School Food Policy Affects Everyone: Retail Responses to the National School Lunch Program*

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Abstract

We study the effect of school lunch provision on local business under the 2010 Healthy, Hunger-Free Kids Act. The act introduced a Community Eligibility Provision (CEP), which requires that participating schools offer lunch free of charge to all students. Schools may adopt the CEP if at least 40% of students participate in other means-tested welfare programs. We exploit the discontinuity in CEP eligibility for estimation, and find that state adoption of CEP leads to a 10-17% decline in grocery sales. Results suggest that the impact of the CEP demand shock propagates spatially: chains that are highly exposed to the CEP lower prices on the order of 5% across all outlets. Further, retail chains are less likely to open new outlets in areas that adopt the CEP. Using a stylized model of grocery demand, we estimate the welfare that this price reduction delivers as an indirect benefit to all households in affected markets.

1 Introduction

This paper studies the supply-side effects of the National School Lunch Program (NSLP), a large food security program in the United States. The NSLP provides meals to children through schools, serving some 30.4 million children in 2016.[1][2] A long literature documents the benefits of the NSLP for students, which include higher attendance and test scores

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[1] For comparison, the largest domestic hunger safety net program, the Supplemental Nutrition Assistance Program (SNAP), gave an average of $125.40 on debit cards to some 44 million recipients in 2016. SNAP restricts allotment spending (e.g. alcohol and to tobacco do not quality), but is far less restrictive than the NSLP.

[2] USDA NSLP Fact Sheet, November 2017.

(Schwartz and Rothbart (Forthcoming), Frisvold (2015), Schanzenbach (2009), and Bhattacharya and Haider (2006)). The typical concerns about in-kind giving through programs like the NSLP concern efficiency; families might spend funds more efficiently than central planners and firms may prepare meals more efficiently than schools. We examine a less-studied consequence of in-kind giving: the effect of public provision on the private market. In the lunch context, parents may spend less at supermarkets if their children receive free meals at school. Grocery stores in turn may adjust product prices and assortment. If the shock is sufficiently large, it may also affect firm entry and exit decisions. This paper presents evidence on the sign and magnitudes of these indirect effects: we find that the NSLP reduces revenue, prices, variety, and entry. Importantly, and in line with recent descriptive evidence that prices do not vary substantially within chain (DellaVigna and Gentzkow (2019), Adams and Williams (2017), Hitsch et al. (2019)), we find that price effects propagate through retail chains, generating disparities in the spatial distribution of the program's indirect effects. Overall, our estimates suggest that supply responses amplify the program; we find that the indirect benefits enjoyed by all households amount to 20% of the direct benefit enjoyed by those families receiving free lunch.

We focus on the Community Eligibility Provision (CEP), an expansion of the NSLP under the Healthy, Hunger-Free Kids Act of 2010. Historically, schools collected lunch applications from families to verify individual student eligibility for free or reduced-price lunch. Schools that adopt the CEP need not collect applications, but rather serve free lunch to all students. The aim of the CEP is to reduce the administrative burden of the lunch program in high-poverty areas, where many students qualify for free lunch under older provisions. In practice, if at least 40% of its enrolled students are "identified" - that is, the students qualify for other means-tested welfare programs - then a school qualifies for the CEP. To be clear, a school where 40% of students qualify for free lunch under standard NSLP provisions now may offer 100% of students free lunch - a 150% increase in the number of free-lunch eligible students. In the 2016-2017 Academic Year, 20,721 schools participated in NSLP under the CEP in 3,538 school districts, with a combined enrollment of 9.7 million students (Food Research & Action Center, 2017). A first finding is that the resultant demand shock has a detectable effect on the private market; we find that school adoption reduces revenues at neighboring stores by 10-15% and that following adoption, expenditures by households with school-aged children fall by 15% relative to households without children.

The paper then focuses on understanding how grocery stores respond to the CEP demand shock. We present a simple model of retailer profit maximization, where the CEP serves to reduce the price of a substitute product. If prices are strategic complements, then the retailer ought to lower the price of store-bought lunch in response to the CEP, generating consumer surplus for adults and children who do not benefit from the CEP directly. This prediction is consistent with Leung and Seo (2018), who find that grocery prices increase with Supplemental Nutrition Assistance Program (SNAP) take-up, and Cunha et al. (2015), who find that the prices of grocery products fall following the institution of a food delivery program in rural Mexico. However, if prices are strategic substitutes, retailers might instead increase prices. Higher prices would dovetail with findings from the pharmaceutical industry, wherein many branded drugs increase prices when generics enter at low prices (Frank and Salkever (1992)). Whether prices are strategic complements or substitutes depends on whether a reduction in the price of school lunch increases or decreases the own-price demand elasticity for groceries. We marry this observation to an insight from the Industrial Organization literature: namely, that grocery retail chains often employ zone pricing policies (DellaVigna and Gentzkow (2019), Hitsch et al. (2019), and Adams and Williams (2017)). The CEP presents an opportunity to test the zone-pricing theory by examining the extent to which firm pricing responds to the local adoption of the CEP and/or chain-level exposure to the program.

A central concern in estimating the causal effect of the CEP demand shock on grocery supply is the selective adoption of schools into the CEP. Because school participation is elective, schools that serve communities where children receive little nutrition at home may be the most likely to join, inducing a correlation between uptake, local grocery revenue, and local grocery prices. We employ two strategies to identify the causal effect of the CEP on grocery stores. First, we compare purchases of households with and without children before and after their local school(s) adopt the CEP. Because adults do not receive food through the NSLP, adult-only households serve as a control group in this comparison. The second strategy exploits two aspects of the program to mitigate endogeneity concerns: the discontinuity in school eligibility at 40% and the staggered roll-out of the program, which became available to different states between 2011 and 2014.

Our findings suggest that grocery prices fall with chain exposure to the CEP. A 6 percentage point increase in chain exposure leads to a 5% reduction in price across all stores in the chain. In contrast, we find no evidence that prices respond to local adoption of the CEP. To our knowledge, this paper is the first to provide empirical evidence that zone pricing dampens price responses in areas where chain exposure is low, but local adoption is high (and vice versa). Our findings further suggest that the CEP affects the entry of retailers. That is, the local market under the CEP supports 3-5% fewer grocery stores, based on panel data from the Census on establishment counts by ZIP code. Because firm exits and entries are lumpy and may react with a lag, the long-run effects could be even larger. Lower prices and reduced entry is consistent with Jaravel (2018), who finds that state-wide SNAP take-up correlates with lower prices and greater product variety.

The large price and entry effects that we document have implications for the welfare effects of policies like the NSLP. To shed light on welfare, we estimate a stylized model of grocery demand. We focus on the indirect effects of the CEP on households located in markets that adopt the CEP or live near chains with high exposure to the CEP demand shock. These households may benefit from the CEP – even if they do not have school age children – through lower prices, but may also suffer from lower product variety and reduced store entry. Using a CES model of demand for groceries, we find that these indirect effects of the CEP are welfare-enhancing for the average household. We note that the use of chain-level pricing facilitates identification of price and distance elasticities separately, and might provide an additional source of variation in other policy analysis.

The rest of the paper proceeds as follows. Section 2 details the Community Eligibility Provision and explores its incentives. Section 4 describes the data. Section 5 contains our estimation strategy and results on revenues. Results on entry, exit, pricing, and assortment are presented in section 6 and Section 7 quantifies the welfare impacts of these changes in local retail environments. Section 8 concludes.

2 Background on the National School Lunch Program

Since 1946, the US Department of Agriculture has administered the National School Lunch Program (NSLP), which provides nutritious, low-cost meals to students in both public and not-for-profit schools. The program is large; it served some 30.4 million children in 2016. Participating schools receive reimbursements from the Federal government for meals served to low-income children. Schools may also receive food directly from the USDA. In return, school meals must meet certain nutritional requirements, and low-income students must receive free or reduced rates (40 cents). Paid rates, which are set locally, averaged \$2.63 in the 2016-2017 academic year.[3] Students can qualify for free or reduced price lunches "categorically" if they or their family participate in another means-tested welfare program, including the Supplemental Nutrition Assistance Program. Students can also qualify based on household income and family size, as follows: those below 130% (130%-185%) of the Federal poverty line are free- (reduced-price) lunch eligible.[4] To qualify for free or reduced-price lunch based on income, families must submit an application to their school or district. In the 2017-2018 academic year, reimbursements were on the order of \$3.31 per meal served to a free-lunch student, \$2.91 for a reduced-price student, and \$0.31 for a paid student.[5] The Community Eligibility Provision, described below, aims at reducing the administrative burden of providing free lunch for high-poverty schools (including application collection and processing).

The CEP is the fourth provision of the National School Lunch Act, rolled out at the state-

[3]School Nutrition Association, "School Meal Trends & Stats."

[4]The National School Lunch Program Fact Sheet, USDA, 2017

[5]Schools in Alaska, Hawaii, and Puerto Rico receive additional payments. Schools may also receive an addition 6 cents per pupil if they comply with additional USDA standards. See: Federal Register, July 2018, Vol 83, No 139.

level between 2011 and 2014 according to the schedule depicted in Table 1 (USDA 2016). The CEP provides participating schools partial reimbursement for meals served, in return for which it requires that participating schools offer all enrolled students free lunch, regardless of each student's individual financial circumstances. Participating schools are reimbursed for meals served according to the school's Identified Student Percentage (ISP), the proportion of students who qualify for the NSLP categorically. The per-pupil reimbursement rate is proportional to 1.6 times the school's ISP. Any school with an ISP above 62.5% therefore receives the maximum per-pupil funding, while those below must fill the gap with state or local funding. Central to our identification strategy presented in section 5, a school must have an ISP at or above 40% to qualify for the provision unilaterally. Schools can also participate as part of an LEA (Local Educational Agency). That is, a school that does not qualify individually can pool with higher-poverty schools, so long as their pooled ISP exceeds 40%.

Table 1: Roll-out of the Community Eligibility Provision

Initial Participation Year	State
2011-2012	Illinois, Kentucky, Michigan
2012-2013	New York, Ohio, West Virginia, the District of Columbia
2013-2014	Georgia, Florida, Maryland, Massachusetts
2014-2015	Remaining States

3 How the CEP Might Affect Retail Pricing

In this section, we illustrate how CEP adoption might affect local grocers, beginning with the household decision of what to buy for lunch. Suppose that each household with school age children chooses between school lunch at price p_S and grocery store-bought lunch at price p_G. Let $q(p_G, p_S)$ represent the household's demand for grocery-bought lunch. The Community Eligibility Provision affects grocery demand because it lowers p_S for some households (those that do not qualify for free lunch under older provisions of the NSLP). Holding grocery prices fixed, the CEP reduces the sale of store-bought lunch so long as the cross-price elasticity is positive:

$$\frac{\partial q(p_G, p_S)}{\partial p_S} > 0.$$

To understand how the CEP affects supermarket pricing, we aggregate the demand for grocery-bought lunch across households with and without children to $Q(p_G, p_S)$. (For simplicity, suppose that the supermarket sells only a single lunch product.) Grocery store profits are then $\pi(p_G, p_S) = (p_G - c) \cdot Q(p_G, p_S)$ where c is marginal cost. If grocery and school lunch are strategic complements (substitutes), then grocery stores ought to reduce (increase) prices in response to the CEP. Complementarity/substitutability depends on the sign of $\frac{\partial^2 \pi}{\partial p_G \partial p_S}$. The first derivative of store profit with respect to own-prices gives the familiar first order condition for a Nash Equilibrium in prices:

$$\frac{\partial \pi}{\partial p_G} = Q(p_G, p_S) + (p_G - c) \cdot \frac{\partial Q(p_G, p_S)}{\partial p_G} = 0. \tag{1}$$

The cross-derivative is then:

$$\frac{\partial^2 \pi}{\partial p_G \partial p_S} = \frac{\partial Q(p_G, p_S)}{\partial p_S} + (p_G - c) \cdot \frac{\partial^2 Q(p_G, p_S)}{\partial p_G \partial p_S}.$$

We expect that the first term is positive, which is to say that as the price of school lunch falls, fewer households will purchase store-bought lunch (aka a positive cross-price elasticity). Therefore, prices are strategic complements unless the cross-partial $\frac{\partial^2 Q(p_G, p_S)}{\partial p_G \partial p_S}$ is large and negative. In standard demand models such as the logit, demand becomes more elastic as the market share of a product falls, but we could see the opposite if a decline in the price of school lunch leaves only the wealthiest (demand inelastic) families shopping for lunch at grocery stores. Thus, we turn to empirical analysis to understand how grocers respond to the CEP in practice.

Finally, we note that this intuition carries over to a model that incorporates zone pricing. Recent evidence suggests that retail outlets do not price according to equation (1), but instead that chain firms set uniform prices to maximize total profits across all outlets. In the CEP context, prices then ought to respond to the retail chain's overall exposure to the program. From the chain's perspective, the CEP reduces the average price of school lunch $\bar{p_S}$ in proportion to program take-up across markets where the chain competes. Uniform pricing implies that prices at some outlets may respond to the CEP even if its nearby schools do not participate in the program. We will test for a zone-pricing response in the empirical analysis below.

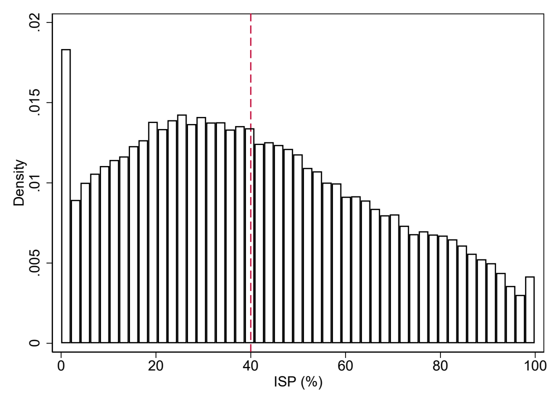
4 Data

Below we describe the three datasets used in our main analysis: school-level CEP participation and eligibility, purchases and demographics of nearby households, and sales of nearby stores. We complement these data with ZIP-level store counts from the County Business Patterns (CBP) provided by the Bureau of Labor Statistics (BLS) and tract-level demographic data from the American Community Survey (ACS).

4.1 Program Participation and Eligibility

Our primary data on CEP participation comes from the National Center for Education Statistics (NCES). This data spans three academic years: 2013/2014, 2014/2015, and 2015/2016. The NCES contains 70,555 schools across fifty states. We collect earlier participation data from the seven states that adopted the program in 2011/2012 or 2012/2013 from websites and FOIA requests of the individual state Departments of Education.[6]

Figure 1: FRL from the NCES Data Set for the 2009-2010 School Years



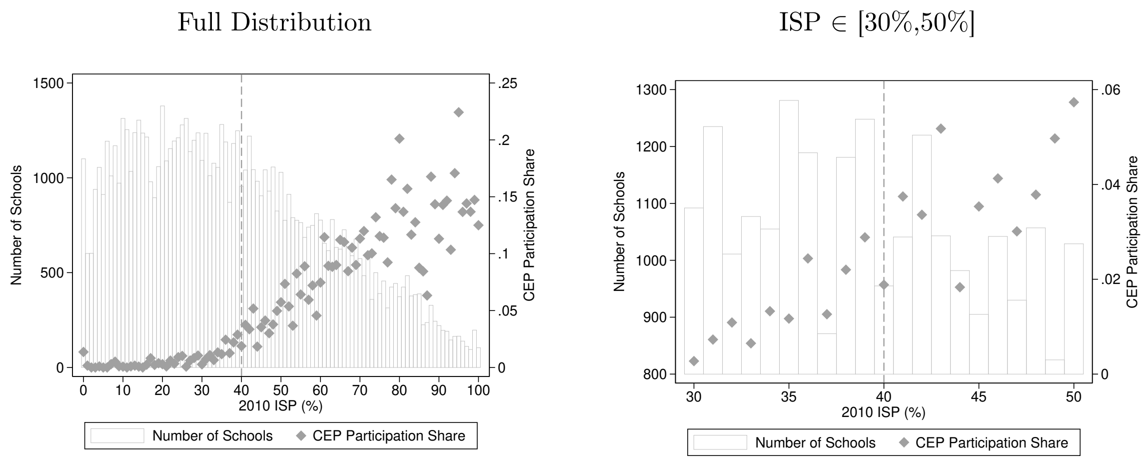
To measure individual school eligibility for the program, we therefore collect school-level student enrollment and free lunch eligibility rates for school years from 2009/2010 through 2015/2016 from the NCES. We use the fraction of students who qualify for free and reduced lunch as a proxy for each school's identified student percentage (ISP). This measure overestimates the true ISP because it includes students who qualify both through direct certification and family income.[7] It is therefore a conservative measure of individual school eligibility. We favor 2010 ISPs because in the following year, some schools adopt the CEP, and thus may report that 100% of their students are free-lunch eligible (as all students do indeed receive free lunches). Further, because CEP schools do not collect lunch applications, there is no record of which students would have successfully applied under the income requirements. Figure 1 shows the distribution of ISPs across schools in 2009-2010, and does not reveal any perceptible bunching above the 40% cutoff. We formally test for manipulation using

[6] For all states, we attempted to gather school-level data on the Identified Student Percentage (ISP), which is used to determine school eligibility for the program. Unfortunately, this data is not available from the early years of the program, and that initially, districts were required to report ISPs only for eligible schools, truncating the distribution of observed ISPs from below.

[7] A student may be directly certified if their family receives SNAP, FDPIR, TANF; if the student is enrolled in a Head Start program; if the student is homeless, runaway, migrant, or a foster child. (USDA, *Community Eligibility: Planning and Implementation Guidance,* January 2016.)

the methodology in McCrary (2008), and cannot reject a null hypothesis of no break in the density of ISPs at 40% (the discontinuity estimate is 0.030 with a standard error 0.024) or 62.5% (p-value of XX). For simplicity, throughout the rest of the paper, we refer to this measure as ISP.

Figure 2: School CEP Adoption Share

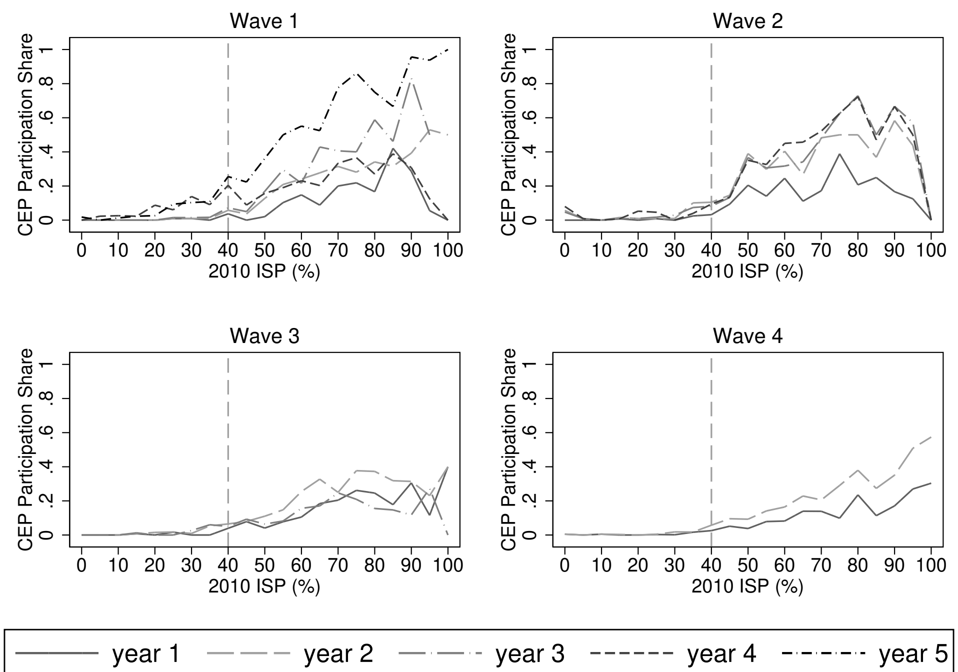


Notes: Each point reflects the share of schools within a given percentile of ISP that participate in the school lunch program. The right-hand plot focuses in on the ISP bins between 30 and 50.

Figure 2 plots average participation rates for schools at each percentile of ISP for the 2014-2015 Academic Year. Participation rates hover right around zero until 30%, and then begin to slope upward. Visual inspection does not suggest a discontinuous increase in participation in the CEP right at 40%. There are two possible reasons that the participation rate is smooth at the 40% ISP cutoff. First, using a proxy for ISP ought to smooth any jump at the threshold (Pei and Shen (Forthcoming)); and second, schools with ISPs below the threshold may participate if they do so as part of a group of schools where the pooled ISP exceeds 40%. For the state of Wisconsin, we collected actual ISP data as well as information on whether schools participate in the CEP individually or as part of a group in the 2016-2017 school year. In this data, there is a sharp discontinuity in individual-school CEP participation, and no schools with an ISP below 40% participate in the program individually. Interestingly, there is no bunching evidence in the Wisconsin data, although the pattern is clear for other states. Thus, in appendix section 2, we use these Wisconsin data to implement a fuzzy regression discontinuity design to study the response of lunches served to the CEP in Wisconsin.

Our chief identification strategy involves a difference-in-difference methodology that exploits data on a wider sample of schools (not just those near the threshold) in years before and after their state introduces the CEP (dates are provided in table 1). The staggered introduction of the CEP across states provides within-year variation in exposure to the program. Figure 3 plots take-up against ISP separately by number of years since introduction for different cohorts. The patterns indicate a lag in take-up among eligible schools. Adoption is higher for schools in states in earlier waves, suggesting that schools are more likely to adopt the provision after the initial year of eligibility. Both average adoption rates and the gradient of adoption and ISP appear more pronounced in the second year and beyond. Interestingly, schools in the second wave of state adoption (New York, Ohio, West Virginia, and Washington, DC) display the strongest kink in participation rates at 40%; these may be states where our NCES ISP proxy aligns more closely with the true ISP.

Figure 3: School CEP Adoption Share by State Adoption Wave



Note: Wave 1: 2011-2012 (Pilot year. Illinois, Kentucky, Michigan); Wave 2: 2012-2013 (New York, Ohio, West Virginia, DC); Wave 3: 2013-2014 (Georgia, Florida, Maryland, Massachusetts); Wave 4: 2014-2015 (all other states, CEP adopted nationally).

4.2 Household spending data

To measure the impact of the CEP on household spending, we use data from Nielsen's Homescan, which contains information on all grocery purchases for a panel of American households from 2011-2016. Table 2 reports summary statistics for the 43,945 panelist households, of which 20% of contain at least one school-age child. Crucially, the dataset contains each household's ZIP code. Because we do not know the identity of the school that children within the household attend, we instead measure each household's exposure to the CEP as the average eligibility and participation of schools within the same ZIP code where the household resides. The results presented below are robust to measuring eligibility with the average ISP in the household's ZIP code.

Table 2: Household Summary Statistics for AY 2014-2015

	Mean	Std.	Min	Max
Household Spending				
Total Spending ($ hundreds)	37.07	26.58	0.01	567.05
Total Grocery Spending ($ hundreds)	19.22	17.67	0.01	221.84
Total Lunch Meat Spending ($ hundreds)	0.34	0.50	0.00	10.95
Household Characteristics				
Household Size	2.41	1.32	1	9
Household Income ($ thousands)	65.58	43.64	2.5	150
Share of Household with School Age Childeren	0.20	0.40	0	1
Share of White	0.81	0.39	0	1
Share of Black	0.10	0.30	0	1
Share of Asian	0.04	0.18	0	1
Share of Other Race	0.05	0.21	0	1
Count	43,945			

Notes: Spending is calculated on an annual basis from September - August in order to align with the academic calendar.

4.3 Store sales data

The third data source crucial for our analysis is the Nielsen Scantrack data, which contains weekly sales and quantities by product (Universal Product Code or UPC) collected by point-of-sales systems located in over 20,000 participating stores across the US in 2011 and 2016.[8] For each store i, we calculate exposure to the CEP as the average participation and eligibility of the schools that share the same ZIP code.[9]

Table 3 contains summary statistics on stores as well as the schools in our sample in the 2014/2015 school year. We aggregate store revenue to the annual level, with each year of sales running from September to the following August to reflect the academic calendar. Sales for the 2014-2015 school year, for example, range from September 2014 to August 2015. We also calculate revenue for breakfast and lunch foods separately, where we hand-code product categories based on Nielsen descriptions. See Appendix Table 11 for a list of breakfast and lunch foods groups. In our main analysis, we focus on grocery stores because they account for the lion's share of breakfast and lunch spending.

[8]The Nielsen data is provided by the Kilts-Nielsen Data Center at the University of Chicago Booth School of Business; refer to http://research.ChicagoBooth.edu/nielsen for information on availability and access.

[9]We also limit the sample to stores for which we have a breakdown of sales by breakfast and lunch foods.

Table 3: Summary Statistics for the 2014-2015 School Year

	All Stores				Grocery Stores			
	Mean	Std.	Min	Max	Mean	Std.	Min	Max
Store Characteristics								
Annual Revenue ($ millions)	0.65	0.85	0.00	10.26	1.55	0.90	0.12	10.26
Lunch and Breakfast Revenue ($ millions)	0.16	0.26	0.00	3.06	0.49	0.28	0.00	3.06
Lunch Meat Revenue ($ millions)	0.01	0.01	0.00	0.18	0.02	0.01	0.00	0.18
Count	21,412				6,160			
Zipcode Characteristics								
Average NCES ISP Proxy (% of enrollment)	48.27	22.83	0.00	100	43.58	22.72	0.00	100
Average I[NCES ISP > 40%]	0.59	0.40	0.00	1	0.52	0.40	0.00	1
Average Participation Rate (%)	14.37	29.67	0.00	100	10.00	25.27	0.00	100
Household Annual Income ($ thousands)	54.18	22.89	3.37	244.53	54.18	22.89	3.37	244.53
Share of African American Population	9.22	17.04	0.00	100	9.22	17.04	0.00	100
Number of Stores	12.94	13.86	0.00	174	3.03	4.70	0.00	125.00
Count	19,619							
Chain-Level Characteristics								
Average NCES ISP Proxy (% of enrollment)	42.85	8.13	9.20	69.31	38.45	9.96	9.20	69.31
Average I[NCES ISP > 40%]	51.10	13.51	5.32	94.23	44.46	16.56	5.32	94.23
Average Participation Rate (%)	10.25	5.12	0.00	51.28	7.53	7.05	0.00	51.28
Count	87				70			

5 The CEP Demand Shock

We begin by documenting how the CEP changes demand for groceries. We first present evidence that the direct beneficiaries of the program (households with school-aged children) reduce their expenditures following the introduction of the CEP. We next present evidence on how the CEP affects supermarket revenues using the discontinuity in CEP eligibility rules and the Kilts Nielsen ScanTrak data on store sales. Appendix section B.2 provides further evidence that the CEP increases the number of lunches served at adopting schools using Wisconsin as a case study. The validity of each of these three strategies rests on different identification assumptions, each with their own vulnerabilities. Together, however, they paint a compelling picture of the impact of the CEP on grocery demand.

5.1 Evidence from Households

In this subsection, we provide direct evidence that households with school-age children reduce their grocery expenditure when their neighboring schools adopt the Community Eligibility Provision. Because adoption may be correlated with other, time-varying factors that directly affect grocery spending, we use adult-only households as a control group for those households with school-age children (only the latter are directly affected by the CEP). The identification assumption is that these households are equally affected by any relevant time-varying factors.

Our regression specification is:

$$\ln E_{ht} = \gamma_0 + \gamma_1 \cdot CEP_{s(h),t} + \gamma_2 \cdot CEP_{s(h),t} \times Kid_h + \Gamma_h + \Omega_t \times State_h + \epsilon_{ht} \quad (2)$$

where E_{ht} is the sum of household h's expenditures in year t; Γ_h are household fixed effects, which control for time-invariant differences in spending across households; Kid_h is an indicator for whether household h includes a school-aged child; $CEP_{s(h),t}$ is the weighted average CEP adoption of the nearest elementary, middle, and high schools to household h's ZIP centroid at time t. Results are presented in table 4. In odd numbered columns, we include a triple interaction of $CEP_{s(h),t} \times Kid_h \times \ln(Income_h)$, with an eye towards understanding which households substitute from store-bought lunch to school lunch when the CEP becomes available. In theory, the lowest income households already receive lunch for free, and so adoption of the CEP for this group operates only through stigma. In contrast, for higher income households, the CEP changes the relative price of two goods.

The baseline results, in column 1 of table 4, suggest that the CEP reduces household grocery store expenditures by 4.6-7.6%, which is both economically and statistically significant (approximately $100 for households in our sample). The results do not speak to heterogeneity across income groups: the triple interaction term in column 2 is statistically insignificant, but the confidence interval is wide. Columns 3 and 4 shows similar effects for product modules that we categorize as breakfast and/or lunch foods (see appendix table 11 for a list of products). Columns 5 and 6 show that CEP adoption lowers the number of shopping trips undertaken by households with school-aged children by 4.8-6.4%.

Table 4: Effect of CEP on Household Spending

	Food Expenditures		B/L Expenditures		Number of Grocery Trips	
	(1)	(2)	(3)	(4)	(5)	(6)
CEP	0.020*	0.027**	0.015	0.022*	0.010	0.015*
	(0.011)	(0.011)	(0.012)	(0.012)	(0.008)	(0.008)
CEP x School Age Kid	-0.047*	-0.079***	-0.034	-0.073**	-0.049**	-0.066***
	(0.027)	(0.028)	(0.028)	(0.029)	(0.021)	(0.021)
CEP x lnIncome		0.026**		0.024*		0.030***
		(0.013)		(0.014)		(0.010)
CEP x School Age Kid x lnIncome		-0.049		-0.050		-0.020
		(0.034)		(0.034)		(0.028)
R-Squared	0.856	0.856	0.847	0.847	0.869	0.87
Observations	262,901	262,901	261,767	261,767	263,592	263,592

Notes: Standard errors, clustered at the ZIP level, in parentheses. All specifications include state-by-year and household fixed effects. All outcomes are measured in logarithms.

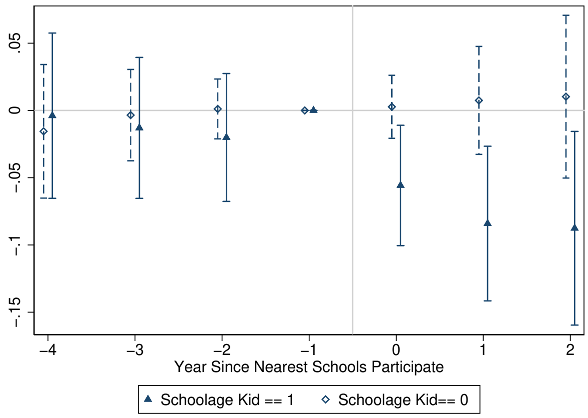
Figure 4 illustrates this difference-in-differences strategy by showing the difference in spending between households with and without kids each year before and after their local school adopts the CEP.[10] Before adoption, the spending difference between households with school-aged children relative to those without is fairly stable. Consistent with a causal effect,

[10]For visual simplicity, we code each household's exposure as binary here by considering a households treated if its nearest school adopts the CEP.

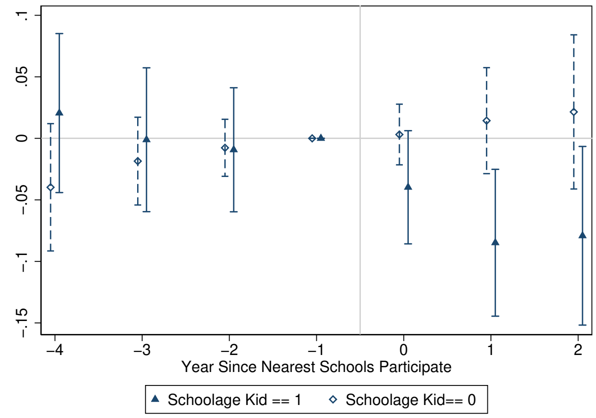
statistically detectable differences emerge only after the CEP is introduced.

Figure 4: Spending Differences between Household with/without a School-Aged Child

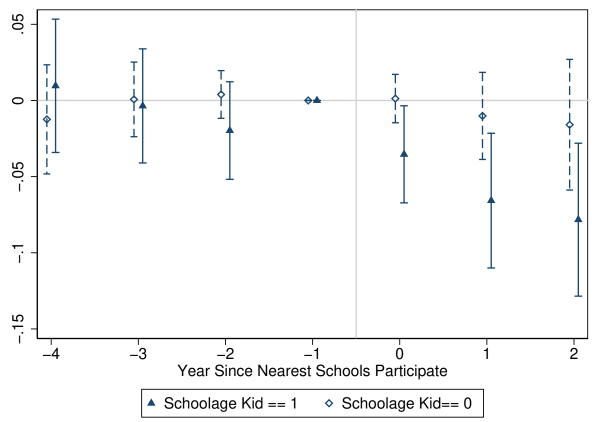
(a) Grocery - Food



(b) Grocery - Breakfast/Lunch



(c) Grocery - Number of Trips



Notes:Using 2007–2017 Homescan data, these figures present the treatment effect of CEP participation on household food and breakfast/lunch expenditures and number of grocery shopping trips. The data includes only the households with the nearest school that has ever participated in the program. All regressions control for state by year and household fixed effects. Standard errors are clustered at zip level. The academic years beyond [t-4, t+2] are binned up on both ends to ensure balanced panel of data, where t denotes the year of participation.

5.2 Evidence from Scanner Stores

We next document that these reductions in grocery expenditures by households with school-age children are associated with aggregate declines in supermarket revenues. Because CEP adoption is endogenous to local economic conditions, we exploit the discontinuity in CEP eligibility rules to identify how the program affects store sales. Though we posit that the estimated impact of CEP adoption on store revenues is driven by the demand shock, the impact of the program on store revenues also reflect supply-side responses (for example, price changes) that are not captured in the household estimation strategy. These responses are the subject of section 6.

5.2.1 Econometric Model

We model the log revenue of store i in year t as a function of the CEP participation rates of neighboring schools (in the same ZIP code) in that year. We also include store fixed effects Ω_s and year-by-county fixed effects $\Gamma_t \times \Delta_{county(i)}$ to pick up any time-invariant drivers of store revenue (e.g. store size) and any county-wide shocks to spending. Our baseline specification is therefore:

$$y_{it} = \beta_0 + \beta_1 \cdot CEP_{it} + \Omega_i + \Gamma_t \times \Delta_{county(i)} + \epsilon_{it}. \tag{3}$$

If households reduce grocery spending when their local school adopts the CEP, then $\beta_1 < 0$.

We present estimates of (3) in column 1 of table 5. The coefficient on the CEP adoption rate suggests that neighboring school participation reduces grocery store revenue on the order of 5%. The drop is 7.5% larger for the breakfast/lunch category, and even larger for lunch meat in particular. We hesitate to interpret the estimates of equation (3) as causal because participation in the CEP is elective. Our concern is that schools might select into the program on the basis of local economic trends (more granular than at the state level), potentially confounding OLS estimates. To identify a causal effect of the CEP, we adopt a difference-in-differences approach.

Table 5: OLS Results of Store Revenue on CEP Adoption

	(1) All	(2) B/L	(3) Lunch Meat
CEP	-0.052***	-0.056***	-0.069***
	(0.008)	(0.009)	(0.008)
R-Squared	0.981	0.984	0.972
Observations	59,076	59,076	58,916

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include store and county-year fixed effects. Standard errors are clustered at store level.Sales are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year. The sample of stores includes both grocery and mass merchandisers.

5.2.2 Difference-in-Differences

Our estimation strategy exploits two features of the CEP: the staggered roll-out of the CEP across four waves and the requirement that schools[11] exceed the 40% ISP threshold to qualify for the provision. The first source of identifying variation allows us to construct an estimator that compares changes in store revenues in early and late adopting states. This variation is similar to the identification in Hoynes and Schanzenbach (2009), who study the effect of SNAP on food spending. Our chief concern is that early-adopting states may differ

[11] Or groups of schools, termed LEAs.

systematically from late adopters in ways that affect year-to-year changes in store revenue. We therefore construct a difference-in-differences estimator using the 40% ISP eligibility requirement.

In our preferred specification, stores near schools below the 40% threshold act as controls for those near schools above the threshold. This strategy is similar in spirit to a regression discontinuity design, but exploits the entire dataset rather than the set of schools near the threshold.[12] Our identification assumption is that changes in the relationship between ISP and an outcome of interest below/above 40% are driven by the CEP, rather than changes in the underlying characteristics of those stores.

Our preferred specification for an outcome y for store i is:

$$\begin{aligned} y_{it} = \alpha_0 + \alpha_1 \cdot 1\left[StateAdopt\right]_{it} + \alpha_2 \cdot 1\left[StateAdopt\right]_{it} \times shareISP40_{it} \\ + \Omega_i + \Gamma_t \times \Delta_{county(i)} + \omega_{it} \end{aligned} \tag{4}$$

where $1[StateAdopt]_{it}$ is an indicator that store i's state had adopted the CEP by year t and $shareISP40_{it}$ is the share of neighboring schools with an 2010 ISP of 40 or above. The coefficient of interest is α_2, which governs the relationship between revenue and the interaction between the ISP threshold and state adoption. The coefficient α_2 therefore captures an intent to treat effect of the CEP - the decline in revenue for local stores when their nearest school becomes eligible for the program. Our baseline specification includes store fixed effects, which control for time-invariant determinants of store revenue, and county-by-year fixed effects, which control for county-wide shocks to the grocery industry. Importantly, all variants of this specification allow the relationship between ISP and store revenue to differ systematically across early and late adopting states for reasons apart from the CEP. The identifying assumption is that any change in this relationship at 40% stems from differences in CEP eligibility.

Because most eligible schools do not adopt the CEP, we estimate how take-up affects stores using instrumental variables to back out the treatment on the treated. In this case, we estimate equation (3) using $1\left[StateAdopt\right]_{it} \times shareISP40_{it}$ as an instrument for the share of local schools[13] that participate in the CEP. This formulation scales the estimator from (4) by take-up (again, in a fashion similar to Hoynes and Schanzenbach (2009)). The first stage specification mirrors equation (4), where the dependent variable is the share of schools participating under the CEP (which takes a value between 0 and 1). The first stage results are presented in table 6. They suggest that schools with an ISP above 40% are around 19 percentage points more likely to adopt the provision than schools in adopting

[12]Recall that RDD is not feasible given our proxy for ISP precise measurement of the running variable (Pei and Shen (Forthcoming)).

[13]Schools that share the same ZIP code as the store.

states with ISPs below 40%. Recall that some schools with low ISPs may qualify for the CEP as part of a larger conglomerate of schools, where the combined ISP is above 40%. Despite this possibility, the results indicate large differences in take-up above and below the ISP threshold.

Table 6: First Stage Results

	(1)	(2)	(3)	(4)
State Adopt x Percent Eligible	0.215***	0.212***	0.202***	0.200***
	(0.008)	(0.007)	(0.007)	(0.008)
Constant	0.012***	0.009***	0.009***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)
ISP Year	2009	2010	2011	2012
R-squared	0.789	0.79	0.787	0.792
Observations	58,249	59,076	59,036	54,594

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include store and county-year fixed effects. Standard errors are clustered at store level.

5.3 Impact of the CEP on Store Revenue

We report our baseline estimates of specification 4 in table 7 and the graphical analogue in figure 5. The coefficient in column 1 implies that stores near schools that are all eligible for the CEP earn 3.8% lower revenues than those neighboring ineligible schools. Estimates are larger in magnitude if we focus on the sales of lunch meats (column 3). We expect that CEP adoption disproportionately affects breakfast and lunch food sales because these categories should suffer most acutely if families no longer pack lunch for children under the CEP. Figure 5 shows that this difference in revenues emerges gradually after state adoption of the policy, consistent with the slow take-up of the CEP among eligible schools shown in figure 3. Appendix figure 17 shows that the difference between treatment and control stores is robust to the inclusion of state-by-year FE and eligibility-by-year FEs.

Table 7: Effect of CEP Eligibility on Log Grocery Revenue

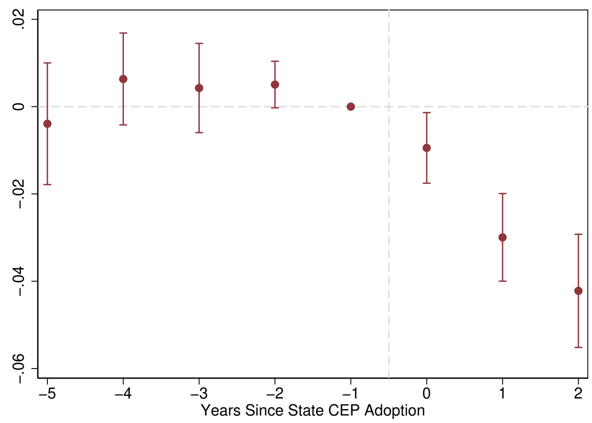
	(1) All	(2) B/L	(3) Lunch Meat
State Adopt x Percent Eligible	-0.039***	-0.043***	-0.064***
	(0.005)	(0.006)	(0.006)
R-squared	0.004	0.004	0.008
Observations	59,076	59,076	58,916

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include store and county-year fixed effects. Standard errors are clustered at store level. Sales are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year. The sample of stores includes both grocery and mass merchandisers.

To aid in interpretability, we present IV estimates of the effect of CEP adoption on neighboring store revenue in table 8. The estimate implies that the CEP reduces grocery revenue for the full calendar year by 16.8% (column 1). The effect is larger for breakfast and lunch sales (column 2) and lunch meats (column 3).

As a benchmark, we compare these estimates to a back-of-the-envelope calculation for

Figure 5: Effect of CEP Eligibility on Log Grocery Revenues



Notes: Among all years since state adoption, [-5,1] are common for all states, while years -8,-7, -6 on the left only exist for states that adopted in 14-15, 13-14 (and after) and 12-13 (and after), and 2, 3, 4 on the right end only exist for states adopted in 13-14 (and before), 12-13 (and before) and 11-12, respectively. (2) Only [-1, 1] are common for all states, while year -4,-3, -2 on the left end only exist for states adopted in 14-15, 13-14 (and after) and 12-13 (and after), and 2, 3, 4 on the right end only exist for states adopted in 13-14 (and before), 12-13 (and before) and 11-12, respectively.

the effect of free lunch and breakfast on the average monthly grocery budget, assuming the average combined daily cost of school lunch and breakfast totaled \$4.15 before the CEP.[14] Then the CEP amounts to a monthly transfer of approximately \$82.93 per child, which we use in conjunction with USDA estimates for the cost of food at home to calculate household CEP savings:[15]

$$
\begin{aligned}
\%\Delta\text{Spending} &= \frac{-\#\text{Children } \times \text{Value of Breakfast \& Lunch}}{\text{Monthly Grocery Expenditures for Family of Four}} \\
&= \frac{-2 \times 82.93}{642.10} \times 100 \\
&= -25.83\%
\end{aligned}
$$

If all Americans were on the "thrifty plan" for the cost of food at home and crowd-out were one-for-one, then we would expect at 25.83% decline in grocery spending for affected families. The reduction in demand is smaller for the "low-cost plan" (19.60%). If participation in school meal programs doubles from 40% to 80% under the CEP, then store revenue would fall by 8%.[16] Our estimates are about twice as large, potentially for three reasons: first, lunch spending might constitute a disproportionate share of the grocery bill (if, for example, families eat dinner at restaurants or other on-premise locations); second, fertility rates are

[14]Based on the average price of school lunch and breakfast from the School Nutrition Association's School Meal Trends & Stats.

[15]Official USDA Food Plans: Cost of Food at Home at Four Levels, U.S. Average, June 2018. Available at:https://www.cnpp.usda.gov/sites/default/files/CostofFoodJun2018.pdf

[16]Assume 40% of households contain no children, but only two adults. Then $\%\Delta\text{Revenue} = \frac{0.6\cdot 0.4\cdot(-165.86)}{0.4\cdot 384.5+0.6\cdot 0.6\cdot(642.1)+0.6\cdot 0.4\cdot(476.24)}$.

higher among low-income women, so that the household expenditure share of children's food is higher (Monte and Ellis (2014)); and finally, because supermarkets may respond to the CEP by reducing prices. We focus on this last mechanism in section 6. As a robustness check, we re-estimate the household regression specification from section 5.1 using expenditures at Nielsen RMS stores as the dependent variable. Results are presented in Appendix table 17. They show that CEP adoption reduces spending by 14%, which is in the order of magnitude of the the point estimates in table 8. Further, we show that these household RMS results combined with the price effects studied below yield estimates similar to the store-level regenue regressions.

With revenue declines on the order of 10-20%, it is possible that some stores become unprofitable, inducing exit as a result of the program. We study whether the CEP induces store exit in the next section, but here it is worth considering that exiting stores might face sharper declines in revenue as they prepare to shut their doors, which would tend to attenuate the estimates.

Table 8: Effect of CEP on Log Grocery Revenue

	(1) All	(2) B/L	(3) Lunch Meat
CEP	-0.184***	-0.203***	-0.301***
	(0.026)	(0.029)	(0.030)
First Stage F-Stat	802	802	802
Observations	59,076	59,076	58,916

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All regressions include a control for the years in which the state has adopted the program, and store and county-year fixed effects. Standard errors are clustered at store level. Sales are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year. The sample of grocery stores includes both grocery and mass merchandisers. The sample of convenience stores considers both drug and convenience stores. B/L indicates that the LHS variable is the log of breakfast and lunch food sales. Instrument is the interaction of dummies for state adoption and the nearest school having 40 percent or more eligible for free and reduced lunch in 2010.

6 Retailer Responses to the CEP

In this section, we examine the supply-side response to the Community Eligibility Provision. Our results in section 5 suggest that RMS stores experience a 17% decline in revenue when their neighboring school adopts the CEP, extending free lunch and breakfast to all students. Our results are consistent with substitution away from home-made lunch (a positive cross-price elasticity). However, the revenue changes we estimate might also reflect changes in store entry, exit, and pricing decisions. The conceptual framework that we develop in section 3 highlights that by changing the demand elasticity for groceries, the CEP may cause retailers to increase or decrease prices–whether or not retailers engage in zone pricing.

6.1 Impact of CEP on Store Exits and Entries

One possibility is that the stores hardest hit by the CEP exit, and further, that the CEP deters entry of new retailers. In this subsection, we directly estimate the effect of the CEP on retailer churn.

We adopt specification 4 so that z denotes ZIP code (rather than an individual retail outlet) and the outcome of interest is the logarithm of the number of firms plus one. Note that we include ZIP code and county-by-year fixed effects (but not retailer FE), as in the following equation

$$\log(1 + NFirms)_{zt} = \alpha_0 + \alpha_1 \cdot 1\left[StateAdopt\right]_{zt} + \alpha_2 \cdot 1\left[StateAdopt\right]_{zt} \times shareISP40_{zt} \quad (5)$$

$$+ \Omega_z + \Gamma_t \times \Delta_{county(z)} + \epsilon_{zy}.$$

If the CEP induces exit among RMS stores, then selection bias is a potential threat to the revenue regression estimates presented in the preceding section (equation 4). If selection occurs, then we expect our estimates would understate the magnitude of crowd-out. Our intuition is as follows: a firm should exit if its scrap value exceeds the profits from remaining in the market, so that stores that are hardest hit by the CEP should be most likely to exit. This type of selection would tend to attenuate the revenue estimates. That is, we would understate the impact of the CEP on grocery revenues.

Table 9 presents estimates of the CEP on exit, entries, and the total number of firms for the RMS sample. Note that we exclude instances when the entire chain enters or exits the RMS dataset; our concern is that these entries and exits are a function of the RMS dataset itself, and do not reflect true changes to the local retail environment. We also exclude stores that we cannot match to a school with CEP participation and eligibility data based on the retailer's ZIP code. Unfortunately, the CEP participation data is spotty, so this eliminates about 20% of the RMS stores. Despite these data limitations, the first stage results, presented in column 4 look similar for this sample of schools. Appendix table 12 displays the sample creation criteria.

The reduced form results, shown in table 9 column 1 provide evidence that high-eligibility ZIP codes support fewer Nielsen retailers following state adoption of the CEP, on the order of 0.6%. This effect seems to be driven by a reduction in entries (column 3), rather than by attrition among existing stores (column 2). This evidence suggests that selection bias is not a concern in our revenue regressions (table 7) because these results consider effects on existing stores. While the difference-in-difference churn effects are small, they become economically meaningful when scaled by take-up; a ZIP code where all neighboring schools are eligible sees a 2.5% decline in the number retailers (column 4). Figure 6 presents the graphical analogue of our regression results, and makes clear the decline in entries occurs slowly, with effects becoming detectable only in the second and third years of the program. This pattern indicates that the long-run effect of the CEP on number of retail outlets may be larger than the ones presented here.

Table 9: CEP and Churn among Nielsen Firms

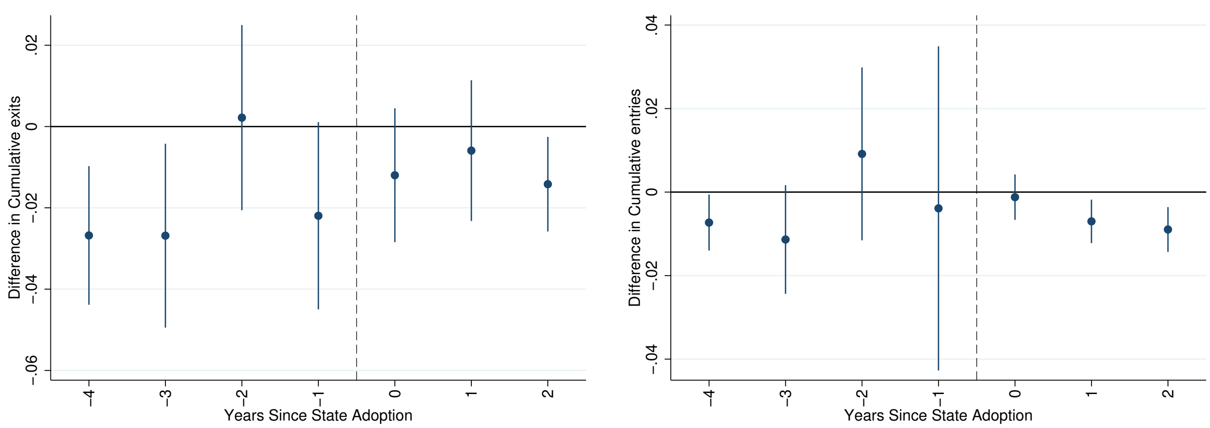
	Reduced Form			First Stage	IV		
	(1) N Firms	(2) Exits	(3) Entries	(4) CEP	(5) N Firms	(6) Exits	(7) Entries
maxadopt					-0.025**	0.013	-0.021**
					(0.010)	(0.022)	(0.010)
state_adopted_avgEligible	-0.006**	0.003	-0.005**	0.233***			
	(0.002)	(0.005)	(0.002)	(0.011)			
Start Year	2010	2010	2010	2010	2010	2010	2010
ZIP FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Std. Cluster	ZIP	ZIP	ZIP	ZIP	ZIP	ZIP	ZIP
F-Stat					481	486	486
Observations	21,706	22,565	22,565	22,565	21,706	22,565	22,565
R-squared	0.244	0.279	0.23	0.776			

Notes: Outcome variable is log(1+column header). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include zip and state*year fixed effects, are clustered at the zip level, and include a constant. The "Percent Eligible" Method assigns each zip code a value between 0 and 1 that represents the percent of stores in that zip code whose nearest school's ISP is greater than 40%. This is an average of binary values. Max CEP is the highest value of the CEP for the store's zip code variable across years after the state adopted the CEP. This captures the highest level of treatment the store experiences over the time frame.

Figure 6: Event Study Exits & Entries

(a) Zip Cumulative Exits

(b) ZIP Cumulative Entries



Finally, we estimate the effect of the CEP on churn separately by ZIP code market structure. We measure market structure as the number of Nielsen stores operating in 2010 and break out results for 1, 2, 3, and 4 plus competitors. Point estimates and standard errors are presented graphically in figure 7. There is no evidence of increased churn for monopoly markets, but we see a reduction for markets with two firms and an even larger effect for markets with four or more firms (approximately 3.88% of ZIP codes in our datasets).

Figure 7: Effect by Number of Competitors

Notes: Market structure measured as the number of Nielsen retail outlets operating in 2010.

6.2 Measuring Prices

Beyond exit, we are interested in whether and to what extent retailers adjust prices in response to the CEP demand shock. Because supermarkets typically stock thousands of products, we construct an inflation index to capture changes in the price of a fixed bundle of goods, whereby avoiding any product heterogeneity biases. In appendix B.1, we examine changes in product assortment, which we find to be tiny.

Following Leung and Seo (2018), we measure inflation as a monthly inflation index for continuing UPCs: those sold in a given store in every month in the current year and at least one month in the previous year. The calculation proceeds in two steps. First, for each product group j, we calculate a month-on-month arithmetic inflation index for each store s. Let u denote a particular product (UPC), and $U_{j,s,m}$ be the set of products sold in store s in group j in month m. Product-group level inflation for a given month is defined as:

$$\frac{P_{s,j,m}}{P_{s,j,m-1}} = \frac{\sum_{u \in U_{j,s,m}} p_{u,s,m} q_{u,s,y(m)-1}}{\sum_{u \in U_{j,s,m}} p_{u,s,m-1} q_{u,s.y(m)-1}}$$

where $p_{u,s,m}$ is the unit price at which UPC u is sold in store s in month m and $q_{u,s,y(m)-1}$ is the quantity of UPC u sold in store s in the calendar year preceding month m.

In the second step, we aggregate across product-groups using a Tornqvist index:

$$\frac{P_{s,m}}{P_{s,m-1}} = \prod_{j \in J} \left(\frac{P_{s,j,m,y}}{P_{s,j,m-1,y}} \right)^{\frac{S_{s,j,m}+S_{s,j,m-1}}{2}}$$

where $S_{s,j,m}$ denotes the expenditure share of product group j in store s in month m.

Store-level monthly inflation is, therefore, a Tornqvist aggregate of Laspeyres-style lagged-weight arithmetic indexes at the product group-store level. The base year in our dataset is January 2010 during which the index is set to 1. We use a rolling sum of the log store-level inflation index as our dependent variable to measure supermarket price responses.

To study store responses to CEP participation, we average these indexes across months in the academic year (September-May) and the full school year (September-August), match them to the yearly school participation dataset, and estimate our difference-in-difference with county×year FE specification from equation 4.

6.3 Price Responses

We reestimate our main difference-in-differences specification with the price index as the dependent variable in order to measure retailer responses to the CEP. However, we adapt equation (4) to allow for an important feature of grocery retail in the US: uniform pricing. As described in DellaVigna and Gentzkow (2019), among others, most retail chains featured in the Nielsen dataset employ zone pricing, wherein prices for the same product (UPC) do not vary across broad geographical regions. Zone pricing implies that any individual grocery outlet is unlikely to adjust prices in response to a local CEP demand shock. However, a retail chain that is highly exposed to the CEP may adjust prices across a broad swathe of stores. In other words, a chain with a high share of stores that experience the CEP demand shock may respond along the price margin, changing prices even in stores that are not exposed.

To test the zone pricing hypothesis, we construct chain-level analogues of CEP adoption (the average adoption of retail outlets in the chain) and exposure (the average neighboring school CEP eligibility of retailers in the chain). In our augmented specification, we estimate whether individual retailer and/or chain exposure affects pricing and variety decisions. Table 10 presents results of chain- and store- CEP adoption on prices. Column 3 shows a null effect of local exposure on prices (albeit measured with a large standard error). In contrast, in column 6, we find a negative effect of chain exposure on price; a one-standard deviation increase in chain exposure leads to a 5.4% decline in the price index. In column 9 we include both local and chain exposure in a single regression and show that the large, though noisy, estimated impact of the store-ZIP CEP exposure on local store prices estimated in column 3 is largely attributable to the chain-level responses to chain-level CEP exposure. We present the results graphically in figure 8, where we can see a gradual increase in the effect of CEP exposure over time at the chain-level (panel a) but not for the store-level analogue (panel b).

Table 10: Effect of CEP Adoption on Prices

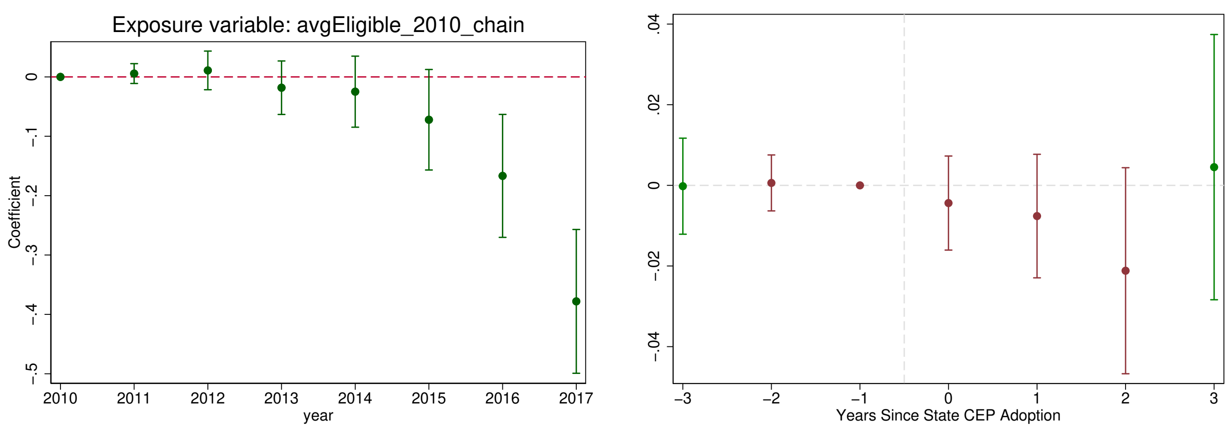
	Local Exposure			Chain Exposure			Chain + Local Exposure		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Store Zip CEP			-0.078						-0.018
			(0.067)						(0.069)
State Adopt x Store Zip Percent Eligible	0.212***	-0.016					0.033**	-0.006	
	(0.007)	(0.014)					(0.016)	(0.014)	
Chain CEP						-0.065***			-0.064***
						(0.011)			(0.011)
State Adopt x Chain Zip Percent Eligible				0.723***	-0.046***		0.719***	-0.045***	
				(0.017)	(0.008)		(0.017)	(0.008)	
Regression	FS	RF	IV	FS	RF	IV	FS	RF	IV
First Stage F-Stat			760			1866			948
Observations	59,076	58,286	58,286	57,924	57,205	57,205	57,924	57,205	57,205
R-Squared	0.79	0.914		0.957	0.915		0.957	0.915	

Notes: Outcome variable is a price index constructed using store-weights. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include store and county-year fixed effects. Standard errors are clustered at store level. Prices are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year. The sample of stores includes both grocery and mass merchandisers.

Figure 8: Effect of CEP Eligibility Exposure on Price Index

(a) Chain-Level Exposure

(b) Store-Level Exposure



Notes: Plots regression coefficients from specification that includes store and county × year FE.

These results offer two implications: first, they suggest that spillovers of the CEP are substantial, lowering prices for households with and without children. If redistribution to consumers in high chain-exposure areas is desirable, then these spillovers present an argument in favor of in-kind giving. Second, they provide evidence for how market structure - in particular, the configuration of retail chains - affects the propagation of demand shocks in the economy. Relative to other work studying the effects of demand shocks on prices (see, e.g., Leung and Seo (2018) and Stroebel and Vavra (2019)), our results confirm that effects could be large, but also suggest that these effects depend crucially on the spatial distribution of chains.

7 Welfare Effects of the CEP on Households without School-Aged Children

In this section, we specify a model of store choice to study the net welfare effect of the price reductions and store exits induced by CEP adoption. We focus on how households without children - for whom school lunch is not an option - allocate food expenditure across different stores. We estimate the parameters that govern this store allocation decision and hold these parameters fixed to measure how the change in the retail landscape induced by CEP adoption affected grocery costs.

7.1 Model Set-up

A representative household residing in ZIP o allocates their grocery expenditure w across the set of open stores $s \in S_t$ at time t to maximize the following CES utility function:

$$U_{ot} = \left[\sum_{s \in S_t} \psi_{ost}^{\frac{1}{\sigma}} \cdot q_{st}^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}}$$

subject to

$$\sum_{s \in S} p_{st} \cdot q_{st} \leq w$$

where q_{st} and p_{st} are consumption and price indexes of store s at time t; $\psi_{ost} > 0$ is the household's perception of the quality of store s at time t; and $\sigma>1$ is the constant elasticity of substitution across stores.

The representative household residing in origin ZIP o allocates a share s_{ost} of their expenditure between stores according to the following expression:

$$s_{ost} = \frac{\psi_{ost} \cdot p_{st}^{1-\sigma}}{\sum_{s' \in S_t} \psi_{os't} \cdot p_{s't}^{1-\sigma}} \tag{6}$$

Substituting this share into utility, we have the standard CES result that indirect grocery utility is equal to retail expenditure (w) divided by a retail price index that summarizes the quality and prices of the stores available to households in origin ZIP o at time t:

$$P_{ot} = \left(\sum_{s \in S_t} \psi_{ost} \cdot p_{st}^{1-\sigma} \right)^{\frac{1}{1-\sigma}}$$

The higher this price index, the more households need to spend to achieve the same grocery utility. The change in grocery utility afforded by a fixed level of expenditure w between two time periods t_0 and t_1 is equal to the inverse ratio of this price index between the two periods, $(P_{ot_1}/P_{ot_0})^{-1}$.

We parametrize perceived quality as follows:

$$\ln \psi_{ost} = \ln \xi_{os} + \ln \tilde{\psi}_{ost} \tag{7}$$

where ξ_{os} is the unobserved time-invariant component of the demand from origin o at store s and $\tilde{\psi}_{ost}$ is the unobserved time-varying component of demand from origin o at store s at time t, which we assume to be orthogonal to the program. We further parameterize ξ_{os} as being log-linear in the distance between ZIP o and store s such that $\xi_{os} = \tilde{\xi}_{os} - \tau \cdot \ln d_{os}$. We proxy for the location of store s with its ZIP d. Abstracting from the unobservable components of demand, we can re-write the utility index as follows:

$$\ln\left(\frac{P_{ot_1}}{P_{ot_o}}\right) = \left(\frac{1}{1-\sigma}\right)\left[\ln\left(\sum_{s\in S_{t_1}} d^{\tau}_{od(s)} \cdot p^{1-\sigma}_{st_1}\right) - \ln\left(\sum_{s\in S_{t_0}} d^{\tau}_{od(s)} \cdot p^{1-\sigma}_{st_0}\right)\right] \tag{8}$$

Equation (8) shows that grocery costs are impacted by two factors: (i) changes in the set of stores open ΔS_t, where the closer an entering or exiting store is to an origin ZIP o ($\tau > 0$) and the lower its price ($\sigma > 0$), the larger the degree by which it will reduce the price index for origin ZIP o, and (ii) changes in the prices charged by continuing stores Δp_{st}.

The analysis above shows that the CEP program impacted both of these factors. If we assume that pricing is uniform across stores in a ZIP or there is at least no selection in the pricing of continuing stores relative to those that enter or exit, then we can write the utility index as a function of the number of stores open in a ZIP code (N_{dt_0}) and the average price level charged by those stores (p_{dt_0}), both at time t_0, before the program is introduced, and the estimated impact of the CEP program on each of these variables ($\widehat{\Delta N}_d$ and $\widehat{\Delta p}_d$):

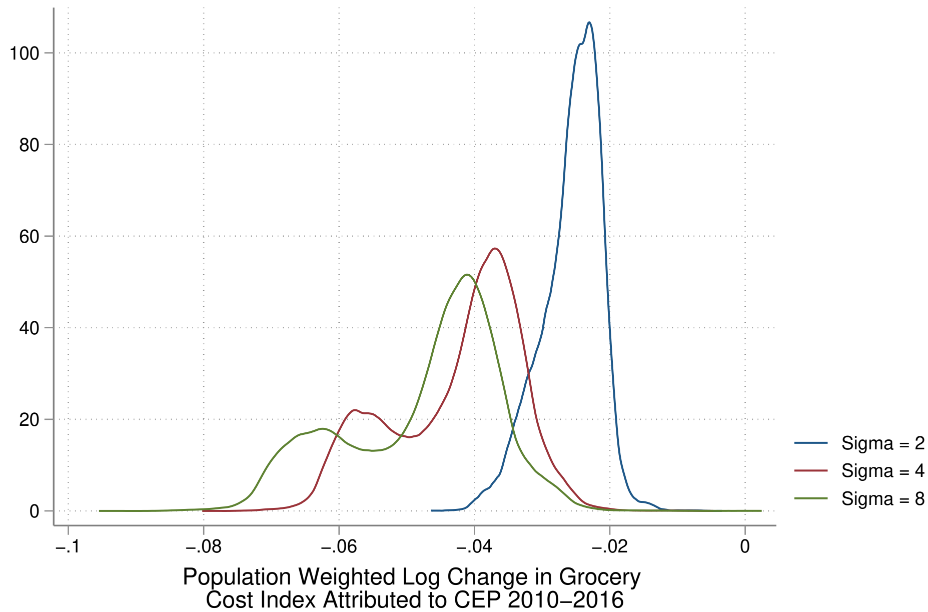
$$\ln\left(\frac{P_{ot_1}}{P_{ot_o}}\right) = \left(\frac{1}{1-\sigma}\right)\left[\ln\left(\sum_{d\in D} d^{\tau}_{od}\left(N_{dt_0} + \widehat{\Delta N}_d\right)\left(p_{dt_0} + \widehat{\Delta p}_d\right)^{1-\sigma}\right) - \ln\left(\sum_{d\in D} d^{\tau}_{od} N_{dt_0} p^{1-\sigma}_{dt_0}\right)\right] \tag{9}$$

To calculate the welfare effects described above (in equation 9), we need data describing ZIP-pair distances (d_{od}) and the initial levels of the retail store variety and prices in each ZIP (N_{dt_0} and p_{dt_0}); estimates of the impact of the CEP program on these ZIP-level retail variables ($\widehat{\Delta N}_d$ and $\widehat{\Delta p}_d$); and estimates of the two key demand elasticities (τ and σ). In the interest of brevity, we describe the data and estimation procedure in the appendix **??**.

7.2 Welfare Results

Plugging the estimates for the impact of the policy on store counts and ZIP-level average prices ($\widehat{\Delta N_d}$ and $\widehat{\Delta p_d}$) and elasticities ($\hat{\sigma}$ and $\hat{\tau}$) obtained in appendix section **??**. into equation 9 provides us with an estimate for the welfare impact of the CEP program in each origin ZIP code o. Welfare is measured as the change in grocery costs, inclusive of travel costs and product prices. The population-weighted distribution of these welfare estimates is presented in Figure 9. The distributions are also shown for higher and lower values of $\hat{\sigma}$. The median effect with our preferred estimate for $\hat{\sigma}$ ($\hat{\sigma} = 4$) is a decrease in grocery costs of approximately 4.5 percentage points, but there is a significant degree of variation across ZIP codes. Part of this variation can be explained by the spatial distribution of CEP take-up and the spatial distribution of chains with differential exposure to areas where the CEP is adopted. Figure 10 shows the correlation between the welfare effects of the program and proximity to ZIP codes with different levels of direct and indirect exposure to the CEP program, measured as the weighted average of direct and indirect exposure using the distance weights (d_{od}^{τ}). ZIP codes with higher direct CEP exposure see lower reductions in grocery costs (9.8 percentage points in ZIPs where all local schools participate relative to 9.1 percentage points in ZIPs where few local schools participate) because the welfare benefits of price decreases are partially offset by lower rates of store entry. ZIP codes with higher indirect CEP exposure, meanwhile, see much larger reductions in grocery costs. In fact, most of the cross-ZIP variation in the estimated welfare impact of the program are explained by variation in the indirect exposure to the program via which chains operate in the vicinity of different ZIP codes. ZIP codes with high densities of heavily exposed chains, which are predicted to reduce their prices in response to the program, saw grocery costs fall by up to 8 percentage points, while ZIPs with low-exposure chains see grocery costs fall by only 3 percentage points. Figure 11 shows that the largest welfare benefits are in ZIPs with zero direct effects and strong indirect effects. The model above allows us to also calculate the change in ZIP-level shopping costs that result from all changes in store prices and net exit (rather than just the changes attributable to the CEP). Figure 12 is a binscatter showing the association between the change in the grocery cost index attributable to the CEP and the total change in the grocery cost index. The x-axis shows that overall grocery price inflation and store exits between 2010 and 2016 have resulted in grocery costs increasing in almost all ZIP codes, with most ZIP codes seeing 10 to 30 percent increases in grocery costs. Our analysis suggests that the CEP program counteracted these general trends, decreasing prices, on average. Figure 12 shows that the CEP program helps to explain some of the spatial variation in price declines: ZIPs with larger predicted spillovers from CEP to shopping costs tended to see smaller increases in their aggregate grocery cost growth.

Figure 9: Effects of the CEP on Grocery Costs between 2000 and 2016



Notes: This figure plots the distribution of CEP effects across ZIP codes. For each ZIP code, we calculate the log change in its grocery cost index caused by the CEP. The magnitude of the effect depends on the exposure of the retail chains located in the ZIP code.

Figure 10: The Effect of Direct and Indirect Exposure to the CEP on Grocery Costs

(a) Direct Exposure

(b) Indirect Exposure

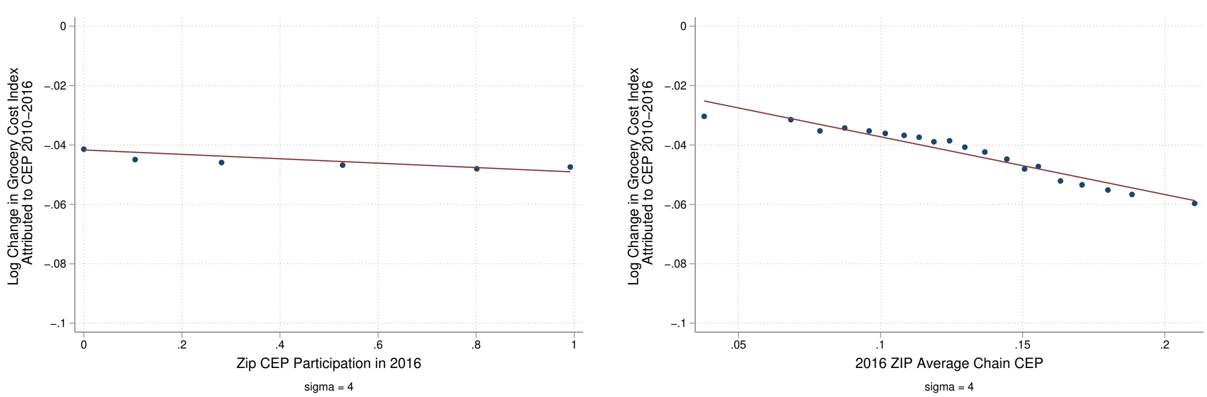


Figure 11: Heat Map of Effect of the CEP on Grocery Costs between 2000 and 2016

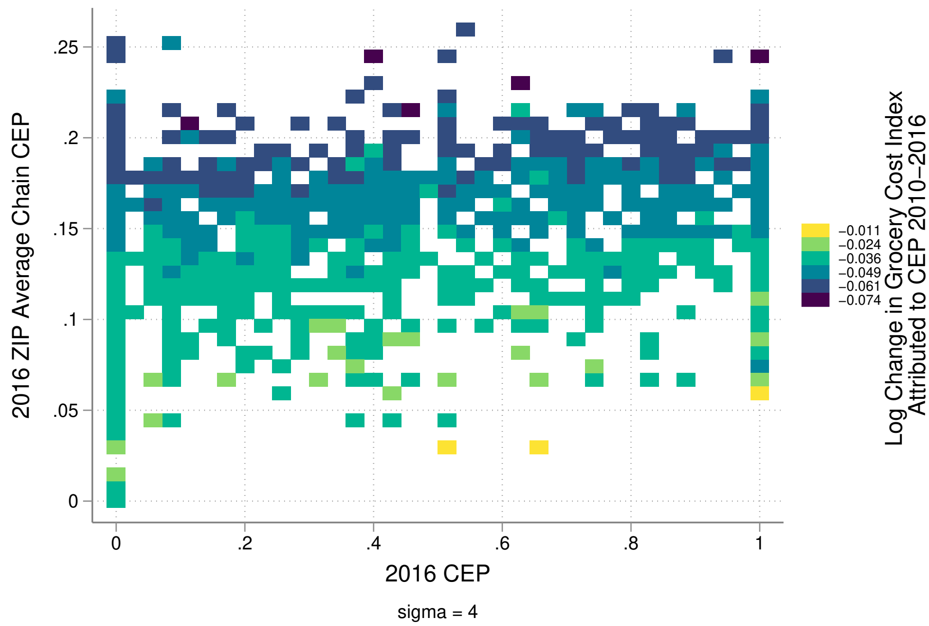
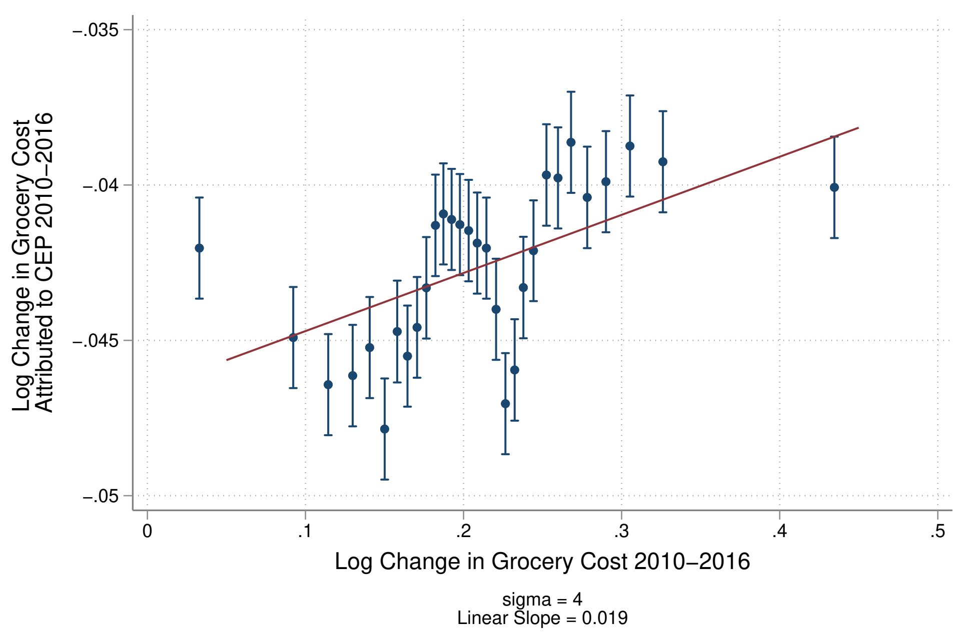


Figure 12: Welfare Effects of the CEP Relative to Overall Changes in Shopping Costs



8 Conclusion

This paper demonstrates that the National School Lunch Program delivers a substantial indirect benefit to communities through its supply-side effects. To establish causality, we leverage an expansion of the NSLP under the Community Eligibility Provision, which requires that participating schools provide free lunch to all students–in essence, lowering the price of a substitue for grocery store lunch products. Panel data on household grocery purchases reveals that households with children reduce their spending by 6% when a local school adopts the CEP. We then show that grocery stores respond to this demand shock by reducing prices. We also present evidence that the CEP demand shock deters entry by new grocery retailers.

To quantify the net benefit of the program to adult-only households, we estimate a CES model of grocery demand. The model allows us to measure how households trade off lower store variety with lower prices. The welfare estimates suggest that the CEP reduces shopping costs in the median effected ZIP code on the order of 4.5%. For comparison, a back-of-the-envelope calculation suggests that the direct benefit of the NSLP for a household with children amounts to a 25% reduction in shopping costs.

A final finding is that the spatial distribution of retail grocery chains determines the distribution of the indirect benefits. Chain geography is important because retail grocery chains in the US employ zone pricing. Thus, we find that retailers do not adjust prices in response to local CEP adoption–rather, retail chains adjust prices in response to their overall exposure across outlets. Consequently, some consumers enjoy lower prices even when their local school does not adopt the program. Taken together, our findings suggest that supply-side forces can meaningfully amplify the benefits of food security policy.

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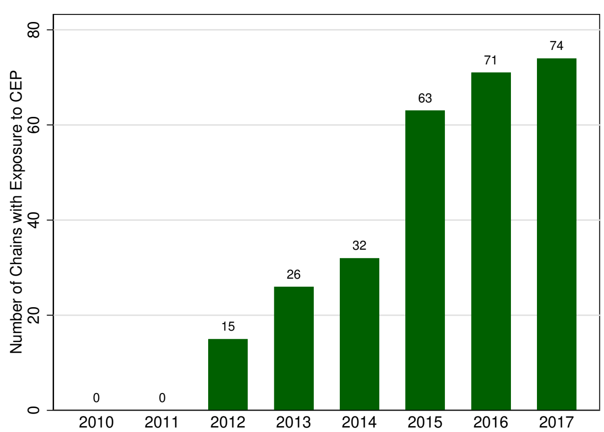
A Data Appendix: Tables & Figures

Table 11: Breakfast and Lunch Food Categories

BAKED GOODS-FROZEN	EGGS
BAKING MIXES	FRESH PRODUCE
BAKING SUPPLIES	FRUIT - DRIED
BREAD AND BAKED GOODS	JAMS, JELLIES, SPREADS
BREAKFAST FOOD	JUICE, DRINKS - CANNED, BOTTLED
BREAKFAST FOODS-FROZEN	MILK
BUTTER AND MARGARINE	NUTS
CEREAL	PACKAGED MEATS-DELI
CHEESE	PREPARED FOOD-READY-TO-SERVE
CONDIMENTS, GRAVIES, AND SAUCES	SALAD DRESSINGS, MAYO, TOPPINGS
COOKIES	SNACKS
COT CHEESE, SOUR CREAM, TOPPINGS	SNACKS, SPREADS, DIPS-DAIRY
CRACKERS	SOFT DRINKS-NON-CARBONATED
DESSERTS, GELATINS, SYRUP	TABLE SYRUPS, MOLASSES
DOUGH PRODUCTS	UNPREP MEAT/POULTRY/SEAFOOD-FRZN
DRESSINGS/SALADS/PREP FOODS-DELI	YOGURT

Notes: This table reports the product modules that we categorize as breakfast and lunch products.

Figure 13: Chain Exposure to the CEP over Time



Notes: This figure plots exposure to the Community Eligibility across retail grocery chains in the Nielsen RMS dataset. The program began in 2012, so exposure is zero in 2010 and 2011. Exposure grows dramatically in 2015 when all states adopt the program. Table 1 provides the timing of adoption across states.

Table 12: Sample Construction for Store Attrition Analysis

Initial Pool = 9,929	
Criterion	
Unbalanced RMS Panel	171
Entire chain entry/exit from RMS	1,489
Store switches chain affiliation	77
No school match with 2010 ISP & Participation Data	1,632
Final Sample = 6,808	

Notes: This table describes the criteria that we use to create the sample of Nielsen RMS stores for our analysis of store exits in section 6.1. Most retail outlets are dropped if the entire chain exits or enters the RMS data so that we cannot observe changes in the outlet's survival or if we cannot match the retail outlet to CEP exposure.

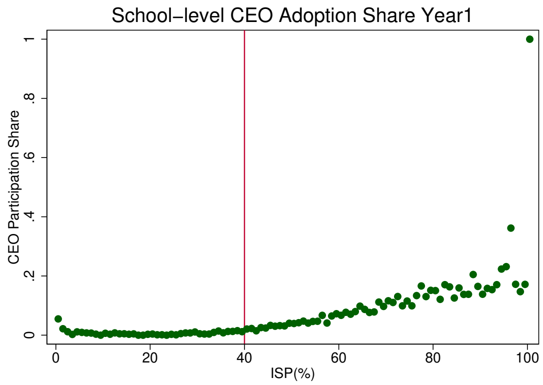
Table 13: Match Rate between NCES and State DOE Data

State	NCES and State CEP	NCES Only	State CEP Only	Total
District of Columbia	80	144	0	224
Illinois	508	7907	139	8554
Kentucky	440	2373	42	2855
Michigan	827	6218	144	7189
Ohio	4	3625	0	3629
West Virginia	249	505	34	788
Total	2108	20772	359	23239

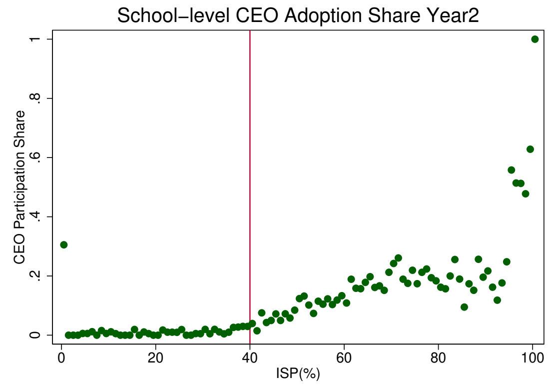
Notes: This table compares CEP participation data from the National Center for Education Statistics and the State Departments of Education. We collect this data for early-adopting states only.

Figure 14: CEP Adoption Rates over Time
School CEO Adoption Share By Year of Adoption

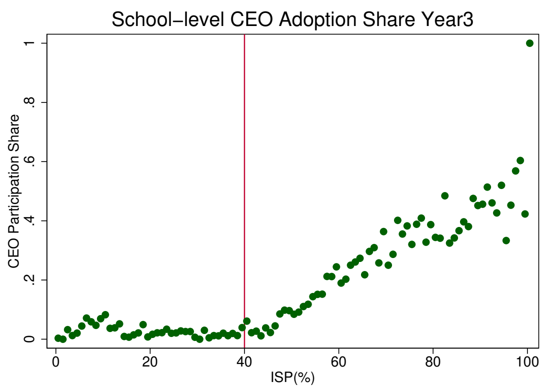
(a) Year 1



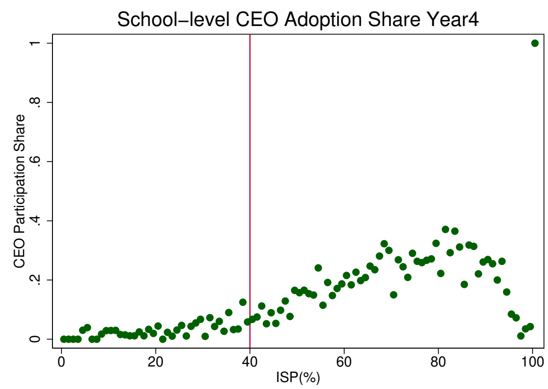
(b) Year 2



(c) Year 3



(d) Year 4



Notes: This figure plots school participation rates in the CEP by ISP and years since state adoption. Table 1 provides information on the timing of state CEP adoption.

B Robustness

B.1 Evidence on Product Variety

In this appendix, we present evidence on how the CEP affects the assortment of goods offered at grocery stores. These types of changes are omitted from the analysis of supply-side effects in section 6. The price indices fail to capture any cost-of-living inflation changes generated by changes in store offerings. Thus, we study changes in the assortment of goods offered using a variety index, which reflects changes in the product assortment.

Our variety index calculates the proportion of products (UPC) sold by store s in month m relative to the total set of products carried across all stores. Each product is weighted by its national sales in month m. The final index is calculated as below

$$V_{s,m} = \sum_{u \in U_{s,m}} \left(\frac{v_{u,m}}{\sum_{u \in U_m} v_{u,m}} \right)$$

where $v_{u,m}$ denotes the national sales of product u in month m. Table 14 presents results of estimating specification (??) using variety as the dependent variable. Across the board, the point estimates are economically insignificant.

Table 14: Effect of CEP Adoption on Variety

	Local Exposure			Chain Exposure			Chain + Local Exposure		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Chain CEP						0.001^{***}			0.002^{***}
						(0.001)			(0.001)
CEP			-0.006^{***}						-0.011^{***}
			(0.002)						(0.002)
State Adopt x Chain Percent Eligible				0.697^{***}	0.001^{***}		0.693^{***}	0.001^{***}	
				(0.018)	(0.000)		(0.019)	(0.000)	
State Adopt x Percent Eligible	0.228^{***}	-0.001^{***}					0.037^{***}	-0.002^{***}	
	(0.007)	(0.001)					(0.011)	(0.000)	
Regression	FS	RF	IV	FS	RF	IV	FS	RF	IV
Products		All	All		All	All		All	All
First Stage F-Stat			1131			1435			752
Observations	65,823	65,823	65,823	64,643	64,643	64,643	64,643	64,643	64,643
R-Squared	0.794	0.989		0.972	0.99		0.972	0.99	

Notes: Outcome variable is the variety index described above constructed using store-weights. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include a constant. Sales are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year. The sample of grocery stores includes both grocery and mass merchandisers.

B.2 Evidence on Lunches Served

An explicit goal of the CEP is to increase the number of students that eat school lunch. If the program is successful, then fewer families ought to buy ingredients at supermarkets to send with children as home-packed lunches. Thus, we estimate the effect of the CEP on lunches served in schools as additional evidence that the CEP affects demand for groceries. However, we note that CEP spillovers may be large event absent an increase in school lunches served. In equilibrium, competitors might respond to the CEP so as to maintain market share, for example, by lowering prices.

Table 15: Summary Statistics on Participation and Eligibility for Wisconsin Schools

	(1) District CEP	(2) LEA CEP	(3) Non Part	(4) School CEP
School ISP	0.682	0.679	0.219	0.545
District ISP	0.622	0.556	0.216	0.329
ADP	0.769	0.717	0.534	0.748
Observations	24	255	1749	24

Notes: This table presents summary statistics broken out by whether/how a school participates in the CEP. For example, the sample in column 1 is the set of schools that participate in the CEP with their entire district, so that the district's ISP is the basis for qualification. Data is from Wisconsin AY 2017-2018. ADP is the Average Daily Participation rate for school lunch. The ISP cutoff is 0.4 for individual school eligibility into the CEP.

We investigate substitution patterns using school-level adoption data from Wisconsin in the 2017-2018 academic year. For each school, the Wisconsin Department of Education provided us information on ISP,[17] CEP participation, and ADP, the average daily participation in school lunch. Table 15 provides summary statistics for this sample, broken out by CEP status. Schools participating under the CEP have higher average daily participation rates in lunch, which is consistent with a positive impact of the program. However, these schools also have higher ISPs than those that do not participate, which could drive the pattern in ADP even absent an effect of the program. Most schools qualify for the CEP as part of a Local Education Agency (LEA), which describes any group of two or more schools in the same district. However, twenty-four schools choose to participate individually, which means that their individual ISP must exceed the 40% threshold. We exploit these schools to estimate the causal effect of the CEP on ADP using a regression discontinuity design (RDD), following the methods outlined in Calonico et al. (2017). One concern is that schools may adjust their ISPs in order to qualify for the program; indeed, in section 4 we present evidence of bunching using a national sample of schools. In figure 15, we test for a discontinuity in the density of Wisconsin schools around 40%, but find no evidence of a bunching. We note that while the smoothness of the pdf is reassuring, the fundamental identification assumption that schools do not game ISP in Wisconsin is untestable.

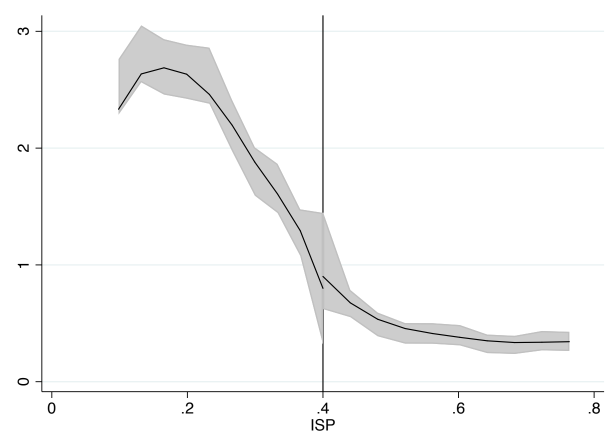
We estimate the effect of the CEP on ADP using the following model:

$$y_i = \beta_0 + \beta_1 \cdot 1\left[ISP_i > 40\right] + \beta_2 \cdot ISP_i + \beta_3 \cdot ISP_i \times 1\left[ISP_i > 40\right] + \beta_4 \cdot log(E)_i + \epsilon_i \quad (10)$$

where ISP_i is the ISP of school i, $1[ISP_i > 40]$ is an indicator for whether the ISP exceeds the 40% threshold, and $log(E)_i$ is log enrollment. We are mainly interested in the coefficient, β_1, which captures any jump in the outcome variable y_i, such as ADP or CEP participation, at the discontinuity. Figure 16 presents the relationship between school ISP and CEP on the left and ADP on the right. There is a clear jump in both CEP participation and ADP rates at 40%. These patterns are mirrored in the regression results, presented in table 16. There is a 16.9 percentage point jump in the likelihood of participation under the CEP at the 40% threshold (column 1). This jump is mirrored by a 6.8 percentage point jump in ADP (column 2). These estimates imply a large impact of the CEP on lunches served; in column 3, we present the fuzzy RD estimates, which indicate that a school near the cutoff experiences a 37.5 percentage point increase in ADP when it adopts the provision. For comarpison, Schwartz and Rothbart (Forthcoming) estimate that universal free lunch increases participation among

[17] The data from Wisconsin includes the true ISP, which governs eligibility for the CEP. Throughout the rest of the paper, we use the percent of free lunch eligible students as a proxy for ISP.

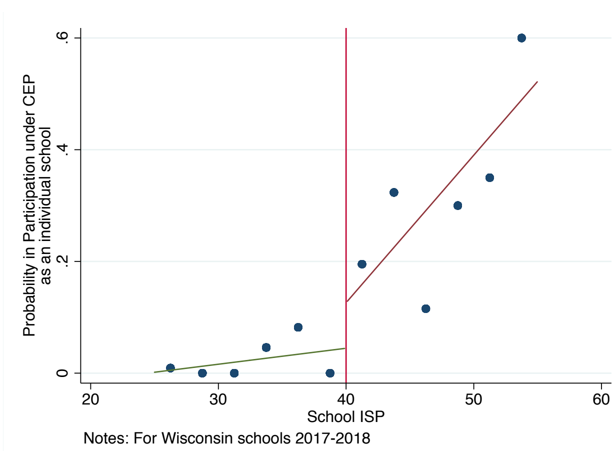
Figure 15: RDDensity Plot: Wisconsin Data



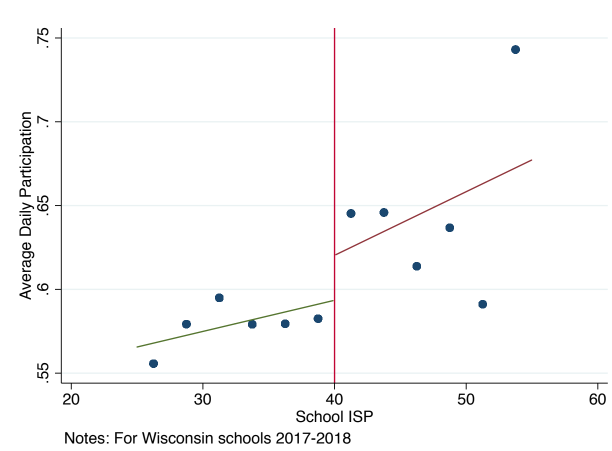
Notes: This figure plots a kernel-smoothed density of ISP rates for Wisconsin schools in the 2017/2018 AY.

non-poor students in New York City by 11 percentage points, which is within the 95% confidence interval that we estimate. While the difference is not statistically significant, the RD point estimate might be larger because it captures the increase in lunches served at marginal schools (with ISPs around 40%) rather than at the average school.

Figure 16: RD Graphs for Wisconsin



(a) Participation under the CEP



(b) Average Daily Participation in Lunch

Table 16: RD Estimates of CEP on Lunches Served

	Reduced Form	First Stage	IV
	(1)	(2)	(3)
	ADP	CEP	ADP
RD_Estimate	0.068**	0.169**	0.375*
	(0.029)	(0.070)	(0.208)
N	2053	2053	2053

Notes: This table presents results of a local polynomial regression-discontinuity design model with robust bias-corrected confidence intervals and an MSE-optimal bandwidth, estimated in Stata via the "rdrobust" command using techniques in Calonico et al. (2017). Coefficients estimate the discontinuity in ADP and CEP adoption at ISP=40% for Wisconsin AY 2017-2018.

These estimates allow us to compute the implied own-price elasticity of school lunch as follows:

$$\begin{aligned}\epsilon &= \frac{66.37}{-100 \cdot Pr\left\{P_0 \neq 0\right\} + 0 \cdot Pr\left\{P_0 = 0\right\}}\\ &= \frac{66.37}{-100 \times .6}\\ &= -1.11.\end{aligned}$$

The numerator comes from the estimated coefficient in table 16 column 3, scaled by the average daily participation at non-CEP schools with ISPs between 30 and 40%. The denominator in the average percent change in price for students at a marginally eligible school. At such a school, 40% of students already qualify for free lunch under the NSLP - for these students, there is no change in the monetary cost of lunch. For the remaining 60% of students, lunch prices fall by 100%, regardless of whether the student qualified for reduced price lunch under the traditional NSLP. The estimates imply that demand for school lunch is elastic.

B.3 Household spending at Nielsen RMS Stores

To reconcile our findings on how the CEP affects household spending at all retailers (a decline of 6%) and RMS store revenues (a decline of 14%) in section 5, we explicitly consider how CEP adoption affects household spending at RMS stores. To be clear, rather than aggregating grocery spending across all trips recorded by panelists, we aggregate only expenditures at stores in the RMS dataset. We estimate specification (2) using this new RMS spending variable. Results are presented in Table 17. The effect magnitudes are statistically and economically meaningful–they imply a 13.5-16% decline in spending among households with children when their local school adopts the CEP. Reassuringly, these effects are on the same order of magnitude as those from the RMS revenue regressions.

Table 17: Household OLS Results - RMS stores only

	Food Expenditures		B/L Expenditures		Number of Grocery Trips	
	(1)	(2)	(3)	(4)	(5)	(6)
CEP	0.049**	0.054**	0.046**	0.052**	0.040***	0.046***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.015)	(0.015)
CEP x School Age Kid	-0.120**	-0.147***	-0.121**	-0.153***	-0.095***	-0.111***
	(0.052)	(0.053)	(0.052)	(0.053)	(0.033)	(0.034)
CEP x lnIncome		0.022		0.017		0.036**
		(0.022)		(0.023)		(0.016)
CEP x School Age Kid x lnIncome		-0.045		-0.077		-0.031
		(0.076)		(0.075)		(0.050)
R-Squared	0.844	0.844	0.833	0.833	0.863	0.863
Observations	200,656	200,656	194,718	194,718	202,582	202,582

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include a constant and household and state by year fixed effects. Standard errors are clustered at zip level. Expenditures are aggregated to the school year, with expenditures from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year. The sample only includes households that exist for all 12 months in any given academic year.

B.4 Interactions with Share Children

As a robustness check, we test whether the CEP has a larger effect in communities with a higher proportion of school-aged children. The idea behind the test is that equal adoption of the CEP should have a larger effect in a state like Utah, where children comprise a relatively large share of the population (1.24 children per family in 2000) compared to West Virginia (0.72 children per family in 2000). [18] To operationalize this test, we use data from the 2010 census on the share of children for each ZIP code and interact this measure with our measure of CEP eligibility: schools with an ISP above 40% in years following their state's adoption of the provision.

Table 18: Summary Statistics on the Share of Children across ZIP codes

	mean	sd	p1	p10	p25	p50	p75	p90	p99
Zip Kid Share	0.17	0.04	0.06	0.13	0.15	0.17	0.19	0.22	0.25
Observations	70427								

In the table below, we present estimates of specification (3) with the share-kids interaction term. We focus on columns 4-6, where the share of children is standardized. In these regressions, the coefficient on CEP adoption is large and negative, but so, too is the coefficient on the interaction term. That is, a one-standard deviation increase in the share children increases the effect of the CEP by approximately 20%. These results bear out our hypothesis that the CEP, in directly affecting hosueholds with children, has a greater effect on store revenues in ZIP codes with a greater share of children.

Table 19: Effect of CEP Eligibility in Areas with High/Low Share of Children

	Not Standardize			Standardize		
	(1) All	(2) B/L	(3) Lunch Meat	(4) All	(5) B/L	(6) Lunch Meat
CEP	-0.011	-0.037	0.039	-0.166***	-0.177***	-0.241***
	(0.062)	(0.066)	(0.071)	(0.018)	(0.019)	(0.020)
CEP x Zip Kid Share	-0.910***	-0.827**	-1.647***			
	(0.347)	(0.373)	(0.411)			
CEP x Zip Kid Share_std				-0.034***	-0.031**	-0.061***
				(0.013)	(0.014)	(0.015)
Observations	63,779	63,779	63,624	63,779	63,779	63,624

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include a constant and store and state by year fixed effects. Standard errors are clustered at store level. Sales are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year. The sample of grocery stores includes both grocery and mass merchandisers.

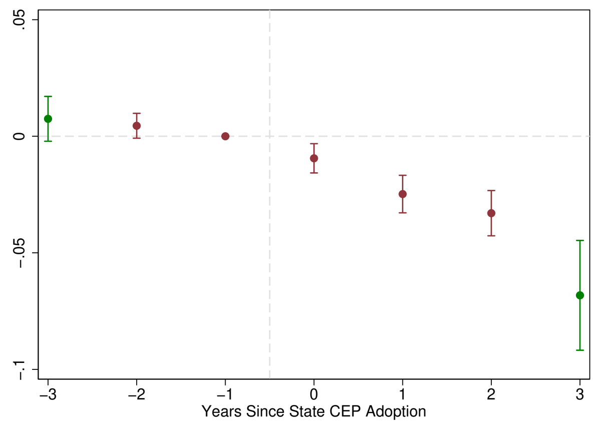
[18] https://www.census.gov/population/socdemo/hh-fam/tabST-F1-2000.pdf

B.5 Alternative Fixed Effects

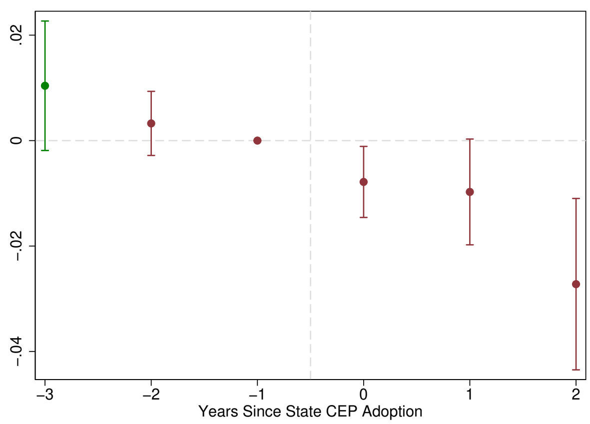
Our main regression specification (4) for estimating the effect of the CEP on store-level outcomes such as revenue and prices includes both store and county×year fixed effects. The aim of the county×year fixed effects is to capture any factors that vary across time and space influence grocery revenues but are not related to the CEP. In essence, we compare within-store changes in revenue for stores near schools that adopt the CEP to stores near schools that do not adopt the CEP, but are located in the same county. In figure 17, we compare estimates across specifications where we allow for alternative fixed effects that allow for different control groups. As a baseline, subfigure 17a presents our preferred estimates. In subfigure 17b, we include state×year fixed effects, broadening the control group, but also ISP×year fixed effects that allow for different time trends for stores in high and low-income neighborhoods. In subfigure 17c, we keep the ISP×year fixed effects but narrow to the county×year fixed effects. The point esitmates are fairly stable across specifications, suggesting that 2 years after the program, stores near schools that are individually eligible for the program to the CEP see a 2-4% drop in revenues. However, there is a semblance of a pre-trend in subfigure 17b. While parallel trends before the introduction of the CEP does not imply that our estimates are causal, they do suggest that the county×year fixed effects may play an important role in isolating economic trends that vary geographically.

Figure 17: Effect of CEP Eligibility on Log Grocery Revenues with Alternative FEs

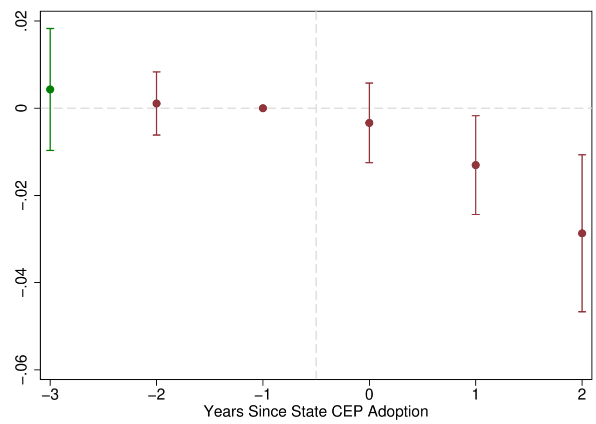
(a) Store and County×Year Fixed Effects



(b) Store and State×Year Fixed Effects + ISP40×Year FE



(c) Store and County×Year Fixed Effects + ISP40×Yr FEs



Notes: This figure plots estimates of the effect of CEP eligibility on grocery store revenues. Subfigures present estimates based on different combinations of fixed effects. Estimates in green are limited to a subset of state-waves for which we observe at outcomes four years after state adoption of the CEP. The regression specification that underlies the estimates is equation 4.