August 2020

THREE ESSAYS ON THE ECONOMICALLY DISADVANTAGED

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Abstract

This dissertation examines disadvantaged students through a unique and novel lens and investigates the effects of Universal Free Meals (UFM) – a program available to schools with sizable economically disadvantaged populations – on student well-being and district financial feasibility. UFM provides free meals to all students, regardless of household income, in an attempt to increase participation in school meals and ensure all students have access to nutritious meals. The Hunger Free Kids act of 2010 expanded the availability of UFM via the Community Eligibility Provision (CEP). CEP allows schools, clusters of schools, or entire districts to adopt UFM if 40 percent or more of students are directly certified eligible for free lunch. The rapid expansion of UFM across the U.S. over the last decade has led to growing empirical evidence of UFM's positive effect on student outcomes such as participation in school food, attendance, test scores, and disciplinary measures. This recent surge in research often characterizes a reduction in stigma as the mechanism through which UFM improves student outcomes. However, this characterization has yet to be empirically examined. As of 2019, a majority of eligible schools across the U.S. have adopted UFM via CEP. The widespread adoption of UFM causes policymakers to speculate if UFM has any unintended consequences, including deleterious effects on student health and district finances.

The first essay in this dissertation sheds light on the impact of UFM on student perceptions of school climate by exploiting the staggered adoption of UFM in New York City middle and high schools. Findings reveal that UFM improves perceptions of bullying, fighting, and safety at school. Moreover, students who would have received free meals in the absence of UFM begin to participate post UFM exposure. This suggests that UFM influences participation and likely perceptions for reasons other than reductions in prices. Another essay examines CEP

adoption in districts across New York State. These findings offer new insights into how districts pay for UFM via CEP while investigating the possible deleterious effects of UFM on student obesity. While the reimbursement structure of CEP is more generous in comparison to other UFM provisions, some fear that CEP exacerbates school food deficits and forces districts to foot the bill. Furthermore, UFM critics worry that students may double up on meals, thereby increasing total caloric intake and contributing to childhood obesity. However, results indicate that UFM improves obesity rates – particularly in older grades and that, on average, federal reimbursements cover increases in expenditures due to meal fee revenue losses and the additional food expenditures that follow an increase in participation.

Economic disadvantage (ECD) is only one of many hurdles students encounter. The last essay in this dissertation descriptively illustrates student disadvantage by examining the prevalence and achievement gaps of the doubly disadvantaged – a group largely ignored in the education landscape. In addition to ECD, disadvantage in this context describes students with disabilities (SWD) and English language learners (ELL). Results indicate that a nontrivial share of students are doubly disadvantaged and that achievement gaps are largest among students that are both ECD and SWD. Furthermore, the essay discusses the implications of ignoring these students for district funding and federal accountability requirements.

While two of the three essays evaluate the effects of providing free school meals on student well-being and district finances among largely ECD populations, the third essay emphasizes the importance of recognizing the complexities of student disadvantage. Together, these essays offer insights into the identification of disadvantaged students and the effects of policies meant to improve circumstances among disadvantaged populations. This dissertation fills gap in the literature by providing profound reflection on the populations these programs serve, as well as the financial feasibility and effects of such programs on student well-being.

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Submitted in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Public Administration

Syracuse University

August 2020

Acknowledgements

This dissertation was made possible thanks to countless mentors, colleagues, friends, and family. The time, energy, and patience these people have provided me are the only reason I have made it this far, and I still pause when recalling each of these experiences on my road to this PhD. I can only hope to provide similar support to others in the same ways in which my mentors have supported me – mentally, intellectually, and spiritually.

I will never be able to accurately express in words the gratitude I feel for the love, support, and encouragement from my husband, Casey Gutierrez. From convincing me (multiple times) that I had what it took to earn my doctorate, to further encouraging me to apply to the best schools – no matter the distance, to then partaking in a long-distance marriage as he finished his own doctoral work in family therapy in Texas. Casey is the warm and fuzzy to my logic and pragmatism, and I am better for it.

To my parents, Mark and Kim Erickstad, who provided me with nothing but opportunity, support, and love. Kim instilled within me a deep independence and persistence that, I think, continues to take her by surprise to this day – though I learned it by watching her daily. She poured her life into her family and made her children her life's work, of which I have benefitted immensely. Since I could talk, I remember Mark always answered my questions with more questions and, therefore, encouraged me to think curiously and critically about the world around me. This is a skill that has saved me from my own ignorance too many times, and it is a tool that I will continue to utilize in both my personal and professional life. Both of my parents imparted core values of thinking of others – which has inspired me and provided me with a solid foundation to pursue policy work concerning public education inequities.

As for my academic journey, many thanks to my advisor Amy Ellen Schwartz for her supervision and the countless lessons learned, all of which I will take with me for the rest of my career and life. I would also like to thank Michah Rothbart for the array of opportunities to work and learn. I cannot say thank you enough to the faculty I had the pleasure of working with while at Syracuse. Sarah Hamersma, Leonard Lopoo, and Katherine Michelmore – I truly appreciate being the beneficiary of your time, knowledge, patience, advice, and support. A special thanks to John Yinger for both the privilege of learning from you and the opportunity to work on timely and policy relevant work.

I would never have made it this far without a few people I had the luck of encountering prior to my doctoral experience. In chronological order, I am grateful for Tisha Emerson for her support, encouragement, and mentorship that began when I was an undergraduate at Baylor. She is responsible for my pursuing my master's degree. Thank you, Scott Cunningham, for aiding me in venturing into the world of education policy and causal inference, as well as your enduring commitment to your students. Pia Orrenius, thank you for an incredible introduction to research as your assistant and for your counsel, guidance, and support.

My cohort, who I learned from and who, very quickly, became friends I will cherish for years to come – Ziqiao Chen, Jeehee Han, Hannah Patnaik, and David Schwegman. Other graduate students who made my research abilities stronger and, more importantly, myself a better person – Stephanie Coffey, Mattie Mackenzie-Liu, Qasim Mehdi, Jud Murchie, Raghav Puri, Laura Rodriguez, and Saied Toosi. Last but certainly not least, thank you to the Center for Policy Research staff – Peggy Austin, Candi Patterson, Emily Minnoe, Laura Walsh, and Katrina Fiacchi – for providing the day to day mechanics that make the work I accomplished possible.

The quote, “If your dreams don’t scare you, they’re not big enough,” is what led me to Syracuse. While the purpose of my time at Syracuse was to gain the knowledge and expertise to write this dissertation and do meaningful work that could, hopefully, make the world a more equitable place, it is was an incredible opportunity for unexpected personal growth, exposure to new ideas, and friendships that will take me around the world and last me for years to come.

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Chapter 1: The Effect of Universal Free Meals on Student Perceptions of School Climate: Evidence from New York City

I. Introduction

In a 2014 report, the United States Department of Agriculture (USDA) found almost half of districts nationwide had some form of “shaming policy,” in which students with unpaid meal debts are publicly acknowledged in front of their peers (USDA, 2014). These actions, in addition to the traditional way in which students pay for school meals, may provide opportunities for students to bullying one another. As of 2019 at least 5,000 districts across the US provide free meals to all students, regardless of household income, under Universal Free Meals (UFM). Because it removes both differential pricing *and* opportunities to shame students, it is likely that UFM affects how students perceive their school climate.

Over 500 schools in New York City (NYC) adopted UFM between 2010 and 2017, while numerous others had already implemented the free meal program prior to 2010. Furthermore, NYC is highly motivated to understand and evaluate school climate, and therefore has administered annual school environment surveys to 6^{th}-12^{th} grade students since 2010. Exploiting the variation in timing of adoption of UFM, as well as rich administrative data and student responses to surveys, I examine whether the UFM expansion in NYC increased meal participation and improved student perceptions of school climate.

School climate describes the quality and character of school life and includes students’ norms, beliefs, relationships, and learning practices, and students’ perceptions of their learning environment can heavily influence their social and emotional well-being, as well as academic success (Freiberg, 2005; Cohen, McCabe, Michelli, & Pickeral, 2009; Wang et. al., 2014; Davis & Warner, 2018; Arseneault, Walsh, Trzesniewski et al., 2006; Juvonen, Wang, & Espinoza, 2011; Lacoe 2016). Schools often administer school environment surveys to gain insight into how students perceive school climate and include questions pertaining to academic climate, student relationships with their peers and teachers, and the institutional environment itself.

In a positive school climate, students feel socially, emotionally, and physically safe (Cohen, McCabe, Michelli, & Pickeral, 2009). A safe environment is necessary for student learning, and inter-group and inter-personal relationships are essential for social and emotional well-being (Maslow, 1970; Cohen, McCabe, Michelli, & Pickeral, 2009; Wang et. al., 2014). However, a negative school climate, in which students feel uncertain about what the day at school will hold, can have detrimental effects on student behavior and academic outcomes. Students preoccupied with their own safety or bullying may be less willing to go to school and less likely to allocate the necessary higher-order thinking skills to the day's lessons, resulting in poor academic outcomes. Prior research finds associations between school climate and academic achievement. More specifically, scholars find bullying affects student feelings of safety, which in turn affects their academic achievement. (Davis & Warner, 2018; Arseneault, Walsh, Trzesniewski et al., 2006; Juvonen, Wang, & Espinoza, 2011; Lacoe 2016).

School cafeterias are particularly salient in shaping school climate as they offer students a daily opportunity to interact with less supervision than what is experienced in a classroom setting. In fact, scholars regularly gain insight into intergroup relations between students of different socioeconomic statuses, abilities, and races by observing student behavior in cafeterias (Carter et. al., 2005; Echols, Solomon, & Graham, 2014). Lunch time for students may be spent a number of ways, including sitting with friends or worrying about with whom to sit. It also provides students the opportunity to eat school lunch, the price of which depends on each student's family income. Given that students with lower household incomes receive meals at a free or reduced price – and that a nontrivial share of schools implement shaming techniques for students with unpaid meal debts – lunch time affords students the opportunity to observe and identify each other as "poor."

UFM removes the visible signals of socioeconomic status by making all meals free for all students. Therefore, one might expect UFM to improve perceptions for those who potentially experience feelings of stigma associated with school food. However, it is possible that UFM improves perceptions for all students. For example, advocates claim the implementation of school uniform policies negate the everyday distraction of "wearing the right thing," particularly for low-income students, by creating an environment in which students' familial resources are not as easily identifiable through students' clothing. However, scholars find more far-reaching effects: school uniforms improve the school environment (i.e., students felt safer and reported less bullying) among *all* students (Murray, 1997; Brookshire, 2016). Similarly, while some may believe UFM is targeted toward providing stigma relief among "poor" students, UFM could improve perceptions of school climate for *all* students, regardless of poverty status or participation behavior.

Over the last decade, an increasing number of schools and districts across the US have adopted UFM. Recent research finds UFM increases participation in school meals, raises test scores, reduces incidences of bad behavior, and may have positive effects on student weight outcomes (Altindag, Baek, Lee, & Merkle, 2018; Gordon & Ruffini, 2018; Schwartz & Rothbart, 2019). Additionally, advocates cite stigma reduction as one of UFM's many benefits, but to date, scholars have yet to provide empirical evidence of this claim.

This paper fills the gap in the literature by being the first to examine whether UFM influences student perceptions of school climate. I use rich/detailed student-level data on meal participation and survey responses for NYC students in grades 6-12 from 2013-2017 to examine whether and to what extent UFM changes student meal participation behavior and perceptions of school climate. The survey covers a range of topics, including whether students observe bullying and feel safe, and response rates range from 80-90 percent of all 6^{th}-12^{th} grade students each year. Survey data is combined with administrative records from the NYC Department of Education (NYCDOE), which includes demographics, certification status for free/reduced-price meals, and the UFM status of the school each student attends. NYC schools adopt UFM at different times across 2013-2017. Therefore, I use a difference-in-differences design and student fixed effects to exploit students' staggered exposure to UFM among those that are ever exposed.

I find UFM increases lunch participation, specifically for students who have ever had to pay for school meals. In addition, UFM improves perceptions of bullying, fighting, and safety for all students, regardless of poverty or participation status. Notably, students that participated in the prior year or have ever certified eligible report feeling safer in less supervised areas of the school, including the cafeteria. An increase in participation is expected when food becomes free for students who, in the absence of UFM, would pay. However, I find UFM induces participation even among students for whom meals were previously free. This suggests that factors beyond price change – perhaps including stigma – influence students' decision to participate.

II. Background & Theory

Background on UFM

The National School Lunch Program (NSLP) and School Breakfast Program (SBP) are federally funded programs that provide subsidized meals to students in over 100,000 public and private schools and childcare centers. All students may participate in school food, however, the *price* each student pays is determined by each student's household income. NSLP and SBP provide free meals to students with household incomes up to 130 percent of the federal poverty line, reduced-price meals to students with household incomes up to 185 percent, and full price meals for all other students. Students are certified as eligible for free/reduced-price meals in one of two ways: 1) returning completed applications indicating the student's household income or 2) through direct certification, in which schools match students to a state-provided database of Supplemental Nutrition Assistance Program (SNAP)/Temporary Assistance for Needy Families (TANF)/Medicaid participants.

SBP and NSLP improve the nutrition of participating students, particularly among disadvantaged students (Bhattacharya, Currie, & Haider, 2006; Gunderson, Kreider, & Pepper, 2012; Smith 2017). However, student participation rates are regarded as low, even among free-lunch certified students. This suggests participation behavior depends on something other than price barriers. There are likely several factors beyond price that influence a student's decision to participate in school food, including school food menus, availability of competing foods in the cafeteria, general attitudes toward school food, and the potential stigma that comes from choosing food associated with "poor kids" (Toosi & Schwartz, 2019; Bhatia, Jones, & Reicker, 2011). Moreover, these attitudes can influence whether students eligible for free/reduce-priced meals turn in applications to be formally certified, and may help explain why, in some contexts, over 10 percent of income eligible students are not certified for free/reduce-priced meals (Domina et. al., 2017).

Student participation in school food, whether by certifying eligible for free/reduced-price meals or simply eating the meals provided, can signal information about familial resources to a student's peers (Stein, 2008). Students demonstrate their agency by choosing whether to participate and make these choices partly based on their perceptions of belonging (Roper & La

Niece, 2009). Moreover, student attitudes toward school food, the idea that it is for "poor kids," and the associated treatment of identified students may affect how participating students feel at school. Therefore, to avoid the stigma of being associated with free food, students may choose to not participate (Bhatia, Jones, & Reicker, 2011; Pogash, 2008).

Furthermore, students may avoid participating in school food to escape their school's consequences for not being able to pay. Some schools force students with unpaid meal debts to throw away their originally provided hot meal and replacing it with a cold sandwich, while others require students to "work off their debts" by cleaning the cafeteria (Reynolds, 2019; Siegel, 2017). Other schools have even sent letters home threatening parents with child protective services for sending their child to school without lunch money (Vera, 2019). While the consequences of these policies can be visible to all students, students directly affected by these policies are those in the position to accrue school food debts (i.e., reduced- and full-price students). Students who are consistently certified for free meals are immune to accruing school meal debt and are therefore immune to the shaming associated with these policies.

Any disincentives to participate among already food-insecure students could have lasting effects on participation behavior. These students may miss out on the benefits associated with school food and full stomachs (Bhattacharya, Currie, & Haider, 2006; Gunderson, Kreider, & Pepper, 2012; Schwartz & Rothbart, 2019). Hunger can make it difficult to concentrate and even potentially increase the likelihood students exhibit aggressive behavior. (Jyoti, Frongillo, & Jones, 2005; Kleinman, R. E. et al, 1998). In an effort to reduce administrative costs and burdens, as well as increase participation in school meals, schools in recent years have adopted more inclusive school meal policies and programs, such as Breakfast in the Classroom (BIC) and

UFM.[1] Schools and school districts can provide UFM for all students, regardless of household income, through federal regulations such as Provision 2 and the Community Eligibility Provision (CEP), among others.[2] These provisions federally reimburse schools or districts depending on certified free/reduced-price meal eligibility rates, thereby decreasing the administrative burden of keeping up with student debts. In addition to simplifying administrative complexities, advocates believe these programs increase participation and decrease the stigma students associate with school-meal program participation (Bhatia, Jones, & Reicker, 2011; Pogash, 2008; Stein, 2008).

Prior research finds UFM increases lunch participation and improves academic and weight outcomes, with few consequences for district finances (Kitchen et al., 2017; Leos-Urbel et. al., 2013; Schwartz & Rothbart, 2019; Rothbart, Schwartz, & Gutierrez, 2020; Davis & Mussadiq, 2018). Moreover, Schwartz and Rothbart (2019) examine potential heterogeneous effects of UFM and find UFM increases lunch participation among both full-price and previously certified free/reduced-price students.

In addition to academic and health outcomes, scholars find UFM decreases student behavioral incidences. Gordon and Ruffini (2018) find UFM through CEP reduces suspension rates among elementary and middle school students, and the West Virginia Department of Education school staff reported a decline in behavioral offenses and disciplinary actions post

[1] BIC provides students with free breakfast in the classroom, as opposed to the cafeteria, after the school day begins.

[2] Since 1980, schools where at least 80 percent of enrolled children are eligible for free or reduce-priced meals can also implement UFM under Provision 1. Since 1995, schools can also offer UFM under Provision 3, which sets reimbursement levels based on the average number of meals served by eligibility group in the most recent year in which the school tracked individual lunch utilization (rather than the average percentages by eligibility group, the method used under Provision 2). Under Provision 3, reimbursements are adjusted for inflation and enrollment, but not for changes in the number of meals served (Schwartz & Rothbart, 2019). Under CEP, a school, cluster of schools, or district can adopt UFM. A school(s) is eligible for CEP if at least 40 percent of the student body is directly certified. There may be a greater incentive to adopt UFM under CEP, as CEP schools and districts are reimbursed at the free lunch rate at 1.6 times the ISP rate. This means schools or districts with direct certification rates greater than or equal to 62.5 percent are reimbursed at the full, “free” federal rate for all meals served.

UFM adoption (Meharie et. at., 2013). Outside of the US, others claim UFM-like programs remove the ability for students to identify others as poor and therefore decrease the incidences of physical fights by 35 percent (Altindag, Baek, Lee, & Merkle, 2018).

The literature often characterizes the mechanism through which UFM affects these outcomes as a decrease in stigma. Exploring the direct effects of UFM on student perceptions of school climate may serve to explain the indirect effects or mechanisms responsible for changes in outcomes already examined in prior research. I contribute to this literature by examining the effect of UFM on student perceptions of school climate, as well as heterogeneous effects of UFM on participation and perceptions by student poverty and participant status. This study uses detailed, student level data to estimate the impact of UFM on 6th-12th grade student participation behavior and perceptions of school climate. Using the longitudinal nature of the panel to identify students' past participation behavior and poverty status, I am able to explore heterogeneous effects for specific students (e.g., those that were eligible for free meals but did not participate in the year prior to UFM exposure). This is the first study to my knowledge that estimates the effects of UFM on student perceptions of school climate using individual meal participation and student responses to school climate surveys.

UFM in NYC

NYC provides a unique environment for studying the effect of UFM on students (Schwartz & Rothbart, 2019). It is the largest school district in the country, serving over 1 million students in 1,500 public schools that are subject to the same rules and regulations. For context, breakfast in NYC has been free for all students since 2004, and students eligible for reduced-price lunch have received free lunch since 2013. The remaining full-price students are responsible for paying $1.75 per school lunch.

Since 2010, over 500 NYC schools have adopted UFM for the first time. According to the NYCDOE Office of School Food, each school's adoption of UFM is based on a myriad of considerations, including but not limited to political, institutional, and administrative factors. The application process can take up to a year, during which a number of items can delay the process, including increased staff workloads, staff turnover, budget considerations, changes in student composition, etc. Similarly, it is unlikely that students, parents, or staff choose schools based on a school's UFM status. However, it is true that schools that adopt UFM are more likely to serve students with similar characteristics. Therefore, the decision to adopt UFM may be endogenous, but each schools' *timing* of UFM adoption is plausibly exogenous.

Prior research has investigated the effect of UFM on stigma and school experiences from the perspective of school principals. Close to three-fourths of NYC principals that responded to surveys agreed that UFM reduced stigma attached to students who qualify for free or reduce-priced meals, and more than half reported improved dining experiences and social interactions among students in the cafeteria (Peralta, 2016). However, scholars have yet to examine the effect of UFM from the student perspective.

UFM takes place in the cafeteria where students can use food to foster connections, show their agency, and manage relationships in a less supervised setting (Neely, Walton, & Stephens, 2014). Figure 1 shows the two theoretical pathways in which UFM may influence student perceptions of school climate. As noted above, UFM likely increases student participation by making meals free. Based on the law of demand, this price elimination largely affects students that, prior to UFM, had to pay for meals. This increase in participation can affect the way students interact in the cafeteria (e.g., more students are taking school meals, standing in line together, and eating the same food). Moreover, these interactions are likely spill over to interactions outside of the cafeteria, such as the classroom or outside of school. Both of these changes in interactions can lead to improved school climate perceptions.

UFM may also affect student perceptions of school climate in ways other than through the price of school food. Prior to UFM, students could identify others with unpaid meal debts as those eating a school-provided peanut butter and jelly sandwich instead of the standard hot meal. Post UFM, all participating students can receive a standard meal for free, eliminating physical signals of poverty. UFM may also alleviate student hunger and, therefore, the subsequent aggression among hungry but formerly not participating students. UFM may also change which students are present in the cafeteria. Some schools have "off-campus" lunch and allow students to leave the school grounds during their lunch break. The availability of free meals may induce students that were previously absent from the cafeteria to eat lunch at school, changing the student composition of the cafeteria.

It is difficult to identify and measure each of these factors individually. However, overall changes in these factors can contribute to the school connectedness vital for creating favorable social environments for all students and influence student interactions in the cafeteria and elsewhere (National Research Council, 2002; Rowe & Stewart, 2009). This "whole school" policy that allows students to interact while minimizing visible social barriers may encourage positive relationships within a school community (Rowe & Stewart, 2011). In turn, these interactions likely influence school climate perceptions.

III. Data

This analysis uses a panel of 6th-12th grade NYC students from 2013-2017 from the NYCDOE and includes individual, daily meal transaction data. The data contains information on over 100,000 unique students, including sociodemographic characteristics such as gender, race/ethnicity, primary language spoken at home, English language learner (ELL) status, free/reduce-priced meal eligibility status, and student with disabilities (SWD) status, as well as individual student responses to the annual NYC School Environment Survey.

NYC School Environment Survey

The NYC School Environment Survey is administered annually to all students in grades 6-12 and includes approximately 60 questions regarding students' experiences in their school environment. To participate, students must be enrolled in their respective school as of early November. Schools administer the survey during the school day and are instructed to give students a full class period to ensure high response rates. Once completed, schools send the survey responses to the office of NYCDOE. The survey period ends and results are collect by March 31 of each year.

The survey uses a Likert scale format, in which students circle the number that corresponds to their answer.[3] Figure 2 shows the standard instructions provided at the top of each survey and describes the voluntary and confidential nature of the survey, as well as the survey's purpose – emphasizing that it is not a test and there are no wrong answers. Approximately 83 percent of the 6^{th}-12^{th} grade student population respond to the survey each year. The NYCDOE provides an annual report regarding the survey results and publicly provides student response rates by school each year. For example, in 2016, 97.8 percent of NYC schools participated, with an average student completion rate of 82.3 percent.

[3] Due to the diverse nature of NYC students, surveys are available in Bengali, Chinese, English, French, Haitian, Creole, Korean, Russian, Spanish, and Urdu.

The NYCDOE collaborates with the Research Alliance for NYC Schools, making minor revisions to the survey each year. General revisions include elimination of items found to be redundant, the addition of items to improve the strength of existing measures, and revision of existing items to improve clarity. Prior research has utilized student responses to the NYC School Environment Survey, examining bullying by grade, the effect of student reported classroom safety on academic achievement, and the impact of school accountability grades on student reported school quality (Lacoe, 2016; Schwartz, Stiefel, & Rothbart, 2016; Rockoff & Turner, 2010).

Prior research by Rockoff and Speroni (2008) finds the NYC School Environment Survey to be an accurate reflection of student perceptions. They find responses are distinct and capture unique perspectives between teachers, students, and parents. Moreover, survey items have high internal consistency and strongly correlate with external measures of the learning environment (Schwartz, Stiefel, & Wiswall, 2016; Rockoff & Speroni 2008; Charbonneau & Van Ryzin, 2012; Nathanson, et. al., 2013). For example, Lacoe (2013) assesses the construct validity of the bullying measure by comparing it to school-level administrative measures of school violence and finds responses are highly correlated with school reports of violence.

Survey Measures

The NYC School Environment Survey includes questions about students' perceptions of bullying, fighting, respect, and feelings of safety. Table 1 lists the survey questions I use as measures of school climate. I treat each question as an individual outcome and code each as indicator variables equal to 1 if the student answered positively (i.e., None of the time/Some of the time for *Bullying* and *Fighting,* and Agree/Strongly agree for *Respect, Safety: Class, Safety:*

Inside, and *Safety: Outside*) and 0 otherwise.[4] As shown in Table 1, survey questions describe peer-reported, general student interactions (i.e., bullying, fighting, respect), as well as self-reported interactions in which the student is a participant (i.e., safety in class, inside, outside). Self-reported measures can provide insight into the perceptions of each specific student, while peer-reported measures are likely to paint a realistic picture of actual peer group interactions during unsupervised times of the school day (Graham, Bellmore, & Juvonen, 2003; Nakamoto & Schwartz, 2010).

Poverty Measures

While all students can participate in school food, eligibility for free or reduce-priced meals, in the absence of UFM, depends on student household income. I define student poverty by his or her certified eligibility for free/reduce-priced meals. Students certify eligible by either returning application forms or direct certification.[5] The majority of schools in NYC use direct certification to determine who is certified eligible for free/reduce-priced meals, however it is possible there are students who are not certified via direct certification (i.e., not a participant in SNAP/TANF/Medicaid) but are income eligible. These students can separately submit free/reduce-priced meal applications to gain access to free/reduce-priced meals. However, the incentive to return forms, and therefore ability of researchers to identify students as poor, declines when schools adopt UFM where all students receive free meals. Therefore, using the free/reduce-priced status of students in UFM years may not be an accurate representation of the

[4] Scholars find using binary indicators captures "empirical action" compared to other ways of categorizing such variables (Cannon, Jacknowitz, & Painter, 2006; Stiefel, Shiferaw, Schwartz, & Gottfried, 2018; Gibbons & Silva, 2011), not to mention ease of interpretation.
[5] A method by which schools match students to a state-provided database of SNAP/TANF/Medicaid participants.

income eligible population. Instead, I use students' poverty status via their certified eligibility for free/reduced price meals in years in which they were not exposed to UFM from 2010-2017.

Michelmore and Dynarski (2017) explore academic achievement by the frequency with which students are observed certified eligible for free/reduce-priced meals and find "sometimes poor" students perform worse than their "never poor" counterparts. However, students observed consistently poor perform far worse than both other groups. Because my poverty indicator directly relates to school meal participation, UFM may affect inconsistently certified eligible students differently than always or never certified students. Therefore, I use students' certified eligibility in nonUFM years to classify students as one of the following time-invariant statuses: *Always Poor, Sometimes Poor*, or *Never Poor*. *Always Poor* equals 1 if a student is consistently observed as poor (certified eligible) in all nonUFM years and 0 otherwise. *Sometimes Poor* equals 1 if the student is observed as poor in one nonUFM year and non-poor in another nonUFM year, and 0 otherwise. *Never Poor* equals 1 if a student is never observed poor in a nonUFM year, and 0 otherwise.

Participation Measures

Student breakfast participation (SBP) and lunch participation (SLP) refer to the percent of school days a student participated each year (i.e., the total number of breakfasts or lunches a student received, divided by the total number of school days in the school year). Daily meal transaction data is available for students who attend schools with Point-of-Service (POS) tracking systems which record meal transactions with student ID and time stamps. These systems require students to either enter their ID number on a keypad or swipe their ID card at the time of transaction to track student account information.

Figure 3 depicts SLP in 2013. A large share of students participates less than 10 percent of the year (~18 days). These are likely instances of students forgetting their brown bag lunch at home and substituting with school meals. The share of students participating more than 10 percent of the time levels off before gradually rising around 50 percent. UFM may affect students' participation and perceptions of school climate differently depending on their past participation behavior. For example, UFM may induce students who rarely participated last year to participate more often. Moreover, if these students did not participate for reasons other than price, UFM may affect their perceptions of school climate. To explore UFM's effect on student participation and perceptions of school climate at the extensive margin, I create the time-varying binary indicator, *Participant* equal to 1 in years in which a student participated in school lunch 10 percent of the time or more *in the prior year.* Similarly, *Non-Participant* is equal to 1 in years a student participated in school lunch less than 10 percent of the time.

Sample Description

The student-level data provided by the NYCDOE is matched using unique student IDs to each student's specific survey response for each year. Because students that ever attend a UFM school are likely different from students that never attend a UFM school, the sample of students includes only to 6th-12th grade students that attended a UFM school at least once from 2013-2017. The analytic sample used in this analysis includes students whose poverty status is observed in a nonUFM year from 2010-2017 and who have at least two years of meal participation and survey response data. Since students must have participation data to be included in the sample, any estimated changes in participation or perceptions cannot be attributed to students' new exposure to POS systems.

Table 2 presents descriptive statistics of Ever UFM students in 2013. Students in the Analytic Sample (Column 2) resemble All Ever UFM students (Column 1). Columns 3-5 display the characteristics of students in the Analytic Sample by subgroup: *Always Poor, Sometimes Poor, and Never Poor.* Students characteristics differ across poverty subgroups. For example, *Always Poor* students are more likely to be Hispanic, speak a language other than English at home, and be ELL and SWD compared to their *Sometimes Poor* and *Never Poor* counterparts. *Never Poor* students, on the other hand, are more likely to be white and less likely to be ELL and SWD.

Always Poor students in sample participate in school meals most often (56 percent of lunches and 13 percent of breakfasts), followed closely by *Sometimes Poor* students. However, *Never Poor* students participate far less in lunch at 37 percent. *Never Poor* students are most likely to positively perceive their school climate. For example, 78 percent of *Never Poor* students report bullying occurs either none or some of the time, compared to 72 percent of *Always Poor* students and 70 percent of *Sometimes Poor* students. Interestingly, *Sometimes Poor* students are the least likely to positively perceive their school environment, consistently reporting 0.5 to 3.0 percentage points below *Always Poor* students. *Sometimes Poor* students may experience more uncertainty than their *Always Poor* counterparts since they are eligible for free meals in some years but not others. Given that baseline perceptions of *Sometimes Poor* students are lower than *Always Poor* students, it is possible that UFM differentially affects their school climate perceptions.

IV. Empirical Strategy

Baseline Model

I use a difference-in-differences strategy with student fixed effects, comparing student perceptions of school climate before and after exposure to UFM from 2013-2017 to estimate the effect of UFM on school meal participation and student perceptions of school climate:

$$Y_{igst} = \beta_0 + \beta_1 UFM_{igst} + \beta_2 \boldsymbol{X'}_{\boldsymbol{isgt}} + \delta_i + \mu_g + \alpha_s + \gamma_t + \varepsilon_{igst} \tag{1}$$

where Y_{igst} is a vector of continuous outcomes regarding meal participation (*SBP*, *SLP)* and binary outcomes of school climate (*Bullying, Fighting, Respect, Safety: Class, Safety: Inside,* and *Safety: Outside)* for student i, in grade g, in school s, in year t. UFM_{igst} takes a value of 1 if student i attends a UFM school in year t. $\boldsymbol{X'}_{\boldsymbol{isgt}}$ is a vector of time-varying student characteristics including ELL and SWD status. Robust standard errors are clustered at the school level, and δ_i, μ_g, α_s, and γ_t are student, grade, school, and year fixed effects, respectively. Given the large size of my panel and therefore, the availability of four different fixed effects, I can compare students to themselves over time, as well as within school while controlling for idiosyncrasies across grades and time. β_1 reflects the effect of UFM exposure on each outcome.

Heterogeneous Effects of UFM

My poverty indicators are directly related to participation in school food since they are based on the price students pay for school meals in the absence of UFM. This, in combination with how students' poverty status might affect how they are perceived at school may lead to differential effects of UFM on student participation and perceptions. Therefore, I examine the effect of UFM by poverty status by interacting a student's time-invariant poverty indicator (i.e., *Always Poor, Sometimes Poor, Never Poor*) with their time-varying UFM status. The coefficient on each interaction identifies the effect of UFM on meal participation and perceptions for each type of student. Because UFM eliminates prices, one might expect participation to increase among those who have previously paid (i.e., Sometimes and Never Poor students). In addition, because UFM potentially removes visible signals of poverty for those ever having been in poverty (i.e., Always and Sometimes Poor students), one might expect these students' perceptions to improve relative to their Never Poor counterparts.

Furthermore, if students base participation decisions on price and/or attitudes toward school food, it is likely that UFM differentially affects student participation and perceptions by participant status. Consequently, I interact each student's prior year participant status (i.e., *Participant, Non-Participant*) with their time varying UFM status. An increase in participation among Non-Participants means UFM induces students to participate, whereas an increase in participation among Participants signifies an increase in the intensity with which students participate. If students associate some sort of stigma with participation in school food, one might expect improvements in perceptions among those that have a history of participating (i.e., Participants).

Prior literature finds increased participation in school lunch post UFM exposure among both poor and nonpoor students (Schwartz & Rothbart, 2019). However, whether students change participation at the intensive or extensive margin remains unknown. The decision to change participation behavior could be a function of several factors, including meal prices and attitudes toward school food. By identifying students using both their participation and poverty status *in the year prior* to UFM exposure, I can examine the heterogeneous effects of UFM on meal participation and student perceptions. Therefore, I restrict the sample to students for whom I observe the year prior to UFM exposure and estimate the following model:

$$Y_{igst} = \beta_0 + \beta_1 UFM_{igst} * PoorParticipant_{t-1} + \beta_2 UFM_{igst} * PoorNonParticipant_{t-1} + \beta_3 UFM_{igst} * NonPoorParticipant_{t-1} + \beta_4 UFM_{igst} * NonPoorNonParticipant_{t-1} + \beta_5 \boldsymbol{X'}_{\boldsymbol{isgt}} + \delta_i + \mu_g + \alpha_s + \gamma_t + \varepsilon_{igst} \quad (2)$$

where *PoorParticipant* is an indicator equal to 1 if the student was certified eligible for free/reduce-priced meals and was a Participant in the prior, nonUFM year. *NonPoorParticipant* equals 1 if the student was not certified eligible and was a Participant. *PoorNonParticipant* equals 1 if the student was certified eligible and was not a Participant, and *NonPoorNonParticipant* equals 1 if the student was not certified eligible and was not a Participant.

Each indicator is interacted with UFM, the coefficient of which identifies the effect of UFM on meal participation and perceptions of school climate for each type of student. For example, *NonPoorNonParticipants* were required to pay for school lunch and did not participate prior to UFM. Therefore, if these students increase SLP post UFM, it is likely due to the decrease in price. However, *PoorNonParticipants* would have received free meals in the absence of UFM and did not participate. If these students increase SLP under UFM where meals are still free, it cannot be attributed to the removal of price barriers. Instead, it is likely due to the other avenue in which UFM can affect participation and perceptions, including factors such as the removal of welfare signals, effects of hunger, and/or composition of the cafeteria (Figure 1).

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Testing for Pre-Trends

I use an event study design to examine whether there are pre-trends in school climate responses prior to UFM exposure. For this analysis, I restrict the sample to students that were exposed to UFM for the first time between 2013 and 2017 and include these students' survey responses from previous years (2010-2012). I compare students that were exposed to UFM early (e.g., 2013) to those that were exposed later (e.g., 2017) and conduct the analysis using:

$$Y_{igst} = \beta_0 + \boldsymbol{UFM\_Year'}_{\boldsymbol{igst}}\boldsymbol{\beta'}_{\mathbf{1}} + \boldsymbol{X'}_{\boldsymbol{isgt}}\boldsymbol{\beta'}_{\mathbf{2}} + \delta_i + \mu_g + \alpha_s + \gamma_t + \varepsilon_{igst} \quad (3)$$

where $\boldsymbol{UFM\_Year'}_{\boldsymbol{igst}}$ is a vector of binary indicators equal to 1 for each year prior to and post UFM exposure. A large share of the variation in treatment comes from the citywide rollout of UFM to all stand-alone middle schools in 2015. The majority of students first exposed between 2013 and 2017 are 7^{th} and 8^{th} graders that, within 1 or 2 years, lose UFM when they move from their UFM middle school to a nonUFM high school. Therefore, post treatment effects should be interpreted as intent-to-treat (ITT) effects and will likely not resemble the average treatment effect on the treated (ATT) effects estimated in difference-in-differences models. I find pretreatment estimates are statistically indistinguishable from zero (see Figure 4), meaning students are not already experiencing improvements in school climate perceptions prior to UFM exposure.

While students lose treatment during this time period, schools that adopt UFM keep it. Therefore, I also conduct this analysis at the school level using school fixed effects and compare *Ever UFM* schools that adopt UFM early to those that adopt later. I examine the pretreatment outcomes among *Ever UFM* schools from 2010-2017 and find pretreatment estimates are statistically indistinguishable from zero (see Figure 5), meaning schools do not appear to adopt UFM based on student perceptions of school environment.

V. Results

Baseline Results

Table 3 shows the effects of UFM on student school meal participation and perceptions of school climate. On average, there is no evidence that UFM increases SBP, which is unsurprising. Because breakfast in NYC has been free since 2004, students do not experience a price change in breakfast under UFM. There is also not strong evidence the UFM increases SLP, which is somewhat unexpected. Prior literature finds UFM increases SLP, and while the point estimate on SLP is positive, it is not statistically significant. It is likely that students facing different price barriers react differently to UFM. Therefore, I further explore heterogeneous effects of UFM on SBP and SLP in the next section.

The effects of UFM on student perceptions of school climate are shown in columns 3-8. Overall, UFM improves student perceptions of bullying, fighting, and safety outside of school with no effect on perceptions of respect, safety in class, or safety inside. UFM improves perceptions of bullying by 2.5 percentage points. While the point estimate may appear small, it is important to remember this analysis takes place in NYC and includes over 100,000 students. To better grasp the magnitude of these effects, 72.3 percent of students reported positive perceptions of bullying in 2013. Therefore, UFM improved perceptions of bullying for 3.5 percent – more than 2,500 students.

UFM improves perceptions of fighting by 3.3 percentage points and feelings of safety outside by 2.3 percentage points.[6] It is likely that UFM changes the composition of students in the cafeteria. Under UFM, students in schools with off-campus lunch policies may be incentivized by free meals to join the cafeteria crowd instead of venturing off campus.[7] If UFM reduces the number of times students take off-campus lunch, it is possible that students are outside less and have fewer opportunities to feel unsafe – leading to an improvement in reported safety outside around the school.

Heterogeneous Effects of UFM

In aggregate, UFM does not affect school meal participation rates. However, effects likely vary by poverty status, as these students face differential price barriers and attitudes toward school food. Upon further examination of heterogeneous effects in Table 4, I find UFM increases SLP among *Sometimes* and *Never Poor* students by 7.3 and 11.8 percentage points, respectively. These results are expected given that students who would have paid for school meals in the absence of UFM now receive the same meals for free. However, I continue to find no effect of UFM on SBP, regardless of student poverty status.

Students who have ever certified eligible for free or reduce-priced meals (*Sometimes* or *Always Poor)* are less likely to report baseline positive perceptions of school climate compared to their *Never Poor* counterparts. School food in the absence of UFM provides opportunities to identify these students as poor and could therefore likely influence their school climate

[6] School meal participation rates in high school are, on average, lower compared to middle school students. Appendix Table A10 shows the effects of UFM on participation and student perception of school climate by middle and high school grades. UFM increases school lunch participation among high schoolers by 15.0 percentage points. The majority of effects on student perceptions (bullying, fighting, and safety outside) happen among middle school students, whereas UFM improves high school students' perception of safety inside.
[7] Specific off-campus policy data available for NYC is unreliable and inconsistent.

perceptions. However, UFM removes the price of school food and the associated signals of socioeconomic status. Table 4, columns 3-8 show the heterogeneous effects of UFM on perceptions by poverty status. UFM shows no effects on perceptions of respect or safety in class, consistent with aggregated effects. Interestingly, all students – regardless of poverty status – report improvements in perceptions of bullying, fighting, and safety outside. UFM improves perceptions of bullying by 2.2-3.2 percentage points, fighting by 3.1-3.9 percentage points, and safety outside of school by 2.0-2.8 percentage points. While one might argue that UFM only removes the stigma associated with free school food for those that are certified eligible for free meals (i.e., Always and Sometimes Poor students), it is the case UFM improves perceptions among all students, including Never Poor students.

One of the "less supervised" areas referred to in *Safe: Inside* is the cafeteria. This question is significant because UFM takes place in the cafeteria. It is possible that, in the absence of UFM, students who have ever been eligible for free meals are stigmatized by their peers for being associated with free meals. These identifications can lead to potential instances of bullying or violence. *Always* and *Sometimes Poor* students report improvements in peer-reported perceptions of bullying and fighting under UFM. However, *Safe: Inside* reveals self-reported information. As shown in column 7, *Always* and *Sometimes Poor* students report feeling safer inside the school (including the cafeteria) by 2.2-2.4 percentage points post UFM exposure. It is possible that these students not only perceive improvements in bullying and fighting overall, but that they themselves were the potential victims of bullying prior to UFM.[8]

[8] Safe: Inside point estimates for Always and Sometimes Poor students are statistically different from Never Poor students at the 10 percent level.

Table 5 shows the effects of UFM by participant status. UFM increases SLP by 3.2 percentage points for students on the intensive margin – that is, those that already participated 10 percent of the time or more in the prior year. *Non-Participants*, on the other hand, are no more likely to participate in school lunch post UFM exposure. Both *Participants* and *Non-Participants* report improved perceptions of bullying by 3.7-4.7 percentage points, fighting by 5.1-5.6 percentage points, and safety outside by 3.3-4.2 percentage points. This means that UFM improves school climate perceptions for all students, regardless of past participation status, as opposed to those that have participated and therefore may be more likely to perceive school climate negatively.

Similar to *Sometimes* and *Always Poor* students, *Participants* may be stigmatized by peers as "poor" for merely participating in school food. In addition, the majority of students in the sample are *Sometimes* or *Always Poor.* Therefore, if students participated, it is likely they were, at some point, eligible for free meals. Just as *Participants* report improvements in peer-reported measures of bullying and fighting, they report improvements in self-reported safety by 2.7 percentage points in less supervised areas. This is, again, consistent with the theory that those associated with school food may be the victims of the bullying and fighting that occur in the absence of UFM.

Schwartz and Rothbart (2019) find UFM increases participation among both poor and nonpoor NYC middle school students. I find increased SLP for all but the poorest 6^{th}-12^{th} grade students. The decision to participate in school food depends on several factors, including meal price. However, if students who were certified to receive free meals in the prior year increase participation rates once exposed to UFM – when meals are still free – there must be a factor other than price affecting their decisions to participate. I further explore the effect of UFM on participation and perceptions of school climate by examining students' participation and poverty status in the year prior to UFM exposure. In the prior year, students are either certified to receive free meals or they are not. Therefore, they are either *Poor* or *NonPoor*. Similarly, students either participated or they did not, and are, therefore, either a *Participant* or a *Non-Participant.*

Table 6 shows the impact of UFM by the interaction of student poverty and participation status in the prior, nonUFM year. Students that were certified to receive free meals but did not participate in the prior year (*PoorNonParticipants)* increase SBP by 4.8 percentage points and SLP by 21.4 percentage points post UFM. Notably, these students did not experience a price change between the prior year and exposure to UFM, suggesting that UFM may eliminate some non-price related barriers to participation and even induce students to participate at the extensive margin.

NonPoorParticipants and *NonPoorNonParticipants,* those that faced a price for meals in the prior year, increase SLP by 9.8 and 21.9 percentage points, respectively. These students experience a price change and increase participation on both the intensive and extensive margins. Meanwhile, UFM does not increase SLP on the intensive margin for those that were eligible for free meals and already participating – *PoorParticipants*. Almost all students report improvements in perception of bullying by 5.1-7.7 percentage points, fighting by 6.8-9.0 percentage points, and safety in the classroom by 4.1-4.5 percentage points. I find no effects on perceptions of respect and positive, but insignificant, point estimates for safety inside and outside.

In summary, I find UFM increases SLP among students who have ever been required to pay for meals in the absence of UFM, as well as students who previously participated in school food. UFM improves perceptions of bullying, fighting, and safety outside the school for all students, regardless of poverty or prior participation behavior. Notably, UFM improves self-reported feelings of safety in less supervised areas among students that may have been marked by their peers as "poor" in the absence of UFM. By investigating effects for students in the year prior to UFM, I find UFM induced participation in both breakfast and lunch among students who, in the previous year were eligible for free meals but did not participate. This finding provides evidence that students' decision to participate consists of factors other than price – such as fear of signaling socioeconomic status via associating with free school food.

Falsification Tests

To provide empirical evidence of exogeneity in student exposure to UFM, I conduct two falsification tests. The first predicts timing of student exposure to UFM using student characteristics and student fixed effects, and the second predicts the timing of schools' UFM adoption using school characteristics and school fixed effects. I restrict the sample to students (schools) without UFM in year t to predict exposure in year t+1 using:

$$UFM_{igst+1} = \beta_0 + \beta_1 \boldsymbol{X'}_{\boldsymbol{isgt}} + \delta_i + \mu_g + \gamma_t + \varepsilon_{igst} \tag{4}$$

where $\boldsymbol{X'}_{\boldsymbol{isgt}}$ is a vector of the previously described outcomes and student (school) characteristics. Additional characteristics include indicators for whether the student (school) has meal transaction availability and principal turnover. As shown in Table 7, Panels A and B student and/or school characteristics do not predict UFM exposure or adoption in the next year.

Robustness Checks

UFM may improve student perceptions of school climate differently over time. For example, the first year of UFM may improve perceptions among students but dwindle as UFM becomes the "new normal." Alternatively, UFM may, on the whole, boost student perceptions of school climate in a consistent and continuous manner. NYC expanded UFM to all free-standing middle schools serving grades 6-8 in 2015. Therefore, a large share of students in my sample are treated in 2015 in 7th and 8th grade, and then lose UFM once they move to a high school without UFM, making it difficult to capture long terms effects of UFM. However, once schools adopt UFM, very few remove it. I examine the long-term effects of UFM at the school level to capture aggregated student perceptions of the school climate in the first, second, and third-plus years of treatment using:

$$Y_{st} = \beta_0 + \beta_1 Year1_{st} + \beta_2 Year2_{st} + \beta_3 Year3_{st} + \beta_4 \boldsymbol{X'_{gt}} + \alpha_s + \gamma_t + \varepsilon_{st} \quad (5)$$

where Year1 takes a value of 1 in the first year of UFM adoption between 2011 and 2017 and 0 otherwise.[9] Year2 takes a value of 1 in the 2nd year of UFM adoption and 0 otherwise, and Year3+ takes a value of 1 in the third year and beyond, and 0 otherwise. $\boldsymbol{X'_{gt}}$ is a vector of control covariates including the percent SWD, ELL, Black, White, and Asian, and α_s and γ_t are school and year fixed effects. As shown in Table A1, UFM improves student perceptions of bullying, fighting, and all types of safety, though effects dissipate over time for bullying. Table A2 shows the long-term effects of UFM at the student level. Given that a large share of students lose UFM within 1 to 2 years, we find little in terms of long-term effects of UFM at the student level.

A nontrivial number of students retained in high school remain classified as 9th graders, and sometimes 10th graders, since students must accumulate credit hours to move on to the next grade. This could alter the effects of UFM if the effect for 9th graders are not estimated using the

[9] To identify which year is the first year of UFM adoption, I have to observe the UFM status in the prior year. Therefore, I use schools that adopt for the first time in 2011.

typical 9th grader definition. Table A3 shows that results are robust to removing these ever-retained students.

Additionally, because survey questions are presented in a Likert scale format and are coded as binary indicators, results may be sensitive to the way in which answers are coded. I recode responses in two ways. First, the binary variable is equal to 1 if the answer resembles anything better than the worst possible response. For example, the bullying question (i.e., At this school, students harass or bully other students), is given a binary indicator equal to 1 for responses “None of the time,” “Some of the time,” and “Most of the time,” and 0 for “All of the time.” This identifies changes students make from the worst possible answer to anything better. Table A4, Panel A shows results using this alternate definition and though point estimates are positive, they are smaller and insignificant compared to baseline results.

Second, I recode responses using a binary variable equal to 1 if the response is the best possible answer and 0 otherwise. For example, the binary indicator for the bullying question is equal to 1 for “None of the time,” and 0 for “Some of the time,” Most of the time,” and “All of the time.” This identifies changes students make from any “worse” response to the “best” response. Table A4 Panel B shows results using this second alternate definition. Point estimates are positive in direction, and though the bullying estimate loses statistical significance, safety in class and inside gain in size and statistical significance. Overall, results are not sensitive to alternate classifications of Likert scale responses.

Survey designers added “I Don’t Know” as a fifth available response to “Respect” from 2015 to 2017. These responses are coded as missing in baseline analyses. Table A5 demonstrates that students who responded “I Don’t Know” are more likely male, Hispanic, and to speak another language at home. As a robustness check, I recode “I Don’t Know” responses to be neutral (0.5 on a scale from 0 – 1) instead of missing and find results do not change significantly, though point estimates change from negative to positive (Table A6).

Lastly, I conduct a Chronbach's alpha analysis to determine whether these survey questions are, overall, representative of school climate, as well as a factor analysis to determine whether questions can be differentiated from one another. Table A7 shows that, in aggregate, questions have a Chronbach's alpha of 0.8. This indicates that responses to questions move in the same direction and are therefore representative of school climate. Furthermore, a factor analysis in Table A8 demonstrates that all safety questions load onto one factor (i.e., Factor 1), whereas bullying, fighting, and respect roughly load onto a second factor (i.e., Factor 2) – though respect loads at a lower rate. As a robustness check, I create Safety Factor 1 and Peer Factor 2 by summing the binary indicators used in the baseline models. Table A9 shows results using these two factor indices. UFM improves students' perceptions of overall safety by 4.9 percentage points and students' perceptions of peer interactions by 5.7 percentage points. These effects on Factor 1 and Factor 2 remain present across poverty and participant status, though point estimates are not statistically different from each other.

VI. Conclusion

Advocates claim UFM increases participation in school food and reduces the stigma associated with participation. Indeed, prior research finds not only does UFM increase participation, it improves test scores, diminishes instances of bad behavior, and provides suggestive evidence of improved weight outcomes for students in participating schools. However, previous research has yet to examine specifically *which* students change participation behavior and the associated changes in student perceptions of school climate.

How students perceive their school climate has consequences for their social and emotional well-being, as well as their academic success. UFM makes meals free for all students and potentially removes visible signals of socioeconomic status. Therefore, UFM has the potential to improve interactions between students in the cafeteria and beyond. Moreover, UFM could differentially affect student participation and perceptions based on their poverty status and/or prior participation behavior.

This paper investigates whether and for whom UFM induces students to change participation behavior, as well as whether and to what extent UFM improves students' school climate perceptions. Using a difference-in-differences framework with student fixed effects, I exploit the staggered exposure of UFM among students ever exposed to UFM. I find UFM increases lunch participation among students with a history of paying for school meals (*Sometimes* and *Never Poor* students). However, students from all poverty and participation designations report improvements in bullying, fighting, and safety outside of school. Notably, students with a history of interacting with school food (*Sometimes* and *Always Poor* students and *Participants)* self-report feeling safer inside the school in less supervised areas, including the cafeteria where UFM takes place. A key finding from this study is that students for whom meals are always free but do not participate prior to UFM (*PoorNonParticipants*) increase participation in school meals under UFM. These students change participation behavior without being subject to price changes, supporting the theory that other factors beyond the price of school food – such as stigma – contribute to students' participation decisions, and that UFM may alleviate these concerns for students.

Though UFM is often directed at improving circumstances for poor or near-poor students, it appears that UFM positively affects student perceptions of school climate, regardless of poverty or prior participation. UFM removes the visible indicators of socioeconomic status associated with free school food. Furthermore, the price removal aspect of UFM can support a more communal and positive atmosphere, as well as cultivate more positive (or at the least fewer negative) interactions between students. Similar to positive effects associated with school uniform policies, these findings suggest that UFM improves school climate perceptions for all students.

While these results are robust to different specifications, results found here are limited to 6^{th}-12^{th} grade students – students who have more autonomy in their school meal participation behaviors. Furthermore, NYC public school students are more likely to be income eligible for free/reduce-priced meals than students in a typical US school district. Moreover, given the cost of living of NYC, even "non-poor" public school students are not what one might consider "high-income."

These results provide the first empirical evidence that UFM improves students' perceptions of school climate. Prior research that examines the effect of UFM on test scores, participation, attendance, and obesity often cites a reduction in hunger and stigma as mechanisms through which UFM might affect these other outcomes. Not only are school climate outcomes examined here an important contribution to the gap in the literature, they can help explain prior findings. School climate influences student social, emotional, and academic wellbeing. Therefore, while UFM aims to provide free meals for those that might otherwise go hungry, the results in this paper suggest that UFM also improves student perceptions of school climate and, in turn, improves student experiences and welfare.

Figure 1: Pathways UFM May Influence School Climate Perceptions

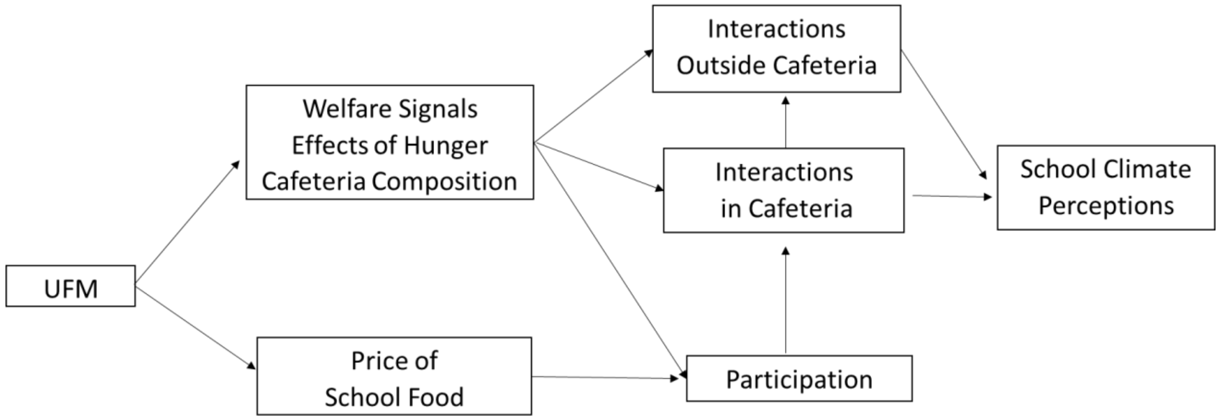


Figure 2: Introductory Instructions for NYC Student Survey, 2016

NYC Department of Education

NYC SCHOOL SURVEY
2016 Student Survey
Go GREEN! Please take this survey on-line! Go to www.nycschoolsurvey.org.

We want to know what YOU think about your school. This survey will give your school important information it can use to improve your education.

This survey is confidential. Your answers will be combined with those of other students at your school. No one at your school will ever see your individual answers. This is not a test and there are no wrong answers. You do not have to answer any question you do not wish to answer, but we hope you will answer as many questions as you can.

When you have finished the survey, include only the answer sheet in the envelope provided and seal the envelope. You may remove your name from the envelope by peeling the label with your name off of the front of the envelope. Hand the sealed envelope with your completed answer sheet to your teacher.

Figure 3: Distribution of Student Lunch Participation, 2013

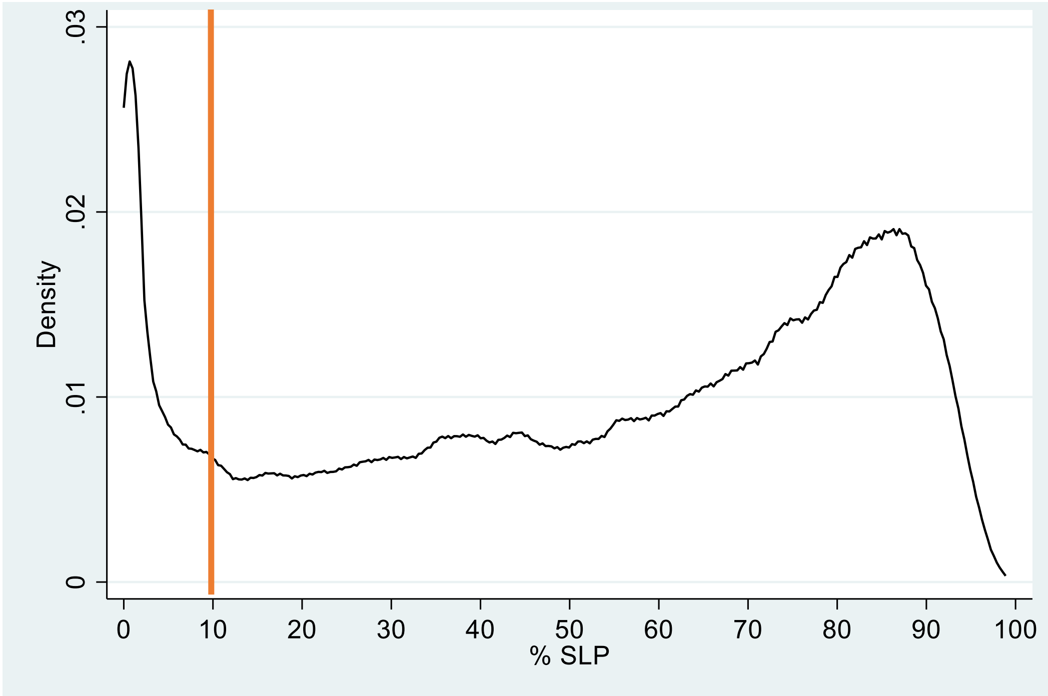
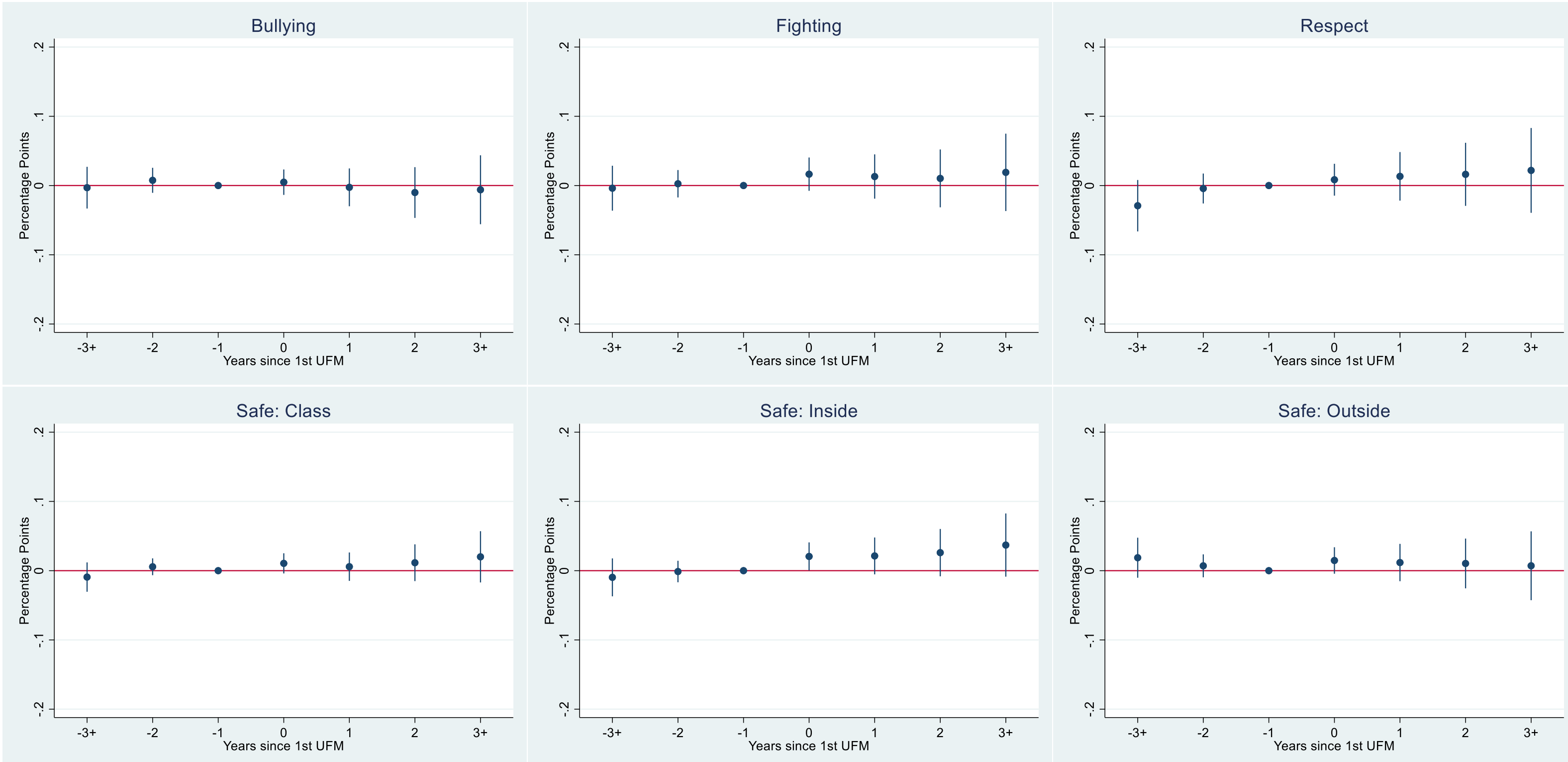
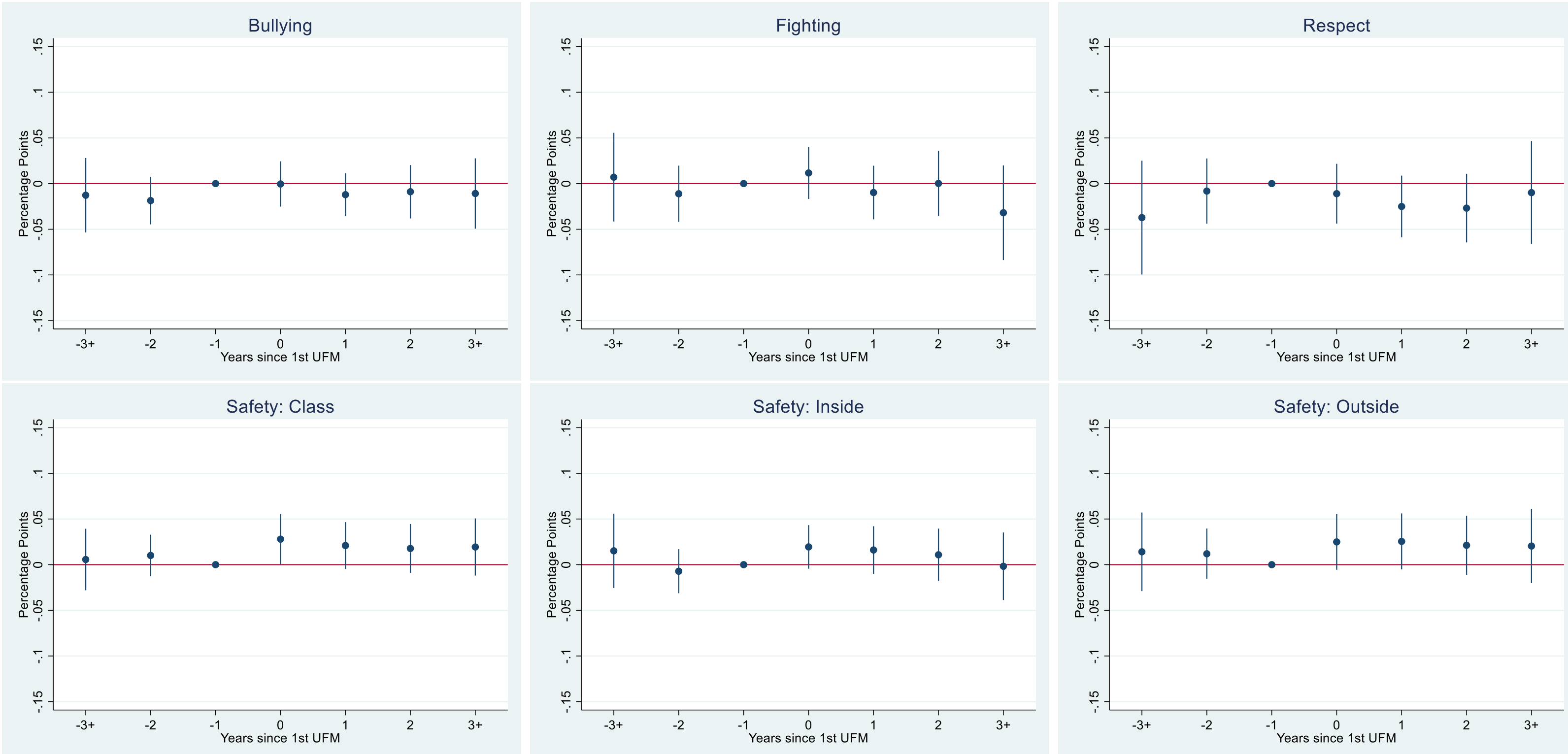


Figure 4. Pre-trend & Event Study Analysis: Initial Exposure as Intent to Treat (ITT), Student-Level, 2010-2017



Notes: Figures display point estimates and bars indicate 95 percent confidence intervals from pre-trend analysis of Ever UFM students first exposed to UFM between 2013-2017 (~85,638 students in ~998 schools). Students exposed to UFM prior to 2013 and students that are "always" UFM from 2013 to 2017 are excluded from this analysis. Models control for SWD and ELL status and include student, grade, school, and year fixed effects. Robust standard errors clustered by school. Zero (0) is the first year of UFM exposure and negative 1 (-1) is the reference year, the year prior to UFM exposure. Data used to estimate "-3+" includes 3 or more years pretreatment data, and data used to estimate "3+" includes 3 or more years of post-treatment data.

Figure 5. Pre-trend & Event Study Analysis: Initial Adoption as Intent to Treat (ITT), School-Level, 2010-2017



Notes: Figures display point estimates and bars indicate 95 percent confidence intervals from event study of Ever UFM schools from 2010-2017 (~2,900 observations of ~400 schools). Always UFM schools (111) are used to estimate post effects. Models control for percent SWD, ELL, Poor, Black, White, Hispanic, and Asian and include school and year fixed effects. Robust standard errors clustered by school. Zero (0) is the first year of UFM exposure and negative 1 (-1) is the reference year, the year prior to UFM exposure. Data used to estimate "-3+" includes 3 or more years pretreatment data, and data used to estimate "3+" includes 3 or more years of post-treatment data.

Table 1: Measures of Bullying, Fighting, Respect, and Safety

Category	New York City School Survey Question	Variable Name	=1 If Respond
Bullying	"At this school, students harass or bully other students."	Bullying	None or some of the time
Fighting	"At this school, students get into physical fights."	Fighting	
Respect	"Most students at this school treat each other with respect."	Respect	Agree or strongly agree
Safety	"I feel safe in my classes at this school."	Safe: Class	
	"I feel safe in the hallways, bathrooms, locker rooms, and cafeteria of this school."	Safe: Inside	
	"I feel safe outside around this school."	Safe: Outside	

Table 2: Characteristics of 6th-12th Grade Ever UFM Students, 2013

	All Ever UFM	Analytic Sample	Analytic Sample		
			Always	Sometimes	Never
Characteristics					
Female	50.2	50.0	50.5	50.0	47.6
White	11.3	14.4	7.3	17.7	43.5
Black	24.0	22.8	23.5	26.3	15.8
Hispanic	47.3	42.2	49.3	32.8	20.3
Asian	16.5	19.7	19.1	22.1	19.7
Other Language	50.4	50.4	56.4	44.5	29.5
ELL	14.1	12.1	15.2	7.5	3.3
SWD	13.0	12.0	12.6	11.5	9.8
Mean No. Obs.	3.9	4.0	3.9	4.4	4.0
Mean Grade	7.1	7.0	7.2	6.6	6.6
Outcomes					
SBP	12.8	12.2	13.4	10.5	8.6
SLP	51.8	52.4	56.5	50.0	36.7
Bullying	73.9	72.3	71.7	70.2	77.7
Fighting	78.7	77.1	77.3	74.5	79.0
Respect	74.2	73.2	73.1	71.3	75.5
Safe: Class	59.6	58.3	58.5	55.0	60.9
Safe: Inside	84.4	83.8	84.0	81.9	85.2
Safe: Outside	88.2	87.9	87.4	86.9	91.0
# Students	83,135	33,553	22,868	5,684	5,001

Notes: Ever UFM students in 6th to 12th grade with at least two years of data from 2013-2017. Analytic sample includes students whose poverty status is observed in a non-UFM year and students with meal participation data.

Table 3: The Effect of UFM on Meal Participation and Perceptions, 2013-2017

	(1) SBP	(2) SLP	(3) Bullying	(4) Fighting	(5) Respect	(6) Safe: Class	(7) Safe: Inside	(8) Safe: Outside
UFM	-0.003	0.019	0.025**	0.033**	-0.002	0.004	0.020	0.023**
	(0.016)	(0.016)	(0.011)	(0.017)	(0.014)	(0.010)	(0.012)	(0.011)
Student Char	Y	Y	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
2013 Means	0.122	0.524	0.723	0.771	0.732	0.583	0.838	0.879
No Students	102,895	102,895	100,109	99,947	95,647	100,847	101,034	100,927
No Schools	867	867	863	863	861	863	862	864
Observations	325,334	325,334	310,875	310,507	290,502	315,743	316,418	315,863
R-squared	0.646	0.739	0.489	0.517	0.525	0.440	0.472	0.483

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM/POS students in 6th to 12th grade with at least two years of data, whose middle or high school poverty status is observed in a nonUFM year from 2010-2017. Models in columns 3-8 control for cohort-specific linear time trends. Student characteristics include indicators for SWD and ELL.

Table 4: The Effect of UFM on Meal Participation and Perceptions by Poverty, 2013-2017

VARIABLES	(1) SBP	(2) SLP	(3) Bullying	(4) Fighting	(5) Respect	(6) Safe: Class	(7) Safe: Inside	(8) Safe: Outside
UFM								
Always Poor	-0.007	-0.027	0.022**	0.031*	-0.002	0.006	0.022*	0.020*
	(0.016)	(0.017)	(0.011)	(0.017)	(0.014)	(0.010)	(0.012)	(0.011)
Sometimes Poor	0.000	0.073***	0.032***	0.033*	0.007	0.003	0.024*	0.028**
	(0.016)	(0.016)	(0.012)	(0.018)	(0.015)	(0.011)	(0.014)	(0.012)
Never Poor	0.009	0.118***	0.030**	0.039**	-0.005	-0.003	0.012	0.028**
	(0.015)	(0.014)	(0.013)	(0.017)	(0.015)	(0.009)	(0.013)	(0.013)
Student Char	Y	Y	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
No Students	102,895	102,895	100,109	99,947	95,647	100,847	101,034	100,927
No Schools	867	867	863	863	861	863	862	864
Observations	325,334	325,334	310,875	310,507	290,502	315,743	316,418	315,863
R-squared	0.646	0.744	0.489	0.517	0.525	0.440	0.472	0.483

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM/POS students in 6th to 12th grade with at least two years of data, whose middle or high school poverty status is observed in a nonUFM year from 2010-2017. Student characteristics include indicators for SWD and ELL. Models in columns 3-8 control for cohort-specific linear time trends. Sample includes ~62,700 (72 percent) Always Poor, ~10,600 (12 percent) Sometimes Poor, and ~14,000 (16 percent) Never Poor students. All SLP point estimates are statistically different from the other at the 1 percent level. Bullying point estimates are not statistically different from each other, with the exception of comparing Always and Sometimes Poor, which is statistically different at the 10 percent level. Individual Fighting and Safe: Outside point estimates are not statistically different from each other. Safe: Inside point estimates are statistically different from each other at the 10 percent level, with the exception of Never and Sometimes Poor estimates.

Table 5: The Effect of UFM on Meal Participation and Perceptions by Participant Status, 2013-2017

VARIABLES	(1) SBP	(2) SLP	(3) Bullying	(4) Fighting	(5) Respect	(6) Safe: Class	(7) Safe: Inside	(8) Safe: Outside
UFM								
Participant	-0.003	0.032*	0.037***	0.051***	0.017	0.007	0.027*	0.033**
	(0.015)	(0.018)	(0.014)	(0.019)	(0.018)	(0.012)	(0.014)	(0.013)
NonParticipant	-0.007	-0.010	0.047***	0.056***	0.022	0.005	0.021	0.042***
	(0.015)	(0.018)	(0.016)	(0.020)	(0.019)	(0.013)	(0.016)	(0.015)
Student Char	Y	Y	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
No Students	73,001	73,001	70,589	70,463	66,122	71,294	71,389	71,289
No Schools	788	788	783	784	776	785	785	785
Observations	200,717	200,717	191,952	191,587	176,476	194,730	195,080	194,716
R-squared	0.692	0.780	0.532	0.558	0.572	0.486	0.517	0.531

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01 Sample includes observations of Ever UFM/POS students in 6^{th} to 12^{th} grade with at least two years of data, whose middle or high school poverty status is observed in a nonUFM year from 2010-2017. Sample uses last year's participation status, and therefore loses one year of observations. Models in columns 3-8 control for cohort-specific linear time trends. Student characteristics include indicators for SWD and ELL. Sample includes ~51,800 (80 percent) Participants and ~13,900 (20 percent) Non-Participants. Participant and NonParticipant point estimates for bullying, fighting, and safety outside are not statistically different from each other.

Table 6: Heterogeneous Effects of UFM on Meal Participation and Perceptions by Poverty and Participant Status in Prior, nonUFM Year, 2013-2017

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	SBP	SLP	Bullying	Fighting	Respect	Safe: Class	Safe: Inside	Safe: Outside
UFM								
PoorParticipant	0.012	0.028	0.051*	0.068**	-0.006	0.045**	0.023	-0.003
	(0.017)	(0.022)	(0.030)	(0.034)	(0.038)	(0.019)	(0.026)	(0.028)
PoorNonParticipant	0.048***	0.214***	0.052	0.076*	-0.022	0.044*	0.014	0.006
	(0.015)	(0.019)	(0.034)	(0.039)	(0.038)	(0.023)	(0.030)	(0.030)
NonPoorParticipant	0.017	0.098***	0.064*	0.070**	-0.001	0.041*	0.021	0.010
	(0.015)	(0.022)	(0.034)	(0.035)	(0.040)	(0.021)	(0.030)	(0.028)
NonPoorNonParticipant	0.048***	0.219***	0.077**	0.090**	0.006	0.026	-0.000	0.028
	(0.015)	(0.025)	(0.033)	(0.036)	(0.040)	(0.021)	(0.028)	(0.032)
Student Char	Y	Y	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
No Students	40,529	40,529	37,307	37,455	34,346	38,769	38,858	38,740
No Schools	588	588	556	555	550	563	561	562
Observations	81,125	81,125	74,679	74,973	68,743	77,603	77,782	77,546
R-squared	0.803	0.871	0.642	0.656	0.648	0.603	0.629	0.628

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01 Sample includes observations of Ever UFM/POS students in 6th to 12th grade with at least two years of data, whose middle or high school poverty status is observed in a nonUFM year from 2010-2017. Sample uses last year's poverty and participation status in the prior, non-UFM year. Models in columns 3-8 control for cohort-specific linear time trends. Student characteristics include indicators for SWD and ELL. Sample includes ~26,400 (65 percent) PoorParticipants, ~3,300 (8 percent) PoorNonParticipants, ~6,100 (15 percent) NonPoorParticipants, and ~4,600 (12 percent) NonPoorNonParticipants. Point estimates by participant and poverty are all statistically different from each other at the 1 percent level for both SBP and SLP. Point estimates by participant and poverty are not statistically different from each other for bullying, fighting, and safety in the classroom, with the exception of classroom safety for PoorParticipants and NonPoorNonParticipants.

Table 7 – Panel A: Regression Results, New UFM Exposure for Students, 2012-2016

	UFM Next Year
Respect	-0.006
	(0.004)
Bullying	-0.001
	(0.004)
Safe: Class	-0.001
	(0.004)
Safe: Inside	-0.007*
	(0.004)
Safe: Outside	-0.001
	(0.004)
Fighting	-0.002
	(0.004)
Clean	-0.001
	(0.003)
ELL	0.004
	(0.011)
Poor	0.009
	(0.007)
POS	0.014
	(0.011)
Principal Change	0.013
	(0.010)
2 Principals	0.003
	(0.017)
Principal Change Next Year	-0.001
	(0.009)
Student FE	Y
Year FE	Y
Grade FE	Y
School FE	Y
No Students	95,471
No Schools	729
Observations	148,748
R-squared	0.956

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM students in 6th to 12th grade with at least two years of data who are not exposed to UFM in the current year from 2012-2016 (2017 observations excluded as UFM status in t+1 is not observed).

Table 7 – Panel B: Regression Results, New UFM Adoption for Schools, 2010-2016

	UFM Next Year
Respect	-0.021
	(0.024)
Bullying	0.022
	(0.026)
Safe: Class	0.001
	(0.005)
Safe: Inside	-0.013
	(0.016)
Safe: Outside	-0.001
	(0.004)
Fighting	-0.019
	(0.022)
Clean	-0.000
	(0.004)
% ELL	0.003
	(0.005)
% Poor	-0.016
	(0.019)
% Black	-0.008
	(0.019)
% Hispanic	0.011
	(0.022)
% White	0.000
	(0.018)
% Asian	0.011
	(0.021)
POS	0.001
	(0.002)
Principal Change	0.004
	(0.005)
2 Principals	0.001
	(0.003)
Principal Change Next Year	0.001
	(0.002)
No Schools	260
Observations	954
R-squared	0.996

Notes**:** Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Includes school and year fixed effects. Sample includes observations of Ever UFM schools serving 6^{th} to 12^{th} grade students with at least two years of data who are not exposed to UFM in the current year from 2010-2016 (2017 observations excluded as UFM status in t+1 is not observed).

VII. Appendix A

Table A1. Differential Effects of UFM Adoption over Time, School Level, 2011-2017

	(1) Bullying	(2) Fighting	(3) Respect	(4) Safe: Class	(5) Safe: Inside	(6) Safe: Outside
UFM						
Year 1	0.021*	0.053***	-0.010	0.028**	0.030**	0.023*
	(0.012)	(0.016)	(0.017)	(0.011)	(0.012)	(0.012)
Year 2	0.009	0.038**	-0.027	0.031***	0.044***	0.043***
	(0.012)	(0.016)	(0.018)	(0.012)	(0.013)	(0.015)
Year 3+	0.001	0.037*	-0.040*	0.026	0.035**	0.036*
	(0.018)	(0.021)	(0.024)	(0.018)	(0.017)	(0.019)
Student Char	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
No Schools	272	272	272	272	272	272
Observations	1,899	1,899	1,901	1,901	1,900	1,900
R-squared	0.659	0.691	0.600	0.450	0.539	0.546

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes ever UFM schools from 2010-2017 that adopted UFM for the first time between 2011 and 2017, and responses are aggregated from 6^{th} – 12^{th} grade Ever UFM students whose poverty status is observed in a nonUFM year. Student characteristics include percent SWD, ELL, Black, Hispanic, White, and Asian. In this sample, 272 schools are used to estimate Year1 estimates, 270 schools are used to estimate Year2 effects, and 258 schools are used to identify Year 3+ effects.

Table A2. Differential Effects of UFM over Time, Student-Level, 2013-2017

	(1) Bullying	(2) Fighting	(3) Respect	(4) Safe: Class	(5) Safe: Inside	(6) Safe: Outside
UFM						
Year 1	0.012	0.019	0.003	-0.004	0.005	0.019
	(0.012)	(0.018)	(0.014)	(0.010)	(0.013)	(0.012)
Year 2	-0.000	0.005	-0.001	-0.014	-0.010	0.012
	(0.015)	(0.019)	(0.020)	(0.011)	(0.015)	(0.014)
Year 3+	0.004	0.001	0.002	-0.009	-0.012	0.008
	(0.020)	(0.024)	(0.022)	(0.013)	(0.016)	(0.016)
Student Char	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
No Students	64,842	64,799	62,876	65,270	65,365	65,327
No Schools	848	847	843	849	845	847
Observations	217,986	217,863	205,259	221,631	222,083	221,725
R-squared	0.472	0.501	0.503	0.419	0.452	0.458

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes Ever UFM/POS students in 6th to 12th grade with at least two years of data, whose middle or high school poverty status is observed in a nonUFM year from 2010-2017. Student characteristics include indicators for SWD and ELL. Models control for cohort-specific linear time trends. 57,864 students contribute to the Year 1 estimate, 37,335 students contribute to Year 2, and 16,494 contribute to the third year and beyond. Over half the sample is exposed to UFM in middle schools in 2015, but these students lose UFM once they move to high school. Therefore, the number of students contributing to the 3rd year and beyond effects is vanishingly small. In addition, 57,000 students are used to estimate Year 1 effects instead of 62,000 because their first year of exposure is not observed.

Table A3: The Effect of UFM on Meal Participation and Student Perceptions, Excluding Ever-Retained Students, 2013-2017

	(1) SBP	(2) SLP	(3) Bullying	(4) Fighting	(5) Respect	(6) Safe: Class	(7) Safe: Inside	(8) Safe: Outside
UFM	-0.001	0.022	0.021*	0.029*	0.005	0.001	0.018	0.025**
	(0.016)	(0.016)	(0.011)	(0.017)	(0.014)	(0.010)	(0.013)	(0.012)
Student Char	Y	Y	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
No Students	96,457	96,457	94,099	93,947	89,909	94,773	94,947	94,857
No Schools	860	860	858	857	852	857	856	858
Observations	305,251	305,251	292,411	292,040	273,283	296,908	297,544	297,030
R-squared	0.646	0.740	0.491	0.519	0.526	0.440	0.473	0.484

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM/POS students in 6^{th} to 12^{th} grade with at least two years of data, whose middle or high school poverty status is observed in a nonUFM year from 2010-2017 and who are never retained between 2009 and 2017. Models control for cohort-specific linear time trends. This removes approximately 6 percent of students from the sample. Student characteristics include indicators for SWD and ELL.

Table A4 – Panel A: The Effect of UFM on Meal Participation and Student Perceptions, 2013-2017 – Movement from "Worst" to Anything Better

	(1) Bullying	(2) Fighting	(3) Respect	(4) Safe: Class	(5) Safe: Inside	(6) Safe: Outside
UFM	0.008	0.006	0.002	0.004	0.006	0.008
	(0.008)	(0.011)	(0.009)	(0.005)	(0.008)	(0.007)
Student Char	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
No Students	100,109	99,947	95,647	100,847	101,034	100,927
No Schools	863	863	861	863	862	864
Observations	310,875	310,507	290,502	315,743	316,418	315,863
R-squared	0.489	0.517	0.525	0.440	0.472	0.483

Panel B: Movement from Anything Worse to "Best"

	(1) Bullying	(2) Fighting	(3) Respect	(4) Safe: Class	(5) Safe: Inside	(6) Safe: Outside
UFM	0.014	0.034**	0.013	0.049***	0.038***	0.026*
	(0.011)	(0.015)	(0.011)	(0.014)	(0.013)	(0.015)
Student Char	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
No Students	100,109	99,947	95,647	100,847	101,034	100,927
No Schools	863	863	861	863	862	864
Observations	310,875	310,507	290,502	315,743	316,418	315,863
R-squared	0.489	0.517	0.525	0.440	0.472	0.483

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM/POS students in 6^{th} to 12^{th} grade with at least two years of data, whose middle or high school poverty status is observed in a nonUFM year from 2010-2017. Student characteristics include indicators for SWD and ELL. Models control for cohort-specific linear time trends. Responses are binary indicators in which the best possible answer equals 1 and 0 otherwise.

Table A5. Characteristics of Students Answering vs. "I Don't Know," 2015-2017

	Respect	
	Answered	"I Don't Know"
Female	51.8	48.1
White	15.0	12.0
Black	21.7	21.6
Hispanic	40.8	45.3
Asian	21.4	20.0
Other Language	50.9	54.5
ELL	8.9	11.7
SWD	11.9	14.2
Mean No. Obs.	4.1	4.0
Mean Grade	8.9	8.8
SBP	9.2	9.4
SLP	34.7	36.2
No. Students	209,921	19,819

Table A6. The Effects of UFM Coding "I Don't Know" as Neutral (.5), 2013-2017

	(1) Respect
UFM	0.003
	(0.013)
Student Char	Y
Student FE	Y
Grade FE	Y
School FE	Y
Year FE	Y
No Students	101,067
No Schools	866
Observations	315,208
R-squared	0.508

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM/POS students in 6th to 12th grade with at least two years of data, whose middle or high school poverty status is observed in a nonUFM year from 2010-2017. Always UFM (2010-2017) students are excluded from this analysis. Models control for cohort-specific linear time trends. Student characteristics include indicators for SWD and ELL.

Table A7. Chronbach's Alpha Analysis, 2010-2017

Item	Observation	Sign	Item-test correlation	Item-rest correlation	Average inter item covariance	Alpha
Bullying	441,085	+	0.70	0.54	0.31	0.76
Fighting	441,196	+	0.69	0.53	0.31	0.77
Respect	445,450	+	0.66	0.44	0.31	0.79
Safe: Class	446,592	+	0.70	0.57	0.32	0.76
Safe: Inside	447,197	+	0.77	0.65	0.29	0.74
Safe: Outside	446,468	+	0.74	0.59	0.30	0.75
Test Scale					0.31	0.79

Table A8. Factor Analysis, 2010-2017

Factor	Variance	Difference	Proportion	Cumulative
Factor1	2.30	0.47	0.38	0.38
Factor2	1.83	.	0.31	0.69

Variable	Factor1	Factor2	Uniqueness
Bullying	0.19	0.85	0.24
Fighting	0.17	0.85	0.24
Respect	0.35	0.52	0.61
Safe: Class	0.84	0.14	0.27
Safe: Inside	0.87	0.21	0.20
Safe: Outside	0.81	0.21	0.30

	Factor1	Factor2
Factor1	0.79	0.62
Factor2	-0.62	0.79

Table A9. The Effects of UFM on Perceptions Using Indexed Student Perceptions, 2013-2017

	(1) Safety Factor 1	(2) Peer Factor 2	(3) Safety Factor 1	(4) Peer Factor 2	(5) Safety Factor 1	(6) Peer Factor 2
UFM	0.049*	0.057*				
	(0.027)	(0.033)				
UFM*Always Poor			0.051*	0.052		
			(0.027)	(0.034)		
UFM*Sometimes Poor			0.057*	0.067*		
			(0.030)	(0.036)		
UFM*Never Poor			0.041	0.063*		
			(0.028)	(0.034)		
UFM*Participant					0.068**	0.103**
					(0.033)	(0.043)
UFM*NonParticipant					0.070**	0.120***
					(0.036)	(0.046)
Student Char	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
No Students	100,062	92,806	100,062	92,806	70,490	63,778
No Schools	861	857	861	857	785	774
Observations	311,319	277,846	311,319	277,846	191,827	168,751
R-squared	0.521	0.593	0.521	0.593	0.564	0.634

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM/POS students in 6^{th} to 12^{th} grade with at least two years of data, whose middle or high school poverty status is observed in a nonUFM year from 2010-2017 and who answered all three questions required for each factor. Safety Factor 1 is an index of binary safety responses and Peer Factor 2 is an index of binary bullying, fighting, and respect responses. Student characteristics include indicators for SWD and ELL. All models control for cohort-specific linear time trends. Models in columns 3 and 4: ~62,700 AlwaysPoor, ~10,600 Sometimes Poor, and ~14,000 NeverPoor. Models in columns 5 and 6 use last year's participation status and therefore loses one year from the sample ~51,800 Participants and ~13,900 Non-Participants.

Table A10: The Effects of UFM on Meal Participation and Student Perceptions by School Level, 2013-2017

	(1) SBP	(2) SLP	(3) Bullying	(4) Fighting	(5) Respect	(6) Safe: Class	(7) Safe: Inside	(8) Safe: Outside
UFM								
Middle	-0.003	0.016	0.026**	0.033*	-0.002	0.003	0.019	0.023**
	(0.016)	(0.016)	(0.011)	(0.017)	(0.014)	(0.010)	(0.012)	(0.012)
High	0.015	0.149***	-0.017	0.050	0.005	0.037	0.038*	0.013
	(0.018)	(0.053)	(0.028)	(0.061)	(0.066)	(0.023)	(0.021)	(0.024)
Student Char	Y	Y	Y	Y	Y	Y	Y	Y
Student FE	Y	Y	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
No Students	102,895	102,895	100,109	99,947	95,647	100,847	101,034	100,927
No Schools	867	867	863	863	861	863	862	864
Observations	325,334	325,334	310,875	310,507	290,502	315,743	316,418	315,863
R-squared	0.646	0.739	0.489	0.517	0.525	0.440	0.472	0.483

Notes: Robust standard errors in parentheses clustered by school (*p < .10; **p<.05; ***p<.01). Sample includes observations of Ever UFM/POS students in 6th to 12th grade with at least two years of data, whose middle or high school poverty status is observed in a nonUFM year from 2010-2017. Models in columns 3-8 control for cohort-specific linear time trends. Student characteristics include indicators for SWD and ELL. Middle refers to students in grades 6-8, and high refers to students in grades 9-12.

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Chapter 2: Educating the Doubly Disadvantaged

I. Introduction

Disparities in academic performance have persisted in the American public education system despite decades of reform. In addition to well-known racial disparities, policymakers and researchers pay particular attention to disadvantage-based achievement gaps. In fact, there is an abundance of evidence documenting the achievement gaps between disadvantaged students – those classified as economically disadvantaged (ECD), students with disabilities (SWD), or English language learners (ELL) – and their non-disadvantaged peers. However, public education circles frequently treat these disadvantages separately without explicitly investigating the intersections of disadvantage (e.g., the student that is both ECD *and* SWD). There is virtually no research examining the characteristics of students at the intersection of these disadvantages – the doubly disadvantaged – and their academic performance.

Overlooking the doubly disadvantaged may have meaningful implications for how we understand disadvantage-based achievement gaps. It is possible that some disadvantages, when combined with others, may artificially exacerbate general achievement gaps. For example, ECD gaps may appear even larger if ECD students are also more likely to be SWD *and* if those students perform worse on average. Moreover, better understanding the heterogeneity within these disadvantage-based achievement gaps may aid in the implementation of federally mandated state accountability systems, which hold schools accountable for test score disparities among the disadvantaged. For instance, if students that are both ECD *and* SWD perform worse on average, schools may be doubly penalized for both ECD and SWD subgroups for the same set of students. If this is true, it is possible that by creating more detailed accountability subgroups, schools could better support the students most in need of improvement – those that are both ECD *and* SWD.

Not accurately understanding the landscape of student disadvantage may have additional consequences for how we design policies to support disadvantaged students, including school funding. School funding formulas frequently acknowledge that disadvantaged students (i.e., those that are ECD, SWD, or ELL) require additional support and, therefore, funding to perform at the same level as their non-disadvantaged peers. Many of these formulas provide additional funding based on districts' shares of disadvantaged students. For example, a formula may allocate to districts an additional 20 percent of per pupil funding for students that are ECD, 40 percent for students that are SWD, and 10 percent for students that are ELL. However, whether these formulas distribute sufficient funds to effectively support student need depends on a multitude of factors, one of which includes whether the effects of disadvantage on academic performance are the same across disadvantages. For instance, while a student that is both ECD and ELL is accounted for in both the ECD and ELL allotments, it is unclear whether the combined monetary aid is sufficient to support the effect of being both ECD and ELL on achievement.

Learning more about the effects of being singly versus doubly disadvantaged can help inform whether current formulas provide aid that proportionately reflects need. This analysis has four possible outcomes. First, it is possible that additional disadvantages have no additional negative effects on student performance than what is already observed with one disadvantage – meaning the effect of being doubly disadvantaged is no larger than the effect of being singly disadvantaged. Second, it could be that the effect of being doubly disadvantaged is worse than the effect of being singly disadvantaged, but still less than what the sum of the two single disadvantages might suggest. Third, it is possible that the effect of being doubly disadvantaged is equal to the sum of the effects of the single disadvantages. Fourth, it could be that the effect of being doubly disadvantaged is larger than what the sum of single disadvantage effects might suggest.

If the reality of disadvantage effects is reflected in scenarios one, two, or three, then there are likely few if any implications for funding formulas that treat ECD, SWD, and ELL students as separate categories. However, we know ECD students begin school performing worse academically than their non-ECD peers (Reardon, 2013), and SWD students perform worse as they get older (Schulte & Stevens, 2015). Therefore, it is possible that the effects of being both ECD and SWD compound to more negatively affect student performance compared to what the sum of single disadvantage effects might suggest. If, on the other hand, the effect of being doubly disadvantaged is greater than the sum of individual disadvantages, policymakers may consider adjusting aid so as to provide the minimum aid to proportionally mirror the reality of disadvantage need.

This paper uses an 8-year panel of North Carolina (NC) 3rd-8th grade students to provide the first statistical portrait of doubly disadvantaged students and answers three primary research questions. First, how common are the doubly disadvantaged and how do their characteristics differ within disadvantage and across race? Second, how do the achievement gaps of the doubly disadvantaged compare to the gaps of their singly and non-disadvantaged peers, and does overlooking the intersection of disadvantage mask heterogeneity within general disadvantage-based achievement gaps*?* These findings could have policy implications for the implementation of federally mandated state accountability systems which hold schools accountable for disadvantaged based achievement gaps. Lastly, do value-added models, which account for past performance, suggest the effects of disadvantage on performance are largest at the intersection of disadvantage? Better understanding the differential effects of being singly versus doubly disadvantaged could inform policy discussions surrounding the use of formulas to fund disadvantaged student needs and whether doubly disadvantaged students are proportionally supported to reflect need.

I answer these questions by first descriptively examining the demographic and disadvantage characteristics of NC students. Utilizing the detailed nature of the student level data, I then identify the prevalence of the doubly and triply disadvantaged and describe student characteristics and academic achievement across and within these newly identified disadvantage groups. Next, I examine the achievement gaps within a regression framework, controlling for student characteristics and grade, year, and school fixed effects. I begin with a naïve model, using the traditional disadvantage definitions (ECD, SWD, and ELL) and calculate the naïve achievement gaps for doubly and triply disadvantaged students using these three estimates.

I then turn to a second model, where I estimate the specific achievement gaps for single, doubly, and triply disadvantaged students by including interactions between each of the disadvantages and compare these to the naïve estimates in an effort to unmask any previously unknown heterogeneity in gaps. Within the second model, I then compare the estimated achievement gaps of each disadvantage group to learn how the doubly disadvantaged perform relative to the single disadvantaged. Lastly, in value-added models, I include students' prior academic performance to estimate the suggested effects of each disadvantage rather than achievement gaps, allowing me to compare the effects of disadvantages on academic performance among the single, doubly, and triply disadvantaged.

To preview my results, I find 13 percent of students are doubly or triply disadvantaged, the majority of whom are both ECD *and* SWD. Moreover, I find a higher share of black and Hispanic students – compared to white and Asian students – are disproportionately likely to be doubly or triply disadvantaged. Thus, if it is the case that students with multiple disadvantages perform even worse, then it is disproportionately concentrated among these minorities. In baseline regression results, I find that achievement gaps are largest among students who are both ECD *and* SWD, thereby negatively contributing to larger ECD achievement gaps. Value-added models, by accounting for baseline academic performance, suggest there are virtually no additional deleterious effects of being ECD in the absence of additional disadvantages. However, the negative effect of being ECD *and* SWD on test scores is largest in magnitude, though not statistically different from the effect of being SWD with no additional disadvantages. That is, the effect of being ECD *and* SWD is similar to the effect of being SWD with no additional disadvantages. Initially, it appears the effect of being both ECD *and* ELL is larger and more positive than the effect of being ELL with no additional disadvantages, however, point estimates are not statistically different from each other.

Baseline regression results suggest that school officials and policymakers should consider explicitly accounting for the doubly disadvantaged to provide more accurate representations of disadvantage-based achievement gaps. Moreover, larger achievement gaps among the doubly disadvantaged could have implications for federally mandated state accountability systems. Recognizing the prevalence of doubly disadvantaged students that are SWD in addition to ECD or ELL may have considerable consequences for schools trying to meet subgroup accountability standards. For example, by including ECD *and* SWD students in the ECD subgroup, these schools are more likely to fail to meet subgroup accountability standards for both ECD and SWD subgroups.

However, value-added results largely suggest the effects of having multiple disadvantages are no worse than the effects of having only one disadvantage – particularly the effects of being SWD or ELL. While this analysis does not speak to the adequacy of monetary support, these findings imply the needs of the doubly disadvantaged may not be particularly different from what single disadvantages suggest, and the additional aid currently provided via funding formulas based on general disadvantage categories may proportionally reflect need. These results indicate few, if any, consequences for funding formulas that already provide additional funding for disadvantage categories ECD, SWD, and ELL.

I begin by providing background information on the three disadvantages (i.e., ECD, SWD, and ELL) in Section II, followed by a review of the existing literature concerning disadvantage-based achievement gaps in Section III. Section IV describes my analytical framework, and Section V details the data and general descriptive statistics. I describe academic performance results using baseline regression and value-added models in Section VI. I further probe results in Section VII by using broader definitions of disadvantage before concluding and discussing policy implications in Sections VIII and IX.

II. Defining Disadvantage

While the definition of "disadvantage" is debated within the education literature, in this paper, disadvantage refers to hindrances that arise from social or economic status that prevent students from gaining the proper educational benefits from school. While there are a number of hindrances that may impede students' ability to succeed, this paper examines three widely identified disadvantages: ECD, SWD, and ELL. Public education uses ECD certifications (i.e., whether students are certified eligible to receive free or reduced-price meals) to identify students from low-income households. Students with household incomes at or below 130 percent of the federal poverty line are eligible for free school meals, whereas students with household incomes at or below 185 percent of the federal poverty line are eligible for reduced-price meals. In the

US, the share of ECD students has been on the rise, climbing almost 15 percentage points in the last two decades, from 38 percent in 2000 to 52 percent in 2018 (USDA-NCES, 2018).

The Individuals with Disabilities Education Act (IDEA) of 1990 defines SWD as a child "with mental retardation, hearing impairments (including deafness), speech or language impairments, visual impairments (including blindness), serious emotional disturbance, orthopedic impairments, autism, traumatic brain injury, other health impairments, or specific learning disabilities; and who, by reason thereof, needs special education and related services." If a student is suspected of having a disability, he or she is evaluated by a teacher and specialist and provided with the appropriate daily or testing accommodations. The share SWD students has remained at roughly 13.2 percent of the US student population for the last 20 years (USDA-NCES, 2018).

ELL students come from environments in which a language other than English has had a significant impact on the students' level of English language proficiency (ESSA, 2016). Students take a state-approved English language proficiency assessment to classify and monitor English proficiency progress. Once reaching proficiency, students are no longer considered ELL, and ELL designations are removed. The percentage of ELL students in the US has been slowly but steadily rising, reaching 9.6 percent of students in 2018 – up from 8.1 percent in 2000 (USDA-NCES, 2018).

III. Prior Literature

Despite additional resources available to disadvantaged students, scholars find general disadvantage-based achievement gaps persist. These disparities in academic performance have widespread consequences and contribute to pervasive gaps in educational attainment which, in turn, lead to more limited employment and economic mobility (Chetty et. al., 2018; Rothstein & Wozney, 2013; Reardon & Galindo, 2009). These life-long consequences may be exacerbated among doubly disadvantaged students if they perform even worse than what is suggested by general disadvantage-based achievement gaps. These consequences, therefore, warrant further examination of those at the intersection of disadvantage.

ECD Students

Low-income students arrive at school in early childhood performing almost 1 standard deviation below their higher-income peers, often due to a lack of early childhood educational resources, and scholars find this gap to remain more or less constant throughout low-income children's academic careers (Hanushek, Machin, & Woessmann, 2016; Reardon, 2013). However, over the past three decades, the performance disparity of ECD students at the beginning of their academic careers has grown by 40 percent (Rampey, Dion, & Donahue, 2009), possibly due to a rise in income inequality, leading to an increase in the share of students identified as low-income and an overall decline in social upward mobility (Reardon, 2013).

Moreover, the ECD test score gap likely contributes to disparities in college completion rates between low- and high-income students. College completion rates for low-income students have remained stagnant while the rates for high-income students have grown sharply in recent decades (Bailey & Dynarski, 2011). Furthermore, high income students make up larger shares of enrollment at high quality colleges and universities over time, even when compared to low income students with similar test scores, further contributing to long term economic disparities (Reardon, Baker, & Klasik, 2012; Bailey & Dynarski, 2011; Belley & Lochner, 2007; Karen, 2002).

SWD Students

Historically, SWD students have been excluded from standardized test taking (Koretz & Hamilton, 2006). However, in recent decades, states have incorporated SWD students into large-scale testing programs through accommodations such as extended time, testing in a separate room, and marking answers in the testing booklet instead of an answer sheet. Schwartz, Hopkins, and Stiefel (2019) find academic outcomes improve for SWD students with learning disabilities following classification into special education. However, SWD students begin their academic careers with lower test scores than the general population and therefore, even with accommodations, have difficulty reaching grade-level proficiency standards in the specified time frame (Eckes & Swando, 2009). For many schools in the past, meeting the SWD adequate yearly progress necessary for accountability purposes is the most challenging goal and often leads schools to fail their progress expectations (Schulte & Stevens, 2015).

SWD students consistently underperform on standardized assessments and improve at slower rates relative to their general education peers (Schulte & Stevens, 2015). Over time, scholars find the achievement gap between SWD and non-SWD students either remains stable or even grows larger over time, with no evidence of the gap narrowing (Judge & Watson, 2011; Wei et al., 2013). Schulte and Stevens (2015) use cross-sectional data to find substantial SWD-gaps across grades, from 0.7 standard deviations among 3rd graders to 1.0 standard deviation among 7th graders. These findings indicate that not only do SWD students have difficulty reaching grade-level proficiency, it becomes more difficult for these students as they age.

Prior research also finds female students are less likely to be identified as SWD. However, rather than male students being over identified as SWD, scholars find female students who would benefit from the services that come with SWD status are underrepresented. Scholars attribute this to teacher referral processes which are based on behavioral differences between male and female students (Arms, Bickett, & Graff, 2008). Wehmeyer and Shwartz (2001) find that SWD students are often identified not by their learning needs but by their behaviors. Female students are less likely to be disruptive, and therefore, less likely to be identified as SWD. For example, females with autism spectrum disorder tend to stay in close proximity to peers, masking their social challenges, whereas males tend to play alone, alerting teachers to instances of social isolation (Dean, Harwood, & Kasari, 2017).

ELL Students

A recent rise in the Hispanic population in the US has contributed to an increasing number of students are being classified as ELL (Hemphill & Vanneman, 2011). ELL students consistently underperform compared to their non-ELL peers, especially on reading assessments (Fry, 2008). An examination of the 2013 National Assessment of Educational Progress (NAEP) scores shows the achievement gap between ELLs and their English-speaking counterparts has remained stagnant for the past 10 years, demonstrating proficiency levels between 23 to 30 percentage points below their English-speaking peers (NCES, 2014a). While ELL students underperform on both reading and math assessments, they, as expected, perform worse on reading. According to the 2014 NAEP, 41 percent of ELL 4th graders scored below basic proficiency in math compared to 69 percent that performed below basic in reading (NCES, 2014b).

Given the nature of the ELL classification, it can be difficult to ascertain academic progress over long periods of time. Once a student has become “proficient,” she exits the ELL program and is accounted for in the general education population. Therefore, we may not expect to see large academic improvements for ELL students over time as research frequently identifies achievement gaps using students that are contemporaneously ELL and excludes students that have reached proficiency and have therefore exited the program.

Doubly Disadvantaged

There is relatively little literature concerning students with overlapping disadvantages. A small but emerging literature examines the lack of ELL students with SWD designations. ELL students are less likely to be identified as learning disabled or as having speech/language impairments, likely because the more visible disadvantage is the lack of English proficiency (Morgan et. al., 2015). However, research finds students that are classified as both SWD and ELL are overrepresented in secondary grade levels and attribute this phenomenon to the fact that students that are both SWD *and* ELL are much less likely to reach English proficiency and exit the ELL program compared to their non-SWD/ELL counterparts (Umansky, Thompson, & Diaz, 2017).

There is a plethora of research examining disadvantage-based achievement gaps. However, this research largely overlooks those at the intersection of disadvantage – both their characteristics and their academic performance. This paper aims to fill the gap in the literature by being the first to thoroughly investigate the prevalence of the doubly disadvantaged, their characteristics and academic achievement, and whether the intersections of these disadvantages reveal heterogeneity in achievement that studies of general achievement gaps have overlooked.

IV. Analytical Strategies

I utilize several empirical strategies to explore the characteristics and academic performance of doubly disadvantaged students. I first descriptively discuss the prevalence of disadvantaged students in NC and investigate the incidence and characteristics of students at the intersection of disadvantage, including the gender, racial/ethnic, and academic disparities that exist within specific disadvantage groups. I then employ the following regression framework to more closely examine achievement gaps:

Baseline Regression Models

$$Y_{igst} = \beta_0 + \beta_1 ECD_{it} + \beta_2 SWD_{it} + \beta_3 ELL_{it} + \beta_4 Char_{it} + \lambda_g + \delta_s + \gamma_t + e_{igst} \quad (1)$$

where Y_{igst} is a vector of test score outcomes for student *i* in grade *g*, school *s*, in year *t*, including test scores on standardized assessments normalized by grade, subject, and year (*zMath* and *zRead*). *ECD*, *SWD*, and *ELL* are indicator variables equal to 1 if student *i* has that disadvantage in year t. $Char_{it}$ is a vector of binary student characteristics including gender (*Female*) and race/ethnicity (*Black, Hispanic, Asian/Other*). λ, δ, and γ, are grade, school, and year fixed effects, and standard errors are clustered at the school level. Coefficients on each disadvantage (i.e., $\beta_1, \beta_2, \beta_3$) capture the regression-adjusted mean, within-school differences in performance between students with each disadvantage and other students. For example, β_1 captures the regression-adjusted mean, within-school disparity in performance between ECD students and others, and $\beta_1 + \beta_2$ reflects the suggested or naïve achievement gap for students that are both ECD *and* SWD.

However, not specifically including indicators for doubly or triply disadvantaged students may mask heterogeneity within disadvantage-based achievement gaps. Therefore, to parse disadvantage-specific achievement gaps, I estimate a similar model (Equation 2) that includes interactions for each disadvantage. β_1 captures the regression-adjusted mean, within-school achievement gap between ECD students with no additional disadvantages and others, and the sum of β_1, β_2, and β_4 reflects the achievement gap between students that are both ECD *and* SWD and others.

$$Y_{igst} = \beta_0 + \beta_1 ECD_{it} + \beta_2 SWD_{it} + \beta_3 ELL_{it} + \beta_4 ECD_{it} * SWD_{it} + \beta_5 ECD_{it} * ELL_{it} + \beta_6 SWD_{it} * ELL_{it} + \beta_7 ECD_{it} * SWD_{it} * ELL_{it} + \beta_8 Char_{it} + \lambda_g + \delta_s + \gamma_t + e_{igst} \qquad (2)$$

I begin by estimating the two models above using parsimonious specifications which only control for grade and year fixed effects. I then add student characteristics to examine how sensitive disparities are to the inclusion of student gender and race before I estimate my preferred models which include school fixed effects. Then, in an exercise to gain further insight into the heterogeneity within each achievement gap, I incrementally add disadvantage interactions to better understand the contribution of each doubly and triply disadvantaged group to original disadvantage-based achievement gap estimates.

Value-Added Models

To shed light on potential causal effects of disadvantage – as opposed to observed, within-school achievement gaps – I next estimate value-added models in which I control for students' prior academic achievement by including lagged test scores. This method enables me to remove baseline performance and examine the effects of each single, double, and triple disadvantage over time. The coefficients in these models provide unbiased estimates of causal effects if students' likelihood of being identified as disadvantaged does not differ within school and there are no heterogeneous effects depending on student characteristics.

I begin by estimating the naïve model shown in Equation 3, which resembles Equation 1 but with the addition of students' lagged test scores. In this model, β_1 captures the regression-adjusted, within-school mean effect of ECD on performance, and $\beta_1 + \beta_2$ reflects the suggested or naïve effect on performance of being ECD *and* SWD. I then compare these naïve effects to the effects estimated in the interacted model shown in Equation 4. Here, β_1 captures the regression-adjusted, within-school mean effect on performance of being ECD with no additional disadvantages, and the sum of β_1, β_2, and β_4 reflects the regression-adjusted, within-school mean effect of being ECD *and* SWD.

$$Y_{igst} = \beta_0 + \beta_1 ECD_{it} + \beta_2 SWD_{it} + \beta_3 ELL_{it} + \beta_4 Char_{it} + \beta_5 Y_{igst-1} + \lambda_g + \delta_s + \gamma_t + e_{igst} \quad (3)$$

$$Y_{igst} = \beta_0 + \beta_1 ECD_{it} + \beta_2 SWD_{it} + \beta_3 ELL_{it} + \beta_4 ECD_{it} * SWD_{it} + \beta_5 ECD_{it} * ELL_{it} + \beta_6 SWD_{it} * ELL_{it} + \beta_7 ECD_{it} * SWD_{it} * ELL_{it} + \beta_8 Char_{it} + \beta_9 Y_{igst-1} + \lambda_g + \delta_s + \gamma_t + e_{igst} \quad (4)$$

V. Data, Sample, & Descriptives

This study uses a student-level panel from the North Carolina Education Research Data Center (NCERDC) that includes 3rd-12th grade NC public education students from 2009 to 2016. It provides longitudinal data on over 2 million students in 2,500 schools and 115 districts. NC school districts cover a variety of contexts. The districts range in size, serving between 500 and 160,000 students, with three-fourths of the districts residing in rural counties. The data include unique student identifiers, socioeconomic characteristics such as race/ethnicity, gender, ECD, SWD, and ELL status, and scale scores of math and reading standardized assessments for 3rd-8th grade students. I use two samples for my analyses. For the descriptive characteristics analysis, I use all 3rd-8th grade students with non-missing sociodemographic information. For the academic performance analysis, my sample includes all 3rd-8th grade students with non-missing test score information.[10]

Sociodemographic Characteristics

[10] While ELL students are often exempt from taking the reading test in first year, they have test scores in later years of the panel, and approximately 98 percent of SWD students take NC standardized assessments.

As shown in Table 8, over half of NC's 658,000 students in 2016 are ECD, 13.5 percent are SWD, and 6.2 percent are ELL.[11] If each of these students had no *additional* disadvantages, we would expect the percent of disadvantaged students to equal the sum of the disadvantage percentages, or 70.9 percent. However, as shown in Column 4, 57.1 percent of students have at least one disadvantage, demonstrating the existence of students with multiple disadvantages. Of these students with any disadvantage, the majority are ECD (89.6 percent), one in four are SWD, and one in ten are ELL. The cross tabulations shown in Columns 1 through 3 begin to reveal the doubly (or triply) disadvantaged nature of NC students. Almost one fourth of ECD students are additionally disadvantaged (17.3 percent SWD; 6.2 percent ELL). More than half of SWD students are also ECD (65.4 percent), and 97 percent of ELL students are also either ECD or SWD.

Table 9 provides a more detailed view of student disadvantage. Few students are triply disadvantaged (7,308 students or 1.1 percent), while almost 12 percent are doubly disadvantaged. Among the doubly disadvantaged, ECD&SWD students make up the largest group with over 50,000 students at 7.7 percent. Very few students, however, are SWD&ELL (0.2 percent). This is consistent with work by Morgan et. al. (2015), which finds ELL students are much less likely to be identified has having a learning disability and therefore less likely to be identified as SWD. Roughly 45 percent of students have only one of the three disadvantages, the largest group being students that are ECD at 38.8 percent.

While Table 9 provides novel information about student disadvantage using mutually exclusive disadvantage groups, it is possible to gain even more information by examining the prevalence of additional disadvantage within each general disadvantage. Table 10 Panel A shows

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[11] Characteristics in 2016 are not substantively different from characteristics in 2009-2015.

a statistical portrait of disadvantage among ECD students. Most ECD students are not additionally disadvantaged (75.7 percent), while one in five is doubly disadvantaged (15.1 percent +SWD; 7.0 percent +ELL). Panels B and C show the majority of SWD students and ELL students are not only additionally disadvantaged, they are also most likely to be ECD (57.2 percent of SWDs; 57.7 percent of ELLs). Nearly one in five ELL students is likely to be triply disadvantaged, a much higher probability compared to ECDs and SWDs.

Table 11 depicts gender and racial characteristics of all students. Almost half of students are white, 25 percent are black, 18 percent are Hispanic, and fewer than 10 percent are Asian/Other. However, the distribution of these characteristics across disadvantage do not reflect the general population. Table 12 displays cross tabulation information of student disadvantage and demographic characteristics where the first number in each cell refers to the number of students, the second number refers to the row percentage, and the third number refers to the column percentage. As shown in Column 2, only 34.3 percent of SWDs are female. ECD students come from all race/ethnicities, however, black and Hispanic students are disproportionately likely to be ECD compared to their white and Asian/other counterparts (71.6 percent and 73.7 percent, respectively). and though almost half of SWD students are white (46.0 percent), black students are the most likely to be SWD at 16.7 percent. Additionally, one-third (29.3 percent) of all Hispanic students are ELL. Additional detailed descriptive statistics within disadvantage group are available in Table B1 in the Appendix.

Table 13 describes the demographic characteristics of students with one, two, and three disadvantages. Column 1 includes students that are disadvantaged but have no additional disadvantages, and Columns 2 and 3 include students that are doubly and triply disadvantaged, respectively. Doubly and triply disadvantaged students are less likely to be female (38.5 percent and 35.3 percent, respectively). Black and Hispanic students are disproportionately likely to be doubly disadvantaged (13.7 percent and 22.5 percent, respectively), similar to the summary statistics presented in Table 12. Finally, one in every 20 Hispanic students is likely to be triply disadvantaged, the highest among all racial/ethnic groups.

To give a brief summary of the novel descriptive information gained in this section about student disadvantage and the associated demographic characteristics, 57 percent of all students are disadvantaged, a nontrivial 13 percent are doubly or triply disadvantaged, and there is significant variation by race. Most doubly disadvantaged students are ECD *and* SWD, and there is very little overlap between SWD and ELL, likely due to the decreased likelihood that ELL students are identified as SWD. While 75 percent of ECD students have no additional disadvantages, the majority of SWDs and ELLs are also ECD. Concerning demographic trends across disadvantages, I find black and Hispanic students are more likely to be additionally disadvantaged, largely because both are disproportionately more likely to be ECD, black students are more likely to be SWD, and Hispanic students are more likely to be ELL. This evidence suggests that if the interaction of disadvantage has compounding negative consequences, it occurs disproportionately among black and Hispanic students.

VI. Academic Performance Results

Descriptive Statistics

Table 14 shows the average academic performance of students by general disadvantage. Students without any disadvantages perform better by 0.2 standard deviations (SD). Overall, disadvantaged students underperform, on average. SWD students perform the lowest on both reading and math at 0.8-0.9 SD below average, and ELL students, unsurprisingly, perform worse on reading assessments compared to math.

However, it is possible that overlooking the intersection of disadvantage masks the heterogeneity within disparities. By comparing academic performance of the doubly and triply disadvantaged, I can begin to parse the naive achievement gaps by each disadvantage group. Table 15 Panel A shows the general ECD achievement gap to be 0.1 SD below average. However, ECD students with no additional disadvantages (No Add Disad) actually perform above average, while students that are both ECD *and* SWD perform practically on par with the triply disadvantaged. As shown in Panel B, SWD students perform similarly, regardless of additional disadvantage. However, students that are both SWD *and* ELL have the lowest performance of all disadvantage intersections, including the triply disadvantaged, with performance on reading and math 0.9-1.0 SD below average. While ELL students perform worse on reading assessments across all intersections of disadvantage, ELL students with no additional disadvantages perform relatively the best (Panel C).

In summary, both ECD and ELL students with no additional disadvantages perform better than what is suggested by the general ECD and ELL achievement gaps. However, all SWD students, regardless of additional disadvantages, underperform similarly at approximately 0.8-0.9 SD below average. Doubly disadvantaged students that are both SWD *and* ELL perform worst among all groups but are closely followed by those that are both ECD *and* SWD. Interestingly, both of these doubly disadvantaged groups perform marginally better than students that are triply disadvantaged.

Baseline Regression Achievement Gap Results

Table 16 displays the baseline regression results for zMath beginning with the parsimonious models (Columns 1 and 4) and incrementally adding student characteristics (Columns 2 and 5) and school fixed effects in my preferred model (Columns 3 and 6). Disparities in performance are surprisingly insensitive to the inclusion of student characteristics and school fixed effects. Controlling for student characteristics explains 0.04-0.05 SD of the ECD achievement gaps but widens the ELL gap by almost the same. Changes in point estimates between Columns 2 and 3 (5 and 6) reflect that achievement gaps in Column 2 (5) are partially driven by differences across schools. The differences between coefficients on ECD, SWD, and ELL between models in Columns 3 and 6 demonstrate that estimates in models that do not specifically account for the doubly and triply disadvantaged mask the heterogeneity and nuance within each general disadvantage disparity. I find similar results for zRead, with the exception of ELL students performing mildly worse, as expected (see Appendix Table B2).

For ease of coefficient interpretation, Table 17 provides both the naïve performance disparities as a result of excluding the disadvantage interactions (Table 16, Columns 1-3), as well as the disparities estimated in preferred models that include such interactions for both zMath (Table 16, Columns 4-6) and zRead. Overall, I find accounting for the doubly and triply disadvantaged suggests meaningful heterogeneity within general disadvantage achievement gaps. Most disadvantage groups perform the same or better than what naïve estimates would suggest on both math and reading assessments, with the exception of ELL students with no additional disadvantages and students that are both ECD *and* SWD. For example, the naive zMath estimates suggest students that are both ECD *and* SWD perform 1.38 SD (1.31 SD for zRead) below average. However, preferred estimates show that students that are both ECD *and* SWD perform marginally worse: 1.45 SD (1.35 SD for zRead) below average. Similarly, preferred estimates demonstrate ELL students without additional disadvantages actually perform 0.07-0.08 SD worse than naïve estimates would suggest.

To shed light on the direction and general contribution of each doubly and triply disadvantaged group to general disadvantage-based achievement gaps, I incrementally add disadvantage interactions to the baseline regression model.[12] As shown in Table 18 Column 1, the general math achievement gaps appear to be -0.08 SD for ECD and -1.30 SD for SWD. However, Column 2 demonstrates that it is the poor performance of those that are both ECD *and* SWD that cause the general gaps to appear larger. By specifically accounting for the performance of students that are both ECD *and* SWD in Column 2, general achievement gaps shrink by 0.02 SD for ECD and 0.14 SD for SWD. Conversely, students that are both ECD *and* ELL cause general ELL math disparities to appear 0.07 SD (zRead: 0.06) smaller, and students that are both SWD *and* ELL cause both SWD and ELL general disparities to appear 0.01 SD smaller. The triply disadvantaged cause general SWD disparities and ELL disparities to appear only marginally worse for math with no difference among reading.

To summarize, performance disparities are largely unaffected by student demographic characteristics, and explicitly accounting for the doubly and triply disadvantaged unmasks heterogeneity previously unobserved within general disadvantage disparities. Naïve estimates suggest most disadvantage groups perform worse than preferred estimates indicate, with the exception of ELL students with no additional disadvantages and students that are both ECD *and* SWD, who perform 0.04-0.08 worse than suggested. By incrementally adding disadvantage interactions, I find students that are both ECD *and* SWD negatively contribute to general ECD disparities, causing general ECD disparities to appear larger. Of note, students that are both ECD

[12] The order in which interactions are added does not substantively change the trends in contribution and direction.

and ELL positively contribute to general ELL achievement gaps and cause general ELL disparities to appear smaller.

Value-Added Model Results

Value-added results are shown in Table 19, and coefficient interpretations are available in Table 20.[13] As shown in Table 20, naïve estimates of the effects of disadvantage (Columns 1 and 3) are similar to estimates in models that include disadvantage interactions (Columns 2 and 4) for almost half of disadvantaged groups. Estimates between the two models are the same for students that are ECD with no additional disadvantages, students that are SWD with no additional disadvantages, and students that are both ECD *and* ELL.[14] However, naïve estimates underestimate the effects of disadvantage being ELL with no additional disadvantages, both ECD *and* SWD, and triply disadvantaged. Interestingly, students that are both SWD *and* ELL perform better on math than naïve estimates would suggest, and slightly worse on reading.

In summary, though estimates between the two models are similar for some groups, models that account for the doubly and triply disadvantaged provide more accurate representations of the effects of student disadvantage. Comparing effects within Columns 2 and 4 in Table 20, I find accounting for prior academic performance suggests that there are virtually no additional deleterious effects of being ECD beyond baseline performance. However, being ECD *and* SWD has the largest negative effect on performance (zMath: -0.14; zRead: -0.08) – even larger than the effect of being triply disadvantaged, though it is not statistically different from the effect of being SWD with no additional disadvantages. Interestingly, there is a positive effect of

[13] Sample size is slightly smaller in value-added models. Baseline regression results using this sample are robust (see Appendix Table A3).
[14] The naïve ECD&ELL math estimate is the sum of ECD: -0.01 and ELL: 0.04.

being ELL with no additional disadvantages (zMath: 0.01; zRead: 0.04), as well being both ECD *and* ELL (zMath: 0.03; zRead: 0.05), thought point estimates are not statistically different from each other. These results are consistent with the notion that ELL students are improving English proficiency over time as part of the ELL program.

VII. Dynamic Effects of Disadvantage

Thus far, this paper has analyzed the effects of cotemporaneous disadvantage. However, identification of disadvantage is not necessarily static. For example, once ELL students demonstrate English proficiency, they are no longer identified as ELL. Similarly, some SWD students exit “SWD-status” during their educational careers. Moreover, students may turn in a free or reduced-price lunch application in one year and not the next.

In my sample between 2009 and 2016, 54 percent of students ever identified as ELL and 21 percent ever identified as SWD exit their respective programs.[15] Furthermore, 21 percent of students ever identified as ECD are not identified as ECD at some point later in their academic career. While student household incomes may well rise above the free/reduced price lunch thresholds during this timeframe, the rising frequency with which schools across NC adopt Universal Free Meals (UFM) via the Community Eligibility Provision (CEP) may help explain this loss of ECD status. CEP requires schools to transition to using direct certification to identify student poverty status instead of the traditional free/reduced-price meal forms.[16] However, there are many students who are income eligible for reduced price meals but are not enrolled in the direct certification means-tested programs and, therefore, are not identified as ECD. Prior to

[15] The majority of these students that exit SWD status previously had a speech-language impairment (47 percent), a specific learning disability (32 percent), or some other health impairment (11 percent), with the remaining 10 percent of students having some other exceptionality.
[16] Direct certification matches students from school rosters to statewide enrollments in SNAP/TANF/Medicaid. Students that match across these rosters are identified as ECD.

CEP's use of direct certification, these students would be incentivized to turn in meal forms. Yet once a school adopts UFM where all students receive meals for free regardless of household income, incentives to return forms all but disappear.

Roughly 10 percent of ever ECD students lose ECD status each year from 2009-2016. However, 27 percent of students lose status in 2015 – the year schools in NC became eligible to adopt UFM via CEP. Because CEP requires schools to use direct certification, it is likely that students previously identified as eligible for reduce-price meals were not enrolled in SNAP/TANF/Medicaid and therefore were no longer identified as ECD.[17]

It is possible that, by not accounting for a student's exit from their ELL and/or SWD classification (i.e., those that have improved to the point of exiting), my prior analyses may overstate ELL and SWD performance disparities. Similarly, by identifying ECD students using a contemporaneous measure of poverty, my previous analyses may overestimate ECD achievement gaps, as it is measuring the "poorest of the poor." I answer this question by redefining student disadvantage as "ever" being identified as having each disadvantage and examine the baseline regression achievement gaps, as well as value-added effects of disadvantage.

Using contemporaneous measures of disadvantage does not include students who have exited their disadvantage programs, nor does it catch the broader, more consistent definition of ECD. I find identifying gaps between students that have *ever* had these particular disadvantages

[17] There are serious implications of counting the poorest students as economically disadvantaged, while the less poor are accounted for in the general education population. First, there are fewer ECD students accounted for, meaning less funding for schools with students that would be identified as ECD had the school used lunch forms instead of direct certification. Second, schools are required to report test scores separately for the ECD subgroups. Students from low socioeconomic backgrounds are more likely to perform worse on standardized testing compared to their more advantaged, but still ECD peers. Moving students that would have qualified for reduced price meals from the ECD category to the general education category will make it appear as though both the ECD students and the general population students are doing worse.

reveals contemporaneous measures of disadvantage cause achievement gaps to appear larger for most disadvantage groups (results available in Table B4 in the Appendix). Moreover, value-added models using broader definitions of disadvantage reveal the effects of each disadvantage are, overall, smaller in magnitude than effects in models using contemporaneous measures of disadvantage would suggest.

VIII. Conclusion

Prior literature largely focuses on the disparities in academic performance between ECD, SWD, and ELL students and their non-disadvantaged counterparts. However, there is little information concerning the characteristics and academic achievement of students at the intersection of these disadvantages. More accurately understanding the doubly disadvantaged may help explain nuances within general disadvantage-based achievement gaps. Furthermore, detailed information about these students may have policy implications for subgroup accountability systems and student support via additional funding.

This paper uses 8 years of student-level, NC data to explore a number of research questions. First, how common are the doubly disadvantaged and how do their characteristics differ within disadvantage and across race? Second, how do the doubly disadvantaged perform, and does overlooking the intersection of disadvantage mask nuance and heterogeneity within general disadvantage-based achievement gaps? Lastly, do value-added models suggest the effects of disadvantage on performance are largest at the intersection of disadvantage?

I find over half of NC students are disadvantaged – 13 percent of whom are doubly or triply disadvantaged. Among students with multiple disadvantages, students are most likely to be both ECD *and* SWD and least likely to be both SWD *and* ELL. Moreover, the majority of SWDs and ELLs are doubly or triply disadvantaged. Black and Hispanic students are disproportionately doubly disadvantaged, meaning that if having multiple disadvantages has compounding negative effects, it affects minority students most.

Descriptively, ECDs and ELLs with no additional disadvantages perform better than what is suggested by general ECD and ELL achievement gaps, whereas SWDs, regardless of additional disadvantages, consistently perform almost 1 SD below average. Moreover, students that are both SWD *and* ELL appear to perform worst of all – even worse than the triply disadvantaged. However, once controlling for student characteristics and school fixed effects, I find explicitly accounting for students at the intersection of disadvantage reveals heterogeneity generally masked within general disadvantage disparities. Students that are ECD *and* SWD demonstrate the largest achievement gap among all disadvantage groups and are responsible for the larger achievement disparities found among ECD students, whereas students that are ECD *and* ELL cause the general ELL gap to appear smaller.

I find that by accounting for prior academic performance, I can shed light on the effects of having each (or multiple) disadvantage(s) on student performance. Value-added models that account for the doubly and triply disadvantaged provide more accurate representations of the effects of disadvantage on student performance. I find virtually no additional deleterious effects of being ECD with no additional disadvantages. However, the disadvantage group with the largest, most detrimental effect on test scores is among students that are both ECD *and* SWD, though this effect is not statistically different from the effect of being SWD with no additional disadvantages. This means that if most SWD students are also ECD, then the ECD gap may mostly reflect differences in performance between SWD and non-SWD students. Furthermore, I find positive effects among ELL students – both those with no additional disadvantages and those that are ECD *and* ELL – however effects are not statistically different from each other.

The coefficients produced by the value-added models are causal estimates of disadvantage if the within-school procedures for identifying disadvantage do not vary across students and there are no heterogeneous effects by student characteristics. However, within-school processes for identifying disadvantage may not be consistent across all students. For example, school processes may over (or under) identify SWD among Black and Hispanic students, under identify SWD among ELL students, and under identify females as SWD (Elder, Figlio, Imberman, & Persico, 2019; Morgan et. al., 2015; Arms, Bickett, & Graff, 2008). Elder et al, 2019 find that Black and Hispanic students are over-identified in schools with relatively small shares of minorities and substantially under-identified in schools with large minority shares, though opposite patterns occur among white students. Future work could address under and over identification concerns by including school-level measures of student race, such as share of minority students.

Furthermore, there may be differential effects of disadvantage by student race/ethnicity and gender. For example, achievement gaps and effects of disadvantage may be exacerbated among minority students compared to their white counterparts. Future research includes examining differential achievement gaps and effects of disadvantage by race/ethnicity and gender and investigating the potential implications of such findings for subgroup accountability standards and state funding formulas.

Lastly, I replace my contemporaneous measures of student disadvantage with more inclusive, "ever" definitions to catch a broader picture of student disadvantage – students that have exited SWD or ELL programs or have lost ECD status potentially due to changes in school lunch form requirements. I find using broader definitions of disadvantage reveals smaller achievement gaps for most disadvantage groups, and value-added models reveal the effects of each (or multiple) disadvantage(s) are, in general, smaller in magnitude that what contemporaneous definitions would suggest.

This paper uncovers evidence of the doubly disadvantaged that has previously been overlooked. And while these findings are the first to provide an in-depth dive into the characteristics and achievement gaps of the doubly disadvantaged, more research regarding those at the intersection of disadvantage is needed. This analysis provides a statistical portrait of students at the intersection of disadvantage in NC; however, it is limited in generalizability by the state-specific rules and regulations surrounding NC student disadvantage.

IX. Discussion & Policy Implications

This new information concerning the doubly disadvantaged can benefit policymakers and school officials alike. Better grasping the details of disadvantage-based achievement gaps can have implications for how districts (and states) support their disadvantaged populations. More specifically, the achievement gap findings in this paper can help improve federally mandated state accountability systems which require reports by subgroup – particularly since the achievement gaps of doubly disadvantaged students (i.e., students that are both ECD *and* SWD) are worse than what general achievement disparities would suggest.

Under the Every Student Succeeds Act (ESSA), state local education agencies are required to create and implement accountability systems and interventions (if necessary), and provide with the necessary funding, in an attempt to achieve equity for all students. Within this system, schools are required to report academic achievement by subgroups, including students that are ECD, SWD, ELL and from major racial and ethnic groups (USDOE, 2017). The analysis performed in this paper demonstrates that general ECD and ELL achievement gaps depend largely on the prevalence of doubly disadvantaged SWD students. Schools often face difficulty meeting SWD accountability benchmarks given how far behind these students are at the beginning of their academic career, in addition to the fact that SWD students generally perform worse as they age. Recognizing the prevalence of doubly disadvantaged students that are SWD in addition to ECD or ELL may have serious implications for schools trying to meet subgroup accountability standards across general ECD, SWD, and ELL subgroups. For example, schools with large shares of ECD *and* SWD students are more likely to struggle more to meet ECD subgroup accountability benchmarks *in addition to* SWD benchmarks each year compared to schools with large shares of ECD students with no additional disadvantages.

In addition, the effects of disadvantage estimated in value-added models can help inform conversations that revolve around supporting disadvantaged students through additional funding. State and local county contributions largely fund local school districts. States often use foundation aid formulas and pupil weights to determine the amount of extra aid school districts receive based on each district's share of disadvantaged students. The formula determines the amount of additional funding per disadvantaged student schools receive by multiplying each disadvantage-specific pupil weight by the share of students in each disadvantage category in the school district. While findings from this paper suggest that achievement gaps are larger among students that are both ECD *and* SWD, I find the *effect* of being ECD *and* SWD is not statistically different from the effect of being SWD with no additional disadvantages. These findings suggest the needs of the doubly disadvantaged may not be particularly different from those of the single disadvantaged, suggesting few, if any, consequences for formulas that provide additional monetary support based on general disadvantages.

Table 8: Student Disadvantage, Grades 3-8, 2016

	ECD	SWD	ELL	Any Disad
	(1)	(2)	(3)	(4)
% of All Students	51.2	13.5	6.2	57.1
ECD	100.0	65.4	75.6	89.6
SWD	17.3	100.0	21.6	23.7
ELL	6.2	9.9	100.0	10.8
# Students	336,894	89,034	40,777	376,149

Notes: Sample includes all 658,161 3-8 grade students in 2016. Any Disad refers to students with any of the three disadvantages.

Table 9: Mutually Exclusive Student Disadvantage, Grades 3-8, 2016

		No Add Disad			Doubly			Triply
	No Disad	ECD	SWD	ELL	ECD &SWD	ECD &ELL	SWD &ELL	ECD& SWD&ELL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
# Students	282,098	255,158	29,313	8,430	50,901	23,527	1,512	7,308
% of All	42.9	38.8	4.5	1.3	7.7	3.6	0.2	1.1

Notes: Sample includes all 658,161 3-8 grade students in 2016. No Disad refers to students without any disadvantages. No Add Disad stands for no additional disadvantages.

Table 10: Parsing Student Disadvantage, Grades 3-8, 2016

Panel A: ECD Students

		Doubly		Triply	
	No Add Disad	+SWD	+ELL	+SWD&ELL	Total
	(1)	(2)	(3)	(4)	(5)
ECD	255,158	50,901	23,527	7,308	336,894
	75.7	15.1	7.0	2.2	100.0

Panel B: SWD Students

		Doubly		Triply	
	No Add Disad	+ECD	+ELL	+ECD&ELL	Total
	(1)	(2)	(3)	(4)	(5)
SWD	29,313	50,901	1,512	7,308	89,034
	32.9	57.2	1.7	8.2	100.0

Panel C: ELL Students

		Doubly		Triply	
	No Add Disad	+ECD	+SWD	+ECD&SWD	Total
	(1)	(2)	(3)	(4)	(5)
ELL	8,430	23,527	1,512	7,308	40,777
	20.7	57.7	3.7	17.9	100.0

Notes: Sample includes all 658,161 3-8 grade students in 2016. No Add Disad stands for no additional disadvantages.

Table 11: Student Demographic Characteristics, Grades 3-8, 2016

	Female	White	Black	Hispanic	Asian/Other	All Students
	(1)	(2)	(3)	(4)	(5)	(6)
# Students	319,567	321,616	165,830	114,918	55,883	658,247
% of All	48.5	48.9	25.2	17.5	8.5	100.0

Notes: Sample includes all 658,161 3-8 grade students in 2016.

Table 12: Student Demographic Characteristics by Disadvantage, Grades 3-8, 2016

	ECD	SWD	ELL
	(1)	(2)	(3)
	163,189	30,567	17,908
Female	51.1	9.6	5.6
	48.4	34.3	43.9
	106,917	40,914	1,719
White	33.2	12.7	0.5
	31.7	46.0	4.2
	118,750	27,749	1,193
Black	71.6	16.7	0.7
	35.3	31.2	2.9
	84,632	14,123	33,690
Hispanic	73.7	12.3	29.3
	25.1	15.9	82.6
	26,595	6,248	4,175
Asian/Other	27.6	11.2	7.47
	7.9	7.0	10.2
# Students	336,894	89,034	40,777
% of All	51.2	13.5	6.2

Notes: The first number in each cell describes the number of students, the second refers to the row percentage, and the third refers to the column percentage. Sample includes all 658,161 3-8 grade students in 2016.

Table 13: Student Demographic Characteristics by Additional Disadvantages, Grades 3-8, 2016

	No Add Disad	Doubly	Triply
	(1)	(2)	(3)
	145,399	29,264	2,579
Female	45.5	9.2	0.8
	49.6	38.5	35.3
	105,804	21,627	164
White	32.9	6.7	0.1
	36.1	28.5	2.2
	102,126	22,636	98
Black	61.6	13.7	0.1
	34.9	29.8	1.3
	60,733	25,821	6,690
Hispanic	52.9	22.5	5.8
	20.7	34.0	91.5
	24,238	5,856	356
Asian/Other	43.4	10.5	0.6
	8.3	7.7	4.9
# Students	292,901	75,940	7,308
% of All	44.5	11.5	1.1

Notes: The first number in each cell describes the number of students, the second refers to the row percentage, and the third refers to the column percentage. Sample includes all 658,161 3-8 grade students in 2016. No Add Disad stands for no additional disadvantages.

Table 14: Academic Performance by Disadvantage, Grades 3-8, 2016

	All Students	No Disad	Any Disad	ECD	SWD	ELL
	(1)	(2)	(3)	(4)	(5)	(6)
zMath	0.00	0.21	-0.16	-0.10	-0.84	-0.20
zRead	0.00	0.22	-0.16	-0.10	-0.88	-0.28
# Math Students	652,537	281,460	371,077	333,747	88,017	37,472
# Read Students	652,482	281,434	371,048	333,767	88,052	37,389

Notes: Sample includes all 3-8 grade students with test scores in 2016. No Disad refers to students without any disadvantages, and Any Disad refers to students with any of the three disadvantages. zMath and zRead for ELL students are statistically different from each other.

Table 15: Parsing Academic Performance by Disadvantage, Grades 3-8, 2016

Panel A: ECD Students

		Doubly			Triply
	ECD	**No Add Disad**	**+ SWD**	**+ ELL**	**+ SWD & ELL**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
zMath	-0.10	0.06	-0.84	-0.03	-0.80
zRead	-0.10	0.07	-0.89	-0.10	-0.89
# Math Students	333,747	254,293	50,338	21,889	7,227
# Read Students	333,767	254,316	50,358	21,861	7,232

Panel B: SWD Students

		Doubly			Triply
	SWD	**No Add Disad**	**+ ECD**	**+ ELL**	**+ ECD & ELL**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
zMath	-0.84	-0.84	-0.84	-0.87	-0.80
zRead	-0.88	-0.86	-0.89	-0.96	-0.89
# Math Students	88,017	28,974	50,338	1,478	7,227
# Read Students	88,052	28,985	50,358	1,477	7,232

Panel C: ELL Students

		Doubly			Triply
	ELL	**No Add Disad**	**+ECD**	**+ SWD**	**+ ECD & SWD**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
zMath	-0.20	0.02	-0.03	-0.87	-0.80
zRead	-0.28	-0.06	-0.10	-0.96	-0.89
# Math Students	37,472	6,878	21,889	1,478	7,227
# Read Students	37,389	6,819	21,861	1,477	7,232

Notes: Sample includes all 3-8 grade students with test scores in 2016. No Add Disad refers to students without any additional disadvantages.

Table 16: zMath Baseline Regression Results, Grades 3-8, 2009-2016

	(1)	(2)	(3)	(4)	(5)	(6)
ECD	**-0.16**	**-0.11**	**-0.08**	**-0.13**	**-0.09**	**-0.06**
	(0.00)	**(0.00)**	**(0.00)**	**(0.00)**	**(0.00)**	**(0.00)**
SWD	**-1.36**	**-1.35**	**-1.30**	**-1.22**	**-1.22**	**-1.16**
	(0.00)	**(0.00)**	**(0.01)**	**(0.02)**	**(0.02)**	**(0.02)**
ELL	**-0.08**	**-0.12**	**-0.12**	**-0.20**	**-0.21**	**-0.19**
	(0.00)	**(0.00)**	**(0.00)**	**(0.01)**	**(0.01)**	**(0.01)**
ECD*SWD	**-**	**-**	**-**	**-0.22**	**-0.22**	**-0.23**
				(0.02)	**(0.02)**	**(0.01)**
ECD*ELL	**-**	**-**	**-**	**0.11**	**0.08**	**0.07**
				(0.01)	**(0.01)**	**(0.01)**
SWD*ELL	**-**	**-**	**-**	0.07	0.07	**0.05**
				(0.04)	(0.04)	**(0.04)**
ECD*SWD*ELL	**-**	**-**	**-**	**0.08**	**0.07**	0.07
				(0.04)	**(0.04)**	(0.04)
Student Char.	N	Y	Y	N	Y	Y
School FE	N	N	Y	N	N	Y
Grade FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
No. Schools	2,073	2,073	2,073	2,073	2,073	2,073
No. Districts	115	115	115	115	115	115

Notes: Data includes student-level observations for grades 3-8 for years 2009-2016. Student characteristic estimates not shown include race and gender and omitted categories are white and male. Columns 1 and 4 are uncontrolled models with grade and year fixed effects, columns 2 and 5 include student characteristics and year and grade fixed effects, and columns 3 and 6 are fully controlled and include school fixed effects, with robust standard errors clustered at the school level. Bold coefficients are statistically significant at p<.01. Models for zMath include 5,259,012 student observations.

Table 17: Achievement Gap Coefficient Interpretations of Baseline Regression Model

	zMath		**zRead**	
	Naïve	**Preferred**	**Naïve**	**Preferred**
	(1)	**(2)**	**(3)**	**(4)**
ECD	-0.08	-0.06	-0.09	-0.08
SWD	-1.30	-1.16	-1.22	-1.15
ELL	-0.12	*-0.19*	-0.19	*-0.27*
ECD&SWD	-1.38	*-1.45*	-1.31	*-1.35*
ECD&ELL	-0.20	-0.18	-0.28	-0.28
SWD&ELL	-1.42	-1.31	-1.41	-1.31
ECD,SWD,&ELL	-1.50	-1.45	-1.50	-1.41

Notes: All point estimates in columns 2 and 4 are statistically significant at $p<.01$. Point estimates in column 2 for ELL and ECD&ELL, as well as ECD&SWD and ECD,SWD,&ELL are not statistically different from each other, and point estimates in column 4 for ECD&SWD and SWD&ELL are not statistically different from each other

Table 18: Parsing Achievement Gaps by the Doubly and Triply Disadvantaged, Grades 3-8, 2009-2016

	zMath					zRead				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ECD	**-0.08**	**-0.06**	**-0.06**	**-0.06**	**-0.06**	**-0.09**	**-0.08**	**-0.08**	**-0.08**	**-0.08**
	(0.00)	**(0.00)**	**(0.00)**	**(0.00)**	**(0.00)**	**(0.00)**	**(0.00)**	**(0.00)**	**(0.00)**	**(0.00)**
SWD	**-1.30**	**-1.16**	**-1.16**	**-1.17**	**-1.16**	**-1.22**	**-1.15**	**-1.15**	**-1.15**	**-1.15**
	(0.01)	**(0.02)**	**(0.02)**	**(0.02)**	**(0.02)**	**(0.01)**	**(0.02)**	**(0.02)**	**(0.02)**	**(0.02)**
ELL	**-0.12**	**-0.12**	**-0.19**	**-0.20**	**-0.19**	**-0.19**	**-0.19**	**-0.25**	**-0.27**	**-0.27**
	(0.00)	**(0.00)**	**(0.01)**	**(0.01)**	**(0.01)**	**(0.00)**	**(0.00)**	**(0.01)**	**(0.01)**	**(0.01)**
ECD*SWD	**-**	**-0.21**	**-0.21**	**-0.22**	**-0.23**	**-**	**-0.11**	**-0.11**	**-0.12**	**-0.12**
		(0.01)	**(0.01)**	**(0.01)**	**(0.01)**		**(0.01)**	**(0.01)**	**(0.01)**	**(0.01)**
ECD*ELL	**-**	**-**	**0.08**	**0.08**	**0.07**	**-**	**-**	**0.07**	**0.07**	**0.07**
			(0.01)	**(0.01)**	**(0.01)**			**(0.01)**	**(0.01)**	**(0.01)**
SWD*ELL	**-**	**-**	**-**	**0.11**	**0.05**	**-**	**-**	**-**	**0.13**	**0.11**
				(0.02)	**(0.04)**				**(0.02)**	**(0.04)**
ECD*SWD*ELL	**-**	**-**	**-**	**-**	0.07	**-**	**-**	**-**	**-**	0.03
					(0.04)					(0.04)
Student Char.	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
No. Schools	2,073	2,073	2,073	2,073	2,073	2,073	2,073	2,073	2,073	2,073
No. Districts	115	115	115	115	115	115	115	115	115	115

Notes: Data includes student-level observations for grades 3-8 for years 2009-2016. Bold coefficients signal statistical significance at p<.01. Student characteristic estimates not shown include race and gender and omitted categories are white and male. Columns 1 and 4 are uncontrolled models with grade and year fixed effects, columns 2 and 5 include student characteristics and year and grade fixed effects, and columns 3 and 6 are fully controlled and include school fixed effects, with robust standard errors clustered at the school level. Models for zMath include are 5,259,012 student observations, and models for zRead include 5,258,599 student observations.

Table 19: Value-Added Results, Grades 3-8, 2009-2016

	zMath		zRead	
	(1)	(2)	(3)	(4)
ECD	-0.01	-0.01	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
SWD	-0.12	-0.12	-0.07	-0.07
	(0.00)	(0.00)	(0.00)	(0.00)
ELL	0.04	0.01	0.05	0.04
	(0.00)	(0.01)	(0.00)	(0.01)
ECD*SWD	-	-0.01	-	-0.01
		(0.00)		(0.00)
ECD*ELL	-	0.02	-	0.01
		(0.01)		(0.01)
SWD*ELL	-	0.06	-	-0.00
		(0.02)		(0.02)
ECD*SWD*ELL	-	-0.06	-	-0.03
		(0.02)		(0.02)
Lagged Test Score	0.86	0.86	0.90	0.90
	(0.00)	(0.00)	(0.00)	(0.00)
Student Char.	Y	Y	Y	Y
Grade FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
School FE	Y	Y	Y	Y
No. Schools	2,034	2,034	2,034	2,034
No. Districts	115	115	115	115

Notes: Data includes student-level observations for grades 3-8 for years 2009-2016. All point estimates are significant at the p<.01. White, non-disadvantaged students are the omitted group. Student characteristics include race and gender. Robust standard errors clustered at the school level. Models for zMath include 3,577,879 student observations, and models for zRead include 3,578,051 student observations.

Table 20: Effects of Disadvantage Coefficient Interpretations of Value-Added Models

	zMath		zRead	
	Naïve	**Preferred**	**Naïve**	**Preferred**
	(1)	**(2)**	**(3)**	**(4)**
ECD	-0.01	-0.01	-0.00	-0.00
SWD	-0.12	-0.12	-0.07	-0.07
ELL	0.04	*0.01*	0.05	*0.04*
ECD&SWD	-0.13	*-0.14*	-0.07	*-0.08*
ECD&ELL	0.03	0.03	0.05	0.05
SWD&ELL	-0.08	*-0.05*	-0.02	*-0.03*
ECD,SWD,&ELL	-0.09	*-0.10*	-0.02	*-0.05*

Notes: All point estimates in columns 2 and 4 are statistically significant at $p<.01$. Point estimates in columns 2 and 4 for SWD with no additional disadvantages and ECD&SWD, as well as ELL with no additional disadvantages and ECD&ELL are not statistically different from each other.

X. Appendix B

Table B1: Parsing Student Demographic Characteristics by Disadvantage, Grades 3-8, 2016

Panel A: ECD Students

			Doubly		Triply
	ECD	**No Add Disad**	**+SWD**	**+ELL**	**+SWD&ELL**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	163,189	131,835	17,807	10,968	2,579
Female	51.1	41.3	5.6	3.4	0.8
	48.4	51.7	35.0	46.6	35.3
	106,917	85,220	20,738	795	164
White	33.2	26.5	6.5	0.3	0.1
	31.7	33.4	40.7	3.4	2.2
	118,750	96,047	21,844	761	98
Black	71.6	57.9	13.2	0.5	0.1
	35.3	37.6	42.9	3.2	1.3
	84,632	53,301	4,623	20,018	6,690
Hispanic	73.7	46.4	4.0	17.4	5.8
	25.1	20.9	9.1	85.1	91.5
	26,595	20,590	3,696	1,953	356
Asian/Other	27.6	36.8	6.6	3.5	0.6
	7.9	8.1	7.3	8.3	4.9
# Students	336,894	255,158	50,901	23,527	7,308
% of All	51.2	38.8	7.7	3.6	1.1

Notes: Sample includes all 658,161 3-8 grade students in 2016. No Add Disad stands for no additional disadvantages. The numbers in each cell refer to the number of students, the row percentage, and the column percentage.

Panel B: SWD Students

			Doubly		Triply
	SWD	**No Add Disad**	**+ECD**	**+ELL**	**+ECD&ELL**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	30,567	9,692	17,375	489	2,579
Female	9.6	3.0	5.6	0.2	0.8
	34.3	33.1	35.0	32.3	35.3
	40,914	19,918	20,738	94	164
White	12.7	6.2	6.5	0.0	0.1
	46.0	68.0	40.7	6.2	2.2
	27,749	5,776	21,844	31	98
Black	16.7	3.5	13.2	0.0	0.1
	31.2	19.7	42.9	2.1	1.3
	14,123	1,630	4,623	1,180	6,690
Hispanic	12.3	1.4	4.0	1.0	5.8
	15.9	5.6	9.1	78.0	91.5
	6,248	1,989	3,696	207	356
Asian/Other	11.2	3.6	6.6	0.4	0.6
	7.0	6.8	7.3	13.7	4.9
	89,034	29,313	50,901	1,512	7,308
All Students	13.5	4.5	7.7	0.2	1.1

Panel C: ELL Students

			Doubly		Triply
	ELL	**No Add Disad**	**+ECD**	**+SWD**	**+ECD&SWD**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	17,908	3,872	10,968	489	2,579
Female	5.6	1.2	3.4	0.2	0.8
	43.9	45.9	46.6	32.3	35.3
	1,719	666	795	94	164
White	0.5	0.2	0.3	0.0	0.1
	4.2	7.9	3.4	6.2	2.2
	1,193	303	761	31	98
Black	0.7	0.2	0.5	0.0	0.1
	2.9	3.6	3.2	2.1	1.3
	33,690	5.802	20,018	1,180	6,690
Hispanic	29.3	5.1	17.4	1.0	5.8
	82.6	68.8	85.1	78.0	91.5
	4,175	1,659	1,953	207	356
Asian/Other	7.47	3.0	3.5	0.4	0.6
	10.2	19.7	8.3	13.7	4.9
	40,777	8,430	23,527	1,512	7,308
All Students	6.2	1.3	3.6	0.2	1.1

Table B2: zRead Baseline Regression Results, Grades 3-8, 2016

	(1)	(2)	(3)	(4)	(5)	(6)
ECD	**-0.16**	**-0.12**	**-0.09**	**-0.15**	**-0.11**	**-0.08**
	(0.00)	**(0.00)**	**(0.00)**	**(0.00)**	**(0.00)**	**(0.00)**
SWD	**-1.28**	**-1.28**	**-1.22**	**-1.22**	**-1.21**	**-1.15**
	(0.02)	**(0.02)**	**(0.01)**	**(0.03)**	**(0.03)**	**(0.02)**
ELL	**-0.18**	**-0.20**	**-0.19**	**-0.30**	**-0.29**	**-0.27**
	(0.00)	**(0.00)**	**(0.00)**	**(0.01)**	**(0.01)**	**(0.01)**
ECD*SWD	-	-	-	**-0.11**	**-0.11**	**-0.12**
				(0.02)	**(0.02)**	**(0.01)**
ECD*ELL	-	-	-	**0.11**	**0.07**	**0.07**
				(0.01)	**(0.01)**	**(0.01)**
SWD*ELL	-	-	-	**0.14**	**0.14**	**0.11**
				(0.04)	**(0.04)**	**(0.04)**
ECD*SWD*ELL	-	-	-	0.03	0.03	0.03
				(0.04)	(0.04)	(0.04)
Student Char.	N	Y	Y	N	Y	Y
School FE	N	N	Y	N	N	Y
Grade FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
No. Schools	2,073	2,073	2,073	2,073	2,073	2,073
No. Districts	115	115	115	115	115	115

Notes: Data includes student-level observations for grades 3-8 for years 2009-2016. Student characteristic estimates not shown include race and gender and omitted categories are white and male. Columns 1 and 4 are uncontrolled models with grade and year fixed effects, columns 2 and 5 include student characteristics and year and grade fixed effects, and columns 3 and 6 are fully controlled and include school fixed effects, with robust standard errors clustered at the school level. Bold coefficients are statistically significant at $p<.01$. Models for zRead include 5,258,599 student observations.

Table B3: Baseline Regression Results, Value-Added Sample, Grades 3-8, 2016

	zMath	zRead
	(1)	(2)
ECD	-0.06	-0.08
	(0.00)	(0.00)
SWD	-1.18	-1.17
	(0.02)	(0.02)
ELL	-0.18	-0.25
	(0.00)	(0.00)
ECD*SWD	-0.19	-0.09
	(0.02)	(0.02)
ECD*ELL	0.06	0.06
	(0.00)	(0.00)
SWD*ELL	0.02	0.07
	(0.05)	(0.05)
ECD*SWD*ELL	0.08	0.03
	(0.05)	(0.05)
Student Char.	Y	Y
School FE	Y	Y
Grade FE	Y	Y
Year FE	Y	Y
No. Schools	2,073	2,073
No. Districts	115	115

Notes: Data includes student-level observations used in value-added models for grades 3-8 for years 2009-2016. All point estimates are significant at the p<.01. Student characteristic estimates not shown include race and gender and omitted categories are white and male. Robust standard errors clustered at the school level.

Table B4: Baseline Regression Results, Ever Disadvantaged, Grades 3-8, 2009-2016

	Baseline				Value-Added			
	zMath		zRead		zMath		zRead	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ever								
ECD	-0.06	-0.06	-0.07	-0.07	-0.01	-0.01	-0.01	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
SWD	-0.73	-0.73	-0.73	-0.73	-0.07	-0.07	-0.04	-0.04
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
ELL	-0.05	-0.05	-0.10	-0.10	0.01	0.01	0.02	0.02
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
ECD*SWD	-0.30	-1.09	-0.22	-1.03	-0.01	-0.10	-0.01	-0.05
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
ECD*ELL	-0.01	-0.12	-0.02	-0.19	0.01	0.00	0.01	0.02
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
SWD*ELL	-0.06	-0.83	-0.03	-0.86	-0.00	-0.06	-0.00	-0.03
	(0.04)	(0.04)	(0.04)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)
ECD*SWD*ELL	0.16	-1.04	0.12	-1.06	0.02	-0.06	0.01	-0.02
	(0.04)	(0.02)	(0.04)	(0.01)	(0.02)	(0.01)	(0.01)	(0.00)
Student Char.	Y		Y		Y		Y	
Grade FE	Y		Y		Y		Y	
Year FE	Y		Y		Y		Y	
School FE	Y		Y		Y		Y	
No. Schools	2,034		2,034		2,034		2,034	
No. Districts	115		115		115		115	

Notes: Data includes student-level observations for grades 3-8 for years 2009-2016. Student characteristic estimates not shown include race and gender and omitted categories are white and male. Odd Columns show regression results from preferred models described in earlier sections: controlling for student characteristics, grade, year, and school fixed effects with robust errors clustered at the school level. Even Columns interpret the coefficients estimated in each prior odd column.

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Chapter 3: Paying for Free Lunch: The Impact of CEP Universal Free Meals on Revenues, Spending, and Student Health

I. Introduction

The vast majority of US schools – approximately 95 percent – serve subsidized meals to over 30 million students on an average day (FRAC, 2019). Under the National School Lunch program (NSLP) and School Breakfast Program (SBP), meals are free for eligible low-income students, with higher prices charged to families with higher incomes. Adopted in 2010, the Community Eligibility Provision (CEP) of the Healthy, Hunger-Free Kids Act (HHFKA) allows schools or districts to adopt Universal Free Meals (UFM), a program that provides free meals to all students, regardless of household income, if at least 40 percent of students are "directly certified" for free meals.[1] Advocates claim UFM reduces stigma, food insecurity, hunger, and administrative burden while improving student nutrition and readiness to learn. Recent research finds UFM increases participation in school food, reduces suspension rates, and improves academic achievement and perceptions of school climate (Schwartz & Rothbart, 2020; Gordon & Ruffini, 2019; Ruffini, forthcoming; Kho, 2018-working paper; Gutierrez, 2020-working paper).

Critics, on the other hand, worry UFM may have unintended consequences such as increased financial burdens for school districts – even while it may reduce the parental burden of providing meals. While CEP's reimbursement structure appears more generous than other UFM provisions, federal reimbursements may not fully cover CEP-induced gaps in school district budgets due to loss of local food revenues (i.e., lunch and breakfast fees) and/or changes in price or costs of production for school meals, among others. Furthermore, if CEP induces school food

[1] Students are directly certified eligible if they participate in specific means-tested programs, including Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), or Medicaid. Students are also eligible if they are in foster care or Head Start, are homeless, are migrant, or participate in the Food Distribution Program on Indian Reservations benefits (FRAC, 2017).

programs to run deficits, do districts reallocate instructional expenditures to make up the difference?

Critics also worry that UFM may exacerbate student obesity. UFM's effect on student health can depend on a number of factors, including the nutritional value of school food, availability of alternatives, student responses to price changes, changes in participation, and whether students increase total caloric consumption by doubling up on meals. There is, unfortunately, little empirical evidence of the effect of UFM on student health. Schwartz & Rothbart (2020) investigate school-level UFM programs in New York City (NYC) offered under an alternative provision, Provision 2. They find UFM increases participation in school lunch and improves test scores, with suggestive but statistically insignificant evidence of beneficial effects on weight. And, Davis and Musaddiq (2019) find that UFM implemented in Georgia under CEP increases the share of students in the healthy BMI weight range.

To be sure, UFM may deliver other unintended consequences or, more broadly, social welfare costs (or benefits). For example, students might benefit from homemade lunches, say, by strengthening family bonds or encouraging self-sufficiency – benefits which might be foregone under UFM. Or, UFM might decrease sales at neighborhood eateries. We leave those questions for a future study, focusing here on whether – and how much – CEP affects school district finances and student weight outcomes.

This paper uses data on New York State (NYS) districts and schools to estimate the effects of CEP on student weight outcomes, explore the potential mechanisms through which CEP may affect obesity (including meal participation and attendance), and estimate novel effects of CEP on district revenues and expenditures. We use data on 698 school districts for 2010-2017 including demographics, enrollment, school food revenues and expenditures, and new data on school meal participation and obesity rates for primary and secondary grade students.

We explore heterogeneity along three dimensions: (1) grades served (primary vs. secondary), (2) urbanicity (metro, town, and rural communities), and (3) differential implementation (selective vs. districtwide) for the following reasons. First, since younger students are more likely than older students to participate in school meals, districts may selectively implement CEP in primary schools first. At the same time, older students may be more responsive than younger students to prices and consume less healthy alternatives to school food. Therefore, increasing participation among older students may have larger effects on weight outcomes.

Second, urban districts may differ from rural districts in a variety of ways. Urban districts are likely to face higher costs (especially wages) while rural locations offer fewer convenient alternatives to school meals. Rural districts have higher school meal participation rates, higher shares of students paying full price for school meals, and lower shares who are free lunch eligible. Thus, rural school districts may see larger losses in revenues from fees and lower reimbursements than urban districts. Moreover, since rural districts are typically smaller, they are more likely to implement CEP districtwide rather than selectively in a subset of schools, suggesting larger estimated district-level effects.

We use a difference-in-differences approach with district fixed effects and compare early to late CEP-adopting districts, exploiting the staggered adoption of CEP to estimate the impact on revenues, spending, and weight outcomes. While districts that adopt CEP may be systematically different from those that do not, the precise timing of adoption is plausibly exogenous among those that adopt CEP at some point in our study period – the "Ever CEP" districts. District fixed effects and time-varying control variables further adjust for time invariant differences between districts and time varying differences between and within districts over time. We then use non-parametric event study models to test the parallel trends assumption and explore the evolution of the effects in the years following the adoption of UFM under CEP.

To preview the results, we find CEP does, indeed, increase school meal participation in lunch by as much as 8.5 percentage points and breakfast by 11.5 percentage points, with effects varying by urbanicity and grade span. There is, however, no meaningful effect on attendance, suggesting any effects on district or student outcomes are not driven by changes in attendance. CEP also improves weight outcomes for secondary school students who are, perhaps, more sensitive to prices and more likely to eat unhealthy substitutes for school meals; effects on elementary school students are not significant. We find local food revenues decline (perhaps mechanically due to the elimination of meals fees), while federal food revenues and total food expenditures grow. Overall, federal revenues more than compensate for changes in school food revenues and expenditures, with no effect on instructional expenditures. CEP, as a result, helps close the school food services gap, on average.

As expected, the effects differ across settings. The impacts of CEP are larger in rural districts – specifically, the increase in breakfast participation and decrease in obesity among secondary school students are larger. Unlike metro and town districts, reductions in school meal fees and increases in food expenditures in rural districts are not fully offset by federal subsidies. We find expenditures increase with expanded CEP implementation. Moreover, we find the declines in the percentage overweight and obese students are larger in districts with wider CEP implementation and occur in both primary and secondary grades. There is little evidence that expansion of CEP leads to the unintended consequence of increased weight; in fact, quite the opposite appears to be true.

In summary, we derive credibly causal estimates of CEP's effect on student weight and district financial outcomes, informing the debate on whether – and how much – the benefits of UFM are coupled with unintended negative consequences for school district finances and/or student weight outcomes. We see little evidence of deleterious effects on the prevalence of obesity or overweight students, or on instructional expenditures. We see large increases in federal reimbursements that, in most districts, trump the size of increased food expenditures. Thus, the "price" of UFM seems to be largely paid by the federal government, with a notable exception for rural districts.

II. Background

The NSLP and SBP provide free and low-cost meals to tens of millions of children in over 100,000 schools and childcare institutions each year – making the NSLP the second largest food and nutrition assistance program in the US (behind the Supplemental Nutrition Assistance Program (SNAP)). These school meal programs cost the Federal government $18.2 billion annually and provide subsidized meals to students based on household income (USDA, 2019). Specifically, students in households earning incomes less than 185 percent of the federal poverty line pay a reduced price, while students with household incomes less than 130 percent receive school meals for free. Students not certified as eligible for free or reduced-price meals – which includes both students with family incomes above the threshold and those who have not obtained the requisite certification – pay the "full" price.

The Community Eligibility Provision (CEP) of the Healthy, Hunger-Free Kids Act (HHFKA) of 2010 allows certain schools and districts to provide free meals to all students. Under CEP, a school, cluster of schools within the same district, or entire district can adopt UFM if at least 40 percent of students are free-lunch eligible. Participating schools or districts use "direct certification" to determine the percent of free-lunch eligible students, also known as the Identified Student Percentage (ISP). Direct certification matches students to administrative records indicating student household participation in SNAP, Temporary Assistance for Needy Families (TANF), or Medicaid, among others.[2] Though introduced in 2010, CEP was piloted in eleven states from 2012-2014 and became available nation-wide in 2015. CEP expanded quickly, reaching over 14,000 schools in 2,200 districts in 2015 to 28,400 schools in 4,600 districts in 2019. As of 2019, almost 65 percent of eligible schools across the nation had implemented UFM via CEP (FRAC, 2019).

Schools or districts may be more likely to adopt UFM under CEP than under other UFM provisions due to CEP's relatively generous reimbursement structure.[3] Under CEP, schools' and districts' reimbursements are the product of four terms: (1) the federal subsidy for free-lunch, (2) the number of meals served, (3) the ISP, and (4) a multiplier of 1.6. Mechanically, this means schools or districts with ISPs greater than or equal to 62.5 percent are reimbursed at the federal free-lunch rate for all meals served (because 62.5 percent x 1.6 = 100 percent). Under Provision 2, for example, schools would be reimbursed at the federal free-lunch rate *only* for the share of

[2] Medicaid was added to the NY list of programs certifiable through direct certification in academic year 2014.
[3] Since 1980, schools in which at least 80 percent of enrolled children are eligible for free or reduced-price meals can also implement UFM under Provision 1. Since 1995, schools can also offer UFM under Provision 3, which sets reimbursement levels based on the average number of meals served by eligibility group in the most recent year in which the school tracked individual lunch utilization – rather than the average percentages by eligibility group, the method used under Provision 2. Under Provision 3, reimbursements are adjusted for inflation and enrollment, but not for changes in the number of meals served (Schwartz & Rothbart, 2020).

meals served to otherwise "free-eligible" students as of some base year – a much less generous reimbursement if free lunch eligibility rates are high.

III. Prior Literature

School meal participation rates are lower than one might expect, especially among certified income eligible students for whom meals are free (Gleason, 1995). A number of factors may affect students' likelihood of participating in the school meals programs. For example, higher school food prices and income are correlated with low participation rates (Akin et al., 1983; Gleason, 1995; Maurer, 1984). Moreover, participation varies by race – with black students participating at higher rates than white students (Akin et al., 1983; Dunifon & Kowaleski-Jones, 2003; Mirtcheva & Powell, 2009). Other factors that influence participation decisions include the quality and variety of school meals and the stigma associated with school food (Glantz, Berg, Porcari, Sackoff, & Pazer, 1994; Mirtcheva & Powell, 2009; Poppendieck, 2010). Mirtcheva and Powell (2009) find poor students' participation rates are lower in schools with fewer poor students, and older students are less likely to participate compared to younger students. These considerations may explain why, in some districts, over 10 percent of income eligible students are not certified for free or reduce-priced meals (Domina et. al., 2018). However, recent research finds that expanding the availability of free meals, through programs such as UFM, increases participation (Leos-Urbel, Schwartz, Weinstein, & Corcoran, 2013; Schwartz & Rothbart, 2020; Ruffini, forthcoming).

According to the USDA, participation in school food – and the HHFKA (2010), in particular – "improves nutrition and focuses on reducing childhood obesity" (The White House Task Force on Childhood Obesity, 2010). Empirical evidence on the nutrition of school food programs is largely positive: the nutritional quality of school meals is usually higher than alternatives (Caruso & Cullen, 2015; Cohen et al., 2014; Farris et al., 2015; Smith 2017) and expanding availability of school meals improves child nutrition (Bhattacharya, Currie, & Haider, 2006; Gundersen, Kreider, & Pepper, 2012). However, evidence on child obesity, a central public health concern, is mixed. Some find that participation in NSLP increases primary school student obesity (Millimet, Husain, & Tchernis, 2010; Schanzenbach, 2009). Those that have examined the impacts of expansions in the availability of free school meals, however, mostly find null effects (Corcoran, Elbel, & Schwartz, 2016; Kitchen et al., 2013; Schwartz & Rothbart, 2020). One potential explanation for the mixed evidence is that some students experience nutritional improvements, while others may double up on meals, increasing total caloric intake and exacerbating childhood obesity. Another explanation is that context matters, particularly as it relates to the availability and nutritional quality of alternatives to school food.

There is growing evidence on the relationship between schools, environment, and student weight outcomes. As an example, existing research finds school food programs have null if not beneficial impacts on student obesity (Corcoran, Elbel, & Schwartz, 2016; Kitchen et al., 2013; Schwartz & Rothbart, 2020; Davis & Mussadiq, 2019). As for CEP in particular, Davis and Musaddiq (2019) find CEP adoption in Georgia schools increases the share of students in the "healthy" BMI range. Using NYS Student Weight Status Category Reporting System (SWSCRS) – data which we also use – Dwicaksono, et al. (2018) explore the environmental and policy correlates of district-level obesity rates, finding suggestive evidence that the obesity of secondary school students is more sensitive than primary school students. The authors offer descriptive evidence that rural districts have higher primary school obesity rates than metropolitan districts and that obesity is more strongly correlated with fast-food restaurant density among secondary students than primary students (perhaps due to differences in food consumption patterns).

Recent research documents positive effects of CEP on a range of student academic and disciplinary outcomes (Ruffini, forthcoming, Gordon & Ruffini, 2019; Kho, 2018-working paper; Comperatore & Fuller, 2018). Kho (2018) utilizes CEP adoption in South Carolina to find a 0.03-0.04 standard deviation improvement in elementary student math scores. Ruffini (forthcoming) utilizes the cross-state variation in the timing of CEP eligibility and finds math performance increases by 0.02 standard deviations in districts with the largest shares of students with CEP. Gordon and Ruffini (2019) similarly investigates the effects of CEP but on suspension rates from the Civil Rights Data Collection and finds modest reductions in elementary and middle but not high school suspensions. Overall, research finds null or modest decreases in student absences post CEP adoption (Comperatore & Fuller, 2018; Kho, 2018).

The growing research on academic outcomes has not, however, been matched by evidence on what school districts pay for UFM, much less CEP. One notable exception is Leos-Urbel et al. (2013) which estimates the user fee revenue lost from providing roughly 3.5 million free breakfasts at approximately $300,000 in 2004. They did not, however, examine any costs or savings due to changes in administrative costs associated with the collection and processing of breakfast fees, the economies of scale, or changes in costs of providing a larger number of meals.

A technical report from the NYC Independent Budget Office (IBO) sheds some light on the monetary implications of adopting CEP. The IBO examined the NYC school lunch program's current costs, as well as the cost of expanding UFM under Provision 2 and CEP from stand-alone middle schools to all elementary schools in NYC (NYCIBO, 2017). Using the citywide ISP rate, the IBO finds expanding CEP to elementary schools at the given participation rates and prices would cost NYC $5.2 million – an amount greater than the cost of traditional NSLP but less than other provisions, such as Provision 2.

IV. Conceptual Issues and Hypotheses

Eliminating school lunch fees through CEP is likely to affect student weight and district finances through two key mechanisms. First, eliminating school food fees may spur participation in both breakfast and lunch, as families choose school food over alternatives that they would have to pay for. Second, the promise of consistent and free meals may increase attendance as students attend school to participate in lunch and/or breakfast. That said, high baseline attendance rates leave little room for improvement, and it may not be possible to identify a meaningful effect.

If school meals are more nutritious than the average alternative, as indicated by previous research (Caruso & Cullen, 2015; Cohen et al., 2014; Farris et al., 2015), then an increase in participation in school food induced by CEP should reduce the incidence of obesity (or overweight), with the magnitude of the effect varying with the change in participation and the characteristics of the foregone alternative food. This suggests effects will vary with the district/school context and characteristics of the students. Thus, effects are likely to vary by age: older students are likely to be more sensitive to price changes and the stigma associated with free school food and more likely to rely on unhealthy alternatives (like fast food) in the absence of school food. Therefore, effects are likely to be larger in districts implementing CEP in schools serving older grades. Notice that student weight outcomes will also depend upon the change in participation rates in both breakfast and lunch.

As for finances, the direct effect of eliminating school meal fees will be a reduction in local school food revenues (i.e., meal fees previously collected from paying students) and increases in federal school food revenues (i.e., reimbursements). Further, if participation increases, as expected, food expenditures should increase – both overall and per pupil. That said, there may be reductions in food expenditures per meal, consistent with economies of scale. Finally, increases in breakfast and lunch participation will increase federal subsidy revenues per pupil (due to increased meals served).

The impact of CEP on revenues and expenditures, therefore, will depend upon the share of students eligible for free lunch (ISP); the user fees (prices) paid for breakfast and lunch by reduced price and "full price" students; the change in participation/utilization for each of these groups in breakfast and lunch; the federal reimbursement rates (which vary by meal type); and changes to the costs of inputs used (ex. less expensive ingredients or lower prices). For example, as noted previously, changes in total food revenues per pupil will depend on federal reimbursements per meal, which under CEP is a direct function of the reimbursement price (one for lunch and another for breakfast) and the ISP rate (multiplied by a factor of 1.6).[4] Mathematically, districts with ISPs greater than 62.5 percent get reimbursed at the full federal rate for each breakfast and lunch served. Districts with ISPs below that ISP rate, however, are essentially only reimbursed for a fraction of each meal. The size of the revenue gap to be filled will also depend, in part, on lost user fees previously charged to students paying "full" and reduced prices for breakfast and lunch, the federal reimbursement rate for free and reduced meals, and the participation rate in breakfast and lunch. In fact, even districts with ISPs above

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[4] Algebraically, under the CEP, federal food revenue per pupil, R, is: $R = (FRS \text{ X } ISP \text{ X } 1.6) \text{ X } M$, where FRS is the Free Rate Subsidy, ISP is the identified student percentage, and M is the meal participation rate. Since FRS and M differ for breakfast and lunch, we estimate separate effects on participation rates for breakfast and lunch.

62.5 might lose net revenues, because they might have previously set the full price lunch (or breakfast) above the federal subsidy for free meals (which was $3.40 per lunch in 2019). If, for any of the above reasons, lost user fee revenues are greater than additional revenues from federal reimbursements, some worry districts will fill these gaps by reallocating funds previously used for classroom instruction.

There could, however, be unintended consequences of CEP's reimbursements. First, school districts may respond to the more generous increases in federal reimbursement revenues by reducing local or state support (as Gordon (2004) found that increases in federal Title I funding crowded out state and local revenues). Second, the switch to direct certification required under CEP may undercount the share of economically disadvantaged students in the district since direct certification identifies only those eligible for free lunch while missing those that would have been eligible for reduced-price meals. Further, direct certification's reliance on SNAP and TANF data may undercut a district's ability to count economically disadvantaged undocumented immigrants.

Districts receive Title I revenues based on Census poverty data; therefore, we have little reason to believe school adoption of CEP and the ensuing changes to counting economically disadvantaged students would affect the total Title I funds districts receive. However, Title I funding is distributed to schools based on school reports of students in poverty. If, post-CEP, CEP schools use direct certification instead of traditional meal forms to count the share of students who are economically disadvantaged, these schools may undercount their share of economically disadvantaged students. Moreover, if the manner in which economically disadvantaged students are accounted for is inconsistent between CEP and nonCEP schools within a district, some worry that CEP schools will not receive the appropriate Title I funds from the district. Unfortunately, school-level Title I funding data is unavailable. However, we have no reason to believe CEP would affect Title I funds received at the district-level and test this hypothesis by estimating the impact of CEP on district-level Title I revenues.

Urbanicity is likely to influence the impact of CEP on fiscal and weight outcomes. Rural districts have higher overall participation rates, fewer certified poor students, higher participation rates among full-price students, and fewer alternatives to school food. Therefore, rural districts may experience greater reductions in local school food revenue, as well as greater reductions in student obesity. Moreover, food preparation costs likely vary with labor costs, which are typically higher in urban areas. Thus, food expenditures are likely to be greater in urban area districts. Weight outcomes will, again, vary based on the nutritional quality of alternatives to school meals, which may very well vary between urban settings (where students have ready access to commercial vendors like restaurants) and rural settings (where these options may be far away).

Lastly, effect sizes likely depend on the extent to which CEP is implemented across schools within a district. While some districts implement district wide, affecting all students, others selectively implement CEP in only some of their schools. We expect larger effects in districts that implement CEP districtwide, compared to those that selectively implement CEP.

V. New York State and CEP

As shown in Figure 6, CEP became available in NYS in 2013 and expanded rapidly across the state. By 2018, 97 of the 698 districts in NYS had at least one CEP school (Figure 7), and as of 2019, over 90 percent of eligible NYS schools offered UFM under CEP (FRAC, 2019). Not only did CEP expand across the state, Figure 8 demonstrates how implementation spreads within NYS districts. NYS districts implement CEP in one of three ways. Some districts implement CEP districtwide, in which all schools in the district adopt CEP in the same year. Other districts selectively implement CEP in some but not all schools within the district. These districts often target schools serving primary grades where school food participation rates are already relatively high compared to secondary grades. Still, other districts begin with selective implementation and gradually adopt CEP districtwide over time.

NYS districts from all urbanicities – metro, town, and rural – adopt CEP. Rural districts serve fewer students and therefore have fewer schools. Consequently, rural districts are more likely to implement CEP districtwide. It is likely that districts in which more students are exposed to CEP experience larger impacts, whereas districts that selectively implement CEP will display attenuated effects. For example, we anticipate smaller district-level effects in districts that opt for selective implementation (e.g., 50 percent of its students) compared to districts with districtwide implementation (100 percent of its students).

VI. Data, Measures, and Samples

Data

We use longitudinal, district- and school-level data from the NYS Education Department (NYSED) spanning 2010-2018. These data include enrollment by grade, attendance rates, student characteristics such as percent of students with disabilities (*SWD*), English language learners (*ELL*), free lunch certified eligible (*FL*), and students by race/ethnicity (*black*, *white*, *Hispanic*, or *Asian/Other*).[5] We link this panel to new school meal data provided by the

[5] Students with disabilities data are unavailable at the school level.

NYSED's Child Nutrition Knowledge Center, including year of each district's (school's) CEP adoption and the number of breakfasts and lunches served by school and year. [6]

We match these data to new, biannual, district-level measures of student obesity surveillance data from the NYS Department of Health (NYSDOH) Student Weight Status Category Reporting System (SWSCRS). Maintained by NYSDOH's Center for Community Health, Division of Chronic Disease Prevention, SWSCRS was created to support state and local efforts to monitor long-term trends in childhood obesity in NYS school districts, excluding NYC (Dwicaksono et al, 2018). These weight outcome measures follow the Centers for Disease Control's guidelines and track the proportion of students who are overweight (BMI exceeding 85^{th} percentile for the same age and sex nationally) and obese (BMI exceeding 95^{th} percentile nationally). SWSCRS reports the proportions of overweight and obese students, aggregated by school district, based on schools' reports on student counts in each weight status category by grade group and sex (Dwicaksono et al, 2018). Since 2010, districts report biannual BMI measures based on mandatory student health forms for selected grades (i.e., "primary:" Pre-Kindergarten, Kindergarten, 2nd, and 4th, and "secondary:" 7th and 10th).[7]

Finally, we link this to district financial data from the Common Core of Data Financial (F33) surveys. The F33 surveys include local, state, and federal school food revenues, personnel and total school food expenditures, instructional expenditures, Title I revenues, and NCES

[6] The NYSED's Child Nutrition Knowledge Center data includes public schools, nonpublic schools, schools that opened/closed, and childcare centers. We use CEP schools that match SRC school data, including 2,890 NYS public schools in 97 public school districts.

[7] Students' health forms are completed by a physician and then submitted to the school. In the absence of submitted health forms, the school nurse completes it. The school nurse then tallies counts of students overweight and obese by grade (i.e. primary and secondary grades) and sends the information to the district office. The district office, using a tally system, counts the share of students who are obese and/or overweight.

urbanicity classifications. These classifications use Census definitions to divide districts into four categories: city, suburban, town, and rural.[8]

Measures

Our binary treatment indicator, *CEP*, takes a value of one if any school within the district offers UFM through CEP. In our analyses that use school level data, *CEP* equals one if the school offers UFM through CEP. A continuous measure of treatment, *PCT_CEP*, is the percentage of students in the district enrolled in a school offering UFM through CEP. This variable captures the degree of CEP implementation – from selective to districtwide – within a district. We define *Districtwide* which takes a value of 1 if the district has CEP in every school (100 percent implementation) and 0 if the implementation is selective, that is, *PCT_CEP* is less than 100.

District characteristics include the percentage of students who are SWD, ELL, FL, black, Hispanic, and Asian/other.[9] We create three indicator variables capturing district urbanicity as *Metro* (cities and suburban districts), *Town*, or *Rural* based upon NCES district locale designations. We combine cities and towns due to similarities between the two in our sample.[10]

[8] "Urbanized area," have populations of 50,000 or more, and "urbanized clusters," have populations between 5,000 and 50,000. City school districts are located inside both an urbanized area and a principal city. Suburb school districts are located inside an urbanized area, but outside of a principal city. Town school districts are located inside urban clusters, and Rural school districts are located outside of urban clusters.

[9] We use the share of students certified for free meals and not the share of students certified for reduced-price meals. Upon CEP adoption, all students receive free meals, eliminating the incentive for reduced-price students to turn in lunch forms. Indeed, when we estimate the effect of CEP adoption on the percent of free lunch students and reduced-price lunch students separately, we find no effect on the percent of free lunch students and a negative and statistically significant effect on the share of reduced-price lunch students – making the share of reduced-price students endogenous to CEP adoption.

[10] The poor suburbs near a city are often quite similar to the city itself and CEP eligible districts have high concentrations of poor children by design. These districts are observationally similar.

School breakfast (lunch) participation, *Bfast* (*Lunch)*, is measured as the total number of breakfasts (lunches) served divided by enrollment and the 183 school days in the year.[11] This captures the average share of days a student participates in school breakfast (lunch). *Attd Rate* is the district or school attendance rate.

We have two weight outcomes - the percentage of students that are overweight (*%Overwgt*) and the percentage that are obese (*%Obese*) – measured at both the district level and separately for primary and secondary grades. There are two measurement challenges to using the NYSDOH SWSCRS weight outcome data. First, the measures are collected in September of each year, so that the outcomes are more akin to end of year measures for the prior academic year than for the ensuing school year. Thus, we link the treatment status for t-1 to the weight outcomes measured in year t.[12] Second, obesity and overweight rates are measured biannually – half of districts each year – rather than annually; further, we do not know the district-specific reporting year. We proceed by assigning weight outcomes to the first year of each two-year cycle and explore the sensitivity of our results to alternative assumptions described below.[13]

Our fiscal outcomes include those related to revenues from school food services (*LocalRev*, *StateRev*, *FederalRev*, and *TotalFoodRev*), expenditures on school food services (*TotalFoodExp* and *PersonnelExp*), and instructional expenditures (*InstSalaries, InstBenefits*, and *InstTotal*). We calculate district revenues and expenditures per pupil (or per meals served),

[11] Enrollment includes total pre-kindergarten, K-12, and ungraded enrollment for each district or school.
[12] Student characteristics in year t reflect the characteristics of students in the academic year in which weight measures were taken, as opposed to t-1, which reflects the characteristics of the student population at the time of treatment.
[13] We assign student weight measures to the second of each two-year cycle as a robustness check and, as expected, find no effects. Results available upon request.

dividing total revenues earned or expenditures incurred by total district enrollment (or total meals served).[14]

District Panel

Our analyses rely upon two data sets: (1) a district panel, which is our primary analytic sample to assess impacts of CEP on district fiscal and student weight outcomes, and (2) a school panel, which we use to explore the mechanisms, namely school meals participation and attendance. Our district panel includes data on school district characteristics, finances, and school food utilization and policy (CEP adoption).

We restrict our district panel to 93 “Ever CEP” independent districts that adopt CEP in at least one school between 2013 and 2018. This excludes dependent school districts, NYC and the “Big 4” city districts (Buffalo, Rochester, Syracuse and Yonkers), because they operate quite differently than other districts and because they are disproportionately poor, non-white, and large.[15] The resulting analytic sample has 740 observations over 8 years.[16] As shown in Table 21, students in Never CEP districts are less likely to be FL, white, overweight, or obese than students in our analytic sample. Moreover, Never CEP districts earn less in school food revenues and accrue fewer expenditures per pupil than districts in our analytic sample.

Prior to the implementation of CEP, overweight and obesity are common in our sample districts: roughly two in five students were overweight and one in five obese. An average of

[14] All dollar amounts are adjusted for inflation using CPI-Urban to 2017 dollars.
[15] The vast majority of NYS school districts are independent special-purpose governments, whereas dependent school districts are controlled by state and local governments and are fiscally dependent, meaning they are not independent property tax levying units. The average “Big 4” district is larger (by an order of magnitude), disproportionately poor, non-white, and receives less local and more federal school food revenue than districts in our analytic sample.
[16] The analytic sample excludes charter schools, NYS Boards of Cooperative Educational Services (BOCES), four districts that consolidated in 2014, and one special education district as they do not reflect the typical, NYS district.

roughly two thirds participate in school lunch and one quarter in school breakfast. Attendance is high with an average attendance rate of 94 percent. As for finances, these districts spend an average of $467 per pupil for school food but only earn $405 per pupil in total school food revenues – resulting an almost $60 per pupil deficit in the absence of CEP.

Among districts in our analytic sample, metro districts are larger (5,424 students), poorer (53.4 percent FL), and less white (47.5 percent) compared to town (2,163, 46.8 percent, and 74.8 percent, respectively) and rural (1,005, 42.6 percent, and 93.1percent, respectively) districts. Rural districts have fewer free lunch certified students, but higher participation (28.6 percent in breakfast; 66.8 percent in lunch) and attendance (94.8 percent) rates than metro (23.8 percent, 60.0 percent, and 92.8 percent, respectively) and town (24.2 percent, 61.9 percent, and 93.8 percent, respectively) districts. Rural districts also earn the most local food revenue ($155.83 per pupil) and spend the most on food services ($510.85 per pupil) compared to metro ($85.27 and $420.50, respectively) and town ($116.53 and $457.11, respectively) districts.[17] Furthermore, the average school food deficit (total food revenues per pupil minus expenditures per pupil) in metro districts is about half of that in town and rural districts ($35.31 versus $69.20 or $73.26).

School Panel

We use panel data on school characteristics, attendance, and school food utilization and policy (CEP adoption) to probe the underlying mechanisms: meal participation and attendance rates.[18] Our school sample includes schools that adopt CEP between 2013 and 2018 and includes

[17] Mechanically, this could mean they charge and spend more on a per meal basis, or that a higher share of students partakes in the school meals programs or a combination of the two.

[18] Student weight data is unavailable at the school level. Meals served data is available at the school level and is aggregated to the district level for district analysis. There are 27 schools in 10 districts in our Ever CEP school sample that report meals served under two different meals programs (CEP and traditional meals programs), which could occur for a number of reasons, including instances in which CEP is offered to some grades and not others, when the program is added mid-year, or other processing or administrative reasons. We remove these schools from our analysis.

321 continuously open schools in 87 districts.[19] We assign each school to one of the following mutually exclusive grade levels (1) primary (enrolls 10 or more students in either 2nd or 4th grade) or (2) secondary (enrolls 10 or more students in 7th or 10th grade).[20] Since many districts have more than one CEP school, the school panel has a larger number of observations than the district panel, potentially increasing power. Further, school level data allow us to more precisely identify the schools (and students) who receive the CEP treatment – potentially improving the precision of our estimates.

VII. Empirical Strategy

We exploit the staggered adoption of CEP over time to estimate the effect of CEP on student weight outcomes, consequences for district revenues and expenditures related to school food programs, and the underlying mechanisms such as meal participation and attendance. We use a district fixed effects, difference-in-differences specification linking outcomes to CEP status and a set of time-varying district characteristics.

Mechanisms

Before turning to estimating impacts on obesity and revenues, we examine the effect of CEP on participation. Notice there may differences in the participation response for breakfast and lunch, and there are differences in the reimbursement rates for those meals. Thus, we examine

[19] We also exclude schools in four districts that consolidated in 2014 and schools in the special education district. We exclude 26 Ever CEP schools that are not continuously open and 34 schools in 24 districts with implausibly high meal participation rates (see footnote 18). Furthermore, we exclude schools in three districts whose meals served data is only available at the district level. Of the "missing" six districts in the school panel (87 versus 93): four districts have only one CEP school and that school in each district has unreliable participation rates, three districts' meal participation data is only available at the district level, and one of these three districts has only one CEP school, which is not continuously open. We find consistent results when we restrict the district analysis to districts that are observed in the school-level panel.

[20] Four "Elementary-Middle" schools and eight "K-12" schools are not included in these analyses.

breakfast and lunch participation separately. In a different vein, both weight outcomes and spending patterns may depend upon student attendance, which we also examine before turning to impact estimates. We begin by estimating the effect of CEP on breakfast and lunch participation and attendance using the district panel as:

$$Y_{dt} = \beta_0 + \beta_1 CEP_{dt} + \mathbf{X'_{dt}}\beta_2 + \gamma_t + \mu_d + \varepsilon_{dt} \quad (1)$$

where Y_{dt} is a vector of outcomes including *Bfast*, *Lunch*, and *Attd Rate* for district d, in year t. X'_{dt} is a vector of district characteristics, including *SWD, ELL, FL, black, Hispanic*, or *Asian/Other*. γ_t and μ_d are year and district fixed effects. β_1 reflects the effect of CEP on meal participation and attendance. All models are weighted by enrollment and we use robust standard errors clustered by district. We estimate the same model using school-level data and school (rather than district) fixed effects.

We then re-estimate the models using an event study specification, substituting a set of indicator variables capturing the number of years prior to (or following) the adoption of CEP in the district for *CEP*. That is, we use *CEP YEAR*, a vector of variables that capture the time between the current academic year (*t*) and the first year a district (or school) offers CEP.

$$Y_{dt} = \beta_0 + \boldsymbol{CEP\ Year'}_{dt}\beta_1 + \mathbf{X'_{dt}}\beta_2 + \gamma_t + \mu_d + \varepsilon_{dt} \quad (2)$$

These models will shed light on any pre-trends in attendance or participation in school food prior to the adoption of CEP and/or the evolution of both following adoption.

Obesity Impacts

Our weight outcomes models are similar to our baseline models. We estimate the following model:

$$Y_{dt} = \beta_0 + \beta_1 CEP_{dt-1} + \mathbf{X'_{dt}}\beta_2 + \gamma_t + \mu_d + \varepsilon_{dt} \quad (3)$$

where Y_{dt} is a vector of variables reflecting weight outcomes for district d, in year t, including *%Overwgt* and *%Obese* across all grades in a district, as well as by primary and secondary grades, separately. CEP_{dt-1} takes a value of 1 if district d has CEP in year t-1. For the reasons discussed in the data section, our data set includes observations for academic years 2011, 2013, 2015, and 2017 only for these models (that is, the odd years only). We cluster standard errors by district and use analytic weights for the number of students enrolled in measured grades in the district.[21] Our coefficient of interest, β_1, reflects the impact of CEP on weight outcomes.

Fiscal Impacts

We then estimate the effect of CEP on local, state, federal, and total school food revenues, personnel and total school food expenditures, and instructional expenditures (salaries, benefits, and total) per pupil, as well as per meal served at the district level, using:

$$Y_{dt} = \beta_0 + \beta_1 CEP_{dt} + \mathbf{X'_{dt}}\beta_2 + \gamma_t + \mu_d + \varepsilon_{dt} \qquad (4)$$

where Y_{dt} reflects the vector of fiscal outcomes, and β_1 equals the effect of CEP on each fiscal outcome. Again, we also estimate an event-study specification, similar to Equation 2.

Exploring Heterogeneity in Context

To explore potential heterogeneity in effects by urbanicity, we introduce interactions between CEP_{dt} and our urbanicity indicators (*Metro*, *Town*, and *Rural*). Finally, we explore how effects vary with the extent of implementation by replacing CEP_{dt} with PCT_CEP_{dt}, the percentage of students in the district attending a CEP school, and $Districtwide_{dt}$, an indicator for districtwide implementation, capturing potential ceiling effects:

$$Y_{dt} = \beta_0 + \beta_1 PCT\_CEP_{dt} + \beta_2 Districtwide_{dt} + \mathbf{X'_{dt}}\beta_3 + \gamma_t + \mu_d + \varepsilon_{dt} \qquad (5)$$

[21] That is, enrollments in Pre-K, K, 2nd, and 4th grade for primary and 7th and 10th grade for secondary.

Here, β_1 provides estimates of the effect of changes in the extent of CEP implementation within a district on outcomes.

VIII. Results

Mechanisms

As shown in Table 22, we find CEP increases average district participation in breakfast and lunch by 7.72 and 6.58 percentage points, respectively. Breakfast effects are larger for primary schools, increasing breakfast in primary schools by 11.49 percentage points, more than double that of secondary schools – 4.66 percentage points. However, CEP increases *Lunch* in both primary and secondary schools by approximately 8.50 percentage points. In terms of changes over base participation rates, CEP increases *Bfast* and *Lunch* in primary schools by 33 and 12 percent, respectively, and 31 and 15 percent in secondary schools, respectively. We find no effects of CEP on attendance rates nor do we find differential effects by school level.

Turning to the event study results shown in Figure 9, we find no evidence of pretrends – that is, there are no statistically significant effects in years prior to CEP adoption – for any outcomes. District results in Panel A show that the effects of CEP on *Bfast* and *Lunch* increase over time. However, Panels B and C, estimated with the school-level panel, show relatively constant participation effects in the years post CEP adoption. This seemingly contradictory result would be consistent with within-district expansions of implementation from selective towards districtwide. As it turns out, two-thirds of districts begin with selective implementation and expand – some eventually to districtwide CEP. Moreover, Figure 8 shows the share of students exposed to CEP grows in the years following initial district implementation, expanding from about 70 percent of students in the year of CEP adoption to 90 percent of students two years later. While at first glance increases in participation in post-CEP years (Panel A) suggests students might become more comfortable with school meals over time, our other results suggest that the growing impacts on participation can be fully explained by the expansion of CEP to more schools and students within CEP districts.

Obesity Impacts

As shown in Table 23, we find CEP decreases the percentage of obese students in secondary grades by 1.83 percentage points. The effect is substantively meaningful: 23.5 percent of secondary students were obese in 2012, translating to a 7.8 percent decrease in the prevalence of obesity.[22] None of the other results are statistically significant. That is, we find no statistically significant effects for students in primary grades. Our results are consistent with the hypothesis that greater responsiveness to price changes and reliance on less healthy alternatives to school food among secondary school students will yield larger effects.

Fiscal Impacts

Of note, Ever CEP districts run deficits in their school food programs prior to CEP (in 2012), with mean deficits of $60 per pupil. (See Table 21; $404.85 and $466.90 of total food service revenues and expenditures, respectively.) As shown in Table 24 Panel A, CEP decreases local food revenues by an average of $23.90 per pupil (column 1), which would exacerbate deficits on its own. However, this loss in revenues is more than offset by the $72.96 per pupil increase in federal food revenues (column 3). Furthermore, we find no effect on state school food

[22] The structure of the health outcomes data allows for only four-year observations, preventing us from executing an event study design similar to what we later perform for meal participation and attendance rates.

revenues – suggesting that federal reimbursements for CEP do not crowd out state funding for school food.

While the above results show CEP increases total food revenues on average (by $51.76 as shown in column 4 of Table 24 Panel A), it is still possible that deficits are exacerbated by increased expenditures resulting from higher participation rates. However, total food expenditures increase by only $38.23 per pupil (column 6), which is less than the increase in total school food revenues per pupil. In fact, it appears that CEP closes the $60 per pupil school food deficit that existed in 2012 by approximately $14 per pupil, with no consequences for instructional expenditures (columns 7-9).

We then explore consequences on a per meal basis in Panel B of Table 24. CEP decreases total revenue per meal by 18 cents per meal (column 4) but decreases expenditures per meal even faster – by 25 cents per meal (column 6). Thus, our estimates suggest that increasing meals served helps close the food services fiscal deficit by about 7 cents per meal on average (at least in this range of increased participation). Decreases in local food revenues (20 cents per meal) drive the decrease in revenues per meals. Food expenditures on personnel (12 cents per meal) and non-personnel (25-12 cents per meal) both contribute to the decrease in expenditures per meals. These food expenditure decreases are consistent with increasing returns to scale – in which districts can provide more meals at a lower cost per meal – but might also reflect reductions in the quality of inputs (i.e., cheaper ingredients).

Turning to the event studies, we find no evidence of pre-trends prior to CEP adoption for financial outcomes; no point estimate is distinguishable from zero (Figure 10). Local food revenues are pretty stable in the years following CEP adoption, while other fiscal outcomes grow over time. Again, this could reflect expansions of implementation within CEP districts, with fiscal consequences growing with the share of students exposed to CEP over time (see Figure 8).

Exploring Heterogeneity in Context

Table 25 shows the effects by district urbanicity. We find metro and town districts respond similarly to CEP but impacts in rural districts are generally larger. Rural district students increase *Bfast* by almost twice as much as students in metro and town districts (column 1 of Table 25) but respond similarly for *Lunch*. As shown in columns 6 and 7, rural districts experience the largest decreases in prevalence of overweight and obesity in secondary grades (obesity effects in column 7 are insignificant). Once again, the effects on weight outcomes for primary school students are insignificant in all settings.

Table 26 shows the effect of CEP on district financial outcomes by urbanicity. We see the largest decline in local school food revenue in rural districts – where a greater share of students pays for school meals prior to CEP. At the same time, CEP increases personnel expenditures per pupil in rural districts, unlike metro and town districts, likely driven by increases in participation. In the absence of CEP, metro districts run school food deficits of about $35 per pupil, while town and rural districts run deficits around $70 per pupil (Table 21). While increases in expenditures are more than offset by revenue increases in metro and town districts, rural districts' school food deficit grows by roughly $30 per pupil (column 3 minus column 5). Again, we find no effects of CEP on instructional spending by urbanicity.

As shown in Table 27, we turn next to exploring the heterogeneity of the results across districts with different percentages of students exposed to CEP. We find a 10-percentage point increase in CEP implementation decreases the percent of overweight and obese students in secondary grades by 2.1 and 1.5 percentage points, respectively. While insignificant, point estimates for overweight secondary students in districts with districtwide implementation are larger and more negative. Effects in primary grades are again smaller and insignificant.

While our event study results provide no evidence of problematic pre-trends that would undermine a causal interpretation of our results, we investigate empirically the extent to which observables predict the timing of CEP adoption, which might undermine our confidence in the causal interpretation. Specifically, we explore whether the timing of CEP adoption is plausibly exogenous by examining whether a school or district's observable characteristics in year t predict CEP adoption in t+1. We restrict the sample to districts (schools) that do not have CEP in year t, using the following model:

$$CEP_{dt+1} = \beta_0 + \mathbf{X'_{dt}}\beta_1 + \gamma_t + \mu_d + \varepsilon_{dt} \qquad (6)$$

where $\mathbf{X'_{dt}}$ describes the previously defined district (school) characteristics and β_1 reflects whether district (school) characteristics predict CEP adoption in the following year. Significant coefficients would suggest timing of CEP adoption is nonrandom. Table 28 shows district and school level results in Columns 1 and 2, respectively. We find no evidence that district (school) characteristics predict timing of CEP adoption, bolstering confidence that the causal interpretation is warranted.

We also investigate the robustness of our findings in two sets of analyses to buttress the evidence for a causal interpretation. First, we re-estimate the effects with a sample that includes the "Big 4" city districts, which were excluded in our preferred specifications. The results, shown in Tables C1 through C3 of the appendix are either consistent or stronger than those from the preferred sample. Table C1 shows meal participation and attendance rate results are robust,

Table C2 shows slightly larger effects for overweight and obesity, and Table C3 panels A and B show, if anything, slightly larger effects revenues and expenditures.

Second, we re-estimate the models with different analytic weights, using unweighted models instead of those weighted by students.[23] The results, shown in Tables C4 through C6 of the appendix are consistent, with some effects even larger than those from the preferred sample. Table C4 shows meal participation and attendance rate results are robust, Table C5 shows slightly larger effects for overweight and obesity (though the effects on obesity are no longer significant), and Table C6 panels A and B show statistically indistinguishable or even slightly larger effects on revenues and expenditures, especially per meal.[24]

Other Outcomes

We explore three ancillary outcomes, Title I funding, proficiency rates in statewide English language arts (ELA) exams, and proficiency rates in statewide math exams.[25] The Title I results address any potential concerns of education administrators CEP will affect the amount of Title I revenues received by districts. The test results are intended to contribute to the growing knowledge on the effects of UFM on student academic performance (previously explored in Ruffini, forthcoming and Schwartz & Rothbart, 2020).

Title I funding is provided to schools with high shares of economically disadvantaged students. Some worry that an unintended consequence of switching to CEP (and increasing reliance on direct certification of ISP students) might be reductions in Title I funding for CEP

[23] As noted above, our main analyses use analytic weights for the number of students related to the outcome (e.g., models estimating impacts on lunch participation rates are weighted by total enrollment).
[24] We also examine the robustness to restricting the district panel to the 87 districts used in the school-level analyses. Results, available from the authors, are substantially unchanged.
[25] For ELA and math exams, we explore effects by grade for grades 3 through 8, so we actually estimate the effects on twelve testing outcomes.

schools measuring economically disadvantage using direct certification. We note, however, that districts must use the same method of counting the share of economically disadvantaged students for all schools in the district, including both CEP and non-CEP schools.[26] We test whether CEP adoption affects district Title I revenues and find it does not. (Results available upon request).

We briefly examine CEP's effect on district-level ELA and Math proficiency rates to contribute to the growing literature of its effects on academic outcomes. Using proficiency rates obtained from the NYSED Student Report Card data, we find CEP increases proficiency rates on the ELA exam by approximately 4 percentage points for 6th, 7th, and 8th graders (Results available upon request). We also find a 5-percentage point increase in Math proficiency rates among 8th grade students, but a 3-percentage point decrease among 3rd grade math students. The remaining point estimates in other grades are small and statistically indistinguishable from zero. That is, consistent with previous research, we find some evidence CEP improves academic achievement in middle school statewide; we find no evidence of these improvements in elementary schools.[27]

IX. Conclusion

School food advocates claim that expanding NSLP and SBP will lead to improved cognitive function and, ultimately, test scores for participating students. Their claims are bolstered by the recent evidence that Universal Free Meals (UFM) programs have, indeed, improved student academic and behavior outcomes. That said, critics worry that expanding such

[26] Districts that include both CEP and non-CEP schools can choose to use 1) direct certification times the 1.6 multiplier for CEP schools and free and reduced-price lunch forms for non-CEP schools, 2) direct certification numbers times the 1.6 multiplier for both CEP and non-CEP schools, or 3) direct certification numbers for both CEP and non-CEP schools without the 1.6 multiplier (CRS, 2016).

[27] This period saw a large increase in students opting out of the standardized testing regime as well as changes in NYS standards for both the ELA and Math exams. If these changes affect early (or late) adopting districts more than late (or early) adopters, then the estimates for effects on achievement would have to be interpreted with caution.

programs will exacerbate weight problems among school children (i.e., obesity and overweight) and place additional financial burdens on school districts. There is, however, little evidence on these unintended and potentially negative effects of the large – and growing – expansion of UFM under the Community Eligibility Provision (CEP). The rapid expansion of CEP to a majority of eligible U.S. schools as of 2019, makes empirical evidence on these effects critical to policymakers as they consider how to best manage this program going forward. This paper aims to provide credibly causal estimates of the effect of CEP on student weight outcomes and district fiscal consequences, as well as the key drivers of such effects, including school meal participation and attendance, by exploiting the staggered adoption of CEP throughout NYS districts and schools.

We find CEP increases student participation in school breakfast and lunch with no effect on attendance rates. Students in primary grades increase participation in breakfast at almost twice the rate of students in secondary grades, however all students increase participation in school lunch by approximately 8.5 percentage points. These increases in participation begin post CEP implementation and grow as districts gradually move from selective to districtwide implementation.

We find no evidence of deleterious effects of CEP on student weight. We find no effects on weight outcomes for primary students, despite large increases in school meals participation in those grades. Moreover, we find CEP reduces obesity in secondary grades with largely negative, albeit statistically insignificant, point estimates on other weight outcomes. The differences in effects by grade level may reflect biological differences between older and younger children or that the food eaten by secondary school students in the absence of CEP is less healthy than that among primary school students. Previous research also suggests that the obesity of secondary school students is more sensitive to the food environment than primary school students, perhaps due to differences in food consumption patterns (for example, Dwicaksono, et al. 2018 find this pattern in New York State school districts).

We further find that CEP reduces local food revenues (i.e., loss of meal fees) while increasing federal food revenues (i.e., reimbursements) and total food expenditures per pupil. By offering free meals to all students, CEP districts lose local school food revenue previously collected from students paying for full or reduced prices for meals. However, these costs are offset by the federal government, which pays districts for the number of meals equal to 1.6 times the district's ISP. CEP reduces both revenues and expenditures per meal – consistent with producing more meals for less. Some worry that districts struggling to cover gaps in revenues and expenditures may dip into instructional expenditures. We find no evidence of CEP reducing funds meant for the classroom.

There is widespread concern over performance and financial viability of rural districts. We find effects of CEP vary depending on district urbanicity, perhaps due to differences in the types of students served, baseline participation rates, availability of alternatives, and/or cost of living. Rural districts appear to be more responsive to CEP and experience larger impacts for almost all significant outcomes. This is likely because rural districts are more prone to implement CEP districtwide, have higher baseline participation rates, and have fewer alternatives to school food. Rural districts also experience larger increases in school food expenditures compared to town and metro districts, particularly among personnel expenditures. This could occur for a host of reasons, including higher food costs in rural areas, less federal reimbursements, or differences in cafeteria capacity across districts in different urbanicities. However, non-personnel costs per meal in 2012, prior to CEP, are smallest for rural districts

($1.45) and largest for town districts ($1.62). Therefore, increases in expenditures are likely not due to differential food costs. Moreover, it is possible that metro and town districts have slack capacity in their cafeterias, whereas rural districts do not, explaining why rural districts experience increases in personnel expenditures. Therefore, the total increases in school food expenditures are driven by expansions in cafeteria capacity, less federal reimbursements, or a combination of both.

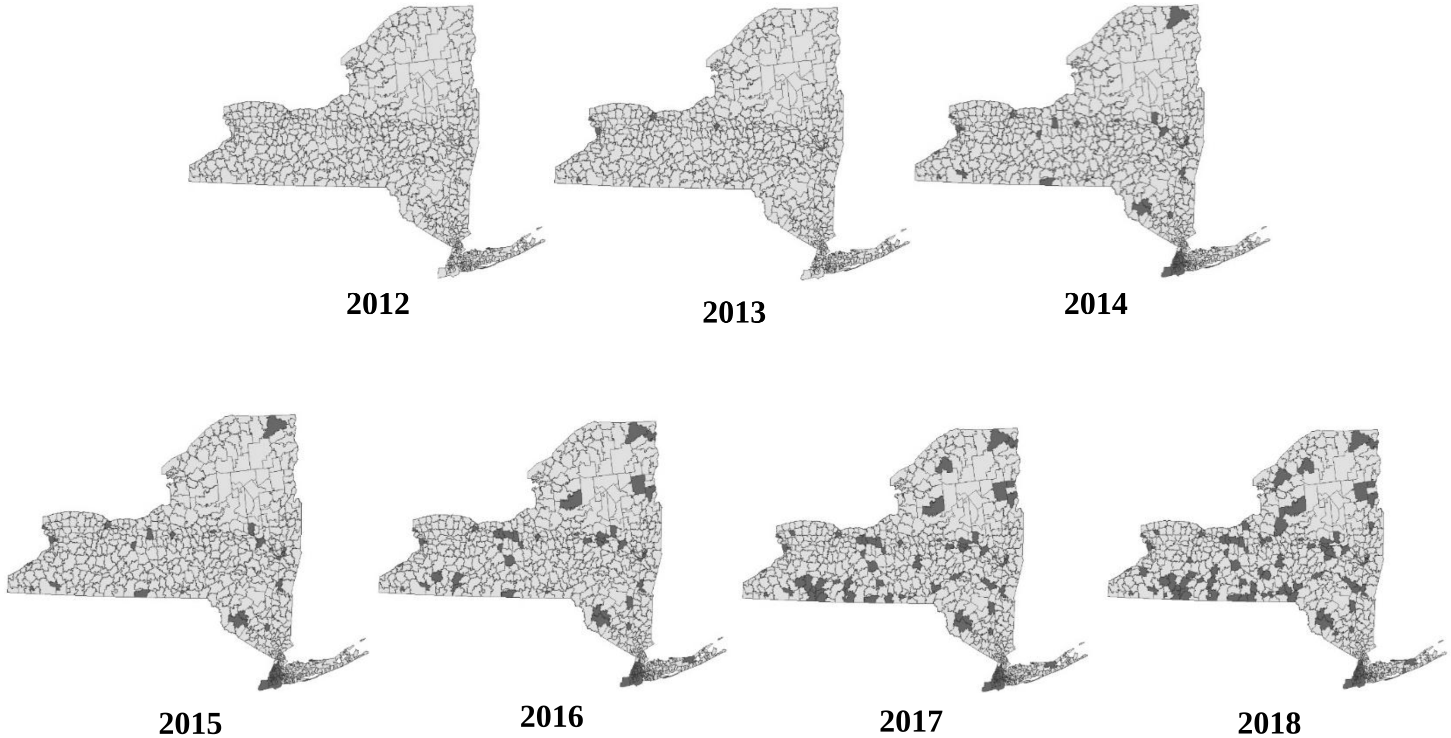
Likely for these same reasons listed above, however, CEP increases the size of school food program deficits in rural districts by $30 per pupil. Conversely, CEP helps close school food program deficits in metropolitan and town districts. This may lead to increased concerns over the fiscal condition of rural districts, who must find a way to cover these gaps. States may want to consider providing financial assistance to CEP-adopting rural districts to help them address the increased financial burden. Finally, we examine heterogeneous effects of CEP by implementation patterns and find wider implementation leads to more substantial effects.

This paper provides evidence that will likely assuage critics' worries, demonstrating that not only does UFM via CEP have no deleterious effects on student weight, it actually improves weight outcomes for students in secondary grades while increasing participation rates, and, on average, covering potential CEP-induced gaps in school food revenues and expenditures. These effects vary by level of implementation and urbanicity – something for those making the decisions to adopt such policies to consider given their particular context.

X. Acknowledgements

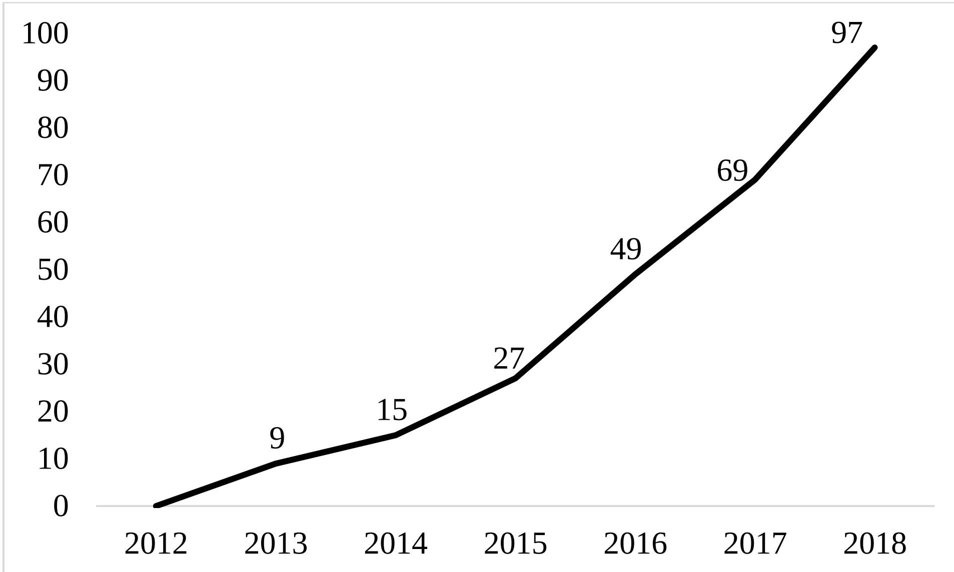
This work is generously supported by The Tufts/UConn RIDGE Program and the National Institutes of Health, Eunice Kennedy Shriver National Institute of Child Health and Human Development (grant 5R01DK097347-02). We thank the NYS Department of Education Child Nutrition Knowledge Center for providing data, especially Todd Bradshaw We also thank Meryle Weinstein, Joanna Bailey, and Henry Dyer Cruzado for their invaluable research assistance. The opinions expressed are those of the authors and do not represent views of the U.S. Department of Agriculture, National Institutes of Health, or NYS Department of Education.

Figure 6: CEP Expands Rapidly Across New York State



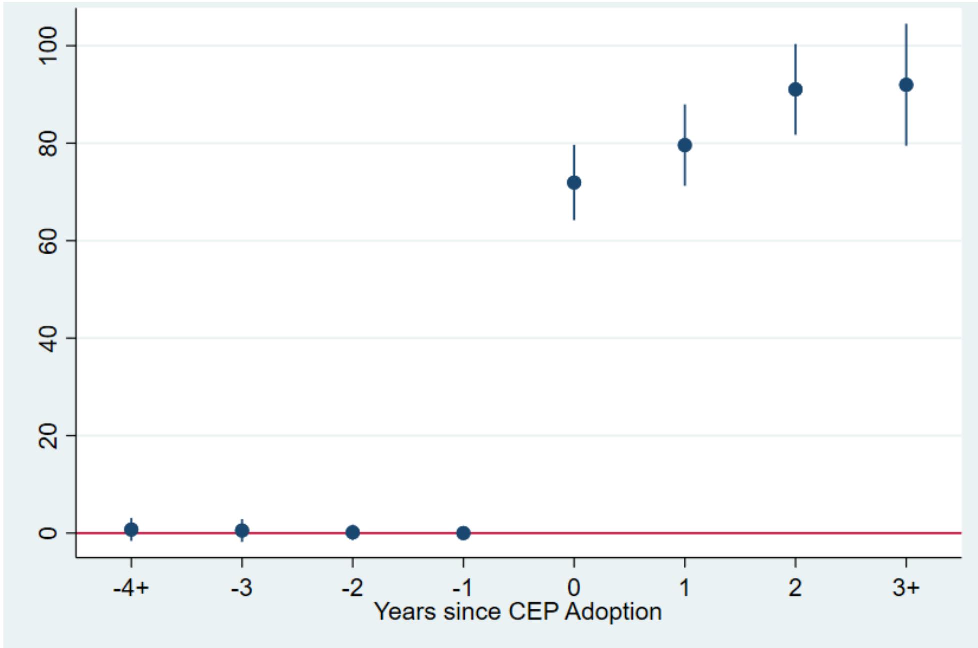
Note: Includes all 97 Ever CEP districts as well as NYC districts.

Figure 7: Number of Districts with at Least 1 CEP School



Notes: Includes all 97 Ever CEP districts but excludes NYC districts.

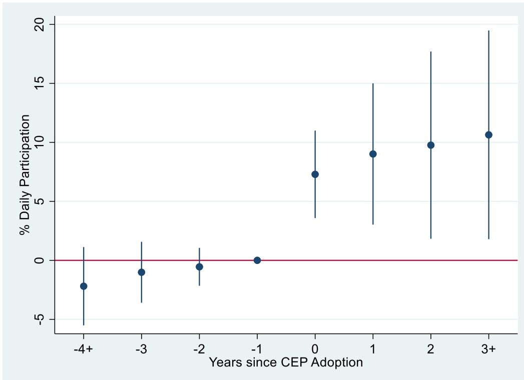
Figure 8: Percent of Students Exposed to CEP by CEP Adoption Year, 2010-2017

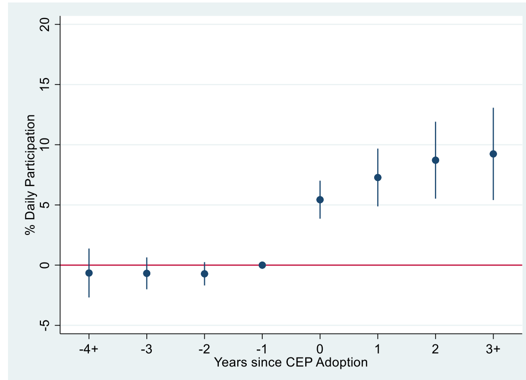


Notes: Figure displays point estimates and 95 percent confidence intervals derived from an event study of Ever CEP districts from 2010 to 2017. Sample excludes NYC, "Big 4" districts (Buffalo, Rochester, Syracuse, and Yonkers), four districts that consolidated in 2014, and one district with incomplete data. Model controls for percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Zero (0) indicates the first year of CEP adoption. Negative 1 (-1) is the omitted reference category. Models use districts with 4 or more years of pre-adoption data to identify "-4+" and 3 or more years of post-adoption data to identify "3+."

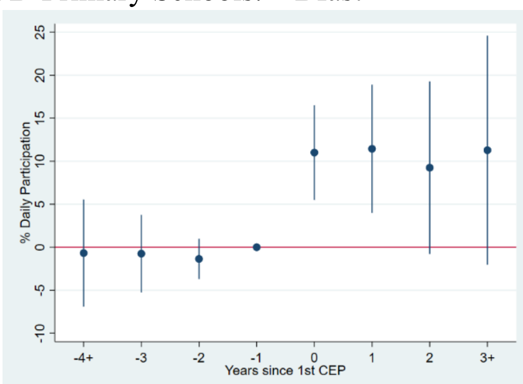
Figure 9: Event Study Depicting Estimated Impacts of CEP on Meal Participation, 2010-2017

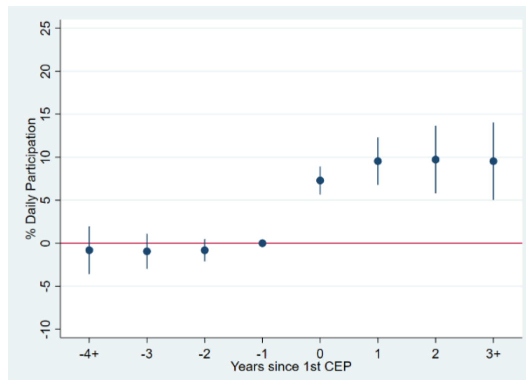
Panel A – District: Bfast Lunch



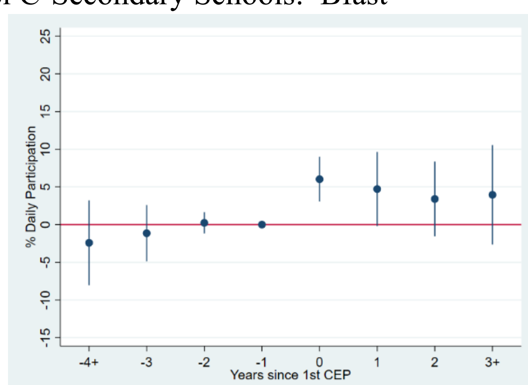


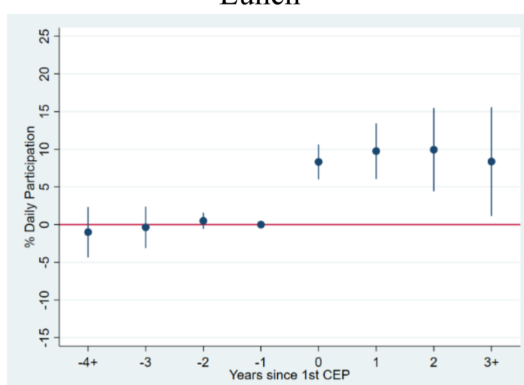
Panel B-Primary Schools: Bfast Lunch





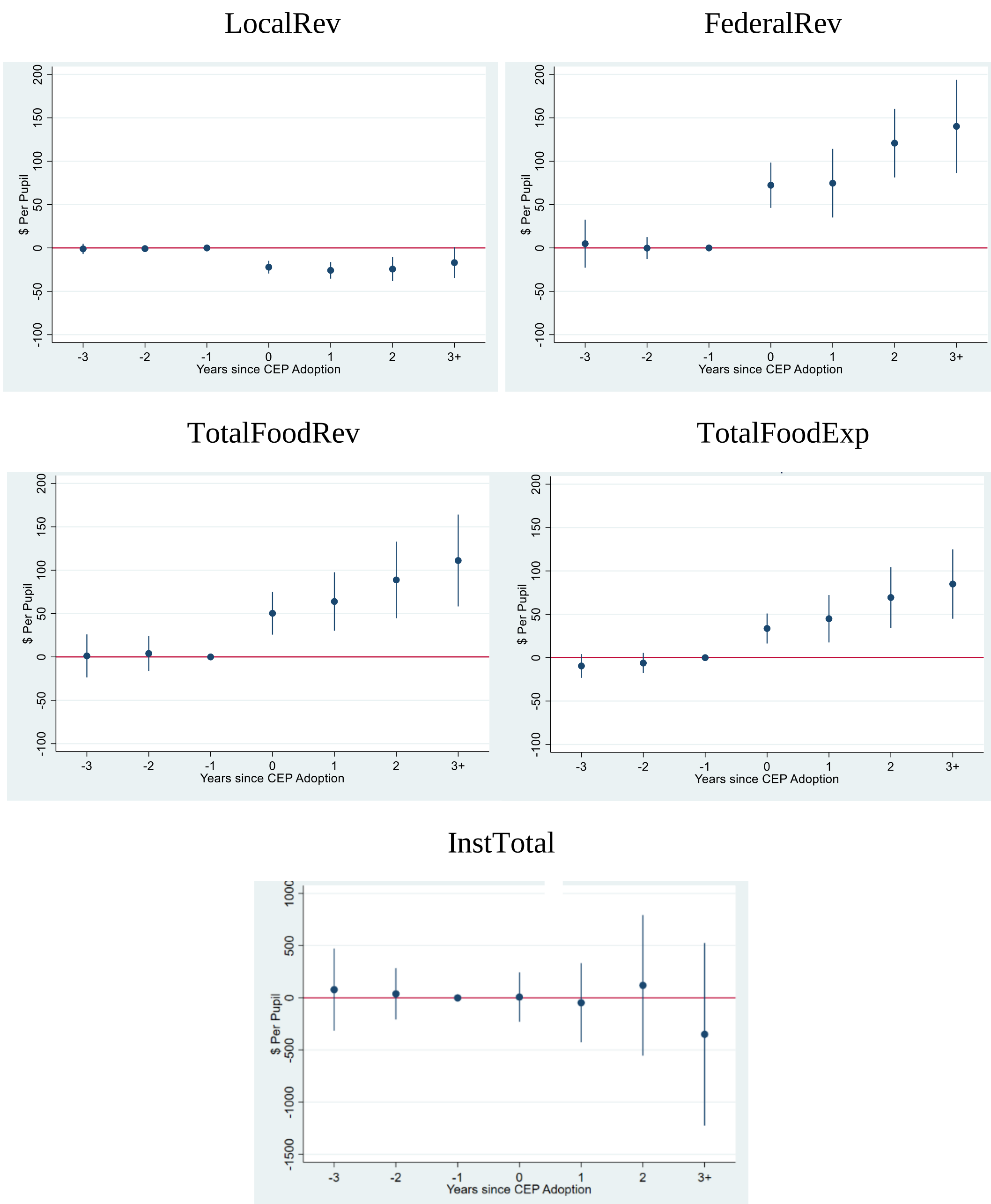
Panel C-Secondary Schools: Bfast Lunch





Notes: Figures display point estimates and 95 percent confidence intervals derived from an event study of Ever CEP districts (Panel A) and schools (Panel B and C) from 2010 to 2017 for meal participation outcomes and 2010 to 2016 for attendance outcome. Samples exclude NYC, "Big 4" districts (Buffalo, Rochester, Syracuse, & Yonkers) and four districts that consolidated in 2014. The school panel includes 198 primary and 93 secondary continuously open schools that adopt CEP between 2013 and 2018, excluding 34 schools in 24 districts with implausibly high meal participation rates, 4 Elementary-Middle schools and 8 K-12 schools. All models control for percent black, Hispanic, Asian/other, English language learners, students with disabilities (unavailable in school-level models), and free lunch students, district (school) fixed effects, and year fixed effects. Estimates weighted by enrollment. Zero (0) indicates the first year of CEP adoption. Negative 1 (-1) is the reference year. Models use districts with 4 or more years of pre-adoption data to identify "-4+" and 3 or more years of post-adoption data to identify "3+."

Figure 10: Event Study Depicting Estimated Impacts of CEP on Revenues and Expenditures per Pupil, 2010-2017



Notes: Sample period covers 2010-2017 and includes Ever CEP districts. Sample excludes NYC, "Big 4" districts (Buffalo, Rochester, Syracuse, & Yonkers), four districts that consolidated in 2014, and one district with incomplete data. All models control for percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Estimates weighted by enrollment. Revenue and expenditures data are in 2017 dollars per pupil. Districts missing data in select years: 1 local food revenue, 6 federal food revenue, and 4 personnel food expenditures. Estimates weighted by enrollment. Zero (0) indicates the first year of CEP adoption. Negative 1 (-1) is the omitted reference category. Models use districts with 4 or more years of pre-adoption data to identify "-4+" and 3 or more years of post-adoption data to identify "3+." "-4+" estimates suppressed.

Table 21: Descriptive Statistics by CEP Status, 2012

		Ever CEP				
	Never CEP	Big 4	Analytic Sample	Metro	Town	Rural
District Characteristics						
Demographics (%)						
FL	23.6	74.0	47.2	53.4	46.8	42.6
White	84.2	19.0	72.0	47.5	74.8	93.1
Black	4.0	46.8	12.5	27.9	8.2	2.1
Hispanic	7.1	26.8	11.3	18.9	11.5	2.2
Asian/Other	4.6	7.5	4.0	5.5	5.3	2.6
ELL	1.8	11.8	3.6	7.2	2.6	0.2
SWD	12.1	16.2	13.8	13.8	13.2	14.3
Public School Enrollment	2,209	26,295	2,769	5,424	2,163	1,005
Mean Number Schools	3.9	47.5	5.2	9.0	4.6	2.7
Pre-Treatment Outcomes						
Weight Outcomes (%)						
Overweight	33.6	37.1	38.7	39.6	37.7	38.6
Obese	17.7	20.6	21.4	22.8	20.0	21.1
Mechanisms (%)						
Breakfast Participation	14.5	37.2	25.8	23.8	24.2	28.6
Lunch Participation	47.9	57.7	63.2	60.0	61.9	66.8
Attendance Rate	95.3	89.75	93.9	92.8	93.8	94.8
Revenue per pupil from food (2017$)						
Local	179.74	40.81	122.62	85.27	116.53	155.83
State	19.19	16.20	25.41	29.01	15.16	24.36
Federal	150.61	403.41	268.36	282.76	267.36	264.55
Total	328.57	460.42	404.85	385.19	387.91	437.59
Expenditures per pupil on food (2017$)						
Personnel	200.35	251.18	231.70	196.88	211.66	269.56
Total	378.47	500.19	466.90	420.50	457.11	510.85
Number Districts	573	4	93	32	24	37

Notes: Analytic Sample includes 93 districts that adopt CEP in at least one school between 2013-2018, and excludes NYC, "Big 4" districts (Buffalo, Rochester, Syracuse, Yonkers), four districts that consolidated in 2014, and one district with incomplete data. Revenue and expenditures data are in 2017 dollars.

Table 22: Estimated Impacts of CEP on Meal Participation and Attendance, 2010-2017

	District			Primary Schools			Secondary Schools		
	Bfast	Lunch	Attd Rate	Bfast	Lunch	Attd Rate	Bfast	Lunch	Attd Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CEP	7.715***	6.584***	-0.348	11.49***	8.511***	-0.424	4.655**	8.409***	-1.779
	(2.282)	(0.911)	(0.237)	(3.145)	(0.977)	(0.877)	(1.760)	(1.520)	(1.433)
2012 Means	25.8	63.2	93.9	34.7	70.8	93.9	14.9	57.4	92.8
District Char.	Y	Y	Y	Y	Y	Y	Y	Y	Y
School FE	N	N	N	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	N	N	N	N	N	N
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	744	744	651	1,584	1,584	1,386	744	744	651
No Schools	-	-	-	198	198	198	93	93	93
No Districts	93	93	93	75	75	75	50	50	50
R-squared	0.753	0.897	0.734	0.731	0.868	0.180	0.610	0.910	0.349

Notes: Robust standard errors in parentheses clustered by district (*p<.10; **p<.05; ***p<.01). Sample period covers 2010 to 2017 for meal participation outcomes and 2010 to 2016 for attendance outcome and includes Ever CEP districts (Columns 1-3) and schools (Columns 4-9). Both samples exclude NYC, “Big 4” districts (Buffalo, Rochester, Syracuse, & Yonkers) and four districts that consolidated in 2014. School panel sample includes 198 primary and 93 secondary continuously open schools that ever adopt CEP from 2013-2018 and excludes 34 schools in 24 districts with implausibly high meal participation rates, 4 Elementary-Middle schools and 8 K-12 schools. All models control for percent black, Hispanic, Asian/other, English language learners, students with disabilities (unavailable in school-level models), and free lunch students, district (school) fixed effects, and year fixed effects. Estimates weighted by enrollment.

Table 23: Estimated Impacts of CEP on Student Weight Outcomes, 2010-2017

	All Grades		Primary Grades		Secondary Grades	
	% Overwgt	% Obese	% Overwgt	% Obese	% Overwgt	% Obese
	(1)	(2)	(3)	(4)	(5)	(6)
CEP	0.030	-0.561	0.605	-0.047	-1.689	-1.831*
	(0.893)	(0.786)	(1.013)	(0.912)	(1.170)	(1.045)
2012 Means	38.7	21.4	37.7	20.7	40.9	23.5
District Char.	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	368	365	364	361	361	358
No. Districts	93	93	93	93	93	93
R-squared	0.729	0.723	0.741	0.726	0.597	0.573

Notes: Robust standard errors in parentheses clustered by district (*p<.10; **p<.05; ***p<.01). Sample period covers 2010-2017 and includes Ever CEP districts. Sample excludes NYC, "Big 4" districts (Buffalo, Rochester, Syracuse, & Yonkers), four districts that consolidated in 2014, and one district with incomplete data. Primary refers to grades K, 2, and 4, and Secondary refers to grades 7 and 10. Weight outcome data assigned to the beginning of the two-year reporting cycle using last year's treatment status. Estimates weighted by student enrollment in measured grades (K, 2, 4, 7, 10). All models control for a vector of district characteristics including percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Data is unavailable for districts with fewer than 5 students in a category. Therefore, the number of observations is inconsistent across outcomes.

Table 24: Estimated Impacts of CEP on Fiscal Outcomes, 2010-2017

Panel A: Per Pupil

	Food Revenue				Food Expenditures		Instructional Expenditures		
	Local	State	Federal	Total	Personnel	Total	Salaries	Benefits	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CEP	-23.90***	-2.11	72.96***	51.76***	7.46	38.23***	-104.95	-63.62	-44.13
	(3.75)	(4.62)	(12.25)	(12.19)	(5.91)	(9.19)	(81.61)	(54.60)	(145.10)
2012 Means	122.62	25.41	268.36	404.85	466.9	231.7	7,186.50	3,596.41	11,726.78
District Char.	Y	Y	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	743	744	733	744	727	744	744	744	744
No Districts	93	93	93	93	93	93	93	93	93
R-squared	0.93	0.67	0.87	0.82	0.95	0.90	0.96	0.95	0.96

Panel B: Per Meal

	Food Revenue				Food Expenditures	
	Local	State	Federal	Total	Personnel	Total
	(1)	(2)	(3)	(4)	(5)	(6)
CEP	-0.20***	-0.04	0.03	-0.18***	-0.12**	-0.25***
	(0.03)	(0.03)	(0.05)	(0.06)	(0.05)	(0.06)
2012 Means	0.79	0.16	1.70	2.58	1.47	2.97
District Char.	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	743	744	733	744	727	744
No Districts	93	93	93	93	93	93
R-squared	0.94	0.68	0.81	0.67	0.91	0.75

Notes: Sample period covers 2010-2017 and includes Ever CEP districts. Sample excludes NYC, "Big 4" districts (Buffalo, Rochester, Syracuse, & Yonkers), four districts that consolidated in 2014, and one district with incomplete data. All models control for percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Estimates weighted by enrollment. Revenue and expenditures data are in 2017 dollars. Panel A outcomes are revenues and expenditures per pupil, and Panel B outcomes are revenues and expenditures per meal served. Districts missing data in select years: 1 local food revenue, 6 federal food revenue, and 4 personnel food expenditures.

Table 25: Estimated Impacts of CEP on Mechanisms and Weight Outcomes by Urbanicity, 2010-2017

	All Grades			Primary Grades		Secondary Grades	
	Bfast	Lunch	Attd Rate	% Overwgt	% Obese	% Overwgt	% Obese
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CEP							
Metro	7.731***	6.406***	-0.335	0.018	-0.519	-0.986	-1.929
	(2.817)	(1.138)	(0.281)	(1.098)	(0.985)	(1.736)	(1.483)
Town	6.049**	6.813***	-0.611	2.162	1.661	-2.733	-0.356
	(2.579)	(1.271)	(0.442)	(2.214)	(1.807)	(2.664)	(1.547)
Rural	11.51***	7.347***	0.399	0.942	-0.830	-4.054*	-4.256
	(3.160)	(1.484)	(0.404)	(1.778)	(1.367)	(2.400)	(2.636)
District Char.	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Observations	744	744	651	364	361	361	358
No. Districts	93	93	93	93	93	93	93
R-squared	0.758	0.904	0.736	0.743	0.732	0.602	0.582

Notes: Sample period covers 2010-2017 and includes Ever CEP districts. Sample excludes NYC, "Big 4" districts (Buffalo, Rochester, Syracuse, & Yonkers), four districts that consolidated in 2014, and one district with incomplete data. All models control for a vector of district characteristics including percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Primary refers to grades K, 2, and 4, and Secondary refers to grades 7 and 10. Weight outcome data assigned to the beginning of the two-year reporting cycle using last year's treatment status. Models in Columns 1-3 weighted by enrollment. Models in Columns 4-7 weighted by student enrollment in measured grades (K, 2, 4, 7, 10).

Table 26: Estimated Impacts of CEP on Fiscal Outcomes by Urbanicity, 2010-2017

	Revenue			Expenditures		Instructional Expenditures		
	Local	Federal	Total	Personnel	Total	Salaries	Benefits	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CEP								
Metro	-14.01***	72.37***	56.39***	5.22	39.96***	-163.96	-81.02	-66.00
	(3.86)	(15.02)	(13.99)	(7.26)	(11.36)	(104.03)	(76.15)	(188.79)
Town	-39.38***	71.77***	52.83**	5.48	30.33**	25.88	-7.71	38.87
	(5.64)	(18.82)	(22.76)	(7.69)	(12.86)	(107.88)	(81.88)	(204.31)
Rural	-57.91***	79.83***	15.49	26.86**	44.12*	17.53	-68.13	-79.78
	(9.02)	(25.29)	(19.31)	(13.41)	(22.41)	(159.39)	(59.86)	(256.55)
District Char.	Y	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	743	733	744	727	744	744	744	744
No. Districts	93	93	93	93	93	93	93	93
R-squared	0.94	0.87	0.82	0.95	0.90	0.96	0.95	0.96

Notes: Sample period covers 2010-2017 and includes Ever CEP districts. Sample excludes NYC, "Big 4" districts (Buffalo, Rochester, Syracuse, & Yonkers), four districts that consolidated in 2014, and one district with incomplete data. All models control for a vector of district characteristics including percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. All models weighted by enrollment.

Table 27: Estimated Impacts of CEP on Weight Outcomes by Extent of Implementation, 2010-2017

	Primary Grades		Secondary Grades	
	% Overwgt	% Obese	% Overwgt	% Obese
	(1)	(2)	(3)	(4)
PCT CEP	-0.073	-0.056	-0.206**	-0.152*
	(0.0817)	(0.0761)	(0.0905)	(0.0882)
Districtwide	-0.922	0.189	-5.524	-1.662
	(2.661)	(2.519)	(3.608)	(4.071)
District Char.	Y	Y	Y	Y
District FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	364	361	361	358
No. Districts	93	93	93	93
R-squared	0.745	0.730	0.609	0.579

Notes: Robust standard errors in parentheses clustered by district (*p<.10; **p<.05; ***p<.01). Sample period covers 2010-2017 and includes Ever CEP districts. Sample excludes NYC, "Big 4" districts (Buffalo, Rochester, Syracuse, & Yonkers), four districts that consolidated in 2014, and one district with incomplete data. Primary refers to grades K, 2, and 4, and Secondary refers to grades 7 and 10. Weight outcome data assigned to the beginning of the two-year reporting cycle using last year's treatment status. Estimates weighted by student enrollment in measured grades (K, 2, 4, 7, 10). All models control for a vector of district characteristics including percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, PCT CEP^2, district fixed effects, and year fixed effects. Data is unavailable for districts with fewer than 5 students in a category. Therefore, the number of observations is inconsistent across outcomes.

Table 28: Predicting CEP Adoption Among Ever CEP Districts & Schools, 2012-2017

	District CEP t+1	School CEP t+1
	(1)	(2)
% Black	-0.02	-0.01
	(0.03)	(0.02)
% Hispanic	0.04	0.00
	(0.03)	(0.02)
% Asian/Other	0.05	0.02
	(0.03)	(0.02)
% LEP	-0.08	-0.01
	(0.06)	(0.01)
% SWD	-0.00	________
	(0.04)	
% Free Lunch	-0.00	0.00
	(0.00)	(0.00)
School FE	N	Y
District FE	Y	N
Year FE	Y	Y
Observations	404	1,154
No. Schools	-	321
No. Districts	93	87
R-squared	0.62	0.47

Notes: Robust standard errors in parentheses clustered by district (*p<.10; **p<.05; ***p<.01). Sample periods cover 2012 to 2017. Column 1 includes Ever CEP districts, and Column 2 includes Ever CEP schools. All models control for percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Estimates weighted by enrollment.

Appendix C

Table C1: Estimated Impacts of CEP on Meal Participation and Attendance, Including Big 4, 2010-2017

	Bfast	District Lunch	Attd Rate
	(1)	(2)	(3)
CEP	8.650***	7.183***	-0.0151
	(1.826)	(0.773)	(0.273)
District Char.	Y	Y	Y
District FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	776	776	679
No Districts	97	97	97
R-squared	0.867	0.911	0.864

Notes: Robust standard errors in parentheses clustered by district (*p<.10; **p<.05; ***p<.01). Sample period covers 2010 to 2017 for meal participation outcomes and 2010 to 2016 for attendance outcome and includes Ever CEP districts. Sample excludes NYC and four districts that consolidated in 2014. All models control for percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Estimates weighted by enrollment.

Table C2: Estimated Impacts of CEP on Student Weight Outcomes, Including Big 4, 2010-2017

	All Grades		Primary Grades		Secondary Grades	
	% Overwgt	% Obese	% Overwgt	% Obese	% Overwgt	% Obese
	(1)	(2)	(3)	(4)	(5)	(6)
CEP	-0.648	-0.159	0.222	0.387	-3.494***	-2.393**
	(1.014)	(1.051)	(1.056)	(1.261)	(1.318)	(1.139)
District Char.	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	384	381	380	377	377	374
No. Districts	97	97	97	97	97	97
R-squared	0.750	0.707	0.760	0.712	0.623	0.589

Notes: Robust standard errors in parentheses clustered by district (*p<.10; **p<.05; ***p<.01). Sample period covers 2010-2017 and includes Ever CEP districts. Sample excludes NYC, four districts that consolidated in 2014, and one district with incomplete data. Primary refers to grades K, 2, and 4, and Secondary refers to grades 7 and 10. Weight outcome data assigned to the beginning of the two-year reporting cycle using last year's treatment status. Estimates weighted by student enrollment in measured grades (K, 2, 4, 7, 10). All models control for a vector of district characteristics including percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Data is unavailable for districts with fewer than 5 students in a category. Therefore, the number of observations is inconsistent across outcomes.

Table C3: Estimated Impacts of CEP on Fiscal Outcomes, Including Big 4, 2010-2017

Panel A: Per Pupil

	Food Revenue				Food Expenditures		Instructional Expenditures		
	Local	State	Federal	Total	Personnel	Total	Salaries	Benefits	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CEP	-21.89***	3.34	93.12***	77.60***	10.00	50.98***	-55.99	-57.86	49.26
	(3.60)	(2.90)	(12.47)	(13.49)	(7.80)	(11.07)	(98.19)	(47.23)	(185.67)
District Char.	Y	Y	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	775	776	765	776	759	776	776	776	776
No Districts	97	97	97	97	97	97	97	97	97
R-squared	0.95	0.66	0.93	0.90	0.93	0.92	0.95	0.94	0.95

Panel B: Per Meal

	Food Revenue				Food Expenditures	
	Local	State	Federal	Total	Personnel	Total
	(1)	(2)	(3)	(4)	(5)	(6)
CEP	-0.18***	-0.01	0.05	-0.12*	-0.17***	-0.25***
	(0.03)	(0.02)	(0.04)	(0.06)	(0.06)	(0.05)
District Char.	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	775	776	765	776	759	776
No Districts	97	97	97	97	97	97
R-squared	0.95	0.67	0.86	0.72	0.90	0.75

Notes: Sample period covers 2010-2017 and includes Ever CEP districts. Sample excludes NYC, four districts that consolidated in 2014, and one district with incomplete data. All models control for percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Estimates weighted by enrollment. Revenue and expenditures data are in 2017 dollars. Panel A outcomes are revenues and expenditures per pupil, and Panel B outcomes are revenues and expenditures per meal served. Districts missing data in select years: 1 local food revenue, 6 federal food revenue, and 4 personnel food expenditures.

Table C4: Estimated Impacts of CEP on Meal Participation and Attendance, Unweighted, 2010-2017

	Bfast	District Lunch	Attd Rate
	(1)	(2)	(3)
CEP	9.101***	7.866***	-0.348
	(1.502)	(0.711)	(0.237)
District Char.	Y	Y	Y
District FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	744	744	651
No Districts	93	93	93
R-squared	0.791	0.876	0.734

Notes: Robust standard errors in parentheses clustered by district (*p<.10; **p<.05; ***p<.01). Sample period covers 2010 to 2017 for meal participation outcomes and 2010 to 2016 for attendance outcome and includes Ever CEP districts (Columns 1-3) and schools (Columns 4-9). Both samples exclude NYC, “Big 4” districts, and four districts that consolidated in 2014. School panel sample includes 198 primary and 93 secondary continuously open schools that ever adopt CEP from 2013-2018 and excludes 34 schools in 24 districts with implausibly high meal participation rates, 4 Elementary-Middle schools and 8 K-12 schools. All models control for percent black, Hispanic, Asian/other, English language learners, students with disabilities (unavailable in school-level models), and free lunch students, district (school) fixed effects, and year fixed effects.

Table C5: Estimated Impacts of CEP on Student Weight Outcomes, Unweighted, 2010-2017

	All Grades		Primary Grades		Secondary Grades	
	% Overwgt	% Obese	% Overwgt	% Obese	% Overwgt	% Obese
	(1)	(2)	(3)	(4)	(5)	(6)
CEP	-0.215	-1.031	0.295	-0.740	-2.899*	-2.096
	(0.978)	(0.836)	(1.171)	(1.080)	(1.541)	(1.304)
District Char.	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	368	365	364	361	362	358
No. Districts	93	93	93	93	93	93
R-squared	0.578	0.595	0.576	0.593	0.393	0.523

Notes: Robust standard errors in parentheses clustered by district (*p<.10; **p<.05; ***p<.01). Sample period covers 2010-2017 and includes Ever CEP districts. Sample excludes NYC, “Big 4” districts, four districts that consolidated in 2014, and one district with incomplete data. Primary refers to grades K, 2, and 4, and Secondary refers to grades 7 and 10. Weight outcome data assigned to the beginning of the two-year reporting cycle using last year’s treatment status. All models control for a vector of district characteristics including percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Data is unavailable for districts with fewer than 5 students in a category. Therefore, the number of observations is inconsistent across outcomes.

Table C6: Estimated Impacts of CEP on Fiscal Outcomes, Unweighted, 2010-2017

Panel A: Per Pupil

	Food Revenue				Food Expenditures		Instructional Expenditures		
	Local	State	Federal	Total	Personnel	Total	Salaries	Benefits	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CEP	-39.49***	0.08	77.05***	42.40***	10.36	39.63***	-44.65	-73.13	-40.96
	(4.49)	(3.64)	(9.17)	(9.34)	(6.42)	(8.26)	(68.09)	(44.31)	(117.10)
District Char.	Y	Y	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	743	744	733	744	727	744	744	744	744
No Districts	93	93	93	93	93	93	93	93	93
R-squared	0.91	0.78	0.87	0.84	0.92	0.89	0.94	0.94	0.94

Panel B: Per Meal

	Food Revenue				Food Expenditures	
	Local	State	Federal	Total	Personnel	Total
	(1)	(2)	(3)	(4)	(5)	(6)
CEP	-0.31***	-0.03	0.01	-0.31***	-0.18***	-0.33***
	(0.03)	(0.02)	(0.04)	(0.06)	(0.05)	(0.06)
District Char.	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	743	744	733	744	727	744
No Districts	93	93	93	93	93	93
R-squared	0.90	0.78	0.82	0.69	0.88	0.78

Notes: Sample period covers 2010-2017 and includes Ever CEP districts. Sample excludes NYC, “Big 4” districts, four districts that consolidated in 2014, and one district with incomplete data. All models control for percent black, Hispanic, Asian/other, English language learners, students with disabilities, and free lunch students, district fixed effects, and year fixed effects. Revenue and expenditures data are in 2017 dollars. Panel A outcomes are revenues and expenditures per pupil, and Panel B outcomes are revenues and expenditures per meal served. Districts missing data in select years: 1 local food revenue, 6 federal food revenue, and 4 personnel food expenditures.

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