2 with the school-level CEP adoption design from RQ 3. 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

[7] Approximately 500 schools serving grades 6-12 and 350 schools serving grades K-5 were not eligible for CEP, and approximately 60 schools serving grades 6-12, and 70 schools serving grades K-5 were eligible but began CEP in 2016-2019.

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.

Specifically, we estimate the following model:

$$
\begin{aligned}
susp_{idm(y)} = \beta_0 \\
&+ \beta_1 \left(PERIOD3 * \mathrm{POST_y} * SNAP_{im(y)} * CEP_{s(im(y))} \right) \\
&+ \beta_2 \left(PERIOD3 * SNAP_{im(y)} * CEP_{s(im(y))} \right) \\
&+ \beta_3 \left(PERIOD3 * \mathrm{POST_y} * CEP_{s(im(y))} \right) \\
&+ \beta_4 \left(PERIOD3 * \mathrm{POST_y} * SNAP_{im(y)} \right) \\
&+ \beta_5 \left(PERIOD3 * \mathrm{POST_y} \right) \\
&+ \beta_6 \left(PERIOD3 * SNAP_{im(y)} \right) \\
&+ \beta_7 \left(PERIOD3 * CEP_{s(im(y))} \right) \\
&+ \beta_8 \, PERIOD3 \\
&+ \cdots \\
&+ \theta_{im(y)} + \epsilon_{idm(y)}
\end{aligned}
$$

(Model 3)

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

Here β_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. We hypothesize that β_1 will be negative, because CEP will protect students against SNAP-induced food insecurity.

We exclude schools that became eligible for CEP after 2015 or adopted CEP in later years to avoid complications from staggered adoption. We again use student-by-month-by-year fixed effects ($\theta_{im(y)}$). This removes time-invariant student characteristics and month- and year-specific factors affecting discipline. This means that our variation comes from within-student, within-month variation that differs by SNAP receipt and CEP adoption status.

As an additional illustration, we also estimate the within-month SNAP-cycle specification (Model 1) separately for four subgroups:

- CEP-eligible take-up schools before CEP implementation (2010-2015),
- CEP-eligible take-up schools after implementation (2015-2019),
- CEP-eligible non-take-up schools before implementation (2010-2015), and
- CEP-eligible non-take-up schools after implementation (2015-2019).

We compare the estimated effects across the four specifications, which provides us with a direct illustration of whether late-month SNAP-cycle effects change only in CEP-adopting schools following implementation.

5. Results

RQ1: Do students on SNAP experience disproportionate rates of suspension?

We find that 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. We estimate that middle and high school students from low-income families who are not receiving SNAP have an average of 1.05 suspensions per 1,000 students. These differences are stark. Annually, 13 percent of students on SNAP receive suspensions, compared to 6 percent of non-recipient students. For non-recipient, low-income students, this rate is 8 percent.

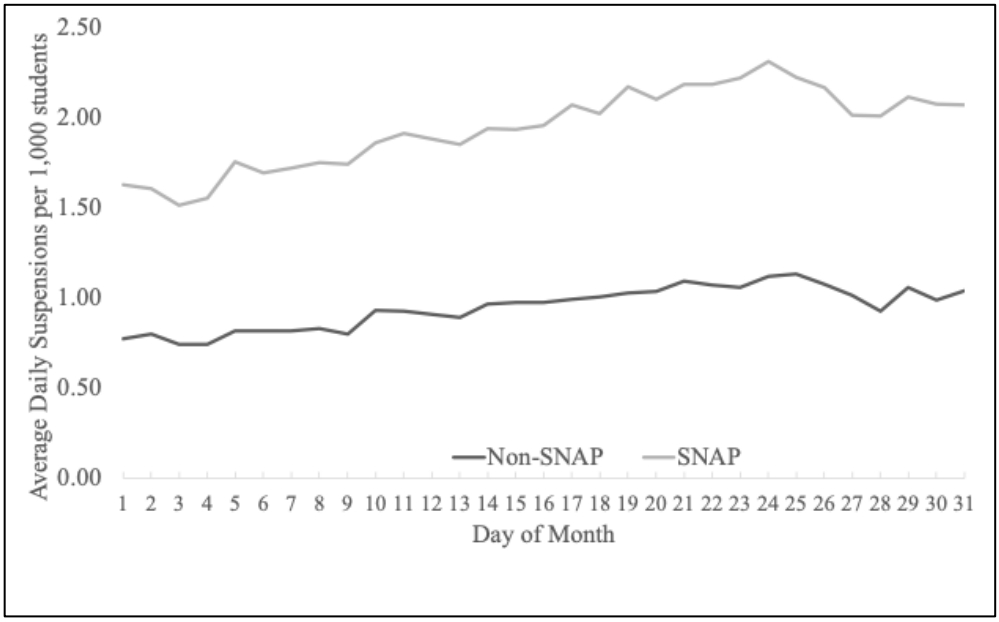
These disparities are particularly high in middle and high schools, where suspensions are more common overall. Across our data, middle and high schoolers receive about 1.09 suspensions per day per 1,000 students compared to .28 for elementary students. Although we focus on middle and high school students in our discussion of results, we also run analyses for elementary school students. Our data show clear evidence that discipline disparities also arise for SNAP participants in elementary schools.

RQ2: What is the effect of the SNAP benefits cycle on suspension rates?

We next investigate whether suspension rates among students on SNAP fluctuate as they move further from benefit receipt. To do so, we examine changes in suspension probability across the benefit month. Figure 2 shows the average daily suspensions per 1,000 students across all study 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 0.6 suspensions per day per 1,000 students, rises gradually, and levels near 1 suspension per day per 1,000 students by the end.

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



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

Next, we estimate Model 1 to find the causal effect of the SNAP cycle on suspension rates. We compare within-student changes in suspension likelihood for students receiving SNAP across Period 2 and Period 3 relative to students not receiving SNAP (see Model 1). Table 2 shows our causal estimates from Model 1, 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 6-12th graders on SNAP have 0.146 additional suspensions per thousand students in Period 3 compared to non-recipients. Across all students on SNAP, the average daily suspension rate in our data is 1.09 (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. Appendix Table 5 shows additional specifications.

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): 6-12th 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, however, at 0.023 suspensions per thousand students in 2010-14 and 0.035 suspensions in 2015-19.

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	173100000	173100000	173100000	181300000	144700000	153300000
R2	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: Observation counts are rounded according to Census Bureau rounding rules. Standard errors in parentheses. *p < 0.05, **p < 0.01, ***p < 0.001.

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 on SNAP, due to the inclusion of student-by-month-of-year fixed effects. 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 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 (Oregon Department of Human Services, 2023), but not every family applies. We find similar results (see Table 3). 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. We also run models separately, using only in-school and only out-of-school suspensions as outcomes, and find similar results, indicating that our findings are not driven by one suspension type.

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: All specifications include school fixed effects, and standard errors are clustered at the school level. Standard errors in parentheses. $p < 0.05$, *$p < 0.01$, **$p < 0.001$.

We additionally run all subgroup analyses shown in Table 3 for elementary students. These results are shown in Appendix Table 6. We find directionally similar results, though of smaller magnitude.

RQ3: Do the established benefits of expanded free lunch on low-income students extend to students on SNAP?

Our third research question examines the effect of CEP on students receiving SNAP. As noted in the review of prior literature, CEP improves educational outcomes (Andreyeva & Sun, 2021; Gordanier et al., 2020; Ruffini, 2022) and has been associated with reductions in disciplinary outcomes (Gordon & Ruffini, 2021; Kho, 2018), including for low-income students in Oregon (Domina et al., 2024). Building on these findings, we ask whether the positive effects of CEP observed among low-income students also apply specifically to students on SNAP.

Error! Not a valid bookmark self-reference. presents the results of Model 2, which estimates the effects of CEP adoption by student income and SNAP status. Model 2 is estimated using annual suspension rates (in contrast to earlier models using daily suspension rates) because

CEP varies at the school-year level. Column 2 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.

Column 3 provides results for average daily suspension rates, which are similar but less 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, but the former estimate is only marginally significant at the $p<0.10$ level.

Table 4 Effects of CEP on Suspension Outcomes by SNAP and Income Measure

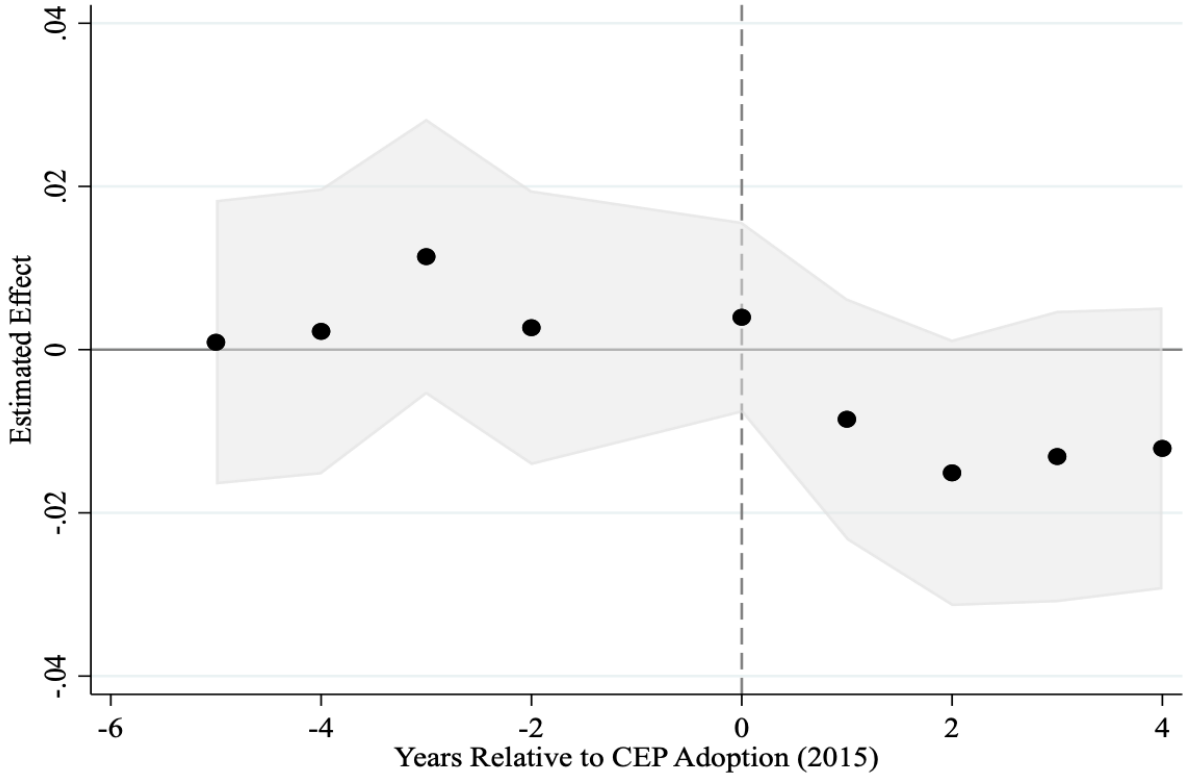
	Ever suspended	**Avg. daily suspensions per 1,000 students**
All students * POST * CEP	-0.0185**	-0.274
	(0.00665)	(0.170)
Students not on SNAP * POST * CEP	-0.0132*	-0.182
	(0.00594)	(0.115)
Students on SNAP * POST * CEP	-0.0245**	-0.376
	(0.00789)	(0.193)
Non-low income students[a] * POST * CEP	-0.0107	-0.147
	(0.00559)	(0.103)
Low income students[a] * POST * CEP	-0.0241***	-0.362*
	(0.00733)	(0.179)

Note: Sample restricted to grades 6-12. 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. $p < 0.05$, *$p < 0.01$, **$p < 0.001$.
[a]Low income is defined as students in households with tax-reported income below 185 percent of the poverty line

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 all close to zero and statistically insignificant, 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.

Figure 3 Event Study



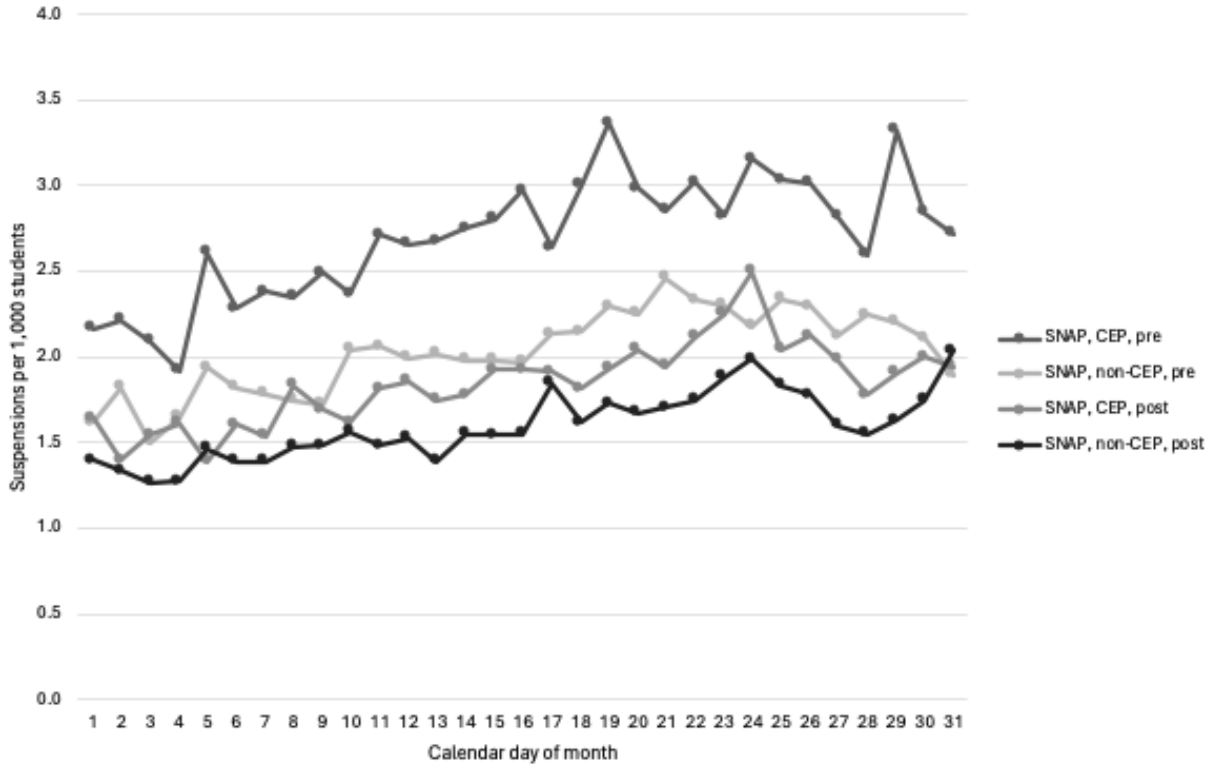
Note: Event-study estimates of the differential change in suspensions for students on SNAP relative to non-recipient students, in CEP schools relative to non-CEP schools, by year. Coefficients are reported relative to the omitted year, 2014. Shaded areas show 95% confidence intervals.

RQ4: 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. All groups continue to show

an upward trend over the 31 days, raising the question of whether CEP moderates the relationship between the SNAP benefits cycle and students' suspension rates.

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



Notes: 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 ≥90 days. Suspensions from other schools are excluded. Suspensions in June and July are excluded.

Table 4 reports estimates separately for the pre-CEP period (2010-2014) and the 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,

and the relationship between SNAP and increased discipline in Period 3 does not disappear in eligible schools after CEP roll-out.

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 weaker SNAP benefit-cycle effects in the pre-period, which appear to increase after the implementation of the universal free lunch program. This suggests that CEP does not alter the benefits cycle's impact, even as it reduces overall suspension rates for students on SNAP.

Table 4 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)

Note: All specifications include school fixed effects, and standard errors are clustered at the school level. Standard errors in parentheses. p < 0.05, *p < 0.01, **p < 0.001.

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 7 presents the results 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 on β_1 is statistically insignificant and positive, 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 **Error! Reference source not found.**).

6. Limitations

One important limitation of our analysis is that other income sources and many regular household expenditures (e.g., rent) tend to occur at the beginning of the calendar month (C. D. Cotti et al., 2021). If these financial constraints are specific to families receiving SNAP, this would make it difficult to fully disentangle the SNAP cycle from cycles in other income streams.

Work by Beatty et al. (2019) finds that the cycle may be more pronounced for workers paid monthly, but there is little evidence that cycles in other income streams (such as other welfare benefit payments or bi-monthly paychecks) affect the SNAP cycle. For this reason, it is important to investigate whether the parents of students on SNAP are more likely to be paid monthly than those of comparison students. We do not observe pay frequency in our data. Although Oregon law requires workers to be paid at least once every 35 days (Oregon Bureau of Labor and Industries, 2024), national data from the U.S. Bureau of Labor Statistics indicate that only about 10 percent of workers are paid monthly, compared with 43 percent paid biweekly, 27 percent paid weekly, and 20 percent paid semimonthly (U.S. Bureau of Labor Statistics, 2023). Moreover, SNAP recipients most commonly report working in healthcare, hospitality, retail, and construction (Center on Budget and Policy Priorities, 2018), sectors in which only a small share of workers are paid monthly (8.1 percent in education and health services, 5.5 percent in leisure and hospitality, and 5.9 percent in construction). This suggests that most SNAP recipients are not paid monthly – in fact, they may be less likely to be paid monthly than the average American. It is, however, important to note that non-SNAP recipients being paid monthly could induce a similar cyclical experience of financial hardship in our comparison group. However, this would bias our estimate toward zero, leading us to understate the impact of SNAP on end-of- benefit-month disciplinary outcomes for students.

While our analysis is the first we know of to estimate the impact of SNAP cycles on discipline using panel data from an entire state, Oregon is unique in some ways. It is a very large

state with a relatively low population density and a slightly higher poverty rate than the national average (HRSA, 2024). For this reason, our findings may not generalize to denser or more urban contexts.

7. 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 that the introduction of the Community Eligibility Provision (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. 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 (A. Gregory et al., 2010; Skiba et al., 2014; Welsh & 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 et al. 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 (which categorically includes students on SNAP) 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. 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.

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 it primarily reduces school lunch stigma, 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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9. Appendix

Table 5 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	173100000	173100000	173100000	173100000	173100000	81300000	181300000	181300000	181300000
r2	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

Table 6 Effects of SNAP Timing on Suspension Outcomes by Gender and Income, 2010-2014

	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: All specifications include school fixed effects, and standard errors are clustered at the school level. Standard errors in parentheses. $p < 0.05$, *$p < 0.01$, **$p < 0.001$.

Table 7 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 1	-0.176***	-0.199***	-0.156***
	(0.0149)	(0.0221)	(0.0199)
Period 3	0.108***	0.119***	0.0981***
	(0.0143)	(0.0215)	(0.0192)
Period 1 * SNAP	-0.170***	-0.143**	-0.194***
	(0.0303)	(0.0448)	(0.0409)
Period 3 * SNAP	0.128***	0.177***	0.0807*
	(0.0287)	(0.0426)	(0.0388)
Period 1 * CEP	-0.0509	-0.0231	-0.0784
	(0.0364)	(0.0501)	(0.0519)
Period 3 * CEP	0.105**	0.127**	0.0644
	(0.0346)	(0.0481)	(0.0487)
Period 1 * SNAP * CEP	-0.0835	-0.120	-0.0423
	(0.0646)	(0.0877)	(0.0940)
Period 3 * SNAP * CEP	-0.0550	-0.0233	-0.186*
	(0.0612)	(0.0832)	(0.0890)
Period 1 * Post	0.0772***	0.0574	0.0945***
	(0.0202)	(0.0307)	(0.0264)
Period 3 * Post	-0.00818	-0.00531	-0.0106
	(0.0192)	(0.0294)	(0.0252)
Period 1 * SNAP * Post	0.114**	0.0441	0.176***
	(0.0401)	(0.0608)	(0.0530)
Period 3 * SNAP * Post	0.00399	-0.0743	0.0770
	(0.0381)	(0.0578)	(0.0503)
Period 1 * CEP * Post	-0.0150	-0.0691	0.0668
	(0.0489)	(0.0700)	(0.0658)
Period 3 * CEP * Post	-0.0831	-0.101	-0.0542
	(0.0470)	(0.0682)	(0.0617)
Period 1 * SNAP * CEP * Post	0.0792	0.180	-0.0125
	(0.0836)	(0.118)	(0.115)
Period 3 * SNAP * CEP * Post	**0.0283**	**0.0831**	**0.0312**
	(0.0795)	**(0.112)**	**(0.109)**
Constant	1.357***	1.580***	1.126***
	(0.00556)	(0.00839)	(0.00724)
Observations	139500000	71070000	68410000
r2	0.0698	0.0730	0.0651
Grade range	6-12	6-8	9-12
Year range	2010-2019	2010-2019	2010-2019
FEs	Student-by-month	Student-by-month	Student-by-month
Cluster SE	Student	Student	Student
Sample	Main	Main	Main
Outcome	Suspensions/1k	Suspensions/1k	Suspensions/1k