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Essays on Household Spending and the Social Safety Net

Author
Brown, Alyssa

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Essays on Household Spending and the Social Safety Net

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy

in

Economics

by

Alyssa Ashleigh Brown

Committee in charge:

Professor Katherine Meckel, Chair
Professor Jeffrey Clemens
Professor Julie Berry Cullen
Professor Gordon Dahl
Professor Todd Gilmer

2022

The Dissertation of Alyssa Ashleigh Brown is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

University of California San Diego

2022

DEDICATION

to my brother, Ethan, for his unending, unconditional love and support

and

to my therapist, Erica, for guiding me on the path of uncovering my full self

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

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Chapter 3, "The Distributional Impacts of Taxes on Health Products: Evidence from Diaper Sales Tax Exemptions" is co-authored with Chelsea Swete and Kye Lippold, two former UCSD graduate students. The dissertation author was a primary author of this chapter. It is currently being prepared for submission for publication of the material.

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Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

VITA

2016	Bachelor of Arts in Economics and Mathematics, College of William and Mary
2017-2022	Teaching Assistant, Department of Economics University of California San Diego
2018	Master of Arts in Economics, University of California San Diego
2018-2021	Research Assistant, Department of Economics University of California San Diego
2021	Associate Instructor, Department of Economics University of California San Diego
2022	Doctor of Philosophy in Economics, University of California San Diego

ABSTRACT OF THE DISSERTATION

Essays on Household Spending and the Social Safety Net

by

Alyssa Ashleigh Brown

Doctor of Philosophy in Economics

University of California San Diego, 2022

Professor Katherine Meckel, Chair

This dissertation investigates three understudied components of the social safety net and their effects on household purchasing. In the first chapter, I estimate the effect of the Summer Food Service Program (SFSP) on household food spending. The SFSP is intended to serve low-income children who receive subsidized meals during the academic year, but it does not directly means test its recipients. I match a large panel of grocery expenditures to plausibly exogeneous variation in SFSP meal provision and find that households reduce their food spending by 2.7% in response to a 10% increase in the number of meals served in their ZIP code. These spending responses are concentrated among middle-income households, showing that the program has large spillovers to unintended populations.

In the second chapter, I study the effect of food pantries on nearby households and grocery retailers. Food pantries have participation rates rivaling the Supplemental Nutrition Assistance Program, but little is known about how they affect household purchasing and retailer sales. I create a panel of food pantries using data on 501(c)(3) approval dates from the Internal Revenue Service and connect it to large panels of household grocery expenditures and retailer transactions. I use the plausibly exogeneous timing of approval date to find that households reduce their food spending by 1.8% when a food pantry opens in their ZIP code, and these spending reductions are concentrated in the types of foods that are commonly available at food pantries (such as canned vegetables). I find no effect on retailers' aggregate sales but find sales increases on produce and dairy that are suggestive of households' shifting of private consumption toward foodstuffs that may be harder for pantries to distribute.

In the third chapter, we study the effect of sales tax exemptions for diapers on diaper purchasing. We find that low-income households have more elastic demand for diapers using state-level tax changes. We additionally find that low-income households increase their spending by 5.0% when sales taxes are removed entirely from diapers in New York and Connecticut. The results suggest that removing diaper taxes can reduce diaper need.

Chapter 1

Who Takes Up a Free Lunch? Food Spending and the Summer Food Service Program

1.1 Introduction

The United States social safety net is not designed for universal take-up. Welfare programs use means testing to determine eligibility and target benefits to their desired recipients. These eligibility requirements add an additional cost to welfare programs by creating an administrative burden in determining which households can take up these programs. Means testing at the community level, rather than at the individual level, is one way to reduce these administrative costs. When programs use community means testing, all residents of a geographical area become eligible for a program, so each recipient does not have to provide proof of her eligibility. This eases the administrative burden but may lead to less effective targeting: some households in an eligible community may be less needy than some households in an ineligible community. Studying the targeting properties of community means-tested programs is challenging because individual-level data on recipients is often not collected by design.

I investigate patterns of take-up among households living in areas served by the Summer Food Service Program (SFSP), a community means-tested source of free meals for children, using changes in food spending in response to plausibly random variation in SFSP meal provision as indicative of household take-up. The aim of the SFSP is to serve the 41% of children who receive a subsidized school meal through the National School Lunch Program (NSLP) when school is not the session; however, it only reaches an estimated 5% of children.[1] The SFSP's budget is also much smaller, costing $466 million in 2014 compared to the NSLP's $16.3 billion. The smaller budget reflects both lower take-up and the reduced administrative costs associated with community means testing. The SFSP designates neighborhoods (defined by census tracts or block groups) as eligible for free summer meals if at least half of residing children are eligible for NSLP-subsidized meals during the school year.[2] Sites provide meals to all children eighteen years or younger who attend. Any non-profit sponsoring organization that demonstrates an ability to procure food can sponsor a SFSP site in an eligible community, and sites are usually only open for a portion of the summer (often depending on the sponsor's availability or during its concurrent programming).

The targeting properties of a program like the SFSP are both theoretically uncertain and empirically unknown. Empirically, there is little information on take-up of the SFSP because the U.S. Department of Agriculture (USDA) does not collect data on the children served by SFSP. There is also theoretical ambiguity about which children are expected to take up SFSP meals because the relative difficulty of attending these sites for children of different income levels is unknown. This relates to the model of ordeals developed by Nichols and Zeckhauser (1982), which suggests that programs should create barriers to take up if they cannot target their desired population using observable characteristics (as in the SFSP). If a barrier to take-up exists and all groups face the same costs to overcome the ordeal, then only groups with a high value of taking up the program will do so. However, the only barrier for households to take up the SFSP is the effort to help their children find and attend the program. These barriers may be larger for low-income households if they have higher informational or transportation costs than do

1. I calculate these percentages using 2018 program totals and the total number of children enrolled in school. Sources for these values from *School Meal Trends & Stats* (2021), *Census Bureau Reports Nearly 77 Million Students Enrolled in U.S. Schools* (2019), and *Summer Food Service Program* (2021).

2. Children are eligible for free- or reduced-price lunch if their households earn below 185% of the federal poverty line, or under $46,865 for a household of four.

higher-income households. Then the ordeal of the SFSP would target higher-income households into the program, even though they value the program less than low-income households.

I shed light on the targeting properties of the SFSP by investigating its effects on household food expenditures. My identifying assumption is that within-household-summer variation in SFSP meal provision is uncorrelated with underlying trends in food spending. Under this assumption, reductions in food spending in response to SFSP exposure provide evidence of take-up. I create a novel dataset consisting of a large panel of household food expenditures, administrative data on SFSP sites throughout the U.S., and records of meals served through the SFSP. My identification strategy exploits within-ZIP code variation codes in the number of meals served across weeks of the summer.[3] I find that households with children reduce their food spending by 0.1% in response to a 10% increase in the number of meals served in their ZIP code. This intent-to-treat parameter translates to a 2.7% reduction in food spending for households that take up an additional meal. I provide evidence in support of my identifying assumption by testing for a relationship between SFSP meals served and food spending for households without children, who should not have food spending responses to variation in program exposure. I do not find similar effects, providing evidence that my results are not driven by other confounding factors.

I test for heterogeneity in food spending responses by income group to provide the first evidence that children who are not eligible for free- or reduced-price lunch take up the SFSP. I define households as low-income if their income is below 200% of the federal poverty line, as middle-income if their income is between 200% and 400% of the federal poverty line, and as high-income if their income is above 400% of the federal poverty line.[4] I find significant reductions in food spending for middle-income households in response to increased SFSP exposure. Following the logic laid out above, this result shows that middle-income households

3. Figure 1.1 provides an example state-year showing the variation in site open weeks and meal provision by ZIP code.

4. I define low-income as roughly those households who are eligible for free- or reduced-price lunch, middle-income as those who are eligible for Affordable Care Act marketplace subsidies, and high-income as those who are ineligible for any federal anti-poverty transfers.

take up the SFSP, and thus provides the first evidence that SFSP meals are not only provided to the low-income children the program targets. I find small and imprecise coefficients for low- and high-income households, suggesting these households have a lower take-up rate. However, they may instead reduce spending on food-away-from-home in response to the SFSP, or they may reduce their consumption of charitable food provision. Low-income households may also have no private food spending reductions if they spend only the amount they receive in transfers from programs such as the Supplemental Nutrition Assistance Program (SNAP) or the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). These responses are not captured in my data, so I cannot be certain that low- and high-income households do not take up the SFSP.

I estimate how households change the composition of their grocery purchases in response to SFSP meal provision, and find that households increase spending on tobacco and alcohol. Lastly, I test for heterogeneity by SFSP site characteristics to determine whether some sites are better at targeting, and to investigate specific ordeals that may be responsible for takeup by middle-income households. I consider the site's sponsoring organization, and the income level and grocery accessibility of its census tract, and I find little evidence of any differences by site-specific measures.

This paper provides novel evidence on the effects of the SFSP on food purchasing and its targeting properties. Economic research on the SFSP is sparse, though Nord and Romig (2006) study the program and find that summertime increases in food insecurity rates for households with school-aged children are higher in states with lower SFSP participation. Existing literature on other school meals programs tends to focus on how they affect child nutritional and educational outcomes (see Hoynes and Schanzenbach (2016) for a review). Handbury and Moshary (2021) study the effects of increased access to school meals on food spending by studying the roll-out of the Community Eligibility Provision, which allows schools to provide free meals to all students rather than only to those who are low income. These meals are easier for children to take up since they are served when children are already at school, whereas households have to seek out

SFSP sites. They find that household food spending falls and food retailers lower prices.

Recent literature on take-up outside of the school meals programs includes Bhargava and Manoli (2019), who find that take-up of the Earned Income Tax Credit is hurt by low program awareness. They find the effect of a reminder mailer on take-up is highest for those with the lowest benefit amount, so the poorest households with the largest gain from the EITC are also most resistant to taking it up. Deshpande and Li (2019) find that disability office closings reduce applications disproportionately among those with lower levels of education and moderately severe levels of disability.[5] My results suggest the SFSP is primarily taken up by middle-income households, contributing to this broad literature that shows those who can be most in need of government programs can be the least likely to take it up.

1.2 Institutional Background

The SFSP was created in 1968 as a summertime substitute for the NSLP. Unlike the NSLP, the SFSP is community means tested and requires only that sites are located in low-income areas, rather than requiring that every recipient child is low-income. There are multiple ways to designate an area as low-income. Sites can be located in the attendance area of a school with at least 50% of children eligible for subsidized lunch. They may also locate in census tracts or block groups in which at least 50% of children are eligible for subsidize lunch.[6] Finally, sites can open in a census block group that has fewer than 50% of children eligible for subsidized lunch if the average of subsidized lunch eligibility taken over up to three adjacent census block groups is at least 50%. This option further requires that at least 40% of children are eligible in each of the census block groups (*About Area Eligibility* 2021). The majority of sites are classified as open sites, in which all children aged eighteen and younger are eligible for free meals. A smaller number of sites are considered closed and only serve those who are enrolled in the site.

5. These papers use data with direct measures of take-up, while I infer take-up from a reduction in food spending; my empirical strategy is closer to Feldman and Slemrod (2007), who estimate tax noncompliance using differences in the relationship between charitable contributions and income by the source of income.

6. Note census tracts are small, usually covering only between 2,500 and 8,000 people, so by matching households to SFSP sites by ZIP code I assign many households in non-eligible census tracts to have SFSP access.

These closed sites must operate in eligible areas as described above, or use individual means testing with at least 50% of their attendees eligible for free- or reduced-price lunch. Finally, a very small number of sites serve the children of migrant farm workers and are classified as migrant sites. Public records data from the USDA Food and Nutrition Service (FNS) are only available for open sites, and so all discussion of SFSP sites is limited to open sites in this paper. A final federal requirement of note is that sites are required to send a public media notice, and open sites must display a banner stating that free food is available for all children. These low advertising requirements have led SFSP sites to struggle with outreach, and may partially explain why take-up of the program is low.[7]

State agencies are responsible for approving sponsors, monitoring sites, and submitting reimbursements to the USDA FNS, but the rules of the program are set at the federal level. Sponsors must be tax-exempt public or private non-profit institutions with demonstrated ability to procure food and manage its distribution. I break these institutions into three groups: schools, community organizations such as food pantries or nonprofits that serve children, and churches. Sponsors may run multiple sites.

There are two other federal programs that provide food to low-income children during the summer. For my empirical strategy to be valid, these programs must not be correlated with SFSP meal provision. The first is the Summer Electronic Benefit Transfer to Children (SEBTC). This experimental program began in 2011 and provides children eligible for free- or reduced-price lunch with Electronic Benefit Transfer (EBT) cards during the summer. Only two states in my analysis operated this pilot program, and it's unlikely that my sample contains many households treated by this program (*Summer Electronic Benefit Transfer for Children (SEBTC)* 2013). Furthermore, EBT cards are provided for the entire summer, so there is no within-summer variation in the program that could be correlated with the intensive margin of SFSP availability.

The second and larger program is the Seamless Summer Option (SSO), which allows

7. A Facebook campaign advising parents on how to find SFSP sites near them was so widely thought by parents to be a scam that fact-checker Snopes wrote an article about the campaign (MacGuill 2017).

school food authorities to provide free summer meals as an extension of the NSLP. The SSO provides a more "streamlined" approach, according to the USDA, but many schools instead choose to provide meals through the SFSP because of its higher reimbursement rates (by \$0.30 per lunch), less frequent state monitoring, and fewer restrictions on open hours (*Comparison of Programs SFSP/NSLP/Seamless Option* 2015). SSO data are not part of the USDA's public records, and so data on their location, hours open, and meals served are only available through public records requests of state administering bodies. Most states either do not use the SSO or do not keep records of the site open dates that are critical for my empirical strategy; therefore, I limit the scope of this paper to SFSP sites.[8] This focus may create bias if some ZIP codes that are untreated by the SFSP are actually treated by SSO sites. I mitigate this potential confound by limiting the untreated portion of my sample to households without SFSP access who live in states with very small or no SSO programs.

1.3 Data

1.3.1 Food expenditures

I use data on household food expenditures from the Kilts-Nielsen Consumer Panel for years 2015-2019. These data include all purchases made by a sample of 40,000 to 60,000 households each year, recruited to create demographically representative samples of the markets in which they live. Participating households use a home scanner to send the Universal Product Code (UPC) of every purchased item to the Nielsen Company, as well as the amount spent on the item and its date of purchase. The data include purchases from grocery stores, department stores such as WalMart, and convenience stores. I estimate the effect of the SFSP on food purchases in the main results.[9] I further test for heterogeneity by specific product categories to determine the types of foods that have the largest spending reductions. Because the data are only available for goods with UPC codes, there is reduced coverage of some grocery store foodstuffs like fresh

8. Table 1.A.1 details the relative size of the SSO by state.
9. I exclude alcohol from the aggregate food spending measure, but include all other food and drink products.

produce or bulk foods, though Allcott, Diamond, Dubé, Handbury, Rahkovsky, and Schnell (2019) show that a majority of produce spending is made on the packaged produce with UPCs that are included in the data. Also left out are all restaurant or fast-food purchases. If households tend to reduce their fast-food spending in weeks with high SFSP access, for example, I would understate effects on their total food budget.

In addition to detailed purchasing information, Nielsen also collects demographic information from households, including their ZIP code of residence, race, household composition, and two-year-lagged income group. I use this demographic information to classify households as low-, middle-, or high-income, and to match them to SFSP sites.

1.3.2 SFSP sites

I create a dataset with geographical and temporal variation in the number of SFSP meals served by collecting data from the USDA FNS and from state Freedom of Information Act (FOIA) requests. The federal data include information on the address of the site, the dates open, and the sponsoring organization for all nationwide SFSP sites for years 2015-2019.[10] I classify sponsoring organizations into either religious organizations, schools, or community centers using key words in their names.[11] I map each site's coordinates to its census tract and merge in census tract characteristics from the USDA Economic Research Service's 2019 Food Access Research Atlas.

SFSP sites vary considerably by size depending on the sponsoring organization and the capacity of the site. To obtain information on the number of meals served, I submitted FOIA requests to state administering bodies and received records of meals served from sixteen states (listed in the first section of Table 1.A.1) for years 2015-2019.[12] States maintain records of total meals served by site at the monthly or annual level. I construct a measure of weekly meals served

10. I received these records by submitting a FOIA request to the USDA FNS.
11. The list of words used to define each category is provided in Table 1.A.2.
12. States were not included in the meals served data if they rejected my FOIA request or if the state did not keep records for more than one or two previous years.

by dividing the number of meals served per month or year by the number of weeks that site was open in the given year. If the data are provided at the sponsor level, I additionally divide the meals equally across sites provided by the same sponsor.[13][14] I merge this state-level data with the USDA FNS data for years 2015-2019 by a fuzzy string match on sponsor and site name; over ninety percent of the sites in the state-level data merge. I sum the number of meals served across all sites within each ZIP code-week, divide by the number of children residing in the ZIP code using 2013-2017 five-year averages from the American Community Survey, and then multiply by 1,000. I use this number of meals served per 1,000 children as the best available measure of SFSP meal provision.

I do not observe the true number of meals available, but rather the number of meals served. This conflates the demand and supply for SFSP meals into one measure. However, meals that are available but not served should not affect household food spending since no child consumes them. Similarly, if a site runs out of meals, the children who do not receive a meal resume their usual summertime food consumption. I expect the number of meals consumed in a given ZIP code-week to reduce household food spending more directly than would a measure of the number of meals available. Furthermore, survey evidence suggests that most sites do not have large gaps between the number of meals available and the number of meals consumed. Gordon and Briefel (2003) describe a survey of SFSP site organizers which found that 81% of sites serve more than 90% of their available meals on a given day, and only 22% reported their site had run out of meals during one or more mealtimes of the summer.

I merge the SFSP meals served data with household food spending by ZIP code-week.[15]

13. As a hypothetical example: suppose the Boys and Girls Club of San Diego runs a site "Beach" in ZIP code 92109 and another site "Park" in 92103. Suppose "Beach" is open for two weeks in June and two weeks in July, and "Park" is open for three weeks in July. The Boys and Girls Club has 100 meals reported in June and 150 in July. I assign "Beach" 50 meals in each of its June weeks, and both "Beach" and "Park" 30 meals in each of their July weeks.

14. The median number of sites open per sponsoring organization/day is two, so taking the average across sites within sponsor is a close approximation.

15. I use the ZIP code of the SFSP site's address, but site eligibility is determined by census tract or block group, which may be only partially included within that ZIP code. Thus a site may be targeted to children who live in adjacent ZIP codes as well. This measurement error could attenuate my results.

I limit the treated portion of my sample to household-years with children who have at least one SFSP meal served in their ZIP code of residence. I drop all household-years with children who are treated by the SFSP but live in a state that did not provide meals served data, reducing the sample of all Nielsen households with children by 44%.[16] I include as controls all household-years with children who do not have access to the SFSP, dropping those who live in states with SSO programs of large or unknown size (listed in the third and fourth sections of Table 1.A.1) since they may have access to a close alternative to the program. This reduces the sample by another 26%, leaving 29,118 household-years with children in the sample. I run placebo tests on households without children.

I construct weeks as Monday-Sunday to be consistent with SFSP site schedules.[17] The SFSP reimburses sites for any meals served between May 1 and September 30, but most sites are not open when school is in session. Limiting the sample to the weeks in which children are on summer break is necessary because I do not want to conflate changes in SFSP access with changes in the school calendar, which likely also affect food spending independent of the SFSP. Given that the average length of summer break is eleven to twelve weeks long with most school breaks starting in May or June and ending in August or September, I limit the sample to the ten weeks of summer with the highest number of SFSP meals served, as these weeks are the most likely to be contained within most households' summer breaks (Nelson 2014). These are the ten weeks of summer between mid-June and late-August, shown in Figure 1.3 as weeks seven through sixteen (or between June 15 and August 23, 2015) from the 22-week window when SFSP sites can operate. I also present estimates for a twelve-week (weeks six through seventeen) window for robustness.

I use this approximation of summer weeks to define my main sample because determining the exact school calendars that households face is largely not possible. Academic calendars in

16. The states with treated households are shown in Figure 1.2.

17. Almost all sites open on a Monday or Tuesday and then end on a Friday, Saturday, or Sunday. If I constructed weeks as Saturday-Friday or Sunday-Saturday, the final week of SFSP provision will be counted as two separate weeks for some sites.

the United States vary by school district (and sometimes by school), so finding the exact dates of summer break for each child in my sample is infeasible without knowledge of which school they attend. As a second-best solution, I match households to the school district that covers the largest fraction of their ZIP code using a crosswalk from the National Center for Education Statistics, but there is still no central repository of school calendars by school district. I submitted public record requests for these data to the treated states in my sample, and also benefited from undergraduate research assistance in collecting a small subset of school district calendars from Google.[18] The main sample shrinks by 86% when limiting the sample of states to those with school calendar data. I use this reduced sample for robustness checks of the main 10-week results.

Summary statistics for household demographics are provided in Table 1.1. The only significant difference between households with and without access to the SFSP is that households with access are lower income. This is as expected since treated households must live in ZIP codes with lower-income census tracts or block groups that are eligible for SFSP site provision. Table 1.2 provides summary statistics for the number of meals served. Each column includes only the treated portion of the sample: household-years with children who live in a ZIP code with at least one SFSP site open for at least one day during the summer, and who live in one of the sixteen states which provided data on meals served. All other households in the sample have zero meals served. The data are at the household-year-week level for the preferred ten-week specification. I separate the summary statistics by income group because I separate my main effect by income group, and want to ensure the results are not driven by differential access to the SFSP across income groups and site types. The income groups have very similar percentages (around 80%) of the summer covered by an SFSP site. The percentage of the summer covered by site types is also near identical across the three groups. The number of meals served is broadly similar, but low-income households have slightly higher meals served per capita across all site types. This implies the sites are weakly targeted to low-income households at the ZIP level; more

18. The states with data on school calendars are shown in Figure 1.4. Thank you to Jaehyun Choi, whose research assistance made this robustness check possible.

meals are served, but not for a larger fraction of the summer.

1.4 Empirical strategy

1.4.1 Theoretical framework

The estimating equations test for household food spending reductions in response to weekly variation in the number of SFSP meals served. I do not directly observe take-up of the SFSP in the Nielsen data and instead infer take-up from changes in food spending. A basic model of how household spending changes with in-kind food provision provides support for why I expect households who take-up the SFSP reduce food spending. As shown in Figure 1.A.1, the budget constraint shifts out in weeks with SFSP access so households can have x_T of equivalent food spending without reducing any of their non-food spending. Consumption of both food and non-food goods increases in these weeks, but household personal spending on food falls. Therefore I expect households that take-up the SFSP will have reduced food spending in response to increased SFSP meal provision.

I map the parameters from the income heterogeneity regressions to take-up under the assumption that reductions in food spending are indicative of take-up. This holds if the number of SFSP meals served is exogeneous to trends in household food spending. Households may take up SFSP meals and not reduce their observed food spending if they instead reduce spending on food from sources other than grocery stores (such as if they reduce their spending at restaurants). Countering this hypothesis, Handbury and Moshary (2021) find that households reduce their grocery spending when they gain access to school meals during the academic year, suggesting that free meals for children are substitutable with grocery foodstuffs. Another consideration is that low-income households' food spending may be constrained by the in-kind food benefits they receive. Households that receive SNAP or WIC benefits may not reduce their food spending when they take up SFSP meals because the transfers are not fungible. This may hold for a subset of the low-income households in my sample, but Hoynes, McGranahan, and Schanzenbach

(2015) find that 61% of households reporting SNAP benefits spend more than their predicted benefit amount, and thus would have some margin to reduce food spending. Given this evidence, a null effect of SFSP meal provision on food spending is suggestive of low take-up.

1.4.2 Estimation

I use variation in the number of SFSP meals served across weeks within a given household-year to estimate how food spending changes in response to increased SFSP meal provision. The main estimating equation is

$$food_{iwt} = \alpha + \beta meals_{zwt} + \gamma_{it} + \delta_{wt} + \varepsilon_{izwt} \tag{1.1}$$

where i is household, z is ZIP code, w is week, and t is year. The dependent variable $food_{iwt}$ is the dollar amount of food spending by household i in week w and year t. This variable is equal to zero in one-quarter of the observations, and is highly skewed.[19] I normalize food spending using the inverse hyperbolic sine of food spending, which is interpreted equivalently to the logarithmic transform for sufficiently large values and is well-defined at zero. I use the same transformation for the independent variable, $meals_{zwt}$, which is the number of meals served per one thousand child residents in that ZIP code-week.[20] The coefficient β is interpreted as the elasticity for the effect of SFSP meal provision on household food spending for sufficiently large values of $food_{iwt}$ and $meals_{zwt}$.[21] The coefficient is an intent-to-treat parameter, and the treatment-on-the-treated should be over ten times as large since fewer than ten percent of households with children take up the SFSP. Standard errors are clustered at the ZIP code level as this is the level at which treatment occurs. Household-year fixed effects γ_{it} are included to control for omitted household

19. Weekly food spending is an approximation of the household's food consumption in that week, since household-weeks with zero food spending either consume food leftover from previous weeks or rely on non-grocery foodstuffs. I show in Table 1.A.3 and Table 1.A.4 that SFSP meal provision predicts zero food spending and the number of grocery trips only very slightly, and so β is largely estimated on the intensive margin of food spending.

20. The number of meals served also has many zeros, primarily from the household-years without any SFSP meal provision who always have zero meals served. Using the inverse hyperbolic sign allows me to keep these household-years in the sample for estimation of time fixed effects, but they are not used to estimate β.

21. Bellemare and Wichman (2020) suggests "sufficiently large" = 10.

characteristics (such as income) that do not vary over the summer and could be endogenous to purchasing behavior. I use household-year fixed effects rather than household fixed effects because these omitted household characteristics may change across the years, but are less likely to change within a summer.[22] I include week-year fixed effects δ_{wt} to control for seasonality in purchasing behavior.

Identification comes from the plausibly exogenous assignment of SFSP meal provision across weeks within each ZIP code. The identification scheme uses high-frequency, weekly variation in food spending and SFSP meals served within each summer. This precise timing allows me to include household-year fixed effects and rule out differences that may occur across summers within a ZIP code.[23] Within a summer, most households do not have access to the SFSP every week, nor do they have the same number of meals served in each week, as shown in Table 1.2. A typical example of how SFSP meal availability varies across ZIP code-weeks is provided in Figure 1.1. Most ZIP codes have some access to the SFSP between mid-June and mid-August, but the intensity of the number of meals served varies across weeks within each ZIP code. These week-to-week changes are driven by the opening and closing of individual sites or variation in the number of meals served within continuously open sites. Sites open and close following the idiosyncratic schedules of their sponsoring organizations, rather than in response to household grocery spending trends. Gordon and Briefel (2003) surveyed SFSP sponsors and found that many sponsors choose to run their sites when they have existing programming for low-income children and when they have staff available. Sponsors report they cannot extend the length of their sites because of financial constraints and site constraints, such as when schools must be vacated for required maintenance. These factors vary widely across ZIP codes and are likely unrelated to underlying trends in household food spending within the ZIP code-summer.

22. Using ZIP-year fixed effects yields identical results after collapsing to the ZIP-year level and weighting by the number of households in each ZIP-year.

23. One example is if ZIP code income falls across years. This may increase SFSP meal provision through increased area eligibility or increased take-up, and may reduce food expenditures through the income effect. Within a short time period, food expenditures are unlikely to be affected by any other confounding factor that may also affect SFSP meal provision, such as area income.

Consider if, despite this evidence, there is an unobserved variable that drives both food spending reductions and take-up of SFSP meals. Suppose low-income households rely on the SFSP more at the end of the month when their SNAP benefits run out. Then their observed food spending is lower and the number of SFSP meals served in their ZIP code is higher at the end of the month mainly because of the SNAP benefit schedule, and not because of SFSP crowd-out. This changes the interpretation of the main result, but I still expect to find a negative and statistically significant relationship between food spending and SFSP meal provision for low-income households, even if some unobserved other variable is driving that relationship. The coefficient is no longer a causal effect of the SFSP on food spending, but it remains indicative of take-up.[24]

Another potential confound is if grocery prices fall in weeks with access to the SFSP. However, Goldin, Homonoff, and Meckel (2022) find that retailers do not change prices across weeks within a month in response to SNAP benefit cycles, which cause predictable changes to customer composition and food spending. It is unlikely that retailers change prices in response to the SFSP when they do not respond to SNAP benefit cycles, as the SFSP is a much smaller program and does not follow a predictable schedule. Within chain retailers, Dellavigna and Gentzkow (2019) find uniform pricing across stores, making it even less likely that individual stores respond to SFSP exposure in their area.

I test for the presence of confounding factors by running the same regression on the placebo group of households without children. These households should not have a large response to SFSP meal provision since SFSP meals are only provided to children; finding such a response would imply that some other summertime factor correlated with SFSP access is the real driver of household food responses. Households without children are not a perfect placebo group, as a 2015 nationally representative sample of SFSP sponsors reports that 17% of SFSP sites offered paid meals for adults (*Summer Food Service Program Infographic* 2019). Thus I may find an

24. In this hypothetical, I've assumed that I have weekly data on the number of SFSP meals served each week, but recall that data are aggregated monthly and across sites. The measurement error in the exact number of meals served protects against this particular endogeneity concern.

effect of SFSP meal provision on their household meal spending, but it should be much smaller than the effect for households with children.

A final issue with using weekly variation is that households likely do not consume all food purchased each week; households may anticipate SFSP meal provision and purchase less before they begin taking up the program, or, they may purchase less after the program ends. I test for the presence of anticipatory and lagging effects of the SFSP by including leads and lags of SFSP meal provision in a modified version of Equation 1.1:

$$food_{iwt} = \alpha + \sum_{i=-6}^{6} \beta_i meals_{zwt} + \gamma_{it} + \delta_{wt} + \varepsilon_{izwt} \tag{1.2}$$

This equation includes the prior six weeks and future six weeks of SFSP meal provision for each of the ten weeks in my main sample.

I return to the main specification to investigate whether household reductions in spending are concentrated in certain types of foods that may be more sensitive to weekly variation SFSP meal provision by breaking down food spending into its components, and by constructing groupings such as SFSP-reimbursable foods, perishable versus non-perishable foods, and tobacco and alcohol. I run equation one using the inverse hyperbolic sine of these sub-components of household spending as the dependent variable.

I next investigate heterogeneity in food spending responses. I consider differential food spending responses to SFSP meal provision by household income group to provide the first evidence on the income status of SFSP recipients, interacting the number of meals served with three income group indicators:

$$\begin{aligned} food_{iwt} = &\alpha + \beta_1 meals_{zwt} \times \text{low-income} + \beta_2 meals_{zwt} \times \text{middle-income} \\ &+ \beta_3 meals_{zwt} \times \text{high-income} + \gamma_{it} + \delta_{wt} + \varepsilon_{izwt} \end{aligned} \tag{1.3}$$

where low-income is an indicator equal to one if household income is below 200% of the federal poverty line, middle-income is an indicator equal to one if household income is between 200%

and 400% of the federal poverty line, and high-income is an indicator equal to one if household income is above 400% of the federal poverty line.

I further run Equation 1.3 using the inverse hyperbolic sine of the sub-components of food spending as the dependent variable to learn if the types of foods that are crowded out are different by household income. I then estimate variations of Equation 1.3 using other indicator variables: type of site sponsoring organization, site census tract's grocery store accessibility, and the income of the site's census tract relative to its ZIP code. I also interact these indicator variables with income to determine site types' targeting properties.

1.5 Results

1.5.1 Main results

I first run the main specification to find how SFSP meal provision affects household food spending. I regress the number of meals served per 1,000 child residents on the amount of household food spending, taking the inverse hyperbolic transform of both variables to adjust for skew and allow for zeros. Results are shown in Table 1.3. I find that a ten percent increase in the number of meals served per one thousand child residents reduces food spending by 0.077%. I perform a back-of-the-envelope calculation to interpret the magnitude of this main estimate. A 10% increase in meals served at the mean corresponds to an increase of 29 meals per 1,000 children, so approximately 2.9% of children receive an additional meal. The resulting 0.077% reduction in grocery spending is the average taken across all households, 97.1% of whom should have no change in spending. The 2.9% of households who receive an extra meal must then reduce their food spending by 2.7%.[25]

The estimate seems high if households prepare all meals at home and spend equal amounts on all household members: a four-person family with three meals per person/day consumes 84 meals per week, and so should reduce spending by 1.2% in response to one provided meal. But

25. $0.029 \times 0.0266 + 0.971 \times 0 = 0.00077$

given that the mean weekly grocery spending of households in my sample is \$66.01, a 2.7% reduction of spending corresponds to \$1.78, which seems reasonable for a child's meal. Also consider that many SFSP sites are open for several hours with activities, and the child may not only miss lunch at home but also any snacks or juices she would typically eat. The result indicates that the SFSP can procure meals for children at a similar cost as their families, with reimbursement rates ranging between \$0.88 and \$3.77 per meal, depending on the meal type (*Summer Food Service Program 2017 Reimbursement Rates* 2017).

I consider if household spending responses are contemporaneous with SFSP meal provision by running Equation 1.2, including six weeks of lags and leads of the number of meals served. Table 1.4 finds a similar magnitude for same-week meal provision, albeit with significance lowered to the 10% level. All lags and leads are smaller in absolute value and statistically not significant. This is suggestive that weekly variation in SFSP meal provision affects weekly food spending, and the program does not have strong anticipatory and lagging effects that might otherwise encourage using annual or geographical variation.

I investigate if the program is well-targeted by allowing the effect of SFSP meal provision on food spending to vary across different income groups. A negative spending response is consistent with take-up of the SFSP, though a null spending response is not definitive proof that an income group does not take up the SFSP, as households may take up the program and not reduce their grocery spending. Table 1.5 provides results for Equation 1.3. I find a large and significant reduction in spending for middle-income households, with a ten percent increase in the number of meals served reducing food spending by 0.17%. The coefficients are smaller and imprecise for low- and high-income households. The coefficient for middle-income households is statistically distinguishable from low-income households at the 1% level, and from high-income households at the 5% level. Assuming SFSP meal provision is exogeneously determined, these results demonstrate that middle-income households take up the SFSP despite not being the targeted group.

1.5.2 Food spending heterogeneity

The main results show that the SFSP reduces food spending for households with children. An important policy consideration is to learn which types of foods the SFSP crowds out, since the SFSP may not only ease the increased cost of meal provision during the summer, but it may also alter the composition of foods consumed if what is available at home differs from what is available at the SFSP sites. I break food spending down into its six main components, and consider that the dry grocery and frozen categories likely contain most of the low-nutrition foods that would preferably be crowded out by SFSP meals.

Table 1.6 displays results for equation one, with each column using a different component of food spending as the dependent variable. The effects of SFSP meal provision are largely similar in size and magnitude across columns and when compared to the main result for food spending. The only insignificant and near-zero coefficient is that on produce, providing some suggestive evidence that households use produce given out at SFSP sites to supplement, rather than replace existing produce purchases. Households appear to reduce their food spending broadly across all other categories.

Households may take up the SFSP and change the composition of the foods they purchase rather than changing the amount they spend. I estimate equation three on these types of food spending in Table 1.7 to look for changes in the composition of broad food spending categories across household income groups. As with the main results, results are driven by middle-income households, who have negative and statistically significant effects for every spending type but produce. All coefficients for low- and high-income households are statistically insignificant and small.

I next create categories based on the types of food that I expect SFSP meals would crowd out. Table 1.8 shows results for equation one using seven such categories. I first create a category called "SFSP" that includes spending on all reimbursable foods that sites can provide to children.[26] The coefficient on SFSP is nearly identical to that on food spending, implying that household reductions in food spending are not concentrated among the types of foods their child consumes at SFSP sites. However, households may not ex ante purchase the types of foods the SFSP provides, such as if an SFSP breakfast crowds out a toaster waffle rather than oatmeal. I create a second category called "Breakfast" that includes all conventional breakfast foods, and a category called "Lunch/Dinner" that includes all conventional lunch and dinner foods. I further classify foods as perishable or non-perishable, as households may reduce their purchasing of perishable foods more than non-perishable foods if there is uncertainty about the amount of at-home food the child will consume when they take up the SFSP. Columns (2) through (5) display the results for these four categories, with all coefficients similar in size and magnitude to the main food spending result.

Households may shift their spending away from food toward other non-food purchases when they take up the SFSP, as in Figure 1.A.1. I first consider tobacco and alcohol purchases in Column (6), and find that a 10% increase in the number of meals served increases the amount spent on tobacco and alcohol by 0.02%. This is about one-third of the size of my food spending result, and significant at the 5% level. This is suggestive that households choose to increase their spending on adult economic bads when their children have a consistent source of meals. The effect on all nonfood spending in Column (7) is negative and statistically insignificant. The negative coefficient could be driven by a reduction in incidental purchases from households making fewer grocery trips.

Table 1.9 considers the same seven categories, allowing for heterogeneity in spending responses by household income group. The coefficients for low-income households are small and statistically insignificant save for marginal significance on nonfood spending. This is further evidence that low-income households are not responding to the SFSP by shifting the composition of their purchases rather than the amount spent, and thus they may not be taking up the program.

26. Figure 1.A.2 shows the foods available at SFSP breakfasts, Figure 1.A.3 shows the foods available at lunches and dinners, and Figure 1.A.4 shows the foods available at snacks.

Effects in Columns (1)-(5) are similar in size and significance to the main food spending result for middle-income households; households do not appear to change the types of foods they purchase when their children take up SFSP meals. The effect for non-food spending in Column (7) is large in absolute value and statistically significant for middle-income households, despite the theoretical predictions in Figure 1.A.1. Again this may be driven by making fewer grocery trips, or by other product types that are complements to food; investigating this question in more detail is difficult given the frequency of zero spending on individual product modules, and the limitations of the inverse hyperbolic sine transformation when a large fraction of the data are equal to zero. All effects for high-income households are small and insignificant.

1.5.3 Site type heterogeneity

I next test if some site types are better at targeting low-income households than others, starting with consideration of site sponsoring organization. Table 1.10 displays results, with the number of SFSP meals served in each ZIP-code week separated by site sponsoring organization type in Column (1) and then interacted with household income group in Column (2). Column (1) finds the effects for the number of meals served by schools and community centers are similar in size and significance to the main result. The effect of meals served by religious organizations is small and insignificant, though this may be because religious organizations serve fewer meals overall and there is less power available to identify an effect. Column (2) shows these results are again driven by middle-income households. The coefficient on religious meals served by middle-income households is similar in size to that on school meals and community center meals, though it remains statistically insignificant. Coefficients for low- and high-income households are consistently insignificant, though magnitude and direction vary. These results are in-line with take-up of school and community meals primarily by middle-income households.

Column (1) of Table 1.11 finds that take-up of the SFSP is concentrated among households who have low access to grocery stores. The coefficient on the number of meals served in low-access census tracts is similar in size and significance to that in the main food spending result;

the coefficient on accessible census tracts is near zero and imprecise. Column (2) interacts site accessibility with income group and finds that the significant effect in low-access census tracts is once more driven by middle-income households, with small and imprecise coefficients for low-income households. Though not the targeted group, middle-income households may have more awareness of the SFSP, and may be more likely to take up the program when the alternative is costly (buying groceries to prepare childrens' meals and transporting them home). This result suggests a unconsidered cost of food deserts: meal programs may be less able to reach their targeted group when the ordeal cost of obtaining a free meal is lower than the cost of visiting a grocery store.

In a final heterogeneity test, I estimate whether the effect of SFSP meals served varies by the income of a site's census tract relative to its ZIP code. Sites in lower-income parts of a ZIP code may be better-targeted to low-income households than sites in the wealthier part of a ZIP code. Column (1) of Table 1.12 shows that effects are indeed driven by meals served in lower-income parts of the ZIP code; however, Column (2) shows these meals are still taken up by middle-income households. Coefficients for both site types remain small and insignificant for low-income households. Targeting properties are no more desirable when sites are open in relatively low-income areas.

1.6 Robustness of main results

I run several robustness checks for the main results (equations Equation 1.1 and Equation 1.3). I first widen the window to include the twelve weeks of summer with the highest number of SFSP meals served, rather than the ten week window in my preferred specifications. Table 1.A.5 shows qualitatively similar results. I then narrow the sample in Table 1.A.6 to only include summer weeks for ZIP codes whose plurality school district had school calendar data. The sample size is much smaller and coefficients are statistically insignificant, but similar in magnitude (save for the coefficient for high-income households, which is large and positive).

These results are broadly in line with my main findings.

Another strategy for controlling for differences in school calendars is to estimate the week-year fixed effects separately by county or school district, assuming that school calendars are largely similar within these regions. The main results are nearly identical when estimating county-week-year fixed effects, as shown in Table 1.A.7, providing further evidence that varying school calendars are unlikely to drive the results. However, Table 1.A.8 shows that statistical significance is eradicated when controlling for school district-week-year fixed effects. This is likely because I assigned school districts based on household ZIP code, and most school districts only correspond to one ZIP code in my data. Treatment is defined at the ZIP code-week-year level so this severely minimizes the amount of variation available to identify an effect.

As a placebo test, I run the specification on households without children, assigning to them the number of meals served per 1,000 children in their ZIP code. The SFSP should not affect the food spending of households without children as much as it affects food spending for households with children, so any large and significant coefficients would provide evidence that some other factor correlated with SFSP meal provision is driving the main results. Table 1.A.9 finds small, imprecise coefficients, with marginal significance only for high-income households in Column (2). The coefficient in column one is about one-third of its equivalent on the sample of households with children, perhaps reflecting the small percentage of sites that offer paid meals to adults, though it remains insignificant. These estimates provide further evidence that SFSP meal provision, and not some confounding factor, drives the results in the main specification.

My preferred specification uses variation in the number of SFSP meals served within a household-summer. The average household is treated for eight of the ten summer weeks, so much of the variation used is from the intensive margin in plausibly exogeneous variation in the number of meals served. Variation on the extensive margin is likely correlated with when students are out of school. Table 1.A.10 provides results using an indicator for whether at least one SFSP site is open in a week. The coefficient in Column (1) is strongly statistically significant and implausibly large in magnitude: food spending is 5.5% lower in weeks with SFSP access;

given that less than 5% of households with children are estimated to take up the SFSP, this corresponds to a more than 100% decrease in spending among households who take up. The results for Equation 1.3 in Column (2) are consistent with those from the main results, with effects only large in absolute value and statistically significant for middle-income households, but the size remains too large to be driven by the SFSP. This robustness check demonstrates the importance of using intensive variation in SFSP meal provision.

The inverse hyperbolic sine is widely used in the economics literature, but there are estimation concerns driven by its arbitrary shift of values near zero (similarly to log(1+x), which is also commonly used). Approximately one-quarter of the food spending observations and one-fifth of the meals served observations are equal to zero in the sample that identifies my effects. This is lower than the one-third maximum recommended by Bellemare and Wichman (2020), but it remains important to ensure the results are not driven by the zeros in the data. First, I create biweekly aggregates of food spending and the number of meals served, such that the number of meals served is now equal to zero for only 14% of the observations and food spending is equal to zero for 9% of the observations. I also consider that the number of meals served is more likely to be equal to zero at the beginning and end of the summer, so I drop the first and last two weeks and create a six-week sample that has 5% of its meals served observations equal to zero (and again 9% of its food spending observations equal to zero). Table 1.A.11 displays the main results using these two-week aggregates in the main ten-week sample and in the narrower six-week sample. The magnitude of the coefficients is qualitatively similar to those in the main results, though the statistical significance is lost in the six-week sample. This is expected because there is little variation across just three observations per household-year. These results show that the zeros in the main results are unlikely to drive the size or significance of the main results.

A second concern in using the inverse hyperbolic sine is that it is sensitive to scaling parameters. Bellemare and Wichman (2020) suggest using the inverse hyperbolic sine only for values of the independent and dependent variables that are greater than ten. Most of the non-zero observations are above this threshold, but to check if the results are sensitive to their scale, I multiply the raw data by k=10, 100, and 1,000 and then take the inverse hyperbolic sine to estimate my main results in Table 1.A.12. The coefficients are not perfectly stable, but the changes across scaling parameters are relatively small and consistently show that the effect on food spending is driven by middle-income households.

As a final robustness check, I consider variation across years within household-weeks. This controls for time-invariant household characteristics, but ZIP codes may have increased SFSP meal provision in one year for reasons that correlate with household food spending. Further, many households are only in the Consumer Panel for one or two years, and there is not much variation in the number of meals served within household-weeks. Despite these shortcomings, I estimate a first-differenced variation of equation one in Table 1.A.13. The coefficient in Column (1) is one-third smaller than the coefficient in the main result and is no longer statistically significant. Column (2) shows broadly similar effects for households of all income types, but none are significant. This robustness check suffers from too many limitations to be conclusive, but is included to demonstrate the importance of using within-household-summer variation.

1.7 Conclusion

In this paper, I estimate the effects of the SFSP on household food expenditures, and find evidence that middle-income households who are not eligible for NSLP subsidized meals take up SFSP meals. There is little existing evidence of the program's ability to lower the summertime food spending burden for households with children, nor is there much evidence that the children who take up SFSP meals are low-income. To examine how households respond to SFSP meal provision, I construct a new dataset of SFSP meals served by ZIP code-week. I connect data from the USDA and state-level FOIAs with detailed household grocery expenditure data to estimate how within-household-summer variation in the number of SFSP meals served within a household's ZIP code affects household food spending. I find households with children reduce their food spending in response to SFSP meals served in their ZIP code. Additionally, I provide evidence that the program is taken up by middle-income households by using their significantly lower food spending in response to increased SFSP meal provision as a proxy for take-up. I find no evidence that low- or high-income households have similar food spending responses.

There could be multiple causes of the lack of a food spending response for low-income households. They may reduce consumption of food-away-from-home in response to SFSP meal provision, or they may reduce their usage of charitable meal provision through food pantries or other organizations. The results are evidence that low-income households do not take up the program only if we assume that low-income households respond to summer meals as they do school-year meals, and rely on existing literature (such as Handbury and Moshary (2021)) that shows that lower-income households reduce their food spending when they take up school meals. However, my empirical strategy provides compelling evidence that SFSP is taken up by middle-income households, as they consistently have large reductions in food spending in response to increased SFSP meal provision.

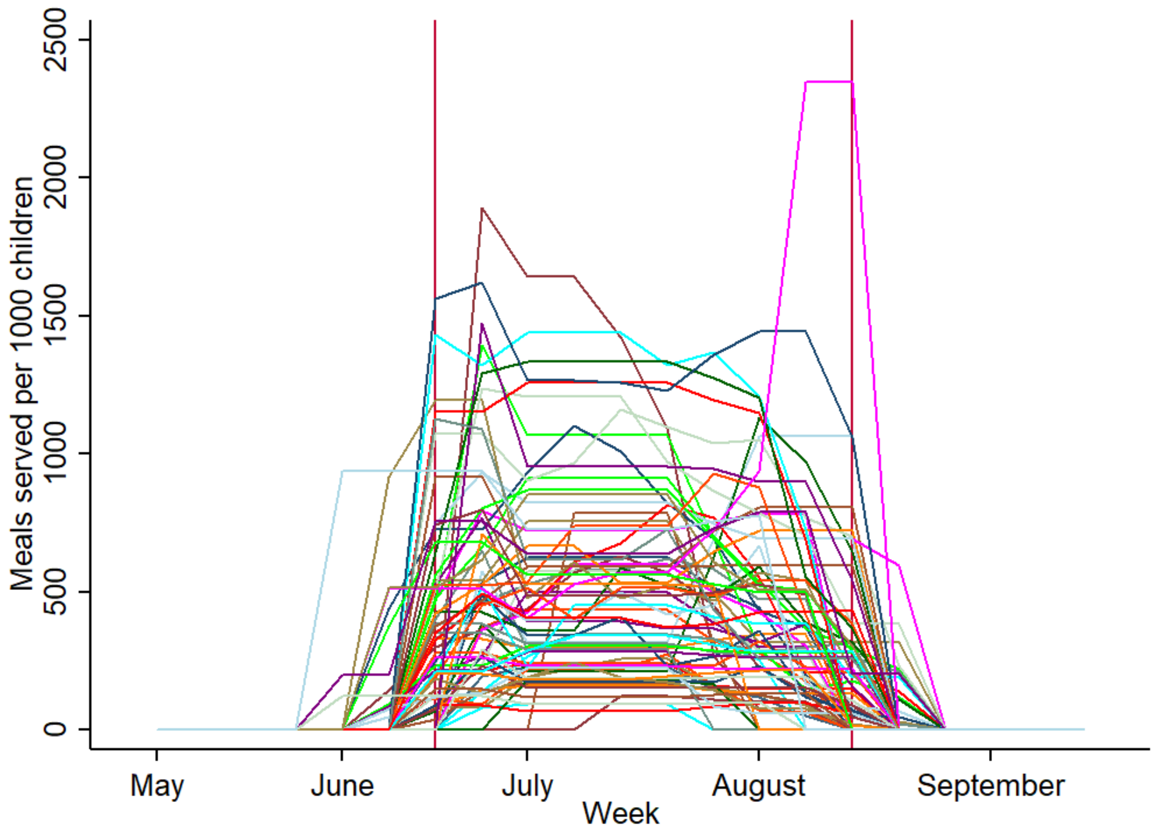
An important consideration is that means-testing is expensive, and difficult in this context if children are intended to visit SFSP sites without adult assistance. Children do not have access to or an understanding of the forms necessary to prove their low-income status. Given this constraint, sites open to all may be the best way to feed low-income children, even if many middle-income children attend the program as well.

Individually means-tested mechanisms for summer meal provision would instead ensure that all funds are directed toward households with low-income children. The Summer Electronic Benefits Transfer was a federal pilot program that provided households with EBT cards to use on food spending (similar to those used in SNAP) during the summer months. Because the COVID-19 pandemic prevented children from accessing school meals both during the academic year and during the summer, the program was expanded into the Pandemic Electronic Benefits Transfer. 43 states provided summertime EBT cards in summer 2021, with a benefit of roughly \$375 per child. These programs may more efficiently provide dollars to low-income households, but they do not ensure that children are the recipients of purchased food, and they do not provide any academic or social benefits. President Biden's American Families Plan proposes to extend Pandemic EBT into a permanent summertime program, and future work may contrast how this program affects household food expenditures compared to the SFSP.

1.8 Acknowledgments

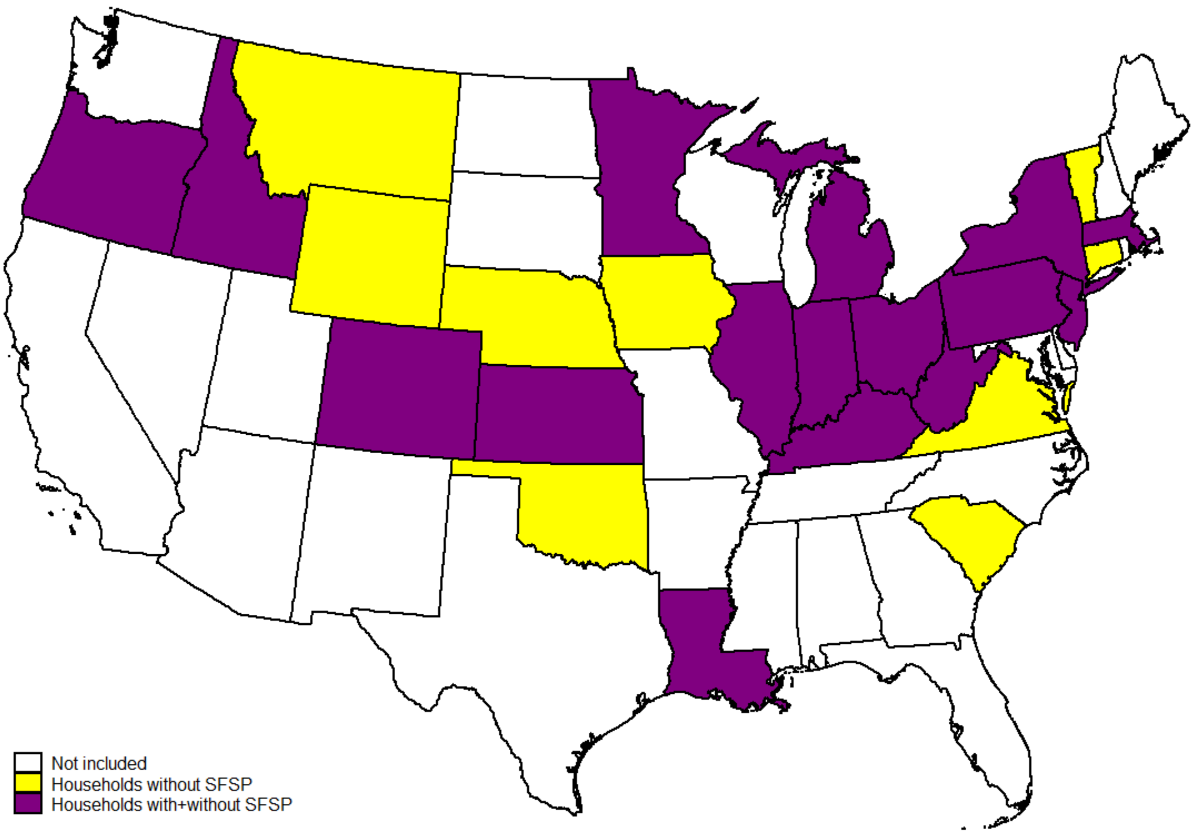
Chapter 1, "Who Takes Up a Free Lunch? Food Spending and the Summer Food Service Program" is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the chapter. Researcher's own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

1.9 Figures and Tables



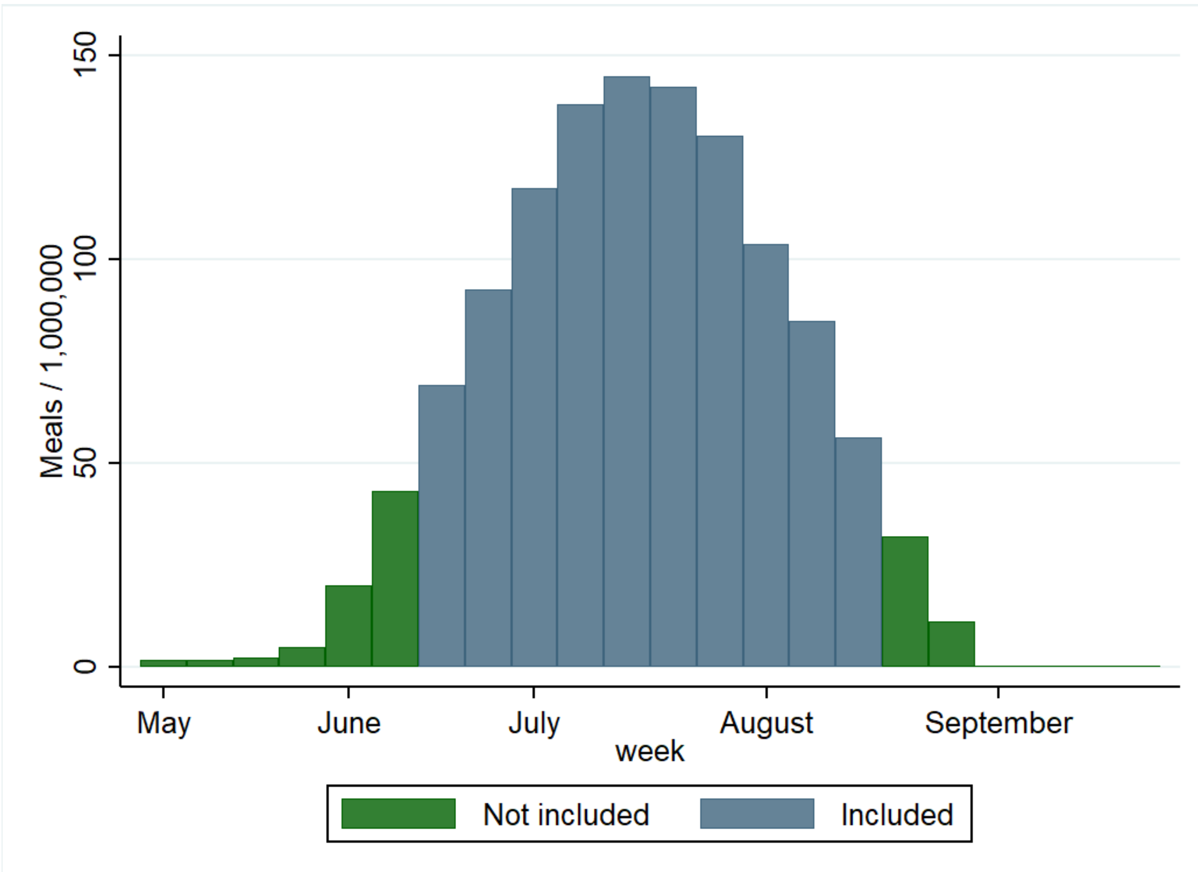
This figure plots each ZIP code's SFSP weekly meal provision per 1,000 children across the summer for the state of Oregon in 2018. Each ZIP code with some SFSP meal provision and at least one Nielsen household living in the ZIP code is represented by one line on the figure. Only the variation within the ten weeks between the vertical red lines is used in the main analysis. This figure demonstrates that ZIP codes have differential SFSP meal availability in an average state-year, with Oregon's 2018 meal provision a typical example. This variation provides further support that SFSP meal provision is plausibly exogenously assigned, rather than correlated with any confounding programs or summer trends that could affect food spending.

Figure 1.1. Oregon's 2018 SFSP Meal Provision, by ZIP Code



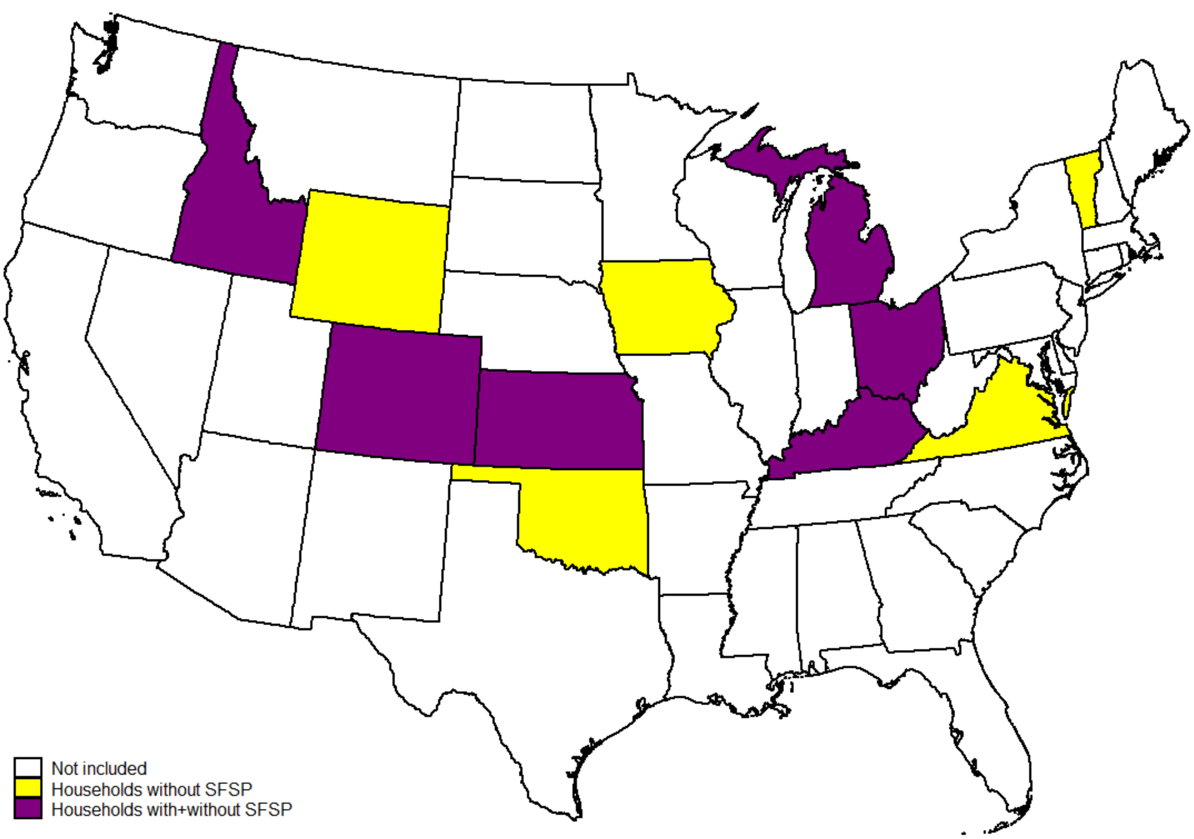
The states in purple provided meals-served data and have no or small SSO programs. The states in yellow did not provide meals-served data, but have no or small SSO programs. The states in white did not provide meals-served data and have substantial SSO programs. The sample in the main regressions includes all household-years with children in the purple states, and all household-years with children and no SFSP access in the yellow states.

Figure 1.2. States in Sample



This figure shows how the number of meals served varies across the summer. These data are collapsed across years to include all treated households for the 22 weeks of summer between early May and late September. The main sample includes the ten weeks of summer with the highest number of SFSP meals served, shaded blue. For robustness, I also run the main regressions on a twelve-week sample that includes one additional June and August week on either side of the shaded blue region.

Figure 1.3. Distribution of Meals Served Across the Summer



This figure shows the subsample of states in Figure 1.2 for which I have data on school calendars. The sample in the summer-break-only robustness check regression includes all household-years with children in the purple states, and all household-years with children and no SFSP access in the yellow states.

Figure 1.4. States in Sample with School Calendars

Table 1.1. Summary Statistics, Nielsen Sample

	Children and SFSP	Children and no SFSP
Household income	64637.30	73534.34
Below 200% FPL	0.35	0.24
Between 200% FPL & 400% FPL	0.52	0.59
Above 400% FPL	0.13	0.17
Household size	4.06	4.06
Married	0.81	0.87
White	0.73	0.82
Black	0.12	0.05
Asian/Other	0.06	0.07
Hispanic	0.09	0.06
Head: 50+ Years Old	0.32	0.32
Obs. (Household-year)	14530	14588

The sample is drawn from years 2015-2019 of the Nielsen Consumer Panel. Summary statistics are given at the household-year level. Household income is given as a two-year-lag in sixteen bins. I assign households the median income in their income bin. I define households as Hispanic if the household head reports Hispanic ethnicity; otherwise, I define them as white, Black, or Asian/Other based on their household head's self-identification. Column one includes all household-years who have at least one child aged eighteen or younger, have a SFSP site open for at least one week in their ZIP code, and live in a state that provided data on SFSP meals served. Column two includes all household-years with children but no SFSP site open in their ZIP code, and who live in a state with a small or nonexistent SSO program.

Table 1.2. Exposure to SFSP Meals, Treated Nielsen Households

	(1) Low-income	(2) Middle-income	(3) High-income
Any meals served	0.81	0.80	0.81
Any meals served at religious org.	0.11	0.11	0.10
Any meals served at schools	0.29	0.28	0.27
Any meals served at community centers	0.58	0.56	0.58
Any meals served at low-access site	0.69	0.70	0.68
Any meals served at accessible site	0.50	0.45	0.47
Any meals served at rel. higher-income site	0.48	0.45	0.45
Any meals served at rel. lower-income site	0.70	0.69	0.70
# Meals served, p.c.	265.04	216.61	223.24
# Religious meals served, p.c.	12.20	9.99	9.62
# School meals served, p.c.	71.78	69.92	65.86
# Community meals served, p.c.	181.06	136.70	147.76
# Low-access meals served, p.c.	172.64	150.54	140.09
# Accesible meals served, p.c.	92.40	66.12	83.15
# Rel. higher-income meals served, p.c.	89.06	68.64	70.58
# Rel. lower-income meals served, p.c.	168.19	141.23	145.98
Obs. (Household-week)	50350	75480	19470

The sample is drawn from public records requests from state administering bodies for the number of meals served by sponsor. These data were available only from the states in the first group of Table 1.A.1, which provided data on meals served by sponsor-month or sponsor-year. These data are merged with the USDA site information to include site open weeks, and to create an approximate measure of meals served by week. Data are at the household-week level, and include all treated household-years with children and at least one week of SFSP meal provision. The number of meals is provided per 1,000 children residing in the ZIP code (using 5-year estimates from the American Community Survey). This table separates characteristics by type of site and by income group to show that the number of meals served by different site types does not vary substantially across income groups. Households are assigned to income categories as described in Table 1.1. Sites are classified as religious- , school-, or community-run using key words in their names, detailed in Table 1.A.1. Sites are classified as low-access if their census tract had more than 500 people, or 33% of the population, living more than half a mile from a supermarket or grocery store in urban tracts, or ten miles in rural tracts, according to the 2019 Food Access Research Atlas. Sites were classified as accessible if they did not meet the criteria to be considered low-access. Sites were classified as relatively higher-income (lower-income) if their census tract had a higher (lower) median income than their ZIP code.

Table 1.3. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases

	(1) Food
Meals p.c.	-0.0077***
	(0.0027)
Household x Year FE	Yes
Week x Year FE	Yes
Food spending mean	66.01
Obs.	290190

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This regression estimates Equation 1.1. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.4. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases

	(1) Food
Meals p.c.	-0.0084*
	(0.0047)
Meals, 1 Week Lag	-0.0006
	(0.0045)
Meals, 1 Week Lead	-0.0026
	(0.0047)
Meals, 2 Week Lag	0.0052
	(0.0044)
Meals, 2 Week Lead	-0.0019
	(0.0046)
Meals, 3 Week Lag	-0.0027
	(0.0046)
Meals, 3 Week Lead	0.0055
	(0.0047)
Meals, 4 Week Lag	-0.0067
	(0.0046)
Meals, 4 Week Lead	0.0022
	(0.0049)
Meals, 5 Week Lag	-0.0026
	(0.0047)
Meals, 5 Week Lead	-0.0032
	(0.0049)
Meals, 6 Week Lag	0.0047
	(0.0043)
Meals, 6 Week Lead	-0.0032
	(0.0044)
Household x Year FE	Yes
Week x Year FE	Yes
Food spending mean	65.97
Obs.	284324

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This regression estimates Equation 1.2. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.5. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, Separately by Income Group

	(1) Food
Meals p.c., Less than 200% FPL	0.0016
	(0.0042)
Meals p.c., Between 200% and 400% FPL	-0.0167***
	(0.0037)
Meals p.c., Above 400% FPL	0.0014
	(0.0070)
Household x Year FE	Yes
Week x Year FE	Yes
Food spending mean	66.01
Obs.	290190

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This regression estimates Equation 1.3. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. All variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.6. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, Separately by Food Type

	(1) Dairy	(2) Deli / packaged meat	(3) Produce	(4) Dry grocery	(5) Frozen foods
Meals p.c.	-0.0060***	-0.0049**	-0.0003	-0.0063**	-0.0054**
	(0.0020)	(0.0020)	(0.0018)	(0.0024)	(0.0022)
Household x Year FE	Yes	Yes	Yes	Yes	Yes
Week x Year FE	Yes	Yes	Yes	Yes	Yes
Spending mean ($)	8.42	10.04	5.89	30.28	11.38
Obs.	290190	290190	290190	290190	290190

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This regression estimates Equation 1.1. Each column includes a different component of food spending and the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.7. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, Separately by Food Type and Income Group

	(1)	(2)	(3)	(4)	(5)
	Dairy	Deli / packaged meat	Produce	Dry grocery	Frozen foods
Meals p.c., Less than 200% FPL	-0.0019	0.0002	0.0030	0.0025	0.0011
	(0.0030)	(0.0031)	(0.0028)	(0.0038)	(0.0034)
Meals p.c., Between 200% and 400% FPL	-0.0096^{***}	-0.0090^{***}	-0.0022	-0.0145^{***}	-0.0116^{***}
	(0.0027)	(0.0028)	(0.0024)	(0.0034)	(0.0030)
Meals p.c., Above 400% FPL	-0.0030	-0.0028	-0.0017	0.0020	0.0007
	(0.0051)	(0.0051)	(0.0043)	(0.0062)	(0.0056)
Household x Year FE	Yes	Yes	Yes	Yes	Yes
Week x Year FE	Yes	Yes	Yes	Yes	Yes
Spending mean ($)	8.42	10.04	5.89	30.28	11.38
Obs.	290190	290190	290190	290190	290190

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This regression estimates Equation 1.3. Each column includes a different component of food spending and the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.8. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, Separately by Spending Type

	(1) SFSP	(2) Breakfast	(3) Lunch/Dinner	(4) Perishable	(5) Nonperishables	(6) Tobacco/Alcohol	(7) Nonfood
Meals p.c.	-0.0071***	-0.0055**	-0.0072***	-0.0064***	-0.0073***	0.0024**	-0.0031
	(0.0025)	(0.0022)	(0.0025)	(0.0023)	(0.0025)	(0.0012)	(0.0024)
Household x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spending mean ($)	34.99	14.68	30.72	24.22	37.52	2.58	33.72
Obs.	290190	290190	290190	290190	290190	290190	290190

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This regression estimates Equation 1.1. Each column includes a different component of grocery spending and the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.9. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, Separately by Spending Type and Income Group

	(1) SFSP	(2) Breakfast	(3) Lunch/Dinner	(4) Perishable	(5) Nonperishables	(6) Tobacco/Alcohol	(7) Nonfood
Meals p.c., Less than 200% FPL	0.0006	-0.0005	-0.0015	-0.0012	0.0021	0.0028	0.0066*
	(0.0040)	(0.0034)	(0.0039)	(0.0036)	(0.0040)	(0.0019)	(0.0039)
Meals p.c., Between 200% and 400% FPL	-0.0144***	-0.0102***	-0.0132***	-0.0117***	-0.0159***	0.0026*	-0.0108***
	(0.0035)	(0.0030)	(0.0034)	(0.0032)	(0.0035)	(0.0016)	(0.0034)
Meals p.c., Above 400% FPL	0.0003	-0.0008	0.0007	-0.0002	0.0004	0.0005	0.0006
	(0.0066)	(0.0054)	(0.0064)	(0.0059)	(0.0065)	(0.0035)	(0.0066)
Household x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spending mean ($)	34.99	14.68	30.72	24.22	37.52	2.58	33.72
Obs.	290190	290190	290190	290190	290190	290190	290190

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This regression estimates Equation 1.3. Each column includes a different component of grocery spending and the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.10. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, Separately by Sponsoring Organization Type and Income Group

	(1) Food	(2) Food
Religious Meals p.c.	0.0007	
	(0.0080)	
School Meals p.c.	-0.0079**	
	(0.0040)	
Community Meals p.c.	-0.0063**	
	(0.0032)	
Religious Meals p.c., Less than 200% FPL		0.0161
		(0.0134)
Religious Meals p.c., Between 200% and 400% FPL		-0.0100
		(0.0115)
Religious Meals p.c., Above 400% FPL		0.0034
		(0.0213)
School Meals p.c., Less than 200% FPL		-0.0025
		(0.0064)
School Meals p.c., Between 200% and 400% FPL		-0.0156***
		(0.0059)
School Meals p.c., Above 400% FPL		0.0067
		(0.0116)
Community Meals p.c., Less than 200% FPL		0.0003
		(0.0050)
Community Meals p.c., Between 200% and 400% FPL		-0.0127***
		(0.0044)
Community Meals p.c., Above 400% FPL		-0.0002
		(0.0086)
Household x Year FE	Yes	Yes
Week x Year FE	Yes	Yes
Food spending mean	66.01	66.01
Obs.	290190	290190

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3, allowing for heterogeneous effects by site sponsoring organization. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. All variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.11. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, Separately by Site Grocery Accessibility and Income Group

	(1) Food	(2) Food
Low Access (>0.5 mi) Meals p.c.	-0.0086***	
	(0.0031)	
Accessible (<0.5 mi) Meals p.c.	0.0009	
	(0.0041)	
Low Access (>0.5 mi) Meals p.c., Less than 200% FPL		0.0020
		(0.0050)
Accessible (<0.5 mi) Meals p.c., Less than 200% FPL		0.0042
		(0.0064)
Low Access (>0.5 mi) Meals p.c., Between 200% and 400% FPL		-0.0159***
		(0.0043)
Accessible (<0.5 mi) Meals p.c., Between 200% and 400% FPL		-0.0065
		(0.0061)
Low Access (>0.5 mi) Meals p.c., Above 400% FPL		-0.0071
		(0.0081)
Accessible (<0.5 mi) Meals p.c., Above 400% FPL		0.0152
		(0.0117)
Household x Year FE	Yes	Yes
Week x Year FE	Yes	Yes
Food spending mean	66.01	66.01
Obs.	290156	290156

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3, allowing for heterogeneous effects by site grocery accessibility. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. All variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.12. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, Separately by Site's Tract's Relative Income and Income Group

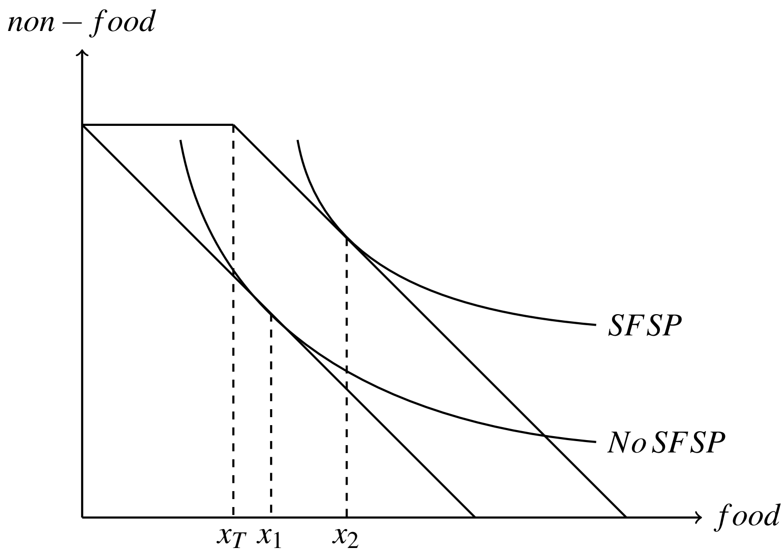
	(1) Food	(2) Food
Tract Income Above ZIP Avg Meals p.c.	0.0035	
	(0.0039)	
Tract Income Below ZIP Avg Meals p.c.	-0.0107***	
	(0.0032)	
TI Above ZIP Avg Meals p.c., Less than 200% FPL		0.0000
		(0.0064)
TI Below ZIP Avg Meals p.c., Less than 200% FPL		0.0022
		(0.0055)
TI Above ZIP Avg Meals p.c., Between 200% and 400% FPL		0.0010
		(0.0057)
TI Below ZIP Avg Meals p.c., Between 200% and 400% FPL		-0.0195***
		(0.0045)
TI Above ZIP Avg Meals p.c., Above 400% FPL		0.0181*
		(0.0106)
TI Below ZIP Avg Meals p.c., Above 400% FPL		-0.0096
		(0.0082)
Household x Year FE	Yes	Yes
Week x Year FE	Yes	Yes
Food spending mean	66.01	66.01
Obs.	290156	290156

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3, allowing for heterogeneous effects by site relative income. TI abbreviates tract income. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. All variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

1.A Appendix Figures and Tables



The budget constraint for households with children shifts out in weeks with SFSP access, allowing them to consume the equivalent of x_T spending on food without any reductions in non-food spending. There will be a reduction in food spending if $x_2 - x_T < x_1$.

Figure 1.A.1. Theoretical predictions

Breakfast Meal Pattern

Select All Three Components for a Reimbursable Meal

1 milk	1 cup	fluid milk
1 fruit/vegetable	1/2 cup	juice,1 and/or vegetable
1 grains/bread2	1 slice 1 serving 3/4 cup 1/2 cup 1/2 cup	bread or cornbread or biscuit or roll or muffin or cold dry cereal or hot cooked cereal or pasta or noodles or grains

These are the reimbursable foods that breakfast-serving SFSP sites offer to all children who attend. Source: SFSP Meal Patterns, 2019.

Figure 1.A.2. SFSP Breakfast Meal Pattern

Lunch or Supper Meal Pattern

Select All Four Components for a Reimbursable Meal

1 milk	1 cup	fluid milk
2 fruits/vegetables	3/4 cup	juice,1 fruit and/or vegetable
1 grains/bread2	1 slice 1 serving 1/2 cup 1/2 cup	bread or cornbread or biscuit or roll or muffin or hot cooked cereal or pasta or noodles or grains
1 meat/meat alternate	2 oz. 2 oz. 2 oz. 1 large 1/2 cup 4 Tbsp. 1 oz. 8 oz.	lean meat or poultry or fish3 or alternate protein product or cheese or egg or cooked dry beans or peas or peanut or other nut or seed butter or nuts and/or seeds4 or yogurt5

These are the reimbursable foods that lunch- or supper-serving SFSP sites offer to all children who attend. Source: SFSP Meal Patterns, 2019.

Figure 1.A.3. SFSP Lunch/Supper Meal Pattern

Snack (Supplement) Meal Pattern

Select Two of the Four Components for a Reimbursable Snack

1 milk	1 cup	>fluid milk
[1]fruit/vegetable	3/4 cup	juice,[1] fruit and/or vegetable
1 grains/bread[2]	1 slice 1 serving 3/4 cup 1/2 cup 1/2 cup	bread or cornbread or biscuit or roll or muffin or cold dry cereal or hot cooked cereal or pasta or noodles or grains
1 meat/meat alternate	1 oz. 1 oz. 1 oz. 1/2 large 1/4 cup 2 Tbsp. 1 oz. 4 oz.	lean meat or poultry or fish[3] or alternate protein product or cheese or egg or cooked dry beans or peas or peanut or other nut or seed butter or nuts and/or seeds or yogurt 4

These are the reimbursable foods that snack-serving SFSP sites offer to all children who attend. Source: SFSP Meal Patterns, 2019.

Figure 1.A.4. SFSP Snack Meal Pattern

Table 1.A.1. Seamless Summer Option utilization

State	SSO : SFSP	Households treated by SFSP in sample?	Untreated households in sample?
Colorado	no SSO	yes	yes
Idaho	no SSO	yes	yes
Illinois	1:10	yes	yes
Indiana	1:60	yes	yes
Kansas	1:500	yes	yes
Kentucky	1:1000	yes	yes
Louisiana	1:25	yes	yes
Massachusetts	no SSO	yes	yes
Michigan	no SSO	yes	yes
Minnesota	1:50	yes	yes
New Jersey	1:5	yes	yes
New York	no SSO	yes	yes
Pennsylvania	1:200	yes	yes
Ohio	1:2	yes	yes
Oregon	1:75	yes	yes
West Virginia	1:6	yes	yes
Connecticut	1:5	no	yes
Iowa	no SSO	no	yes
Montana	no SSO	no	yes
Nebraska	no SSO	no	yes
Oklahoma	1:12	no	yes
South Carolina	1:40	no	yes
Vermont	1:10	no	yes
Virginia	no SSO	no	yes
Wyoming	no SSO	no	yes
Arizona	3:1	no	no
California	2:1	no	no
Georgia	10:1	no	no
Missouri	2:3	no	no
North Carolina	4:1	no	no
Texas	3:4	no	no
Utah	2:1	no	no
Delaware	n/a	no	no
Florida	n/a	no	no
New Hampshire	n/a	no	no
New Mexico	n/a	no	no
Washington	n/a	no	no
Wisconsin	n/a	no	no
Alabama	n/a	no	no
Arkansas	n/a	no	no
Maine	n/a	no	no
Maryland	n/a	no	no
Mississippi	n/a	no	no
Nevada	n/a	no	no
North Dakota	n/a	no	no
Rhode Island	n/a	no	no
South Dakota	n/a	no	no
Tennessee	n/a	no	no

All households with children are included from states whose administering bodies provided meals served records for years 2015-2019, listed in the first section of the table. These states have small SSO programs that likely do not affect the untreated households. The second section is states without meals served data but with small SSO programs. Untreated households with children from this set of states are included in the sample, but treated households are not included. The third section includes states with sizable SSO programs; no households from these states are included, since they may be unobservably treated by the SSO. The fourth section includes states who were unable or unwilling to provide me with meals served data or information on the size of their SSO programs; again, no households from these states are included because of unobservable summer meal provision. Kansas, Indiana, and Oregon did not provide data for 2019, so households with access to the SFSP in those states in 2019 are not included in the data.

Table 1.A.2. Key words used to classify sponsoring organization type

Religious	Church, Temple, Christ, Gospel, Baptist, Cathedral, Iglesia, Deliverance, Resurrection, Minister, Ministry, Bible, Bethel, Bethlehem, Nazareth, Chapel, Apost, Fellowship, Commandment, Holiness, Holy, Methodist, Divina, Mission, Lutheran, UMC, God, Covenant, Pentecostal, Catholic, Zion, Jerusalem, Revival, Congregation, St John, St Paul, St Andrews, Worship, Prayer, Faith, Tabernacle, Shalom, Seventh Day, Ramadan, Mosque, Parish
School	School, Elem, Middle, High, Junior, Senior, Secondary, Charter, ES, MS, HS, ISD, PS, GCS, Charter, Intermediate
Community	all other programs (with exceptions)

The SFSP site data from the USDA FNS contain information on string name for each sponsoring organization. These organizations are not subdivided into categories. I categorize organizations into three broad categories using key words in their string names. I first categorized sponsoring organizations into schools and religious organizations using the above words, which were all relatively high-frequency in my analysis of the strings. I then parsed through those assigned to schools or religious organizations and ensured that the assignment was correct, and fixed those that were not (e.g. “Junior Park Rangers” is a community organization, rather than a school). I assigned all other organizations as community-run, read over the list, and reassigned any that sounded like religious organizations or schools to the appropriate categories.

Table 1.A.3. Elasticities for the Effect of SFSP Meal Provision on the Likelihood of Zero Food Spending

	(1) Zero food spending	(2) Zero food spending
Meals p.c.	0.0016***	
	(0.0005)	
Meals p.c., Less than 200% FPL		0.0001
		(0.0009)
Meals p.c., Between 200% and 400% FPL		0.0031***
		(0.0007)
Meals p.c., Above 400% FPL		-0.0005
		(0.0014)
Household x Year FE	Yes	Yes
Week x Year FE	Yes	Yes
Zero food spending mean	0.25	0.25
Obs.	290190	290190

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3, substituting a binary for zero food spending as the dependent variable. The number of meals served is per 1000 children in the ZIP code. The independent variable is transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.A.4. Effect of SFSP Meal Provision on the Number of Grocery Trips

	(1) Number of grocery trips	(2) Number of grocery trips
Meals p.c.	-0.0062**	
	(0.0025)	
Meals p.c., Less than 200% FPL		0.0024
		(0.0042)
Meals p.c., Between 200% and 400% FPL		-0.0127***
		(0.0034)
Meals p.c., Above 400% FPL		-0.0048
		(0.0064)
Household x Year FE	Yes	Yes
Week x Year FE	Yes	Yes
Number trips mean	2.49	2.49
Obs.	290190	290190

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3, substituting the number of grocery trips as the dependent variable. The number of meals served is per 1000 children in the ZIP code. The independent variable is transformed using the inverse hyperbolic sine, so each coefficient gives the change in the number of trips in response to a 1% increase in the number of meals served per 1,000 children in the household's ZIP-week.

Table 1.A.5. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, 12 weeks

	(1) Food	(2) Food
Meals p.c.	-0.0063***	
	(0.0022)	
Meals p.c., Less than 200% FPL		0.0002
		(0.0033)
Meals p.c., Between 200% and 400% FPL		-0.0130***
		(0.0030)
Meals p.c., Above 400% FPL		0.0014
		(0.0055)
Household x Year FE	Yes	Yes
Week x Year FE	Yes	Yes
Food spending mean	66.17	66.17
Obs.	348228	348228

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3 on a 12-week summer sample. The weeks of summer included is described in Figure 1.3. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.A.6. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, No-School Weeks

	(1) Food	(2) Food
Meals p.c.	-0.0069	
	(0.0093)	
Meals p.c., Less than 200% FPL		-0.0031
		(0.0133)
Meals p.c., Between 200% and 400% FPL		-0.0155
		(0.0136)
Meals p.c., Above 400% FPL		0.0159
		(0.0233)
Household x Year FE	Yes	Yes
Week x Year FE	Yes	Yes
Spending mean ($)	66.23	66.23
Obs.	39749	39749

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3 on a sample that includes only school closure weeks in ZIP codes with data on their school districts' calendars. The included states are shown in Figure 1.4. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.A.7. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, Seasonality by County

	(1) Food	(2) Food
Meals p.c.	-0.0071**	
	(0.0028)	
Meals p.c., Less than 200% FPL		0.0030
		(0.0043)
Meals p.c., Between 200% and 400% FPL		-0.0161***
		(0.0038)
Meals p.c., Above 400% FPL		0.0005
		(0.0072)
Household x Year FE	Yes	Yes
County x Week x Year FE	Yes	Yes
Food spending mean	66.01	66.01
Obs.	289800	289800

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3 using county-week-year fixed effects. A small number of observations are dropped from the main sample because the household's county is missing (though ZIP code is non-missing). Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. All variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.A.8. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, Seasonality by School District

	(1) Food	(2) Food
Meals p.c.	-0.0007	
	(0.0069)	
Meals p.c., Less than 200% FPL		0.0093
		(0.0085)
Meals p.c., Between 200% and 400% FPL		-0.0096
		(0.0077)
Meals p.c., Above 400% FPL		0.0091
		(0.0109)
Household x Year FE	Yes	Yes
School District x Week x Year FE	Yes	Yes
Food spending mean	66.08	66.08
Obs.	230570	230570

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3 using school district-week-year fixed effects. Not all ZIP codes have an assigned school district in the National Center for Education Statistics data, limiting the sample size. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. All variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.A.9. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases for the Placebo Group of Households Without Children

	(1) Food	(2) Food
Meals p.c.	-0.0019	
	(0.0012)	
Meals p.c., Less than 200% FPL		-0.0021
		(0.0024)
Meals p.c., Between 200% and 400% FPL		-0.0002
		(0.0020)
Meals p.c., Above 400% FPL		-0.0033*
		(0.0020)
Household x Year FE	Yes	Yes
Week x Year FE	Yes	Yes
Food spending mean	53.05	53.05
Obs.	2085730	2085730

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3 on a sample that includes only households without children, assigned the number of meals served to children in their ZIP code. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.A.10. Effect of SFSP Site Availability on Grocery Purchases

	(1) Food	(2) Food
Site open	-0.0549***	
	(0.0161)	
Site open, Less than 200% FPL		0.0031
		(0.0258)
Site open, Between 200% and 400% FPL		-0.1041***
		(0.0223)
Site open, Above 400% FPL		-0.0128
		(0.0416)
Household x Year FE	Yes	Yes
Week x Year FE	Yes	Yes
Food spending mean	66.02	66.02
Obs.	291180	291180

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3, substituting an indicator for whether at least one SFSP site is open in the household's ZIP code-week for the number of meals served. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. The dependent variable is transformed using the inverse hyperbolic sine, so coefficients can be interpreted as the percent change in food spending when a ZIP code-week has at least one SFSP site open.

Table 1.A.11. Effect of SFSP Meal Provision on Grocery Purchases, Biweekly Aggregates

	(1) Food	(2) Food	(3) Food	(4) Food
Meals p.c.	-0.0098***	-0.0073		
	(0.0031)	(0.0082)		
Meals p.c., Less than 200% FPL			-0.0059	-0.0094
			(0.0303)	(0.0753)
Meals p.c., Between 200% and 400% FPL			-0.0231***	-0.0187
			(0.0060)	(0.0157)
Meals p.c., Above 400% FPL			0.0085	0.0290
			(0.0093)	(0.0261)
Household x Year FE	Yes	Yes	Yes	Yes
Two Week x Year FE	Yes	Yes	Yes	Yes
Weeks	10	6	10	6
Food spending mean	65.95	65.50	65.95	65.50
Obs.	145095	87057	145095	87057

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3, using biweekly aggregates of food spending and the number of meals served. The ten (six) week sample then includes five (three) observations per household-year, so the sample size is proportionally smaller to that in the main results using weekly spending measures. Food spending is measured in dollars and the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.A.12. Effect of SFSP Meal Provision on Grocery Purchases, Different Scaling Parameters

	k=10		k=100		k=1000	
	Food	Food	Food	Food	Food	Food
Meals p.c.	-0.0084***		-0.0087***		-0.0089***	
	(0.0028)		(0.0028)		(0.0029)	
Meals p.c., Less than 200% FPL		0.0007		0.0002		-0.0001
		(0.0044)		(0.0046)		(0.0047)
Meals p.c., Between 200% and 400% FPL		-0.0173***		-0.0175***		-0.0175***
		(0.0038)		(0.0039)		(0.0039)
Meals p.c., Above 400% FPL		0.0015		0.0016		0.0017
		(0.0073)		(0.0075)		(0.0076)
Household x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Week x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Food spending mean ($)	660.06	660.06	6600.64	6600.64	66006.43	66006.43
Obs.	290190	290190	290190	290190	290190	290190

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate Equation 1.1 and Equation 1.3. Food spending is originally measured in dollars and the number of meals served is per 1000 children in the ZIP code, but both measures are here multiplied by k=10 in Columns (1) and (2), by k=100 in Columns (3) and (4), and by k=1000 in Columns (5) and (6). Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Table 1.A.13. Elasticities for the Effect of SFSP Meal Provision on Grocery Purchases, First Differenced

	(1) Food, fd	(2) Food, fd
Meals p.c., fd	-0.0046	
	(0.0030)	
Meals p.c., Less than 200% FPL, fd		-0.0031
		(0.0054)
Meals p.c., Between 200% and 400% FPL, fd		-0.0050
		(0.0040)
Meals p.c., Above 400% FPL, fd		-0.0059
		(0.0076)
Household FE	Yes	Yes
Week x Year FE	Yes	Yes
Food spending mean	65.58	65.58
Obs.	166030	166030

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These regressions estimate a first-differenced version of Equation 1.1 and Equation 1.3, with variation coming within household-weeks-of-the-summer across years. The first difference of food spending is measured in dollars and the first difference of the number of meals served is per 1000 children in the ZIP code. Both the independent and dependent variables are transformed using the inverse hyperbolic sine, so coefficients can be interpreted as elasticities.

Chapter 2

The Effect of Food Pantries on Household Grocery Expenditures and Grocery Retailer Sales

2.1 Introduction

Food banks and pantries are a vital source of nutrition assistance, serving an estimated 40 million individuals in 2019, and 60 million individuals in 2020 (with the large increase driven by the COVID-19 pandemic) (*Charitable Food Assistance Participation* 2022). More individuals participate in charitable food assistance programs than in the largest federal nutrition program, the Supplemental Nutrition Assistance Program (SNAP), with approximately 36 million households taking up SNAP in 2019 and 40 million taking up in 2020 (*Supplemental Nutrition Assistance Program Participation and Costs* 2022). However, food pantries are not run by the government and are highly decentralized, with little publicly available data on where pantries are located, when and why they first opened, what types of food they provide, or any demographics of the households they serve. The lack of a large, nationwide data set has limited the ability for researchers to study the effects that food pantries have on their communities.

Both the size and direction of these effects are unknown. Households that visit food pantries may shift their consumption away from food toward other needs, or they may use pantries to supplement their food spending if they can only spend the amount they receive from non-fungible government transfers. The effects on the amount of food spending are thus ambiguous, but the composition of foods purchased should change even if the amount spent does not. Households can shift their consumption away from the types of foods they receive at the food pantry toward other or complementary foods. It is also unclear whether food pantries are competition for retailers, or if they attract more households to the area and increase the number of shoppers. Retailers also receive tax benefits for donating food to pantries which may affect prices.

In this paper, I provide the first nation-wide, non-survey-based evidence on how food pantries affect the composition of household food spending and grocery retailer sales. I use a publicly available data set from the Internal Revenue Service, the Exempt Organization Business Master File, which provides the month and year of 501(c)(3) approval for all currently-operating 501(c)(3) organizations. I create a panel of food pantries that opened between 2006-2017 and remained open for at least five years. I connect these food pantries to a large panel of household grocery spending data from the Kilts-Nielsen Consumer Panel, and to a panel of grocery sales that includes 50% of the point-of-sale transactions made at grocery retailers in the United States from the Kilts-Nielsen Retail Scanner Data. I provide evidence that where food pantries open up is not as good as random (i.e. food pantries open up in ZIP codes with pre-trends), but when they open up (conditional on having an opening) appears to be uncorrelated with trends in ZIP code characteristics. I use this finding to motivate my empirical strategy, in which I create a "stacked" data set using only ZIP codes that have a food pantry open up during my sample, with later-treated ZIP codes serving as controls for early-treated ZIP codes.

I find that households reduce their food spending by 1.8%, but in the aggregate the amount of store sales does not change. I break products into broad categories and investigate how the composition of household purchases and store sales changes. I find statistically significant effects for household spending reductions on prepared foods, snack foods, and processed vegetables (which includes canned vegetables), implying that these foods are crowded out by the types of foods that are available at food pantries. There are not significant changes to aggregate sales in these products, but instead there are significant increases in spending on the types of foods that may be more difficult for food pantries to store, such as dairy and produce. These results provide evidence on the types of foods available at food pantries, as well as the types of foods that households prefer to shift their spending toward when they receive food from food pantries.

The literature on charitable food assistance is not as large as that on federal food assistance programs, particularly within the economics field. Mook, Murdock, and Gundersen (2020) provide an overview of food banking with much discussion focused on the United States, summarizing the literature's main findings and providing institutional background. Prendergast (2017) details how food banks acquire food and how much they are willing to pay for different product categories in organized auctions. Gundersen, Dewey, Hake, Engelhard, and Crumbaugh (2017) and Gundersen, Engelhard, and Hake (2017) make use of a large survey of food pantries in the United States and find that charitable food assistance is more available in lower-population counties, and that food pantry visitors face unique financial and psychological barriers. To the best of my knowledge, there are no papers studying food pantries' effects on nearby grocery stores. I complement this literature by using the plausibly exogeneous timing of a pantry opening (conditional on the area experiencing an opening) to causally identify a pantry's effects on household food spending and retailer sales.

2.2 Institutional Background

The first food bank was established in 1967, when a soup kitchen volunteer, John van Hengel, considered the value of "banking" surplus food and distributing groceries rather than relying on ready-made meals to serve those in need. His St. Mary's Food Bank in Phoenix, Arizona grew to become Feeding America, a network of 200 food banks and 60,000 food pantries and the largest hunger relief organization in the United States (*Our History* 2022). There is a hierarchy and distinction between food banks and food pantries. Food banks typically serve as the centralized clearing house for food pantries in the geographic region, collecting and storing large donations of food from Feeding America, food manufacturers and retailers, and the U.S. Department of Agriculture (*CSFP Fact Sheet* 2019). Food banks then distribute this food to food pantries who then connect directly with individuals. Food pantries can also receive food directly from these primary sources, as well as from private individuals (Mook, Murdock, and Gundersen 2020). Some food pantries require recipients to provide proof of income or residency within the pantry's area of service, but administrative and time costs are lower than when applying for SNAP. A majority of food pantry visitors are engaged for less than a year, but those that visit for several years (either episodically or continuously) make up the bulk of visits (Black and Seto 2020; *Hunger Facts & Research* 2022).

Because food banks and food pantries are not run by the government, there is little publicly available information on which pantries are connected to which banks, or on the exact percentage breakdown of the origins of distributed food. Prendergast (2017) describes the author's work with Feeding America to allocate food more efficiently across food banks, and he finds that only one-quarter of the food available at food banks is provided by Feeding America, with the rest donated directly to food banks by private companies or, to a lesser extent, by USDA commodity programs and by individuals. The Center for Science in the Public Interest estimates that donations from food retailers and distributors constituted 30% of food bank inventory in 2020, and that this fraction has fallen in recent years due to improved retailer inventorying (*Healthier Food Donation Guide for Retailers and Distributors: A Corporate Resource* 2021). Therefore, a substantial fraction of the food allocated to food banks during my sample was provided by food retailers and manufacturers.

Donations from these corporations are in part driven by the significant tax incentives that encourage them to donate their unsold foodstuffs to food banks and pantries. The first such tax incentive was implemented in 1969, allowing companies that donate a food product to deduct the amount of the product's basis (the amount it cost for the company to produce or purchase the item) from its taxes. The product must be donated to a 501(c)(3) organization that will provide the food at no-cost to the ill, the needy, or minor children. This deduction was expanded for C-corporations in 1976, allowing C-corporations to deduct the lesser of twice the basis or the basis plus one-half of the difference between the product's price and its basis. The expansion was temporarily extended to all corporation types in 2005 and intermittently re-extended until being permanently implemented for all corporation types in 2015 (*Federal Enhanced Tax Deduction for Food Donation: A Legal Guide* 2016). My data do not include retailer corporation type so I cannot know which stores were only temporarily subjected to the extended deduction prior to 2015, but all retailers had at least some tax incentive to donate food during my sample. Businesses are also protected from legal liability if the donated food is spoiled or causes harm to a recipient, so long as the food is donated in good faith (*Bill Emerson Act* 2022). In addition to these tax incentives, recent laws against food waste in six states require food retailers to donate their excess, edible food to food banks or pantries, or else face fines (Cohen 2021).

These tax write-offs and liability protections require that the food is donated to a 501(c)(3) organization, and that the food is in turn provided to those in need. 501(c)(3) approval is also required for food pantries to receive food from food banks or Feeding America. Thus there is a large incentive for a food pantry to apply for 501(c)(3) approval if it is not already affiliated with a 501(c)(3) organization (such as a religious organization or broader human services charity).

There is little aggregate data on the types of products available at food pantries. Products may be donated because they are surplus, near (or past) their expiration date, have minor defects, or are seasonally outdated (*Rescuing Food for Hungry Californians* 2022). Much of the food available at food pantries is nutritionally balanced, with an estimated 72% of food provided through the Feeding America network classified as "promoting good health" (*Feeding America Annual Report 2020* 2021). Food pantries also often provide non-food household staples and hygiene products. The best large-scale data that sheds light on the types of products food banks hope to provide comes from Prendergast (2017), in which the author describes the "Choice System" that Feeding America uses to allocate food to the food banks in its network. This system created an auction for food to food banks that can purchase items with the "shares" the system provides to it based on its community's need. Items that sell for a high share price are thus revealed as more desirable, though it must be recalled that these auctions are only for foods that are allocated through Feeding America, and banks may be getting plenty of the less desirable foods from alternative sources. As shown in Figure 1 in Prendergast (2017), and reproduced here as Figure 2.1, shelf-stable foods such as cereal, pasta, and rice have very high prices per pound, as do diapers, so food banks must not have enough of these products to provide to their recipients. Produce has the lowest price per pound, suggesting that either food recipients do not want produce, or that food banks have plenty of produce already.

2.3 Data

2.3.1 Household grocery expenditures

I use data on household grocery spending from the Kilts-Nielsen Consumer Panel for years 2006-2017. This is a panel data set of between 40,000-60,000 households who scan the Universal Product Codes (UPCs) for their grocery purchases using a home scanner that records each item's cost and its date of purchase. Households are instructed to scan all purchases made at supermarkets, drug stores, department stores such as WalMart, and convenience stores, so purchases are mostly composed of foodstuffs and household products. Households are recruited to participate in the panel with monetary incentives, and are targeted to create a demographically representative sample of each geographic region's market. The data include information on households' ZIP code of residence, race, household composition, and two-year-lagged income group.

I estimate the effect of a food pantry opening in a household's ZIP code of residence on their food and non-food purchases, both in aggregate and broken down into the finer product categories detailed in Prendergast (2017). One concern is that items without UPCs are poorly covered in the data, so I likely do not see all spending on items such as fresh produce or bulk foods, though Allcott, Diamond, Dubé, Handbury, Rahkovsky, and Schnell (2019) find that a majority of produce spending is on pre-cut or otherwise packaged produce that does have UPCs. The data also do not include spending on other sources of food-away-from-home (such as restaurants), or on spending on home goods at retailers that do not sell food (such as craft or hardware stores).

2.3.2 Grocery volume and pricing

In addition to investigating how food pantries affect household purchases, I use the Kilts-Nielsen Retail Scanner Data to investigate impacts on retailer pricing decisions and volume by product type, again for years 2006-2017. The data include approximately 50% of the point-of-sale transactions made at grocery and drug stores. Total sales and the average price of each UPC is collected weekly for every store in the sample, which I aggregate to the product group-month-store level. I weight prices by the number of units sold. Unlike households in the Consumer Panel, stores in the Scanner Data only have the first three digits of their ZIP code listed. I assign stores the modal ZIP code of residence of its shoppers in the Consumer Panel over the entire sample, weighting each shopper household by the number of trips they made to that store. I match retailers to food pantries using this ZIP code proxy.

2.3.3 Food pantries

I use data on food pantry locations and 501(c)(3) approval dates from the Internal Revenue Service's (IRS) Exempt Organizations Business Master File, accessed through the Urban Institute's National Center for Charitable Statistics. This file includes the employer identification number, name, address and ZIP code, National Taxonomy of Exempt Organizations (NTEE) code, assets and income if gross receipts are greater than $50,000, and month and year of tax-exemption approval for all tax-exempt organizations.[1] The Exempt Organizations Business Master File updates each month to include all current tax-exempt organizations, and the IRS does not provide previous iterations of the data. In response to this lack of historical data,

1. All food pantries in my sample are coded as 501(c)(3) organizations, but there are other tax-exempt organization types included in the master file.

the Urban Institute's National Center for Charitable Statistics (NCCS) has collected previous extractions, with at least one file available between 1996-2020 (*Internal Revenue Service Exempt Organizations Business Master File* 2021).[2] Organizations listed in each extraction were not necessarily still operating, as closing dates are not provided, and organizations remain in the master file for up to three years after filing their last annual notice or tax return, at which point they have their 501(c)(3) approval revoked.[3] My empirical strategy requires that food pantries remain open for two years after their 501(c)(3) approval date, so I require that food pantries remain listed in the Business Master File for five years after their approval date. I further limit my sample to food pantries that open in ZIP codes that do not have an existing food pantry, and that no additional food pantries are opened within the next two years.

Figure 2.2 shows the percent of food pantries in each year of the 2006-2017 extractions that are not included in my sample because of the five year restriction. There are higher attrition rates for pantries that open during the financial crisis, perhaps because these pantries were only intended to serve an immediate and temporary need in their communities. Figure 2.3 shows the 501(c)(3) approval dates of the remaining pantries. Approval dates do not appear to be linearly determined, but any bunching in the IRS's approval process does not negate the validity of using the 501(c)(3) approval date as the first month that a food pantry can legally receive food from retailers and food banks. Further, this bunching is not concentrated in predictable months of the year, as shown in Figure 2.4.

I use each organization's NTEE code to include only those listed as "Food Banks, Food Pantries" in my sample. Though this category includes food banks, I use the term food pantries throughout this paper for simplicity, especially because the sample restrictions I make are unlikely to include many food banks. The data only include a fraction of the food pantries that likely exist, as NTEE codes classify organizations in terms of their primary exempt activities. A homeless

2. I made extractions of the Exempt Organizations Business Master File Extract in 2021 and 2022 that I use to supplement this data. I also use previous extractions of the file from Wayback Machine for years 2017-2020 to supplement those from the NCCS, as the files appear to be cut off for organizations opening after April 2016.

3. For example, a food pantry that is present in the 2006-2011 extractions, but not in the 2012-2017 extractions, may have closed any time between 2009 and 2012.

shelter or religious organization that runs a food pantry in addition to another tax-exempt activity is unlikely to be included in my sample. Additionally, there are alternative forms of providing food to those in need, including organizations that deliver food directly to recipients, or that serve meals to be consumed at the organization. I focus my analysis on food pantries because they likely provide more food to recipients and thus have a larger impact on household spending and on retailer's likelihood of donating food to or facing competition from the organization. Table 2.1 provides support of this hypothesis, showing that food banks and pantries have higher average income than the other food-related 501(c)(3) organization types, and higher average assets than most other types. Note that this table does not include organizations with less than $50,000 in gross receipts because they do not have to report their assets and income.

I match food pantries to households in the Consumer Panel and to stores in the Retail Scanner Data by ZIP code-month. After making my sample restrictions, there are 661 pantries matched to 1,692 households in the Consumer Panel, and 855 pantries matched to 2,359 retailers in the Scanner Data. Table 2.2 shows that households with a food pantry that opens up in their ZIP code are very similar to households that never have a food pantry open. The only large difference between these households is that households with a food pantry opening are much more likely to have an age 50+ head of household. This could speak to the increased need of charitable food assistance for seniors, or that retirees may have more time and financial resources to start a food pantry. The differences between retailers with and without a food pantry opening in their ZIP code are also small, as shown in Table 2.3. Stores with a pantry opening have marginally higher sales and lower median and mean incomes, but these differences are all less than 2%. These summary statistics are suggestive that food pantries do not only open up in very needy communities, or more worryingly, that there are many unseen food pantry openings in the untreated sample.

2.4 Empirical strategy

2.4.1 Theoretical framework

It is not ex ante obvious how a food pantry opening will affect nearby households and retailers. Visiting a food pantry enables households to substitute the provided foods for those they would otherwise purchase at a grocery store, so we may predict food spending reductions after a food pantry opens up, especially in the types of food that pantries provide. However, households who spend only what they receive in SNAP benefits may have little margin to reduce their private spending on food if they are food insecure and not spending enough on food at baseline, and the subset of households that visit food pantries are even more likely to qualify as food insecure than households who receive SNAP benefits (48% versus 28% according to Fan, Gundersen, Baylis, and Saksena (2021)). We may therefore predict no change in total spending for households when a food pantry opens up if only the most constrained individuals visit the food pantry. However, the composition of purchased foods will likely change, as a food pantry does not provide the same bundle of foods that households would purchase at a grocery store.

Effects on retailers are similarly ambiguous. The products available at food pantries are in large part substitutable with those at food retailers, and this competition may hurt retailer sales on those products. However, a nearby food pantry may also draw more shoppers to the area and increase the convenience of the store, such that some pantry visitors choose to shop at the retailer that is near the food pantry rather than the one nearest their home. This may increase sales broadly, though less so on pantry-available products. Additionally, the tax benefits that retailers receive when donating their excess foodstuffs could encourage retailers to raise prices since the cost of unsold food is effectively lower, though Dellavigna and Gentzkow (2019) find evidence of uniform pricing across stores within retail chains. Finally, retailers may also change the composition of the types of foods they offer or their prices depending on how the composition of their shoppers changes: does the food pantry attract more low-income consumers to the store; or, does it crowd out the private consumption of low-income households and leave a wealthier population of shoppers, that may prefer different products or be willing to pay higher prices?

In addition to studying overall trends in household spending and retailer sales volume, focusing on the product categories of Prendergast (2017) is of particular interest because it is unclear whether items with low prices on Feeding America's auctions are undesirable, or food banks already have an abundance of those items. For example, suppose that food banks have many alternative sources of produce outside of Feeding America. Then the share price may be low not because food pantry recipients do not value produce, but because food banks would rather spend their shares on items they cannot get elsewhere. In this case, we would expect pantry visitors to receive produce and lower their private spending on it. Alternatively, if the share price is low because pantry recipients do not want produce, or because food pantries do not have adequate facilities to store it, they will not receive it from the food pantry and there will be no spending change.[4] Analogous arguments follow for items with high auction prices. Therefore we can use household spending and retailer sales responses to infer the types of foods that are provided at food pantries and to learn more about why food pantries place a higher value on some products compared to others.

2.4.2 Identification motivation

To learn anything causal about how a nearby food pantry opening affects households and retailers, I would ideally exploit some quasi-random policy variation that causes food pantries to open in some areas but not in others. This gold standard of identification is missing in the literature on food pantries largely because they are highly decentralized and privately organized, so there are no clear policy changes to study. I instead rely on evidence that *where* food pantries open is not random, but *when* they open appears to be, at least within a two-year window.

I match data on the number of business establishments and employment from the 2006-2017 ZIP Codes Business Patterns (ZBP) to pantry openings by ZIP code. The ZBP data is

4. Of course, this may also occur for households that are sufficiently constrained in their food spending that they do not reduce their private consumption when they visit a food pantry, again considering the SNAP household who only spends the amount they receive in benefits.

only provided annually, but it provides the best information on the local economy at the ZIP code level. Figure 2.5 shows that a pantry opening is associated with other types of business openings, as the number of establishments has a steady upward trend when considering the three years before the pantry opens and the first three years after it opens. Consider if some of these establishments are restaurants. I may find that a food pantry opening is associated with reduced food spending not because households are visiting the food pantry, but because they are consuming more food-away-from-home. Increasing establishment counts further suggest that communities may generally be improving; indeed, Figure 2.6 shows that employment is also increasing in treated ZIP codes around the opening. This may cause food spending to increase, again for reasons that are unrelated to the food pantry.

ZIP codes with pantry openings appear to have underlying trends in their local economy, but the timing of pantry opening may not be sensitive to these trends. I limit the sample to ever-treated ZIP codes (i.e. ZIP codes with a pantry opening between 2006-2017) and find that the trends in establishment counts and employment are no longer associated with the timing of the pantry opening, as shown in Figure 2.7 and Figure 2.8. This motivates my empirical strategy, in which I limit the sample to ZIP codes that are ever treated in my sample and create a stacked data set around two years of the pantry opening. This eliminates pre-trends in my outcome variables of interest.

I therefore focus on the small subset of ZIP codes that have one food pantry opening between 2006-2017, have no subsequent food pantry opening for two years after the first opening, and that had no previous food pantry that opened before 2006 but operated at any point between 2006-2017. These ZIP codes, while similar to each other in their underlying economic trends, clearly have important differences from untreated ZIP codes which may affect the external validity of my results. One concern may be that food pantries open in areas that are underserved by other forms of social support, lacking the philanthropic or religious organizations that often provide food assistance without being labeled as a food pantry in the IRS data. The results could then be much larger than expected if a food pantry were to open in an area that was already well-served by these other types of institutions. Table 2.4 counters this hypothesis, showing that ZIP-years with a food pantry opening have more related 501(c)(3) organization types than ZIP-years without a food pantry opening. This difference is smaller when considering that ZIP-years with an opening also have an approximately 25% higher population; still, there are many more religious organizations than would be expected by population alone.

A final consideration is that a food pantry's effect on households and retailers may grow over time as it becomes established in the community. Studying the first two years after a pantry's opening may underestimate the effects of food pantries if food pantries are smaller and reach fewer households after they first open. I consider the food pantries in my main sample and show how the average level of assets and income changes over the first five years in Figure 2.9. Income increases by about 50% between the first and fifth year (with an inexplicable spike during the fourth year) and assets increase by nearly 100%. This figure only includes income and asset levels when reported; the share of pantries that have gross receipts less than $50,000 and do not have to report their financial data are shown in Figure 2.10. The share of pantries with sufficient low receipts falls by more than fifteen percentage points over the five years considered. This growth in income and assets implies that my results would likely be larger if I considered the long-run impacts of food pantries in their communities.

2.4.3 Estimation

I use variation in the timing of a food pantry opening to estimate how food pantries affect household spending and retailer pricing and sales. I create a "stacked" data set similar to that in Deshpande and Li (2019), limiting the sample to ZIP codes that ever have a food pantry opening and treating later-treated ZIP codes as controls for early-treated ZIP codes. For each month between January 2008 and December 2015, I keep all ZIP codes with a pantry opening in that month as well as all ZIP codes that have their pantry opening more than 24 months in the future. I consider the ZIP codes with their pantry openings in that month as "treated", and the ZIP codes with later food pantry openings as "untreated". I create a panel for 24 months before and after that treatment month, and label this data set as one "group". I repeat this process for every month, each time creating different groups. I append all data sets and then match these ZIP code-months to household-months in the Consumer Panel and retailer-months in the Retail Scanner Data.

The estimating equation is:

$$y_{izot} = \beta_0 treated_{zo} + \sum_{\tau} D_{ot}^{\tau} + \beta_1 (treated_{zo} \times post_{ot}) + \beta_2 zero_{ot} + \alpha_i + \gamma_\tau + \varepsilon_{izot} \tag{2.1}$$

where y_{izot} is the outcome variable for household or retailer i in ZIP code z for opening group o in month t, $treated_{zo}$ is equal to one if ZIP code z is treated in opening group o, $\sum_{\tau} D_{ot}^{\tau}$ are indicator variables for the relative month t of opening o, $post_{ot}$ is equal to one for all months after the pantry opening month in group o, and $zero_{ot}$ is equal to one for the opening month in opening o. The regression also includes household or retailer fixed effects, as well as month-year fixed effects. Standard errors are clustered by opening group, creating 96 clusters. The coefficient of interest is β_1, which provides the effect of a food pantry opening on the outcome variable.

The outcome variable y_{izot} is the inverse hyperbolic sine of household spending or the log of retailer sales, pricing, or the count of sold products. I transform the household spending data using the inverse hyperbolic sine because households do not purchase all products each month, so there are many zeros in the data. The inverse hyperbolic sine can be interpreted as an elasticity similarly to the logarithmic function, but its recommended usage is only for data with less than one-third of its observations equal to zero according to Bellemare and Wichman (2020). Some product categories have a higher fraction of their observations equal to zero; this information is reported in my tables and discussed in the results. I estimate the regression for all households, and for the subset of households with income below 200% of the federal poverty line when the food pantry opens. These low-income households are most likely to use the food pantry. In the retailer regressions, I limit the sample to include only those retailers who ever have positive sales of that product category. This leaves very few zeros and so I transform the data using log as in a standard elasticity estimate.

In addition to the primary difference-in-differences regression, I also run an event study specification:

$$y_{izot} = \beta_0 treated_{zo} + \sum_{\tau} D_{ot}^{\tau} + \sum_{\tau} \beta_{\tau}(treated_{zo} \times D_{ot}^{\tau}) + \alpha_i + \gamma_{\tau} + \varepsilon_{izot} \tag{2.2}$$

and plot β_τ for the main outcome variables of interest. I only create event studies using the log of household food spending and the log of store food sales volume as y_{izot}. Note that there are very few zeros for household food spending at the monthly level, so I can transform the data using logarithm instead of relying on the inverse hyperbolic sine.

2.5 Results

2.5.1 Household spending

I estimate Equation 2.1 on the stacked Consumer Panel data for overall food spending, as well as for spending on each of the food-related product categories in Prendergast (2017), and for spending on diapers. Results are provided in Table 2.5. The first row shows that a food pantry opening is associated with a 1.8% decrease in the amount of food that households purchase, and this coefficient is statistically significant at the 5% level. A simplified back-of-the-envelope calculation justifies the size of this coefficient: food pantries served 40 million people in 2019, or an estimated 12% of the population.[5] If 12% of households visit the food pantry after it opens up, then the coefficient implies that households that visit the pantry receive 15% of their monthly food from the pantry.[6] The size of food pantries is very heterogeneous and there is little evidence on the amount of food that the average pantry provides to households, but this number at least sounds plausible.

The subsequent regressions on different product categories mostly return small and

5. I use the pre-pandemic level of food pantry participation as more representative of usage during my sample, given that the count of individuals served jumped to 60 million in 2020.

6. Assuming that households that do not visit the pantry have no change to their food spending: $0.12 \times -0.15 + 0.88 \times 0 = -0.018$.

statistically insignificant coefficients. This may in some part be driven by the large share of zeros in the data for some product categories, as many households do not have positive spending on every product category in every month. The inverse hyperbolic sine behaves strangely around zero and having a large fraction of zeros in the data causes the coefficient estimates to less reliably approximate the logarithmic transform. When considering product categories that meet the less-than-one-third-zeros rule-of-thumb, there are statistically significant reductions in spending on prepared foods at the 5% level and on snack foods at the 10% level, and on processed vegetables (including canned vegetables) at the 10% level (though this category just misses the one-third rule). The results are suggestive that households are more likely to receive prepared foods, snacks, and processed vegetables at food pantries, and shift their private purchasing away from these foods and toward other product categories.

In Table 2.6, I limit the sample to households with income below 200% of the federal poverty line when the food pantry opens. The sample shrinks by 75% and nearly all results are statistically insignificant, likely at least in part because of the loss of power. The only statistically significant increase is on diaper purchasing, with households spending 9.8% more on diapers after a food pantry opens up in their ZIP code. This suggests that households are able to spend less out-of-pocket on foodstuffs and more on products that federal benefits do not provide, such as diapers. However, with zero spending on diapers for 89% of household-months, this result should be interpreted with caution.

Figure 2.11 shows an event study for household food spending changes after a food pantry opens. There is a small reduction in food spending after a food pantry opens, but the difference is often only marginally statistically significant. The effect remains mostly constant over the 24 months after the pantry opens, which is surprising given that pantries' income and assets grow over time. A longer time horizon may need to be considered to see any growth in the effect.

2.5.2 Store volume, sales, and pricing

In Table 2.7, I estimate Equation 2.1 on the stacked Retail Scanner Data to find the effect of a food pantry opening on the sales, count of products sold, count of units (i.e. packages) sold, average price of individual products sold, and average price of units for the product categories of Prendergast (2017). The first row shows a statistically insignificant and small effect of a food pantry opening on a store's overall food sales, with a food pantry opening causing a 0.3% increase in sales. However, there are some statistically significant effects on sales by product category, with an associated 1.7% increase on condiment sales, a 0.8% increase on snack sales, a 2.3% increase on dairy sales, a 1.1% increase on juice sales, and a 8.4% increase on produce sales, when considering only the sales coefficients that are significant at least at the 5% level. The effects for all these product categories are driven by an increase in the count of the product sold, either as a raw count or as the number of units. There are no more statistically significant effects on price than we would expect as false positives, providing further support to the existing evidence of uniform pricing within retailer chains.

The statistically significant effects on sales are all positive, implying that food pantries complement nearby stores rather than crowding them out. They are all also on product categories without significant reductions in household food spending from the Consumer Panel regression, except for snack foods. This is suggestive evidence supporting two hypotheses. The first is that a food pantry opening attracts more shoppers to the geographic area, causing overall sales to increase even as many low-income households spend less. The second is that households can shift their consumption away from the types of foods that are typically provided at food pantries and toward products that food pantries may have a more difficult time storing or may have fewer sources providing.

The four product categories with the lowest prices per pound in Feeding America's auctions (reproduced from Prendergast (2017) in Figure 2.1) were dairy, juice, drinks, and produce. Sales increase in all four of these categories after a food pantry opens up in the store's

ZIP code, and these increases are statistically significant at least at the 10% level. This suggests that these products are desirable to food pantry visitors (because households that do not visit the food pantry should not change their spending, and thus should not be responsible for the increase in sales), and the auction prices are low because food pantries have a difficult time storing these products rather than because visitors do not consume those foods.[7] The lack of a spending response for the most expensive foodstuffs in Feeding America's auctions implies that households do not change their purchasing behavior on these products, and so food pantries must have too little of these products available to provide the optimal amount to their visitors, explaining their high price.

Finally, Figure 2.12 shows how retailer food sales changes after a food pantry opens. There seems to be some seasonality in food sales despite including month-year fixed effects in Equation 2.2. Nonetheless, there is no significant effect of a food pantry opening on the overall amount of sales in any of the 24 months considered. Food pantries appear to enable households to change the consumption of the foods they purchase more than their total amount of spending.

2.6 Conclusion

In this paper, I provide the first suggestive evidence of how household food spending and retailer sales and pricing are affected when a food pantry opens up on their ZIP code. I proxy for pantry opening dates using their 501(c)(3) approval month, and connect pantries to large panels of households and stores on ZIP code. I find that households reduce their spending on prepared foods, snack foods, and processed vegetables. Prepared foods includes prepared salads, sandwiches, and deli items that cannot remain on the shelf indefinitely, so it seems sensible that households often receive these foods from food pantries as grocery stores donate them at a high frequency. Processed vegetables include the canned vegetables many households donate at a "food drive", so this result also seems plausibly driven by the food pantry opening. Retailers face significant increases in their sales on product groups that have low sale prices on Feeding

7. Juice and drinks may also be heavier products, given that prices are only provided per pound.

America's auctions, suggesting that these product groups are indeed desirable by food pantry visitors, but pantries lack the storage or some other capability to adequately provide them.

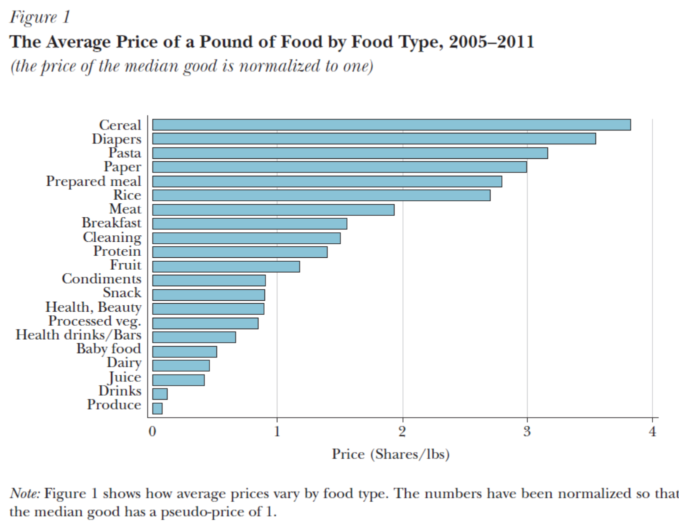
As discussed throughout this paper, many (likely, most) food pantries are not included in my sample. Some of the treated ZIP codes may have pantries run through another type of 501(c)(3) organization prior to the opening that I observe. Matching households and stores to food pantries by ZIP code is also not ideal, as the size of ZIP codes is heterogeneous and households and stores could be affected by pantries outside of their ZIP code. And, I do not control for the size of food pantries even though some serve thousands of households and others serve only a few dozen. Still, given the lack of administrative data, this paper takes a first stab at an important issue and provides a pathway forward for researchers with access to any institutional data that may exist. One state (Idaho) was responsive to my Freedom of Information Act requests, and though the Kilts-Nielsen data have too few Idaho observations, others may be able to connect data from a state administering body on other outcomes. Feeding America's Hunger in America survey also provides information on food pantry locations that may be connected with the IRS data and the Kilts-Nielsen data in interesting ways. With 40 million Americans regularly served each year, food pantries are too important for data limitations to prevent economists from studying them.

2.7 Acknowledgments

Chapter 2, "The Effect of Food Pantries on Household Grocery Expenditures and Grocery Retailer Sales" is currently being prepared for submission for publication of the material. The dissertation author was the sole author of the chapter. Researcher's own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had

no role in, and was not involved in analyzing and preparing the results reported herein.

2.8 Figures and Tables

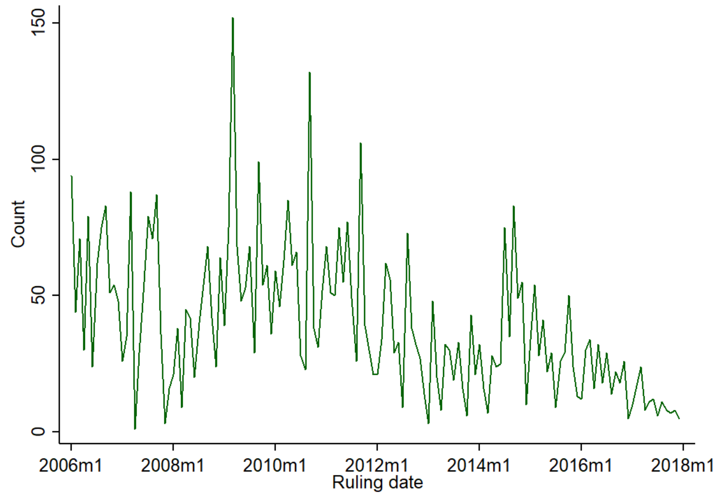


From: “How Food Banks Use Markets to Feed the Poor.” by Canice Prendergast, 2017, *Journal of Economic Perspectives* 31(4): 145-162. Copyright American Economic Association; reproduced with permission of the Journal of Economic Perspectives.

Figure 2.1. Average Prices for Feeding America’s Auctions

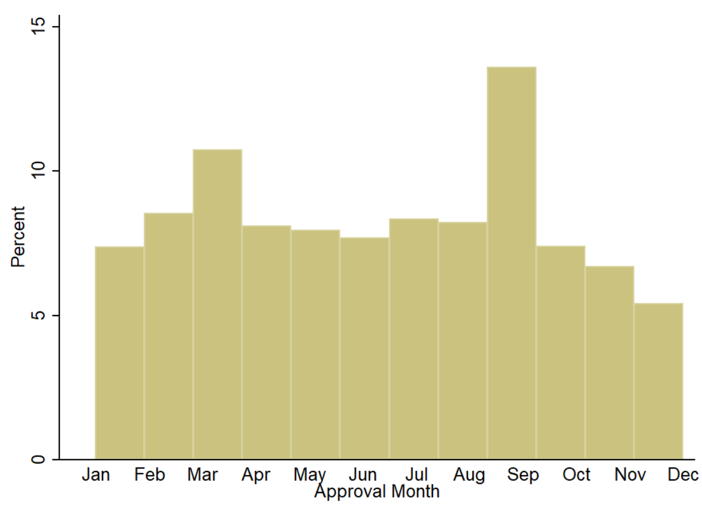
This figure presents the percent of food pantries in each extraction of the Exempt Organization Business Master File that do not remain in the file for at least five years, and thus are not included in the sample.

Figure 2.2. Food Pantry Attrition After Five-Year Restriction



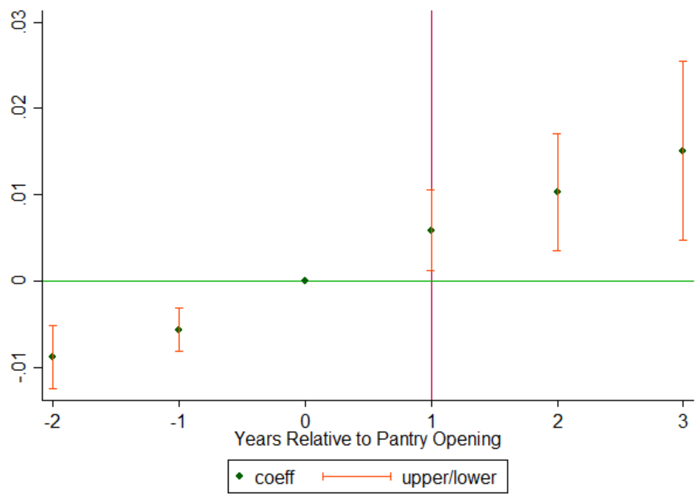
This figure shows the count of food pantries in the sample by 501(c)(3) ruling date.

Figure 2.3. Ruling Dates of Food Pantries



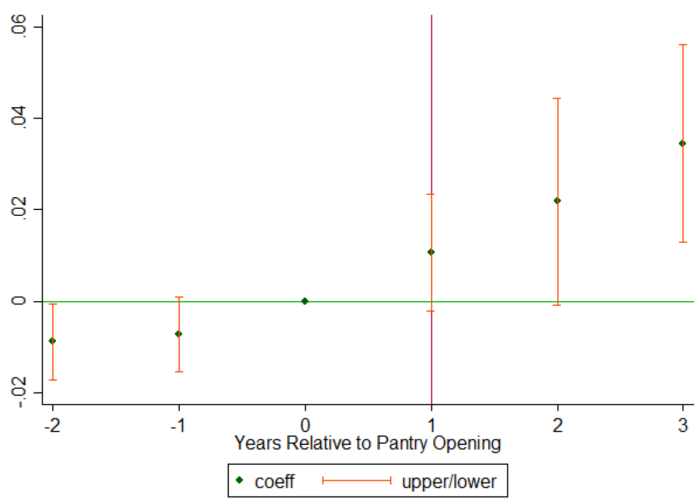
This figure shows the distribution of the months of food pantry 501(c)(3) ruling dates in the sample.

Figure 2.4. Ruling Months of Food Pantries



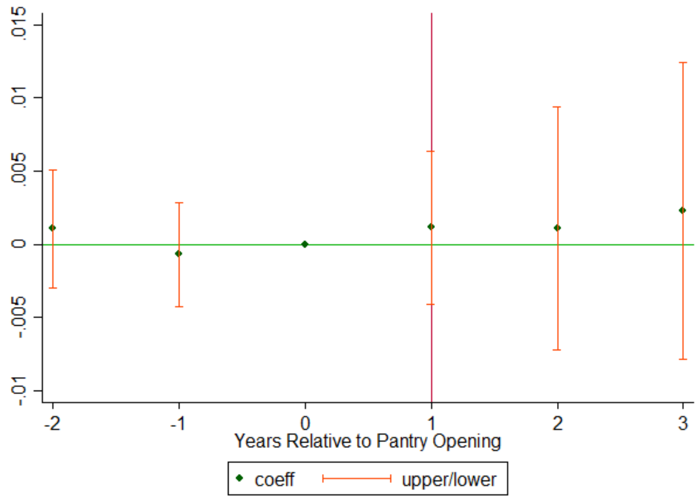
This figure shows how the log of the number of establishments in a ZIP code is associated with food pantry openings using data from the ZIP Code Business Patterns. All ZIP codes are included in the balanced stacked data sample, created as described in the estimation strategy.

Figure 2.5. Log Establishment Count, All ZIP Codes



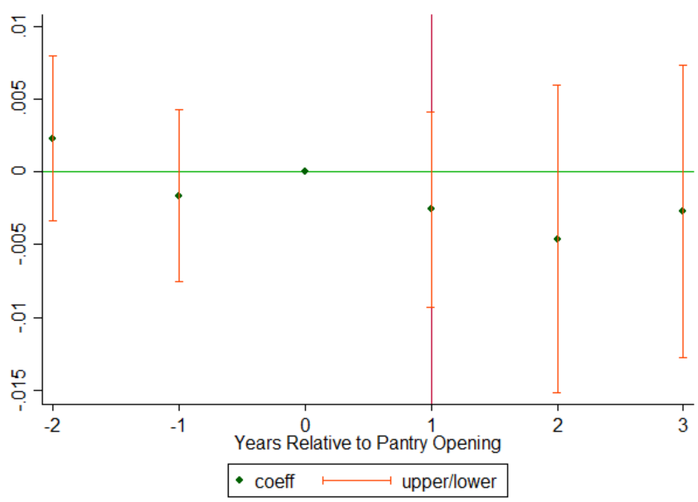
This figure shows how log employment is associated with food pantry openings using data from the ZIP Code Business Patterns. All ZIP codes are included in the balanced stacked data sample, created as described in the estimation strategy.

Figure 2.6. Log Employment, All ZIP Codes



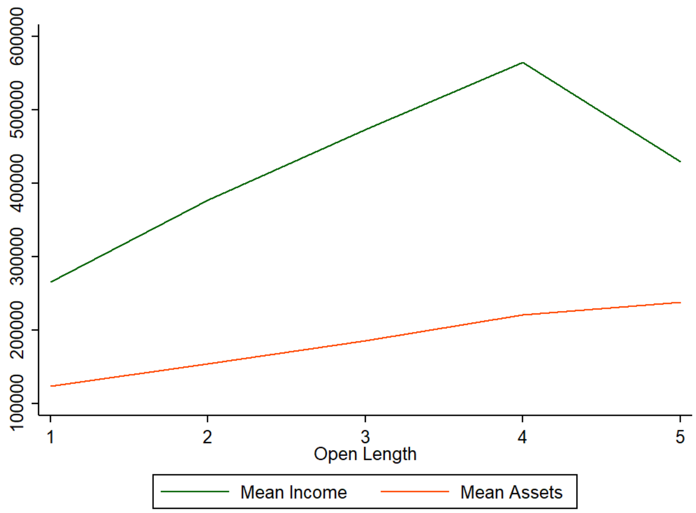
This figure shows how the log of the number of establishments in a ZIP code is associated with food pantry openings using data from the ZIP Code Business Patterns. Only ZIP codes that have a food pantry opening are included in the balanced stacked data sample, created as described in the estimation strategy.

Figure 2.7. Log Establishment Count, Treated ZIP Codes



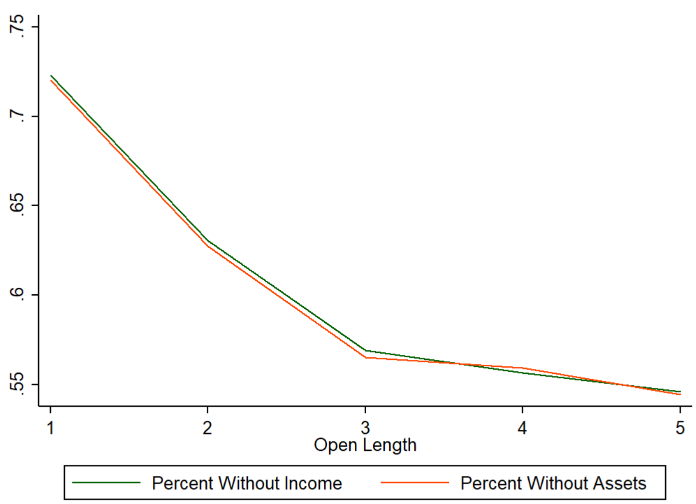
This figure shows how log employment is associated with food pantry openings using data from the ZIP Code Business Patterns. Only ZIP codes that have a food pantry opening are included in the balanced stacked data sample, created as described in the estimation strategy.

Figure 2.8. Log Employment, Treated ZIP Codes



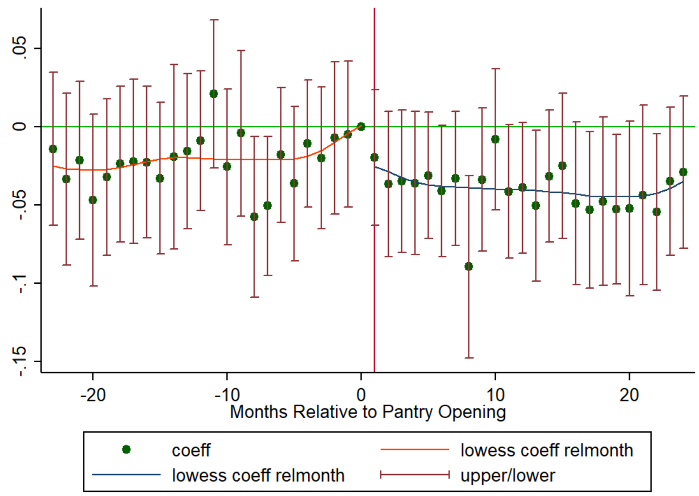
This figure shows the average annual income and assets of food pantries in the main sample for the first five years that they appear in the Exempt Organization Business Master File. Only pantries with at least $50,000 in gross receipts must submit data on their income and assets; those with lower gross receipts are not included in this figure. This is a balanced panel because all pantries in the sample are required to be in at least five years of these files.

Figure 2.9. Average Annual Income and Assets of Food Pantries After Opening



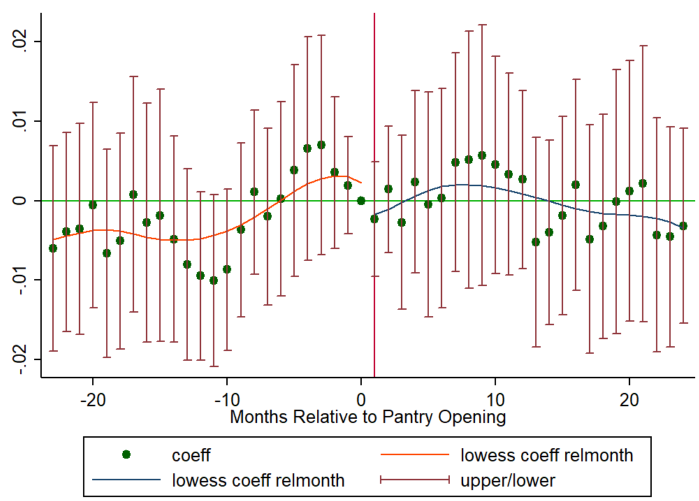
This figure shows the share of food pantries in the main sample with no reported annual income or assets for the first five years that they appear in the Exempt Organization Business Master File. Food pantries do not have to report their income or assets if their gross receipts are less than $50,000; a very small number choose to report one number voluntarily, so the lines in the figure do not overlap. This is a balanced panel because all pantries in the sample are required to be in at least five years of these files.

Figure 2.10. Log Employment, Treated ZIP Codes



This figure shows the event study for Equation 2.2 using the stacked Consumer Panel Data. 95% confidence intervals are plotted with coefficient estimates.

Figure 2.11. Effect of a Food Pantry Opening on Household Food Spending



This figure shows the event study for Equation 2.2 using the stacked Retail Scanner Data. 95% confidence intervals are plotted with coefficient estimates.

Figure 2.12. Effect of a Food Pantry Opening on Store Food Sales

Table 2.1. Assets and Income for Food-Related 501(c)(3) Organizations

	Food Banks and Pantries	Food Service	Congregate Meals	Eatery Sponsored	Meals on Wheels
Assets	567,463	346,362	676,826	829,834	365,466
Income	1,778,880	821,709	1,041,256	1,300,495	525,918
Obs.	727	639	35	80	92

The table shows the average assets and income for all food-related 501(c)(3) organizations in the December 2017 Exempt Organizations Business Master File. The sample restrictions described in the data section of this paper do not apply to the organizations in this table.

Table 2.2. Summary Statistics for Households With and Without a Pantry Opening

	Pantry Opening	No Pantry Opening
Household income	55054.19	56290.19
Below 200% FPL	0.25	0.29
Between 200% FPL & 400% FPL	0.39	0.38
Above 400% FPL	0.36	0.33
Household size	2.21	2.62
Married	0.63	0.65
White	0.82	0.79
Black	0.10	0.09
Asian/Other	0.04	0.05
Hispanic	0.04	0.07
Head: 50+ Years Old	0.73	0.52
Obs. (Household)	1692	165756

The sample is drawn from years 2006-2017 of the Kilts-Nielsen Consumer Panel. Summary statistics are given at the household level. Household income is given as a two-year-lag in sixteen bins. I assign households the median income in their income bin. I define households as Hispanic if the household head reports Hispanic ethnicity; otherwise, I define them as white, Black, or Asian/Other based on their household head's self-identification. I assign households the average of their reported annual income if they are in the panel for multiple years; the other variables are unchanging. Column one includes households with a food pantry opening in their ZIP code of residence that meets all sample qualification criteria: the food pantry must open between 2006-2017, have no older food pantries operating in its ZIP code, have no subsequent food pantry openings within two years of its opening, and it must remain in the Exempt Organization Business Master File for five years after its opening. Column two includes households without a pantry opening.

Table 2.3. Summary Statistics for Stores With and Without a Pantry Opening

	Pantry Opening	No Pantry Opening
Monthly sales	650894	646209
Median income in ZIP code	57710	58542
Mean income in ZIP code	73465	74610
Obs. (Retailer)	2359	26585

The sample is drawn from years 2006-2017 of the Kilts-Nielsen Retail Scanner Data. Summary statistics are given at the store level. Sales are given as the average monthly sales of each store. Store ZIP code is assigned using the trip-weighted modal ZIP code of households that visit the store between 2006-2017. I match stores to their ZIP code median and mean income from the 2011 American Community Survey 5-year Average Estimates. Column one includes stores with a food pantry opening in their ZIP code that meets all sample qualification criteria: the food pantry must open between 2006-2017, have no older food pantries operating in its ZIP code, have no subsequent food pantry openings within two years of its opening, and it must remain in the Exempt Organization Business Master File for five years after its opening. Column two includes stores without a pantry opening.

Table 2.4. Other 501(c)(3) Organizations by Pantry Opening Status

	ZIP-Years With Opening	ZIP-Years Without Opening
Pantry $<$ 2006	0.08	0.06
Pantry $>$ 2017	0.00	0.01
Other Food	0.87	0.61
Religious	19.23	10.97
Housing	2.29	1.61
Philanthropy	6.88	4.96
Human Services	6.86	5.39
Population	22705.54	18169.43
Obs.	1,044	231,781

The table shows the number of establishments by organization type in ZIP-years with a food pantry opening and in ZIP-years that never have a food pantry opening between 2006-2017. It also shows the percent of ZIP-years with a food pantry opening before 2006 and after 2017; note that the 8% of ZIP-years with a food pantry opening that also have a food pantry opening before 2006 are cut from the main sample. Population is provided at the ZIP code level as the five-year average from the 2011 American Community Survey.

Table 2.5. Effects on a Food Pantry Opening on Household Spending

	β	Std. err.	N	Y mean	% 0
Food	-0.018**	0.009	1876656	202.105	0.00
Cereal	0.012	0.022	1876656	4.108	0.58
Diapers	0.021	0.013	1876656	0.740	0.90
Pasta	-0.013	0.014	1876656	1.507	0.65
Prepared	-0.037**	0.017	1876656	26.819	0.08
Rice	-0.051**	0.022	1876656	3.510	0.76
Meat	-0.033	0.026	1876656	19.729	0.15
Breakfast	-0.005	0.019	1876656	4.343	0.49
Fruit	0.001	0.015	1876656	2.281	0.63
Condiments	0.005	0.020	1876656	9.757	0.19
Snacks	-0.031*	0.017	1876656	23.132	0.11
Processed Veg	-0.059*	0.030	1876656	7.259	0.38
Baby Food	0.003	0.010	1876656	0.493	0.98
Dairy	0.001	0.018	1876656	30.625	0.03
Juice	0.034	0.025	1876656	5.285	0.49
Drinks	-0.027	0.026	1876656	22.298	0.13
Produce	0.010	0.025	1876656	10.493	0.37

Source: Kilts-Nielsen Consumer Panel
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table estimates Equation 2.1 for the stacked Consumer Panel data. Each row represents a different regression using the inverse hyperbolic sine of monthly household spending on the listed product category as its dependent variable. Variables in rows 2-18 are listed in the order of average price per pound in Figure 2.1. Y mean is the mean of the level of spending on the dependent variable. The final column provides the percent of the dependent variable that is equal to zero. β is the coefficient on $treated_{zo} \times post_{ot}$. Standard errors are clustered by opening group. All regressions include household and month-year fixed effects.

Table 2.6. Effects on a Food Pantry Opening on Low-Income Household Spending

	β	Std. err.	N	Y mean	% 0
Food	-0.031	0.022	458016	190.739	0.00
Cereal	-0.033	0.044	458016	3.863	0.59
Diapers	0.098***	0.028	458016	0.860	0.89
Pasta	0.043	0.027	458016	1.359	0.67
Prepared	-0.077*	0.042	458016	25.314	0.08
Rice	-0.055	0.040	458016	3.079	0.77
Meat	-0.036	0.048	458016	17.560	0.16
Breakfast	-0.056	0.047	458016	4.084	0.48
Fruit	-0.028	0.034	458016	2.236	0.63
Condiments	0.023	0.044	458016	9.326	0.19
Snacks	-0.006	0.032	458016	21.342	0.11
Processed Veg	-0.062	0.052	458016	6.031	0.40
Baby Food	0.022	0.030	458016	0.820	0.97
Dairy	0.011	0.035	458016	28.425	0.03
Juice	-0.036	0.050	458016	4.717	0.51
Drinks	-0.025	0.054	458016	22.405	0.12
Produce	-0.009	0.048	458016	9.064	0.38

Source: Kilts-Nielsen Consumer Panel
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table estimates Equation 2.1 for the stacked Consumer Panel data. Each row represents a different regression using the inverse hyperbolic sine of monthly household spending on the listed product category as its dependent variable. Variables in rows 2-18 are listed in the order of average price per pound in Figure 2.1. Y mean is the mean of the level of spending on the dependent variable. The final column provides the percent of the dependent variable that is equal to zero. β is the coefficient on $treated_{zo} \times post_{ot}$. Standard errors are clustered by opening group. All regressions include household and month-year fixed effects. The sample is limited to households with their reported income below 200% of the federal poverty line in the year of the food pantry opening.

Table 2.7. Effects on a Food Pantry Opening on Retailer Sales and Pricing

	Sales	Count	Units	Count Price	Unit Price	Sales Mean	N
Food	0.003					752193.125	3202704
	(0.006)						
Cereal	0.001	-0.004	-0.004	0.003	0.004**	12831.242	3196648
	(0.007)	(0.008)	(0.008)	(0.002)	(0.002)		
Diapers	0.003	0.003	0.003	0.003*	-0.000	5471.241	3201832
	(0.007)	(0.008)	(0.007)	(0.002)	(0.001)		
Pasta	0.019*	0.023**	0.020*	-0.003	-0.001	2640.209	3042032
	(0.011)	(0.010)	(0.011)	(0.003)	(0.003)		
Prepared	0.010	0.007	0.011	0.003	-0.001	63970.008	3194655
	(0.007)	(0.007)	(0.007)	(0.003)	(0.002)		
Rice	-0.003	-0.001	-0.000	-0.004	-0.003	3374.310	2441223
	(0.013)	(0.015)	(0.012)	(0.007)	(0.005)		
Meat	0.021*	0.026**	0.024**	-0.006	-0.003	36656.027	3189499
	(0.011)	(0.013)	(0.011)	(0.005)	(0.004)		
Breakfast	0.012*	0.010	0.010	0.004	0.003	4886.695	3202177
	(0.007)	(0.008)	(0.007)	(0.003)	(0.002)		
Fruit	0.015	0.008	0.015	0.001	-0.001	4288.678	3199570
	(0.009)	(0.011)	(0.011)	(0.005)	(0.004)		
Condiments	0.017**	0.022***	0.019**	-0.009*	-0.002	28305.051	3200095
	(0.007)	(0.008)	(0.008)	(0.005)	(0.002)		
Snacks	0.008**	0.008**	0.008**	-0.000	0.000	56984.969	3202703
	(0.004)	(0.004)	(0.004)	(0.001)	(0.001)		
Processed Veg	0.012	0.007	0.011	0.004	0.000	12837.132	3094538
	(0.010)	(0.012)	(0.012)	(0.004)	(0.005)		
Baby Food	0.000	0.004	0.003	-0.004	-0.003	7546.410	3196034
	(0.008)	(0.009)	(0.008)	(0.004)	(0.004)		
Dairy	0.023***	-0.027	0.021**	-0.003	0.002	73323.242	3179990
	(0.009)	(0.019)	(0.009)	(0.006)	(0.003)		
Juice	0.011**	0.007	0.005	0.001	0.005***	19842.416	3202491
	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)		
Drinks	0.007*	0.005	0.008*	-0.001	-0.000	74712.922	3202631
	(0.004)	(0.005)	(0.004)	(0.003)	(0.002)		
Produce	0.084**	0.083*	0.073*	0.012	0.011	431689.344	1681978
	(0.040)	(0.046)	(0.039)	(0.008)	(0.007)		

Source: Kilts-Nielsen Retail Scanner Data
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table estimates Equation 2.1 for the stacked Retail Scanner Data. Each cell in the first five columns represents a different regression, where the dependent variable is the log of the column variable for the row's product category. Each of these cells displays the coefficient on $treated_{zo} \times post_{ot}$ and its standard errors in parentheses. Variables in rows 2-18 are listed in the order of average price per pound in Figure 2.1. Sales mean is the mean monthly sales of that product category. Standard errors are clustered by opening group. Stores are only included in the regressions when they ever have positive sales of the product category. All regressions include store and month-year fixed effects.

Chapter 3

The Distributional Impacts of Taxes on Health Products: Evidence from Diaper Sales Tax Exemptions

3.1 Introduction

Many economists argue that commodity taxes should be uniform across products, as in the classic results of Atkinson and Stiglitz (1976). There are two main ways that policies often alter these recommendations: excise or “sin” taxes on goods such as cigarettes or gas, and sales tax exemptions on goods such as groceries or health products. In this paper, we analyze sales taxes on diapers, a product that has direct health implications but is not always considered a health product in tax codes.

Policies determining sales taxes are subject to a trade-off between efficiency and equity. Since lower income households spend a greater share of their budget on consumption goods, sales taxes are regressive, which decreases equity. However, it is more efficient to tax goods for which demand is relatively inelastic, as this minimizes distortions to behavior. This has led some commentators to argue that health products should be subject to tax without exemptions (Kaeding 2017).

There is evidence from the cigarette tax literature that while all consumers respond to the more salient posted excise tax, only poorer consumers respond to the sales tax applied at the register (Goldin and Homonoff 2013). This opens the possibility that poorer families could be disproportionately responsive to sales taxes on diapers as well. Adda and Cornaglia (2006) find that smokers respond to cigarette taxes by purchasing fewer in number but using them more intensely, therefore inhaling higher concentrations of nicotine and carcinogens. A related concern for diapers is the risk that parents may purchase too few diapers for their children, requiring a longer duration for each diaper use and leading to potential health consequences.

There is reason to believe that the elasticity of demand for diapers is particularly variable in income. For high-income households, diaper purchases may already be at their optimum level regardless of price, as there are only so many diaper changes necessary per day. However, many American families struggle to afford enough diapers for their children. A survey of low-income mothers in New Haven found that 27.5% reported diaper need (Smith, Kruse, Weir, and Goldblum 2013), and an Obama White House blog post concurred that one in three American families face this difficulty (Muñoz 2016). If low-income households are liquidity constrained, their demand for diapers could be especially elastic (as diapers represent a greater share of their budget), which would imply a higher excess burden of sales taxes on diapers for this group. Thus, setting sales tax policy based on inelastic responses by high-income households could mean low-income households may be under-spending on diapers relative to the socially optimal amount.

Both survey and medical evidence suggest that diaper need is associated with negative health consequences. When poor families are not able to purchase enough diapers, they are forced to decrease the frequency of changing their children's diapers. Infrequent diaper changes lead to increased risks of urinary tract infections, diaper dermatitis, and secondary diaper dermatitis infections such as staph (Sugimura, Tananari, Ozaki, Maeno, Tanaka, Ito, Kawano, and Masunaga 2009; Adalat, Wall, and Goodyear 2007; Fernandes, Machado, and Oliveira 2009). These increased infections lead to more doctors' visits and emergency room visits. Smith, Kruse, Weir, and Goldblum (2013)'s survey of low socioeconomic status women showed a correlation between diaper need and increased stress and depression for mothers. The proposed mechanism for this relationship is that the financial inability to change a child's diaper when necessary increases their discomfort and crying, which can reinforce feelings of inadequacy and helplessness for their mothers.

To assess these issues, we study the response of diaper purchasing behavior to changes in taxes on diapers between 2006 and 2013. In addition to state-level changes in sales tax rates, there were four larger changes to diaper sales tax exemptions in this period. Connecticut exempted diapers from sales taxes under a clothing exemption until it was repealed in 2011. Meanwhile, New York eliminated their state sales taxes on diapers in 2006 as part of an overall sales tax exemption for clothing, and temporarily reinstated these taxes between 2010 and 2011, creating three changes in policy. New York counties were allowed to individually decide whether to apply this exemption to local taxes, which created county-level variation in the changes in tax rates over this period. We use this variation to examine how diaper purchasing patterns respond after sales tax changes, and whether these responses are higher in low-income households and communities.

Our data sources are the Kilts-Nielsen Consumer Panel and the Kilts-Nielsen Retail Scanner Data, which are uniquely positioned to study the effects of sales taxes given their detailed product-level information (as previously used by Harding, Leibtag, and Lovenheim (2012) and Kroft, Laliberté, Leal-Vizcaíno, and Notowidigdo (2019)). We first use the Consumer Panel to investigate diaper purchasing patterns at the household level. We demonstrate that low-income households buy fewer diapers and spend less on diapers than high-income households.

We then analyze the effects of taxes on diapers by examining the Retail Scanner Data, which provides information on the universe of point-of-sale transactions from a large panel of retailers. Overall, we find that stores in lower- (below-median) income ZIP codes show a high (2.7) elasticity of diaper sales with respect to sales tax rates, with smaller responses in high-income areas. Focusing on the four main changes to diaper tax exemptions in New York and Connecticut, we find the tax changes are almost completely passed through to consumers by retailers, leading to a 5.0% increase in the quantity of diapers sold at stores in low-income areas when diapers are tax-exempt. There is only a small and negative (-1.5%) change in sales for high-income areas, implying that low-income consumers are indeed more responsive to sales taxes on diapers.

We also investigate changes in spending on health-related products that may be associated with increased diaper purchasing or diaper need. The results are mixed: spending increases on both products that are associated with purchasing more diapers (baby powder and baby oil), and on products that may be associated with purchasing too few diapers (UTI pain medications and children's pain medications). These results are largely not driven by low-income households, suggesting that there may be underlying trends in purchasing that are associated with the sales tax exemptions. We further find significant spending increases on products that are unrelated to diaper purchases, providing further concern that our main results may not be entirely driven by the sales tax exemptions; still, our results are suggestive that taxing diapers has important spillovers on low-income households' purchasing decisions.

The structure of this paper is as follows: Section 2 provides more detailed policy context about the tax changes we analyze and describe current tax changes; Section 3 describes our methods, including the data and models; and Section 4 summarizes the results of the descriptive Consumer Panel analysis, price changes, and main findings from the Retail Scanner Data diaper sales analyses. Section 5 concludes.

3.2 Policy Context

Federal transfer programs such as the Supplemental Nutrition Assistance Program (SNAP) and the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) do not cover diapers with their benefits. Thus, low-income households potentially face liquidity constraints when buying diapers that are not addressed by current programs.

This paper focuses on changes in diaper taxes between 2006 and 2013. Six states exempted diapers from sales taxes prior to 2006: Massachusetts, Minnesota, New Jersey, Pennsylvania, Rhode Island, and Vermont (Weir, Smith, Vaynerman, and Rice 2014). Also, several states (Alaska, Delaware, Montana, New Hampshire, and Oregon) do not have sales taxes. These states did not change their tax treatment of diapers during the study period. Our first source of variation involves the remaining states, which experienced small changes in taxes on diapers due to variations in their regular sales tax rates.

Additionally, there were several larger changes to sales taxes on diapers in New York. First, diapers were included in an overall state sales tax exemption for clothing under $110 starting April 1, 2006. New York took the unusual step of allowing counties to decide whether local sales taxes would also exempt clothing; some did and some did not. The state sales tax rate in New York at the time was 4% and counties levied local sales taxes ranging from 0 to 5.5%. Additionally, New York repealed this exemption temporarily between October 1, 2010 and March 31, 2011 (with the phased ending of the exemption suspension announced at the time of implementation). The variation in county-level taxes this created is graphed in Figure 3.1, and shown as a map in Figure 3.2; there was considerable geographic heterogeneity in diaper taxes.

As of 2006, Connecticut exempted clothing purchases under $50 from sales tax, and diapers are categorized as clothing in their tax code. The clothing exemption was repealed effective July 1, 2011. This meant that diapers went from not being taxed to being taxed at the state rate of 6.35%. Connecticut would later pass a diaper-specific sales tax exemption in 2016 that went into effect July 1, 2018, but this occurred after the time period covered in this paper.

The New York and Connecticut tax changes applied to all clothing purchases under certain amounts, so the effects of the tax changes cannot be attributed to diapers alone. Annual spending by families in the lowest quintile of incomes on apparel averaged $845 in 2006 and $774 in 2010, meaning the income change from this channel should be relatively modest at $50-$60.[1] However, to the degree that the tax reduction generates income effects, our results will overstate the effects of diaper taxes alone on behavior. We address this issue through model

1. Figures from "Quintiles of income before taxes: Average annual expenditures and characteristics, Consumer Expenditure Survey" (https://www.bls.gov/cex/csxstnd.htm.)

specifications comparing stores in high- and low-income regions, which should help control for economic trends such as changing incomes.

Connecticut's diaper-specific exemption is part of a recent trend in states considering specific policies to exempt diapers from sales taxes, often in conjunction with exempting feminine hygiene products such as tampons. In 2017 alone, 18 states introduced bills that would eliminate or decrease sales taxes on diapers (Loughead 2018). A potential law that would add diapers to sales tax exemptions failed in California in 2016 after it was vetoed by the governor, but a later effort passed an exemption for 2020-2021. In 2020, Virginia lowered taxes on diapers and other hygiene products to the lower (2.5%) rate at which food is taxed. Washington D.C. eliminated sales taxes on diapers in October 2019; Louisiana, in 2021. Findings on earlier changes to diaper taxes could help inform debates about these current laws.

3.3 Methods

3.3.1 Data

Our first data source is the Nielsen Consumer Panel, a panel that tracks the purchases of 40,000-60,000 households. Participants use a home-scanner to scan all purchases, amounts, and trips for both food and non-food spending intended for in-home use. Demographic information about the panelists includes years of birth for children, race, geography, and lagged income categories. We use this purchasing data from 2004-2013 to illustrate diaper purchasing patterns by income at the household level, restricting the sample to household-years with a child born within the previous three years and at least one diaper purchase.[2]

As the sample size of the Consumer Panel is limited for small geographic areas, we also use the Retail Scanner Data to investigate diaper purchases. This data includes the universe of scanned point-of-sale retail transactions for a large sample of businesses, including about 50% of U.S. grocery and drug stores. Information is recorded on sales and average prices at

2. Since children with the birth year in that panel year are not reported until the following year, the sample is also restricted to households that are in the data the following year.

the store-UPC-week level, which we aggregate into information about product categories at the store-month level from 2006-2013. The main product category we focus on is disposable diapers, but we also use data on product categories that are complementary to diaper purchases, as well as on unrelated products to use as placebo tests. In addition to information on sales and average prices, we also collect information on the number of diapers sold as a raw count and by the number of packages (or units) sold, as well as the average package size and the amount of sales broken down into sales of generic (or storebrand) and branded diapers. These variables allow us to capture if exempting diapers from sales taxes allows households to purchase higher-quality diapers or to purchase in bulk. Our sample includes all stores with at least one sale of diapers in the study period.

While the size of the Retail Scanner Data provides considerable precision, one limitation is that the data contains information only on stores, without information on individual households purchasing at those stores. To focus on low-income households, we use the location of stores in low-income areas as a proxy for customer characteristics. The finest measure of geography provided in the Retail Scanner Data is county, but many counties are quite large with substantial heterogeneity in consumers' income within them. In our main specification, we proxy for store ZIP code using the trip-weighted modal ZIP code of households in the Consumer Panel who ever visit the store.[3] This requires that stores are ever visited by a Consumer Panel household, and so there are many fewer stores when relying on this method rather than when using store county. We define low-income stores as those whose ZIP code is in the bottom 50% of median household income as measured by the American Community Survey's 2005-2010 five-year estimate.

In the appendix, we also provide results defining low-income stores by their provided county, again measured by the American Community Survey's 2005-2010 five-year estimate. There are more low-income stores in this sample: 13.5% of the sample is defined as low-income, compared to 8.7% when using estimated stores' assigned ZIP codes. There are no counties

3. For a very simple example, suppose a store has three shoppers, A, B, and C. A and C live in ZIP code 92103 and each visit the store once, and B lives in ZIP code 92109 and visits the store three times. The store is assigned B's ZIP code, 92109.

classified as low-income in Connecticut, so the county-level results that rely on the binary sales tax exemption treatment only include variation from New York. Figure 3.3 displays the distribution of low-income counties across New York State (the main area where treatment varies in the sample); as the figure shows, the low-income counties are geographically diverse, including both the Bronx and more rural counties upstate.

To gather information on diaper sales tax exemptions, we consulted a report by the National Diaper Bank Network (Weir, Smith, Vaynerman, and Rice 2014), supplemented by tax rate notices in the state of New York. We also gathered information on sales tax rates by county over time from the Avalara AvaTax API (a commercial software product used by retailers to compute sales taxes due).

3.3.2 Models

When analyzing the consumer response to a sales tax change, it is important to consider how much of these tax changes are passed through to consumers. Diapers are a very concentrated market with two main producers (Procter & Gamble and Kimberly-Clark), but they are also often sold as a loss leader at stores (Neff 2006). Dellavigna and Gentzkow (2019) demonstrate that there are largely uniform prices across grocery store chains, so we might not expect large local price responses to local tax changes. In order to test whether prices changed at stores following the changes in tax exemption status, we run the following model in the Retail Scanner Data for each change to statewide tax exemptions of diapers:

$$\ln\left(p_{iwu}\right) = \alpha_i + \theta_w + \gamma_u + \beta \ \text{TaxChange}_{sw} + \varepsilon_{iwu} \tag{3.1}$$

where *TaxChange* indicates the first full week of the tax change through the rest of the calendar year, and p_{iwu} is price in store i in week w for UPC u (Universal Product Codes identify a specific brand, type, and count of a package of diapers). By having store, week, and UPC fixed effects, the coefficient of interest on *TaxChange* shows how prices at the UPC level changed after the change to the state diaper exemption policy.

To investigate the effects of sales taxes on diaper purchases in the Retail Scanner Data, we use a treatment intensity model exploiting spatial and temporal variation in the tax rates applied to diapers. This method has the advantage of using the most variation in treatment status, as it allows sales tax changes other than a full repeal to contribute to identification. We use the following model:

$$\ln(y_{it}) = \alpha_i + \theta_t + \beta \ln(1+\tau_{ct}) + \varepsilon_{it} \tag{3.2}$$
$$\iff \text{low}_{ct} = 1$$

where i indexes stores located in state s and county c, t indexes (monthly) time periods, y_{it} is an outcome, and τ_{ct} is the *ad valorem* tax rate on diapers in county c at time t. Outcomes include the count of diapers sold, units of diapers sold (packages or boxes), sales revenue from diapers, price per diaper, and price per unit. The coefficient of interest β thus captures the elasticity of the measure of interest with respect to the change in the diaper sales tax rate. This specification limits the sample to stores located in low-income ZIP codes (indicated by low_{ct}).

We also examine the relative response in low- and high-income areas with a triple-difference model,

$$\ln(y_{it}) = \alpha_i + \theta_t \cdot \text{low}_{at} + \gamma \ln(1+\text{Tax}_{ct}) + \beta \ln(1+\text{Tax}_{ct}) \cdot \text{low}_{at} + \varepsilon_{it} \tag{3.3}$$

where stores in all areas are included, and the tax variable and time fixed effects θ_t are interacted with the indicator low_{ct} for a store being located in a low-income ZIP code. The coefficient of interest β thus captures the change in elasticity for low-income stores relative to higher income stores in ZIP codes with a tax change.

To examine effects of a discrete change in exemption status (rather than marginal changes), we also use a difference-in-differences model,

$$\ln(y_{it}) = \alpha_i + \theta_t + \beta \text{ Notax}_{st} + \varepsilon_{it} \iff \text{low}_{ct} = 1 \tag{3.4}$$

where Notax_{st} is an indicator for whether state s has no sales tax on diapers at time t. As discussed above, only New York and Connecticut vary the exemption status of diapers during the study period, so identification of the coefficient of interest β comes from within-store changes in outcomes before and after the policy changes in those states. The sample is still limited to stores located in low income areas.

We use a triple-difference model analogous to Equation 3.3 to investigate distributional effects:

$$\ln(y_{it}) = \alpha_i + \theta_t + \theta_t \cdot \text{low}_{ct} + \gamma \text{ Notax}_{st} + \beta \text{ Notax}_{st} \cdot \text{low}_{ct} + \varepsilon_{it} \tag{3.5}$$

and to see how treatment effects vary over time, we use an event study model building on Equation 3.4,

$$\ln(y_{it}) = \alpha_i + \theta_t + \sum_{d=-q, d\neq -1}^{p} \beta_d \cdot I(t = e_s + d) + \varepsilon_{it} \iff -q \leq t \leq p \text{ and } \text{low}_{ct} = 1 \tag{3.6}$$

where the model includes indicators $I(\cdot)$ of time to the treatment event e_s, ranging from q periods before the event to p periods after, giving time varying coefficients β_d. There are two possible treatments in the policy context during our sample period: going from no sales tax exemption to having an exemption (New York in 2006 and 2011), or losing an existing exemption (New York in 2010 and Connecticut in 2011). We run separate models for the two cases.

We rely on Equation 3.4 and Equation 3.5 when estimating effects on related health products and on unrelated, placebo products. The continuous variation in sales taxes that the other equations rely on would affect spending on these items in their own right, rather than only through effects on diaper purchasing behavior. Instead, the binary specifications that consider sales tax exemptions for diapers should not impact spending on other products directly (save for through a very small income effect), since they do not imply any tax changes on these products. We can therefore interpret the results as spillovers from any spending changes on diapers.

3.4 Results

3.4.1 Differences By Household Income

First, we examine the Nielsen Consumer Panel for diaper purchasing patterns at the household level. Although there is insufficient power to analyze changes across the two tax changes of interest, the advantage of this data is the ability to look at diaper purchases by household income.

Descriptive statistics on demographics by income level are in Table 3.1. The average number of children three and under is relatively similar at 1.25 for households with income over \$35,000 and 1.27 for households with income under \$35,000.[4] This number is over one in both cases, so to facilitate interpretation, further results are divided by the number of children three and under in the household. The lower-income group has more members of racial minorities and lower levels of education for female heads of household.

The Consumer Panel data shows that low-income families buy fewer diapers per child than high-income families. Table 3.2 reports a t-test on the mean of yearly diaper quantity purchased. Higher-income families purchase nearly 1,000 diapers per child each year while lower income families purchase about 880; this difference is statistically significant. A further breakdown by more granular income levels is shown in Figure 3.4.[5] Diaper quantity monotonically increases

4. Child age is inferred from year of birth; this variable includes children with the concurrent year of birth and previous 3 calendar years.

5. These levels map to the stratified sampling of the Nielsen Consumer Panel.

with household income category. Yearly diaper spending is also lower for low-income households; Table 3.3 shows that households with yearly income over \$35,000 spend \$32.05 more per year than households with yearly income under \$35,000. We would expect our observed levels of yearly spending to be an underestimate for a year of a child's life because our sample restriction of a child born within the past 3 years depends on calendar year. A child could be born part way through the year, so diapers would not be needed until then, or a child could stop using diapers part way through the year, so diapers would not be needed after. Additionally, spending incorporates both price and quantity, so differences in spending could reflect a combination of lower quantities and cheaper options.

There is some seasonality in diaper purchases. Figure 3.5 shows monthly purchases by income level.[6] Although there are some differences by month, there are not drastically different monthly patterns by income.

3.4.2 Pricing and Passthrough

Looking at the effects of sales taxes on prices at the product level, results from the model of price passthrough (from Equation 3.1) appear in Table 3.4. Two of the tax changes, NY in 2006 and NY in 2010, show a small but statistically significant change in price. The signs on the coefficients are consistent with less than complete passthrough to consumers; following a sales tax decrease, prices slightly rose, and following a sales tax increase, prices slightly decreased. However, these changes are very small. The tax changes were 4% at minimum (because of the state tax changes, with additional 0-5.5% changes in county tax rates) and the responses in prices were both less than 0.6%. Neither tax change in 2011 was associated with a statistically significant change in prices. Because there was slightly less than perfect passthrough, our estimates for responses to the stated tax rates and changes could be considered underestimates of

6. The Nielsen panel years do not map perfectly to calendar years because of a cutoff in reporting the last Saturday of December, so only household-months with full coverage of a month are used. Household-months are counted from the first diaper purchase to the last diaper purchase, with any intervening months without diaper purchases assumed to be 0.

responses to overall prices.

3.4.3 Primary Regression Results

Turning now to the primary regression specification in the Retail Scanner Data, we measure elasticities of diaper sales outcomes with a treatment intensity model (as given in Equation 3.2). This method has the advantage of using all variation in tax rates that do not represent a complete repeal, including county variation in New York and changes to overall sales tax rates in other states. Table 3.5 shows results consistent with our hypothesis that low-income households will have elastic demand; the elasticity of diapers sales with respect to the tax rate is 2.7, indicating these sales are quite elastic. There is also a statistically significant relationship between the tax rate and the price of a diaper that contradicts our passthrough hypothesis, with higher taxes predicting higher prices. However, this tax increase is not present when considering unit sales of diapers (i.e. the price of a package of diapers, rather than the price of a package divided by the number of diapers it contains). The insignificant results for the effects on unit price and package size, as well as the similarly sized coefficients for sales of generic and branded diapers, are inconclusive in determining what drives this price increase. Table 3.A.1 shows that consumer demand is relatively less elastic when the sample is defined using stores in low-income counties. The effects on price are also insignificant. This sample likely includes more stores in high-income areas, and so the initial coefficients may be attenuated.

The difference-in-difference results limit the sample to stores in low-income areas, but we may expect a small response in stores in higher-income areas that still have some lower-income consumers (or cost-conscious higher-income consumers). The triple-difference results in Table 3.6 demonstrate just this. There are relatively inelastic elasticity estimates for the raw and unit counts of diapers, as well as for their sales, in high-income stores. The table also suggests that low-income households may be purchasing smaller packages of diapers relative to the baseline for high-income households, and also have substantially larger elasticity estimates for sales of branded diapers (on the order of 10 times larger for branded diapers but only 3.5 times larger for generic diapers). The effect on diaper price may be driven by the reduction in package size. The results are even starker when defining low-income stores by county in Table 3.A.2. There is no effect on the count of diapers sold or total sales in high-income stores, though there remains evidence of higher unit prices.

To examine discrete effects of tax changes, we consider the difference-in-differences model of Equation 2.1. This method reduces the amount of variation used because it considers only the changes in state-level exemptions in New York and Connecticut. However, these tax eliminations may be more salient for diaper purchasing than small sales tax changes that affect all goods. Table 3.7 shows a 5.0% increase in diapers sold in stores in low-income ZIP codes after diaper taxes are repealed. We obtain similar results when measuring the number of units sold or the amount of total dollar sales. Prices per diaper sold fall only slightly (1.4%), consistent with the results in Table 3.4 on nearly complete passthrough. The price of the average package sold rises by 0.7%, which is consistent with an increase in bulk purchasing, further shown by the rise in average package size by 1.7%. Unlike in the continuous model, households increase their purchasing on generic diapers more than on branded diapers. This could help to explain the price changes: individual diapers are cheaper, but households purchase larger packages and thus the unit price is marginally larger. Table 3.A.3 shows very similar results in statistical significance and magnitude when using county-level income measurements.

One potential concern is that because the difference-in-difference models compare stores in low-income areas across states, there may be underlying changes in New York or Connecticut over time that contaminate the results. Table 3.8 presents results of a triple-difference specification (Equation 3.5), and finds that in the states that changed their diaper tax exemptions, there is a small (1.5%) decrease in sales of diapers, along with a small (0.5%) fall in price per diaper. It is only for stores in low-income areas that sales increase, by 5.7% compared to the 1.5% reduction in sales in stores in higher-income areas. This supports the hypothesis that low-income consumers are more responsive to the diaper tax exemptions. These results hold when using county-level area income, as shown in Table 3.A.4.

3.4.4 Effects on Health-Related Products

We complement our analysis of how diaper tax exemptions affect diaper purchasing to investigate spending responses on related health products. Our finding that low-income households purchase more diapers when they are exempt from sales taxes may also allow households to spend less on products that are associated with afflictions like diaper rash, or to spend more on products that are used when changing a diaper such as baby powder or oil. Table 3.9 provides results. There are significant increases in purchases on baby powder and baby oil, which are likely complementary purchases to diapers. We additionally expected to find significant reductions in spending on products associated with diaper need, such as baby ointments, UTI pain medications, and children's pain medications. However, there are instead significant increases in spending on UTI pain medications and children's pain medications, and no statistically significant effect on spending on baby ointments. This is the first clue that there may be some bias in our binary treatment difference-in-differences specification. The effects in the specification using county-level income, shown in 3.A.5, are quite different, with a reduction in spending on children's pain medications the only strongly statistically significant effect. The results are very sensitive to how we classify store income, and thus are largely inconclusive.

The triple difference specification in Table 3.10 shows that the spending responses are driven by stores in higher-income areas, even though the bulk of the spending responses on diapers were driven by stores in lower-income areas. This holds when using county-level income measures as well, as shown in Table 3.A.6. Therefore these spending responses on health-related products are unlikely to be associated with any lower diaper need caused by sales tax exemptions for diapers, though the spending increases may in very small part be driven by the income effect of the tax exemption.

3.4.5 Event Study

To examine how the sales tax removal affects outcomes over time, Figure 3.6 shows an event study (as in Equation 3.6) of changes in diaper sales before and after statewide exemptions were applied in New York (in April 2006 and again in April 2011). Because the Retail Scanner data only begins in January 2006, we are only able to examine three months of pre-period observations. Diaper sales are slightly higher in New York in the months prior to the exemption, which does raise some concerns about parallel trends; however, the increase in sales after the tax exemption is higher in magnitude and appears to be sustained.

Figure 3.7 displays similar information for the times exemptions were ended (New York in 2010 and Connecticut in 2011).[7] In these cases, we see roughly parallel trends before the change (with treated areas having slightly lower diaper sales), and lower levels after the change (although results are noisy). Because the October 2010 change in New York was reversed 6 months later, the time period to observe effects is limited.

3.4.6 Robustness Checks

We also examine effects on sales of unrelated products in Table 3.11, expecting the sales tax exemption for diapers to have no effect on these goods save for the potential for an income effect. However, recall that income will only increase by an estimated \$50 or \$60 per year for the average household, so this income effect is likely indiscernible. This is not what we find. Effects are statistically significant at the 1% level and large in magnitude, ranging for a 2.6% increase in spending on sanitary napkins to a 19.5% increase in spending on alcohol (though coverage of alcohol sales in the Nielsen data is limited, making this result less reliable). The significant effects largely persist when defining store income by county, shown in Table 3.A.7. Effects for cigarettes and food are no longer significant, but the effects for sanitary napkins and tampons persist even though these products were not affected by the sales tax exemption.

7. Connecticut has no counties in the bottom half of median incomes, so all variation in the current specification comes from New York.

Moreover, these spending responses appear driven by low-income households, as shown in Table 3.12's triple difference specification. This is cause for concern that the main results for diaper purchases are driven by another associated factor, or that there is bias arising from using a difference-in-differences specification with variation in treatment timing. The results in 3.A.8 are somewhat reassuring with insignificant spending responses on all products for low-income households. Perhaps the variation in New York that this specification solely relies on is better at capturing how households respond to diaper sales tax exemptions, or the county-level income construction is more accurate than relying on proxies for store ZIP code. While our main results remain suggestive that exempting diapers from sales taxes allows low-income households to purchase more diapers, the mixed nature of our robustness results encourage further study before a conclusive policy implication is made.

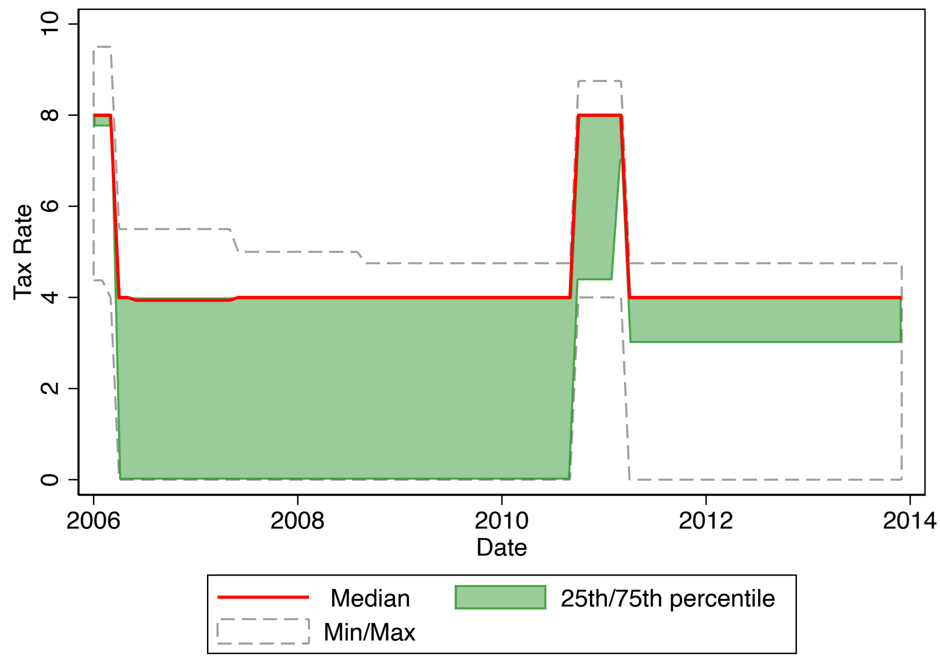
3.5 Conclusion

Since diapers are not covered under current federal aid programs and have potential health consequences for children, the policies surrounding regressive sales taxes on diapers are important to study. We find that consumers in low-income areas have a higher elasticity of demand for diapers than consumers in higher-income areas, and removing taxes on diapers allows low-income households to purchase more diapers. However, these tax changes are also associated with changes in spending on unrelated products, and there is some concern that an unrelated factor is driving part of the spending response that we find. While our findings have policy relevance for states that are currently considering adding diapers to their lists of tax-exempt products, future work may provide more conclusive evidence by getting exact location data from stores, or comparing purchases from households that use SNAP to those that do not.

3.6 Acknowledgments

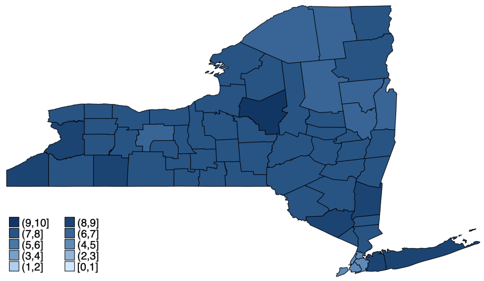
Chapter 3, “The Distributional Impacts of Taxes on Health Products: Evidence from Diaper Sales Tax Exemptions” is co-authored with Chelsea Swete and Kye Lippold, two former UCSD graduate students. The dissertation author was a primary author of this chapter. It is currently being prepared for submission for publication of the material. Researcher’s own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

3.7 Figures and Tables

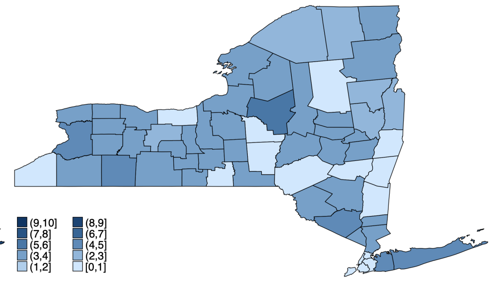


Source: New York Department of Taxation and Finance, Publication 718-C, various years.

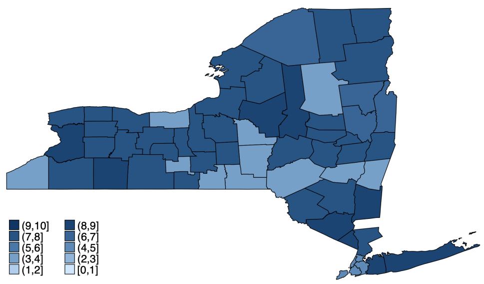
Figure 3.1. Distribution of County Tax Rates on Diapers in New York State over Time



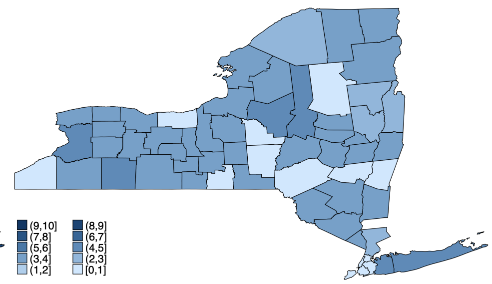
(a) Jan. 2006



(b) Apr. 2006



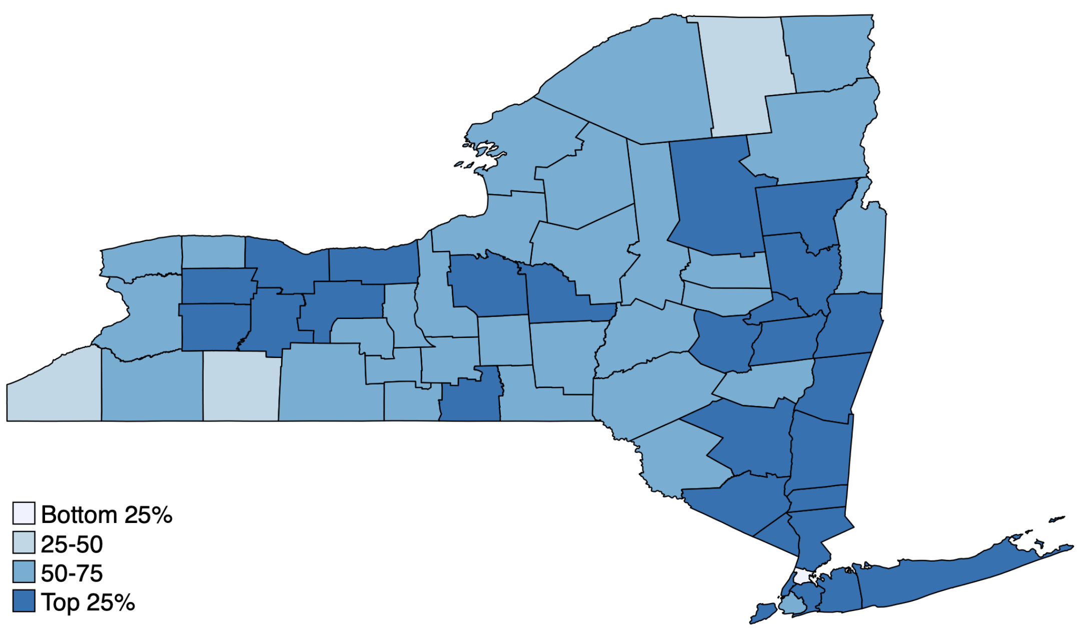
(c) Oct. 2010



(d) Apr. 2011

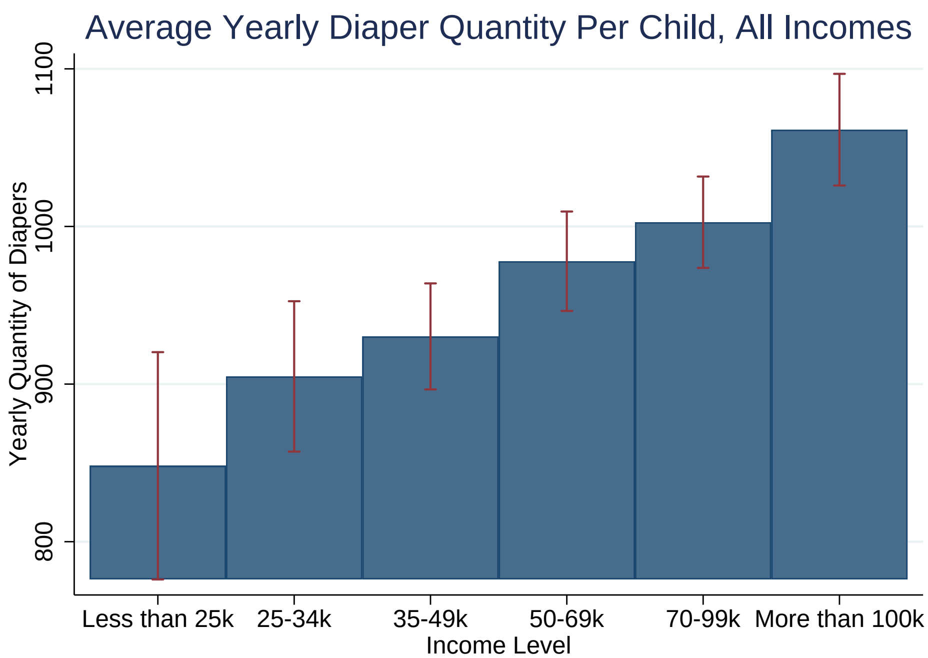
Source: New York Department of Taxation and Finance, Publication 718-C, various years.

Figure 3.2. Tax Rates by County in New York State over Time



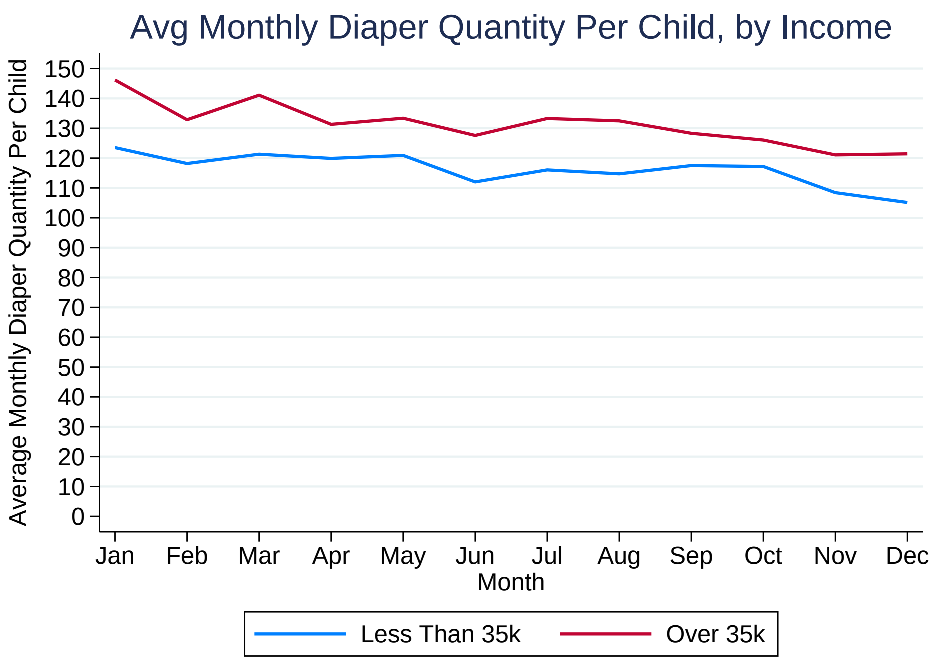
Source: American Community Survey, 2005-2010.

Figure 3.3. Counties in New York State by Quartile of Median Income



Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

Figure 3.4. Consumer Panel Yearly Quantity of Diapers Per Child, By Income Levels

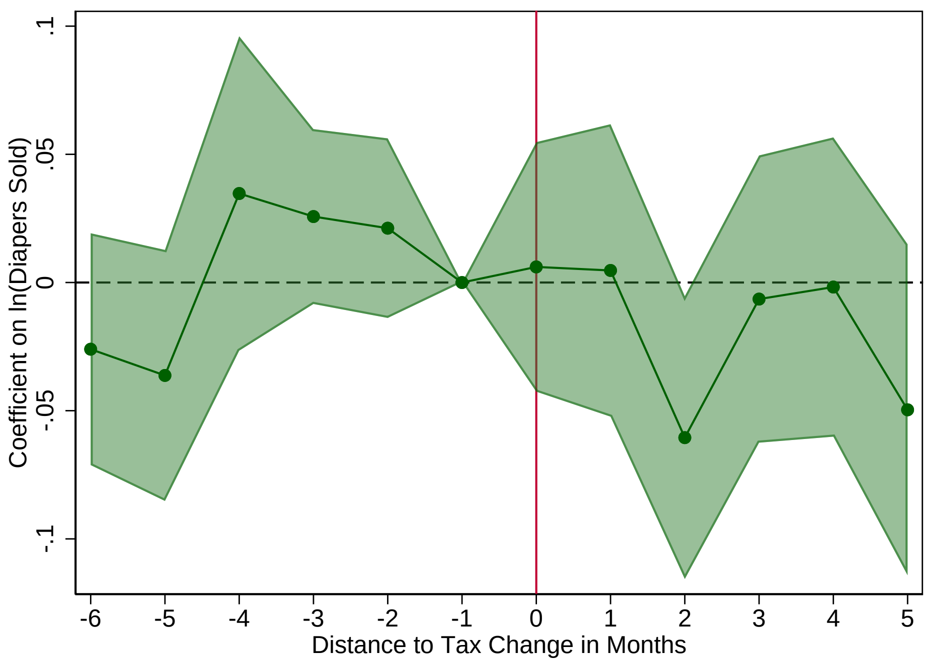


Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year and for whole month, that made at least 1 diaper purchase that year.

Figure 3.5. Consumer Panel Monthly Quantity of Diapers Per Child, By Income

Source: Nielsen Retail Scanner Data.
Notes: Graph depicts coefficients from a regression of diaper sales on treatment status (NY in April 2006 or April 2011). Shaded ares are 95% confidence intervals.

Figure 3.6. Event Study at Beginning of Exemption for Diapers



Source: Nielsen Retail Scanner Data.
Notes: Graph depicts coefficients from a regression of diaper sales on end of treatment status (NY in October 2010 or CT in July 2011). Shaded ares are 95% confidence intervals.

Figure 3.7. Event Study at End of Exemption for Diapers

Table 3.1. Descriptive Statistics By Income Level, Consumer Panel

	Mean Over 35k	**Mean Under 35k**
Num Children 3 & under	1.25	1.27
Race & Ethnicity		
White Non-Hispanic	0.76	0.72
Black	0.07	0.11
Asian	0.07	0.04
Other	0.06	0.09
Hispanic	0.09	0.11
Female Head Education		
Less Than HS	0.01	0.05
HS	0.10	0.25
Some Coll	0.22	0.32
College	0.48	0.34
Post College	0.20	0.06

Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

Table 3.2. Yearly Diaper Quantity Per Child Under 3, by Income Level

	Mean	**Std Err**	**Obs**
Over 35k	994.57	8.22	6071
Under 35k	879.89	21.13	1228
Difference	114.68	7.72	
Pr 2-sided T-test	0.000		

Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

Table 3.3. Yearly Diaper Spending Per Child Under 3, by Income Level

	Mean	**Std Err**	**Obs**
Over 35k	227.65	2.00	6071
Under 35k	195.60	4.67	1228
Difference	32.05	1.85	
Pr 2-sided T-test	0.000		

Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

Table 3.4. Prices After Tax Changes, UPC Level

	NY 2006 4-9.5% Tax Dec. Ln(Unit Price)	NY 2010 4-9.5% Tax Inc. Ln(Unit Price)	NY 2011 4-9.5% Tax Dec. Ln(Unit Price)	CT 2011 6.35% Tax Inc. Ln(Unit Price)
Tax Change	0.00587***	-0.00371***	0.0000361	-0.00188
	(0.000768)	(0.000948)	(0.00106)	(0.00270)
Constant	2.418***	2.436***	2.478***	2.476***
	(0.0000297)	(0.0000106)	(0.0000387)	(0.0000131)
N	59891011	64149230	64176220	61612013
Clusters	30947	33639	33428	31805

Source: Kilts-Nielsen Retail Scanner Data
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store, week, and UPC fixed effects. Tax change indicates the area has changed its sales tax on diapers by the column description. The sample for each regression contains the store-week-UPC sales for the whole year. The NY 2011 sample does not contain CT and the CT 2011 sample does not contain NY.

Table 3.5. Effects on Diaper Purchases in Low-Income ZIP Codes, Treatment Intensity Model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ln(Count)	Ln(Units)	Ln(Sales)	Ln(Count Price)	Ln(Unit Price)	Ln(Pkg Size)	Ln(Gen Sales)	Ln(Brand Sales)
Tax Rate	-2.9280***	-2.8429***	-2.7420***	0.1845**	0.0641	-0.0840	-2.2969***	-2.7047***
	(0.4310)	(0.4327)	(0.4243)	(0.0797)	(0.0992)	(0.1025)	(0.5950)	(0.4616)
Y mean	15632.16	320.86	4237.05	0.34	11.92	40.66	768.25	3506.03
N	201817	201817	201817	201817	201817	201817	192852	201639
Clusters	2424	2424	2424	2424	2424	2424	2381	2420

Source: Kilts-Nielsen Retail Scanner Data

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Tax rate is measured as $\ln(1+\tau_{ct})$. Sample is limited to stores whose predicted ZIP code has a below-median average income. Count is the number of diapers sold, units is the number of packages of diapers sold, sales is the total amount of sales on diapers, count price is the average price of a diaper, unit price is the average price of a package of diaper, package size is the number of diapers in each package, gen sales is the total amount of sales on generic or storebrand diapers, and brand sales is the total amount of sales on branded diapers. Y mean is the mean of the level of the dependent variable.

Table 3.6. Effects on Diaper Purchases in Low-Income ZIP Codes, Treatment Intensity Model, Triple Difference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ln(Count)	Ln(Units)	Ln(Sales)	Ln(Count Price)	Ln(Unit Price)	Ln(Pkg Size)	Ln(Gen Sales)	Ln(Brand Sales)
Tax Rate	-0.2696**	-0.5474***	-0.2854***	-0.0228	0.2524***	0.2875***	-0.5128***	-0.2478**
	(0.1088)	(0.1052)	(0.1094)	(0.0234)	(0.0325)	(0.0314)	(0.1799)	(0.1076)
$LI \times TR$	-2.7001***	-2.3298***	-2.4937***	0.2090**	-0.1917*	-0.3784***	-1.8010***	-2.4927***
	(0.4449)	(0.4453)	(0.4383)	(0.0828)	(0.1042)	(0.1070)	(0.6214)	(0.4739)
Y mean	20742.60	418.94	5658.53	0.35	11.96	39.87	39.95	39.88
N	2331356	2331356	2331356	2331356	2331356	2331356	2280535	2330433
Clusters	27593	27593	27593	27593	27593	27593	27323	27582

Source: Kilts-Nielsen Retail Scanner Data

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. $LI \times TR$ abbreviates low-income $\times$ tax rate. Tax rate is measured as $\ln(1+\tau_{ct})$. Count is the number of diapers sold, units is the number of packages of diapers sold, sales is the total amount of sales on diapers, count price is the average price of a diaper, unit price is the average price of a package of diaper, package size is the number of diapers in each package, gen sales is the total amount of sales on generic or storebrand diapers, and brand sales is the total amount of sales on branded diapers. Y mean is the mean of the level of the dependent variable.

Table 3.7. Effects on Diaper Purchases in Low-Income ZIP Codes, Difference in Differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ln(Count)	Ln(Units)	Ln(Sales)	Ln(Count Price)	Ln(Unit Price)	Ln(Pkg Size)	Ln(Gen Sales)	Ln(Brand Sales)
Treated	0.0497***	0.0349***	0.0375***	-0.0137***	0.0069*	0.0168***	0.0593***	0.0285**
	(0.0122)	(0.0110)	(0.0112)	(0.0037)	(0.0037)	(0.0041)	(0.0168)	(0.0121)
Y mean	15632.16	320.86	4237.05	0.34	11.92	40.66	768.25	3506.03
N	201817	201817	201817	201817	201817	201817	192852	201639
Clusters	2424	2424	2424	2424	2424	2424	2381	2420

Source: Kilts-Nielsen Retail Scanner Data

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. Sample is limited to stores whose predicted ZIP code has a below-median average income. Count is the number of diapers sold, units is the number of packages of diapers sold, sales is the total amount of sales on diapers, count price is the average price of a diaper, unit price is the average price of a package of diaper, package size is the number of diapers in each package, gen sales is the total amount of sales on generic or storebrand diapers, and brand sales is the total amount of sales on branded diapers. Y mean is the mean of the level of the dependent variable.

Table 3.8. Effects on Diaper Purchases in Low-Income ZIP Codes, Triple Difference

	(1) Ln(Count)	(2) Ln(Units)	(3) Ln(Sales)	(4) Ln(Count Price)	(5) Ln(Unit Price)	(6) Ln(Pkg Size)	(7) Ln(Gen Sales)	(8) Ln(Brand Sales)
Treated	-0.0110**	-0.0068	-0.0153***	-0.0048***	-0.0073***	-0.0049***	-0.0284***	-0.0117**
	(0.0054)	(0.0053)	(0.0056)	(0.0014)	(0.0017)	(0.0017)	(0.0096)	(0.0055)
T × LI	0.0641***	0.0445***	0.0556***	-0.0092**	0.0141***	0.0221***	0.0869***	0.0428***
	(0.0137)	(0.0124)	(0.0127)	(0.0040)	(0.0041)	(0.0044)	(0.0194)	(0.0135)
Y mean	20743.31	418.95	5659.63	0.35	11.96	39.87	963.89	4718.60
N	2335007	2335007	2335007	2335007	2335007	2335007	2284178	2334082
Clusters	27597	27597	27597	27597	27597	27597	27327	27586

Source: Kilts-Nielsen Retail Scanner Data

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. T × LI abbreviates treated × low-income. Sample is limited to stores whose predicted ZIP code has a below-median average income. Count is the number of diapers sold, units is the number of packages of diapers sold, sales is the total amount of sales on diapers, count price is the average price of a diaper, unit price is the average price of a package of diaper, package size is the number of diapers in each package, gen sales is the total amount of sales on generic or storebrand diapers, and brand sales is the total amount of sales on branded diapers. Y mean is the mean of the level of the dependent variable.

Table 3.9. Effects on Log Sales of Health Products in Low-Income ZIP Codes, Difference in Differences

	(1) Baby Powder	(2) Baby Oil	(3) Baby Ointments	(4) UTI Pain Meds	(5) Child Pain Meds
Treated	0.0324***	0.0402***	-0.0113	0.0359**	0.0476***
	(0.0114)	(0.0130)	(0.0112)	(0.0175)	(0.0117)
Y mean	71.18	56.96	151.06	59.17	384.94
N	200266	196033	199336	170375	201845
Clusters	2420	2418	2411	2285	2425

Source: Kilts-Nielsen Retail Scanner Data
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. Sample is limited to stores whose predicted ZIP code has a below-median average income. Y mean is the mean of the level of the dependent variable.

Table 3.10. Effects on Log Sales of Health Products in Low-Income ZIP Codes, Triple Difference

	(1) Baby Powder	(2) Baby Oil	(3) Baby Ointments	(4) UTI Pain Meds	(5) Child Pain Meds
Treated	0.0235***	0.0070	-0.0166***	0.0274***	0.0440***
	(0.0046)	(0.0058)	(0.0058)	(0.0065)	(0.0064)
T × LI	0.0094	0.0359**	0.0071	0.0078	0.0066
	(0.0123)	(0.0144)	(0.0127)	(0.0186)	(0.0135)
Y mean	96.82	73.71	219.83	68.01	533.38
N	2330160	2314104	2324875	2063854	2333753
Clusters	27597	27587	27555	26698	27611

Source: Kilts-Nielsen Retail Scanner Data
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. T × LI abbreviates treated × low-income. Y mean is the mean of the level of the dependent variable.

Table 3.11. Effects on Log Sales of Unrelated Products in Low-Income ZIP Codes, Difference in Differences

	(1) Sanitary Napkins	(2) Tampons	(3) Cigarettes	(4) Alcohol	(5) Food
Treated	0.0264^{***}	0.0435^{***}	0.0660^{***}	0.1951^{***}	0.0362^{***}
	(0.0095)	(0.0095)	(0.0149)	(0.0250)	(0.0096)
Y mean	1364.62	780.40	11096.40	20504.97	434765.37
N	202157	202133	202099	162024	202334
Clusters	2425	2425	2424	2386	2426

Source: Kilts-Nielsen Retail Scanner Data
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. Sample is limited to stores whose predicted ZIP code has a below-median average income. Y mean is the mean of the level of the dependent variable.

Table 3.12. Effects on Log Sales of Unrelated Products in Low-Income ZIP Codes, Triple Difference

	(1) Sanitary Napkins	(2) Tampons	(3) Cigarettes	(4) Alcohol	(5) Food
Treated	0.0064	0.0052	-0.0124	0.2627^{***}	-0.0252^{***}
	(0.0043)	(0.0048)	(0.0090)	(0.0118)	(0.0076)
Treated $\times$ Low Inc	0.0228^{**}	0.0422^{***}	0.0811^{***}	-0.0681^{**}	0.0678^{***}
	(0.0107)	(0.0111)	(0.0176)	(0.0275)	(0.0136)
Y mean	1884	1360	16114	46266	721836
N	2337011	2337064	2336822	1907259	2338288
Clusters	27613	27613	27615	27308	27623

Source: Kilts-Nielsen Retail Scanner Data
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. Y mean is the mean of the level of the dependent variable.

3.A Appendix Tables

Table 3.A.1. Effects on Diaper Purchases in Low-Income Counties, Treatment Intensity Model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ln(Count)	Ln(Units)	Ln(Sales)	Ln(Count Price)	Ln(Unit Price)	Ln(Pkg Size)	Ln(Gen Sales)	Ln(Brand Sales)
Tax Rate	-1.5610***	-1.5708***	-1.6018***	-0.0395	-0.0265	0.0236	-1.3647***	-1.5754***
	(0.3291)	(0.3333)	(0.3369)	(0.0527)	(0.0554)	(0.0602)	(0.4243)	(0.3699)
Y mean	9995.90	223.78	2655.48	0.31	10.46	38.22	607.54	2062.81
N	533919	533919	533919	533919	533919	533919	521684	533673
Clusters	6621	6621	6621	6621	6621	6621	6453	6614

Source: Kilts-Nielsen Retail Scanner Data

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Tax rate is measured as $\ln(1 + \tau_{ct})$. Sample is limited to stores in counties with below-median incomes. Count is the number of diapers sold, units is the number of packages of diapers sold, sales is the total amount of sales on diapers, count price is the average price of a diaper, unit price is the average price of a package of diaper, package size is the number of diapers in each package, gen sales is the total amount of sales on generic or storebrand diapers, and brand sales is the total amount of sales on branded diapers. Y mean is the mean of the level of the dependent variable.

Table 3.A.2. Effects on Diaper Purchases in Low-Income Counties, Treatment Intensity Model, Triple Difference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ln(Count)	Ln(Units)	Ln(Sales)	Ln(Count Price)	Ln(Unit Price)	Ln(Pkg Size)	Ln(Gen Sales)	Ln(Brand Sales)
Tax Rate	0.0065	-0.1969*	-0.0294	-0.0488**	0.1572***	0.2093***	-0.2139	-0.1051
	(0.1106)	(0.1070)	(0.1116)	(0.0207)	(0.0262)	(0.0253)	(0.1582)	(0.1100)
LI × TR	-1.5503***	-1.3566***	-1.5524***	0.0064	-0.1820***	-0.1858***	-1.0937**	-1.4666***
	(0.3450)	(0.3482)	(0.3528)	(0.0562)	(0.0611)	(0.0650)	(0.4540)	(0.3831)
Y mean	18891.13	366.65	5035.78	0.34	11.31	39.24	39.45	39.25
N	3092075	3092075	3092075	3092075	3092075	3092075	3019552	3090946
Clusters	38657	38657	38657	38657	38657	38657	37043	38640

Source: Kilts-Nielsen Retail Scanner Data

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. LI × TR abbreviates low-income × tax rate. Tax rate is measured as $\ln(1+\tau_{ct})$. Count is the number of diapers sold, units is the number of packages of diapers sold, sales is the total amount of sales on diapers, count price is the average price of a diaper, unit price is the average price of a package of diaper, package size is the number of diapers in each package, gen sales is the total amount of sales on generic or storebrand diapers, and brand sales is the total amount of sales on branded diapers. Y mean is the mean of the level of the dependent variable.

Table 3.A.3. Effects on Diaper Purchases in Low-Income Counties, Difference in Differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ln(Count)	Ln(Units)	Ln(Sales)	Ln(Count Price)	Ln(Unit Price)	Ln(Pkg Size)	Ln(Gen Sales)	Ln(Brand Sales)
Treated	0.0535***	0.0433***	0.0473***	-0.0076*	0.0073**	0.0122**	0.0688***	0.0334**
	(0.0146)	(0.0126)	(0.0137)	(0.0044)	(0.0037)	(0.0055)	(0.0221)	(0.0156)
Y mean	9995.90	223.78	2655.48	0.31	10.46	38.22	607.54	2062.81
N	533919	533919	533919	533919	533919	533919	521684	533673
Clusters	6621	6621	6621	6621	6621	6621	6453	6614

Source: Kilts-Nielsen Retail Scanner Data

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. Sample is limited to stores in counties with below-median incomes. Count is the number of diapers sold, units is the number of packages of diapers sold, sales is the total amount of sales on diapers, count price is the average price of a diaper, unit price is the average price of a package of diaper, package size is the number of diapers in each package, gen sales is the total amount of sales on generic or storebrand diapers, and brand sales is the total amount of sales on branded diapers. Y mean is the mean of the level of the dependent variable.

Table 3.A.4. Effects on Diaper Purchases in Low-Income Counties, Triple Difference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ln(Count)	Ln(Units)	Ln(Sales)	Ln(Count Price)	Ln(Unit Price)	Ln(Pkg Size)	Ln(Gen Sales)	Ln(Brand Sales)
Treated	-0.0111**	-0.0074	-0.0140**	-0.0039***	-0.0053***	-0.0042***	-0.0164*	-0.0054
	(0.0055)	(0.0053)	(0.0055)	(0.0012)	(0.0013)	(0.0013)	(0.0085)	(0.0051)
$T \times LI$	0.0565***	0.0413***	0.0532***	-0.0019	0.0142***	0.0175***	0.0679**	0.0334**
	(0.0158)	(0.0146)	(0.0153)	(0.0046)	(0.0040)	(0.0056)	(0.0286)	(0.0161)
Y mean	18905.82	366.76	5039.96	0.34	11.31	39.25	900.72	4161.85
N	3097203	3097203	3097203	3097203	3097203	3097203	3024672	3096072
Clusters	38671	38671	38671	38671	38671	38671	37057	38654

Source: Kilts-Nielsen Retail Scanner Data

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. $T \times LI$ abbreviates treated $\times$ low-income. Count is the number of diapers sold, units is the number of packages of diapers sold, sales is the total amount of sales on diapers, count price is the average price of a diaper, unit price is the average price of a package of diaper, package size is the number of diapers in each package, gen sales is the total amount of sales on generic or storebrand diapers, and brand sales is the total amount of sales on branded diapers. Y mean is the mean of the level of the dependent variable.

Table 3.A.5. Effects on Log Sales of Health Products in Low-Income Counties, Difference in Differences

	(1) Baby Powder	(2) Baby Oil	(3) Baby Ointments	(4) UTI Pain Meds	(5) Child Pain Meds
Treated	0.0316*	-0.0290	-0.0186	0.0012	-0.0623***
	(0.0178)	(0.0220)	(0.0187)	(0.0259)	(0.0177)
Y mean	43.68	36.26	82.99	54.92	233.81
N	530435	522658	523103	357289	544537
Clusters	6591	6511	6491	6190	6824

Source: Kilts-Nielsen Retail Scanner Data
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. Sample is limited to stores in counties with below-median incomes. Y mean is the mean of the level of the dependent variable.

Table 3.A.6. Effects on Log Sales of Health Products in Low-Income Counties, Triple Difference

	(1) Baby Powder	(2) Baby Oil	(3) Baby Ointments	(4) UTI Pain Meds	(5) Child Pain Meds
Treated	0.0129**	-0.0091*	-0.0232***	0.0244***	0.0311***
	(0.0065)	(0.0054)	(0.0054)	(0.0058)	(0.0070)
T × LI	0.0163	-0.0243	-0.0002	-0.0250	-0.0914***
	(0.0187)	(0.0232)	(0.0191)	(0.0259)	(0.0195)
Y mean	82.47	66.29	179.71	63.29	426.98
N	3082971	3054452	3051801	2343532	3139240
Clusters	38174	37494	37425	35738	39151

Source: Kilts-Nielsen Retail Scanner Data
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. T × LI abbreviates treated × low-income. Y mean is the mean of the level of the dependent variable.

Table 3.A.7. Effects on Log Sales of Unrelated Products in Low-Income Counties, Difference in Differences

	(1) Sanitary Napkins	(2) Tampons	(3) Cigarettes	(4) Alcohol	(5) Food
Treated	0.0308**	0.0282**	0.0060	0.2195***	0.0147
	(0.0127)	(0.0120)	(0.0263)	(0.0300)	(0.0133)
Y mean	867.88	541.12	9767.97	20500.95	303042.04
N	551445	553662	555468	368511	555724
Clusters	6834	6834	6852	6718	6853

Source: Kilts-Nielsen Retail Scanner Data
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. Sample is limited to stores in counties with below-median incomes. Y mean is the mean of the level of the dependent variable.

Table 3.A.8. Effects on Log Sales of Unrelated Products in Low-Income Counties, Triple Difference

	(1) Sanitary Napkins	(2) Tampons	(3) Cigarettes	(4) Alcohol	(5) Food
Treated	0.0065*	0.0132***	0.0115	0.2200***	-0.0147**
	(0.0039)	(0.0045)	(0.0118)	(0.0108)	(0.0061)
Treated × Low Inc	0.0132	0.0058	-0.0174	-0.0213	0.0180
	(0.0157)	(0.0142)	(0.0301)	(0.0391)	(0.0173)
Y mean	1616	1102	14861	41947	549429
N	3206339	3223652	3262380	2345075	3264066
Clusters	39513	39526	40064	38941	40079

Source: Kilts-Nielsen Retail Scanner Data
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered by store. All models include store and month-year fixed effects. Treated indicates area has no sales tax on diapers in effect. Y mean is the mean of the level of the dependent variable.

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