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Essays on the Economic Determinants of Health

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ABSTRACT

ESSAYS ON THE ECONOMIC DETERMINANTS OF HEALTH

By

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This dissertation consists of three chapters on the economic determinants of health, specifically those related to the pathways of nutrition and government nutrition assistance policy.

I evaluate the Community Eligibility Provision's (CEP's) effects on child weight outcomes in my first two chapters, a program that allows certain schools to offer universally free school meals to all students. My first chapter uses child-level data from a nationally representative survey which follows a single sample of children from Kindergarten to fifth grade. I use these data to identify the effect of attending a CEP school on outcomes of child weight. I find that CEP school attendance increases a child's Body Mass Index (BMI) percentile score, decreases their likelihood of falling within the healthy weight range, and increases their probabilities of being overweight and obese.

In my second chapter, I utilize school-level data for the universe of K-12 schools in the state of Georgia. My data set includes aggregate measures of child weight including average child BMI and the percentage of students attending a school who fall within the healthy weight range. I find that adopting the CEP decreases average child BMI and increases the percentage of healthy weight students. Differences in the results of Chapters 1 and 2 highlight the likelihood that the CEP's effects on child weight may vary by location and student characteristics.

Finally, my third chapter proposes a new model for the measurement of food security. Specifically, I construct a Bayesian Graded Response Model (BGRM) which can be used to measure food security with responses to the United States Department of Agriculture's core Food Security Module (FSM). I use a simulated data exercise to evaluate the performance of my model in a controlled environment. I find that my model properly retrieves the set of data generating parameters. Com-

paring the performance of my model to the most commonly used measure of food security, the FSM scale, I find that my model more accurately assigns the food security status of households in all cases.

ESSAYS ON THE ECONOMIC DETERMINANTS OF HEALTH

By

George William Davis

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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DEDICATION

To the 37 million Americans who struggle with food insecurity each year. May you one day find hunger nothing but a fading memory.

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I am eternally grateful to many people who have guided me, inspired me, and supported me throughout the writing of my dissertation. This document is just as much the product of my friends, family, and colleagues as it is a product of myself.

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Last, but certainly not least, I would like to thank my friends both old and new. I am privileged to have many people in my life that I can call friends. Each of you are precious and appreciated more than words could ever convey.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

INTRODUCTION

This dissertation consists of three essays related to the economic determinants of health. The specific goal of my work is to shed additional light on the relationship between food security, government nutrition assistance programs, and the health of America's most disadvantaged residents.

My first chapter estimates the effect of the Community Eligibility Provision (CEP) on child weight outcomes. The CEP allows schools with high percentages of disadvantaged students to offer universally free breakfast and lunch to their entire student body. This represents a substantial change from the traditional system where students were required to qualify and apply for free and reduced-price school meals on an individual basis. Data for the study come from the Early Childhood Longitudinal Study, Kindergarten Class of 2010-11 (ECLS-K:2011), a nationally representative panel survey which follows a single sample of children who started Kindergarten during the 2010-2011 school year until fifth grade. I use these data along with an external CEP data set to identify the effect of attending a CEP school on child weight outcomes. I find that attending a CEP school increases a child's expected Body Mass Index (BMI) percentile score, decreases their likelihood of falling within the healthy weight range, and increases their probability of being overweight and obese. I find that this relationship is nearly universal across my various specifications, sensitivity analyses, and robustness checks.

My second chapter also seeks to answer the question of how universal free school meals affect child weight. Like the first chapter, Chapter 2 examines the CEP's effect on child weight, but at a different level for a different population of students. I utilize school-level data for the universe of K-12 schools in the state of Georgia. My data set includes aggregate measures of child weight, namely school-level average child BMI and the percentage of students attending a school who fall within the healthy weight range. This approach differs from Chapter 1 where data come from an individual level data set of only children in late elementary school at the time of the CEP's introduction. In Chapter 2, I find that adopting the CEP decreases school-level child BMI and increases the percentage of students who fall within the healthy weight range. Differences between the results of Chapters 1 and 2 highlight the likelihood that the CEP's effects on child weight may vary by characteristics like student age and location. By combining the results of multiple studies, researchers and other interested parties can gain a better understanding of the CEP's full effect on child health outcomes.

Finally, my third chapter proposes a new model for the measurement of food security. Specifically, I construct a Bayesian Graded Response Model (BGRM) which can be used to measure household food security with responses to the United States Department of Agriculture's core Food Security Module (FSM). Unlike the most common food security measure which uses a scale to assign households into food security categories based on their responses to the set of FSM questions, my method samples from a distribution of food security for each household and provides several advantages over the classic approach. After deriving and presenting my model, I evaluate its performance in a simulated data exercise. I find that the model properly retrieves the set of data generating parameters, but similar to other studies using similar methods, convergence of the response threshold parameters specifically is slow. Comparing the performance of my model to that of the traditional food security scale, I show that my model does a better job classifying households as food secure or food insecure than the FSM scale when the traditional 3 positive response criteria is used. Adjusting the number of positive responses needed to classify a household as food insecure under the FSM scale to match the share of food insecure households in the simulated data closes the gap in performance between the two approaches, but I still find that the BGRM outperforms the FSM scale in all cases.

Chapter 1

SHOULD KIDS HAVE THEIR LUNCH AND EAT IT TOO? ESTIMATING THE EFFECT OF UNIVERSAL FREE SCHOOL MEALS ON CHILD WEIGHT

1.1 Introduction

More than half of the 44 million school meals served each day in the United States are provided to students for free or at a reduced-price (USDA, 2017a). In keeping with the school meal program's earliest policy goals of preventing hunger and malnutrition among students, these Free and Reduced-Price (FRP) meals represent an important and reliable source of food for millions of disadvantaged children (Gunderson, 2014). While school meals have played an important role in the fight against food insecurity and child malnutrition for more than 80 years, room for improvement exists as roughly 6.5 million U.S. children remain food insecure under the existing program (Coleman-Jensen et al., 2018).

In an effort to reach children from disadvantaged backgrounds whose needs were not adequately met through the traditional FRP system, 2010's Healthy, Hunger-Free Kids Act (HHFKA) introduced the Community Eligibility Provision (CEP). Through the CEP, qualifying schools and districts serving high percentages of students from disadvantaged backgrounds provide free lunch and breakfast to their entire student body. These free meals are made available to students without consideration of their own household income level, participation in various government assistance programs, or other related characteristics. Alternatively, students attending non-CEP schools must qualify and apply for FRP meals on an individual basis through the traditional application–based system. By adopting the CEP, schools remove these applications and ensure that all students can access at least two complete meals during each school day.

Many of the CEP's supporters claim that the provision of universal free school meals will improve student health and reduce child obesity. There are two commonly cited avenues through which this may occur. First, students who did not participate in school meals due to cost under the traditional system may be incentivized to switch after all meals become free. Depending on the quality of school meals relative to a student's alternative options, this switch may improve overall dietary quality and health. The second avenue is the removal of stigma. More specifically, by making school meals universally free to all students, the CEP removes the stigma surrounding free school meals. Removal of this stigma may in turn increase participation rates and improve dietary quality among FRP-eligible students. In contrast, some existing evidence suggests that participation in school meals may worsen, rather than improve, child weight outcomes (Schanzenbach, 2009; Millimet, Tchernis, and Husain, 2010). However, these studies evaluate the traditional system where students self-select into FRP meals which differs from the CEP's universal enrollment of all students into free school meals. Furthermore, much of the existing evidence concerns the period prior to the HHFKA's changes to minimum meal nutrition standards. If the quality of school meals has sufficiently improved, then the effects estimated by previous studies may not hold in the post-HHFKA environment.

Given the substantial changes to the traditional school meal system caused by the CEP, understanding the provision's effect on child health is vital to the evaluation of school meal policy. In this study, I estimate the CEP's effect on child weight and body composition. Child-level data come from the Early Childhood Longitudinal Study, Kindergarten Class of 2010-11 (ECLS-K:2011) which follows a nationally representative sample of students who started Kindergarten during the 2010-2011 school year until fifth grade. My data set includes objectively measured child height and weight along with variables related to each student's sociodemographic characteristics, household, and school. I utilize a panel Difference-In-Differences (DID) framework to estimate the plausibly causal effects of attending a CEP school on my primary outcomes of interest: Body Mass Index (BMI) percentile and the probability of underweight, healthy weight, overweight, and obesity. Identification of my effects of interest relies on conditionally exogenous variation in CEP eligibility and participation as well as variation in the timing of the program's introduction. I test the robustness and sensitivity of my results to various sample restrictions and alternative specifications designed to address several specific threats to identification.

I find that attending a CEP school produces significant increases in a child's expected BMI percentile. For a 10 year old boy of average height and weight, CEP school attendance leads to a roughly 0.5 pound increase in weight, all else unchanged. I also estimate the effect of CEP school attendance on BMI percentile by gender, race, pre-CEP period household income level, and region. These sub-group specific analyses allow me to determine how the CEP's effects on weight may differ for students from different backgrounds with potentially different pre-CEP period school meal environments. I find that attending a CEP school leads to increases in expected BMI percentile for all sub-groups. The effect is substantially larger for girls than boys, implying that the program produces differential impacts by gender. In line with my *a priori* assumptions, I also find some evidence to suggest that children with higher pre-CEP period household income levels see larger changes in their BMI than lower income peers. Alternatively, I do not find notable differences in the CEP's effect on BMI percentile by race or region.

Aside from the CEP's direct effects on BMI percentile, my results indicate that attending a CEP school increases a child's likelihood of being overweight and obese while decreasing their probability of falling within the healthy weight range. For the full sample students, I find that CEP school attendance increases the probability of obesity by 1.41 percentage points. I find generally similar effects in my sub-group analyses by gender, race, pre-CEP period household income level, and region. One exception is for students living in the Northeast, where I find that attending a CEP school increases the probability of healthy weight and decreases the probability of overweight.

My study represents a contribution to two primary literatures. First, it contributes to the literature on the effect of school meals and other government assistance programs on child weight. My study builds on Davis and Musaddiq (2019), the study covered in Chapter 2, who use school-level data from the state of Georgia to estimate the effect of CEP participation on average student BMI and percent of healthy weight students. Unlike Davis and Musaddiq (2019), my data allow me to estimate the CEP's effect on weight at the individual child level for the entire nation. Second, my study contributes to the general literature on universal versus targeted government assistance program design. Universally providing free school meals through the CEP represents a significant departure from the traditional system in which students must qualify and apply for school meals directly. Most notably, the CEP is a rare example of shifting the level of self-selection for an existing program upwards. By moving the free school meal enrollment decision from the child to the school, I expect to find different effects than those observed in existing work evaluating the traditional system.

Whether across or within socioeconomic groups, disparities in nutrition and food security among children may lead to lifelong gaps in health, education, and economic outcomes. While the CEP was designed to improve such inequalities, I find that it comes at the cost of potential weight gain and increased rates of childhood overweight and obesity. With these results, policymakers can improve existing programs and design supplemental policies targeting children who remain at the highest levels of poor health outcome risk in a post-CEP school meal environment.

The remainder of the study is structured as follows: Section 1.2 provides a review of the literature concerning the effect of school meals on child health, Section 1.3 gives a detailed overview of the CEP and discusses its potential effects on student weight, Section 1.4 provides information on data used in the study, Section 1.5 presents the study's methodology, Section 1.6 discusses the results of my estimation, Section 1.7 details the various tests used to evaluate the sensitivity and robustness of my primary results, and Section 1.8 concludes.

1.2 Literature Review

Historically, studies examining the effects of school meals on child health have produced mixed results. Comparing children just above and below the income threshold for reduced-price lunch in a regression discontinuity design, Schanzenbach (2009) finds that reduced-price eligible students are more likely to be overweight relative to barely ineligible peers. Millimet, Tchernis, and Husain (2010) also find that participation in school lunch increases child weight after controlling for student self-selection into school breakfast. Alternatively, the authors show that school breakfast participation leads to an expected decrease in weight, suggesting that the effects of school meals are not necessarily homogeneous across lunch and breakfast. Capogrossi and You (2017) compare children participating in both school lunch and school breakfast to students participating in only one of the two programs. The authors find that school lunch participation increases a child's probability of being overweight, especially among children living in the South, Northeast, and rural areas of the country. Exploiting variation in the federal provision of school meal funds, Hinrichs (2010) finds no evidence to support a long-term effect of school meal availability on adult BMI. Finally, Gundersen, Kreider, and Pepper (2012) use a worst-case bounding model which allows for misreporting to show that participation in school lunch significantly decreases rates of child obesity, food insecurity, and poor health.

While many existing studies find a positive relationship between school lunch and child BMI, participating in school breakfast is often shown to either decrease BMI or have no effect. Given that the CEP makes lunch and breakfast universally free to all students, it is unclear what the program's net impact on child weight will be if the two meal types produce competing effects. In addition to implementing the CEP, the HHKFA also mandated improvements to minimum school meal nutrition standards for the first time in 15 years. Most studies looking at the relationship between school meal participation and child weight concern the period prior to these minimum nutrition standard changes. While meals served in CEP and non-CEP schools are subject to the same nutritional requirements, it is possible that school meals have differential impacts on child weight in the pre- and post-HHFKA periods due to variation in their average quality. Smith (2017) examines the effect of school lunch and breakfast participation on the diets of children across the distribution of initial diet quality in years after the HHFKA's changes to minimum nutrition standards. The author finds that both programs improve the diet quality of nutritionally disadvantaged children, but the effect varies considerably depending on initial diet quality with detrimental effects for children located in the distribution's upper tail. This finding is especially important for my study as most CEP schools serve a combination of disadvantaged and non-disadvantaged students. Under the assumption that the average diet quality of students in each group is systematically different prior to the CEP's introduction, I expect that offering universally free meals will have a differential impact on child weight.

Most closely related to my study is the literature examining how alternatives to the traditional school meal system affect child health; specifically programs where school lunch, breakfast, or a combination of both are provided to all students for free. Schwartz and Rothbart (2017) evaluate changes in weight caused by the switch to universal free school lunch in New York City where breakfast had already been free to students for several years. The authors find little effect of universal free school lunch on BMI and some evidence that the change improved weight outcomes among non-poor students.

Finally, in very first study of the CEP's effect on child weight, Davis and Musaddiq (2019) use school-level data from the population of K-12 schools in Georgia to show that CEP participation leads to lower average student BMI scores and higher percentages of students falling within the healthy weight range. While the effect differs by grade and location type, the authors find no significant evidence to support a detrimental effect of CEP participation on measures of child weight aggregated at the school level. This study builds on the work of Davis and Musaddiq (2019) by using individual-level panel data from a nationally representative sample rather than school-level data from a single state. Alternatively, my study concerns children who are in late elementary school during the post-CEP period while Davis and Musaddiq (2019) utilize data for all K-12 schools. I discuss the differences in results and their potential causes between Chapters 1 and 2 more thoroughly in Chapter 2.

1.3 The Community Eligibility Provision

Part of 2010's Healthy Hunger-Free Kids Act (HHFKA), the Community Eligibility Provision (CEP) was made available to qualifying schools in all states beginning in June of 2014.[1] Under the

[1] Prior to 2014, the CEP was piloted at different points in 11 states. Illinois, Kentucky, and Michigan were the first states to pilot the provision during 2011; the District of Columbia, New York, Ohio, and West Virginia were added in 2012; and Florida, Georgia, Maryland, and Massachusetts became pilot states in 2013.

CEP, schools serving a high percentage of students from disadvantaged backgrounds are given the opportunity to provide universally free lunch and breakfast to their entire student body. Roughly 14,000 schools (1 in 10) in 2,200 districts adopted the CEP during its first year (Neuberger et al., 2015). Among schools serving the nation's most severely disadvantaged students, roughly 3 in 5 schools adopted the CEP.[2] In its second year, around half of all CEP eligible schools in the U.S. (18,000) adopted the provision, implying that more than 8.5 million students were attended a CEP school in 2015 (USDA, 2016).

CEP eligibility is determined by each school's Identified Student Percentage (ISP). ISP represents the percentage of students attending a school who are eligible for free school meals through participation in other government assistance programs such as the Supplemental Nutrition Assistance Program (SNAP) and Medicaid or by meeting other special criteria like being from a migrant family or homeless.[3] Students are identified through either direct certification, which relies on state and federal data matching, or by a qualified school representative tasked with identifying migrant, runaway, and homeless students.

Schools with an ISP of 40% or above are eligible to participate in the CEP. While schools play a part in their own participation decision, school districts ultimately make the choice to enroll the school in the provision or have them continue under the traditional system. Furthermore, if the average ISP of schools in a district is 40% or greater, the entire district may enroll in the CEP. Through district-wide enrollment, all schools in the district participate in the CEP. In some cases, individually ineligible schools with ISPs below 40% participate in the CEP under this district-wide enrollment option.

While the 40% ISP level is a strict threshold for school-level CEP eligibility, schools with ISPs just above the eligibility threshold are the least likely to participate. The low CEP participation rate among barely eligible schools is most likely due to how the United States Department of Agri-

[2]This group of schools represents those with an identified student percentage of 60% or greater. Identified student percentage is discussed in detail in the following paragraph.

[3]In full, students are deemed "identified" if (i) the student's family is a SNAP, TANF, or FDPIR recipient, (ii) the student is a Head Start or Early Head Start participant, (iii) the student is a migrant, runaway, homeless, or foster child, or (iv) the student is on Medicaid.

culture (USDA) reimburses CEP schools for each meal served. Under the traditional school meal system, every lunch and breakfast provided by the school is reimbursed at a set amount depending on whether the student paid full price for the meal, paid a reduced price for the meal, or received the meal for free. Full price meals earn the lowest level of reimbursement since the school receives more money directly from the child. Reduced-price meals earn a larger reimbursement than full price meals, but free meals are reimbursed at the highest level. The amount of these reimbursements also varies considerably between lunch and breakfast. For example, the free lunch and breakfast reimbursement rates during the 2016-2017 school year were \$3.16 and \$1.71, respectively while the full price reimbursement amounts for the same meals were only \$0.30 and \$0.29 (USDA, 2016). Comparing full price and free rates, a full price lunch earns 9.5% of a free lunch reimbursement and a full price breakfast earns 17%.

While FRP meals receive significantly higher reimbursements relative to full price meals, the money students pay for each meal is assumed to largely offset the difference under the traditional system. Alternatively, the reimbursement scheme for CEP schools is considerably different. Even though all meals are provided to students for free under the CEP, the amount reimbursed by the USDA varies with school ISP. More specifically, the USDA reimburses a share of $1.6 \times ISP$ of all meals served at the free rate while the remaining $(100 - 1.6 \times ISP)$ are reimbursed at the much lower full price rate. In the case of a barely eligible school with an ISP of 40%, adopting the CEP would lead to 64% of their meals being reimbursed at the free rate while the remaining 36% would only earn the full price reimbursement. Given the substantial difference between full price and free meal reimbursement amounts, barely eligible schools face a disincentive to CEP participation caused by the USDA's reimbursement system.

To see if the effect of this disincentive is supported by the data, I present the relationship between ISP and CEP participation for schools in my data set in Figure 1. Figure 1 shows bins of ISP on the x-axis along with each bin's corresponding CEP participation rate on the y-axis.[4] The figure

[4]My final bin only includes schools with ISPs between 90% and 95%. There are two schools in my data set with ISPs greater than 95%, but I remove them from the figure because their participation rate of 0.5 produces a visually misleading change.

shows that CEP participation fluctuates around zero among schools with ISPs below 40% with some exceptions caused by district-wide enrollment. For schools with ISPs above the 40% cutoff, Figure 1 shows that the CEP participation rate is increasing in ISP. Furthermore, CEP participation is strictly increasing in ISP until the 60%-65% bin. Given that CEP schools begin receiving free meal rate reimbursements for 100% of their meals at the 62.5% level, this change is likely caused by removing the reimbursement disincentive from each school's participation decision.

Aside from how ISP relates to a school's CEP eligibility and reimbursement rate, it is important to note that ISP differs from the percentage of students attending a school who were previously enrolled in FRP meals. Specifically, identified students represent a subset of all students who receive meals for free or at a reduced-price (Levin and Neuberger, 2013). ISP is preferable to FRP percentage when determining school CEP eligibility for two primary reasons. First, the CEP was designed to satisfy the needs to children living in disadvantaged families who were not already enrolled in FRP meals under the traditional system. These families may choose not to enroll their child in FRP meals due to inadequate information or misunderstandings about their school meal options. Another possible cause for non-participation is stigma, wherein children or parents do not want to face scrutiny from their peers by utilizing FRP meals (Askelson et al., 2017). Alternatively, all students are enrolled in free meals under the CEP, removing the stigma surrounding participation (USDA, 2016).

The second reason why ISP is a better fit for determining CEP eligibility than FRP percentage relates to each measure's relative ability to reflect student disadvantage. While FRP percentage, namely free and reduced-price lunch percentage, is often used as a proxy for household disadvantage, recent research has shown that FRP percentage poorly captures disadvantage even with respect to household income (Domina et al., 2018). ISP, however, represents the percent of students attending a school who qualify for FRP meals through participation in a wide range of government assistance programs. While participation in some programs that factor into a school's ISP like TANF directly relate to household income, others capture additional aspects of household disadvantage. For example, SNAP and Medicaid participation relates to a family's reliance on government assistance for food and healthcare in addition to direct cash transfers.

As for the CEP's connection to weight, I expect that attending a CEP school has the potential to affect outcomes among three primary groups *a priori*. First, I anticipate an effect among children who were ineligible for FRP meals under the traditional system. If students in this FRP-ineligible group paid full price for school meals prior to the CEP, then the switch to free meals may not directly change a child's diet since the school meals they receive will remain unchanged.[5] Even if diet quality remains consistent, however, there is potential for an income effect caused by no longer spending money on school meals. As an example of this income change, the average full price for a lunch and breakfast served in an elementary school is $2.48 and $1.46, respectively.[6] During a 180 day school year, an elementary school student paying full price for a school lunch and breakfast each day is expected to spend roughly $710 on school meals. Following adoption of the CEP, these same meals are provided to the student for free, thereby eliminating their cost. For a two-person household with family income at 200% of the federal poverty line, the cost savings from attending a CEP school amount to roughly 2.2% of their yearly household income. Depending on how households spend this money, attending a CEP school may result in beneficial, detrimental, or negligible changes in child weight.

Second, I expect the group of students who brought meals from home to be differentially effected by the CEP relative to children who were already participating in school meals. Students may choose to bring "brown bag" meals under the traditional system due to their cost relative to school meals or a preference for certain types of foods brought from home. For the first set of students who bring brown bag meals due to cost, making school meals free through the CEP may produce an adequate incentive to switch. These cost savings may also induce switching among students who bring brown bag meals due to taste if their preference is outweighed by the reduction in meal costs. In either case, I expect the CEP to have differential effects depending on the rela-

[5] An exception would be if enrolling in the CEP causes schools to systematically change the quality or types of school meals they offer. While this possibility does not threaten my ability to identify the treatment effects of interest, it would mean that the CEP's effect on health among students participating in school meals during all periods operates through a change in school meal quality rather than other potential mechanisms.

[6] Average school level meal prices come from the School Nutrition Association's (SNA's) State of School Nutrition 2018 survey (SNA 2018).

tive quality of both meal types. If a child switches from higher quality brown bag meals to lower quality school meals, their weight outcomes will most likely worsen under the CEP. If brown bag meals are less healthy, however, then participation in school meals could have a beneficial effect on student weight.

In addition to students who fully switch from brown bag meals to school meals following CEP adoption, there are likely children who will continue bringing brown bag meals due to preference. If the diet of these students is unaffected by the CEP, then I expect to see no change in their health outcomes. This assumption would not hold if peer effects caused by the change in behavior of students whose dietary habits *were* affected by the CEP leads to a change in the weight of students who only eat food brought from home in all periods. Alternatively, it may be the case that students bringing meals from home consume some combination of their brown bag and free school meals under the CEP.[7] If students prefer certain components of their brown bag and school meals, they may choose to eat portions of both. This behavior has an ambiguous effect on diet quality conditional on a child's food preferences. In some cases, choosing between components of a brown bag and school meal may have a beneficial effect on the quality of a student's diet. If children prefer unhealthy foods, however, I expect that having the choice to eat portions of both meals will result in an overall increase in total calories consumed and a worsening of weight outcomes.[8] In some cases, children may even choose to eat most or all of both meals which has the potential to substantially increase their weight.

The final group of children whose weight I expect to change after adoption of the CEP are those who were eligible for reduced-price, but not free, meals. For the set of reduced-price eligible students who participated in school meals prior to the CEP, I expect any change in weight to come

[7] In conversation with the nutrition director of a large urban school district in Georgia, I learned that many students who brought their meals from home prior to the CEP continued to do so once free meals were made available. The director believed this was primarily caused by the students' distrust in the reliability of posted school meal menus during earlier years due to one of the district's previous food vendors. It was unclear at the time of our conversation if students were regularly eating portions of their brown bag and free school meals rather than choosing to eat only one of the two types.

[8] Aside from the affect on diet quality and health, if a child bringing brown bag meals in a CEP school eats portions of both meals and discards the remainder, their actions will likely lead to increases in food waste. Evaluating the CEP's effect on the amount of food wasted in American schools is a promising avenue for future research.

from an income effect due to cost savings or the removal of stigma. While the amount charged for a reduced-price meal is relatively low compared to the full price, the savings may be large enough to affect a student's weight. Alternatively, for children who are eligible for reduced-price meals but choose to bring brown bag meals from home under the traditional system, offering meals for free may or may not induce changes in the ways mentioned earlier.

1.4 Data

The primary source of data for my study is the Early Childhood Longitudinal Study, Kindergarten Class of 2010-11 (ECLS-K:2011) restricted-use data set. The ECLS-K:2011 is a nationally representative sample of children who began Kindergarten during the 2010-2011 school year.[9] The survey follows the same group of children from Kindergarten until fifth grade in the 2015-2016. The survey period covers four pre-CEP years and two years of post-CEP data.[10]

The ECLS-K:2011 consists of multiple questionnaires administered to each child's family, teachers, and school administrators. Data used in my study primarily come from the family and school questionnaires. My child weight outcomes of interest are child Body Mass Index (BMI) percentile and binary weight category indicators of underweight, healthy weight, overweight, and obesity.[11] Unlike many similar surveys which include self-reported anthropometric outcomes, child height and weight are directly measured by survey administrators in the ECLS-K:2011. This feature of the data avoids concerns regarding sources of non-classical measurement error inherent to self-reported weight and height.

I separate my covariates of interest from the ECLS-K:2011 into two sets. The first set is child covariates which includes: If the child's family income is at or below 200% of the federal poverty line, if the child belongs to a single parent household, if the child's mother was married at the

[9] While the ECLS-K:2011 was designed to be nationally representative for the 2010-2011 school year, the same may not be true during later waves of the study due to attrition or demographics shifts at the national-level.

[10] As spoken to in Section 1.3, the CEP was piloted in 11 states prior to the 2014-2015 school year. For my primary results, I impose the restriction that all pilot state schools were non-participants in the CEP during the pilot period. Given that children attending pilot state schools may have been exposed to the CEP in years before the national rollout, I expect my primary estimates to be conservative. I evaluate the impact of this restriction on my results in Section 1.7.

[11] All BMI percentiles and weight categories are calculated using the zanthro package in STATA.

time of birth, child age in months, if the child is black, and if the child is male. The second set of covariates relate to features of a child's school, including: If the child attends an urban school, if the child attends a rural school, and the percentage of non-white students attending the child's school.

To determine ISP, CEP eligibility, and CEP participation status for each school in the data set, I rely on the restricted-use ECLS-K:2011's Common Core of Data (CCD) identification number to merge the set of ECLS-K:2011 schools with an external CEP data set. Since 2014 each state has been required to submit a list to the USDA containing the CEP eligibility, CEP participation status, and ISP of every school providing meals through the national school lunch and breakfast programs. These data are then made publicly available by the Food Research and Action Center (FRAC) and Center on Budget and Policy Priorities (CBPP). With the data for schools in each state, I can identify not only which schools participated in the CEP, but their ISP and CEP eligibility status. I use a self-created fuzzy string matching algorithm to merge my CEP data set with the corresponding CCD identifiers based on school name, district name, and state.[12] Following the merger, each match was checked by hand to ensure accuracy. Finally, I merged the resulting school CEP data set back to the ECLS-K:2011 child-level data.

Table 1 shows summary statistics for my independent and dependent variables of interest along with the set of child and school covariates separated by eventual CEP participation status and the pre- and post-CEP period. Overall, the average child weight outcomes and rates of good and excellent health are generally similar between both groups during the pre-CEP period. Non-CEP schools have approximately 4 percentage points more healthy weight children, 4 percentage point fewer overweight children, and 3 percentage point fewer obese children relative to CEP schools. As expected, students attending CEP schools are more likely to live in a single parent household, be born to an unmarried mother, be black, and have a household income below 200% FPL. CEP schools are also more likely to be urban, less likely to be rural, and have higher percentages of non-white students on average.

[12] All fuzzy string matching was done in Python.

During the post-CEP period, the gap in average BMI between CEP participating and non-participating schools widens to roughly 0.8 BMI points. While both school types see a decrease in the average percentage of healthy weight students and an increase in the percentages of overweight and obese students, the relative difference across the groups is larger during the post-CEP period.

Finally, to see how the averages of my health outcomes vary across time for the set of students attending CEP and non-CEP schools, Figures 2 through 6 show unconditional trends in child BMI and each weight category indicator for both sets of schools across time. Starting with Figure 2, I find that pre-trends in average BMI were roughly equivalent between the two groups of schools. Figure 3 shows a considerable difference between the levels and trends of child underweight prevalence between the set of CEP and non-CEP schools. The magnitude of these changes is small, however, as a relatively small number of students are underweight at any one time and there is relatively little variation across time. In Figures 4, 5, and 6, I find similar pre-trends in rates of healthy weight, overweight, and obesity across the set of CEP and non-CEP schools with rates of healthy weight decreasing over time and rates of overweight and obesity increasing over time.

1.5 Methodology

The primary effect of interest for my study is the expected change in child weight caused by attending a CEP participating school. Naturally, a threat to proper identification of this effect is that CEP eligible schools serve higher numbers of students from disadvantaged backgrounds than their CEP ineligible counterparts. Estimating a simple regression of child weight on CEP school attendance would likely produce estimates which are biased by factors common to students who attend CEP eligible schools. More specifically, children attending CEP schools have substantially higher individual likelihoods of belonging to single parent households, having low household incomes, living in poorer neighborhoods, etc. Furthermore, since a school's ISP is largely determined by the share of students enrolled in government assistance programs, attending a CEP school increases the probability of a child participating in these same programs. If these factors are correlated with

CEP eligibility and child weight, then it is unlikely that estimates from a naïve regression would reflect the CEP's causal effects on weight.

To account for these sources of potential bias in my estimates, I utilize the longitudinal structure of the ECLS-K:2011 within a panel Difference-In-Differences framework. For child i attending school j in year t, weight outcomes are modeled such that:

$$Y_{ijt} = X_{ijt}\beta + \delta CEP_{jt} + \alpha_i + \lambda_t + \epsilon_{ijt} \tag{1.1}$$

where Y_{ijt} is either continuous child BMI percentile or binary indicators equal to 1 if a child is underweight, healthy weight, overweight, or obese and 0 otherwise, X_{ijt} is a vector of child- and school-level time varying covariates, CEP_{jt} is a binary indicator equal to 1 if the the child's school participated in the CEP during year t and 0 otherwise, α_i is a child-level fixed effect, λ_t is a year specific fixed effect, and ϵ_{ijt} is the model's idiosyncratic error term.

The effect of interest in equation (1.1) is δ which shows the expected change in each weight outcome caused by attending a CEP participating school during a post-CEP year. Since the ECLS-K:2011 is a longitudinal study, I am able to control for sources of time-invariant heterogeneity in each child's weight outcomes using my child fixed effects.[13] In cases where a child's level of disadvantage is relatively constant across time, my child fixed effects will remove their bias from my estimates of δ. In addition to my child fixed effects, I also control for observable covariates which change across time. Since household income level can vary across time, I include an indicator which is equal to 1 if a child's household income is below 200% of the federal poverty line and 0 otherwise as a control variable in X_{ijt}. This addition allows me to address the concern that my primary estimates are driven by large shifts in household income which are correlated with CEP participation rather than actual effect of CEP school attendance. Furthermore, while all students in my sample started Kindergarten during the same year, I control for variation in weight outcomes

[13]It is also the case that time-invariant factors at the school level may be correlated with CEP participation and child weight. This school-level time-invariant heterogeneity could be controlled for using school fixed effects. Since I observe children from Kindergarten until fifth grade, however, most students attend a single school during the entire survey period and adding school fixed effects to the model has a largely trivial effect on my results. I also show that my results are robust to removing students who changed schools at some point from my sample in Section 1.7.

related to differences in age by including child age in months in X_{ijt}. I also include if a school is Urban, if a school it Rural, and the percent of a school's students who are non-white in X_{ijt} to control for potential effects of school demographic characteristics on child weight. My year fixed effects account for potential changes in weight across time which are caused by unobserved factors that equally affect all students at the same time. For example, the HHFKA imposed a set of changes to minimum school meal nutrition standards prior to 2014 which took effect simultaneously for all schools across the country.

Estimating the causal effect of CEP school attendance on weight in my model relies on the assumption that both the timing of the program's introduction and each schools participation decision are independent of within-child variation in weight across time conditional on my child fixed effects, year fixed effects, and set of control variables. There are three primary cases where this assumption would not hold. The first is if the timing of the CEP's introduction was in response to differential trends in child weight among students attending CEP eligible schools. In this case, my estimates would conflate the effect of the program's introduction with pre-existing differences in the change of average student weight across time. This concern is lessened by the fact that I find generally similar unconditional pre-CEP period trends in the weight outcomes of children attending CEP and non-CEP schools in my sample. Furthermore, since the timing of the CEP's introduction was set in place prior to students in my sample starting Kindergarten, it is unlikely that relative trends in their weight would directly influence introduction timing.

The second threat is school migration in which families self-select into, or out of, CEP participating schools due to unobservable factors correlated with child weight. Given that the majority of students in my sample do not change schools during the survey period, school migration is unlikely to influence my results. The ECLS-K:2011 does provide information on if a child changes schools between years as well as the reason for the move. I test the sensitivity of my results to student migration in Section 1.7 by removing students who changed schools at any point during the survey period from my sample.

The final threat to proper identification of δ is the possibility that a school's CEP participation choice during the post-CEP period is influenced by unobserved factors which are also related to the weight of their students. For example, if schools are more likely to adopt the CEP because the weight of their students is growing faster than neighboring schools, then the estimated effect of attending a CEP school may be biased by the pre-existing differences weight trajectories. I use a number of techniques to evaluate the potential sensitivity of my estimates to bias from school-level self-selection on unobservables in Section 1.7. First, I follow Millimet and Tchernis (2013) and use each school's pre-CEP period observable characteristics to construct a sub-sample which minimizes the potential bias of self-selection on unobservables in my estimates of the average treatment effect on the treated. Second, I estimate my effects of interest using various specifications of CEP eligibility and ISP as instruments for CEP school attendance in a two-stage regression model. This approach allows me to estimate my results using the variation in CEP participation explained by the plausibly exogenous timing of the provision's introduction and ISP rather than self-selection into the program.

1.6 Results

I begin with my Difference-In-Differences (DID) estimates from the regression of child BMI percentile on CEP school attendance for the full sample of students. Table 2 shows the estimated effect of attending a CEP participating school in each post-CEP period year on BMI percentile with and without the set of child and school control variables and child fixed effects. In Column 1 of Table 2, I find that attending a CEP school increases expected BMI percentile by roughly 1.5 percentile points without child fixed effects or my set of control variables. With child and school control variables in Column 2 and the further addition of child fixed effects in Column 3, I find that CEP school attendance causes an expected increase in child BMI percentile of 1.35 and 1.22 percentile points, respectively. The effect is statistically significant in all cases below either the 5% or 1% level.

To provide context for my estimates, the average BMI of children attending CEP schools during the pre-CEP period is approximately 17.4. At an average height of 50.4 inches, an 8-year-old boy with a BMI of 17.4 would weigh 63 pounds. An increase of 1.5 BMI percentile points for the same child represents a weight gain of roughly 0.5 pounds, all else equal. While the magnitude of this effect is modest, it is important to note that my estimates capture the effect of CEP school attendance for a maximum of two years. If the CEP produces similar effects in subsequent years, then the program is expected to produce substantial increases in child BMI.

While I find evidence that CEP school attendance leads to an expected increase in child BMI percentile, an important factor is how these changes in BMI relate to movements within the underlying distribution of BMI, primarily at the thresholds of standardized weight categories. For example, if CEP school attendance only increases BMI among underweight students the policy may be thought to have an overall beneficial effect on child weight. Alternatively, if CEP school attendance leads to increases in the probability of a child being overweight or obese, then the program would have an overall detrimental effect on weight. I examine this relationship by regressing indicator variables of child underweight, healthy weight, overweight, and obesity on CEP school attendance in my DID framework.

Table 3 shows estimates for my regressions of each child weight category on CEP school attendance for the full sample of students. Each regression includes year fixed effects, the set of child and school covariates, and child fixed effects. My results suggest that each year of CEP school attendance decreases the probability that a student falls within the healthy weight range by 1.65 percentage points. The effect is marginally statistically significant at the 10% level. For the probabilities of overweight and obesity, I find that CEP school attendance leads to increases of 1.49 and 1.41 percentage points, respectively. The effect is marginally statistically significant at the 10% level for the probability of overweight and statistically significant at the 5% level for the probability of obesity. Alternatively, while the effect of CEP school attendance on underweight probability is positive, it is small and statistically insignificant. While CEP school attendance may truly have no effect on underweight probability, the lack of significance could also be the result of having so few underweight children in my sample. Since only 3% of all students in the ECLS-K:2011 are underweight at any given time on average, I would likely need a much larger sample size to detect an effect.

Given the possibility for significant increases in the percentage of students with BMIs above the healthy weight range, my results suggest that the CEP may lead to worsened child health as far as it relates to body composition. Furthermore, these estimates concern children who were exposed to the CEP for a maximum of two school years.[14] If students are expected to experience similar weight increases in each year following CEP adoption, the cumulative impact of attending a CEP school may produce substantial long-run changes in weight. Alternatively, it may be the case that the CEP's weight effects are temporary or that the initial weight changes stabilize following a child's initial exposure rather than continuing to increase BMI in a similar manner during each year. If the effects are temporary, then the provision's total impact on weight will likely be trivial in the long run. If the increases in weight caused by CEP stabilize, however, then we would likely see worsened health outcomes in the short and long run.

In addition to the effects of attending a CEP school on weight for the full sample of children, I also evaluate how these effects vary by child characteristics. Specifically, I estimate my results separately by gender, race, household income level during the pre-CEP period, and region. These sub-sample results allow me to detect differences in how the CEP affects child weight for various groups of students who likely have different pre-CEP period dietary environments.

Beginning with my results separated by gender, Table 4 shows estimates from the regression of child BMI percentile on CEP school attendance for boys and girls. Starting with male students in Panel A of Table 4, I find that CEP school attendance leads to small and largely statistically insignificant changes in BMI percentile. Alternatively, looking at the results for female students in Panel B of Table 4, my results show that attending a CEP school has a large and highly statistically significant effect on BMI percentile. With the set of child and school control variables and child fixed effects in Column 3, each year of CEP school attendance is found to increase the BMI percentile of girls by roughly 1.92 percentile points. This effect is both larger and statistically different from the corresponding estimate for boys of 0.54 percentile points, implying that the CEP

[14]Students living in CEP pilot states may have been exposed to the program in years prior to 2014. I examine the effect of excluding pilot state children from my sample in Section 1.7.

does have a differential effect on child BMI by gender.

The difference between my BMI percentile results by gender may be the result of differences in development rates between boys and girls (Rogol, Roemmich, and Clark, 2002). More specifically, if rates of physical or mental development during childhood vary by gender, I would expect to see differences in the effect of universal free school meals on BMI. Furthermore, it may be the case that boys and girls face different levels of stigma surrounding free and reduced-price school meal participation during the pre-CEP period. For example, if girls are more susceptible to the stigma of their peers, they may be less willing to participate in free or reduced-price school meals regardless of their availability. Following the removal of this stigma during the post-CEP period, a greater share of girls may switch to the now socially normative free meals relative to boys. Regardless, my estimates represent an initial evaluation of the CEP's weight effects by gender and additional research is needed to understand the mechanisms driving these effects.

Moving to the results of my weight category regressions by gender, Table 5 shows the effect of CEP school attendance on probabilities of underweight, healthy weight, overweight, and obesity for boys and girls. Beginning with boys in Panel A of Table 5, I find that attending a CEP school produces small and statistically insignificant effects for the probability of underweight, healthy weight, and overweight. Alternatively, the increased probability of obesity is economically significant at roughly 1.8 percentage points, but only marginally statistically significant at the 10% level. In Panel B of Table 5, I find that CEP school attendance leads to a substantial decrease in the probability of healthy weight of 3.3 percentage points for girls and an increase in the probability of overweight of 3.7 percentage points. Both effects are statistically significant below the 1% level. I do not, however, find a significant effect for the probability of obesity. Taken together, the results of Table 5 suggest that CEP school attendance leads to movement from the overweight to obese range for boys. The largest change for girls, however, comes from a movement out of the healthy weight range into the overweight range. Aside from effect magnitudes, I find that the CEP leads to worsened child weight outcomes for both genders.

I now examine my estimates of the CEP's effect on child weight separated by race. Because of differences in social and economic characteristics among children from different racial groups, I expect to find different effects of universal free school meals on their health. To estimate these race specific effects, I divide my sample into white and non-white students.

Table 6 shows the estimated effect of CEP school attendance on child BMI percentile by race. For white students in Panel A of Table 6, I find that attending a CEP school leads to positive and statistically significant changes in expected BMI percentile. With my set of control variables and child fixed effects in Column 3, CEP school attendance is found to increase BMI percentile among white students by 1.37 percentile points. While positive, I find smaller effects among non-white students in Panel B of Table 6. The effect is also marginally statistically significant with the addition of my control variables and child fixed effects. Comparing the coefficients from both groups, I do not find them to be statistically different from one another.

Table 7 shows the results from my regressions of child weight classifications by race. I find that CEP school attendance leads to decreases in the probability of healthy weight and increases in the probability of overweight and obesity for both groups, but the effects on healthy weight and overweight are only marginally statistically significant for non-white students and insignificant for white students. Similar to my BMI percentile results in Table 6, the coefficients of my weight classification regressions are not statistically different between white and non-white students. While not statistically different, Tables 6 and 7 provide limited evidence for white students being more effected by universal free school meals than non-white students. If white students were less likely to qualify or enroll in free school meals prior to the CEP, then I would expect to see larger changes in their weight following adoption of the provision. Nevertheless, additional work is needed to determine if there is, or is not, a difference in the CEP's weight effects by race.

I now examine my results separated by pre-CEP period household income level. More specifically, I separate my sample into the set of students whose household income was below 200% of the federal poverty line (200% FPL) at some point during the pre-CEP period and students whose household income was never below 200% FPL. I expect children in the never below 200% FPL group to have lower likelihoods of participating in government assistance programs during the pre-

CEP period, implying that the introduction of free school meals may have a greater impact on their weight outcomes. Furthermore, I expect children in the never below 200% group to have lower rates of participation in free and reduced-price school meals during the pre-CEP period since both the free and reduced-price school meal income eligibility thresholds are below 200% of the poverty line under the traditional system.

Table 8 shows the results from my BMI percentile regressions by pre-CEP period household income level. Beginning with children whose household income fell below 200% FPL at some point during the pre-CEP period in Panel A of Table 8, I find modest and largely economically insignificant increases in BMI percentile caused by CEP school attendance. Alternatively, I find that attending a CEP school leads to statistically significant increases in the BMI of children who were never below 200% FPL. These differences support my assumption that never below 200% FPL students will see larger changes in their weight following the CEP's introduction because they are less likely to qualify for free and reduced-price meals under the traditional system. Comparing the coefficients for both groups more formally, however, I do not find evidence that the effects are statistically different from one another with any sufficient degree of certainty. Again, future research is required to fully understand the differences in the CEP's effect on weight by pre-CEP period household income level.

I examine the effect of CEP school attendance on the probability of underweight, healthy weight, overweight, and obese by pre-CEP period household income level in Table 9. The results of Table 9 suggest that the effects are similar between both groups with the CEP leading to an increased probability of underweight, overweight, and obese, and a decreased probability of falling within the healthy weight range. In both cases, the coefficients are statistically insignificant.

In the final set of results, I estimate my models of child BMI percentile and weight classifications separately by region. Specifically, I separate my sample into the four Census Bureau regions of West, Midwest, South, and Northeast. Because of regional variation in social and political institutions prior to the CEP's introduction, I expect to find differences in how CEP school attendance affects child weight conditional on location. Furthermore, I expect the differences in CEP participation rate across the country to impact my results as well.

Table 10 shows the DID estimates from my regressions of child BMI percentile on CEP school attendance by region. My results suggest that there is a large degree of variation in the effect of attending a CEP school on BMI percentile contingent on where in the country a child lives. I find positive effects for children living in all regions, but only the coefficient for South is statistically significant. My estimate for children living in the South is also larger in magnitude at 1.27 BMI percentile points relative to the corresponding coefficient for the full sample of 1.22 points. Comparing the coefficient from my regression for Southern students to the other regions more formally, however, I do not find statistically significant differences.

Finally, Tables 11, 12, 13, and 14 show the effects of CEP school attendance by region on the probabilities of underweight, healthy weight, overweight, and obesity, respectively. As is the case with my other sub-sample results, I find no evidence of a statistically significant effect of attending a CEP school on the probability of underweight. Alternatively, Table 12 shows differences in the CEP's effect on the probability of healthy weight. Specifically, while I find negative effects for students in the West, Midwest, and South, I find that CEP school attendance leads to a statistically significant increase in the probability of healthy weight for students in the Northeast. Furthermore, the differences between the Northeast's effect and those of the other three regions are statistically different from one another. I also find a negative effect of CEP school attendance on the probability of being overweight for students in the Northeast while the effect is positive for students in the West, Midwest, and South. In Table 16, I find positive, but statistically insignificant, effects of attending a CEP school on the probability of obesity for students in all regions.

Taken together, my BMI percentile and weight classification results by region suggest that the increases in child BMI caused by CEP school attendance are accompanied by movements out of the healthy weight range and into the overweight range for students in the West, Midwest, and South. Alternatively, CEP school attendance in the Northeast leads to movement from the overweight range to the healthy weight range.

Some potential reasons for this regional variation are differences in socioeconomic, political, and demographic characteristics at the child, school, or societal level. Naturally, factors like race, income, education, etc. are likely to vary by region, but another important difference which could affect the impact of CEP school attendance is each region's pre-CEP period school meal environment. Looking at related data, I do find some key differences. During the 2017-2018 school year, schools in the South served the greatest share of FRP lunches and breakfasts at 82% of all meals served while schools in the Midwest provided the fewest at 68%.[15] Perhaps more applicable to my study, however, is that southern schools had the highest CEP participation rate, with 45% of all eligible schools enrolling in the provision in 2014. Given that more southern schools began offering universal free school meals through the CEP in 2014 and that the South also serves a highest proportion of FRP meals in years after, it seems intuitive that my primary results for the full sample of students are driven by children attending schools in the South, masking the potential benefits received by students in the Northeast. Evidence from future research examining the source of these regional differences will be important to policymakers as it can illustrate which parts of the provision should be adjusted so that it benefits students across the country rather than just those living in the Northeast.

1.7 Sensitivity Analysis and Robustness Checks

I now test the robustness and sensitivity of my results to multiple sample restrictions and alternative specifications designed to correct for sources of potential bias. First, I examine the effect that including children from pilot states has on my primary estimates. While the CEP took effect nationally during the 2014-2015 school year, it was piloted in 11 states at different points from the 2011-2012 to 2013-2014 school year. Since data on school CEP eligibility and participation during my sample period are only available for the 2014-2015 and 2015-2016 school years, I cannot identify the CEP eligibility or participation status of pilot schools during their state's pilot period. In my primary results, I assume that schools in all states were non-participants prior to 2014. While

[15]Data come from the Food Research and Action Center's "State of the States: Profiles of Hunger, Poverty, and Federal Nutrition Programs" report. The data can be downloaded directly from http://frac.org/research/resource-library/state-of-the-states-interactive-tables.

this assumption holds for children living in the 39 non-pilot states, students in pilot states may have been exposed to the CEP prior to the national rollout. As an alternative to including all students, I exclude children living in pilot states from my sample. This approach restricts my analyses to students in states which were first exposed to the CEP during 2014.

I expect that excluding pilot state students from my sample will produce larger estimates relative to my primary results regardless of the effect's direction. As an example, if attending a CEP school leads to increases in child weight, including children who attended CEP schools during the pilot period would likely bias my results downwards, providing estimates which are smaller than the true effect. The same is true if the CEP has a beneficial effect on weight, in which case I would also expect estimated effects to be smaller in magnitude. This may not be the case if my primary results reflect the effect of some unobservable factor rather than the true effect of CEP school attendance.

To test the impact of excluding children from pilot states, I estimate my models of child BMI percentile and weight categories using the set of children living in the 39 non-pilot states. Table 15 shows results from regressions of child BMI percentile on CEP school attendance for this non-pilot sample. Averaged across my specifications, I find that attending a CEP school during the post-CEP period leads to an increase in child BMI percentile of roughly 1.68 percentile points. Each coefficient is also statistically significant below the 1% level. Compared to my results for the full sample of children, the magnitudes of my estimates are larger after excluding pilot state students. The difference in effect magnitudes support my assumption that including pilot state students in my primary sample provides conservative estimates of the CEP's effect on BMI. While larger, I do not find the estimates for my non-pilot sub-sample regressions to be statistically different from one another with any sufficient degree of certainty.

Table 16 shows estimates from my child weight classification regressions without pilot state students. Like my results for BMI percentile, I find that excluding pilot state students increases both the magnitude and level of statistical significance for my DID weight category estimates, but the coefficients are not statistically different from those of the full sample.

Another potential threat to my identification strategy is student migration into, or out of, CEP schools. Specifically, if parents choose to attend or leave a school after adoption of the CEP due to factors related to the weight of their child, my results could be biased. For example, parents may see the CEP as a signal of poor quality and decide to migrate to a non-CEP school. If these parents are also more likely to be concerned with their child's weight, then I would potentially misidentify the effect of CEP school attendance. This concern is less pressing in my study since all students are in elementary school and generally expected to attend the same school during the entire sample period. Regardless, I evaluate the sensitivity of my results to migration by estimating my models of BMI percentile and weight classifications for the set of students who did not move schools at any point during the survey period.

Table 17 shows the results from my BMI percentile regressions for the sample of students who attended one school during the sample period. I again find that CEP school attendance leads to a statistically significant increase in BMI percentile even after the addition of my child and school covariates and child fixed effects. The coefficients are larger in my never moved sub-sample relative to the full sample results, but they are not statistically different from one another.

Table 18 provides coefficients from the regressions of my weight classification indicators on CEP school attendance for the sub-sample of students who attended one school during the entire sample period. Again, while I find larger effects from the restricted sub-sample regressions relative to my full sample results, they are not statistically different across specifications. Taken together, the results of Tables 17 and 18 suggest that my results are not sensitive to assumptions regarding the self-selection of parents into or out of CEP schools.

In my primary results, I compare students across the entire available distribution of ISP to one another. This specification implies that students attending schools with ISPs in the left tail of the distribution are compared to students at schools towards the opposite end of the distribution. Naturally, these students are not only likely to differ with respect to their probability of CEP exposure, but also in their personal and school characteristics. While my panel data DID estimates identify the CEP's effects off of within-child variation, it is possible that unobserved differences between very low and very high ISP schools are biasing my estimates and driving the primary results.

To evaluate the sensitivity of my results to the inclusion of students attending schools located in the tails of the ISP distribution, I restrict my sample to the interquartile range of ISP. This approach produces a sub-sample of students who attend more similar schools on average at the cost of halving my original sample size.

Table 19 shows the results of my BMI percentile regressions for the sub-sample of students attending schools within the ISP interquartile range. Compared to my full sample results, I find smaller coefficients for my restricted ISP sub-sample. The estimates remain statistically significant, with a marginally significant effect following the inclusion of my covariates and child fixed effects. This change may be driven by the exclusion of students attending schools in the tails of ISP, but it may also be the result of losing half my sample size. Regardless, I do not find that the coefficients are statistically different between the restricted ISP specification and my full sample results.

Table 20 shows the results of my weight category regressions for the set of students in my restricted ISP sub-sample. Similar to my full sample results, I find that CEP school attendance leads to increases in the probability of overweight and obese, and a decrease in the probability of healthy weight. Unlike my full sample results, however, I do not find the effects on probability of overweight or obese to be statistically significant. The estimated effect of CEP school attendance on the probability of healthy weight remains statistically significant for the restricted ISP sub-sample, but only marginally so at the 10% level. Comparing the coefficients from Table 20 to their full sample counterparts, I do not find that they are statistically different from one another with any sufficient degree of certainty. Taken together, the results of Table 19 and Table 20 suggest that my results are not entirely driven by the inclusion of students attending schools in the tails of the ISP distribution.

For my next test, I evaluate the sensitivity of my estimates to bias from school-level self-selection on time-variant unobservables which may influence both the CEP participation decision and a child's weight outcomes. My primary model assumes that the timing of the CEP's introduction and uptake is exogenous to each child's weight outcome trajectory. More simply, while children attending CEP schools are likely to differ from non-CEP students in several ways, I assume that the unobservable component of these differences within-child is uncorrelated with the timing of the CEP's introduction and each school's participation decision conditional on my set of fixed effects and observable control variables. This assumption is more likely to hold in my context since children do not directly decide whether or not to participate in the CEP, but it may be the case that CEP eligible schools take up the program because of unobservable factors related to the weight of their students. For example, if certain schools see that the BMI of their students is rising faster over time relative to neighboring schools, they may decide to adopt the CEP in an effort to address the trend. The potential correlation between time-variant changes in my outcomes and CEP participation could therefore conflate the effect of existing trends with the true effect of CEP school attendance, leading to possible bias in my estimates.

I use two methods to address the potential bias caused by school-level self-selection on unobservables. First, I follow Millimet and Tchernis (2013) and restrict my sample to the set of students that minimize the potential bias from self-selection on unobservables. I begin by collapsing my full data set by school ID, giving me school-level averages of each variable.[16] I then calculate a propensity score for the probability a school ever participates in the CEP during the post-CEP period using data from the pre-CEP period. The variables used to construct my propensity scores include the pre-CEP period growth rates of child BMI percentile, rate of underweight, rate of healthy weight, rate of overweight, and rate of obesity, percent of non-white students attending the school, average student household income level, total student enrollment, if the school is in an urban area, and if the school is in a rural area. Using the propensity score, $P(X_j)$, for school, j, based on the set of pre-CEP period observables, X_j, I restrict my sample to the set of students attending schools with propensity scores in the neighborhood around $P(X) = 0.5$ such that $P(X_j) \in (0.33, 0.67)$. Because the bias in the average treatment effect on the treated caused by self-selection on unob-

[16]The ECLS-K:2011 was designed such that multiple students in the survey attended each school with an average sample size of 23 students per-school. Therefore, aggregating my data set to the school level involves averaging the outcomes of multiple students rather than simply using data from a single child.

servables is minimized at $P(X) = 0.5$, this restriction significantly reduces the potential for error in my estimates.

Table 21 shows the results from my BMI percentile regressions using the minimum bias (MB) sub-sample. The results show that CEP school attendance leads to economically and statistically significant increases in expected BMI percentile. Compared to their full sample counterparts, the effect for my MB sub-sample are similar in both statistical significance and magnitude. One notable difference between the two specifications is sample size. After restricting my sample, I lose roughly 73% of my original sample size.

Table 22 shows the results of my weight classification regressions on CEP school attendance for children in the MB sub-sample. Like my full sample results, I find that attending a CEP school leads to increases in the probability of underweight, overweight, and obesity, and a decreased probability of healthy weight. The MB sub-sample coefficients are also similar in magnitude to my full sample estimates. Alternatively, I do not find the effects to be statistically significant. This lack of significance may be due to the reduction of some bias in my estimates caused by self-selection on unobservables, but it is likely the result of substantially reducing my sample size. Taken together, Table 21 and 22 show that the effects of CEP school attendance on my weight outcomes of interest are qualitatively similar in magnitude and direction for the MB sub-sample and full sample.

While restricting my sample is one option for minimizing the potential bias in my results caused by self-selection on unobservables, another is to estimate my effects of interest using the portion of variation in CEP participation explained by the plausibly exogenous timing of the program's introduction and CEP eligibility. Specifically, I use specifications of school CEP eligibility and ISP as instrumental variables (IVs) for CEP school attendance in two separate models. This approach allows me to estimate the effect of attending a CEP school on my child weight outcomes using the variation in CEP participation caused by each school's eligibility and ISP during the post-CEP period rather than self-selection into the provision.

I begin with a model using binary CEP eligibility as an instrument for participation. For child

i attending school j in year t, the binary eligibility model's first stage is given as:

$$CEP_{ijt} = X_{ijt}\gamma + \phi ELIG_{jt} + \alpha_{1i} + \lambda_{1t} + v_{ijt} \tag{1.2}$$

where $ELIG_{jt}$ is a binary indicator equal to 1 if school j is eligible for the CEP in year t and 0 otherwise, v_{ijt} is the model's error term, and all other variables hold the same interpretation given in (1). Estimation of my binary eligibility model entails replacing CEP_{jt} in (1) with the predicted value of CEP_{ijt} from (2).

While the binary eligibility specifications allows me to identify the effect of interest using variation caused by program eligibility and timing rather than self-selection, my instrument only captures variation at the extensive margin of eligibility. As discussed in Section 1.3, CEP participation varies within the set of eligible schools by ISP which determines meal reimbursement rates. To account for differences in participation caused by ISP, I also use a set of ISP interaction terms as instruments for CEP participation. The first stage of my model using the set of ISP interactions is given by:

$$CEP_{ijt} = X_{ijt}\gamma + \eta ELIG_{jt} * ISP_{jt} + \theta(ELIG_{jt} * ISP_{jt})^2 + \alpha_{1i} + \lambda_{1t} + v_{ijt} \tag{1.3}$$

where $ELIG_{jt} * ISP_{jt}$ is the interaction of school CEP eligibility status and continuous ISP and $(ELIG_{jt} * ISP_{jt})^2$ is the quadratic of the same interaction. Using the set of ISP interactions as instruments for CEP participation allows me to account for the extensive and intensive margins of eligibility as they relate to program participation. Furthermore, I am able to capture both linear and non-linear features of the relationship between CEP participation and ISP among the set of CEP eligible schools. These features give me a strong set of instruments which capture more of the variation in CEP school attendance than simply using binary CEP eligibility. Estimation of my model using the ISP interactions specification entails replacing the value of CEP_{jt} in (1) with the predicted value of CEP_{ijt} from (3).

Table 23 shows the results of my child BMI percentile regressions using binary eligibility and the set of ISP interactions as instruments for CEP participation.[17] Under both specifications, I find that attending a CEP school leads to a large statistically significant increase in expected BMI percentile. These effects are qualitatively similar to my primary results with regards to directionality, but both coefficients are notably larger than their DID counterparts. The same holds for the IV estimates of my weight category regressions presented in Table 24. I again find that attending a CEP school leads to an expected decrease in the probability of healthy weight and increases in the probability of overweight and obese under both instrument specifications. Like my BMI percentile results, I find that the IV estimates are significantly larger than their DID equivalents. Taken together, the results of Table 23 and 24 suggest that my primary results are somewhat sensitive to the use of instruments for CEP school attendance. However, while my IV estimates are larger than their DID counterparts, both sets of results tell the same story – attending a CEP school leads to non-trivial increases in expected BMI percentile and moves students out of the healthy weight range into the overweight and obese ranges.

1.8 Conclusion

In this paper, I estimate the effect of attending a school providing universal free meals through the Community Eligibility Provision (CEP) on child weight outcomes. To my knowledge, I am the first to do so using individual-level child data from a nationally representative sample. Unlike the traditional school meal system where children are required to qualify and apply for free and reduced price meals directly, the CEP allows schools and districts serving large numbers of students from disadvantaged backgrounds to offer free lunch and breakfast to their entire student body. The CEP has proven to be a popular program since its introduction in 2014 and nearly 10 million students in the U.S. now attend a CEP school. While the introduction of universal free school meals was meant to improve student health and reduce rates of childhood obesity, the provision's true effect on child weight is theoretically ambiguous. For example, students who switch from meals brought from home to free school meals under the CEP may experience different weight effects

[17] While not explicitly shown here, both instrument specifications have a strong first stage with F-stats far above the typical threshold of 10. Results can be provided upon request.

conditional on the relative quality of both meals. Given the number of students who now attend a CEP school and the program's potential impacts on child weight, it is especially important that we understand how universal free school meal availability affects student outcomes.

I use a panel Difference-In-Differences (DID) framework to estimate the effect of CEP school attendance on several child weight outcomes. My child-level data come from the Early Childhood Longitudinal Study, Kindergarten Class of 2010-11 (ECLS-K:2011), a longitudinal survey which follows a nationally representative sample of students starting Kindergarten during the 2010-2011 school year until fifth grade. My weight outcomes of interest include child Body Mass Index (BMI) percentile and binary indicators of underweight, healthy weight, overweight, and obesity. I also estimate heterogeneous treatment effects by gender, race, pre-CEP period household income level, and region. In addition to my primary analyses, I show that my results are generally robust to a number of different sensitivity tests designed to address specific sources of potential bias in my estimates.

I find that CEP school attendance increases expected child BMI percentile. My estimates for the full sample of students suggest that CEP school attendance corresponds to a weight gain of roughly 0.5 pounds for an 8-year-old boy of average height and weight. Looking to the effect of CEP school attendance on the probability of child underweight, healthy weight, overweight, and obesity, I find evidence that attending a CEP school leads to a decreased likelihood of healthy weight and increases in the probabilities of overweight and obesity. My estimates suggest that attending a CEP school decreases the probability of falling within the healthy weight range by 1.65 percentage points and increases the probability of overweight and obesity by 1.49 and 1.41 percentage points, respectively. In total, my results for the full sample of students suggest that attending a CEP school not only leads to significant increases in expected BMI percentile but higher likelihoods of child overweight and obesity. The CEP's overall detrimental effects on student weight stands in contrast to the policy's stated goals of reducing childhood obesity and improving child health.

In addition to my average treatment effects for the full sample of students, I examine the possibility that attending a CEP school produces different effects conditional on various child characteristics. Looking at my results by gender, I find large statistically significant effects from attending a CEP school on BMI percentile for girls but not boys. This difference suggests that introducing universal free school meals leads to larger changes in the BMI of female students on average. My child weight category regressions by gender suggest that female students are more likely to move from the healthy weight range to the overweight range while male students only see a statistically significant increase in the probability of obesity following adoption of the CEP.

Separated by race, I find that CEP school attendance leads to increases in child BMI percentile for both white and non-white students, with larger and more consistently significant effects for white students. Similar to my full sample results, I find that CEP school attendance leads to an increase in the probabilities of overweight and obesity for white and non-white students and a decrease in the probability of healthy weight. I do not, however, find the coefficients to be statistically different between both groups.

Looking at my results separated by pre-CEP period household income level, I find positive effects for children whose household income was never below 200% of the federal poverty line (200% FPL) and children whose household income was below 200% FPL at some point during the same period. My coefficients are larger in magnitude and more consistently statistically significant for children who never fell below 200% FPL. This difference supports my assumption that students with higher family incomes are more likely to see larger changes in weight following the introduction of universal free school meals. I again find that attending a CEP school leads to increases in the probability of overweight and obese and a decrease in the probability of falling within the healthy weight range, but the coefficients are statistically insignificant for both groups.

Separating my BMI percentile results by region, I find universally positive effects for students in the West, Midwest, South, and Northeast, but the effect is only statistically significant for students in the South. Looking to my child weight classification regressions by region, I find positive effects from the CEP on the probability of overweight and obesity and a negative effect on the probability of healthy weight for students in the West, Midwest, and South. Alternatively, I find that attending a CEP school leads to a statistically significant increase in the probability of falling within the healthy weight range and a decrease in the probability of being overweight for students in the Northeast. This is the only evidence of a beneficial effect from the CEP on child weight across all of my various specifications.

My results differ from those of Davis and Musaddiq (2019) who find that CEP participation decreases school-level average child BMI and increases the percentage of a school's students who fall within the healthy weight range. There are three potential reasons for this discrepancy. First, rather than relying on school-level averages, my data are at the individual child-level. While I largely find that attending a CEP school causes expected increases in BMI percentile and the probability of poor weight outcomes, the provision's aggregate effects could be negative if a sufficient number of students see a decrease in weight from the CEP. Second, Davis and Musaddiq (2019) use data from all K-12 schools while my data set only includes children in elementary school. If the effect of CEP participation on weight varies by child age, the aggregate effect of CEP school attendance may be different for the set of all K-12 schools than. Finally, Davis and Musaddiq (2019) examine the population of schools in Georgia while my sample includes children from nearly every state in the country. My regional analyses suggest that there is a considerable degree of variation in the effect of attending a CEP school by location. While it may be that the CEP improves average child weight outcomes in Georgia, the same may not be true in other areas. Unfortunately, I cannot recreate the authors' analyses since the ECLS-K:2011 does not include children from Georgia who attend a CEP school during the post-CEP period. These differences and their implications are covered more thoroughly in the following chapter.

While I find evidence to suggest that CEP participation comes at the cost of potentially worsened child weight outcomes among elementary school students, the program may still lead to an over all improvement in child health. Future work examining non-weight related health outcomes is needed. There is also significant room for work evaluating the CEP's impact on mental health outcomes at the child- and parent-level. If the CEP effectively removes stigma surrounding free and reduced-price meal participation and reduces rates or food insecurity, the program may produce improvements in mental health which are not reflected in its effects on weight. Furthermore, my study includes data covering two years after the CEP's national rollout. Future work focusing on the CEP's health effects across time will provide much needed information regarding the provision's long-run effects. Additional research is also needed to show how CEP school attendance affects individual child health for students of all ages. While I find that the CEP leads to detrimental weight changes, my effects only concern children in the final two years of elementary school.

Finally, a combination of further quantitative and qualitative research is needed to determine the pathways through which CEP school attendance affects child health. If the nutritional quality of school meals are driving my findings, then improvements in minimum nutrition requirements may eliminate or reverse the CEP's weight effects. Alternatively, if children attending CEP schools are eating both meals brought from home as well as their free school meals each day, steps could be taken to inform families and prevent substantive increases in calories consumed following the switch to CEP. I also show initial evidence for heterogeneous effects by various child characteristics. Examining the causes of these differences is a promising avenue for future research. Regardless of the mechanisms underlying my results, studies evaluating both the impacts and pathways through which universal free school meals affect child health are required to properly evaluate existing and future school meal policies.

Chapter 2

ESTIMATING THE EFFECTS OF UNIVERSAL FREE SCHOOL MEAL ENROLLMENT ON CHILD WEIGHT: EVIDENCE FROM THE COMMUNITY ELIGIBILITY PROVISION IN GEORGIA SCHOOLS

Note: This chapter represents coauthored work with my colleague, Tareena Musaddiq. Additionally, Chapter 2 does not include the literature review and policy sections found in the initial study since they are covered in Chapter 1.

2.1 Introduction

During each academic year, children in the United States recieve between one-third and one-half of their daily calories from meals eaten in school (Schanzenbach, 2009, Briefel et al., 2009). The majority of these meals come from subsidized school meal programs like the National School Lunch Program (NSLP) and School Breakfast Program (SBP). The NSLP alone is the nation's second largest nutrition assistance program and provides school lunch in 95 percent of public schools at an annual cost of $13.6 billion dollars (FRAC, 2017). Both the NSLP and SBP also offer school meals at a free or reduced-price to students from low-income households. Of the 44.6 million children who participated in school meals during 2016, roughly 75 percent received meals either for free or at a reduced price (USDA, 2017a, USDA, 2017b). These school meals represent a critical source of nutritious and consistently available food for many of the nation's disadvantaged children. For the most at-risk students, school meals may make the difference between going hungry and having the food necessary for successful learning and development (NKH, 2017).

As discussed in Chapter 1, the Healthy Hunger Free Kids Act's (HHFKA's) Community Eligibility Provision (CEP) led to a considerable change in how America's low-income schools provide meals to their students. In this study, we estimate the CEP's effect on school-level aggregate measures of child weight among the population of K-12 schools in the state of Georgia. More specifically, we estimate models of school-level student weight outcomes using different specifications of CEP eligibility as instruments for CEP participation. Our approach allows us to estimate plausibly causal effects of providing universal free meals to students in public schools on aggregate weight measures. We provide separate estimates for the full sample of K-12 schools, elementary schools, middle schools, and high schools. We also estimate our results separately for schools in urban areas, rural areas, and suburbs/towns in Georgia.

Our results suggest that CEP participation increases the percentage of students who fall within the healthy weight range and reduces average Body Mass Index (BMI) scores for K-12 schools in the state of Georgia. This stands in contrast to my findings in Chapter 1 and those of some other studies that find free and reduced-price school meals provided under the traditional system lead to worsened weight outcomes. We find no statistically significant evidence of similar changes following CEP participation. We find that CEP participation is expected to increase the number of healthy weight students attending a school by 13 for the full sample and decrease average BMI by approximately 1 percentage point. Looking at our results by grade type, we do not find statistically significant effects on the aggregate weight outcomes of high schools, implying that CEP participation leads to smaller changes in the average weight outcomes of schools serving older children.

We also find that CEP participation leads to statistically significant increases in the percentage of healthy weight students attending urban and rural schools, but we do not find significant impacts on the healthy weight percentage of schools located in suburbs and towns. Furthermore, while rural schools are located in Georgia's poorest counties and provide the fewest free and reduced-price meals during the pre-CEP period, urban schools are far more likely to participate in the CEP conditional on eligibility. This runs contrary to the CEP's primary goal of targeting schools where students were inadequately covered by the existing free and reduced-price meal system. Given the potentially beneficial weight effects of CEP participation found in our results, differences in take-up by location type may create or worsen area-level child health disparities in Georgia if the CEP cannot be made effective, feasible, and attractive to all schools.

Comparing this study to the study presented in Chapter 1 highlights interesting similarities and differences. First, both studies estimate the CEP's effect on child weight, but at different levels and for different groups of students. While the estimates of Chapter 1 come from a nationally representative sample of students in late elementary school, this study uses data from the universe of K-12 schools in Georgia. Therefore, while Chapter 1 is likely to have greater external validity, Chapter 2 covers students of different ages at all schools in one state. These differences affect what can be learned from each study, a topic I will cover in more detail later in this chapter, but comparing the results of Chapter 1 and 2 highlights the potential differences in how the CEP affects child weight for students of different ages in different areas of the country.

2.2 Data

Our study utilizes several sources of data from K-12 schools in Georgia over the 2011-2012 to 2016-2017 school years. The data set contains variables related to school-level average child weight outcomes, Identified Student Percentage (ISP), CEP participation and eligibility, and student sociodemographic characteristics. Data on weight outcomes come from the FitnessGram. Each year, physical education instructors in Georgia public schools are required to administer the FitnessGram; a collection of tests which measure the physical fitness, height, and weight of students attending each school. FitnessGram data aggregated at the school-level are publicly available for our sample period through the Georgia Department of Education (GaDOE).[1] Our primary outcomes of interest from the FitnessGram relate to child body composition, namely average child Body Mass Index (BMI) score and the percentage of children who are of a healthy weight.

Unlike the child-level data used in Chapter 1 which could be used to calculate child BMI percentile, changes in BMI at the school-level are difficult to interpret and compare across schools

[1] Data can be found on the GaDOE website for the 2011-2012, 2012-2013, 2013-2014, and 2014-2015 school years: http://www.gadoe.org/Pages/Home.aspx. Data for the 2015-2016 and 2016-2017 school years were obtained through an open data request.

serving children of different ages. One contributing reason is that only observing a change in the mean provides no information as to where in the weight distribution the change is taking place. For example, obese or underweight children losing weight can cause an identical decrease in mean BMI with obviously different implications for overall student health. The second issue is that child BMI score interpretations vary considerably by age and gender.

To remove some of this ambiguity, we primarily focus on another FitnessGram variable showing the percentage of children at each school who fall "In the Healthy Fitness Zone" (InHFZ%) for BMI. A child is considered in the BMI healthy fitness zone if their score falls within the 5th and 85th percentile range for their age and gender as determined by the Centers for Disease Control and Prevention (CDC).[2] Therefore, InHFZ% is equivalent to the percentage of healthy weight children attending a school. Unlike mean BMI, changes to InHFZ% have direct implications for child health. An increase (decrease) in InHFZ% relates to an improvement (worsening) of school-level health regardless of where in the weight distribution the change occurs. Going one step further, the combination of changes to mean BMI and InHFZ% suggests additional information. If mean BMI decreases and InHFZ% increases, then the dominant change in weight likely comes from overweight or obese children losing weight and moving into the healthy weight range. This interpretation does not rule out the possibility of concurrent weight changes elsewhere in the BMI distribution, but it does allow us to identify probable locations of a change.

Similar to Chapter 1, we collect CEP data for the 2014-2016 school years from the Center on Budget and Policy Priorities (CBPP) who gathers and provides the data in a joint effort with the Food Research and Action Center (FRAC).[3] The USDA began requiring that each state submit a list containing the CEP eligibility, participation status, and ISP of all applicable schools and districts in 2014. Unfortunately, even though Georgia was a CEP pilot state during the 2013-2014 school year, information is only available for the 2014-2015 school year onward. To account for this limitation, the 2013-2014 school year is excluded from our primary analysis. We test the sen-

[2] *See* Plowman and Meredith, 2013.

[3] Data are available through the CBPP's website: https://www.cbpp.org.

sitivity of our results to this assumption in Section 2.5.[4]

Data used to identify each school's location type come from the National Center for Education Statistics' (NCES') Common Core of Data (CCD).[5] Schools are categorized as either urban, rural, or suburb/town. We also collect school-level revenue, expenditure, and student sociodemographic data for the entire analysis period through the Governor's Office of Student Achievement (GOSA).[6] Finally, county-level data on poverty percentages by age range and median household income for each year come from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) program.

Summary statistics for the dependent variables of interest, independent variables of interest, and control variables are presented in Table 25. As Table 25 shows, the mean BMI for schools in our sample is approximately 20.35. Unlike adult BMI which has a consistent interpretation across age and gender, a child BMI score of 20.35 falls within the obese weight range for a six-year-old boy and the healthy weight range for a 14-year-old boy. As an alternative view of child weight, our InHFZ% variable shows that roughly 58.88% of Georgia students fall within the healthy weight range during the sample period, implying that 41.12% of children are some combination of underweight, overweight, and obese. Our "Ever CEP Eligible" variable shows that roughly 47% of Georgia's K-12 schools were eligible for the CEP at some point during the 2014-2015 to 2016-2017 period. Our "Ever CEP Participating" variable indicates that approximately 26.87% of all schools participated in the CEP at some point during the same period, giving us a CEP take-up rate of roughly 57% among eligible schools. Our "ISP if CEP Eligible" variable shows that the average ISP of CEP eligible schools is roughly 55.96%. Alternatively, our "ISP if CEP Participant" variable shows that the average ISP for schools that participate in the CEP is roughly 60%, further suggesting a positive correlation between ISP and the CEP participation rate. The final set

[4]Removing Georgia's CEP pilot period from the sample differs from the approach taken in Chapter 1. In Chapter 1, I had nationally representative data from students in nearly all states. Given that only 11 states had CEP pilot periods, the majority of my states and students did not had the CEP until the national rollout in 2014-2015. Alternatively, Chapter 2 uses data only from the state of Georgia in which case all schools had a pilot state period, making the threat to identification caused by leaving in 2013-2014 more acute. Regardless, I test both the sensitivity of my results to the inclusion/exclusion of the pilot state period observations in both chapters.

[5]Data are available directly through the CCD website: https://nces.ed.gov/ccd/pubschuniv.asp.

[6]Data are available directly from the GOSA website: https://gosa.georgia.gov/downloadable-data.

of control variables used in our analyses include: Percent Black Students, Percent White Students, Percent Migrant Students, Percent Special Education Students, Percent English as a Second Language (ESL) Students, and Percent Gifted Students.

Figures 8 and 9 provide graphical illustrations of the across-year change in InHFZ% and mean BMI for the group of ever CEP eligible and never CEP eligible schools. Figure 8 shows that there is a considerable difference between the average InHFZ% of never CEP eligible and ever CEP eligible schools, with the never eligible group having a higher average percentage of healthy weight students in all years. We also see that the InHFZ% of both groups increased non-trivially beginning in the 2015-2016 school year. The bulk of this increase is due to a widening of the CDC's healthy weight thresholds in 2015 which lead to a greater number of children falling within the 5th-85th percentile range. Since this change to InHFZ% affects all students and schools simultaneously, any impact of the measurement change on our results should be removed through the use of year fixed effects. We test this assumption explicitly in Section 2.5.[7]

Finally, Figure 9 shows us that the average BMI of ever CEP eligible schools is higher than that of never eligible schools during both the 2011-2012 and 2012-2013 school years. The average BMI of both groups saw comparable decreases during this period, suggesting that both sets of schools had similar pre-trends in child weight during our two pre-CEP periods. The reason for this decrease may be due to improvements in school meal minimum nutrition standards caused by the HHFKA directly before and during the pre-CEP period. Interestingly, the average BMI level of CEP eligible schools begins to fall below that of never eligible schools starting in the 2013-2014 school year which is when Georgia implemented the provision as a pilot state prior to the national roll-out in 2014-2015. The average BMI of both school types continues to decrease during 2014-2015 and 2015-2016, but increases in 2016-2017 to roughly their 2014-2015 levels.

[7]One case where the healthy weight threshold change would bias our analyses is if either the CEP eligible or CEP ineligible group of schools had a disproportionate number of students with BMIs just outside the pre-2015 healthy weight range, implying that more students would fall within the new threshold relative to the other group of schools. To test this, we estimate our primary results for InHFZ% excluding the 2015-2016 and 2016-2017 school years. We do not find that this fundamentally changes our results.

2.3 Methodology

To eliminate potential biases caused by the self-selection of schools into the CEP, we use two different specifications of CEP eligibility as instrumental variables for CEP participation in a two-stage linear regression model. Unlike Chapter 1 where students did not directly make the CEP participation choice, the threat to identification from self-selection on unobservables is much more salient when using school-level data. Therefore, we focus on an IV approach rather than a difference-in-differences approach.[8]

We begin with a model using binary CEP eligibility as an instrument for CEP participation. The first stage of our model under the binary eligibility specification for school i in year t is given by:

$$CEP_{it} = Z_{it}\gamma + \phi ELIG_{it} + \alpha_{i1} + \lambda_{t1} + v_{it} \tag{2.1}$$

where CEP_{it} is equal to 1 if school i participates in the CEP during year t and 0 otherwise, Z_{it} is a vector of time-variant control variables, $ELIG_{it}$ is a binary variable equal to 1 if school i is eligible to participate in the CEP during year t and 0 otherwise, α_{i1} captures school-level sources of time-invariant unobserved heterogeneity, λ_{t1} captures year-level sources of unobserved heterogeneity, and v_{it} is the model's normally distributed idiosyncratic error term. The primary effect of interest in equation (2.1) is ϕ which gives us the estimated effect of being CEP eligible on a school's probability of participating in the CEP during the same year, all else unchanged.

The binary eligibility specification above predicts CEP participation using plausibly exogenous variation in program timing and eligibility rather than other unobserved factors affecting self-

[8]As discussed in more detail in Chapter 1, CEP eligibility is technically determined by a discontinuity in ISP at 40%, implying that a regression discontinuity (RD) design would seem like a natural approach for our analyses. Unfortunately, using an RD model is not well suited in this context. This is because few schools with ISPs just at or above the 40% level participate in the CEP due to USDA's meal reimbursement rates. The inverse relationship between ISP and reimbursements combined with our limited sample size gives us too few CEP participating schools just above the threshold to precisely estimate an RD with school-level data from only one state. Furthermore, variation in our weight outcomes at the eligibility threshold is driven by the underlying CEP participation rate. Since the set of barely eligible schools for which the the barriers to participation do not prevent enrolling in the provision are likely different from the average school just below the eligibility threshold, this may lead to improper estimates of the treatment effect.

selection. The primary assumption supporting the validity of this approach is that, conditional on our control variables and set of fixed effects, the timing of the CEP's introduction is independent from changes in student characteristics that our correlated to our weight outcomes of interest.[9] However, our binary specification only considers the extensive margin of CEP eligibility. The assumption that CEP participation in a certain year depends solely on a school's eligibility status abstracts from the non-linear relationship between CEP participation and ISP discussed in Chapter 1.

In order to allow CEP participation to vary non-linearly with CEP eligibility and ISP, we also use an alternative specification in our model's first stage such that for school i in year t:

$$CEP_{it} = Z_{it}\gamma + \phi(100 - ISP_{it}) * ELIG_{it} + \eta((100 - ISP_{it}) * ELIG_{it})^2 + \alpha_{i1} + \lambda_{t1} + v_{it} \tag{2.2}$$

where ISP_{it} is the Identified Student Percentage (ISP) of school i in year t and all other terms hold the same definition they are assigned in equation (2.1). In addition to isolating the variance in CEP participation caused by eligibility, the specification given by equation (2.2) captures two features present in the relationship between ISP and CEP participation. First, the effect of CEP eligibility on participation is allowed to vary linearly with its running variable, ISP. Second, our ISP interaction specification allows for a non-linear effect of CEP eligibility interacted with ISP on CEP participation. With this feature, increases in ISP among CEP eligible schools may raise or lower the probability of participation depending on a school's initial identified student percentage.

In our model's second stage, we utilize the variation in CEP participation caused by CEP eligibility and ISP using one of the two first stage specifications discussed above to construct instrumental variable estimates of CEP participation's effect on school-level weight such that for school i in year t:

$$Y_{it} = Z_{it}\beta + \delta CEP_{it} + \alpha_{i2} + \lambda_{t2} + e_{it} \tag{2.3}$$

[9] We test the validity of this assumption in Section 2.5 using a pre-CEP period placebo test.

where Y_{it} is either InHFZ% or average BMI, CEP_{it} is CEP participation, and all other variables hold the same interpretation given in (2.1) and (2.2). Estimation of (2.3) involves replacing CEP participation with its predicted value from our first stage regression using either the binary eligibility or ISP interaction specification given by equations (2.1) and (2.2), respectively.

The primary coefficient of interest in (2.3) is δ which represents the expected change in InHFZ% and average BMI caused by CEP participation conditional on the model's other covariates. Consistent estimation of δ faces two primary challenges. The first problem which was mentioned above is the potential for bias caused by self-selection into the CEP based on unobservable factors related to student health. More succinctly, if CEP eligible schools participate in the provision because of unobserved factors that also affect the weight of their students, then our estimates of δ will be inconsistent. We address this concern by instrumenting for CEP participation using our specifications of CEP eligibility. This approach allows us to estimate our effect of interest in our model's second stage using variation in CEP participation caused by timing of the CEP's introduction and eligibility for the provision rather than the set of unobservables determining self-selection. We test this assumption more thoroughly in Section 2.5.

The second challenge to consistent identification of δ is that CEP eligibility is determined by ISP, implying that CEP eligible schools necessarily serve a greater number of disadvantaged students who are enrolled in government assistance programs during all periods. Since our outcome of interest is at the school-level, we are able to make two reasonable assumptions to control for the effect of these programs on weight using school and year fixed effects. Our first assumption is that the proportion of students participating in government assistance programs changes relatively little for each school over our sample period. Under this assumption, we are able to isolate the effect of CEP participation on our aggregate weight outcomes net of time-invariant differences in student government assistance program participation using school fixed effects. Furthermore, we use year fixed effects to control for potential variation in government assistance program participation rates caused by any national or state level changes which simultaneously affect all schools in our sample.

2.4 Results

We use two different instrumental variable (IV) specifications in our model's first stage to produce our primary results.[10] First, we use a binary indicator of CEP eligibility during each post-CEP school year as an instrument for CEP participation. Panel A of Table 26 shows the estimated effect of this binary eligibility instrument on CEP participation from our model's first stage for the full sample of schools along with separate estimates for elementary schools, middle schools, and high schools. We find that CEP eligibility is highly predictive of CEP participation with coefficients that are statistically significant below the 1% level for the full sample of schools and all grade types. Being eligible for the CEP during the post-CEP period increases the likelihood of participation by roughly 46.7 percentage points for the full sample of schools conditional on our set of control variables, school fixed effects, and year fixed effects. Looking at the results of our first specification by grade type, we find that CEP eligibility has the smallest effect on CEP participation among elementary schools at 41.7 percentage points compared to 52.5 and 60.5 percentage points for middle and high schools, respectively.

In the second specification of our model's first stage, we instrument for CEP participation using (100-ISP) interacted with the binary CEP eligibility indicator from our first specification. We also include the square of the same term to account for nonlinear effects. Specifically, this specification allows us to capture nonlinearities in the effect of CEP eligibility and its running variable, ISP, on CEP participation which are discussed in Chapter 1. Panel B of Table 26 shows our first stage estimates under the ISP interaction specification for the full sample of schools along with separate estimates for elementary, middle, and high schools. For the full sample of schools, the positive coefficient on our ISP interaction combined with the negative coefficient on the squared interaction term imply that the probability of CEP participation is initially increasing in ISP, but at a point the effect becomes negative. The turnaround point for our full sample occurs at a (100-ISP) of 29 which corresponds to an ISP of roughly 71%. At an average ISP of 56% for CEP eligible schools, a

[10]Since our analyses are at the school-level, we weight all regressions by school student population in the 2014-2015 school year. Estimates our models using student population in 2015-2016 or 2016-2017 as regression weights does not fundamentally change our results.

1 percentage point increase in ISP would lead to a 3.13 percentage point increase in the probability of CEP participation. Similar to the case with our binary CEP eligibility specification, we find that the effect of our ISP interaction instruments on CEP participation is smaller for elementary schools relative to both middle and high schools. The F-stat of each regression shows that our instruments are well powered to estimate the effects of interest in our model's second stage.

We use the specifications above to estimate the impact of CEP participation on two outcomes of interest: the percent of students attending a school who are of a healthy weight (InHFZ%) and average school-level BMI. Beginning with our binary CEP eligibility specification, Table 27 shows the estimated effects of CEP participation on InHFZ% and average BMI for the full sample of schools as well as separately for elementary schools, middle schools, and high schools. Beginning with the full sample of schools in Panel A of Table 27, we find that CEP participation leads to a statistically significant increase in the percentage of healthy weight children attending a school of roughly 1.8 percentage points relative to non-participating schools. At the average student population of 719 and average InHFZ% of 58.8% among the set of CEP eligible schools, our binary specification results suggest that participation in the CEP is expected to increase the number of healthy weight students attending a school by 13. Looking at our binary specification results by grade type, we see that CEP participation leads to positive effects on the InHFZ% of elementary, middle, and high schools. While, the coefficients are statistically insignificant for each of the three grade types. The lack of statistical significance for our results by grade type is potentially caused by the reduction in sample size, but it may also be the case that CEP participation only leads to more healthy weight children at the aggregate K-12 level.

Panel B of Table 27 shows the estimated effect of CEP participation on average BMI using our binary CEP specification. We find that CEP participation leads to a statistically significant decrease of 0.197 BMI points in school average BMI for the full sample of K-12 schools. Looking at our results by grade type, we find that CEP participation leads to lower average BMI scores for elementary, middle, and high schools, but the effect is only statistically significant for elementary schools.

Moving now to the primary results of the model using our ISP interaction terms as instruments for CEP participation, Table 28 shows the estimated effect of CEP participation on InHFZ% and average BMI for the full sample of schools as well as by school grade type. Beginning with Panel A of Table 28, we find that CEP participation leads to a statistically significant increase in a school's expected percentage of healthy weight students of around 1.3 percentage points for the full sample of schools. While smaller than the effect we find under the binary eligibility specification, our results indicate that CEP participation is expected to increase the number of healthy weight students attending an average CEP eligible school by 9. Examining our results by school grade type, we find that CEP participation leads to a statistically significant 2.29 percentage point increase in the InHFZ% of middle schools. The average middle school size and InHFZ% among CEP eligible schools is 796.4 and 53.66% respectively, implying that participating in the CEP is expected to increase the number of healthy weight students attending an average CEP eligible middle school by roughly 18 students. While positive, the effect of CEP participation on InHFZ% is statistically insignificant for elementary schools under our ISP interaction specification. Alternatively, the effect for high schools is found to be negative under our ISP interaction specification, but the effect is close to zero with a relatively large standard error.

Looking at Panel B of Table 28, we find that CEP participation is expected to reduce a school's average BMI by roughly 0.1 BMI points for the full sample of schools under our ISP specification. While statistically significant and positive, using our ISP interaction terms as instruments for CEP participation gives us an estimate nearly half the size of our results found using binary CEP eligibility. Looking to our results by school grade type, we find that CEP participation leads to decreases in school-level average BMI for elementary, middle, and high schools, but the effect is only statistically significant for middle schools and marginally so.

Taken together, the results of Tables 27 and 28 for our full sample of schools suggest that the expected change in InHFZ% and average BMI following CEP participation are most likely driven by overweight and obese children losing weight and falling into the healthy weight range. If it is the case that changes in the percent of healthy weight students are driven by underweight students gaining weight and moving into the healthy weight range, then we would not expect to see negative effects of CEP participation on BMI. Alternatively, if the estimated decrease in average BMI is driven by healthy weight or underweight children losing weight, it would not likely be accompanied by an increase in InHFZ%. While it is entirely possible that participating in the CEP also leads to variation elsewhere in the distribution of child weight, we believe that weight loss among overweight and obese children following adoption of the CEP is the most plausible explanation of our findings.

In addition to potential heterogeneity in the effect of CEP participation on child weight by school grade type, we also expect that CEP participation may have differential impacts among schools in different location types. To test for this location specific heterogeneity, we estimate our primary results separately for schools in urban areas, rural areas, and suburbs/towns. The primary reasons why we would expect the relationship between school meals and child health to differ for schools in different areas *a priori* relate to area specific trends in factors like food insecurity and institutional beliefs and practices. For example, children attending a low-income urban school may be more likely to live in a food desert, implying that the nutritional quality of meals may be the most crucial component of school meals rather then their caloric content. Alternatively, families in rural areas may be less likely to enroll their child in a nutrition assistance program due to stigma or personal beliefs regarding government assistance programs.

Table 29 shows the results of our first stage estimates separated by school location type under the binary CEP eligibility specification and the ISP interaction specification. Beginning with Panel A of Table 29, we find that CEP eligibility leads to positive and statistically significant increases in the probability of participation for schools in all location types. Urban schools see a substantially larger effect at 73.1 percentage points compared to 48.5 percentage points and 37.8 percentage points for rural schools and schools in suburbs/towns, respectively. This difference is likely the result of urban schools having higher average ISPs than rural schools or schools in suburbs/towns, making them more likely to participate in the CEP. Less clear is why the estimated change in the probability of CEP participation caused by CEP eligibility is 22% higher for rural schools relative to schools in suburbs/towns given that both sets of schools have roughly the same average ISP among the set of eligible schools at 53.6% and 54.1%, respectively. Looking at Panel B of Table 29, we find that rural schools are more reactive to changes in ISP relative to schools in suburbs/towns with respect to CEP participation. This differential may explain why CEP eligible rural schools are found more likely to participate in the provision under our binary CEP eligibility specification relative to schools in suburbs/towns.

Table 30 shows our primary results under the binary CEP eligibility specification separated by school location type. In Panel A of Table 30, we find that CEP participation leads to positive effects on the percentage of healthy weight students attending urban schools, rural schools, and schools in suburbs/towns. The effect is only found to be statistically significant at the 10% level for urban and rural schools, but statistically insignificant for schools in suburbs/towns. We find the largest effect among urban schools where CEP participation is estimated to increase the percentage of healthy weight students attending a school by 2.54 percentage points relative to the 1.94 percentage point increase for rural schools. Given the average number of students and InHFZ% of urban and rural CEP eligible schools, CEP participation is expected to cause roughly 16 more children to fall into the healthy weight range for urban schools and 13 students in rural schools. While statistically insignificant, the effect for schools in suburbs/towns indicates that roughly 13 additional students will fall into the healthy weight range following adoption of the CEP. While the implied number of students is the same for rural schools and schools in suburbs/towns, the overall percent change is smaller for suburbs/towns since they are larger on average relative to their rural counterparts.

Panel B of Table 30 shows the primary results for average BMI under the binary eligibility specification separated by school location type. We find that CEP participation leads to an expected decrease in average BMI for schools in all location types. Surprisingly, the effect is statistically insignificant for urban schools and only significant at the 10% level for rural schools, but statistically significant below the 1% level for schools in suburbs/towns. We also find that the effect for schools in suburbs/towns is significantly larger in magnitude compared to our estimates for urban and rural schools.

Table 31 shows our primary results for InHFZ% under the ISP interaction specification separated by school location type. Beginning with Panel A of Table 31, we again find that the CEP participation leads to increases in the expected percentage of healthy weight students attending a school for schools in all location types. We find the effect of CEP participation on InHFZ% to be statistically significant below the 1% and 5% levels for urban and rural schools, respectively, and statistically insignificant for schools in suburbs/towns. Compared to our results using the binary CEP eligibility specification, we see a substantial increase in the statistical significance of our estimates under the ISP interaction specification for urban and rural schools. Furthermore, we find a meaningful increase in the effect's magnitude for urban schools and a decrease in the effect size for rural schools. While we expect urban schools to see roughly 16 more healthy weight students following adoption of the CEP under our binary eligibility specification, we estimate that an additional 23 students will be within the healthy weight range using our ISP interaction specification. Alternatively, the same increase for rural schools goes from 13 additional students under the binary specification to 10 using our ISP interactions as instruments for CEP participation.

Finally, Panel B of Table 31 shows the results of our model for average BMI under the ISP interaction specification separated by school location type. While the effect of CEP participation on average BMI is negative for schools in all location types, the effects are statistically insignificant. These results stand in contrast to those of our binary eligibility specification in Panel B of Table 30. Compared to our ISP interaction specification estimates, we find larger effects of CEP participation on BMI for rural schools and schools in suburbs/towns. While we find no statistically significant effect using our ISP interaction instruments, we find the effect of CEP participation on average BMI to be statistically significant for rural schools and schools in suburbs/towns using our binary specification at the 10% and 1% levels, respectively.

2.5 Sensitivity Analysis

We now test the validity of our estimation strategy and the sensitivity of our results presented in Section 2.4. First, we perform a placebo test using data from the pre-CEP period. In placebo testing, the primary analysis is replicated using a pseudo outcome that is expected *not* to be affected by the treatment (Athey and Imbens, 2017). In other words, the true value of the point estimate for the pseudo outcome should be zero. Rejecting the null hypothesis in this case would bring the credibility of our original analysis into question. While various pseudo outcomes can be tested, we use variables related to future CEP eligibility and participation as independent variables of interest in models of pre-CEP period outcomes.

Our falsification test involves designating the group of schools that were eligible to participate in the CEP at some point during the 2014-2016 period and indicating the 2012-2013 school year as a false post-treatment period. We then perform the placebo test with a difference-in-differences (DID) model of our aggregate weight outcomes using data from the 2011-2012 and 2012-2013 school years. Our approach can be likened to comparing the pre-CEP period trends of InHFZ% and mean BMI for the groups of ever and never CEP eligible schools. Finding an effect from future CEP eligibility during the pre-CEP period would suggest that trends in the aggregate weight outcomes of interest differed by CEP eligibility prior to the provision's introduction, implying that our estimates may not represent valid treatment effects.[11]

Table 32 shows the results of our placebo test for the full sample of schools and by school grade type using future CEP eligibility status as a false treatment indicator during the pre-CEP period. We find no statistically significant effect from future CEP eligibility on either InHFZ% or average BMI during the pre-CEP period. The results of our placebo test suggest that, conditional on our model's other covariates, the trends in our aggregate weight outcomes were not statistically different between the set of CEP eligible and ineligible schools prior to the provision's introduction. This supports our approach using plausibly exogenous timing in CEP eligibility as an instrument for CEP participation. Furthermore, since the estimated effect of future CEP eligibility on InHFZ% is negative during the pre-CEP period, the positive effect of CEP participation we find in our primary results are likely conservative. We similarly find positive coefficients on future CEP

[11] While this approach compares trends in our outcomes of interest between the CEP eligible and non-eligible groups, it is limited by the fact that we only have two pre-CEP school years. More pre-CEP period data would be preferred, but unfortunately data only go back to the 2011-2012 school year.

eligibility in our placebo test of BMI for elementary, middle, and high schools, implying that the negative effects of CEP participation on average BMI we observe in our primary results are likely to be conservative as well.

Moving now to the sensitivity of our results to alternative specifications, one potential concern with our study is that the average treatment effects we estimate may largely be driven by the set of schools with the highest ISPs. We test the sensitivity of our results to the exclusion of these high ISP outliers by omitting 195 schools with ISPs above the ISP 90th percentile of 66%. For both our binary CEP eligibility and ISP interaction specifications, we find that excluding these high ISP schools has no notable impact on our first stage results for the full sample of schools, our results by school grade type, or our results by school location type.

With regards to the effect of CEP participation on InHFZ% after excluding the set of high ISP schools, the only noteworthy changes we find are the change from 1.27 percentage points to 1.36 percentage points for the full sample of schools and a slight change in our estimate magnitudes for suburbs/towns. Our results for average school-level BMI also remain largely unchanged after omitting schools with the highest ISPs. We find no significant change in our BMI estimates under the binary eligibility specification. The estimates for our ISP interaction specification also remain robust for the entire sample after omitting the subset of high ISP schools. The most notable change in our results by school grade type is in the effect for middle schools which changes from marginally significant to statistically insignificant when we exclude high ISP schools, but the effect magnitudes are nearly identical to one another.

We omit the 2013-2014 school year from our primary analyses since we are unable to identify which schools were eligible and/or participating in the CEP during Georgia's year as a pilot state. Alternatively, we can make the assumption that schools have the same ISP, CEP eligibility, and CEP participation status in 2013-2014 that we observe in 2014-2015. While this assumption affords us an additional year of data and likely holds true in many cases, imputing our CEP variables and including data from 2013-2014 introduces additional noise into our estimations.

To see how including the 2013-2014 school year changes our results, Tables 33 and 34 show our first stage results with the set of imputed 2012-2014 CEP variables by school grade and location type, respectively. As the table's show, our results remain generally robust to the inclusion of the 2013-2014 school year under both the binary CEP eligibility and ISP interaction specifications. We find slightly smaller effects of CEP eligibility on CEP participation on average after including 2013-2014, a change we would expect to see if CEP eligible schools were generally less likely to participate in the program during the provision's pilot year.

Tables 35, 36, 37, and 38 show the results of our model's second stage under the same specifications used to create our primary results after including the 2013-2014 school year. The most common changes in our estimates with 2013-2014 are decreases in effect magnitudes and levels of statistical significance. Given that the CEP was piloted in Georgia during the 2013-2014 school year, we expect that many schools were considerably less likely to participate in the CEP during their pilot year relative to later years.[12] The likely reason for this is uncertainty in the program's effects, likelihood of lasting longer than the pilot period, and costs. Assuming that true participation rates are lower during the pilot period, we would expect to see smaller effects from CEP participation on student weight outcomes relative to our primary results because many schools will be misclassified as having participated in 2013-2014 when they did not truly adopt the CEP until 2014-2015. Regardless, we find that including 2013-2014 in our analysis does not quantitatively change our results or their implications.

2.6 Conclusion

In this study, we estimate the Community Eligibility Provision's (CEP's) effect on school-level measures of child weight for the population of public K-12 schools in the state of Georgia. We use two specifications of CEP eligibility and Identified Student Percentage (ISP) as instruments for CEP participation in regressions separated by school grade type and school location type in addition to regressions using the full sample of schools. Our primary outcomes of interest are the percentage of students attending a school who are in the healthy weight range and average school-

[12]Lower CEP participation rates during the CEP pilot period are shown in studies using data from CEP pilot states like Ruffini (2018) and Gordon and Ruffini (2018).

level Body Mass Index (BMI) score.

Our results suggest that CEP participation simultaneously increases the percentage of healthy weight students attending a school and decreases school-level average BMI. Our estimates are largely consistent across specifications and we find no statistically significant evidence to support a detrimental effect from CEP participation on school-level child weight. We find that CEP participation produces the largest increases in healthy weight percentage for middle schools, urban schools, and rural schools. We also find that elementary and middle schools see the most significant decreases in average BMI following CEP adoption.

Our results stand in contrast to seminal studies looking at the effect of school lunch on child weight. While Schanzenbach (2009) and Millimet et al. (2010) find that school lunch participation increases child weight, we do not find evidence to support the assumption that universal free school meals worsen child health. One possible cause of this discrepancy is that the CEP makes both lunch and breakfast free to all students. Given that some existing studies have found participation in school lunch, rather than breakfast, leads to worsened health outcomes (Millimet et al. 2010), it may be the case that the beneficial effects of CEP participation are driven by breakfast rather than lunch. Alternatively, Schwartz and Rothbart (2017) find some evidence of a positive effect from providing universal free school lunch on child weight among non-poor eighth graders in New York City even though universal free breakfast had been in place for years prior.

The effects of CEP participation we observe may also differ from the results of previous studies on school meal participation due to changes in meal quality during the pre-CEP period. In addition to creating the CEP, 2010's Healthy Hunger Free Kids Act(HHFKA) changed the nation's minimum nutrition standards for school meals. Prior to the HHFKA's revised minimum nutrition standards, meals served in school may have been more likely to be lower quality relative to meals brought from home, implying that we would expect to see fewer students with improved weight outcomes following the switch to school meals. If so, increased meal participation could lead to the detrimental health effects observed by early studies. In light of these nutrition standard changes, it is especially important that we revisit the relationship between school meals and child health.

Finally, the variations in free school meal enrollment following participation in the CEP also occurs at different margins than changes to factors like family income eligibility or categorical eligibility laws. Most notably, the CEP affects children who were already eligible for free meals but were not participating *and* children living in families with incomes above the existing free or reduced-price eligibility range. CEP participation removes child-level self-selection into free school meal programs entirely, implying that the negative health effects found in previous studies may be due to adverse selection into school meals under the traditional system; a theory supported by Millimet et al. (2010). Furthermore, it is possible that the beneficial effects we observe are driven by mechanisms other than changes to meal consumption. For example, it may be the case that removing the stigma surrounding free lunch participation in CEP schools produces weight improvements among students who were already eating school meal in the pre-CEP period. Unfortunately, we are not able to evaluate this possibility more thoroughly using our current data.

In addition to the difference between our results and those of previous school meal participation studies, our findings also differ from those presented in Chapter 1. In Chapter 1, I find that attending a CEP school leads to an expected decrease in the probability of healthy weight and an expected increase in BMI percentile. The reason for this discrepancy may be driven by a number of different factors. First, both studies utilize samples taken from different populations and at different levels of aggregation. In Chapter 1, data come from a nationally representative child-level longitudinal survey of elementary school aged students. We use school-level data in this chapter from the population of K-12 schools in the state of Georgia. Differences in how the CEP affects students of different ages and students in different areas of the country may account in the opposite relationship between the CEP and child weight found in both studies. Additionally, the use of school-level data may also be the reason. For example, the school-level outcomes for elementary schools used in this chapter do not allow us to isolate the effects for students in late elementary school who are the subjects of interest in Chapter 1.

Unlike Chapter 1, my results do agree with the findings of other studies measuring the effects of the CEP on child weight like Rothbart, Schwartz, and Gutierrez (2020) and Schwartz and Rothbart

(2017). Like the results found in this chapter, however, both Rothbart, Schwartz, and Gutierrez (2020) and Schwartz and Rothbart (2017) use data from a single state and single city, respectively. Furthermore, both studies only use data for students in a narrow age range, further limiting the external validity of their results. Looking to the current literature concerning the CEP's effect on child weight, there are two obvious questions which are still outstanding. First, which areas of the country/types of students see beneficial effects from CEP participation and which do not? Second, why? As it stands, additional studies covering more places and types of students are needed to find definitive answers.

Moving past the results found in other studies, given that our results suggest participation in the CEP leads to improved school-level child weight outcomes, an important question becomes - what factors determine a school's participation choice in Georgia? While not explicitly presented here, results from a simple model of CEP participation gives some insight into possible decision factors. First, schools with more students enrolled in free or reduced-price lunch during the pre-CEP period are less likely to sign up for the provision. The cause of this relationship may be that schools with the majority of their students already receiving free or reduced-price meals deciding that the small increase in uptake caused by the CEP is not worth the effort. This possibility stands in contrast to the assumption that schools with high numbers of students enrolled in free and reduced-price meals are still adequately incentivized to participate in the CEP due to the reduction in administrative costs caused by removing meal applications.

We also find CEP eligible schools with identified student percentages below 62.5% are less likely to participate since CEP schools with ISPs between 40% and 62.5% only have a portion of their meals reimbursed at the free rate by the USDA. This further supports the idea that program costs play a role in each school's likelihood of participation discussed in Chapter 1. Furthermore, we find that schools within the 40% to 62.5% range are more likely to participate as their ISPs increase. If barely eligible schools are dissuaded from participating in the CEP because of reimbursement rates, our results suggest that the USDA may be able to significantly improve child weight by changing the CEP's current reimbursement scheme to raise the CEP enrollment rate among schools right above the eligibility threshold.

County-level poverty also seems to play a complex role in the CEP participation decision. For example, we find that the overall percentage of a school's county living in poverty is negatively correlated with CEP participation, indicating that schools in counties that are poorer overall are less likely to adopt the provision. While this relationship may again be due to differences in pre-CEP free and reduced/price meal enrollment rates, we find that the poorest counties in Georgia do not have more children enrolled in free school meals on average. Alternatively, schools in counties with higher levels of child poverty are more likely to adopt the CEP, implying that the distribution of poverty by age group within a county affects the participation decision.

Finally, we find that eligible schools in urban areas are more likely to participate in the program than schools in suburbs/towns while rural schools are not. Rural schools in Georgia have the lowest number of students enrolled in free and reduced/price meals during the pre-CEP period even though they serve children in the state's poorest counties. Therefore, the low uptake rate among rural schools diverges from the CEP's primary goal of providing free school meals to children who were not adequately reached by the traditional system. If disadvantaged schools in different areas continue to participate at different rates, the CEP may unintendedly perpetuate location specific disparities in child health.

While the results of our study provide important evidence regarding the CEP's effect on school-level measures of child health, future research is needed to understand the effects of universal free school meals at the child-level. As spoken to throughout our study, school-level measures of health identify specific moments of an underlying child-level distribution, making it impossible to determine where changes stem from. The results of Chapter 1 work towards filling this gap in the research, but those still deal with a reasonably small age range of students. Furthermore, our study ignores other mechanisms through which free school meal provisions could either improve or harm the lives of children and their families. Other studies like Schwartz and Rothbart (2017), Ruffini (2018), Gordon and Ruffini (2018), Rothbart, Schwartz, and Gutierrez (2020), and Gordanier et al. (2019) examine some of these additional outcomes, but more work is still needed to fully understand the CEP's effects on individuals at all levels.

Unlike Chapter 1, we are also limited by the use of data from schools in only one state. While this provides us with some advantage in that all schools share the same state-level environment, we are unable to examine the effects of CEP participation on school-level weight in other states. This limitation is especially important given the degree of variation in pre- and post-CEP school environments and CEP participation rates across state lines. For example, while 92.2% of CEP eligible schools chose to participate in Ohio during the 2016-2017 school year, only 15.1% of eligible California schools participated in the CEP during the same year. It is most likely the case that differences across states affect the school meal environment of low-income schools as well as how the provision of universal free meals impacts child health.

Finally, additional work is needed to better understand the possible interactions, decisions, and outcomes schools face when choosing whether or not to participate in the CEP. Aside from the observable determinants of participation, one possible factor which we have not seen considered in the literature is school-level stigma. If schools choose not to adopt the CEP because they feel that it will negatively effect their public perception, our results indicate that the choice of non-participation may come at the expense of forgone improvements to the health of their students.

Chapter 3

A FLEXIBLE MODEL OF FOOD SECURITY: ESTIMATION AND IMPLICATIONS FOR PREDICTION

Note: This chapter represents coauthored work with Rusty Tchernis and Christian Gregory. Funding for this project comes from a cooperative agreement with the United States Department of Agriculture's Economic Research Service.

3.1 Introduction

The United States Department of Agriculture (USDA) defines food insecurity as "... a household-level economic and social condition of limited or uncertain access to adequate food" (Coleman-Jensen et al., 2016). Naturally, this definition is broad as the intersectional interpretation of food insecurity varies across sociodemographic and community-level contexts. For example, while not having a reliable source of fresh fruits and vegetables may be the primary cause of food insecurity for an inner-city household living in a food desert, not having the income needed to purchase foods that are readily available may lead to food insecurity among rural households. Given the issue's complexity, measures of food security and insecurity must incorporate information spanning multiple dimensions of economic and social determinates in order to properly evaluate the conditions of households facing different issues.

The most common measure of domestic household food security utilizes questions from the core Food Security Module (FSM) which was initially piloted in the 1995 Current Population Survey (CPS). The FSM uses a 10-item questionnaire for all households, and an additional 8 questions regarding child food security for households with children. The questionnaire covers a broad range of conditions and behaviors that are more and less severe such as “We worried whether our food would run out before we got money to buy more” and “We couldn’t afford to eat balanced meals.”[1]

With a household’s total number of affirmative responses to the FSM questions, researchers commonly use an accompanying food security scale to assign households one of three food security statuses and one of three child food security statuses if the household has children.[2] The initial construction of this food security scale was partially data-driven in that final question selection and determination of each question’s relative contribution to a household’s food security level was determined using three years of nationally representative pilot data and techniques from Item Response Theory (IRT), more specifically a 1-parameter Rasch (1PR) model.[3] This approach stands in contrast to many indices which rely entirely on expert opinion regarding the assumed contribution of each observed variable to assign weights deterministically.

In general, there are two main benefits to measuring food security using the standard FSM scale. First, since the FSM only includes at most 18 items, the questionnaire can be incorporated into new or existing surveys without placing considerable burden on survey participants and interviewers. Since its initial introduction, many large U.S. surveys have incorporated the FSM including the CPS, the Panel Study of Income Dynamics (PSID), the Survey of Income and Program Participation (SIPP), and the National Health and Nutrition Examination Survey (NHANES). Second, categorical measures of food security are easy to interpret, and assigning a status to each household in a data set can be done quickly. This ease of use makes the complex concept of food security convenient and approachable for researchers, policy makers, and other interested parties.

While the FSM scale and other similar methods may be simple and convenient, researchers have consistently voiced the need for better measures (Barrett, 2010, Headey and Ecker, 2013, Ruel, 2003, Maxwell et al., 1999). Overall, concerns regarding the FSM scale seem to be more common than issues with the questions themselves. The first issue is that using discrete categories to measure food security necessarily suppresses information about a continuous latent outcome

[1] For a full list of questions, *see* Coleman-Jensen et al. (2016).

[2] In this context, an affirmative response is one which indicates an undesirable outcome with regards to food security.

[3] For more information regarding the measure’s construction, *see* Ohls, Radbill, and Schirm (2001).

of interest. In the case of the FSM scale, the cutoffs for each food security level were chosen by expert opinion (Ohls, Radbill, and Schirm, 2001). The use of deterministic cutoffs partially defeats the purpose of using a data-driven model like the 1PR to measure food security. While data inform each question's weight in the measurement model, opinion determines the ultimate scale used by practitioners. Furthermore, the placement of households into a small number of categories abstracts away from any within-category variation in food security. For example, Gregory and Coleman-Jensen (2017) find differences in the health of adults living in marginally food secure and fully food secure households even though the traditional FSM scale does not delineate between the two groups.

The second issue concerns the scale's suppression of information at the question-level. The 1PR measurement model can only be used with binary "affirmative/negative" type observed variables. While some FSM questions like "In the last 12 months, did you ever cut the size of any of the children's meals because there wasn't enough money for food?" are answered as either yes or no, many of the FSM's questions have more than two potential responses. Specifically, 6 questions can be answered as "often, sometimes, or never", and 3 have potential responses of "almost every month, some months but not every month, in only 1 or 2 months, or never." Converting each question into a binary indicator ignores potentially valuable information regarding the underlying severity of each household's latent food security level. This restriction implies that for two households, one answering "sometimes" for all appropriate questions and the other with "often" will be effectively indistinguishable from one another. Naturally, one would assume *a priori* that "often" indicates a more severe condition than "sometimes."

The third issue is that there is no way to incorporate additional information into the existing food security scale. While the FSM covers a range of topics, it does not include potentially important information like having the transportation required to get food, the number of individuals in the household, participation in nutrition assistance programs, etc. Since the scale was created using only the 18 FSM variables, the contribution of each variable would be invalidated after adding additional variables. There is no intuitive way to incorporate additional information into an existing scale without re-estimating the underlying measurement model, and since many of these potential observed variables would also be non-binary, they cannot be used in a 1PR model without alteration.

The fourth issue is that the existing measure does not account for the inherent uncertainty in measuring a latent variable. While the scale was created using a probabilistic measurement model, it does not incorporate any measure of uncertainty directly. Having no measure of uncertainty is especially important when comparing the food security levels of different households or groups of households. While the scale assigns each household with a single value, it is possible that certain households could fall into more than one category if their statistically plausible range of latent food security falls across one or more category thresholds.

Finally, in addition to the FSM scale's technical limitations, the existing scale was designed using estimates from data that are more than two decades old at the time of this writing. While the scale's creators showed that the 1995 estimates were stable across time using data from 1996 and 1997, the underlying food security environment of households in the U.S. has almost certainly changed during the years since. To this point, the scale's designers state "... we recommend continuing to estimate the IRT model item parameters each year, as data become available."[4] If the model's parameters are re-estimated each year, the benefit of estimating the measurement model once and then using a more convenient scale in future research is diminished. Furthermore, even if the 1PR model is stable across time using data from the same survey, it may be the case that estimates are unstable when using data from other sources even though the same set of questions are used.

To address these limitations, we propose an alternative measure to the standard 1PR derived FSM food security scale. Specifically, we estimate household-level food security using a Bayesian Graded Response Model (BGRM). The BGRM has several attractive features which correct for each of the issues discussed above regarding the FSM scale and measurement model. First, the BGRM estimates latent food security as a continuous variable, removing the need for discreet cat-

[4] For a detailed discussion of the 1PR measurement model's stability across time, see Ohls, Radbill, and Schirm, 2001.

egories of food security based on deterministic cutoffs. Second, the BGRM allows us to estimate food security using both binary and ordinal variables. This feature allows us to incorporate any information about response severity at the intensive margin provided by the FSM's non-binary questions. Third, the BGRM is flexible enough to include different sets of observed variables. Examples of these could include new questions related to the distinct domains of food security like "When we did not have enough food to eat, we received food from a food bank, church, etc." which would capture the role of community aid in determining food security. By adding or removing variables, researchers can evaluate different dimensions of food security which are not captured by the FSM. Finally, as a Bayesian model, the BGRM estimates draws from a posterior distribution of food security for each household. Using approximated moments of these distributions, we can provide measures of uncertainty along with point estimates.

In addition to addressing concerns with the FSM scale and 1PR measurement model, our methodology has two primary strengths. The first is that only questions for which a household provides valid responses are used when estimating their food security level. This feature allows us to estimate food security for households who only have valid data for a subset of questions in addition to households with complete responses. Finally, similar to the logic behind using the original 1PR measurement model, the relative contributions of each manifest variable to latent food security is informed by the data. Unlike the traditional method, however, the purpose of our model is not to ultimately create a food security scale, but rather to estimate latent food security separately in each application. Our model can therefore capture potential changes in the underlying relationship between observable variables and latent food security across time, groups, and data sets while the standard scale cannot.

In the remainder of this study we present the BGRM framework, derive the model, present the Markov Chain Monte Carlo (MCMC) algorithm used to estimate the model's parameters, and discuss estimation results from a simulated data exercise.

3.2 The Bayesian Graded Response Model of Food Security

Our model of food security uses the following parameters and variables:

1. y_{ij} denotes the response of household $i = 1, ..., I$, to FSM question $j = 1, ..., J$. Each question j has a set of potential responses ranging from 1 to C_j, implying that $y_{ij} \in \{1, ..., C_j\}$.

2. The variable δ_i represents the latent food security level of household i which is invariant across the set of FSM responses provided by the household.[5]

3. The set of question specific intercept parameters $\mu = \mu_1, ..., \mu_J$.

4. Response thresholds for each question j denoted by $\gamma^j = \gamma_0^j,\ \gamma_1^j,\ ...,\ \gamma_{C_j}^j$, such that $\gamma_{k-1}^j \leq \gamma_k^j$ for all $k = 1, ..., C_j$.

5. Random variation in the elicited food security level of household i with respect to question j, e_{ij}, which captures the fact that a question's ability to measure latent food security is subject to error.

With these variables, the model of household responses is defined as follows. Household i answers question j with response $y_{ij} = k$ if and only if:

$$\gamma_{k-1}^j < \mu_j + \delta_i + e_{ij} \leq \gamma_k^j \tag{3.1}$$

Assume that the distribution of each e_{ij} is Normal with mean 0 and question specific variance σ_j^2. Furthermore, we set the first and last response threshold of each question j such that $\gamma_0^j = -\infty$ and $\gamma_{C_j}^j = \infty$.

The model of FSM responses given by (3.1) implies that the probability household i provides

[5] Throughout this paper, we refer to δ as latent food security, but in the case of the FSM all questions are thought to be negatively related to food insecurity. This difference is an issue of interpretation and is not fundamentally important to our method.

response k to question j is equal to:

$$P(y_{ij} = k) = \Phi\left(\frac{\gamma_k^j - (\mu_j + \delta_i)}{\sigma_j}\right) - \Phi\left(\frac{\gamma_{k-1}^j - (\mu_j + \delta_i)}{\sigma_j}\right) \tag{3.2}$$

where $\Phi()$ is the CDF of the standard Normal distribution.

The response probabilities given by (3.2) describe the likelihood of a graded response model with a probit link, given formally by:

$$L(\mu, \delta, \gamma, \sigma^2) = \prod_i \prod_{j \in \mathcal{C}_i} \left[\Phi\left(\frac{\gamma_{y_{ij}}^j - (\mu_j + \delta_i)}{\sigma_j}\right) - \Phi\left(\frac{\gamma_{y_{ij}-1}^j - (\mu_j + \delta_i)}{\sigma_j}\right)\right] \tag{3.3}$$

The outer product of (3.3) extends over all households in the set of observed data, and the inner product extends over the set of questions j answered by household i, denoted by $\mathcal{C}_i$.

Looking at the model's likelihood in equation (3.3), we see that not all of the model's parameters can be identified. As is the case with many ordinal response models, the value of the likelihood does not change with affine transformations of each question's response thresholds, intercepts, error variances, or latent food security levels. To address this, we restrict $\gamma_1^j = 0$ and make the normalizing assumption that $\delta_i \sim N(0, 1)$.

3.3 Parameter Prior Distributions

The prior distributions used in our model for structural parameters μ, σ^2, and γ are given as:

$$\mu_j \sim N(\bar{\mu}, \sigma_\mu^2), \ \forall j = 1, ..., J$$

$$\sigma_j^2 \sim IG(\alpha, \beta), \ \forall j = 1, ..., J$$

$$\gamma_k^j \sim U(a, b)\mathbb{1}(\gamma_{k-1}^j < \gamma_k^j \leq \gamma_{k+1}^j), \ \forall k = 1, ..., C_j - 1$$

where $IG()$ denotes the inverse gamma distribution and $U()$ denotes the uniform distribution.

In addition to the set of parameter prior distributions, we also define a latent variable, y_{ij}^*, such that:

$$y^*_{ij} = \mu_j + \delta_i + e_{ij} \tag{3.4}$$

which gives the following augmented likelihood function over our model parameters and augmented data:

$$L(\mu, \delta, \gamma, \sigma^2, y^*) = \prod_i \prod_{j \in \mathcal{C}_i} \phi\left(\frac{y^*_{ij} - (\mu_j + \delta_i)}{\sigma_j}\right) \mathbb{1}(\gamma^j_{y_{ij}-1} < y^*_{ij} \leq \gamma^j_{y_{ij}}) \tag{3.5}$$

Estimation of the model's structural parameters, latent food security variables, and augmented data values is performed using the Markov Chain Monte Carlo (MCMC) algorithm outlined in the following section.

3.4 Estimation Algorithm

Drawing from the posterior distributions of the model's parameters and latent variables is done using the following Gibbs Sampler algorithm.

Step 1. Sample δ.

For $\delta = [\delta_1, ..., \delta_I]'$, let the estimation equation be given as:

$$y^* - \mu \otimes 1_I = \Lambda\delta + e$$

where $y^* = [y^*_1, ..., y^*_J]'$ is a stacked vector of y^*'s, 1_I is a $I \times 1$ vector of 1's, $\Lambda = I_I \otimes 1_J$, I_I is an $I \times I$ identity matrix, 1_J is a $J \times 1$ vector of 1's, $e = [e_1, ..., e_J]'$, and $\otimes$ denotes the Kronecker product.

All elements of δ are drawn simultaneously from the following full conditional posterior distribution:

$$\delta|\mu, \sigma^2, y^*, Y \sim N(d, D) \tag{3.6}$$

where $D = \left[I_I + \Lambda'\Sigma^{-1}\Lambda\right]^{-1}$, $d = D\left[\Lambda'\Sigma^{-1}(y^* - \mu \otimes 1_I)\right]$, and Σ is a $J \times J$ variance-covariance matrix with diagonal elements $(\sigma_1^2, ..., \sigma_J^2)$ and zeros for the off diagonal elements.

Step 2. Sample elements of μ.

For each element of $\mu = [\mu_1, ..., \mu_J]'$, the estimation equation is given as:

$$y_j^* - \delta = 1_I \mu_j + e_j$$

Each μ_j is then drawn from the following full conditional posterior distribution:

$$\mu_j|\delta, \sigma^2, y^*, Y \sim N(v, V) \tag{3.7}$$

where $V = \left(\frac{1}{\sigma_\mu^2} + \frac{1_I' 1_I}{\sigma_j^2}\right)^{-1}$ and $v = V\left[\frac{\bar{\mu}}{\sigma_\mu^2} + \frac{1_I'(y_j^* - \delta)}{\sigma_j^2}\right]$.

Step 3. Sample elements of γ.

Samples for each element γ_k^j of $\gamma^j = [\gamma_0^j, \gamma_1^j, ..., \gamma_{C_j}^j]$, such that $k = 2, ..., C_j - 1$, are drawn from the following full conditional posterior distribution:

$$\gamma_k^j|\gamma_{-k}^j, y^*, Y \sim U(L, R) \tag{3.8}$$

where $L = max[max(y_j^*|y_{ij} = k-1), \gamma_{k-1}^j]$, and $R = min[min(y_j^*|y_{ij} = k+1), \gamma_{k+1}^j]$.

Step 4. Sample elements of y^*.

Samples for each element y_{ij}^* of $y^* = [y_1^*, ..., y_J^*]'$ are drawn from the following full conditional posterior distribution:

$$y_{ij}^*|\mu,\delta,\sigma^2,\gamma,Y \sim N\left(\mu_j+\delta_i,\sigma_j^2\right) \tag{3.9}$$

truncated to the interval $(\gamma^j_{y_{ij}-1},\gamma^j_{y_{ij}})$.

Step 5. Sample elements of σ^2.

For each element of $\sigma^2 = [\sigma_1^2, ..., \sigma_J^2]'$, the estimation equation is given as:

$$y_j^* = 1_I\mu_j + \delta + e_j$$

Each σ_j^2 is then drawn from the following full conditional posterior distribution:

$$\sigma_j^2|\mu,\delta,\gamma,Y \sim IG(a,b) \tag{3.10}$$

where $a = \alpha + \frac{I}{2}$, and $b = \left[\frac{1}{\beta} + \frac{(y_j^*-1_I\mu_j-\delta)'(y_j^*-1_I\mu_j-\delta)}{2}\right]^{-1}$.

3.5 Simulated Data Exercise

To test the predictive capabilities of the BGRM, data are simulated for a set of $I = 10,000$ households answering a set of $J = 10$ questions. The potential responses of each question is set to mimic those of the adult food security component of the FSM, implying that $C = [C_1, C_2, ..., C_{10}]$ is defined as $C = [3,3,3,2,3,2,2,2,2,3]$. This mix of binary and polytomous response data is something that strictly dichotomous or strictly polytomous IRT models can not estimate, but our BGRM is well suited for such cases.

The set of data generating structural parameters is given in Table 39 below. The food security level of each household i is constructed such that $\delta_i \sim N(0,1)$. The error term for each household i's response to question j is constructed such that $e_{ij} \sim N(0,\sigma_j^2)$, and each y_{ij}^* and y_{ij} are constructed using (3.4) and (3.1), respectively. The Gibbs Sampler algorithm outlined in Section 3.4 was run for 20,000 iterations with the first 10,000 draws removed for burn-in.

Beginning with visual inspection of the estimation process, Figures 10 through 13 show the draws of our model's structural parameters and mean latent factor draws over all iterations. Starting with Figure 10, we see that samples from the posterior distributions of each μ are reasonably stable around the true value of 1 from the first few hundred iterations onward. None of the μ's see a structural deviation in their draw chain from the true data generating value. Like the behavior shown in Figure 10, Figure 11 shows that draws of σ^2 are similarly stable across the full set of iterations and centered around their true values. Likewise, we see that the history of draws of the posterior mean of δ is reasonably stable during the full sampling period and centered around the true value of δ's mean, 0.

Examining the samples of γ_2 for questions 1, 2, 3, 5, and 10 in Figure 13, we see that the sampling history of our threshold parameters is less well behaved. While three of the threshold parameters converge to the neighborhood around the data generating values, the threshold parameters of the other two questions do not. The figure shows a notable decrease in the sampling variance of our γ_2's around the 10,000 iteration mark, but they do not seem to fully converge across the entire sampling period. One potential reason for the sizable variance in our samples of γ_2 is that the sampler simply did not run for long enough to converge. The slow convergence of parameters in Bayesian polytomous data models is a well documented problem even in the earliest literature (Albert and Chib, 1993, Cowles, 1996, Nandram and Chen, 1996). Alternatively, it may be the case that traditional convergence of our γ_2's to a tight bandwidth around their true value is not feasible given how little information is given by the observed response data. Estimation of the model using the true data generating values of y^* leads to substantially faster and tighter convergence of γ, implying that the additional information provided by the continuous y^* leads to the improvement in parameter retrieval accuracy we would expect *a priori*.

Moving on from our graphical analysis, Table 40 compares the data generating values of our parameters to their estimated posterior mean and 95% credible intervals.[6] Beginning with our estimates of μ, we see that the posterior mean value is qualitatively close to the data generating value in all cases. Furthermore, the true data generating value of each μ_j falls within the posterior 95% credible interval in all cases. Looking to our estimates of σ^2, we again see that the posterior mean and data generating values are reasonably close to one another. Alternatively, the 95% credible interval of σ^2 does not cover the true value in the single case of question 5. Finally, moving to the estimates of γ_2 shown in Table 40, we find that while seemingly similar to their data generating values, the 95% credible intervals of γ only cover the true parameter value in three out of the five total cases. This again may be due to not having enough iterations to achieve convergence, but it may also be the result of utilizing limited response data.

As an alternative to checking whether the true value falls within the estimated 95% credible interval of each parameter, similar studies often rely on various forms of the Root Mean Squared Error (RMSE) calculated using the data generating and estimated parameter values (Zhu and Stone, 2011, Kieftenbeld and Natesan, 2012, Broomell and Bhatia, 2014). Taking this approach, we calculate the RMSE of our estimates for each parameter using the full set of post-burn-in draws from the posterior distribution. The RMSE of each parameter type is then averaged, giving us the average RMSE of μ, σ^2, and γ_2. We find that the average RMSE of μ, σ^2, and γ_2 are 0.029, 0.0431, and 0.0367, respectively. In line with other common statistical problems, there is no single RMSE value that signifies adequate parameter retrieval. However, we do find that our RMSE's fall well below the thresholds used in similar studies (Zhu and Stone, 2011, Kieftenbeld and Natesan, 2012).

We now discuss our model's ability to accurately predict each household's latent food security variable. Figure 14 shows the posterior mean value of δ for each household along the x-axis and true data generating values of δ on the y-axis, along with a 45 degree line. Looking at the figure suggests two obvious traits. First, our estimated values of δ correspond well with their true values.

[6]The values in parentheses in Table 40 show 95% credible intervals given by the post-burn-in posterior draw of each parameter corresponding to the specified percentile value. In this specific case, the 10,000 post-burn-in iterations are first ordered from smallest to largest. The left credible interval value is then given by the 250th draw and the right interval as the 9750th draw. The "NE" designation implies that the specified parameter is fixed and therefore not estimated by the model.

Households with higher posterior mean values of δ are associated with higher data generating food security levels. Second, Figure 14 shows a substantial amount of binning in our estimates of δ. Specifically, the posterior mean values of δ fall into easily identifiable groups with discreet jumps across most groups. This binning is a byproduct of relying on ordinal polytomous data. For a given set of households with identical responses in the observed data, the model can only partially distinguish between the relative food security level of households within the set. This limitation leads to households with the same responses having similar posterior mean values of δ, forming bins. Furthermore, in this specific exercise, values of μ, σ^2, and γ are constructed so that they are the same across all questions. With equal parameter values across questions, questions are also treated as equal in so far as how they affect δ. For example, an individual who answers 3 to question 1 and 1 to all other questions will be placed into the same bin of posterior mean δ as another individual who answers 3 to question 2 and 1 to all others. Alternatively, variation in parameter values across questions increases both the predictive power of δ posterior mean the number of final bins since questions are now distinct from one another in how they relate to δ. Given that assigning identical parameters across questions can therefore be seen as a less than optimal scenario, we still find that our model produces estimates that generally correspond to their true value.

Finally, we compare the relative abilities of both the BGRM and FSM scale to accurately predict household food security status. First, we designate the 20th percentile of our data generating δ's as the cutoff between food security and food insecurity, implying that households with values of δ below the 20th percentile are categorized as "food insecure" while households with δ's above the 20th percentile are "food secure". Next we categorize households as either food secure or food insecure using the posterior mean estimates of δ from our BGRM. To then categorize households using the FSM scale, we use the standard approach where households are considered food insecure if they have 3 or more responses indicative of food insecurity.[7] Finally, we redefine the food insecurity cutoff point to the 50th and 5th percentiles to evaluate the sensitivity of both measures to changes in underlying food insecurity rates. This involves changing the percentile used for both

[7]Since responses to each of the 10 questions are positively related to food security, responses indicative of food insecurity include a response of 1 or 2 to questions 1, 2, 3, 5, and 10, and a response of 1 to questions 4, 6, 7, 8, and 9.

the data generating δ's and posterior mean δ's while keeping the FSM scale the same.

We measure classification accuracy using the proper match rate and the mismatch rate. To define these, note that each household i has a true food security status F_i such that F_i is equal to 1 if the household is food secure and 0 otherwise. Given each household's estimated food security status $\hat{F}_i$ from either the BGRM or FSM scale, we then define a proper match $P_i = 1$ as the case where $F_i = \hat{F}_i$ and a mismatch $P_i = 0$ as the case where $F_i \neq \hat{F}_i$. The proper match rate is then calculated as $(\sum_{i=1}^{I} P_i)/I$ and the mismatch rate is given by 1 minus the proper match rate. Table 41 shows the proper match rates and mismatch rates for both the BGRM and FSM scale under our three percentile thresholds of food security. Beginning with the 20th percentile definition in Panel A of Table 41, we see that the proper match rate for the BGRM and FSM scale are 0.94 and 0.52, respectively. This implies that while food security categories estimated using the BGRM were correct 94% of the time, categorizations from the FSM scale were only correct for roughly 52% of households. We find that the BGRM similarly outperforms the FSM scale when the food security cutoff is set to the 50th and 5th percentile in Panel B and Panel C, respectively.

While we find that the BGRM's estimates of food security categories outperform those produced using the FSM scale with the 3 response rule, we can also adjust the FSM scale such that both measures more closely match the true data. Specifically, we adjust the FSM scale's number of "food insecurity indicative responses" needed to classify a household as food secure until the share of food secure and food insecure households produced by the FSM scale more closely matches the true data. For example, with the food security threshold set to the 20th percentile, adjusting the FSM scale to require 7 indicative responses produces shares of food secure and food insecure households equal to roughly 77% and 23%, respectively. This same process is repeated for our alternative thresholds and the results are then given in Table 42. Table 42 shows that adjusting the number of responses needed in the FSM scale to mirror the data increases its performance considerably. However, while the gap in performance between the BGRM and FSM scale categorizations is smaller, the BGRM still outperforms the FSM scale in all cases. Therefore, while the vast majority of studies stick to the traditional 3 response FSM scale regardless of the sample, the BGRM is better able to assign households food security status in our simulated data exercise even in the case where the scale is adjusted to match a known threshold.

3.6 Conclusion

In this study, we propose a new method for measuring household food security. Our Bayesian Graded Response Model (BGRM) is well suited to the measurement of food security for several reasons. First, the BGRM estimates latent food security as a continuous variable, removing the need for discreet food security categories common to other models. Second, the BGRM allows us to estimate food security using both binary and polytomous ordered response variables. This flexibility lets us avoid having to first turn each core Food Security Module (FSM) question into a binary variable before constructing our measure. Importantly, being able to utilize all data without initial variable restrictions incorporates information about response severity at the intensive margin for non-binary questions into our estimates. Third, questions not included in the FSM can be included in the BGRM. By adding or removing variables, researchers can evaluate different dimensions of food security which may not be covered by existing measures. Finally, as a Bayesian model, the BGRM estimates draws from a posterior distribution of food security for each household. Using approximated moments of these distributions, we can provide measures of uncertainty along with point estimates.

After presenting our model and estimation algorithm, we test our model's performance using a simulated data exercise. We find that our model adequately retrieves structural parameters, but convergence is slow and potentially not achieved for our set of threshold parameters. While this is commonly cited in the literature using similar models, improving the estimation speed of the BGRM would likewise improve accessibility to the method and is a promising avenue for future research. After classifying each household in our simulated data set as food secure or food insecure to match the traditional scale, we then test the ability of both the BGRM and FSM scale to accurately classify households. We show that the using the BGRM produces significantly better categorization accuracy than the FSM scale when using the official 3 response cutoff. Adjusting the number of responses needed to classify a household as food insecure under the FSM to match the simulated data reduces this gap in performance, but we still find that the BGRM outperforms the FSM scale in all cases.

With the new measure for household food security presented in this chapter, additional work is needed in two major areas. First, the sampling algorithm would greatly benefit from alterations that increase its efficiency. This is especially true for researchers who may be facing more computational constraints due to hardware limitations. If the model can be estimated more easily, the method would be increasingly approachable to the target audience of food security researchers. The second area where additional work is needed relates to estimation and evaluation of the model using real world data. Our BGRM can be used to measure food security with any one of the major surveys that contain the FSM. BGRM estimates of food security from real survey data could be compared to the traditional FSM scale across several dimensions. For example, both measures could be compared on their ability to predict outcomes we associate with a household's level of food security like physical and mental health. If future work finds that the BGRM out performs the FSM scale in terms of predictive power, its advantages would be further strengthened.

TABLES AND FIGURES

Figure 1: CEP Participation Rate by Identified Student Percentage During the 2014 and 2015 School Years

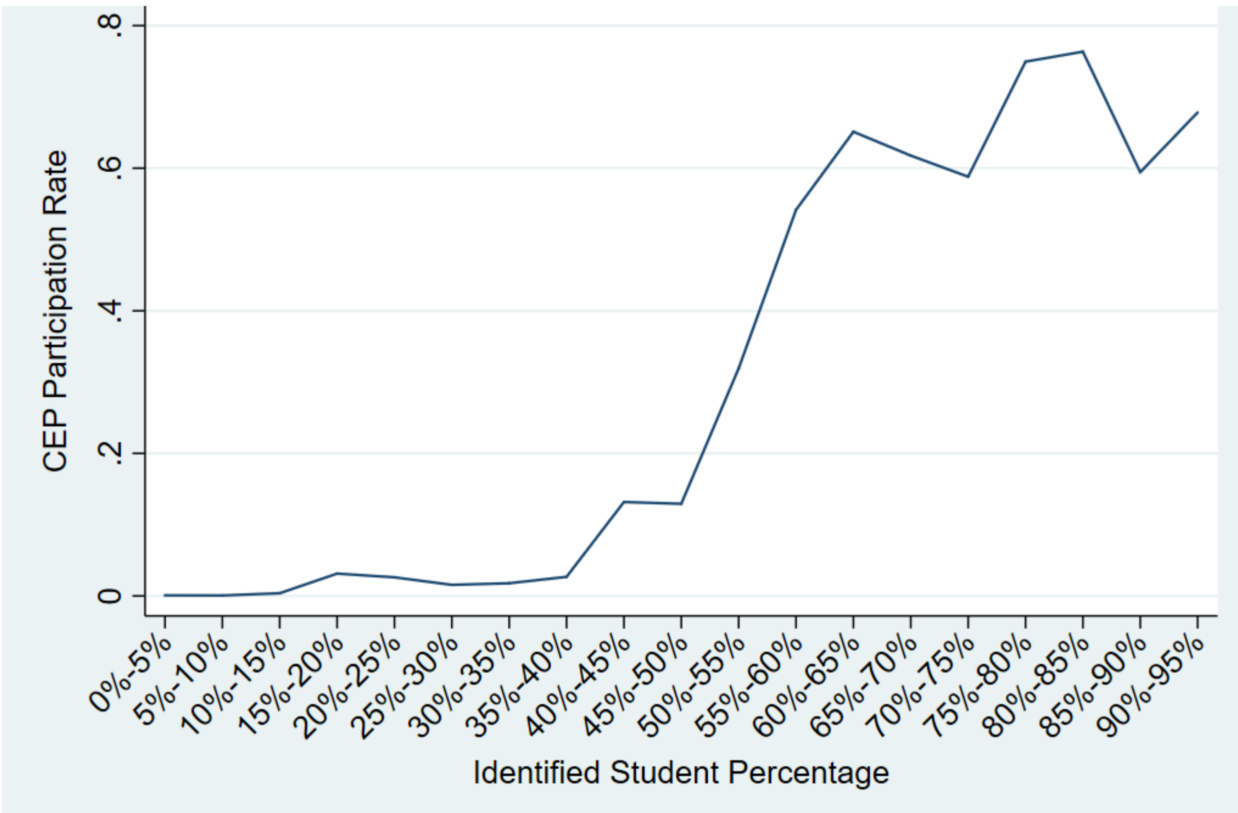

Figure 2: Average BMI Score for CEP and non-CEP schools by Years

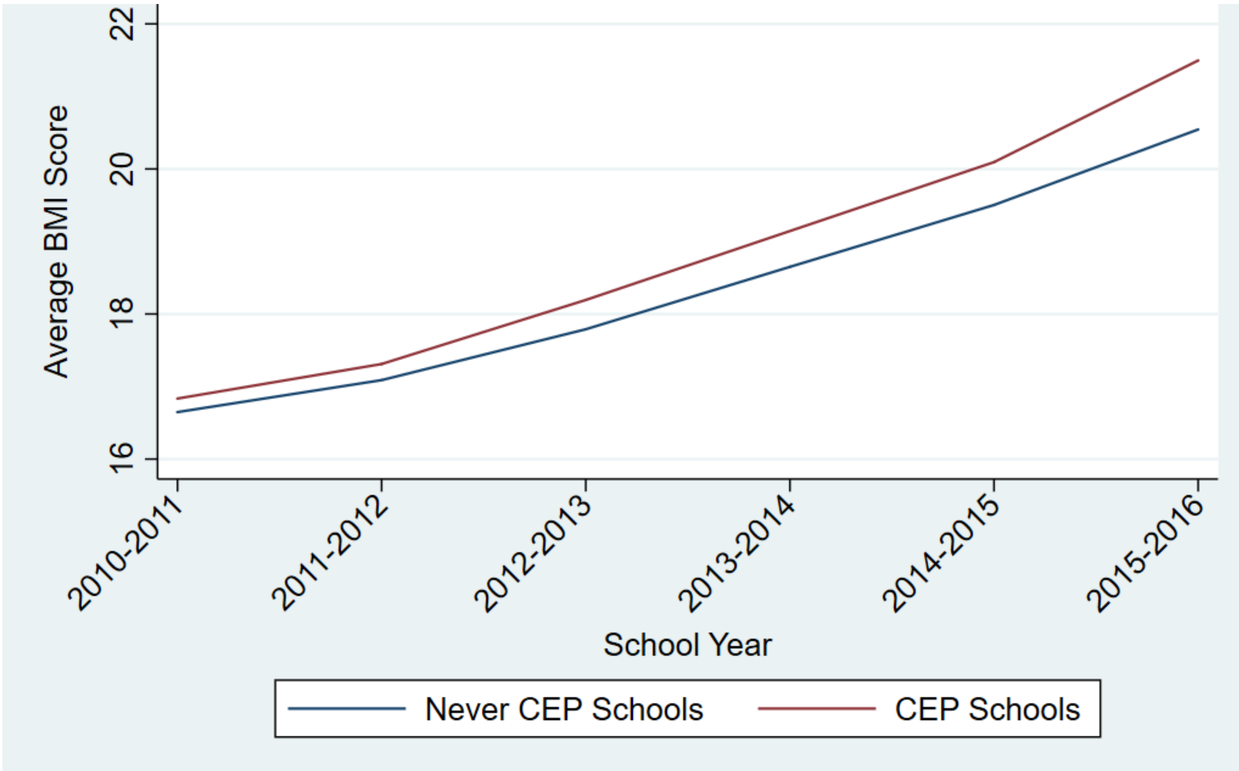

Figure 3: Rate of Child Underweight for CEP and non-CEP schools by Years

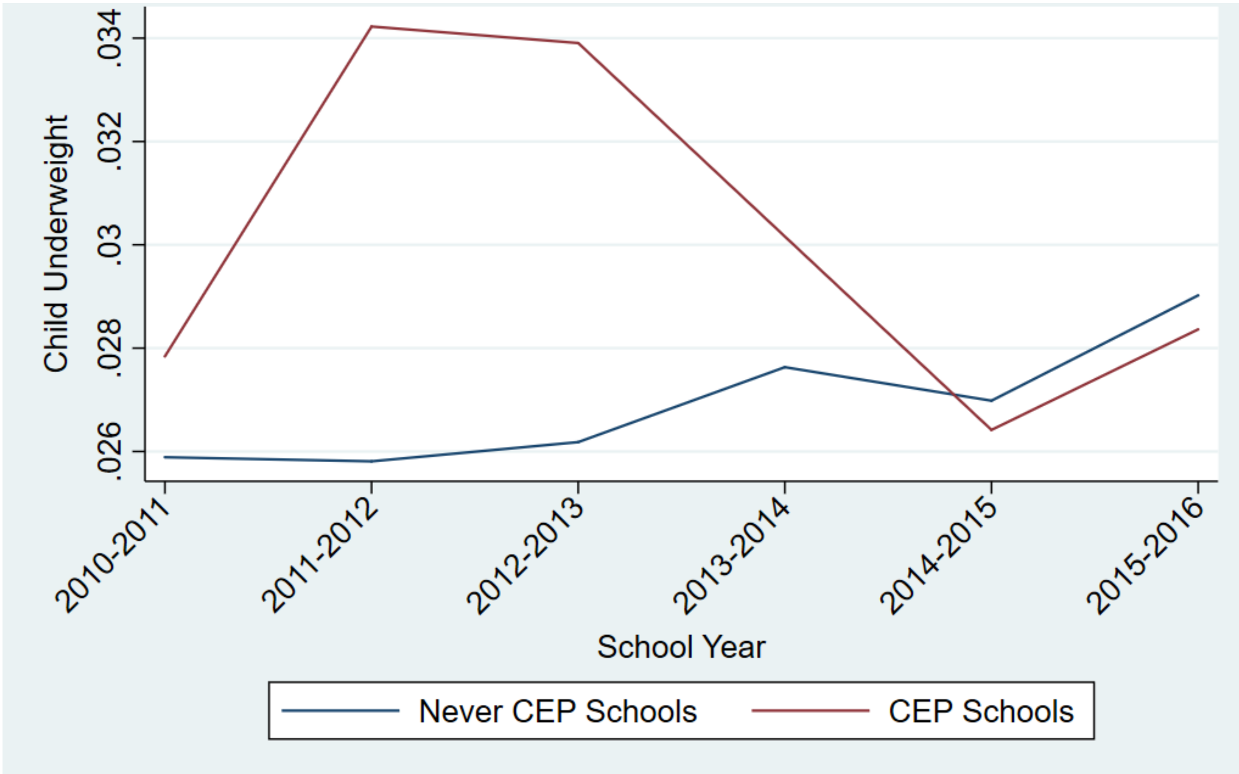

Figure 4: Rate of Child Healthyweight for CEP and non-CEP schools by Years

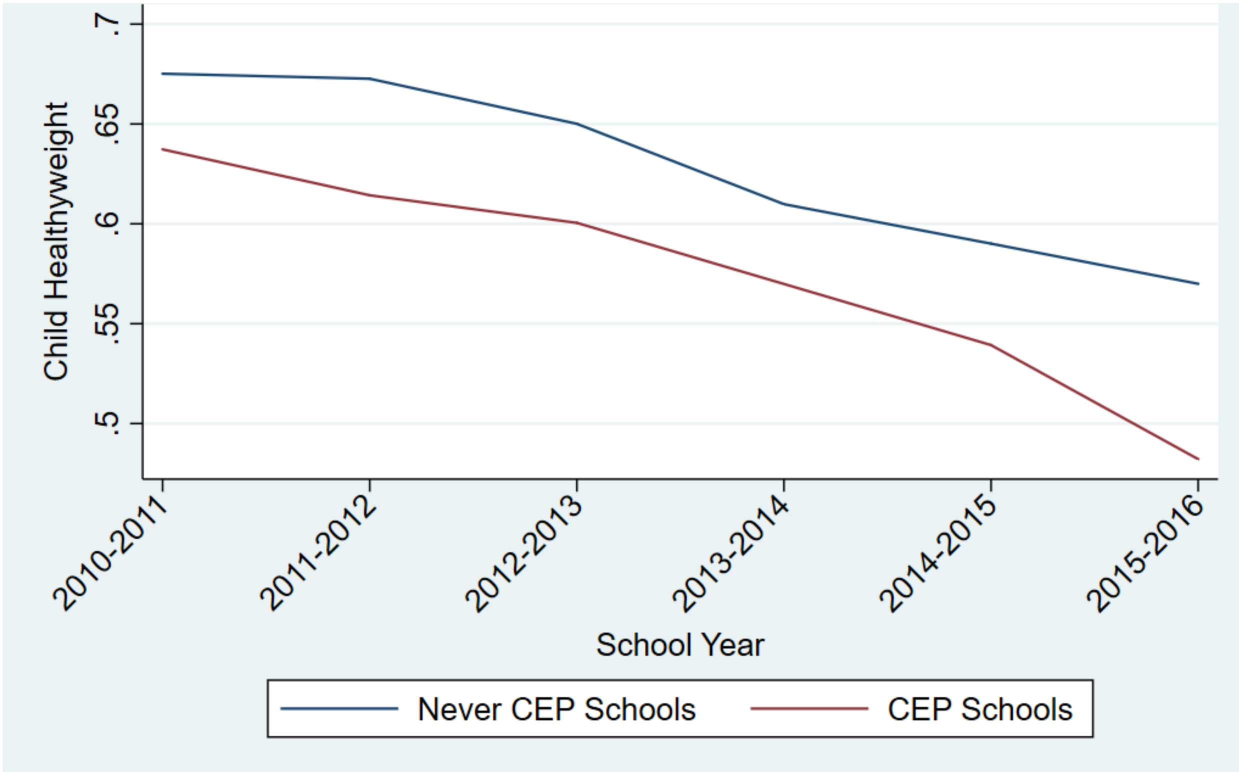

Figure 5: Rate of Child Overweight for CEP and non-CEP schools by Years

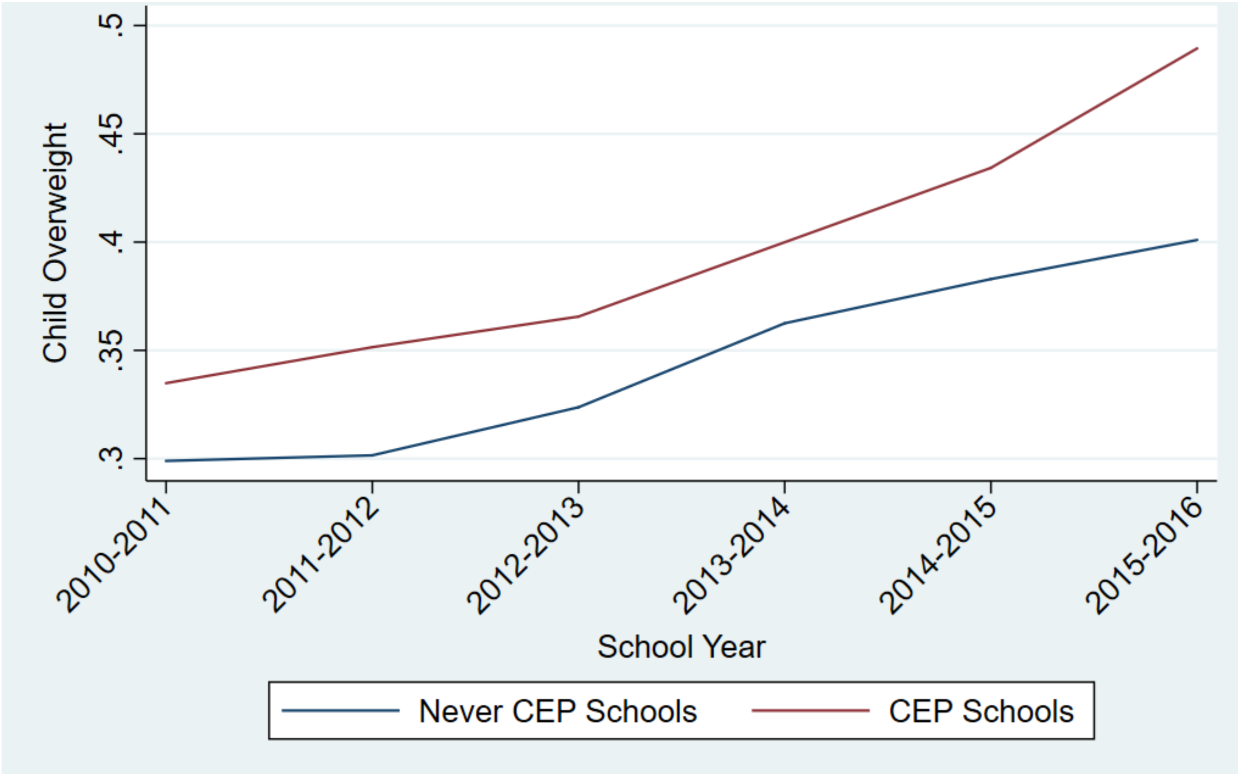

Figure 6: Rate of Child Obesity for CEP and non-CEP schools by Years

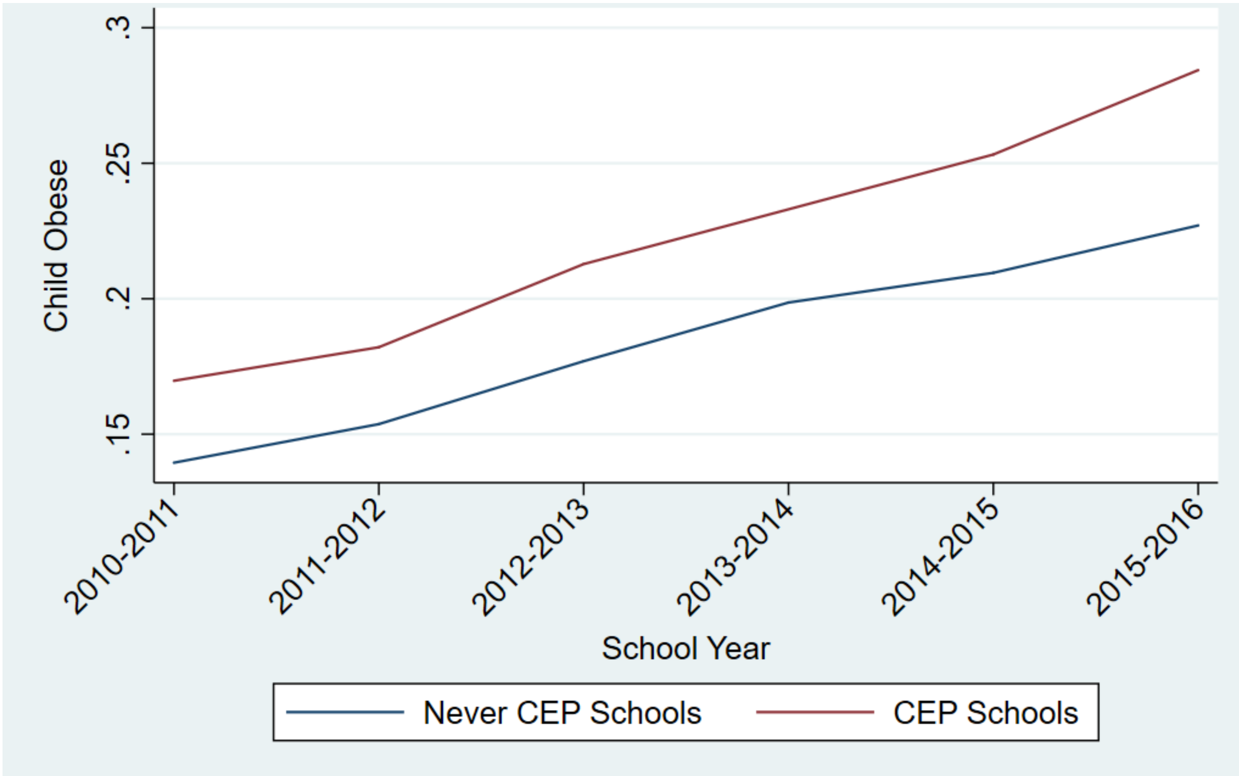

Figure 7: CEP Participation by Identified Student Percentage

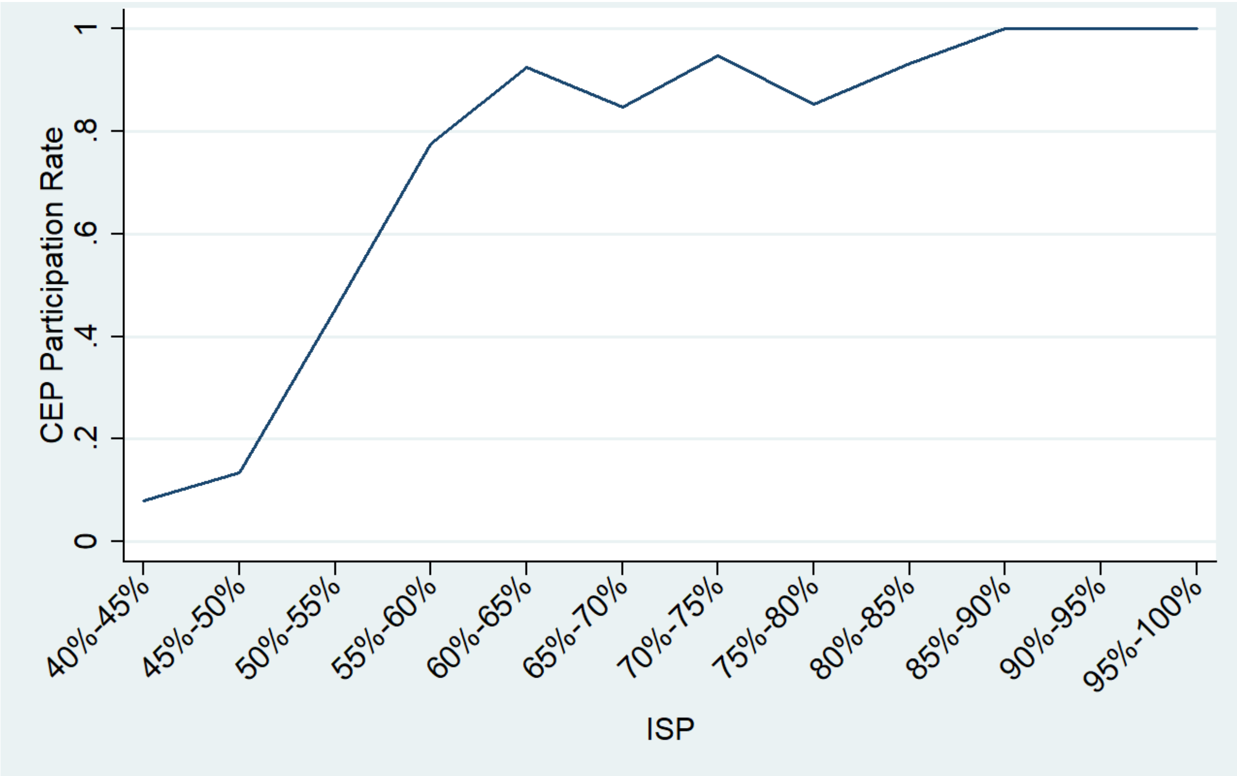

Figure 8: Mean InHFZ% by CEP Eligibility Status Across Time

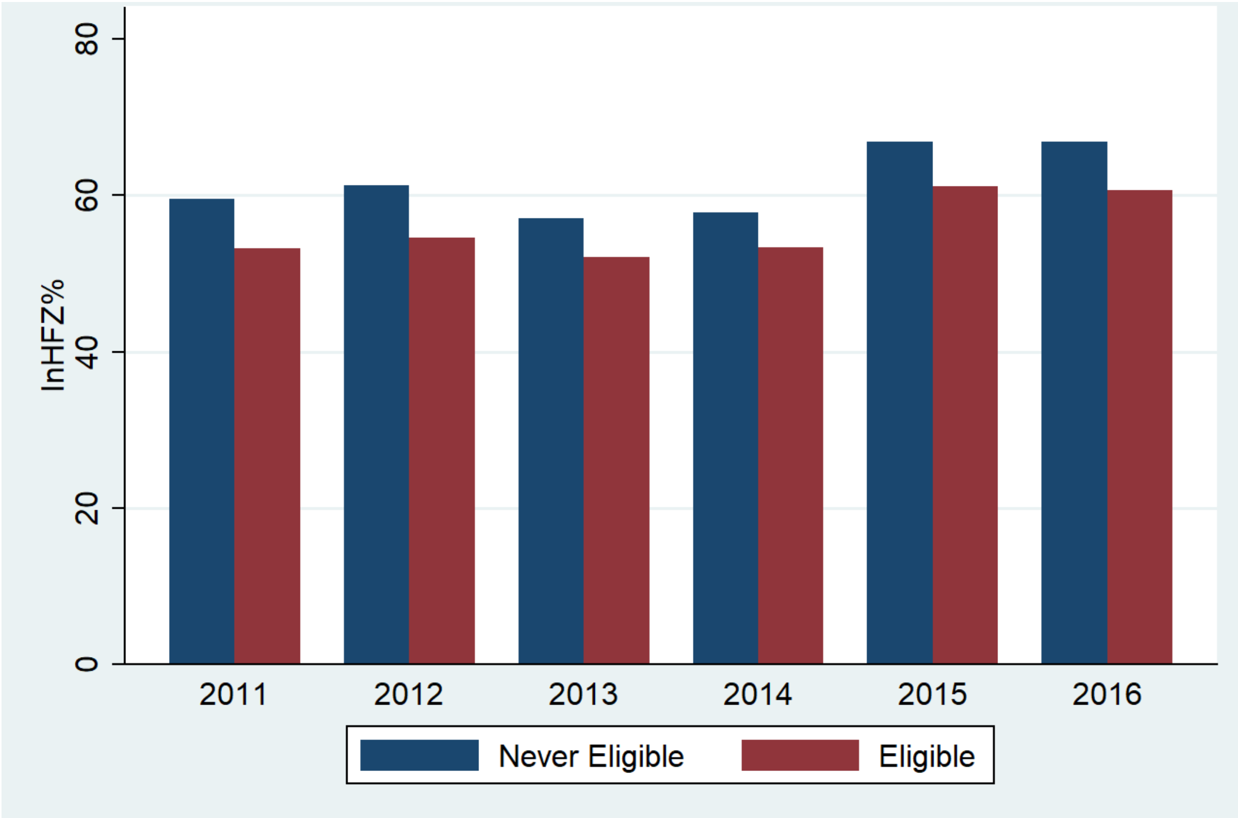

Figure 9: Mean BMI by CEP Eligibility Status Across Time

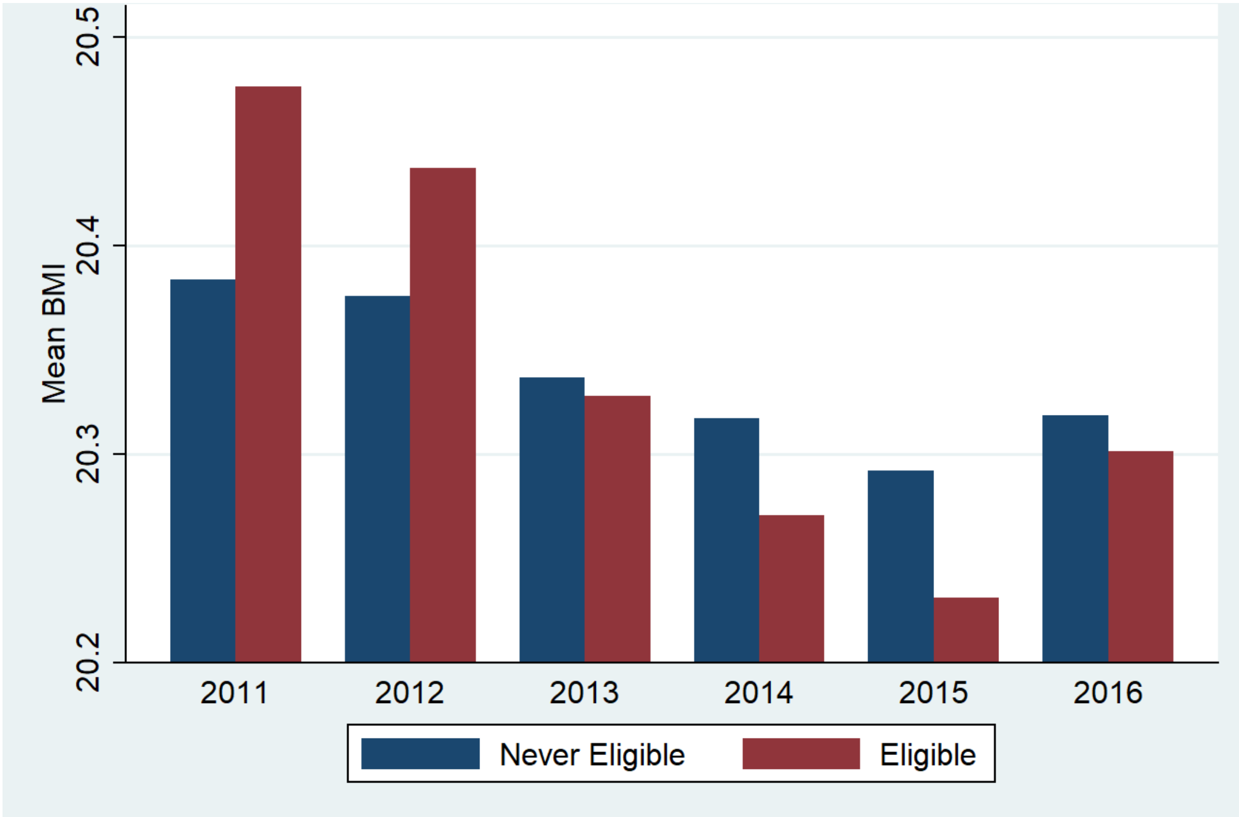

Figure 10: Posterior Draws of μ

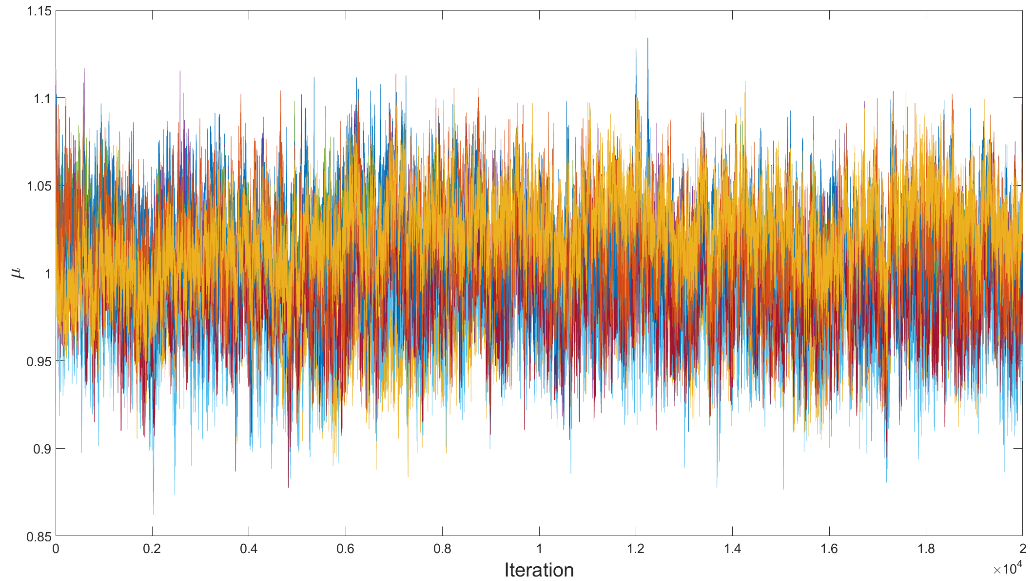

Figure 11: Posterior Draws of σ^2

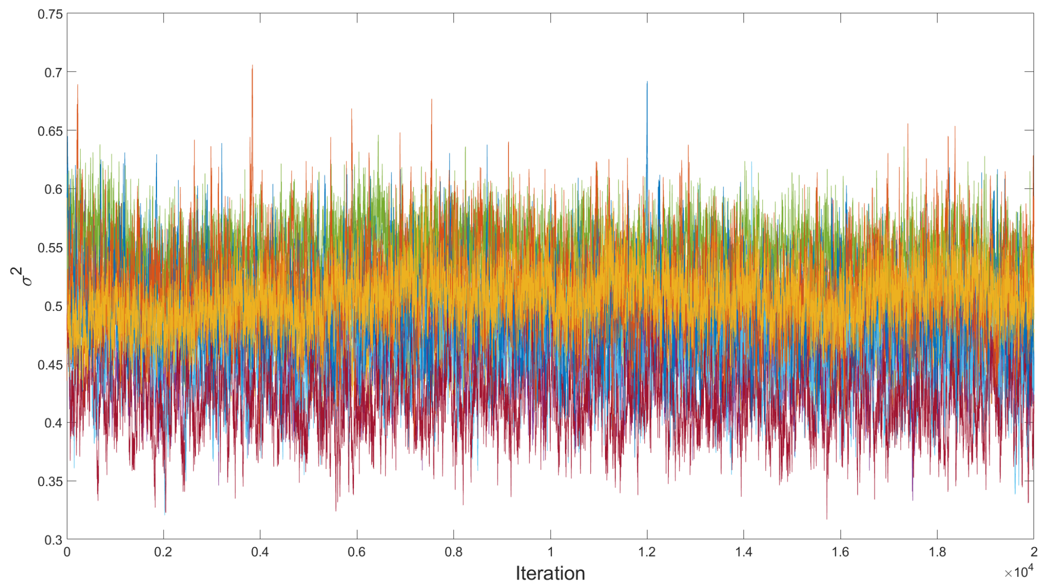

Figure 12: Posterior Draws of δ Mean

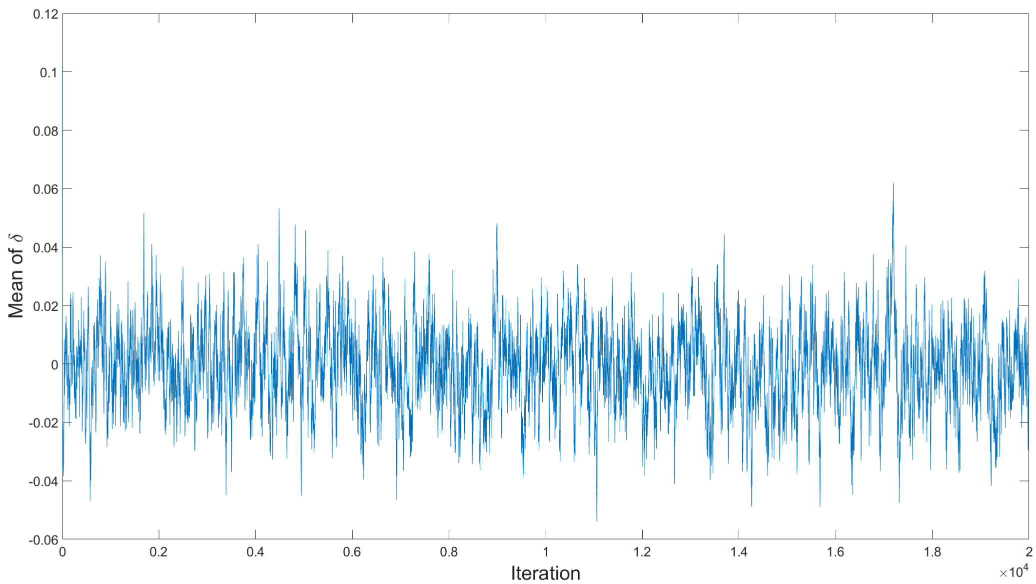

Figure 13: Posterior Draws of γ_2

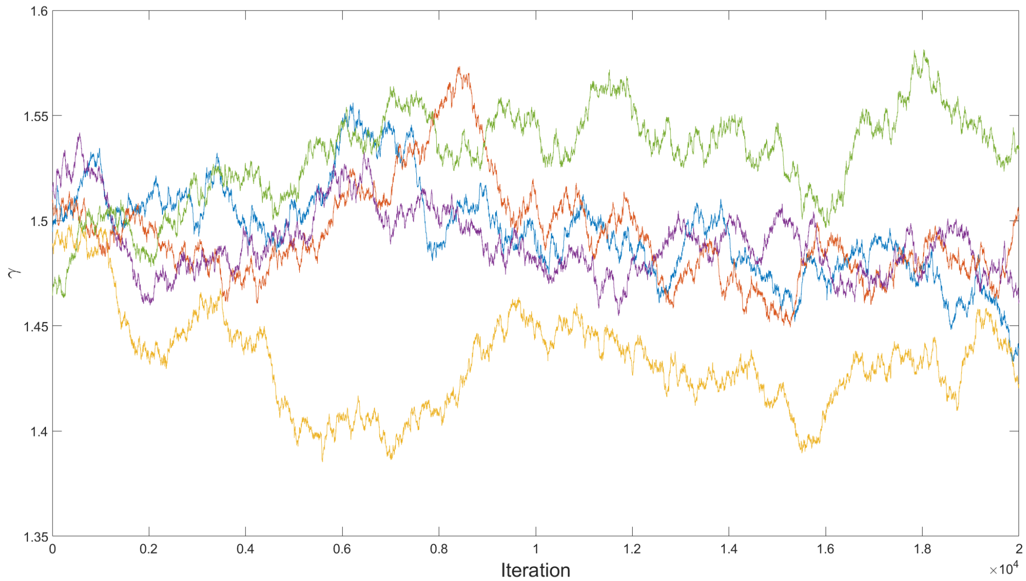

Figure 14: Data Generating Values of δ vs. Posterior Mean Draws of δ

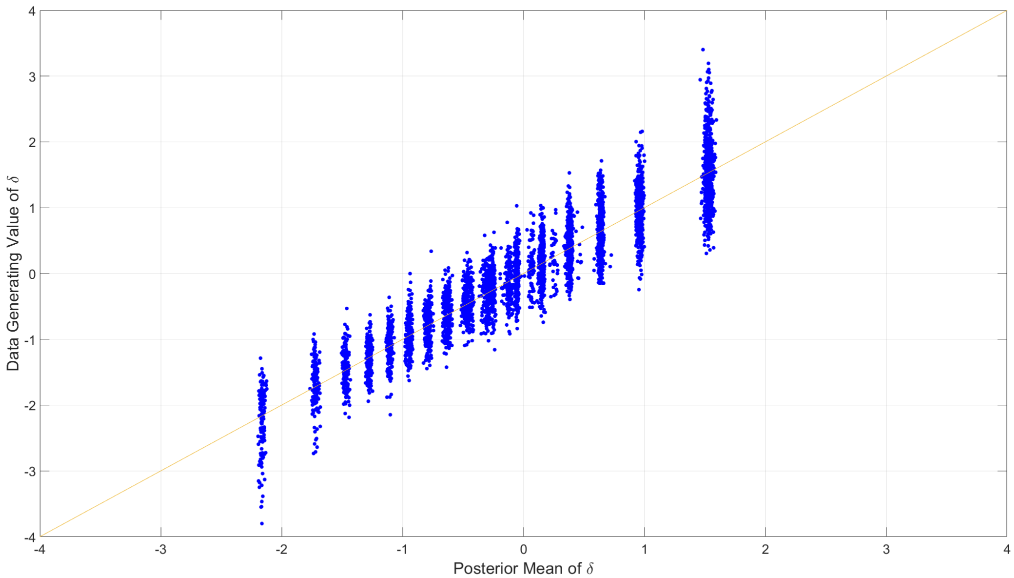

Table 1: Variable Summary Statistics

Pre-CEP Period (2010-2013)	Non CEP Participating Schools					CEP Participating Schools				
	N	Mean	Std. Dev.	Min	Max	N	Mean	Std. Dev.	Min	Max
Child BMI	38,060	17.47	3.35	6.46	55.8	8,820	17.77	3.62	6.36	56.12
Child BMI Percentile	37,880	64.22	28.48	0	99.98	8,770	66.41	28.97	0	99.99
Underweight	37,880	0.03	0.16	0	1	8,770	0.03	0.17	0	1
Healthy Weight	37,880	0.65	0.48	0	1	8,770	0.61	0.49	0	1
Overweight	37,880	0.32	0.47	0	1	8,770	0.36	0.48	0	1
Obese	37,880	0.16	0.37	0	1	8,770	0.19	0.39	0	1
Single Parent Household	32,130	0.3	0.46	0	1	7,120	0.43	0.5	0	1
Mother Married at Time of Birth	39,510	0.66	0.47	0	1	9,480	0.48	0.5	0	1
Child Black	46,150	0.1	0.3	0	1	11,350	0.32	0.47	0	1
Child Male	46,110	0.51	0.5	0	1	11,350	0.51	0.5	0	1
Household Income Below 200% FPL	32,130	0.5	0.5	0	1	7,120	0.71	0.45	0	1
Attends an Urban School	38,660	0.31	0.46	0	1	9,040	0.62	0.49	0	1
Attends a Rural School	38,660	0.25	0.43	0	1	9,040	0.12	0.32	0	1
Non-White % of Students Attending School	38,830	49.23	32.8	0	100	9,160	69.75	33.85	0	100
Post-CEP Period (2014-2015)	Non CEP Participating Schools					CEP Participating Schools				
	N	Mean	Std. Dev.	Min	Max	N	Mean	Std. Dev.	Min	Max
Child BMI	13,650	19.94	4.76	6.07	78.49	2,940	20.75	5.04	6.05	55.4
Child BMI Percentile	13,590	65.81	30.07	0	99.94	2,930	70.7	28.92	0	99.85
Underweight	13,590	0.03	0.16	0	1	2,930	0.03	0.16	0	1
Healthy Weight	13,590	0.58	0.49	0	1	2,930	0.51	0.5	0	1
Overweight	13,590	0.39	0.49	0	1	2,930	0.46	0.5	0	1
Obese	13,590	0.22	0.41	0	1	2,930	0.27	0.44	0	1
Single Parent Household	12,150	0.33	0.47	0	1	2,810	0.44	0.5	0	1
Mother Married at Time of Birth	17,120	0.66	0.47	0	1	4,540	0.48	0.5	0	1
Child Black	20,030	0.1	0.3	0	1	5,430	0.31	0.46	0	1
Child Male	20,020	0.51	0.5	0	1	5,420	0.51	0.5	0	1
Household Income Below 200% FPL	12,150	0.48	0.5	0	1	2,810	0.69	0.46	0	1
Attends an Urban School	13,920	0.32	0.46	0	1	3,310	0.55	0.5	0	1
Attends a Rural School	13,920	0.18	0.38	0	1	3,310	0.13	0.34	0	1
Non-White % of Students Attending School	14,050	51.75	32.51	0	100	3,370	70	32.87	0	100
Identified Student Percentage	19,770	0.32	0.18	0	0.96	4,820	0.62	0.14	0.04	0.99

All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

Table 2: Regressions of Child BMI Percentile on Attending a CEP Participating School

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	1.475***	1.347***	1.216**
	(0.320)	(0.382)	(0.384)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	67,700	49,880	49,880

Robust standard errors in parentheses. Standard errors are clustered at the child-level. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 3: Regressions of Child Weight Categories on Attending a CEP Participating School

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.00165	-0.0165*	0.0149*	0.0141**
	(0.00375)	(0.0088)	(0.00803)	(0.00715)
N	49,880	49,880	49,880	49,880

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 4: Regressions of Child BMI Percentile on Attending a CEP Participating School by Gender

Panel A: Males

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	0.794*	0.709	0.536
	(0.45)	(0.546)	(0.55)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	34,740	25,670	25,670

Panel B: Females

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	2.219***	2.016***	1.916***
	(0.455)	(0.532)	(0.534)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	32,960	24,220	24,220

Robust standard errors in parentheses. Standard errors are clustered at the child-level. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 5: Regressions of Child Weight Categories on Attending a CEP Participating School by Gender

Panel A: Males

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.00662	-0.000432	-0.00619	0.0184*
	(0.00521)	(0.0122)	(0.0111)	(0.0102)
N	25,670	25,670	25,670	25,670

Panel B: Females

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	-0.00367	-0.0332***	0.0369***	0.00909
	(0.0054)	(0.0127)	(0.0116)	(0.01)
N	24,220	24,220	24,220	24,220

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 6: Regressions of Child BMI Percentile on Attending a CEP Participating School by Race

Panel A: Whites

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	1.669***	1.386**	1.372**
	(0.544)	(0.6)	(0.6)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	29,950	24,130	24,130

Panel B: Non-Whites

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	1.110***	1.025**	0.856*
	(0.397)	(0.493)	(0.497)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	37,700	25,760	25,760

Robust standard errors in parentheses. Standard errors are clustered at the child-level. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 7: Regressions of Child Weight Categories on Attending a CEP Participating School by Race

Panel A: Whites

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.00239	-0.00282	0.000429	0.0112*
	(0.00748)	(0.0145)	(0.0125)	(0.0111)
N	24,130	24,130	24,130	24,130

Panel B: Non-Whites

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.000821	-0.0207*	0.0198*	0.0169*
	(0.00422)	(0.0112)	(0.0105)	(0.0093)
N	25,760	25,760	25,760	25,760

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 8: Regressions of Child BMI Percentile on Attending a CEP Participating School by Pre-CEP Period Household Income Level

Panel A: Household Income Below 200% FPL at Some Point During the Pre-CEP Period

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	0.813*	0.78	0.671
	(0.441)	(0.528)	(0.531)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	28,570	20,440	20,440

Panel B: Household Income Never Below 200% FPL During the Pre-CEP Period

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	1.889***	1.486***	1.414**
	(0.509)	(0.563)	(0.565)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	34,450	29,320	29,320

Robust standard errors in parentheses. Standard errors are clustered at the child-level. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child male, child black, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 9: Regressions of Child Weight Categories on Attending a CEP Participating School by Pre-CEP Period Household Income Level

Panel A: Household Income Below 200% FPL at Some Point During the Pre-CEP Period

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.00311	-0.0165	0.0134	0.0101
	(0.00493)	(0.0121)	(0.0112)	(0.0103)
N	20,440	20,440	20,440	20,440

Panel B: Household Income Never Below 200% FPL During the Pre-CEP Period

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.000631	-0.00574	0.0051	0.0135
	(0.00585)	(0.0132)	(0.0118)	(0.00986)
N	29,320	29,320	29,320	29,320

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child male, child black, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 10: Regressions of Child BMI Percentile on Attending a CEP Participating School by Region

	(1) West	(2) Midwest	(3) South	(4) Northeast
CEP School Attendance	1.778	1.09	1.266**	0.147
	(1.24)	(0.761)	(0.523)	(1.177)
N	13,130	9,490	19,570	7,710

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 11: Regressions of Underweight on Attending a CEP Participating School by Region

	(1) West	(2) Midwest	(3) South	(4) Northeast
CEP School Attendance	0.0169	0.00206	0.00493	-0.0125
	(0.0131)	(0.00776)	(0.00463)	(0.0135)
N	13,130	9,490	19,570	7,710

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 12: Regressions of Healthy Weight on Attending a CEP Participating School by Region

	(1) West	(2) Midwest	(3) South	(4) Northeast
CEP School Attendance	-0.0439*	-0.0174	-0.0282**	0.0585**
	(0.0252)	(0.0185)	(0.0123)	(0.0255)
N	13,130	9,490	19,570	7,710

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 13: Regressions of Overweight on Attending a CEP Participating School by Region

	(1) West	(2) Midwest	(3) South	(4) Northeast
CEP School Attendance	0.027	0.0153	0.0232**	-0.046**
	(0.0224)	(0.0168)	(0.0115)	(0.0221)
N	13,130	9,490	19,570	7,710

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 14: Regressions of Obese on Attending a CEP Participating School by Region

	(1) West	(2) Midwest	(3) South	(4) Northeast
CEP School Attendance	0.0169 (0.0131)	0.00871 (0.0143)	0.0151 (0.0102)	0.00751 (0.0211)
N	13,130	9,490	19,570	7,710

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 15: Regressions of Child BMI Percentile on Attending a CEP Participating School for Children in CEP Non-Pilot States

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	1.7^{***} (0.384)	1.791^{***} (0.456)	1.556^{***} (0.459)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	51,660	38,010	38,010

Robust standard errors in parentheses. Standard errors are clustered at the child-level. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 16: Regressions of Child Weight Categories on Attending a CEP Participating School for Children in CEP Non-Pilot States

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.00286	-0.0197*	0.0168*	0.0245***
	(0.00429)	(0.0104)	(0.00962)	(0.00825)
N	38,010	38,010	38,010	38,010

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 17: Regressions of Child BMI Percentile on Attending a CEP Participating School for Children who Attended One School During the Sample Period

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	1.829***	1.638***	1.483***
	(0.396)	(0.465)	(0.468)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	48,810	36,320	36,320

Robust standard errors in parentheses. Standard errors are clustered at the child-level. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 18: Regressions of Child Weight Categories on Attending a CEP Participating School for Children who Attended One School During the Sample Period

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.00129	-0.0228**	0.0215**	0.0187**
	(0.00476)	(0.0106)	(0.00964)	(0.00884)
N	36,320	36,320	36,320	36,320

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 19: Regressions of Child BMI Percentile on Attending a CEP Participating School for Children in the Restricted ISP Sub-Sample

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	1.057**	0.985*	0.993*
	(0.479)	(0.56)	(0.564)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	35,520	26,020	26,020

Robust standard errors in parentheses. Standard errors are clustered at the child-level. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 20: Regressions of Child Weight Categories on Attending a CEP Participating School for Children in the Restricted ISP Sub-Sample

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.00929	-0.0253*	0.016	0.00405
	(0.00608)	(0.0136)	(0.0124)	(0.00974)
N	26,020	26,020	26,020	26,020

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 21: Regressions of Child BMI Percentile on Attending a CEP Participating School for Children in the Minimum Bias Due to Self-Selection on Unobservables Sub-Sample

	(1) Child BMI Percentile	(2) Child BMI Percentile	(3) Child BMI Percentile
CEP School Attendance	1.312***	1.597**	1.416**
	(0.509)	(0.629)	(0.638)
Year Fixed Effects	x	x	x
Child and School Covariates		x	x
Child Fixed Effects			x
N	19,600	13,270	13,270

Robust standard errors in parentheses. Standard errors are clustered at the child-level. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 22: Regressions of Child Weight Categories on Attending a CEP Participating School for Children in the Minimum Bias Due to Self-Selection on Unobservables Sub-Sample

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.0071	-0.0198	0.0127	0.0199
	(0.0057)	(0.0138)	(0.0127)	(0.0122)
N	13,270	13,270	13,270	13,270

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 23: Regression of Child BMI Percentile on Attending a CEP Participating School Instrumented for Using Binary Eligibility and ISP Interactions

Panel A: Binary Eligibility

	(1)
	Child BMI Percentile
CEP School Attendance	6.596***
	(1.419)
N	40,390

Panel B: ISP Interactions

	(1)
	Child BMI Percentile
CEP School Attendance	4.070***
	(0.984)
N	40,130

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 24: Regressions of Child Weight Categories on Attending a CEP Participating School Instrumented for Using Binary Eligibility and ISP Interactions

Panel A: Binary Eligibility

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	0.00236	-0.0973***	0.0949***	0.0618**
	(0.0139)	(0.0338)	(0.031)	(0.0252)
N	40,390	40,390	40,390	40,390

Panel B: ISP Interactions

	(1) Underweight	(2) Healthy Weight	(3) Overweight	(4) Obese
CEP School Attendance	-0.00588	-0.0485**	0.0544**	0.0471**
	(0.00838)	(0.0229)	(0.0214)	(0.0187)
N	40,130	40,130	40,130	40,130

Robust standard errors in parentheses. Standard errors are clustered at the child-level. All regressions include year fixed effects, child fixed effects, and child and school covariates. Child and school covariates include: household below 200% FPL, single parent household, mother married at time of birth, child black, child male, child age in months, child attends an urban school, child attends a rural school, and school's percent of non-white students. All sample sizes are rounded in accordance with the ECLS-K:2011 restricted-use data reporting requirements.

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

Table 25: Variable Summary Statistics 2011-2016

	Mean	StD	Min	Max	Count
Percent Students In Healthy Fitness Zone	58.88	10.51	1.47	92.08	8797
Mean Body Mass Index Score	20.35	2.27	14.97	27.25	8797
Percent Free and Reduced Price Lunches	63.02	26.27	5	100	8797
Ever CEP Eligible	.472	.4992	0	1	8797
Ever CEP Participating	.2687	.4433	0	1	8797
Number of Students	866.08	476.37	75	4192	8797
Percent Black Students	33.37	27.90	0	100	8797
Percent White Students	46.44	28.47	0	99	8797
Percent Migrant Students	.3066	1.325	0	24	8797
Percent Special Education Students	10.95	3.4	0	30	8797
Percent ESL Students	5.63	9.70	0	79	8797
Percent Gifted Students	10.71	8.41	.1	74.3	8797

Table 26: Regressions of Child Weight Categories on Attending a CEP Participating School Instrumented for Using Binary Eligibility and ISP Interactions

Panel A: First Stage Regression of CEP Participation on binary CEP Eligibility

	All Schools	Elementary Schools	Middle Schools	High Schools
CEP Elig	0.466^{***}	0.417^{***}	0.525^{***}	0.605^{***}
	(0.0128)	(0.0141)	(0.0293)	(0.0459)
F-stat	1,327.90	869.49	322.34	173.72
N	7430	4716	1533	1167

Panel B: First Stage Regression of CEP Participation on ISP Interaction Instruments

	All Schools	Elementary Schools	Middle Schools	High Schools
$(100\text{-ISP}) * \text{CEP Elig}$	0.0602^{***}	0.0579^{***}	0.0670^{***}	0.0647^{***}
	(0.0015)	(0.0019)	(0.003)	(0.0028)
$(100\text{-ISP}) * \text{CEP Elig}^{2}$	-0.001^{***}	-0.001^{***}	-0.0011^{***}	-0.0011^{***}
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
F-stat	1,783.29	1,078.1	514.13	385.55
N	7406	4711	1533	1162

Clustered robust standard errors in parentheses. CEP Eligible is an indicator of CEP eligibility for a given school in a given year. ISP represents a school's identified student percentage in a given year. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

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

Table 27: Second Stage Binary CEP Eligibility IV Estimates of CEP Participation Effects on Weight Outcomes by School Grade Type

Panel A: IV Estimated Effects of CEP Participation on Percentage of Healthy Weight Students

	All Schools	Elementary Schools	Middle Schools	High Schools
CEP Participation	0.0181^{***}	0.0098	0.0225	0.0014
	(0.0066)	(0.0072)	(0.016)	(0.0142)
N	7416	4716	1533	1167

Panel B: IV Estimated Effects of CEP Participation on average BMI

	All Schools	Elementary Schools	Middle Schools	High Schools
CEP Participation	-0.197^{***}	-0.201^{**}	-0.0424	-0.147
	(0.0562)	(0.0816)	(0.0896)	(0.118)
N	7416	4730	1533	1168

Clustered robust standard errors in parentheses. CEP participation is an indicator of enrollment in the CEP for a given school in a given year instrumented for using binary CEP eligibility. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 28: Second Stage Identified Student Percentage Interaction IV Estimates of CEP Participation Effects on Weight Outcomes by School Grade Type

Panel A: IV Estimated Effects of CEP Participation on Percentage of Healthy Weight Students

	All Schools	Elementary Schools	Middle Schools	High Schools
CEP Participation	0.0127^{***}	0.0047	0.0229^{**}	-0.0049
	(0.0048)	(0.0052)	(0.0109)	(0.0123)
N	7406	4711	1533	1162

Panel B: IV Estimated Effects of CEP Participation on average BMI

	All Schools	Elementary Schools	Middle Schools	High Schools
CEP Participation	-0.0997^{**}	-0.0551	-0.13^{*}	-0.0195
	(0.0443)	(0.0598)	(0.0759)	(0.102)
N	7406	4711	1533	1162

Clustered robust standard errors in parentheses. CEP participation is an indicator of enrollment in the CEP for a given school in a given year instrumented for using ISP interaction terms. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 29: First Stage Estimates by School Location Type

Panel A: First Stage Regression of CEP Participation on binary CEP Eligibility

	Urban	Rural	Suburbs/Towns
CEP Eligible	0.731^{***}	0.485^{***}	0.378^{***}
	(0.0467)	(0.0324)	(0.0277)
F-stat	421.7	508.07	470.43
N	851	3145	3434

Panel B: First Stage Regression of CEP Participation on ISP Interaction Instruments

	Urban	Rural	Suburbs/Towns
$(100\text{-ISP}) *$ CEP Eligible	0.0649^{***}	0.0625^{***}	0.056^{***}
	(0.003)	(0.0025)	(0.0024)
$(100\text{-ISP}) *$ CEP Eligible ^{2}	-0.0011^{***}	-0.0011^{***}	-0.001^{***}
	(0.0001)	(0.0000)	(0.0000)
F-stat	728.16	738.64	567.88
N	834	3141	3431

Clustered robust standard errors in parentheses. CEP Eligible is an indicator of CEP eligibility for a given school in a given year. ISP represents a school's identified student percentage in a given year. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 30: Second Stage Binary CEP Eligibility IV Estimates of CEP Participation Effects on Weight Outcomes by School Location Type

Panel A: IV Estimated Effects of CEP Participation on Percentage of Healthy Weight Students

	Urban	Rural	Suburbs/Towns
CEP Participation	0.0254^{*}	0.0194^{*}	0.0156
	(0.0134)	(0.0099)	(0.012)
N	840	3142	3434

Panel B: IV Estimated Effects of CEP Participation on average BMI

	Urban	Rural	Suburbs/Towns
CEP Participation	-0.0106	-0.164^{*}	-0.28^{***}
	(0.114)	(0.085)	(0.101)
N	840	3142	3434

Clustered robust standard errors in parentheses. CEP participation is an indicator of enrollment in the CEP for a given school in a given year instrumented for using binary CEP eligibility. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 31: Second Stage Identified Student Percentage Interaction IV Estimates of CEP Participation Effects on Weight Outcomes by School Location Type

Panel A: IV Estimated Effects of CEP Participation on Percentage of Healthy Weight Students

	Urban	Rural	Suburbs/Towns
CEP Participation	0.0364^{***}	0.0159^{**}	0.0037
	(0.0116)	(0.0066)	(0.0084)
N	834	3141	3431

Panel B: IV Estimated Effects of CEP Participation on average BMI

	Urban	Rural	Suburbs/Towns
CEP Participation	-0.0256	-0.0859	-0.11
	(0.102)	(0.0613)	(0.0731)
N	834	3141	3431

Clustered robust standard errors in parentheses. CEP participation is an indicator of enrollment in the CEP for a given school in a given year instrumented for using ISP interaction terms. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 32: Pre-CEP Period Falsification Test by School Grade Type

Panel A: Regression of Percentage of Healthy Weight Students on False CEP eligibility in 2012

	All Schools	Elementary Schools	Middle Schools	High Schools
False CEP Eligibility	-0.0025	-0.0049	-0.0047	-0.0027
	(0.0026)	(0.0039)	(0.0043)	(0.0046)
N	2936	1869	608	459

Panel B: Regression of Average BMI on False CEP Eligibility in 2012

	All Schools	Elementary Schools	Middle Schools	High Schools
False CEP Eligibility	-0.023	0.0626	0.0313	0.0406
	(0.0365)	(0.0484)	(0.0629)	(0.0636)
N	2936	1869	608	459

Clustered robust standard errors in parentheses. False CEP eligibility status is an indicator assigned to schools in 2012 which are CEP eligible after the program's implementation. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 33: First Stage Estimates by School Grade Type with 2013

	All Schools	Elementary Schools	Middle Schools	High Schools
Panel A: First Stage Regression of CEP Participation on binary CEP Eligibility				
CEP Eligible	0.427^{***}	0.387^{***}	0.464^{***}	0.542^{***}
	(0.0197)	(0.0223)	(0.0442)	(0.0635)
F-stat	1,340.83	836.57	343.23	198.24
N	8804	5608	1824	1372
Panel B: First Stage Regression of CEP Participation on ISP Interaction Instruments				
	All Schools	Elementary Schools	Middle Schools	High Schools
$(100\text{-ISP}) * \text{CEP Eligible}$	0.0595^{***}	0.057^{***}	0.0655^{***}	0.0657^{***}
	(0.0016)	(0.0019)	(0.0035)	(0.0029)
$(100\text{-ISP}) * \text{CEP Eligible}^{2}$	-0.001^{***}	-0.001^{***}	-0.0011^{***}	-0.0011^{***}
	(0.0000)	(0.0000)	(0.0001)	(0.0001)
F-stat	1,771.02	1,046.03	525.14	406.86
N	8794	5603	1824	1367

Clustered robust standard errors in parentheses. CEP Eligible is an indicator of CEP eligibility for a given school in a given year. ISP represents a school's identified student percentage in a given year. Schools are assigned the same CEP eligibility status, ISP, and CEP participation status in 2013 as 2014. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 34: First Stage Estimates by School Location Type with 2013

	Urban	Rural	Suburbs/Towns
Panel A: First Stage Regression of CEP Participation on binary CEP Eligibility			
	Urban	Rural	Suburbs/Towns
CEP Eligible	0.697^{***}	0.445^{***}	0.343^{***}
	(0.0536)	(0.0325)	(0.0271)
F-stat	421.7	508.07	470.43
N	983	3735	4086
Panel B: First Stage Regression of CEP Participation on ISP Interaction Instruments			
	Urban	Rural	Suburbs/Towns
$(100\text{-ISP}) *$ CEP Eligible	0.0666^{***}	0.0623^{***}	0.0547^{***}
	(0.0031)	(0.0025)	(0.0026)
$(100\text{-ISP}) *$ CEP Eligible ^{2}	-0.0012^{***}	-0.0011^{***}	-0.001^{***}
	(0.0001)	(0.0000)	(0.0000)
F-stat	713.86	755.17	564.16
N	977	3734	4083

Clustered robust standard errors in parentheses. CEP Eligible is an indicator of CEP eligibility for a given school in a given year. ISP represents a school's identified student percentage in a given year. Schools are assigned the same CEP eligibility status, ISP, and CEP participation status in 2013 as 2014. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 35: Second Stage Binary CEP Eligibility IV Estimates of CEP Participation Effects on Weight Outcomes by School Grade Type with 2013

Panel A: IV Estimated Effects of CEP Participation on Percentage of Healthy Weight Students

	All Schools	Elementary Schools	Middle Schools	High Schools
CEP Participation	0.0149^{**}	0.0071	0.0157	0.0023
	(0.0074)	(0.0074)	(0.018)	(0.0141)
N	8791	5596	1824	1371

Panel B: IV Estimated Effects of CEP Participation on average BMI

	All Schools	Elementary Schools	Middle Schools	High Schools
CEP Participation	-0.176^{***}	-0.184^{**}	-0.0126	-0.177
	(0.0562)	(0.0811)	(0.0898)	(0.116)
N	8804	5608	1824	1372

Clustered robust standard errors in parentheses. CEP participation is an indicator of enrollment in the CEP for a given school in a given year instrumented for using binary CEP eligibility. Schools are assigned the same CEP eligibility status and CEP participation status in 2013 as 2014. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 36: Second Stage Binary CEP Eligibility IV Estimates of CEP Participation Effects on Weight Outcomes by School Location Type with 2013

Panel A: IV Estimated Effects of CEP Participation on Percentage of Healthy Weight Students

	Urban	Rural	Suburbs/Towns
CEP Participation	0.0301^{**}	0.024^{**}	0.0031
	(0.015)	(0.0106)	(0.0131)
N	983	3735	4086

Panel B: IV Estimated Effects of CEP Participation on average BMI

	Urban	Rural	Suburbs/Towns
CEP Participation	-0.003	-0.173^{**}	-0.223^{**}
	(0.114)	(0.086)	(0.101)
N	973	3732	4086

Clustered robust standard errors in parentheses. CEP participation is an indicator of enrollment in the CEP for a given school in a given year instrumented for using binary CEP eligibility. Schools are assigned the same CEP eligibility status and CEP participation status in 2013 as 2014. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 37: Second Stage Identified Student Percentage Interaction IV Estimates of CEP Participation Effects on Weight Outcomes by School Grade Type with 2013

Panel A: IV Estimated Effects of CEP Participation on Percentage of Healthy Weight Students

	All Schools	Elementary Schools	Middle Schools	High Schools
CEP Participation	0.0109^{**}	0.0024	0.0128	-0.001
	(0.0049)	(0.0051)	(0.0117)	(0.0115)
N	8794	5603	1824	1367

Panel B: IV Estimated Effects of CEP Participation on average BMI

	All Schools	Elementary Schools	Middle Schools	High Schools
CEP Participation	-0.0767^{*}	-0.0352	-0.104	-0.0109
	(0.0418)	(0.0567)	(0.0733)	(0.0926)
N	8781	5591	1824	1366

Clustered robust standard errors in parentheses. CEP participation is an indicator of enrollment in the CEP for a given school in a given year instrumented for using ISP interaction terms. Schools are assigned the same CEP eligibility status and CEP participation status in 2013 as 2014. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 38: Second Stage Identified Student Percentage Interaction IV Estimates of CEP Participation Effects on Weight Outcomes by School Location Type with 2013

Panel A: IV Estimated Effects of CEP Participation on Percentage of Healthy Weight Students

	Urban	Rural	Suburbs/Towns
CEP Participation	0.0425^{***}	0.0187^{***}	-0.0021
	(0.0124)	(0.0066)	(0.0084)
N	977	3734	4083

Panel B: IV Estimated Effects of CEP Participation on average BMI

	Urban	Rural	Suburbs/Towns
CEP Participation	-0.023	-0.0487	-0.0916
	(0.0954)	(0.0578)	(0.0703)
N	967	3731	4083

Clustered robust standard errors in parentheses. CEP participation is an indicator of enrollment in the CEP for a given school in a given year instrumented for using ISP interaction terms. Schools are assigned the same CEP eligibility status and CEP participation status in 2013 as 2014. Control variables include percent black students, percent white students, percent migrant students, percent special education students, percent ESL students, and percent gifted students. All regressions include year and school fixed effects. Regressions are weighted by student population in 2014.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 39: Simulated Data Exercise Data Generating Parameters

j	μ	σ^2	γ_2
1	1	0.5	1.5
2	1	0.5	1.5
3	1	0.5	1.5
4	1	0.5	∞
5	1	0.5	1.5
6	1	0.5	∞
7	1	0.5	∞
8	1	0.5	∞
9	1	0.5	∞
10	1	0.5	1.5

Table 40: Simulated Data Exercise Data Generating Parameters vs. Posterior Means and 95% Credible Intervals

j	μ	$\hat{\mu}$	σ^2	$\hat{\sigma}^2$	γ_2	$\hat{\gamma}_2$
1	1	1.011	0.5	0.485	1.5	1.479
		(0.972,1.054)		*(0.443,0.529)*		*(1.449,1.505)*
2	1	0.987	0.5	0.5	1.5	1.482
		(0.946,1.028)		*(0.459,0.544)*		*(1.455,1.510)*
3	1	0.971	0.5	0.471	1.5	1.43
		(0.93,1.012)		*(0.431,0.514)*		*(1.395,1.455)*
4	1	1.005	0.5	0.459	∞	NE
		(0.954,1.056)		*(0.398,0.524)*		
5	1	1.01	0.5	0.543	1.5	1.483
		(0.968,1.05)		*(0.494,0.589)*		*(1.464,1.501)*
6	1	0.97	0.5	0.451	∞	NE
		(0.922,1.021)		*(0.39,0.517)*		
7	1	0.984	0.5	0.419	∞	NE
		(0.936,1.038)		*(0.363,0.49)*		
8	1	1.013	0.5	0.495	∞	NE
		(0.96,1.066)		*(0.426,0.57)*		
9	1	1.011	0.5	0.513	∞	NE
		(0.96,1.065)		*(0.444,0.591)*		
10	1	1.028	0.5	0.510	1.5	1.54
		(0.985,1.07)		*(0.467,0.554)*		*(1.505,1.572)*

Table 41: Food Security Categorization Accuracy of BGRM and FSM Scale

	Proper Match Rate	Mismatch Rate
Panel A: Food Insecurity Defined as δ below the 20th Percentile		
	Proper Match Rate	Mismatch Rate
BGRM	0.9412	0.0588
FSM Scale	0.5194	0.4806
Panel B: Food Insecurity Defined as δ below the 50th Percentile		
	Proper Match Rate	Mismatch Rate
BGRM	0.9076	0.0924
FSM Scale	0.8078	0.1922
Panel C: Food Insecurity Defined as δ below the 5th Percentile		
	Proper Match Rate	Mismatch Rate
BGRM	0.9724	0.0276
FSM Scale	0.3694	0.6306

Table 42: Food Security Categorization Accuracy of BGRM and Adjusted FSM Scale

	Proper Match Rate	Mismatch Rate
Panel A: Food Insecurity Defined as δ below the 20th Percentile		
BGRM	0.9412	0.0588
FSM Scale	0.9158	0.0842
Panel B: Food Insecurity Defined as δ below the 50th Percentile		
	Proper Match Rate	Mismatch Rate
BGRM	0.9076	0.0924
FSM Scale	0.8810	0.1190
Panel C: Food Insecurity Defined as δ below the 5th Percentile		
	Proper Match Rate	Mismatch Rate
BGRM	0.9724	0.0276
FSM Scale	0.9654	0.0346

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