The Impact of Increased SNAP Benefits on Childhood Obesity: Examining the Natural Experiment Resulting from the 2009-2013 ARRA SNAP Benefit Increase

A thesis submitted for partial fulfillment of the requirements for a Bachelor of Arts degree in Public Policy by Elena Jáuregui.

ABSTRACT

Childhood obesity is a public health concern of epidemic proportions in the United States. The US federal and state governments have numerous programs in place to improve nutritional and health outcomes; these initiatives are often intertwined with poverty assistance. One such program is SNAP (the Supplemental Nutrition Action Program), which is meant to provide the necessary supplementary income for families at or below 130% of the federal poverty threshold to afford a nutritious diet. A 2009-2013 increase of 13.6% in SNAP benefits as part of the ARRA (American Recovery and Reinvestment Act) provided a perfect natural experiment with which to investigate a potential causal relationship between the amount of SNAP benefits and childhood obesity prevalence in low-income populations. Using 2003-2016 data from NHANES (the National Health and Nutrition Examination Survey), I calculated cross sectional childhood obesity prevalence for each NHANES cycle in children and adolescents ages 2-19. Using these statistics for SNAP recipients and a near-eligible comparison population with subgroups by age, gender, and race/ethnicity, I ran difference-in-differences analyses in both R and Stata to examine the impact of the ARRA SNAP increase on childhood obesity rates. Trend differences between SNAP recipients and the comparison population in the overall 2-19 age group were not statistically significant. In subgroup analyses, the regressions of boys' data and the 4-10 age group were the only two which approached statistical significance: in these groups, SNAP recipients were less likely to be obese than their non-SNAP low-income counterparts. In fact, for all but the 2-4 age group, I found SNAP participation to be negatively correlated with obesity prevalence. However, none of these relationships were statistically significant (not even in boys or the 4-10 age group), with many having p-values above 0.5. Given these results, I conclude that while there are indications that the ARRA SNAP benefit increase may have had positive effects on childhood obesity in certain groups, the high level of standard error in these data prevent such interpretation. I conclude that the ARRA SNAP benefit increase did not impact obesity rates in children ages 2-19 to a statistically significant level, a result which held for all age, gender, and racial/ethnic groups analyzed. As such, my policy recommendations involved investment in research regarding the expansion of SNAP-Ed and the indefinite extension of the COVID-19 P-EBT.

ACKNOWLEDGEMENTS

Many thanks to Kelsey Berryman, the understanding and knowledgeable preceptor who guided us through this process even in the midst of a pandemic. You have been incredible! Additional thanks to my faculty mentor Professor Diane Lauderdale, whose suggestions in the beginning of the process in finding a dataset were immeasurably helpful.

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INTRODUCTION

Childhood obesity is a public health concern of epidemic proportions in the United States (CDC. "The Childhood Obesity Epidemic). As of 2019, about 17% of US children and adolescents were classified as obese (*Obesity Prevention Policies*); these rates have been on the rise, doubling in children and tripling in adolescents over the last 30 years (*Childhood and Adolescent Obesity in the United States*).

US federal and state governments have numerous programs in place to improve nutritional and health outcomes. However, childhood obesity intervention research has largely focused on school-based behavioral policies such as regulating snack food and beverages, afterschool PE programs, and health education programming (*Obesity Prevention Policies*); economic policies which might be expected to impact childhood obesity on a structural level have not been explored as deeply. A World Health Assembly brief on fighting childhood obesity advocated a more holistic approach, including: addressing early life exposures to improve growth patterns, improving community understanding and social norms, addressing the exposure of children to the marketing of foods, influencing the food system and food environment, and improving nutrition in neighborhoods (Rincon 2014). As focused as the recent literature has been on school-based interventions, there is a relative paucity of research regarding the impact of economic interventions—such as financial support programs--in the development and progression of childhood obesity.

In this paper, I examine how the 2009-2013 ARRA increase in SNAP benefits (via an increase of 13.6% in the TFP) impacted childhood obesity rates (ages 2-19) within the population of SNAP recipients. Through this investigation, I hope to determine whether an increase in the monetary benefits of a supplementary program providing for basic needs can impact childhood obesity rates; that is, what the 2009-2013 SNAP increase can teach us about the relationship between the provision of funds to struggling families and the development or maintenance of childhood obesity in these low-income populations. Given the complexity of both childhood obesity's etiology and the economic incentives involved in SNAP, a quantitative approach is necessary to determine the direction, magnitude, and significant of this relationship. I use cross-sectional, individually identified data from NHANES (the National Health and Nutrition Examination Survey) to compare the rates of childhood obesity longitudinally in SNAP participants and a comparison low-income population from 2003 to 2016. Using these point estimates as well as individual level childhood obesity data for children and adolescents ages 2-19, I perform difference-in-differences analyses to determine to what extent differences in trends between these two groups can be attributed to the 2009-2013 ARRA SNAP benefit increase. I perform additional subgroup analyses by age, gender, and race/ethnicity to examine trend differences by these main demographic factors. The regression results in the overall data, as well as for all but one of the subgroups, show negative interaction coefficients of relatively large magnitude, pointing towards a protective effect of the ARRA increase against childhood obesity; however, none of these effects are statistically significant. I apply these and previous results to the current public health landscape in making policy recommendations.

BACKGROUND

Childhood Obesity in the US

Childhood obesity is an important public health issue for the US. Furthermore, studies examining decades of national health survey data suggest that the worst of the trend of rising BMIs is a fairly recent phenomenon (von Hippel, 2013). The male BMI distribution was stable from birth year 1930 until 1970, after which overweight and obesity began their meteoric rise; the same is roughly true for girls, whose obesity increase began around 1980 (von Hippel, 2013). Between the 1980s and 2000s, the prevalence of obesity rose from below 5% to over 10% in children, while the prevalence of overweight grew from 15% to over 30% (von Hippel, 2013). Today, about 17% of US children are obese, with minorities--black adolescent girls and Mexican-American adolescent boys, in particular--disproportionately affected ("The Nation's Childhood Obesity Epidemic"). These rising rates have been largely attributed to a confluence of factors which have produced an obesogenic environment, where inactivity and overeating are the norm ("The Nation's Childhood Obesity Epidemic"). Americans have changed the types and amounts of foods they eat, diminished physical activity, and switched over to more passive leisure activities ("The Nation's Childhood Obesity Epidemic").

In the 1960s, only 21% of a family's food budget was spent eating out; in 2008, it had risen to 42% (Jacobs & Shipp, 1990, U.S. Bureau of Labor Statistics, 2011). The fast food industry in particular has exploded, and disproportionately markets to ethnic minority youth (Harris, Schwartz, & Brownell, 2010). Racial and income disparities in obesity have further been heightened by the emergence of urban food deserts, where a lack of transportation and an unwillingness of food retailers to enter unappealing markets create barriers to accessing healthful foods in low-income neighborhoods (Hilmers, 2012). With the growth of suburbs in the second half of the twentieth century, commuters have turned to cars for transportation (rather than walking, etc.); leisure activities have become more sedentary. Video games, cell phones, computers, and other stationary means of entertainment have also grown exponentially since the 1970s and 80s (“The Nation's Childhood Obesity Epidemic”). These lifestyle habits, and many others, have helped foster the current state of widespread childhood obesity in the US.

This rise in obesity prevalence carries health consequences for affected individuals. In addition to being associated with numerous comorbidities ranging from hypertension to sleep apnea, obesity’s negative psychological impact (lowered self-esteem, depression, etc) on children has been well documented (Rincon). Childhood obesity increases the risk of adult obesity in a given individual, the development of which is associated with lower age onset of noncommunicable diseases (i.e. chronic diseases which are not passed from person to person, such as diabetes) (“WHO | Why Does Childhood Overweight and Obesity Matter?”). Disturbingly, the burden of this condition is not evenly distributed. Prevalence of obesity among non-Hispanic black (22%) and Hispanic (25.8%) children and adolescents is higher than in non-Hispanic white (14.1%) and non-Hispanic Asian (11%) populations of the same age (2-19 years), and lower socioeconomic groups experience significantly higher levels of childhood obesity than do wealthier cohorts (*Childhood and Adolescent Obesity*). In addition to the detrimental effects of obesity on quality of life and health, the social burden of this disease is substantial: the annual cost to the US health-care system was estimated to be as high as $147 billion in 2012, with 23% of that sum covered by Medicare (*State- and Payer-Specific Estimates*, Finkelstein). Considering that the prevalence of obesity has continued its meteoric rise since 2012, we can conclude that American taxpayers are not only suffering higher rates of obesity and its comorbidities, but are shouldering greater personal and national health costs. Childhood obesity’s upward trend threatens to worsen these issues.

Previous studies have determined childhood obesity to be an extremely complex condition with multiple interacting risk factors: genetics, gene-environment interactions, epigenetic modifications, nutrition and diet, physical activity, sleep, and stress have all been implicated (Brown). Recommended interventions are similarly multifaceted, and vary based on developmental stage. For example: for ages 4-12, the literature offers that considering technological interventions may be wise, as well as implementing at least 60 minutes of moderate to vigorous physical activity daily; for ages 13-18, peer group involvement is also recommended (Brown). Despite the complexity of this condition, studies of treatments in children have often limited their scope to interventions delivered in schools—failing to address research findings that children with overweight and obesity actually gain more weight during the summer than during the school year, which would suggest that such interventions might be insufficient (Brown). As a result, despite the significant number of studies regarding childhood obesity interventions, there is a gap in the literature regarding the impact of economic programs which operate outside of schools.

SNAP

One such program which has a profound day-to-day impact on the lives of many low-income children is SNAP, the Supplemental Nutrition Assistance Program. SNAP provides monthly benefits which allow participants to purchase food items for use at home; the amount of money provided is based on the "Thrifty Food Plan", which is adjusted for CPI (Consumer Price Index, i.e. inflation), cost-of-living, age-sex groups, family size, and economies of scale in household food purchases (Caswell 2013). Within these constraints, the TFP is meant to provide the necessary supplementary income to afford "an assortment of foods that represents as little change from average food consumption of families with relatively low food costs as required to provide a nutritious diet, while controlling for cost" (90th Congress 1977). Families use their SNAP benefits via an Electronic Benefits Transfer (EBT) account, which is debited to reimburse SNAP authorized retail stores when a participant purchases food at a participating store (*"What is EBT?")*. Eligibility for SNAP generally requires passing two income tests and two asset tests; a primary criterion is the gross income test, which requires that gross income be less than 130% of the federal poverty line (Caswell 2013). However, categorical eligibility is available to most recipients of Temporary Assistance for Needy Families (TANF), Supplemental Security Income (SSI), and General Assistance; these groups are eligible regardless of income or assets (Caswell 2013). Over two-thirds of SNAP participants are part of families with children, while a third are in households with seniors or people with disabilities ("Policy Basics"). Given SNAP's month-to-month basis, continual benefit adjustments, and eligibility based on PIR (Poverty Income Ratio), it is considered to be the second most responsive federal program in an economic downtown—ranked below unemployment insurance ("Policy Basics").

2009 ARRA SNAP Benefit Increase

Prior to the COVID-19 (SARS CoV-2) pandemic, the most recent major economic crisis in the US was the Great Recession. Even with the built-in responsiveness of the SNAP program, President Obama's administration saw a need to expand SNAP as part of its recession response package. The American Recovery and Reinvestment Act (ARRA) of 2009 included a temporary expansion of SNAP benefits: from April 2009 to November 2013, household SNAP benefits were increased by 13.6%, stemming from a 13.6% increase in the TFP (Morrissey 2020). This created a natural experiment for researchers seeking to compare outcomes among families with different SNAP benefit levels. For the purposes of this paper, the ARRA SNAP benefit increase provided a unique opportunity to track how SNAP benefit levels impact childhood obesity rates.

LITERATURE REVIEW

THEORETICAL FRAMEWORK

To understand how the ARRA SNAP increase could plausibly impact childhood obesity, it is crucial to look some of the multiple mechanisms by which obesity is induced, maintained, and exacerbated in children. Genetics, peer groups, behaviors, health knowledge, parental choices and modeling, environmental constraints, preferences, and advertising are some of the many factors which have been linked to obesity (Sahoo 2015). While the majority of these factors are unimpacted by the level of a monetary benefit, there do exist paths by which a SNAP increase could have an effect. In particular, a mechanism by which SNAP could reasonably operate to impact childhood obesity is by easing economic constraints on food purchases.

It is important to acknowledge that the overall impact of the program on this health outcome could be significantly attenuated by factors which lie entirely outside the realm of policy. At its most fundamental level, obesity results from an imbalance of energy intake and expenditure (Sahoo 2015). Energy expenditure—i.e. choice of leisure activities, physical activity, mode of transportation, frequency of exercise, etc.—is determined independent of SNAP benefits. Equally unrelated to policy is input from genetics: the fact that individuals respond differently to similar obesogenic stimuli is a simple demonstration of genetic factors in childhood obesity (Sahoo 2015). For example, a low basal metabolic rate (genetically determined) increases susceptibility to gaining weight (Sahoo 15). The field of epigenetics has also begun to explore how genetic variants and environmental exposures (gene-environment interactions) induce changes in DNA methylation, impacting gene transcription in ways which can predispose an individual to obesity and related comorbidities (Samblas 2019). Knowledge about health—which foods are healthy, which behaviors are necessary to stave off obesity, why gaining weight can be harmful, etc.—is also independent of monetary SNAP benefit levels; so are individual preferences for unhealthy foods and advertising schemes.

Despite all this, there are multiple ways in which SNAP benefits can induce change in children's food intakes and eating behavior. One such mechanism is the substitution effect: providing SNAP benefits for the purchase of groceries for at-home cooking lowers the relative price of at-home cooking compared to eating out, which is generally the unhealthier option. There is also an income effect: assuming that healthy groceries are a normal good (i.e. are demanded more as a person's income rises), the increase in SNAP benefits would correspond to an increase in healthy food purchases. A sample of Chicago households in the Study of Household Purchasing Patterns, Eating, and Recreation (SHOPPER) found that, compared to higher income households (after controlling for education, marital status, and race), lower income households had significantly lower Healthy Eating Index (HEI) scores, lower total vegetable scores, and lower dairy scores (French 2019). Given that healthier diets can cost up to $1.54/2000kcal more than less healthy options (Rao 2013), it is a reasonable hypothesis that the increasing SNAP benefits would provide low-income parents with the option to purchase healthier foods than they could have pre-increase. However, it is unclear whether the increase in income would lead to an increase in the quality or merely the quantity of foods; depending on whether recipients choose to buy healthier foods or a greater quantity of unhealthy food, the increase in SNAP benefits could either combat or contribute to an obesogenic environment.

An interesting idea presented in a paper by Hudak is SNAP benefits could be subject to quasi-hyperbolic discounting—that is, the natural human tendency to weight present gratification over future utility could lead SNAP recipient households to overspend in the first part of the month (since benefits are received monthly), leaving themselves restricted and left to buy cheaper, less healthy food at the end of the month (Hudak 2021). This pattern has been termed the “food stamp cycle”, and is linked to weight gain; a previous study by Todd (2015) found that the ARRA benefit increase smoothed this cycle, indicating that the benefit increase was protective against weight gain due to quasi-hyperbolic discounting.

A final mechanism to consider is the strong impact that social norms of obesity can have on youth. Children and adolescents whose social networks (i.e. parents and friends) lean more towards overweight have been shown to underestimate their own weight more frequently, as well as to develop inaccurate perceptions of what constitutes a healthy weight (Salvy 2014). Given that low-income zip codes are clustered together, and that public schools are usually structured by zip code, we can conclude that low-income children in SNAP are more likely to interact with each other than would be expected by chance. As a result, impacts of SNAP on eating habits could be amplified by these social network effects--where friends see the weight or meals of their other friends and are moved in the same healthy direction, as part of a positive feedback loop. Similarly, parents in the same SNAP household who now might consume healthier food could positively influence the eating habits (and, consequently, weights) of their children.

It is not guaranteed that the shift towards healthier food purchases would happen as a result of SNAP benefit increases; given that parental food preferences and time constraints are not changed by the SNAP policy, and that lack of access to healthy food in food deserts is not mitigated by extra spending money, it cannot be guaranteed with any level of certainty. However, the possibility introduces a plausible theory as to how SNAP benefits could influence childhood obesity.

PREVIOUS RESEARCH

There is a decent body of research looking at associative relationships between SNAP and obesity, as well as a separate group of studies which seized on the ARRA SNAP increase natural experiment to look at how the SNAP benefit amount impacts food security, healthcare utilization, and more. A study performed an intent-to-treat analysis on both overweight and obesity with NHANES data, but used different criteria for obesity and did not examine SNAP enrollees specifically: as such, my paper fills a gap in the literature by looking at the impact of increased benefits on childhood obesity in various demographics for SNAP enrollees specifically, using the WHO classifications of obesity.

SNAP and Obesity

The relationship between SNAP participation and obesity is muddled, perhaps varying by racial/ethnic group or between adults and children. A 2015 study of a nationally representative sample (NHANES—the same dataset I use in this paper) investigated the relationship between

SNAP, food security, dietary quality, and obesity among US adults (Nguyen 2015). Researchers found that SNAP participants with marginal to very low food security had better diets than those with similar food insecurity who were not enrolled in SNAP; additionally, SNAP enrollees were found to have lower BMI and lower probability of obesity, indicating that SNAP participation was protective against low dietary quality and high BMI in populations with marginal, low, or very low food security (Nguyen 2015). These benefits were disproportionately enjoyed by whites, compared to black and Hispanic participants in the survey (Nguyen 2015); the authors posited that neighborhood disparities in access to healthy food may account for these differences.

A similar association study by Leung et al. (also using NHANES) examined whether dietary quality and obesity differed by participation in SNAP, specifically looking at low-income children from 1998-2008 (Leung 2013). They found that, regardless of SNAP participation, low-income children were below national recommendations for whole grains, fruits, vegetables, fish, and potassium, while exceeding recommended limits for processed meat, sugar-sweetened beverages, saturated fat, and sodium (Leung, 2013). Stunningly, zero percent of low-income children met at least 7 of 10 dietary recommendations, and SNAP participants did not show any significant differences total energy, macronutrients, or Healthy Eating Index scores (Leung 2013).

A 2018 study sought to move beyond associative studies, adopting a novel approach in examining the impact of SNAP benefits at the intensive margin for causal interpretation (Almada 2018). Cleverly, they used the proportion of school-age children eligible for free in-school meals (which are not included in SNAP benefit calculations) as a proxy for exogenous increases in the amount of SNAP benefits available per adult (Alamada 2018). Their examination of data from the National Longitudinal Survey of Youth-1979 using this method found that additional SNAP benefits reduced BMI and the probability of being obese for SNAP adults (Alamada 2018). This result makes a well-argued case for the causal benefit of SNAP amounts on adult obesity.

Taken together, these studies indicate that SNAP participation may improve adult diet quality and BMI outcomes for those with the option to purchase healthier food. However, for children, SNAP does not appear to be associated with healthy eating habits or improved health outcomes. This presents a puzzling picture which is further confounded by the fact that Nguyen and Leung's studies cannot be interpreted causally. As a result, the relationship between SNAP enrollment and obesity (particularly child obesity) remains unclear in the literature.

2009-2013 ARRA SNAP Increase

Research on the impact of the 2009-2013 ARRA SNAP benefit increase has been performed on a wide range of topics. One study found that SNAP participation was associated with a reduced likelihood of hospitalization in older adults dually enrolled in Medicare and Medicaid (Samuel 2018); another, also in the healthcare sphere, found that the increase in SNAP benefits was associated with a 65% reduction in outstanding medical needs due to affordability among SNAP-eligible children, relative to low-income, ineligible children (Morrissey 2020). The majority of research, however, has not focused on medical expenses; instead, there is a wide body of literature regarding the impact of the ARRA increase on food security and health outcomes. Its effects on food security are unclear, while a single study examining its relationship with obesity also found mixed results by age group and gender.

Research by the USDA, published by Nord and Prell in 2011, found that food security of income-eligible SNAP households improved from 2008-2009; the researchers posited that a “substantial share” of that improvement could be attributed to the ARRA increase in SNAP benefits. As evidence of this, they noted that food security did not increase for households only a little above the SNAP eligibility level (Nord and Prell 2011). Given that food insecurity has been associated with preference for energy-dense foods (Seligman 2010), the increase in food security was a promising sign for corresponding BMI measures.

Yet the USDA results were not always replicated: another NHANES study (using waves from 2007-2012) found, using a difference-in-differences framework, that the ARRA SNAP benefit increase was not significantly associated with food security (Hudak 2021). Furthermore, the direct link between food security and BMI is itself tenuous; the same study noted that measures of dietary quality did not significantly change from the pre-ARRA period to the ARRA period, a result which did not differ by age range (Hudak 2021). This result is important to highlight, given two of my proposed mechanisms by which SNAP could positively impact childhood obesity: if dietary quality is not improved by the SNAP benefit increase, it throws doubt on the potential benefits implied by the substitution or income effects.

In addition to these studies examining the link between the ARRA and food security, a 2021 paper by Hudak and Racine directly investigated the impact of the SNAP benefit increase on overweight and obesity (using CDC criteria) in SNAP-eligible children (Hudak and Racine 2021). They generally found that weight outcomes in SNAP eligible youth did not change as a result of the SNAP benefit increase. Their intent-to-treat analysis found an inconsistent relationship between the ARRA increase and weight in children and adolescents by age group and gender, with very few of those results attaining statistical significance; those which did hinted that weight outcomes may have improved in some age groups.

The literature surrounding the ARRA SNAP increase is muddled, unable to clearly demonstrate its effects on food security or childhood obesity with a high degree of certainty. In the analyses which follow, I attempt to help clarify the direction and magnitude of this relationship with difference-in-differences analyses designed to calculate the impact of the benefit increase on childhood obesity in SNAP enrollees specifically.

DATA METHODS & ANALYSIS

Data: NHANES

For my research I selected the National Health and Examination Survey (NHANES). NHANES is a continuous, nationally representative, repeated cross-sectional survey which is designed to assess the health and nutritional status of adults and children in the US (*NHANES* 2020). More accurately, it is a collection of studies under the NHANES umbrella: data collected range from measurements of anemia and hearing loss to questions about citizenship status and STI status (*NHANES* 2020) Participants are selected through a complex four-stage sampling design from 15 locations around the country. Data is collected throughout the year, but publicly released every in two-year cycles. About 5,000 individuals of all ages are surveyed each year; Hispanic, black, Asian, low-income (<=130% of the federal poverty level), and elderly (>80 years of age) populations are all oversampled to provide more precise and stable estimates of health parameters. Information is gathered through both an interview and a physical examination performed by experts. This is one of the key differentiating aspects of NHANES; other surveys often rely on self-reported health metrics, which are prone to bias (Ng 2019). As a result, NHANES is widely considered the "gold standard" for nationally representative obesity metrics in the US population. (*Read "Assessing")*

NHANES: Variables Chosen for Analysis

Given NHANES' cross-sectional nature, I decided to calculate cross-sectional point estimates of childhood obesity prevalence in each two-year cycle of the survey in relevant demographic groups. This approach required variables for SNAP status, age, gender, income, and obesity, as well an identifying number for each individual whose data I would be working with. I found appropriate variables to satisfy these requirements by combining three of NHANES' multiple datasets: the Food Security portion of the Questionnaire, the Body Measures portion of the Examination Data, and the Demographics dataset. For each two-year cycle from 2003 to 2016 (2003-2004, 2005-2006, etc.), I downloaded these three datasets into Stata/SE 16.0 and merged them on the id variable (i.e. the identifier variable). The 2017-2018 cycle, despite being the most recent round of publicly available and complete data, was excluded from analyses because it lacked the Food Security module.

To make the most of NHANES' sampling structure, I deemed it prudent to use the survey commands in Stata for calculating relevant means. To do so, I used svyset to apply strata (sdmvstra) and primary sampling units (sdmvpsu) from the Demographics dataset, with weights (wtmec2yr) from the Examination data. I used the Examination data sample weights rather than those from the Demographic data to reflect the fact that all individuals with examination data also had demographic data, while the reverse did not hold true—i.e., not all individuals with demographic data were also in the Examination dataset. Guidance from the CDC advises that a "good rule of thumb" is to use the "least common denominator", i.e. the sample weights where the variable of interest was collected on the smallest number of respondents ([NHANES]: Analytic Guidelines). Given that my analyses require BMI, it was preferable to use the examination sample weights.

To avoid grouping infant obesity rates with child obesity rates, I adopted an age range of 2-19 years (24 to 228 months) in my operational definition of "childhood" obesity. I used a variable in the Body Measures data denoting the age of individuals at the time of their physical exam in months (ridageex in the first two cycles, ridexagm in all subsequent cycles) to create an age-relevant subsample. All indicator variables that follow were restrained to this age-relevant subpopulation for ease and accuracy of analysis.

Within the Food Security dataset, the variables addressing SNAP status varied by cycle. I decided to focus on receipt in the last 12 months, given that "currently receiving" or "ever received" did not, in my opinion, accurately depict the cycle-by-cycle SNAP participation necessary for my time-dependent analyses. For the 2003-2004 and 2005-2006 cycles, fsq170 indicated households authorized for food stamps in the last 12 months. For all cycles thereafter, fsq171 indicated households which received benefits in the last 12 months. Accordingly, I used fsq170 (only for the first two cycles) and fsq171 (for all other cycles) to create a SNAP enrollment indicator variable (in SNAP/not in SNAP). I could not choose a continuous measure of SNAP benefits (ex: amount per month), given that earlier cycles lacked variables measuring the amount of SNAP benefits received. Furthermore, looking at SNAP receipt on the extensive margin (i.e. "receiving SNAP" or "not receiving SNAP") was the more robust approach, given that NHANES data on the amount of SNAP benefits received (i.e. on the intensive margin) is not scaled for confounding factors such as family size or number of children.

In the Body Measures datasets, only the later NHANES cycles had a direct obesity classification as a coded variable; as a result, I chose to create my own obesity indicator variable using BMI, which was measured in all cycles. I chose to define obesity as a binary condition for the primary analysis rather than using a continuous BMI measure because BMI is not an equivalent measure across years; raw obesity thresholds vary by age and gender, and even the WHO standard deviation qualification for obesity abruptly shifts between 4 and 5 years old. The weight measure which was consistently collected across all relevant cycles was BMI itself (bmxbmi), so this was chosen as the basis for creating a childhood obesity status indicator variable (obese/not obese). Conditions for obesity were created by age, gender, and standard deviations from the WHO median BMI for each age-gender group; these conditions were drawn from the WHO BMI-for-age charts (*available here and here*). According to the WHO definitions for childhood obesity, children aged 2-4 and 3 standard deviations or more above the median BMI for their age were classified as obese, while children aged 5-19 and 2 standard deviation or more above the median were classified obese (*Obesity and Overweight*). Given these conditions, individuals within the age-relevant subpopulation were classified as obese or not obese within the indicator variable.

To analyze the impact of the ARRA SNAP increase on SNAP recipients, I defined the control population as children with incomes just above the eligibility threshold who were not receiving SNAP. This choice of comparison population is the most likely to satisfy the parallel trends assumption necessary for a difference-in-differences analysis—that is, in the absence of the ARRA increase (which is only experienced by SNAP recipients, i.e. the treatment group), we would expect children of similarly low incomes to experience approximately the same trends in childhood obesity as their SNAP-enrolled peers. The barely-not-eligible-for-SNAP population also avoids selection bias, since they did not have the option to opt into or out of SNAP. I chose the poverty income ratio (PIR) in the Demographics data (indfmpir) as the metric by which to define this comparison subgroup. This variable was the most suitable choice, given that the poverty threshold is adjusted for family size and updated annually for inflation; income alone is an unadjusted measure. Given that SNAP income eligibility is for those at 130% or less of the poverty threshold, I set the PIR condition for the comparison population to be $1.3<PIR<2.3$. The range was necessarily wide to ensure a sufficient comparison population size for calculations. About a fourth of SNAP recipients qualify through categorical eligibility rather than income eligibility (a statistic which I confirmed in my data), which meant that many of those in the PIR range just above 1.3 received SNAP despite not being income eligible. Even with this 100% range of PIR values, the number of age-eligible SNAP recipients was usually about 2 times as large as the number of individuals in my comparison subgroup for each demographic. I deemed those sample sizes sufficient for calculating a mean point estimate. With my chosen PIR condition, I created an indicator variable for being in the comparison group.

In addition to comparing all children aged 2-19 in the SNAP recipient and near-eligible comparison group, I also performed subgroup analyses by relevant demographic factors: age, gender, and race/ethnicity. Ages 2-4 I classified as “preschool age” and ages 4-10 as roughly “elementary school age”. The WHO defines adolescents to be children between the ages of 10 and 19, a category which I broke into 10-14 and 14-19 to ensure that differences in trends between early and late adolescence would not be obscured by overly grouping data. This gave me four age subgroups. For gender, I analyzed boys and girls in separate analyses. For race/ethnicity, I used the variable ridreth1, which was present from all waves from 2003-2016. This variable only contained 5 categories namely: Mexican-American, Other Hispanic, Non-

Hispanic White, Non-Hispanic Black, and Other Race, Including Multi-Racial. The variable ridreth5 also included Non-Hispanic Asian, but was only added in the 2011-2012 NHANES wave—so I was forced to use the more limited classification in ridreth1. It is worth noting that the data presented in the main body of this paper exclude statistics or analyses for "Other Hispanic" and "Other Race, Including Multi-Racial". This is a direct result of extremely small sample sizes for these demographics within both the SNAP enrollee and comparison groups, which I deemed to render calculated point estimates statistically unreliable. (The data for those groups are, however, provided in the Appendix 2.)

Analytical Methods

I determined a difference-in-differences (DID) analysis to be appropriate and ideal for this research. Given that the ARRA SNAP increase satisfies the parallel trends assumption (as described in the previous section *NHANES: Variables Chosen for Analysis*), DID analysis is a powerful tool for establishing causality in this natural experiment.

Difference-in-Differences Regressions

I performed difference-in-differences (DID) analyses with cohort and year fixed effects. I used a two-period design, where waves from 2003-2008 constituted the pre-treatment period and waves from 2009-2014 constituted the post-treatment period (since the ARRA SNAP increase was implemented from April 2009 to November 2013). For all regressions, I calculated cross-sectional percentages by NHANES wave in Stata and regressed across those values in R. As a secondary method, I also performed one additional regression for the overall age 2-19 group (i.e. all children in NHANES), using individual-level data in Stata.

For all analyses, I ran a DID in R using the overall and subgroup cross-sectional childhood obesity percentages (see Appendix 3) as calculated from 2003 to 2014. (The 2015-2016 numbers were dropped to allow for a clear 2-stage non-treatment and treatment design.) The formula for this DID regression is as reported below:

$$\textit{Cross sectional obesity rate (\%)} = a^*(T\_d) + b^*(P\_d) + c^*(\, T\_d^*\, P\_d) + X$$

where a, b, and c are the regression coefficients for their respective variables, T_d is the dummy variable representing treatment group (1 in the SNAP population, 0 in the comparison population), P_d is the dummy variable representing treatment period (0 in waves before 2013, 1 in waves after 2013), T_d*P_d is the interaction term between the two dummy variables, and X is the intercept (noise) term. In R, this linear regression was coded as follows:

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didreg = lm(co ~ dummy_treat + dummy_period + intxn, data = DID)
```

For the individual-level regression that I ran on the data for all children ages 2-19, I created the same variables as I had in R (treatment dummy variable, period dummy variable, and interaction term) and appended the six relevant waves of NHANES data (2003-2004, 2005-2006, 2007-2008, 2009-2010, 2011-2012, 2013-2014). In Stata, childhood obesity was coded as 1 or 0 (obese or not obese) for each individual child, rather than the cross-sectional values which were used to run regressions in R. Using these variables, I ran a DID in Stata according to the regression below:

$$CO\_b = a^*(T\_d) + b^*(P\_d) + c^*(T\_d^* P\_d) + X$$

where a, b, and c are the regression coefficients for their respective variables, CO_b is the binary childhood obesity indicator and T_d, P_d, T_dd*P_d, and X are as described above for the DID regression in R (i.e. T_d is the dummy variable representing treatment group (1 for individuals in the SNAP population, 0 for those in the comparison population), P_d is the dummy variable representing treatment period (0 for individuals in waves before 2013, 1 for those in waves after 2013), T_d*P_d is the interaction term between the two dummy variables, and X is the intercept (noise) term.) In Stata, this linear regression was coded as written below:

```
reg child_obese treatmark treatdummy intxn, r
```

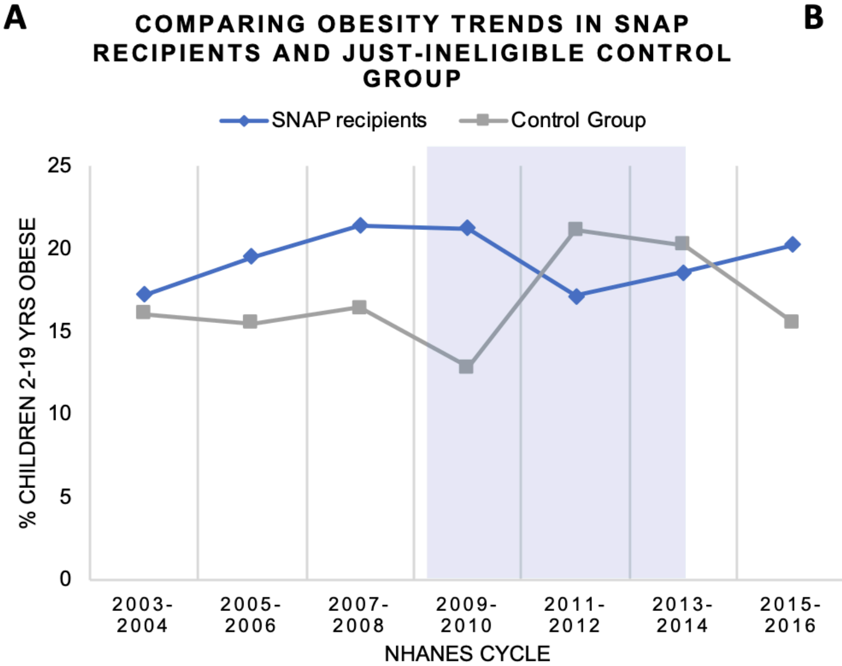
Full regression code runs are posted in the Appendices (Appendix 3). For the purpose of the DID analyses, the key value is the interaction term coefficient**: it describes the additional impact on the outcome (obesity percentage) experienced by the treatment group associated with being in the treatment group during the treatment time period (when both T_d and P_d are equal to 1). A negative interaction coefficient indicates that the ARRA increase was protective against obesity, while a positive interaction coefficient indicates that the increase had obesogenic effects. Additionally: the magnitude of the term describes the strength of the effect, and the p-value corresponding to *c* (the interaction term coefficient) provides indication as to whether the perceived interaction is statistically significant or not. A p-value greater than 0.05 (a typical alpha value) means that the impact of the interaction term is not statistically significant within the regression--and that, consequently, that the ARRA SNAP increase did not drive changes in obesity rates in the group of SNAP-receiving children being examined.

**Note: in the analyses which follow, I will refer to the crucial interaction coefficient term *c* as the interaction coefficient, the coefficient term, the coefficient, and *c* interchangeably.

FINDINGS

My broadest analysis was to look at trends in all children aged 2-19 who were either enrolled in SNAP or part of the near-eligible control group. For this analysis, I calculated the cross sectional mean of the childhood obesity indicator variable (thus giving an obesity rate percentage) in the age-relevant subpopulation for both the comparison and SNAP groups; I calculated this mean using the svy, subpop() option to ensure that weights were applied appropriately across the age-relevant subsample. The results of this calculation are summarized in the table and graph below (Figure 1):

A



B

% CHILDREN OBESE (AGES 2-19)		
NHANES Cycle	SNAP Recipients	Control Group
2003-2004	17.24	16.08
2005-2006	19.51	15.5
2007-2008	21.39	16.43
2009-2010	21.25	12.86
2011-2012	17.18	21.13
2013-2014	18.58	20.23
2015-2016	20.23	15.54

Figure 1. Comparison of trends in SNAP recipients and comparison population.
(A) Comparison of obesity trends in SNAP recipients and comparison group subjects for all children ages 2-19. Highlighted region denotes the time period of the ARRA SNAP benefit increase (April 2009-November 2013).
(B) Exact percentages for data displayed in Figure 1a. Obesity percentages were calculated cross-sectionally for each 2-year NHANES cycle using Stata 16.0.

Examining Figure 1 visually, there is [roughly] a drop in obesity rates for SNAP recipients over the ARRA period which contrasts sharply with the spike in obesity prevalence that occurs in the comparison group over that same time period. Both of these trends hint at a protective impact of the SNAP benefit increase on obesity rates, since the pre-ARRA trends for both groups were fairly similar. However, while the quantitative DID analysis (run in R) of these cross-sectional data yielded a negative interaction coefficient term (-2.447), the p-value of which (0.457) was not significant (Figure 5). This result was replicated when I ran a second regression over the same data at the individual level (rather than using cross-sectional rates) in Stata: the coefficient calculated was -0.135, with a p-value of 0.415. Just as in the DID regression over cross-sectional rates, this individual level DID determined the relationship between the ARRA increase and childhood obesity rates to be in the negative direction, but insignificant. As such, I conclude from these results that the ARRA SNAP benefit increase did not impact obesity rates in the overall population of SNAP recipients aged 2-19.

(Additionally: I performed the individual-level DID in Stata to see whether that form of analysis would preserve a higher level of certainty and reduce the standard error which seemed inherent to the cross-sectional data. However, after running these analyses, I decided that the high similarity between the p-values for these two regression methods was sufficiently convincing for me to apply the only the cross-sectional DID method (in R) to all subgroup analyses. Consequently, all regressions which follow were performed in R using cross-sectional rates calculated in Stata, not individual-level data.)

Given the lack of clear relationships in the regressions for the overall 2-19 age group, my next step was to run further DID analyses by splitting the 2-19 group into four age ranges: 2-4, 4-10, 10-15, and 15-19 years old. The resulting data are visually represented in Figure 2 (below), with the statistical DID results in Figure 5; the exact cross-sectional prevalences can be found in Appendix 2.

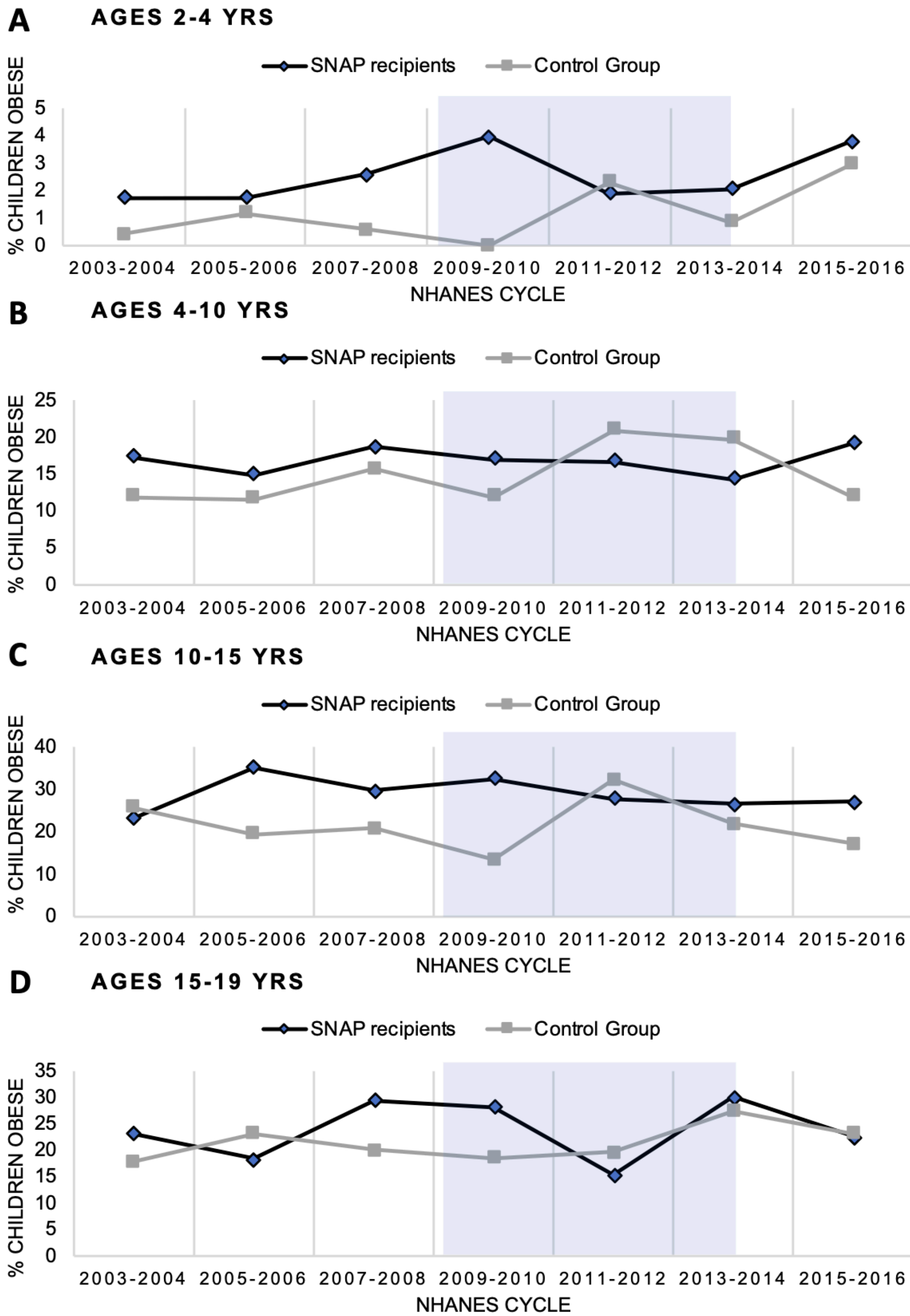


Figure 2. Comparison of trends in SNAP recipients and comparison population, segmented by age groups. (A) Comparison of obesity trends in SNAP recipients and comparison group subjects in the 2-4 year age group. (B) Comparison of obesity trends in SNAP recipients and comparison group subjects in the 4-10 year age group. (C) Comparison of obesity trends in SNAP recipients and comparison group subjects in the 10-15 year age group. (D) Comparison of obesity trends in SNAP recipients and comparison group subjects in the 15-19 year age group. Highlighted regions denote the time period of the ARRA SNAP benefit increase (April 2009-November 2013). Plotted obesity percentages were calculated cross-sectionally for each 2-year NHANES cycle by age group using Stata 16.0.

Interestingly, the graphs in Figure 2 display different trends than the aggregate data for ages 2-19 in Figure 1A. Figure 2A, in particular, appears to deviate from the overall pattern which is qualitatively fairly similar between Figures 1A, 2B, 2C, and 2D. Visually, Figure 2A shows a spike in SNAP participant obesity rates after the ARRA increase for children ages 2-4; the DID analysis confirms this quantitatively with a positive interaction coefficient of 0.310 (see Figure 5 for DID results by age subgroup). This is the only age group regression in which the coefficient term was positive; regressions for the other three age groups (corresponding to Figures 2B-2D) produced negative interaction coefficients, which hint at to protective effects of the ARRA increase. The singular positive term would (if significant) make the 2-4 age group the only age range for which the benefit increase *raised* obesity levels. However, the high p-value of this coefficient term (0.768) leads me to conclude that the ARRA SNAP increase did not impact childhood obesity rates in the 2-4 age group. In fact, the impact of the benefit increase was also non-significant for the 4-10, 10-15, and 15-19 age groups; none of these regressions produced interaction coefficient terms of statistical significance (see Figure 5 for exact p-values). Accordingly, I conclude that the result of the aggregate (ages 2-19) regression holds for each of these four age ranges: the ARRA increase did not impact the obesity outcomes of SNAP recipients in preschool, elementary school, early adolescence, or late adolescence, either positively or negatively.

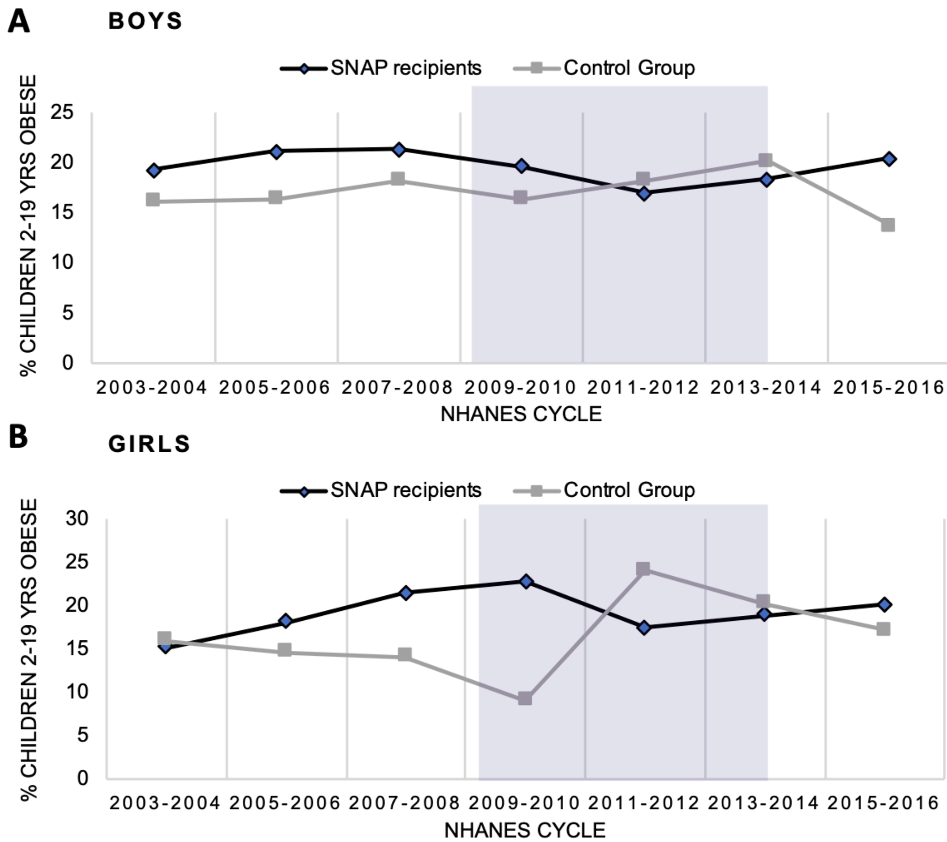


Figure 3. Comparison of trends in SNAP recipients and comparison population, by gender. (A) Obesity trends in boys ages 2-19, split between SNAP recipients and comparison group subjects. (B) Obesity trends in girls ages 2-19, split between SNAP recipients and comparison group subjects. Highlighted regions denote the time period of the ARRA SNAP benefit increase (April 2009-November 2013). Plotted obesity percentages were calculated cross-sectionally for each 2-year NHANES cycle by gender using Stata 16.0.

Taking into account how obesity and overweight are experienced differently by males and females, especially during adolescence, I performed the same DID analyses separately for boys and girls aged 2-19. The results are plotted in Figure 3, above (full cross-sectional values in Appendix 2). Visually, obesity rates appear more stable for boys than for girls across this 13-year span. Over the ARRA increase period, both genders appear to display similar trend changes (i.e. obesity increased for the control group and decreased for SNAP recipients)—but the changes in slope for both trend lines are much more dramatic in the Figure 3B, the girls' data.

DID analyses revealed, however, the boys actually had the more compelling data (results in Figure 5). While DIDs for both genders produced coefficients of negative sign (indicating a protective effect of the ARRA increase against obesity), the girls' DID interaction coefficient had a p-value of 0.784 (clearly insignificant.) In contrast, the p-value for the boys' DID regression was 0.056—a mere 0.006 away from statistically significant at the alpha=0.05 threshold. From these gendered regressions, I conclude that the ARRA increase did not impact obesity outcomes of female SNAP recipients in the 2-19 age group.

While the DID results for the boys did not strictly meet the standards for statistical significance, the low p-value of the interaction coefficient is still notable given the limits of the data and the extremely high p-values which have characterized all of my other DID regressions. Furthermore, the magnitude of the interaction coefficient (-3.633) indicates that the protective effect is not trivial. I do, therefore, tentatively posit that the increase in SNAP benefits may have had a protective effect against obesity for boys ages 2-19 who were enrolled in SNAP in the 2009-2014 period.

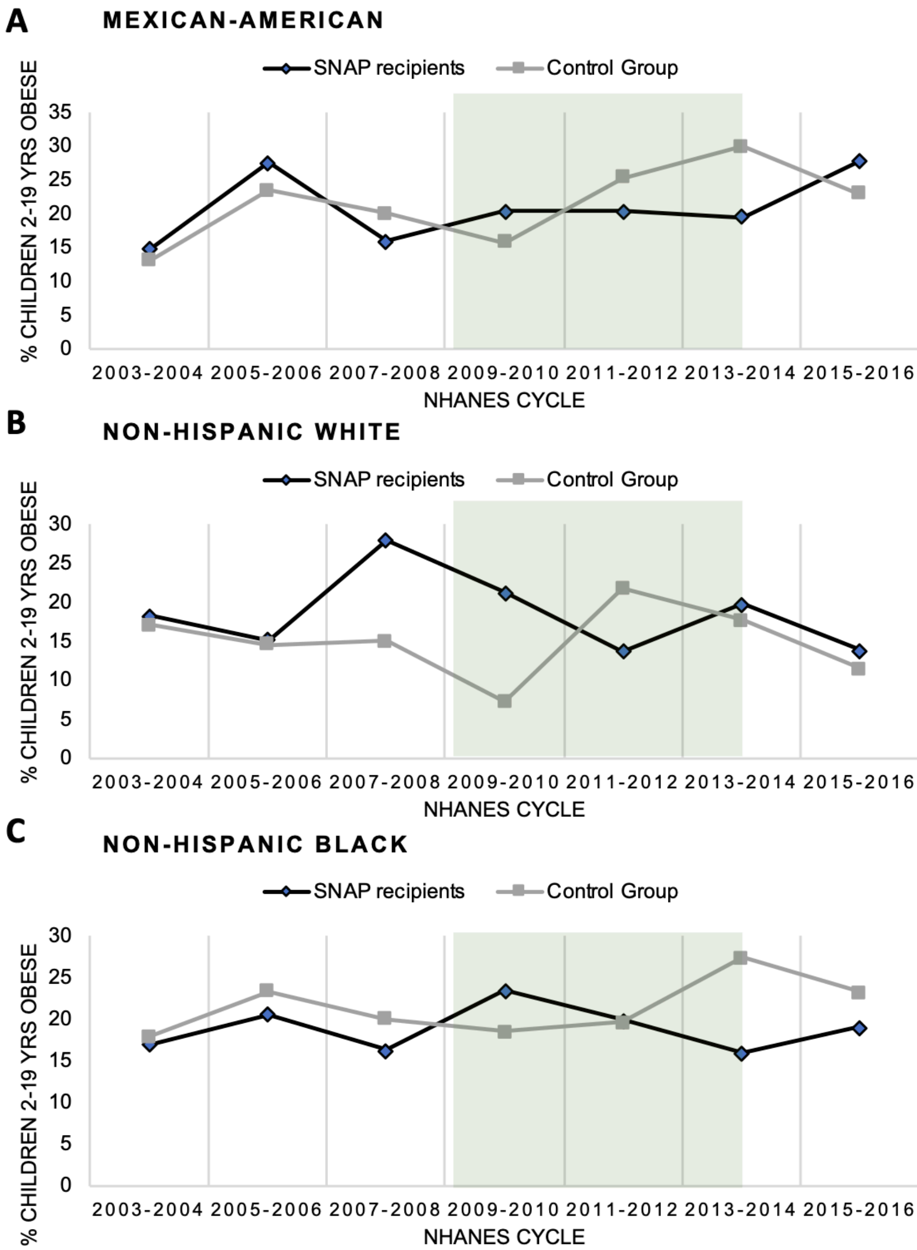


Figure 4. Comparison of trends in SNAP recipients and comparison population, by race/ethnicity. (A) Obesity trends in Mexican-American children ages 2-19, split between SNAP recipients and comparison group subjects. (B) Obesity trends in non-Hispanic white children ages 2-19, split between SNAP recipients and comparison group subjects. (C) Obesity trends in non-Hispanic black children ages 2-19, split between SNAP recipients and comparison group subjects. Highlighted regions denote the time period of the ARRA SNAP benefit increase (April 2009-November 2013). Plotted obesity percentages were calculated cross-sectionally for each 2-year NHANES cycle by race/ethnicity categorization using Stata 16.0.

My final set of subgroup analyses were performed by race/ethnicity for children ages 2-19: graphs of the results are in Figure 4 (above), while the exact cross-sectional values and DID results can be found in Appendix 2 and Figure 5, respectively. Looking at Figures 4A-4C, visually it appears that Figure 4C differs from the other two graphs in Figure 4. While 4A and 4B (data for Mexican-American and non-Hispanic white children) show decreased obesity rates for SNAP participants following the 2009 ARRA increase, the non-Hispanic black SNAP recipients experienced a sharp rise in obesity rates between the 2007-2008 cycle to the 2009-2010 cycle. The DID results confirm this qualitative observation. The interaction coefficient term for the non-Hispanic black DID regression was not statistically significant, but did have a positive sign—indicating that, while the ARRA benefits were associated with an increase in obesity rates for black children receiving SNAP, the impact of that association was not significant. The DID coefficients were negative for both the Mexican-American and non-Hispanic white subgroups (-4.147 and -2.233, respectively), but—as with the analysis for non-Hispanic black children aged 2-19—p-values for these coefficients were far too high for any protective effects to be statistically significant (see Figure 5). Given these results, I conclude that the ARRA SNAP benefit increase did not impact obesity rates in Mexican-American, non-Hispanic white, or non-Hispanic black children ages 2-19.

DIFFERENCE-IN-DIFFERENCES MODELS		
Model	Coefficient (of interaction term)	P-value (of coefficient)
Overall (children ages 2-19)		
Cross-sectional	-2.447	0.457
Individualized Data	-0.135	0.415
Age Subgroups		
Ages 2-4	0.310	0.768
Ages 4-10	-5.453	0.145
Ages 10-15	-0.860	0.905
Ages 15-19	-0.577	0.932
Gender Subgroups		
Boys	-3.633	0.056
Girls	-1.460	0.784
Race/Ethnicity Subgroups		
Mexican-American	-4.147	0.549
Non-Hispanic White	-2.233	0.731
Non-Hispanic Black	2.370	0.573

Figure 5. Difference-in-differences models: coefficients and p-values.

"Model" indicates the group or subgroup whose obesity data were used in the linear difference-in-differences regression.

Cross-sectional obesity percentages* calculated in Stata were run through difference-in-differences linear regressions in RStudio. The coefficient value is that of the interaction term as calculated in the linear regression; the p-value listed is the p-value corresponding to that coefficient.

The regression equations used to calculate these values are detailed in Methods section; pre-treatment (i.e. pre-ARRA SNAP increase) was defined as 2003-2008 waves, while 2009-2014 waves were defined to be the treatment period (given that the ARRA increase lasted from April 2009-November 2013.) Calculations performed using Stata 16.0 and RStudio Version 1.2.5042.

*The one exception to this method is the "Individualized Data" model, where obesity status for each individual record in each wave was used to run a difference-in-differences linear regression, run using the individual entries. All calculations for this model were performed in Stata.

The table above (Figure 5) contains the key results of the DID analyses described in this Findings section. The coefficient and p-value information it summarizes were crucial to the interpretation of my data findings, which can be most compactly expressed as follows:

> My findings point to the conclusion that the ARRA SNAP benefit increase did not impact obesity rates in children ages 2-19 who were receiving SNAP, nor did it impact obesity rates for children in the smaller age groups 2-4, 4-10, 10-15, or 15-19 specifically. Furthermore, the increase in benefits did not impact obesity prevalence differentially by race/ethnicity; it had no impact in the race/ethnicity subgroups I analyzed. The only subcategory which yielded semi-significant results was gender: I conclude that my DID

analyses of data for boys and girls hint that the ARRA benefit increase may have been protective against obesity in boys only, not in girls, for children ages 2-19 enrolled in SNAP.

It is important to note that the lack of statistical significance in my results indicates the lack of decisive result, rather than a clear interpretation. For example: a result which would tell me with high certainty that the ARRA increase had *no* impact would be an interaction coefficient with a value near 0 and a p-value below 0.05 or 0.01. The high p-values my analyses produced are a testament to the uncertainty that remains when interpreting my results.

DISCUSSION

There are some interesting things which Figure 5 does not address, however, which intrigued me in the graphical representations of my data. For one, in both my graphs and in those of Hudak and Racine (2021), the control group appears to undergo an increase in obesity prevalence over the 2009-2014 period. Given that this entire natural experiment is founded on the fact that only SNAP enrollees experienced the "treatment" (i.e. the ARRA benefit increase), it was surprising to me to see the control group experience drastic trend changes over that timeframe—and to see that my data visualization replicated those in another paper, indicating that the comparison group increase was not an artifact of some fluke in my calculations.

My results aligned with those of Hudak and Racine, the only other paper in the literature which specifically examined the impact of the ARRA increase on childhood obesity prevalence. Despite the fact that their treatment population consisted of SNAP-eligible children (rather than SNAP recipients) and they used CDC growth charts rather than the WHO BMI z-charts, we produced similar results. In other word, we found similar inconsistencies in the relationship between the ARRA increase and childhood obesity. However, my conclusion that the ARRA increase did not impact childhood obesity rates in any of the subgroups, nor in the overall 2-19 age eligible subpopulation, differs slightly from Hudak and Racine's assertion that they found significant ARRA impacts for three of the four youth age groups they analyzed (Hudak and Racine 2021). There is likely a difference in methodology which can account for how their analyses produced significant results, while in age subgroup analyses I had p-values as high as 0.932; yet a careful reading of their supplemental figures shows a collection of high p-values. I believe that further investigation by a third party using these NHANES datasets to re-analyze the impacts of the ARRA increase would be warranted.

Expanding outwards from the topics of my paper, I believe that further research should investigate more deeply the relationship between increased benefits and health outcomes (in particular, childhood obesity.) Specifically, I would like to see RCTs which randomly assign additional benefits to SNAP enrollees, independent of any federal increases. That would provide solid empirical data for how benefits impact health outcomes in a field whose literature is currently heavily dependent on natural experiments.

Limitations

There are multiple limitations to the analyses which I conducted, both analytical and data-related. Firstly: the complex sampling structure of NHANES (which renders the survey nationally representative) was not incorporated into my difference-in-differences analyses. The complex survey design carries unique sample weights for each individual within a wave, nontransferable between cycles; in calculating both the original cross-sectional obesity prevalence statistics and running the DID analyses, I was not able to incorporate the extensive survey weightings in the data I ran. As a result, the resulting standard errors do not reflect the uncertainty of estimation of the cohort averages used to run the DID regressions. However, it is worth noting that in the overall regression where I use dindividual-level data, the p-value was quite similar to the p-value in the regression of cross-sectional averages (0.457 for the cross-sectional data and 0.415 for the individual-level data (Figure 5)). The fact that the individual-level p-value was lower indicates that the cross-sectional significance estimates may even be conservative. In the analyses I performed, the issue of how my handling of individual weights affected standard errors is not an overly important consideration; all DID regressions, excluding that of the boys-only subgroup, had extremely high p-values regardless. This limitation does, however, lend more credence to my cautious interpretation of the boys-only DID analysis: the 0.006 difference from the 0.05 significance threshold may have been attainable with a different handling of the individual survey weights.

Additionally: while NHANES is a powerfully informative survey, it is cross-sectional. Individuals are not tracked across time periods, which limits the ability to perform causal inference. The DID framework is appropriate for causal inference in a natural experience such as the 2009-2013 ARRA increase, and can be modified to work with repeated cross-sectional data; however, it is worth acknowledging that causal relationships would have been more naturally established with longitudinally-linked individual data.

Another limitation concerns the obesity metric used to classify children as obese or non-obese. While NHANES is exemplary in that body measures were measured directly by experts (rather than by self-report), there is debate in the field about the usefulness of BMI as an indicator of childhood and adolescent obesity. In general, overweight and obesity can be defined as an excess of body fat (Sahoo 2015); while BMI is a useful estimator, it does not directly measure body fat quantity or percentage, which are the metrics associated with negative health outcomes. A study examining the usefulness of BMI in screening for adiposity in children and adolescents found BMI classifications of overweight and obesity to have high specificity (95-100%) but low sensitivity (36-66%) (Lazarus 1996). Another study found BMI z-score distributions to be poor predictors of percent body fat in children younger than nine years (Vanderwall 2017). Given BMI's high specificity and low sensitivity, it is likely that rates of obesity were underestimated (i.e. obese children were misclassified as non-obese, a false-negative.) However, the CDC states that BMI is an acceptable indicator of excess body fat in obese children, cautioning only against its use for classifications of overweight (*Body Mass Index)*—and, crucially, my DID analyses is trend-based. So long as the classification errors do not bias the data, it is reasonable to conclude that they would not significantly affect my regression results. Furthermore, my choice to examine only obesity rather than overweight means that I selected a more robust measure which, according to the CDC, is less likely to result in misclassification when using BMI for children. While better metrics for body fat percentage would be preferable, it is reasonable to conclude that this limitation does not invalidate my results—especially since the raw obesity rates are not essential to my interpretation of the data.

POLICY RECOMMENDATIONS

Invest in Research Regarding Expansion of SNAP-Ed

The results of my analyses indicate that increased SNAP benefits do not impact childhood obesity rates. Given the immediacy of the child obesity epidemic in the US, my consequent policy recommendation is that changes and expansions to SNAP-Ed be researched with the aim of helping to address this public health crisis; since the data say that increased benefits alone will not induce such changes, it is important to invest in research as to how this other element of the SNAP program can be used to combat childhood obesity.

The current health education program associated with SNAP—SNAP Education, i.e. SNAP-Ed—is not well supported empirically. A 2019 study, for example, found SNAP-Ed's nutrition education programming to be ineffective in improving nutrition or dietary outcomes (Rivera 2019). The most recent research indicates that policies and strategies aimed at reducing population obesity risk (i.e. community interventions) showed promise when they involved health clinics, community partnerships, or community and school partnerships (Smith 2020). However, a recurring theme in the literature is that this class of intervention has not been deeply investigated, and lacks empirical support as a result. SNAP-Ed is a federal program which, according to the USDA website, works to build partnerships with community organizations and to provide nutritional education (*[SNAP-Ed]).* Its current lack of effectiveness is a missed opportunity; yet the current research is simply not robust enough to provide a basis for recommendations about program improvement.

Therefore, my first policy recommendation is that the federal government invest in quasi-experimental studies of different types of SNAP-Ed programming with childhood obesity as one of the outcome measures, as well as any other variables the department deems fit (dietary outcomes, food security, etc.). This would allow the USDA to draw from the wide range of obesity intervention literature to test which changes can be made to SNAP-Ed to improve the program, and to evaluate the impacts of those changes on key health outcomes in children (including obesity.) If possible, it would be ideal for this research to use direct measures of adiposity (such as percent body fat, fat mass index, etc.) rather than BMI in order to avoid the issues I described in my Limitations section.

Investment in community intervention research to combat childhood obesity as part of the SNAP program would be a powerful choice in the growing fight against this noncommunicable disease. Given results of my analyses—that increased benefits do not impact childhood obesity rates—it seems even more crucial to invest in figuring out which interventions *do* hold promise for effectively impacting obesity prevalence in children.

Extend COVID-19 P-EBT Indefinitely

I recommend, given the statistically insignificant yet suggestive results of both my paper and that of Hudak and Racine (2021), COVID-19 P-EBT benefits be maintained indefinitely. An additional, secondary benefit of doing so is that this situation would also serve as another natural experiment--similar to the ARRA increase--which could be analyzed to determine whether an increase in SNAP benefits leads to better health outcomes for children.

As part of the US response to the ongoing COVID-19 pandemic, the US government introduced Pandemic Electronic Benefit Transfer (P-EBT) under the authorization of the Families First Coronavirus Response Act of 2020 *(State Guidance,* 2021). On January 22, 2021, the USDA announced that it would be increasing the P-EBT benefit by about 15% to help fill in missing meals caused by school and childcare closures *(State Guidance,* 2021).

My results led me to conclude that the ARRA SNAP increase did not impact childhood obesity rates. However, as Hudak and Racine noted in their study, the DID results do in fact point toward potential protective effects of the additional benefits, which simply fail to pass the test of statistical significance. Given that I worked with only 6 waves of data compressed into six point estimates to perform the DID analyses, it cannot be ruled out that this lack of significance reflects limitations in the data--rather than a weak or non-existent relationship between the ARRA increase and childhood obesity rates. Keeping that in mind, it is possible that the government runs the risk of increasing childhood obesity rates by removing the extra SNAP benefits provided in response to the COVID-19 pandemic.

From that possibility, I argue that it is without loss (from a policy standpoint, not a taxpayer perspective) for the federal government to maintain the higher P-EBT benefits that have been implemented due to the ongoing pandemic. The main hurdle faced by this policy is, of course, that it requires taxpayer money. Additionally, I would expect public support for such a welfare-oriented policy to wane as US citizens become numb to the ongoing pandemic; that makes it harder and harder to garner political will for increased program benefits. However, one of Biden's recently proposed reforms even includes a stipend for families with children. If that gains the political will to pass, I believe that an incremental SNAP benefit increase has a chance. It might also be strategically beneficial to frame the 15% increased value as the status quo, so that politicians which want to strip the money away from SNAP are forced to argue for taking *away* funds from low-income families' grocery budgets.

CONCLUSION

In this paper, I used seven waves of NHANES data to calculate childhood obesity in both SNAP recipients and a comparison low-income group. From these cross-sectional means, I performed multiple difference-in-differences analyses to examine how the 2009-2013 ARRA increase in SNAP benefits impacted childhood obesity prevalence. My findings confirmed similar results in previous studies: in the overall 2-19 age group, as well as in each of the age, gender, and race/ethnicity subgroups I analyzed, the ARRA SNAP benefit increase did not have a statistically significant impact on childhood obesity prevalence. Given these results, I recommended research into program improvements in SNAP-Ed and the maintenance of P-EBT benefits as options with greater potential to combat childhood obesity. While the direction of the relationship between SNAP benefit expansion and childhood obesity prevalence in the overall US youth population remains muddled, it is certainly a topic which still warrants further exploration.

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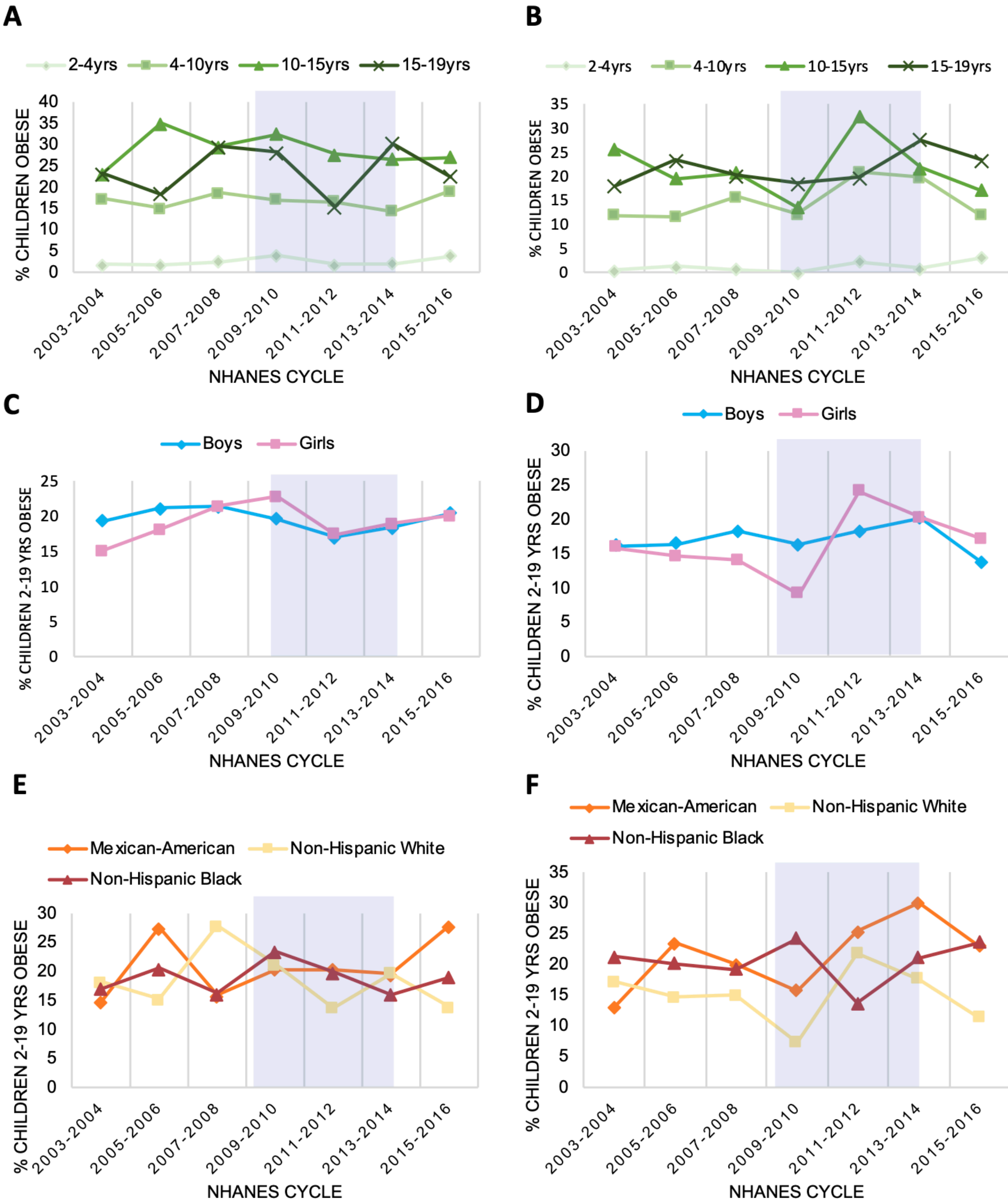
APPENDICES

APPENDIX 1: SUPPLEMENTAL FIGURES



Supplemental Figure 1. Comparison of trends in SNAP and control population, by subgroup. (A) SNAP recipients, by age. (B) Comparison population, by age. (C) SNAP recipients, by gender. (D) Comparison population, by gender. (E) SNAP recipients, by race/ethnicity. (F) Comparison population, by race/ethnicity. Highlighted regions denote the time period of the ARRA SNAP benefit increase (April 2009-November 2013). Plotted obesity percentages were calculated cross-sectionally for each 2-year NHANES cycle by age, gender, or racial/ethnic group using Stata 16.0.

APPENDIX 2: SUPPLEMENTAL TABLES/DATA

Tables 1a & b: Cross Sectional Rates by Age

Supplemental Table 1a: Age Categories, SNAP Participants

% Children Obese, SNAP				
Two-Year Cycle	2-4yrs	4-10yrs	10-15yrs	15-19yrs
2003-2004	1.74	17.28	23.24	23.26
2005-2006	1.73	14.95	35.09	18.39
2007-2008	2.59	18.58	29.62	29.59
2009-2010	3.98	17.01	32.55	28.27
2011-2012	1.89	16.66	27.74	15.38
2013-2014	2.07	14.24	26.60	30.18
2015-2016	3.82	19.19	27.05	22.55

Supplemental Table 1b: Age Categories, Comparison Group

% Children Obese, Comparison Group				
Two-Year Cycle	2-4yrs	4-10yrs	10-15yrs	15-19yrs
2003-2004	0.41	11.86	25.8	17.91
2005-2006	1.19	11.52	19.45	23.32
2007-2008	0.59	15.66	20.76	20.00
2009-2010	0.00	11.96	13.49	18.50
2011-2012	2.30	20.86	32.30	19.62
2013-2014	0.84	19.68	21.74	27.43
2015-2016	2.98	11.89	17.13	23.24

Tables 2a & b: Cross Sectional Rates by Gender

Supplemental Table 2a: Gender Categories, SNAP Participants

% Children Obese, SNAP		
Two-Year Cycle	Boys	Girls
2003-2004	19.30	15.11
2005-2006	21.15	18.10
2007-2008	21.39	21.40
2009-2010	19.63	22.80

2011-2012	17.02	17.36
2013-2014	18.31	18.85
2015-2016	20.42	20.04

Supplemental Table 2b: Gender Categories, Comparison Group

% Children Obese, Comparison Group		
Two-Year Cycle	Boys	Girls
2003-2004	16.22	15.91
2005-2006	16.37	14.61
2007-2008	18.21	14.07
2009-2010	16.36	9.04
2011-2012	18.26	24.08
2013-2014	20.20	20.25
2015-2016	13.79	17.15

Tables 3a & b: Cross Sectional Rates by Race/Ethnicitiy

Supplemental Table 3a: Racial/Ethnic Categories, SNAP Participants

% Children Obese, SNAP					
Two-Year Cycle	Mexican American	Other Hispanic*	Non-Hispanic White	Non-Hispanic Black	Other Race, incl. Multiracial*
2003-2004	14.66	14.58	18.28	17.04	20.21
2005-2006	27.48	29.10	15.18	20.53	9.70
2007-2008	15.91	15.42	27.93	16.27	26.59
2009-2010	20.33	19.53	21.21	23.48	18.12
2011-2012	20.44	18.16	13.68	19.84	14.84
2013-2014	19.48	25.78	19.74	15.98	11.20
2015-2016	27.86	26.32	13.80	19.00	15.58

** indicates small sample size, enough to be deemed insufficient for DID analysis*

Supplemental Table 3b: Racial/Ethnic Categories, Comparison Group

% Children Obese, Comparison Group					
Two-Year Cycle	Mexican-American	Other Hispanic*	Non-Hispanic White	Non-Hispanic Black	Other Race, incl. Multiracial*
2003-2004	12.99	16.5	17.14	21.22	7.65

2005-2006	23.51	4.67	14.64	20.18	0.94
2007-2008	19.95	18.20	14.99	19.22	7.78
2009-2010	15.77	22.39	7.28	24.16	15.23
2011-2012	25.31	16.80	21.78	13.74	22.15
2013-2014	30.01	17.50	17.65	21.07	19.61
2015-2016	22.98	17.69	11.41	23.66	18.39

** indicates small sample size, enough to be deemed insufficient for DID analysis*

APPENDIX 3: DID Code Outputs

Appendix 3A: Stata Output--Childhood Obesity Prevalence DID Regression with Individual-Level Data

```
. reg child_obese treatmark treatdummy intxn, r

Linear regression                               Number of obs     =      9,895
                                                F(3, 9891)        =       0.39
                                                Prob > F          =     0.7600
                                                R-squared         =     0.0001
                                                Root MSE          =     .38533

------------------------------------------------------------------------------
             |               Robust
 child_obese |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
   treatmark |   .0088377   .0135577     0.65   0.515    -.0177381    .0354135
  treatdummy |   .0120695   .0112575     1.07   0.284    -.0099976    .0341365
       intxn |   -.013531    .016613    -0.81   0.415    -.0460959    .0190339
       _cons |   .1737773   .0086448    20.10   0.000     .1568317    .1907229
------------------------------------------------------------------------------
```

APPENDIX 3B: R Output--Cross-Sectional Childhood Obesity Prevalence DID Regressions

Sample: Ages 2-19 (entire age-eligible subpopulation)

```
Call:
lm(formula = co ~ dummy_treat + dummy_period + intxn, data = DID_1)

Residuals:
    Min      1Q  Median      3Q     Max 
-5.2133 -0.8333  0.1033  2.0467  3.0567 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)    
(Intercept)    16.003      1.565  10.224 7.19e-06 ***
dummy_treat     3.377      2.214   1.525    0.166    
dummy_period    2.070      2.214   0.935    0.377    
intxn          -2.447      3.131  -0.782    0.457    
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 2.711 on 8 degrees of freedom
Multiple R-squared:  0.259,	Adjusted R-squared:  -0.01891 
F-statistic: 0.932 on 3 and 8 DF,  p-value: 0.4687
```

Age Subcategories

Ages 2-4 yrs

```
Call:
lm(formula = co ~ dummy_treat + dummy_period + intxn, data = DID_2)

Residuals:
    Min      1Q  Median      3Q     Max 
-1.0467 -0.3842 -0.2433  0.4875  1.3333 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)
(Intercept)    0.7300     0.5088   1.435    0.189
dummy_treat    1.2900     0.7195   1.793    0.111
dummy_period   0.3167     0.7195   0.440    0.672
intxn          0.3100     1.0176   0.305    0.768

Residual standard error: 0.8813 on 8 degrees of freedom
Multiple R-squared:  0.5299,	Adjusted R-squared:  0.3536 
F-statistic: 3.006 on 3 and 8 DF,  p-value: 0.09476
```

Ages 4-10 yrs

```
Call:
lm(formula = co ~ dummy_treat + dummy_period + intxn, data = DID_3)

Residuals:
    Min      1Q  Median      3Q     Max 
-5.5400 -1.5525  0.5167  1.7775  3.3600 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)    
(Intercept)    13.013      1.691   7.696 5.76e-05 ***
dummy_treat     3.923      2.391   1.641   0.1395    
dummy_period    4.487      2.391   1.876   0.0975 .  
intxn          -5.453      3.382  -1.613   0.1455    
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 2.929 on 8 degrees of freedom
Multiple R-squared:  0.3434,	Adjusted R-squared:  0.09724 
F-statistic: 1.395 on 3 and 8 DF,  p-value: 0.3132
```

Ages 10-15 yrs

```
Call:
lm(formula = co ~ dummy_treat + dummy_period + intxn, data = DID_4)

Residuals:
    Min      1Q  Median      3Q     Max 
-9.0200 -2.4108 -0.9967  3.6392  9.7900 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)    
(Intercept)   22.0033     3.4795   6.324 0.000227 ***
dummy_treat    7.3133     4.9208   1.486 0.175533    
dummy_period   0.5067     4.9208   0.103 0.920526    
intxn         -0.8600     6.9591  -0.124 0.904697    
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 6.027 on 8 degrees of freedom
Multiple R-squared:  0.3294,	Adjusted R-squared:  0.07789 
F-statistic:  1.31 on 3 and 8 DF,  p-value: 0.3367
```

Ages 15-19 yrs

```
Call:
lm(formula = co ~ dummy_treat + dummy_period + intxn, data = DID_5)

Residuals:
    Min      1Q  Median      3Q     Max 
-9.2300 -2.7125 -0.4483  4.1375  5.8433 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)    
(Intercept)   20.4100     3.2590   6.263 0.000242 ***
dummy_treat    3.3367     4.6089   0.724 0.489718    
dummy_period   1.4400     4.6089   0.312 0.762697    
intxn         -0.5767     6.5180  -0.088 0.931676    
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 5.645 on 8 degrees of freedom
Multiple R-squared:  0.1119,	Adjusted R-squared:  -0.2212 
F-statistic: 0.3359 on 3 and 8 DF,  p-value: 0.8001
```

Gender Subcategories

Boys, ages 2-19

```
Call:
lm(formula = co ~ dummy_treat + dummy_period + intxn, data = DID_6)

Residuals:
     Min       1Q   Median       3Q      Max 
-1.91333 -0.86000 -0.01167  0.90167  1.92667 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)    
(Intercept)   16.9333     0.8127  20.836 2.95e-08 ***
dummy_treat    3.6800     1.1493   3.202   0.0126 *  
dummy_period   1.3400     1.1493   1.166   0.2772    
intxn         -3.6333     1.6254  -2.235   0.0558 .  
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 1.408 on 8 degrees of freedom
Multiple R-squared:  0.5698,	Adjusted R-squared:  0.4085 
F-statistic: 3.533 on 3 and 8 DF,  p-value: 0.06815
```

Girls, ages 2-19

```
Call:
lm(formula = co ~ dummy_treat + dummy_period + intxn, data = DID_7)

Residuals:
    Min      1Q  Median      3Q     Max 
-8.7500 -1.1925 -0.1783  2.6275  6.2900 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)    
(Intercept)    14.863      2.579   5.764 0.000422 ***
dummy_treat     3.340      3.647   0.916 0.386482    
dummy_period    2.927      3.647   0.803 0.445405    
intxn          -1.460      5.157  -0.283 0.784282    
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 4.466 on 8 degrees of freedom
Multiple R-squared:  0.1862,	Adjusted R-squared:  -0.119 
F-statistic: 0.6102 on 3 and 8 DF,  p-value: 0.6271
```

Race/Ethnicity Subcategories

Mexican-American children, ages 2-19

```
Call:
lm(formula = co ~ dummy_treat + dummy_period + intxn, data = DID_8)

Residuals:
    Min      1Q  Median      3Q     Max 
-7.9267 -3.7525  0.3017  2.3833  8.1300 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)    
(Intercept)   18.8167     3.3105   5.684 0.000463 ***
dummy_treat    0.5333     4.6818   0.114 0.912112    
dummy_period   4.8800     4.6818   1.042 0.327731    
intxn         -4.1467     6.6211  -0.626 0.548579    
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 5.734 on 8 degrees of freedom
Multiple R-squared:  0.1423,	Adjusted R-squared:  -0.1793 
F-statistic: 0.4425 on 3 and 8 DF,  p-value: 0.7291
```

Non-Hispanic White children, ages 2-19

```
Call:
lm(formula = co ~ dummy_treat + dummy_period + intxn, data = DID_9)

Residuals:
   Min     1Q Median     3Q    Max 
-8.290 -2.770  0.465  2.310  7.467 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)   
(Intercept)    15.590      3.133   4.976  0.00108 **
dummy_treat     4.873      4.431   1.100  0.30337   
dummy_period   -0.020      4.431  -0.005  0.99651   
intxn          -2.233      6.266  -0.356  0.73074   
---
Signif. codes:  
0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 5.426 on 8 degrees of freedom
Multiple R-squared:  0.175,	Adjusted R-squared:  -0.1344 
F-statistic: 0.5655 on 3 and 8 DF,  p-value: 0.653
```

Non-Hispanic Black children, ages 2-19

```
Call:
lm(formula = co ~ dummy_treat + dummy_period + intxn, data = DID_10)

Residuals:
    Min      1Q  Median      3Q     Max 
-5.9167 -1.1592  0.0233  1.7058  4.5033 

Coefficients:
             Estimate Std. Error t value Pr(>|t|)    
(Intercept)    20.207      2.018  10.014  8.4e-06 ***
dummy_treat    -2.260      2.854  -0.792    0.451    
dummy_period   -0.550      2.854  -0.193    0.852    
intxn           2.370      4.036   0.587    0.573    
---
Signif. codes:  0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

Residual standard error: 3.495 on 8 degrees of freedom
Multiple R-squared:  0.08338,	Adjusted R-squared:  -0.2604 
F-statistic: 0.2426 on 3 and 8 DF,  p-value: 0.8643
```

APPENDIX 4: Link to NHANES Datasets (Publicly Available)

https://wwwn.cdc.gov/nchs/nhanes/ (*Last accessed 15 April 2021)*