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Three Essays on the Implementation of Food Assistance Programs

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Abstract

This dissertation uses experimental and quasi-experimental methods to evaluate policy instruments available to state and local officials administering food assistance programs for improving children's utilization of services, nutritional intake, and food security. More specifically, this dissertation consists of three chapters pertaining to the National School Lunch Program (NSLP) and the Supplemental Nutrition Assistance Program (SNAP).

In the first chapter, co-authored with Amy Ellen Schwartz, I use large, uniquely detailed longitudinal administrative data on New York City students and schools, including the different lunch menus they offer over time, to present the first plausibly causal evidence on the link between school menus and participation in the NSLP, and their implications for disparities in program utilization across students from diverse backgrounds. Using student and school fixed-effects models, I find that the introduction of new menus increases the share of students participating on both the extensive and intensive margins, and can help to close racial, gender, and socioeconomic gaps in the utilization of school lunch. In one extension, I find evidence that increases in participation are driven by the adoption of newer, more innovative menus. I find no evidence of changes in attendance or adverse weight outcomes. These findings provide evidence in support of the efforts that many school districts are taking to boost lunch participation by reformulating their menus and help to inform the decisions of those considering similar changes.

While students stand to gain from participating in school lunch, it is also important that they make the right dietary choices as they make their way through the lunch line. In the second chapter, I use primary data collected through a month-long field and survey experiment I designed and conducted to investigate the efficacy of using cheap material rewards to induce better dietary choices among low-income Black and Hispanic children—who are more likely to

be obese than their high-income or white peers—in a school lunch setting. While existing studies have shown material rewards to be effective in the short term and when introduced intermittently, this study shows that their effect can dissipate quickly over time when offered daily. I find no evidence that the introduction of extrinsic incentives crowded out intrinsic dietary behaviors. These findings have implications for the design and implementation interventions using material rewards for improving dietary habits among school-aged children.

In the third chapter, I broaden the scope of my research to include SNAP, which helps to safeguard the food security of millions of children. The politicization and racialization of the program have made it a target of reforms that effectively limit its coverage and efficacy, and contribute to its disparate implementation across states and counties in ways that exacerbate social inequity. I designed a large survey experiment evaluating the efficacy of highlighting the child beneficiaries of SNAP for inducing greater public support for the program. I find that emphasizing its child beneficiaries can increase support overall and across key political constituencies, though more so when those children are characterized as White than Black. As an extension, I also examine the generalizability of these findings to the Temporary Assistance for Needy Families program, and again find that highlighting child beneficiaries leads to increases in support, though the estimated effects are less pronounced and even more so dependent on the children's race. These findings can help to inform the outreach efforts of program administrators.

This dissertation adds to the existing literature by providing new insights and expanding on previous work. The results of the first chapter corroborate the many anecdotal accounts suggesting that school menus influence student lunch participation, but also show that other barriers, such as the price of meals or stigma, may be more important. The second chapter extends previous work on using material rewards to induce better dietary behaviors by presenting

evidence that they may not be as cost-effective or easy to implement as previously thought. The findings of the third chapter show that providing information on the child beneficiaries of welfare programs can change public attitudes towards them and offer fresh evidence that public opinion of government policy is often based on the demographic groups perceived to benefit from them. This dissertation also highlights the implications that policy instruments can have for social equity and economic equality by focusing on disparities in program utilization, access, and outcomes across race, gender, age, and socioeconomic status. Lastly, it offers guidance for policy makers and program administrators by providing new evidence about the efficacy of various policy instruments available to them for administering food assistance programs.

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As my mother loved to recount, when I was a young child, I would sit next to my uncle while he worked on his dissertation and proudly proclaim that I was doing the same while scribbling on scrap paper. Unfortunately, my mother, who was perhaps more excited than anyone for me to obtain my doctorate, passed away several months before the completion of this dissertation. Although I am saddened that she was not able to share this moment with me in person, I am happy that I was able to fulfill one of her dreams for me. I dedicate this work to her.

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Chapter 1

What's for Lunch? The Relationship between School Menus and Student Lunch Participation

Introduction

Students stand to gain a lot from eating school lunch, which has been linked to improved nutritional intake, health outcomes, and academic performance (e.g., Gundersen et al., 2012; Smith, 2017; Bartfeld, 2015; Hinrichs, 2010; Schwartz & Rothbart, 2019; Lin et al., 2019). Schools also stand to benefit as higher student participation can boost test scores and help schools avoid potential sanctions under accountability systems (Figlio & Winicki, 2005). However, realizing these benefits requires that students participate, yet nearly half of them do not (Hoynes & Schanzenbach, 2015). While the nutritional quality of school meals and their cost to students has received much attention, little is known about how school menus—the set of meals served throughout an academic year—influence participation in school lunch.

Although the federal government sets nutritional standards for meals served in schools participating in the National School Lunch Program (NSLP), school districts have considerable discretion in setting school food policies, including what to serve their students. School menus may therefore constitute an important policy instrument for increasing lunch participation at the local level and districts nationwide have taken steps to reformulate their menus in an effort to boost participation (Pew and RWJF, 2016; School Nutrition Association, 2017). In addition to boosting participation overall, the choice of menus may also help close gaps in lunch participation across students from different backgrounds, with potential implications for social equity. Yet there is a dearth of rigorous research documenting and exploring student's responsiveness to new menus.

This study constitutes the first large-scale investigation of this relationship. Using unique, detailed longitudinal student-level data and exploiting variation in the types of menus offered across nearly 800 New York City (NYC) traditional public schools, we examine the relationship between menus and the propensity for and intensity of lunch participation among middle and high school students.[1] Specifically, we use student fixed-effects models leveraging variation in the types of menus students are exposed to over time while attending the same school together with a range of control variables to estimate the impact of a new menu on lunch participation in schools with Point of Service (POS) systems tracking lunch transactions. This study is the first to make use of school menu data on this scale, and is among the first to make use of POS data—a substantial improvement over the survey data on lunch participation used in previous work (Moore, Hulsey, & Ponza, 2009). We also document difference in lunch participation across students from diverse backgrounds and explore heterogeneous responsiveness to new menus by student characteristics. In a series of extensions, we examine the interaction of new menus with the introduction of free meals for all students and the relationships between different menu types and lunch participation. Lastly, we assess potential linkages between menus and attendance and weight outcomes. This paper focuses on middle and high school students as they are more likely to make autonomous decisions about lunch than their elementary school counterparts (Gordon & Fox, 2007; Fox & Condon, 2012).

New York City is an ideal setting for such a study, offering several advantages. First, the NYC Department of Education (NYCDOE) is unique in that it offers over a dozen different lunch menus that school principals can choose to serve their students, with many schools adopting new menus over time, thereby providing the requisite variation for an analysis of menus

[1] Propensity for lunch participation and lunch participation intensity can be understood as participation at the extensive and intensive margins, respectively.

and participation. Second, while implementation of school lunch varies across schools in the district, all are subject to uniform nutrition and competitive food policies. Third, the district's large and diverse student population facilitates the exploration of differential responsiveness to school menus across students from diverse backgrounds. Fourth, the NYCDOE has diversified and expanded its menus in ways similar to other school districts nationwide (e.g., Pew & RWJF, 2016; Student Nutrition Association, 2016, 2017). The results from this study may therefore help to inform other school districts about the efficacy of their own initiatives.

To preview the results, we find that menus do matter for lunch participation. Overall, adopting a new menu may increase the propensity and intensity of students participating by up to four and three percentage points, respectively. Relative to baseline participation rates, these constitute meaningfully large effects. Responsiveness to new menus also varies considerably by student characteristics, suggesting that menus can help close demographic and socioeconomic disparities in school lunch participation. The type of menus offered may also matter, as students appear to respond more favorably to some than others. Lastly, we find no associations between new menus and attendance, and no evidence of adverse effects on weight outcomes.

Background on the NSLP and Student Lunch Participation

The National School Lunch Act of 1946 established the NSLP to reduce child hunger, improve children's health, and promote the consumption of domestic agricultural commodities. The program has since grown to become the second largest food assistance program in the United States after the Supplemental Nutrition Assistance Program and provides federally subsidized free and low-cost meals to over 30 million children each school day in over 100,000 schools and childcare centers nationwide at a cost of $14 billion. While all students can participate in the program, meals are typically provided using a three tiered system wherein

students from households with incomes below 130 percent of the federal poverty line are eligible for free lunch, those between 130 and 185 percent eligible for reduced price lunch (no more than $0.40), and those over 185 percent paying full price.

The program has undergone several reforms aimed at expanding its coverage and improving the nutritional content of its meals. The most recent was the Healthy, Hunger-Free Kids Act of 2010 (HHFKA), which mandated more stringent nutritional standards for school meals and beverages, set maximum calorie limits per meal, strengthened weekly requirements for fruits and vegetables, and authorized the United States Department of Agriculture to regulate the nutritional content of all foods sold in schools.[2] Additionally, the act expanded coverage of the program through the Community Eligibility Provision, which allows schools to offer free meals to all students (known as Universal Free Meals, or UFM).[3]

Meals served as part of the NSLP typically meet nutrition standards and are healthier than most alternative meals and snacks available to students through vending machines, snack bars, fast food outlets, or packed lunches from home (Gordon & Fox, 2007; Fox & Condon, 2012, Au et al., 2016; Ralston et al., 2017). Students taking advantage of the program consume more protein, essential nutrients, milk, and whole grains as compared to those that do not, and fewer desserts, snacks, and other beverages (Bogden, Brizius, & Walker, 2012; Lin et al., 2019). Although effects vary across studies depending on the data, population, and methodology used, several studies suggest that the NSLP may improve health outcomes and mitigate the prevalence

[2] As of February 2019, some of these changes have been reverse. These include the requirement that schools serve only whole-grain rich foods and non-flavored milk. The deadline to reduce the amount of sodium in foods served was also extended (USDA, 2018).

[3] Schools can offer UFM through the Community Eligibility Provision if at least 40 percent of students are categorically eligible for free meals through enrollment in a food or cash assistance program, in the foster care system, or homelessness. Food and cash assistance programs by which categorical eligibility is determined includes the Supplemental Nutrition Assistance Program, Temporary Assistance for Needy Families, and the Food Distribution Program on Indian Reservations.

of food insecurity among participating students (Gundersen et al., 2012; Bartfeld, 2015; Smith, 2017). A growing body of evidence also suggests that participation in the program may boost test scores (Figlio & Winicki, 2005; Hinrichs, 2010; Schwartz & Rothbart, 2019), increase attendance, and reduce occurrences of disruptive behavior during the school day (Murphy et al., 1998; Poppendieck, 2010).

Despite these potential benefits, not all students take advantage of school lunch. In 2014, only 56% of all kindergarten through 12th grade students participated, down from a peak of 60% in 2010 (Hoynes & Schanzenbach, 2015). While participation is lowest among higher-income students, a substantial number from lower-income households also do not participate despite eligibility for free or reduced-price lunches. In 2010, only 79% and 73% of students certified eligible for a free or reduced-price lunch participated, respectively, and participation was 70% in urban areas in 2013, where higher concentrations of poverty might be expected. Participation also declines as students age and is particularly low among middle and high school students (Fox & Condon, 2012; Carson, 2015).

Several reasons may explain the low and declining participation rates in the program. First, the price of meals may be cost-prohibitive for those not eligible for free or reduced-price lunches (Gordon & Fox, 2007; Moore, Hulsey, & Ponza, 2009; Schwartz & Rothbart, 2019). Second, stigma may deter participation if eating a school lunch signals a student's socioeconomic status (Mitcheva & Powell, 2009; Poppendieck, 2010). Third, competition from foods higher in sugar and fat content sold inside schools but outside of the NSLP (e.g., vending machines or a la carte options) and off-site vendors (e.g., fast-food restaurants) may divert students away from school meals (Bhatia, Jones, & Reicker, 2011; Miller et al., 2016). Fourth, the school environment and related lunch policies—such as cafeteria capacity, time of day lunch

is served, length of the lunch line, and amount of time available for eating—might also deter participation (e.g., Smith, Cunningham-Sabo, & Auld, 2015). Lastly, and widely expressed, is that the meals served through the program may be unappealing to students, especially as increased nutrition standards have made meals healthier (Poppendieck, 2010; Woo Baidal & Taveras, 2014).

While studies have investigated other factors associated with school lunch participation, the evidence on whether the menus offered might themselves be a determinant is limited and mostly anecdotal. Nevertheless, school districts nationwide are working to enhance the appeal of their lunches by reformulating their menus. According to a 2017 survey of over 500 school district directors from across the country, 60% reported offering at least one menu item that featured an international flavor and 26% were considering or testing such meals. Just over 87% reported offering a salad bar, entrée bar, self-service flavor stations, or some other customizable menu option (School Nutrition Association, 2017). In another survey, 32% of school directors reported increasing menu options in an effort to boost participation and consumption (Pew and RWJF, 2016). In this paper, we assess the efficacy of schools adopting new menus to increase lunch participation.

Literature

Existing literature on the relationship between school menus and student lunch participation is scarce. Aside from the sensory appeal of the meals offered (e.g., taste and aesthetics), the effect of adopting a new menu on student lunch participation is unclear and may vary depending on its particular features. Conventional economic theory suggests that menus offering more food options are better able to satisfy diverse preferences, leading to increased take-up of school lunch. Affording student's greater choice may also enhance their sense of self-

autonomy and satisfaction with their chosen option (Scheibehenne, Greifeneder, & Todd, 2010; Chernev, Böckenhold, & Goodman; 2015).[4] Yet menu variety may still do little to increase student participation if the food items offered lack appeal or variety alone is not sufficient to overcome other barriers to participation. Studies investigating the link between food variety and consumption among children and adolescents are few and present mixed results. In an experiment involving Dutch children ages 4 to 6, Zeinstra et al. (2010) found no difference in consumption across those given a choice between two vegetables and those who were not offered a choice. In contrast, Dominguez et al. (2013) found that Spanish children of the same age who were given a choice between two vegetables had a higher rate of consumption than those offered no choice. Altintzoglou, et al. (2015) finds that Norwegian 11 and 12-year olds offered a choice between two fish meals expressed greater satisfaction with their meal, though consumption was no different as compared to the group offered no choice. These studies rely on small convenience samples and were conducted under controlled experimental conditions with a limited set of options. It is therefore difficult to generalize these results to the cafeteria setting.

Alternatively, as suggested by consumer theory, the novelty of a new menu might serve to increase student participation, though the effect may be short-lived (McAlister and Pessemier, 1982; Kahn, 1995). However, if students are more comfortable with what they are familiar with, the introduction of a new menu may disrupt established behaviors and eating patterns leading to a reduction in participation. For example, when asked how students responded to the introduction of a healthier menu in 2012, cooks and school food managers from a district in

[4] The literature on "choice overload" also suggests that expanding the number of options available might increase the cognitive cost of making a choice and induce apprehension, which can deter a decision altogether or leave students regretful and unsatisfied. However, the circumstances under which increased choice may become burdensome is unclear and it is unlikely that set of food items often available for lunch is large enough to induce a significant burden.

Louisiana stated that middle and high school students were disappointed that food items they were familiar with were no longer offered and were less likely to participate as a result (Murimi et al., 2015). Conversely, a strand of literature examining the relationship between healthier school lunches and student participation in the United States found no adverse effects on the number of meals served, school revenues, or food waste (Cohen et al., 2014; Cohen et al., 2016; Cullen and Dave, 2016; Andersen, Gallagher, and Ritchie, 2018). These findings would suggest that students may not respond negatively to changes in school menus. However, these studies fail to account for pre-existing trends, use coarse measures of lunch participation, or do not account for other changes at the school or district level which could have influenced how students responded to new menus.

Convenience may also matter, as qualitative evidence suggests that students are deterred from participating in lunch by long lunch lines (Smith, Cunningham-Sabo, & Auld, 2015). Menus allowing students to receive their meals more quickly may therefore encourage participation, though the opposite could occur if the kinds of meals that might allow for faster access are also unappealing or lack variety.

This study is the first to investigate the relationship between school menus and lunch participation on a large-scale and in the school setting, and controls for a host of unobservable and observable student (e.g., innate traits) and school characteristics (e.g., other food policies) that might be related to the menus schools choose to serve and lunch participation outcomes. Although this study can only link menus to taking a lunch, and not consumption explicitly, it constitutes a substantial improvement over existing research and contributes significantly to the literature on NSLP participation, particularly as it pertains to the ability of school districts and individual schools to influence lunch participation.

School Lunch in NYC

The NYCDOE is the nation's largest school district, serving over 1.1 million students across more than 1,800 traditional and charter public schools. The district's student body is diverse, with Hispanics, Blacks, Asians, and Whites constituting 41, 26, 16, and 15 percent of students, respectively. Most students are also economically disadvantaged, with 74 percent eligible for a free or reduced-price school lunch.[5]

School meal programs in the district are administered by the Office of Food and Nutrition Services (OFNS), which operates with a budget of over $550 million, employs over 9,000 people, and serves nearly one million meals and snacks each school day (Perlman, et al., 2012; Hoffman, O'Hagan, K., & Sompura, 2018).[6] Beginning in 2001, NYCDOE implemented a series of reforms to make its school meals healthier and develop new recipes. As a result, nutrition standards for school lunches in New York City meet or exceed those required under federal guidelines. The district has also expanded free meal coverage, notably in academic year 2014, when it made free lunch available to all reduced price eligible students while raising the cost of a full price lunch to $1.75 (from $1.50), and in academic years 2015 and 2018, when it introduced UFM to schools serving grades 6 through 8 and then all schools, respectively.[7]

The NYCDOE also took steps beginning in 2003 to restrict competitive foods in school by eliminating all sodas from vending machines and permitting only water, milk, 100% juice, and low-fat snacks. These efforts were reinforced in 2008 when the city government imposed additional nutrition restrictions for all foods offered in city agencies, including schools.[8] This

[5] New York City Department of Education, accessed May 2019: https://on.nyc.gov/2K0hklV
[6] Meals served includes breakfast, lunch, snacks, and after school meals.
[7] Prior to the HHFKA, schools in NYC provided free meals to students at certain schools under Provision 2 of the National School Lunch Act. See Schwartz and Rothbart (2019) for details.
[8] These included restrictions on artificial trans-fats, deep-fried foods, sodium levels, and certain sugar-sweetened beverages.

was followed by additional NYCDOE nutrition restrictions in 2010 for all competitive foods offered in schools, including fundraisers, bake-sales, a la carte options, and vending machines (Perlman, et al., 2012). Efforts to regulate competitive foods in schools, such as those taken by the NYCDOE, have been shown to be associated with lower access to less healthy alternatives to school provided meals (Merlo et al., 2014).

In 2004, OFNS hired chefs to develop menu items more appealing to students and which could be prepared in all schools, regardless of kitchen space and equipment (Perlman et al., 2012).[9] Reflecting these goals, OFNS began making available additional lunch menus—each consisting of a set of meals provided daily based on a service schedule—that schools could serve starting in 2009 and greatly expanded their choice set beginning in 2012, as shown in **Figure 1**. As of 2019, OFNS boasts more than 130 recipes offered across 13 lunch menus.

These menus vary by number of food items (entrées and salads), type of food items (e.g., hot vs. cold), their delivery method (e.g., express or service line), and their preparation (e.g., "scratch-cooked" or off-site) and can be organized broadly into two categories, standard and non-standard, that include menus reflecting nationwide trends in changes to school lunch menus. The set of standard menus are those most often served by middle and high schools and include more food items and variety (e.g. hot and cold food items as well as culturally diverse meals) as compared to most other menus. The set of non-standard menus includes the alternative, express, special needs, early childhood, vegetarian, and food court menus. The set of alternative menus includes more scratch-cooked meals prepared on-site using raw ingredients and fewer processed

[9] Appealing to the tastes of its diverse student body is a primary concern. As stated on the OFNS website, the department invests a "considerable amount of time in…reformulating food items to guarantee…citywide menus that are specific to the various school divisions…and meet the needs of our students," and "…to guarantee that the diverse palates and requests of the 1.1 million NYC students…are met." Quotes from OFNS website and accessible at: http://www.schoolfoodnyc.org/MenusAndRecipes/menus.htm

foods. Express "grab and go" menus offer fewer food items and less variety, and either serve hot (e.g., burgers) or cold (e.g., deli sandwiches) foods. Special needs menus offer a subset of the meals offered in the standard menus, as does the early childhood menu, which includes fewer culturally diverse meals and no pizza. The vegetarian menu serves meals without meat protein. Lastly, the food court menu offers the greatest variety of foods—with daily offerings of sandwiches, salads, pizzas, French fries, and popcorn chicken in addition to options from the HS standard menu—in cafeterias designed to mimic a food court-style environment.[10] Among the set of standard menus are the high school (HS), middle school (MS), and Kindergarten through eighth grade (K-8) standard menus. Alternative menus include both a HS and K-8 version, as do the express cold, express hot, and special needs menus.

While there is overlap in the entrees and salads that appear across menus, there are food items that appear in some but not others. For example, as seen in **Figure 2**, the HS standard menu offers 108 food items while the middle school (MS) menu offers 94, of which 87 are included in the former but seven are not (e.g., "oven roasted turkey & cheese hero").[11]

Another source of variation across menus is their food item service schedule. To illustrate this point, panel A of **Figure 3** depicts what is served as part of the HS standard menu in a typical week. As with most menus, students are offered an entrée, salad, and a side each day. Panel B then compares what is served as part of the HS standard menu with that in the HS alternative menu for the same week in the same academic year. Each day, entrées, salads, or both differ across the two menus. On Monday, students in schools offering the HS standard menu

[10] Some food items served as part of the food court menu are displayed as hot or cold options under heat lamps or in refrigerated display cases, which allows students to self-select meals. In some cases, this menu was accompanied by changes to the dining area, such as new seating arrangements (e.g., booths and round tables instead of rectangular tables) or renovations of the floors, walls, and lighting of the cafeteria.

[11] Officials at OFNS affirmed that contents of menus were relatively stable between school years 2013-2014 and 2017-2018, after which there have been substantial changes, such as the discontinued use of processed meats in school meals beginning in the 2019-2020 school year.

were offered a "celery and apple salad" while those serving the HS alternative menu were offered the "Asian slaw salad." However, on some days, like Tuesday, the same food item appears across both menus—in this case the "turkey cheeseburger." As seen in **Figure 4**, the number of food items available on any given day may also vary, with the standard set of menus offering more options as compared to most non-standard menus.

School principals decide which menus to serve their students. Although nutritional standards and menus are set by OFNS and apply to all schools, principals have substantial discretion over their school's food policies. School principals decide what (e.g. cafeteria-based programs), when (e.g., time of day), and who (e.g., grade levels) to serve. They also decide whether their students can leave school during lunch and if snack and junk foods are permissible on school grounds and where and when they can be eaten (Leardo et al., 2018). According to OFNS, school principals typically choose which menu to serve early in the academic year.[12]

These features of the NYC school district—the variation in menus across and within schools over time in a single district with uniform nutrition and competitive food policies—together with its size and diversity make it an ideal setting to study the link between menus and lunch participation on a large scale and the efficacy of introducing new menus as a policy instrument. Insights gleaned from NYC can guide other districts and school administrators considering introducing new menus and better inform those already doing so.

Data and Measures

This study uses unique, richly detailed longitudinal data on NYC students and schools for academic years 2013-2014 (AY 2014) to 2017-2018 (AY 2018) provided by the NYCDOE and

[12] According to officials at OFNS, principals can choose which menus to serve for any reason, including personal preference, though practical considerations such as staff workload, kitchen capacity, equipment, and student's tastes play a significant role.

OFNS. Specifically, we focus on students in schools with point-of-service (POS) systems tracking meal transactions to facilitate the construction of our lunch participation measures.[13]

Student characteristics include race/ethnicity, gender, participation in special education services, primary language spoken at home, certified eligibility for free or reduced priced lunch, daily attendance, school attended, and, for a large subset of students, weight, height, and daily breakfast and lunch transactions. We create two measures of lunch participation. The first is an "ever participate in lunch" ($EverLP_1$) binary variable that reflects the propensity for lunch participation and takes a value of 1 if a student has at least one lunch transaction in an AY and 0 otherwise. We further supplement this with binary variables capturing at least 10 ($EverLP_{10}$) and twenty ($EverLP_{20}$) transactions in an AY. The annual "lunch participation rate" (*LPrate*) variable captures lunch participation intensity and is measured as the percent of days a student participated in school lunch of all days they attended school. It is constructed by dividing the number of lunch transactions a student had in an AY by the number of days they attended school in that same year and multiplying by 100. We create measures for participation in school breakfast similarly ($EverBP_1$, $EverBP_{10}$, $EverBP_{20}$, and *BPrate*).

Student race/ethnicity is captured by a set of binary variables that take a value of 1 if a student is Asian or Other (e.g., Native American or Pacific Islander), Hispanic, Black, or White, and 0 otherwise. Gender, participation in special education services, and primary language spoken at home are measured as binary variables that take values of 1 if a student is female (*Female*), has an individualized education program (*SWD*), or speaks a primary language other than English at home (*Non-English at Home*), and 0 otherwise. The free and reduced-price lunch (FRPL) measure is time-invariant and takes a value of 1 if a student was certified eligible for a

[13] Point-of-service systems require that students input an identification number as they receive their school meals, thereby creating a record of their lunch transaction.

free or reduced-price lunch in any year between 2001 and 2018 (*Ever FRPL*) and 0 otherwise.[14] Annual student attendance is measured as the number of days present divided by the total number of school days multiplied by 100. Data on schools attended is used to construct a binary variable that takes a value of 1 if a student attends a different school than in the previous year (*New School*), and 0 otherwise.[15] Data on student weight and height is used to calculate student's Body Mass Index (BMI). Weight variables include BMI measured as z-scores and normalized by grade-year (*zBMI*), the natural logarithm of BMI (*ln(BMI*)), and indicator variables for underweight (BMI percentile $\leq 5^{th}$), overweight (85^{th} < BMI percentile $\leq$ 95th), or obese (95^{th} < BMI percentile), and 0 otherwise, based on age- and sex-specific growth charts from the Centers for Disease Control and Prevention.

School characteristics include the number of students enrolled at the school (*Enrollment*), the school's principal, UFM status, and menu served.[16] To proxy for principal decisions that may influence lunch participation, a binary variable that takes a value of 1 the year a school gets a new principal and 0 otherwise is included (*New Principal*). School UFM status is measured by a binary variable that takes a value of 1 the years a school provides all students with free meals and 0 otherwise (*UFM*). We create a variable for years a school does not provide all students with free meals similarly (*No UFM*).

[14] This measure better captures the economic disadvantage faced by some students in NYC and protects against potential under-reporting of individual eligibility for meal subsidies among students in UFM schools whose eligibility status cannot be determine through direct certification (i.e., by enrollment in another means tested program) or whose households may no longer submit forms used to determine eligibility status. See Schwartz and Rothbart (2019) for additional details.

[15] In our main analysis, we use a sample of students that do not change schools and for whom variation in exposure to new menus is due to their schools choosing to adopt a new menu. As a robustness check, we also run the analysis including students who change schools and for whom variation in exposure to a new menu may be due to their schools adopting a new menu or them attending a new school that serves a menu that differs from their previous schools. We elaborate on this further below.

[16] We have menu data as of December 31st for AY 2014, and October 31st for each subsequent year. According to officials at OFNS, most schools have selected their menus by October and rarely make changes thereafter, but if a school not identified as having introduced a new menu in a particular year actually did so then our estimates will be biased downwards. We use student and school identifiers to match students to menus.

Our key variables capture the relationship between school menus and lunch participation. A binary variable *Post New Menu* measures the overall effect of adopting a new menu and takes a value of 1 in the year a student is exposed to a menu that differs from what they were served the previous year and each year thereafter, and 0 otherwise. For example, if a student was served the HS standard menu in AY 2014 and the HS alternative menu in AY 2015-2017, *Post New Menu* would take a value of 0 in the former and a value 1 one for each of the years in the latter period. To allow the effect of a new menu to vary over time, we create binary variables *Post New Menu 1st AY* and *Post New Menu gt. 1 AY* (where "*gt.*" Denotes "greater than") that take a value of 1 in the first year of a new menu and two, three, or four years after, respectively, and 0 otherwise. Using the previous example, *Post New Menu 1st AY* would take a value of 1 in AY 2015 and 0 in both AY2016 and 2017, while *Post New Menu gt. 1 AY* would take a value of 0 in AY 2015 and 1 in each of AY2016 and 2017.

As an extension to our main analysis, we create four other measures. The first two are a binary variables *Non-Standard Menu* and *Standard Menu* that take a value of 1 if a student is served any one of the non-standard (for the former) or standard (for the latter) menus, and 0 otherwise. The other two variables are an interaction of *Post New Menu* with *Non-Standard Menu* and *Standard Menu*, and capture the effect of a new non-standard or standard menu. Lastly, to investigate differential responsiveness by the type of menu offered, we create a set of indicator variables for each menu that appears in our analytic samples. These variables capture whether a student is served an alternative menu, express menu, food court menu, K-8 standard menu, MS standard menu, or HS standard menu.

Analytic Samples

The analytic "stable school" sample includes students in grades 6 through 12 attending traditional public schools with POS systems serving a single menu and excludes students in charter and special education schools, and those who change schools.[17,18] To leverage the longitudinal nature of the data, students must also be observed in at least two years. An alternative "Full" sample includes students who change schools between academic years during the sample period.[19]

Table 1 presents descriptive statistics pooled across AY 2014-2018, beginning with those for all NYC middle and high school students in traditional public schools in column 1, those not included in the stable school and full samples in columns 3 and 6, and for the stable school and full samples in columns 2 and 5. The stable school sample includes roughly 330,000 unique students across 787 schools and 890,000 student-year observations. Reflecting the diversity of the district, the stable school sample is majority-minority and nearly half of all students speak a primary language other than English at home. Almost all students have been certified eligible for a free or reduced-price lunch at least once. The full sample is similar to both the stable school sample and the population of all students and includes roughly 480,000 unique students across 885 schools and over 1.4 million student-year observations. Neither the students in the stable school sample nor the full sample are meaningfully different from their excluded counterparts, as shown in columns 4 and 7. For each of the two samples, **Table 2** presents descriptive statistics

[17] Appendix **Table A1** depicts the distribution of schools serving middle and high school students with POS systems across the sample period. Point-of-service systems were introduced in the mid-2000's and the number of schools adopting them has readily increased since. Students in schools with POS systems are not, however, qualitatively different than those in non-POS schools on observable characteristics, as seen in columns 2 and 3 of Appendix **Table A2**, nor are they substantively different from the general population of students.

[18] To be included in the sample, observations must also have valid attendance (non-missing or non-zero) so that lunch participation measures can be constructed, but this condition only drops a very small fraction (<0.01%).

[19] This sample still excludes students who switch schools within an AY since the precise timing of a school switch cannot be determined. Less than 1% of the sample switches schools within an academic year.

for students who are ever and never exposed to a new menu. The characteristics shown in columns 3 and 6 do not reveal any meaningful differences across the two types of students in either sample.

Table 3 shows the variation in the two samples. The average student is observed roughly three times in both samples. Either by switching schools or their school deciding to make changes between academic years, 33 percent of the full sample and 7 percent of the stable school sample experience at least one new menu, while about a third of students in both samples get a new principal. As seen in **Table 4**, there is substantial variation in the types of menus served across and within schools over time in the stable school sample between AY 2015 and AY 2018.

Turning again to **Table 2**, roughly 87 percent of students in the stable school sample participate in lunch at least once, 72 percent at least 10 times, and 65 percent at least 20 times in an AY across the sample period. The average lunch participation rate across all years is 40 percent, though there is substantial heterogeneity across students from different backgrounds, as shown in **Figure 5** for AY 2017. As seen in Panel A, Asian or Other students are most likely to participate in school lunch, followed by Hispanic, Black, and White students. Lunch participation is about 10 percentage points higher among students who primarily speak a language other than English at home as compared to those who speak English at home.[20] Male students are eight percentage-points more likely to participate in school lunch as compared to females. Students never certified eligible for a free or reduced-price lunch have the lowest participation rate at 20 percent. Lunch participation also declines as students age—from over 50

[20] The relatively higher lunch participation rates for Asians/Other students in New York, as compared to nationally, may be related to their nativity status, as students from immigrant backgrounds may be more likely to participate in school lunch. While we do not know students' place of birth, we proxy for this using primary language spoken at home. Among Asian/Other students, 68% primarily speak a language other than English at home. This figure is also high among Hispanic students at 64%, but low for Black and White students, at 10% and 34%, respectively. Among Asian/Other, Hispanic, Black, and White students who primarily speak English at home, lunch participation rates are 37%, 40%, 38%, and 25%, respectively.

percent in grade six to under 30 percent in grade 12—with a steep drop-off between grades eight and nine, as shown in Panel B.

Empirical Strategy

Baseline Model

The centerpiece of our empirical work is a student fixed-effects model linking lunch participation to new menus as follows:

$$(1)\ Participation_{ist} = \beta_0 + \beta_1 Post\ New\ Menu_{ist} + \beta_2 \boldsymbol{X}_{ist} + \beta_3 \boldsymbol{Z}_{st} + \delta_g + \lambda_t + \alpha_i + \varepsilon_{i,s,t}$$

where subscripts *i*, *s,* and *t* represent student, school, and academic year, respectively. When investigating the association between a new menu and propensity for lunch participation (i.e., likelihood of participating in lunch), the outcome—$Participation_{ist}$—is *EverLP*. For lunch participation intensity (i.e., frequency of lunch participation), the outcome of interest is *LPrate*. $Post\ New\ Menu_{ist}$ captures the overall effect of a new menu. $\boldsymbol{X}_{ist}$ and $\boldsymbol{Z}_{st}$ are vectors of time-varying student and school characteristics including primary language spoken at home and participation in special education services, and UFM status, total enrollment, and new principal, respectively. Grade, year, and student fixed-effects are represented by δ_g, λ_t, and α_i, respectively. Standard errors are clustered by school as decisions regarding menus are made at the school level and students are clustered in schools.[21] To estimate the effect of a new menu over time, we estimate the same model substituting *Post New Menu 1st AY* and $Post\ New\ Menu\ gt.\ 1\ AY_{ist}$ for $Post\ New\ Menu_{ist}$.

We estimate this model on the stable school sample, which also controls for time-invariant school characteristics since students in this sample do not change schools. Estimated coefficients will capture causal effects if school adoptions of new menus are unrelated to student

[21] We make these clustering choices following Cameron and Miller (2015).

characteristics and if there are no other concurrent changes in school policies, practices, and characteristics that might affect lunch participation rates. The student fixed-effects serve to control for unobserved student and school specific time-invariant factors that might influence their participation in lunch.[22] The addition of controls help to account or proxy for important time varying factors, such as changes in the price of school lunch, cafeteria capacity, and changes in lunch policies. Grade fixed-effects help to control for factors common to all students in a particular grade that may influence lunch participation (e.g., taste preferences at a particular age) while year fixed-effects help to control for shocks that affect all students in a particular year (e.g., food fads). We show evidence to support the assumption that the adoption of new menus is unrelated to school characteristics below.

We do not include weight controls in our primary specification to avoid dropping observations, as we do not have data for roughly 20% of the students in both our full and stable school samples.[23] As a robustness check, we estimate a model including controls for whether a student is underweight, overweight, or obese. Additionally, we also estimate the baseline model on the more inclusive full sample of students, which includes students who change schools between academic years. For this analysis, we amend the specification above to include school fixed-effects and a control for school changes.[24,25]

[22] At the student level, these characteristics may include intrinsic motivation for seeking out substitutes for school meals or family background. At the school level, these may include the social setting, time of day lunch is served and how long students are given to eat, or whether school faculty and staff spend time in the cafeteria and eat with students, among other lunch practices.

[23] Dropping these observations also eliminates much of the variation in new menus. In the stable school sample, we lose 15% of students exposed to a new menu.

[24] As another robustness check, we substitute school fixed-effects for student fixed-effects in the baseline model. In these specifications, the vector of control variables also includes student race and ethnicity, gender, and FRPL status.

[25] As a falsification test, we also estimate the baseline model substituting lunch participation outcomes with measures of breakfast participation.

Heterogeneity Analysis

The differences in lunch participation rates across students documented above also suggests that the relationship between new menus and lunch participation may vary across race and ethnicity, gender, economic status, and grade. Some menus may, for example, better cater to the taste preferences of particular demographics by offering healthier or more culturally diverse meals. It may also be that older or higher-income students with greater access to school lunch alternatives may react differently to menus that offer greater daily variety as compared to their younger or lower-income peers. We explore differential responsiveness to new menus by estimating the baseline model separately for each set of characteristics and interacting *Post New Menu* with each characteristic as follows:

$$(2)\ Participation_{ist} = \beta_0 + \beta_1 Post\ New\ Menu_{ist} * \boldsymbol{Student\ Characteristics}_{ist} + \beta_2 \boldsymbol{X}_{ist} + \beta_3 \boldsymbol{Z}_{st} + \delta_g + \lambda_t + \alpha_i + \varepsilon_{i,s,t}$$

The specifications for race and ethnicity include interactions for Asian/Other, Hispanic, Black and White. The specification for gender includes interactions with binary variables for male and female. The specification for FRPL status includes interactions with binary variables for never FRPL and ever FRPL. Lastly, the specification for grade includes interactions with binary variables for students in middle and high school.

Exploring Moderating Factors, Mechanisms, and Indirect Effects

As an extension to our main analysis, we explore complementarities between the introduction of new menus and UFM, which not only eliminates prices but may also reduce the stigma associated with school lunch (Schwartz & Rothbart, 2019). If the cost of meals or stigma associated with school lunch constitute major barriers to participation, then students may be most

responsive to new menus in schools that have adopted UFM. To examine this, we estimate the following model:

$$(3)\ Participation_{ist} = \beta_0 + \beta_1 Post\ New\ Menu_{ist} * UFM_{ist} + \beta_2 Post\ New\ Menu_{ist} * No\ UFM_{ist} + \beta_3 \boldsymbol{X}_{ist} + \beta_4 \boldsymbol{Z}_{st} + \delta_g + \lambda_t + \alpha_i + \varepsilon_{i,s,t}$$

We also explore potential mechanisms by examining the relationship between specific menu types and lunch participation. We do this in two ways. First, we assess the extent to which the adoption of non-standard menus drive the relationship between new menus and participation. While the standard menus offer a greater number of food items and more variety than most of the non-standard menus, the latter have unique features that might make them more or less appealing to students. We do this as follows:

$$(3)\ Participation_{ist} = \beta_0 + \beta_1 Non\text{-}Standard\ Menu_{ist} + \beta_2 Post\ New\ Menu_{ist} * Standard\ Menu_{ist} + \beta_3 Post\ New\ Menu_{ist} * Non\text{-}Standard\ Menu_{ist} + \beta_4 \boldsymbol{X}_{ist} + \beta_5 \boldsymbol{Z}_{st} + \delta_g + \lambda_t + \alpha_i + \varepsilon_{i,s,t}$$

Second, we estimate models relating specific menu types to participation using indicator variables for HS standard, MS standard, K-8 standard, alternative, express, and food court menus.

For our final extension, we examine potential linkages between new menus and attendance and weight outcomes by substituting lunch participation outcomes in the baseline model with measures of attendance and weight.

Main Results

New Menus and Lunch Participation

Regression results for the relationship between new menus and the propensity for lunch participation (*EverLP*) are shown in columns 1 through 3 of **Table 5**. As seen in panel A, the adoption of a new menu is associated with a 3-percentage point increase in the likelihood of a

student participating in school lunch at least once and a 4-percentage point increase for participating at least 10 times. The estimated effect for the likelihood of participating at least 20 times is positive and similar in magnitude though statistically insignificant. Panel B presents estimates for the effect of a new menu over time. The first year of a new menu is associated with an increase in the likelihood of participating at least once, 10 times, and 20 times in an AY by 2.7, 4.1, and 3.4 percentage points. These effects may persist in later years as the likelihood of participating at least once is often larger in the years after adoption. Column 4 presents the results for lunch participation intensity (*LPrate*). The adoption of a new menu is associated with a 2.5 percentage point increase in lunch participation rates overall. As seen in panel B, the effect is concentrated in the first year of an adoption, when lunch participation rates increase by 2.6 percentage points on average.

These estimates are meaningfully large, constituting a 4 to 6 percent increase in the share of students participating in school lunch at least once and ten times in an academic year relative to baseline rates of 85 and 71 percent. The estimates are similarly large for lunch participation rates, translating to a 6.5 percent increase in lunch participation rates among all students in the sample. Together, these results suggest that the introduction of a new menu increases the propensity for and intensity of lunch participation. Although imprecisely estimated, the analysis of the effect of new menus over time suggests that their effect may persist over time, implying that specific features of menus may be important and that novelty may not be the sole mechanism involved.

Differential Responsiveness to New Menus Across Students

Figure 6 present the results for the propensity and intensity of participation by student race and ethnicity, gender, FRPL status, and grade. The adoption of new menus is associated

with increases in the propensity for participation by 3.2 percentage points among Black students and 8.6 percentage points among White students, while estimates for the participation of Asian and Hispanic students are smaller and statistically insignificant. The associated increase in the propensity for participation among females is 4.3 percentage points, twice that of males, and 14 percentage points among never FRPL students, seven times that of students ever certified for FRPL. New menus seem to have no differential effect on middle school students but increase the propensity for participation among high school students by 5.5 percentage points.[26]

Turning to lunch participation intensity, the results show increases of 4 and 5 percentage points among Black and White students, respectively. Lunch participation rates also increase by about 3 percentage points for male and high school students. As before, the largest effects are observed among never FRPL students, whose lunch participation rates increase by 7.5 percentage points. To put these estimates into perspective, the effect of a new menu on participation intensity for White and never FRPL students is roughly half to two-thirds the size of the effect that UFM has been found to have on lunch participation rates among "non-poor" students (11 percentage points) and on par with or larger than the effect on "poor" students (5.4 percentage points) (Schwartz & Rothbart, 2019). These substantial increases suggest that these students may be better served when their schools introduce new menus.[27]

Furthermore, participation in school lunch has been found to be higher among those eligible for free or reduced price meals relative to those not, younger students, and, in other contexts, Hispanics and Blacks relative to Whites and Asians (Dunifon and Kowaleski-Jones, 2003; Newman & Ralston, 2006; Gordon & Fox, 2007; Fox & Condon, 2012). Given the

[26] We find similar results when using $EverLP_{10}$ and $EverLP_{20}$ as dependent variables. Results available upon request.
[27] Regression results presented in Appendix **Table A14**. We also run the analysis on the full sample. Results are largely consistent and shown in Appendix **Table A15**.

benefits that school lunch may have for student health and academic outcomes, heterogeneity in lunch participation could have substantial implications for disparities in the growth and development of children and adolescents from different backgrounds. The results therefore also suggest that offering new menus, perhaps those satisfying a broader range of tastes, could be one way that school districts can close racial and socioeconomic gaps in lunch participation and, potentially, disparities in academic and health outcomes.

Extensions

Responsiveness to New Menus in UFM Schools

Table 6 presents the results for the relationship between new menus, UFM, and lunch participation. The introduction of UFM has a large and statistically significant positive effect on lunch participation, increasing the propensity for participating by 8 to 9 percentage points and the intensity of participation by about 4.5 percentage points. The estimated coefficients for the effect of a new menu in schools without UFM across the different lunch participation measures are positive but statistically insignificant and qualitatively smaller than their counterparts for schools offering free meals to all students, wherein the introduction of a new menu increases the propensity for participation by 4 to 5 percentage points and the intensity of participation by roughly 3 percentage points. One implication may be that the introduction of free meals could make students more responsive to trying new foods, either by eliminating prices, reducing stigma, or both. However, the two sets of coefficients are not statistically distinguishable from one another and so effects of new menus in non-UFM schools cannot be ruled out.[28]

[28] We also run the analysis on the full sample. The results remain consistent and are shown in Appendix **Table A16**.

Relationship between Specific Menu Types and Lunch Participation

We explore potential mechanisms underlying the link between new menus and lunch participation by exploring how the relationship varies across the different types of menus offered. We start by examining whether the adoption of new non-standard menus drives our main results. The results in **Table 7** suggest that much of the relationship between new menus and lunch participation may be driven by the adoption of new non-standard menus, which is associated with a 6 to 7 percentage point increase in the propensity for participating and a 6.4 percentage point increase in the intensity of participation. The null hypothesis of equality for the coefficients on the two interaction terms are rejected at conventional significance levels for the measures of participating at least one and 10 times in a school year. The estimates for participating at least 20 times and lunch participate rates are not, however, statistically different from their counterparts for the introduction of a new standard menu, which are positive but statistically insignificant and qualitatively smaller.[29]

Figure 7 shows the results further disaggregating menus by specific types.[30] Relative to the HS Standard menu, the most common menu served across schools in the analytic samples, the food court menu is associated with large increases both in the propensity and intensity of participation. This might be expected as the food court menu offers several likely quite popular options (e.g., pizza and popcorn chicken) daily in addition to what is served as part of the HS standard menu, which indicates that either the type of meals offered, variety, or both are important factors for inducing greater lunch participation. Furthermore, the food court menu's

[29] Testing the null hypothesis of equality for the coefficients on the two interaction terms produces p-values of 0.08 for $EverLP_1$, 0.02 for $EverLP_{10}$, 0.16 for $EverLP_{20}$, and 0.10 for *LPrate*, respectively. We also ran the analysis on the full sample and find similar results, as shown in Appendix **Table A17**.
[30] Regression results presented in Appendix **Table A18**.

self-service feature might also add to the appeal of school lunch by providing students with a greater sense of autonomy and reducing wait times in lines. In schools that also underwent renovations, changes to the cafeteria environment may have also mattered.

Though imprecisely estimated, the magnitude and direction of the coefficients for the express menus across all measures of participation likely rule out the potential for any positive effects from these menus. While "grab and go" options may reduce wait times for lunch in the cafeteria, these menus offer a limited set of options comprised mostly of hot and cold sandwiches or salads that may be, or become over time, unappealing to many students. Similarly, though imprecisely estimated, alternative menus serving more scratch-cooked and less processed foods may increase the propensity for participation. Together, these results suggest that the type of meals served and how they're prepared may also matter.

Consequences for Other Outcomes

In addition to lunch participation, new menus may also have notable indirect effects. Offering a new menu may entice students to attend school more regularly so as to take advantage of school lunch. Of particular concern, lunch participation induced by the introduction of new menus may have consequences for weight outcomes through consumption of school meals. **Table 8** presents the results for the relationship between new menus, attendance, and weight. The introduction of a new menu is not associated with changes in attendance rates. Importantly, new menus are also unrelated to worse weight outcomes.[31] These results are consistent with those

[31] It is worth noting that height and weight measurements are captured in the first three months of fall for roughly half of students. As such, the results for the weight outcomes may reflect only short run associations. Results were consistent when restricting the analysis to students whose weight and height data was collected in the months after December. Results available upon request. We also run the analysis on the full sample and find similar results, as shown in Appendix **Table A19**.

of Schwartz and Rothbart (2019), which found the introduction of UFM to have no effect on attendance and no evidence of adverse effects for weight outcomes.

Probing the Main Results

Robustness Checks

We test the robustness of the main results in four ways. First, we explore the sensitivity of our findings to sample composition. To assess the generalizability of our findings to a larger population of students, we expand the sample to include those who change schools between academic years and re-estimate the student fixed-effects models above. In this more inclusive sample, the effect of a new menu is identified by variation in exposure to new menus within students over time due both to their schools adopting new menus and students changing schools between academic years. As seen in **Table A3** of the Appendix, the results are similar. To further test the sensitivity of our results to sample composition, we restrict the stable school sample to those students who ever experience a new menu. If students who are ever exposed to a new menu differ from those who are not, our initial impact estimates may be spurious. As shown in **Table A4**, the estimates for this smaller sample are unsurprisingly less precise but remain positive. As a last check, we restrict the stable school and full samples to students with a history of lunch participation—defined as having participated in school lunch at least once in period t-1—and rerun the analysis for lunch participation intensity, again finding similar results as seen in **Table A5**.

Second, we employ school fixed-effects instead of student fixed-effects. While the student fixed-effects models estimated above rely on variation in exposure to new menus within students over time, school fixed effects models are identified based on new menu adoptions within schools over time. In the stable school sample, 3,904 students are first observed the year

their school adopts a new menu, but do not experience a new menu thereafter. In the school fixed-effects models using the stable school sample, these students are now treated as being exposed to a new menu. The results are shown in **Table A6** and remain consistent though estimated with less precision. We repeat this exercise for schools in the full sample, treating students as exposed to a new menu if the school they are attending adopts a new menu, and find similar results as seen in **Table A7**.

Third, we include weight control variables for whether a student is underweight, or overweight or obese. **Table A8** presents the results. Although the sample changes due to missing data for roughly a fifth of students, the results remain consistent. Lastly, to determine whether our key findings are driven by cafeteria renovations rather than adoption of new menus, we restrict the stable school sample to exclude exposure to the food court menu, which was sometimes coupled with changes to the food environment. Although we lose substantial variation in this smaller sample, the results, presented in **Table A9**, remain consistent with our key findings, though the effect on lunch participate intensity is smaller and statistically insignificant. The robustness of our results to alternative samples, identifying assumptions, and model specifications lend support to a causal interpretation of our impact estimates.

Do Observables Predict New Menus?

A causal interpretation of our main impact estimates requires the identifying assumption that new menu adoptions at the school level are unrelated to student characteristics and lunch participation. To test the plausibility of this assumption, models with school-fixed effects are estimated using time-varying school level characteristics in period *t* to predict the adoption of a new menu in period *t+1*:

$$New\ Menu_{s,t+1} = \beta_0 + \theta \boldsymbol{Student\ Composition}_{s,t} + \lambda Year_t + \alpha_s + \varepsilon_{s,t}$$

Where "*Student Composition"* variables are school level aggregates of the student variables defined above. We do this for all schools in the stable school sample and those with no new menu adoption in period t.[32] Qualitatively small and statistically insignificant estimates would provide support for a causal interpretation of our main results.

The regression results at the school level for the adoption of a new menu are presented in Appendix **Table A10**. The estimates are qualitatively small across both specifications, particularly the coefficients on the measures for *EverLP* and *LPrate*, which are near zero. The F-statistics for the joint significance of the time-varying variables are also small.

It may also be problematic if particular types of students are more or less likely to be exposed to a new menu, as that might suggest selection into schools. Parents are unlikely to know in advance of the academic year if principals will decide to introduce a new menu or to prioritize school meals—among all school characteristics—when choosing schools for their children. Nevertheless, we examine whether student characteristics can predict exposure to a new menu by repeating the above test at the student level. The results are shown in appendix **Table A11**. Again, the estimates are close to zero and mostly statistically insignificant, with low F-statistics.[33]

[32] We also run models including variables for percent of students in each of grades 7 through 12, enrollment, linear and quadratic terms for principal tenure, adoption of UFM in period t+1, and new principal in period t+1. Estimates in these models remain qualitatively small and statistically insignificant with low F-statistics for joint significance. Additionally, we perform this test restricting the analysis to only those schools that ever adopt new menus. Though the coefficients in this smaller sample are less precisely estimated and thus statistically insignificant, they remain qualitatively small. Results available upon request.

[33] We run the student level analysis including indicator variables for underweight, overweight, and obese. Results remain qualitatively small and statistically insignificant with low F-statistics for joint significance. We also run the analysis restricting the stable school sample to students who are ever exposed to a new menu. Again, estimates are qualitatively small and statistically insignificant, with low F-statistics for joint significance. Results available upon request. Lastly, we find similar results when we run the analysis using only school fixed-effects and including controls for student race and ethnicity. Results available upon request.

The size of the coefficients and statistical insignificance of estimates across both sets of models suggest that school and student characteristics do not predict menu changes, thereby further boosting confidence in a causal interpretation of the main results.

Falsification test

If the link between a new menu adoption and student lunch participation is causal, then a future new menu adoption should have no impact on current outcomes and estimates should be statistically insignificant. Otherwise, our estimates may be reflecting changes in participation that pre-date and perhaps precipitate a new menu adoption. We test by recoding our "Post New Menu" variable to take a value of one in the year prior to a new menu adoption, "Early Post New Menu," and re-run the analysis as above. Since this new variable conflates an incorrect and correct timing of a new menu adoption, we would expect the estimates to be qualitatively smaller than the estimates from our main specification, statistically insignificant, or both. As seen in Panel A of **Table A12**, estimates are smaller and statistically insignificant. Examining the effect overtime in Panel B reveals statistically significant estimates in line with those above for "Early Post New Menu gt. 1 AY," which reflects the correct timing of a new menu adoption.

Additionally, we examine the relationship between a new menu and student participation in school breakfast. Since the menus considered in this analysis pertain to lunch, it would be surprising if we found similar effects on breakfast participation. We rerun the baseline model substituting the lunch participation outcomes for measures of breakfast participation and present the results in **Table A13**. As expected, a new lunch menu is not related to breakfast participation. These tests are further evidence in support of a causal interpretation of our key findings.

Conclusions

As the second largest food assistance program in the United States, the NSLP can improve nutritional intake and reduce the prevalence of food insecurity among children and adolescents. Participating in school lunch is also associated with better academic outcomes, which benefits both students and schools. However, realizing these benefits requires that students participate, but nearly half of all school-aged students do not take advantage of the program. In response to common criticism that meals served by schools are unappealing and therefore deterring student participation, school districts nationwide have increased efforts to reformulate their menus. Yet the link between menus and lunch participation has to date not been rigorously investigated. This paper begins to fill this gap.

Using unique, detailed longitudinal data on students, schools, and lunch menus in NYC, this study suggests that introducing new menus increases student participation in school lunch. Students who are exposed to new menus are more likely to participate in school lunch and participate more frequently. These results are robust to different samples, identifying assumptions, and model specifications, thereby encouraging a causal interpretation of the results. Furthermore, stratifying the analysis by student characteristics suggests increases in participation among Black, White, female, higher income, and high school students—all groups with low participation rates in New York City. New menus may therefore help to close demographic and socioeconomic gaps in lunch participation.

Exploring the relationship between new menus and UFM suggests that the effect may be driven primarily by students in schools offering free meals, which could indicate that prices or stigma constitute a barrier to lunch participation that the introduction of new menus alone cannot overcome. Effects in non-UFM schools could not be ruled out, however. Disaggregating the

analysis by specific menu types reveals that what is served, how it is served, and the environment in which it is served also matters, and that innovative non-standard menus may be more effective. The food court menu—offering the greatest variety, daily access to popular food items, self-service stations, and, in some cases, including cafeteria renovations—is associated with the greatest increase in lunch participation. The alternative menus—offering more scratch cooked and less processed meals—may also increase the share of students participating, though likely not how often they participate. In contrast, the express menus serving mostly hot and cold sandwiches are associated with lower participation. Together, these results suggest that the variety, type, and preparation of meals served as part of menus matter, but that the method and environment in which they are served is also important. We plan to explore these nuances further in future work. Lastly, we find no evidence that new menus are related to attendance or adverse weight outcomes.

While New York City offers a unique opportunity to study the relationship between school menus and student lunch participation, it is worth noting that the findings of this study may not be generalizable to other contexts. Although the New York City school district reflects the growing diversity of the United States' middle and high school student population and has implemented innovative approaches to school meals similar to those trending nationwide, it enjoys economies of scale that other, smaller districts may not have. To the extent that scale matters, the results presented in this study may be most applicable to other large school districts or groups of districts that have formed purchasing cooperatives. Arguably, however, the insights offered in this study can help school officials make more informed decisions about their lunch programs and provides evidence in support of the efforts that many are currently taking to reformulate their menus.

To summarize, this paper presents the first large-scale, rigorous evidence on the link between school menus and lunch participation, and suggests that the efforts of school districts nationwide to introduce new menus may prove fruitful. However, our findings also suggest reformulated menus alone are not a panacea, and that the cost of meals to students and features of menus and the cafeteria environment warrant serious consideration. Furthermore, while it is important to get students to participate in school lunch, it is also important that they make the appropriate dietary choices as they go through the lunch line (e.g., Toossi, 2017). Future work should also assess the effect that new menus have on the student's dietary choices.

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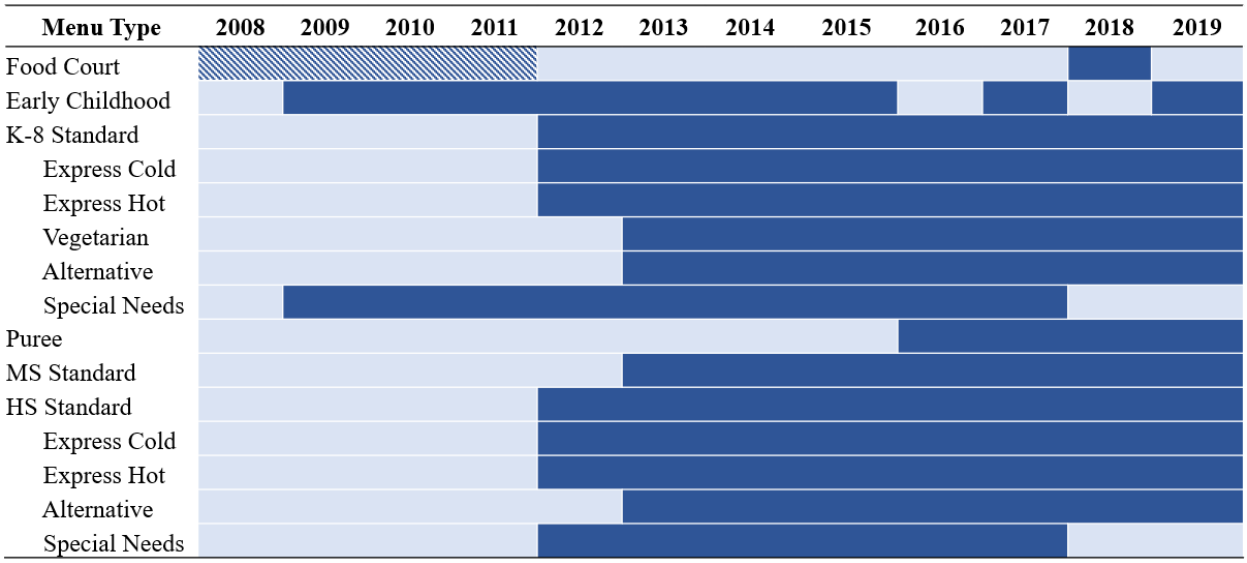
Figures & Tables

Figure 1. Lunch Menus Offered by Schools by Year



Note: Figure depicts the lunch menus available to schools by year as listed on the OFNS website. Dark blue bars indicate menu availability. The food court menu available in years 2008-2011 differs from that introduced in 2018. “HS” denotes high school, “MS” denotes middle school, and “K-8” denotes kindergarten through eighth grade. “X” marks menu availability.

Figure 2. Overlap across Menus in the Types of Food Items Offered

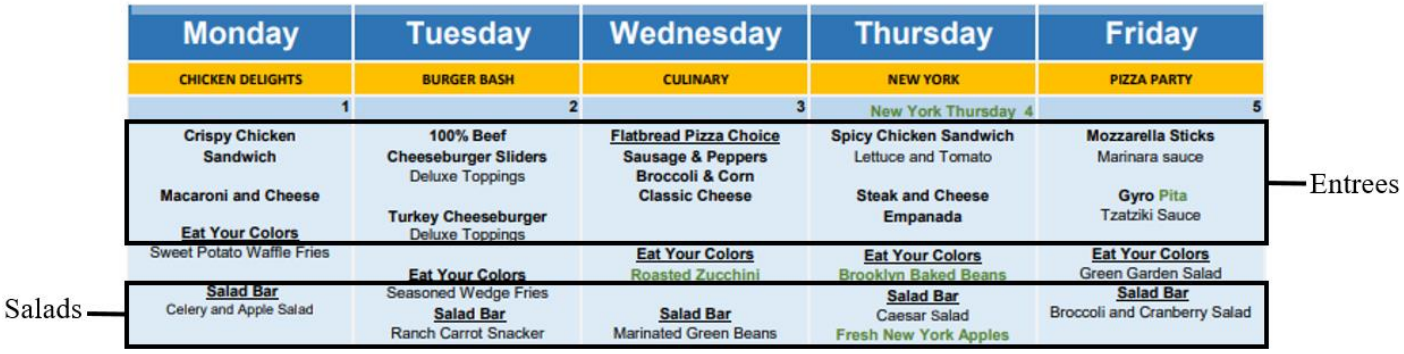
Menu	HS Standard	HS Alternative	HS Express Cold	HS Express Hot	MS Standard	K-8 Standard	K-8 Alternative	K-8 Express Cold	K-8 Express Hot
HS Standard	108								
HS Alternative	52	65							
HS Express Cold	11	9	21						
HS Express Hot	38	19	5	40					
MS Standard	87	49	13	36	94				
K-8 Standard	75	48	12	32	80	83			
K-8 Alternative	45	49	10	13	43	43	54		
K-8 Express Cold	12	10	20	5	14	13	11	24	
K-8 Express Hot	45	21	6	34	44	41	19	6	48

Note: Figures in the diagonal cells represent the total number of food items in a menu. Figures in other cells represent the number of food items that appear on any two menus. Data for AY 2017.

Figure 3. Menu Service Schedules

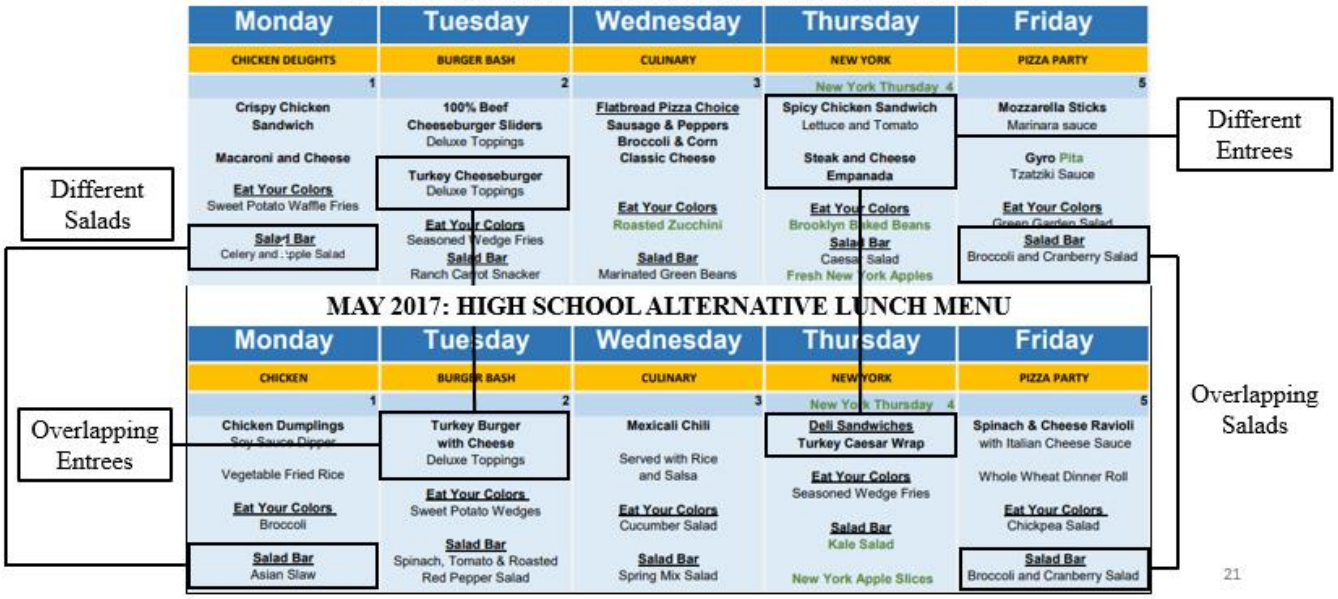
Panel A.

MAY 2017: HIGH SCHOOL STANDARD LUNCH MENU



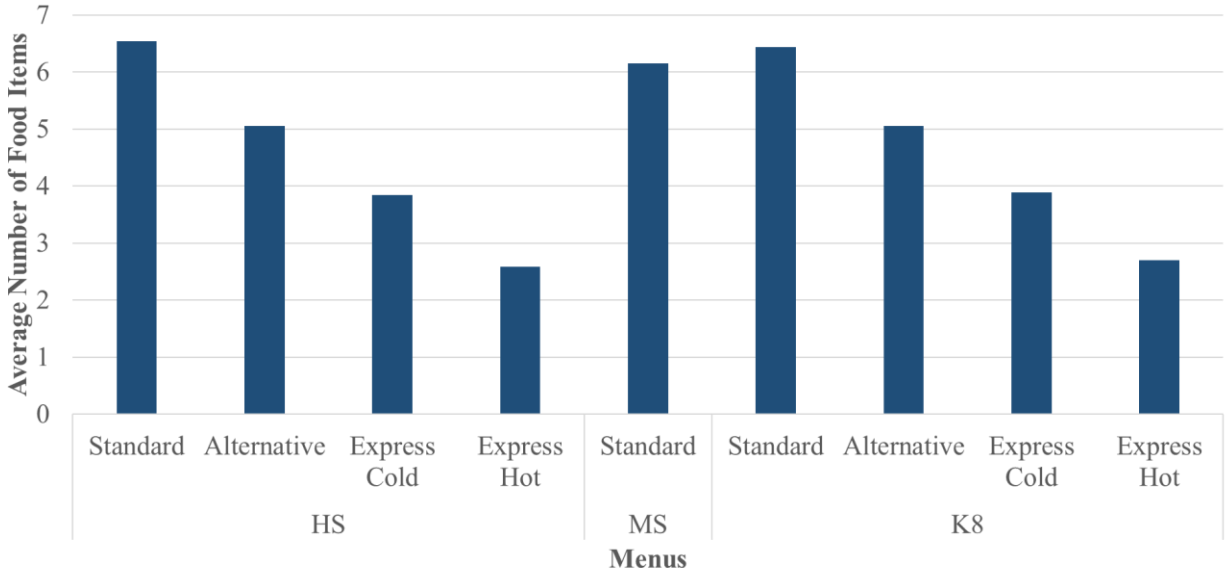
Panel B.

MAY 2017: HIGH SCHOOL STANDARD LUNCH MENU



Note: Panel A depicts food items served as part of the HS standard menu for the week of May 1st through May 5th of 2017. Panel B depicts food items served across the HS standard and alternative menus for the same week in 2017.

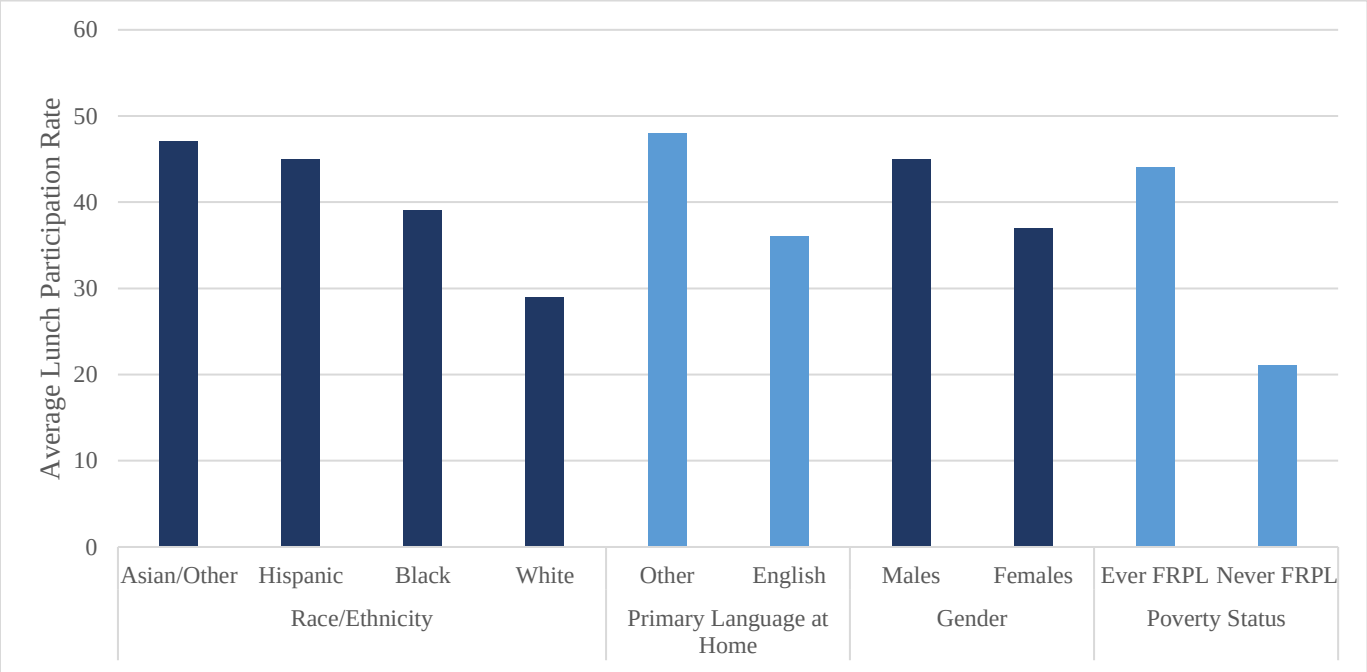
Figure 4. Average Number of Food Items Available Daily Across Menus



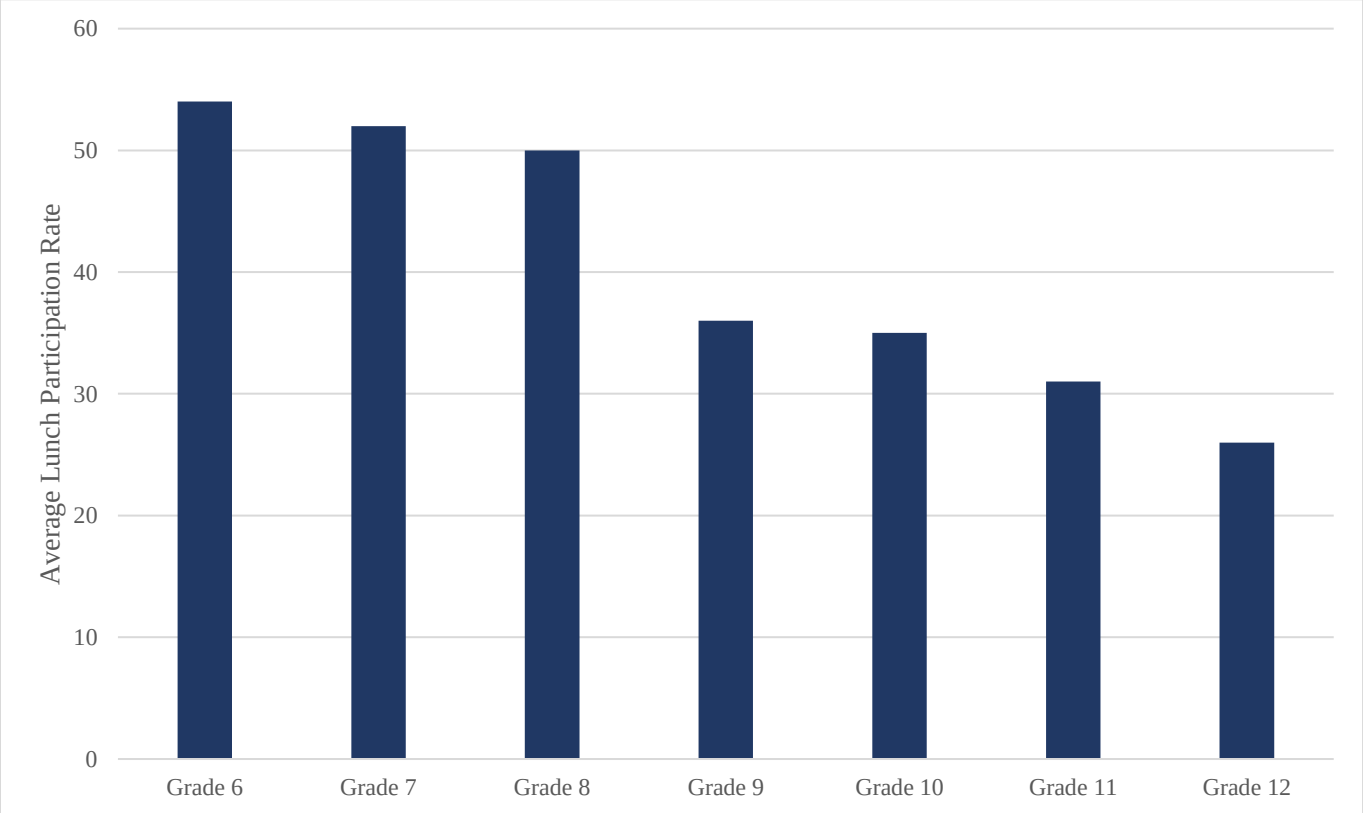
Note: Figure depicts the average number of food items offered daily for lunch across menu types for AY2017.

Figure 5. Differences in Lunch Participation Rates Across Students

Panel A: By Student Characteristics

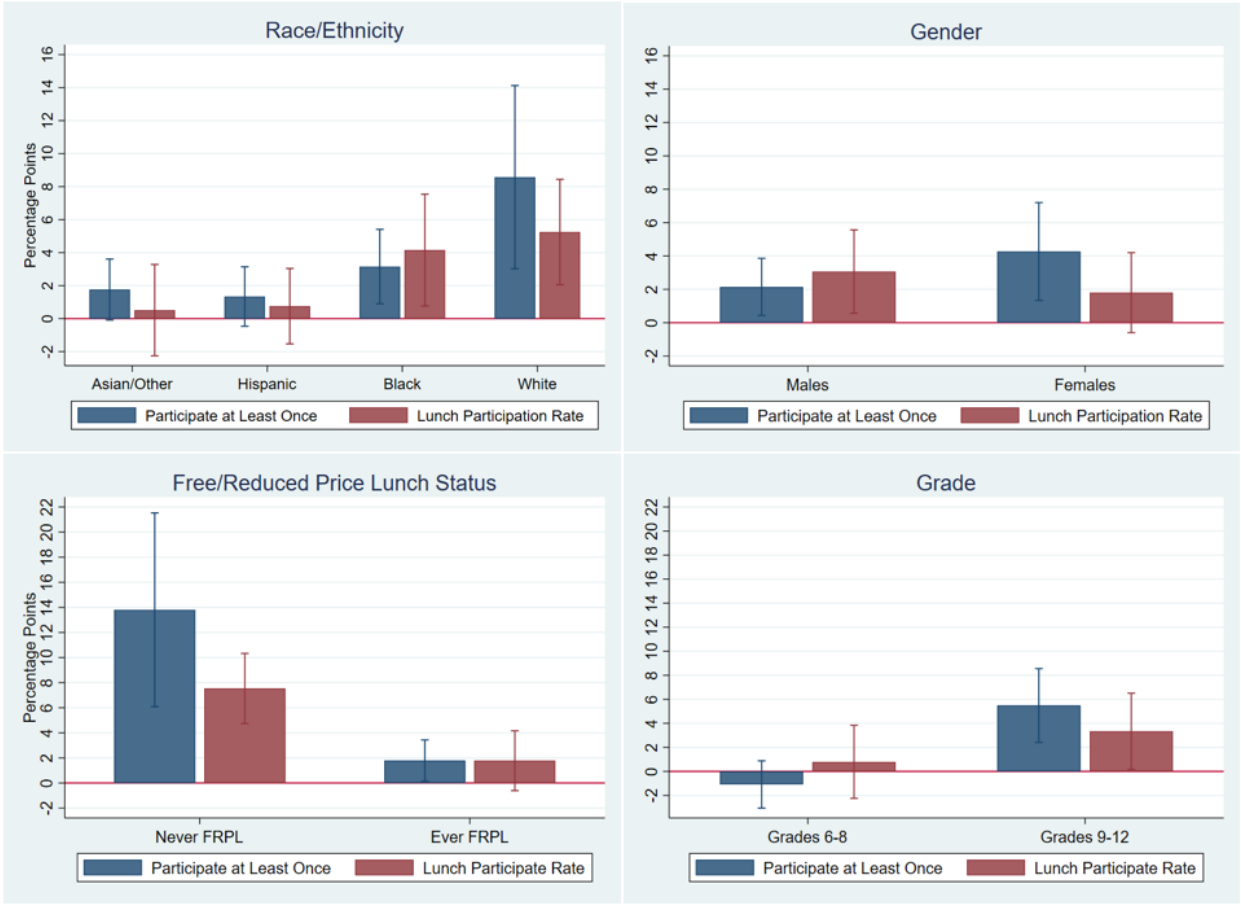


Panel B: By Grade



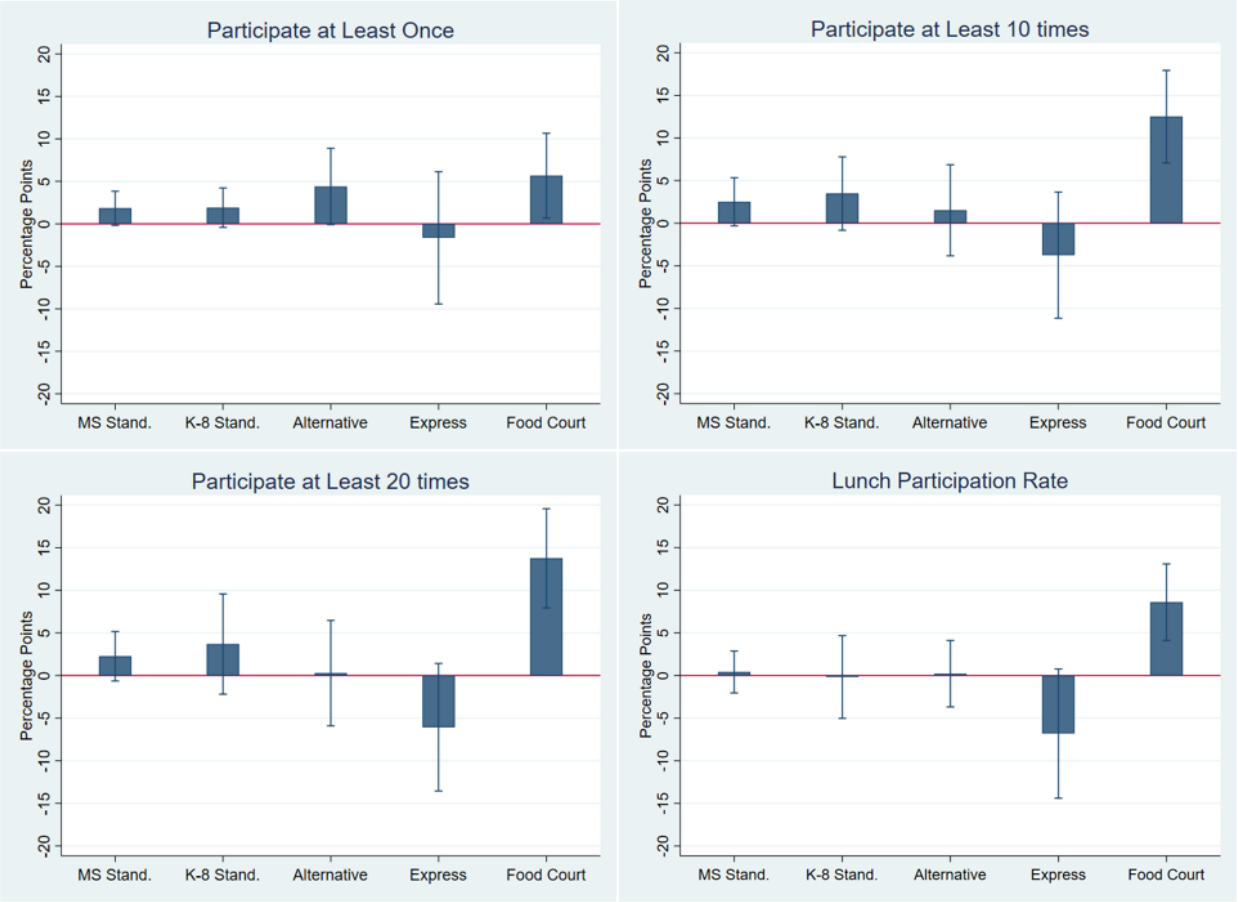
Note: Descriptive statistics for AY2017

Figure 6. Differential Responsive to New Menus by Student Characteristics



Note: Stable school sample including grade 6 through 12 traditional public school students who do not change schools and attend schools serving a single menu. All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school UFM status, principal change, and enrollment. Confidence intervals are shown at the 90% level.

Figure 7. Relationship between Specific Menu Types and Lunch Participation



Note: Stable school sample including grade 6 through 12 traditional public school students who do not change schools and attend schools serving a single menu. All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school UFM status, principal change, and enrollment. Reference category in each is the HS standard menu. Confidence intervals are shown at the 90% level.

Table 1. Descriptive Statistics for Analytic Samples

Variable	All in NYC (1)	Stable School (2)	Not in Sample (3)	Diff. (4)	Full (5)	Not in Sample (6)	Diff. (7)
Hispanic	.40	.39	.40	-.01	.39	.41	-.01
Asian/Other	.19	.20	.20	.00	.21	.17	.04
Black	.26	.26	.26	.00	.25	.28	-.03
White	.15	.15	.15	.00	.15	.14	.01
Non-English at Home	.47	.46	.47	-.01	.47	.46	.01
Female	.50	.49	.49	.00	.50	.49	.01
Ever FRPL	.91	.91	.91	-.01	.91	.90	.01
SWD	.14	.14	.14	.00	.14	.14	-.01
Attendance Rate	83	84	82	2.00	85	78	7.00
In UFM School	.45	.42	.49	-.07	.49	.41	.08
Students	844,216	334,126	510,090		478,238	365,978	
Observations	2,331,963	889,613	1,255,294		1,406,789	642,762	
Schools	1,013	787	1013		885	1013	

Note: Descriptive statistics pooled across AY 2014-2018. Column 1 presents statistics for all grade 6-12 students attending traditional public schools in NYC. Columns 2 and 5 present statistics for students in the stable school and full samples, respectively. Columns 3 and 6 do so for those not included in the samples. Columns 4 and 7 present the differences between students included and excluded from the samples.

Table 2. Descriptive Statistics for Ever and Never New Menu Students in Analytic Samples

	Exposure to a New Menu					
	Stable School Sample			Full Sample		
	Ever	Never	Diff.	Ever	Never	Diff.
Variable	(1)	(2)	(3)	(4)	(5)	(6)
LPrate	40	40	0.00	41	40	1.00
$EverLP_1$	.87	.85	.02	.89	.85	.04
$EverLP_{10}$	.72	.71	.01	.76	.71	.04
$EverLP_{20}$	.65	.64	.01	.68	.65	.04
Hispanic	.41	.39	.02	.38	.40	-.01
Asian/Other	.15	.20	-.05	.22	.20	.02
Black	.27	.26	.01	.22	.26	-.04
White	.17	.15	-.02	.17	.14	.03
Non-English at Home	.43	.46	-.04	.48	.47	-.01
Female	.49	.50	-.01	.50	.50	.00
Ever FRPL	.89	.91	-.02	.91	.91	.00
SWD	.16	.13	.02	.13	.14	.00
Attendance Rate	84	85	-1.00	86	84	2.00
In UFM School	.44	.42	.02	.59	.43	.16
Students	20,278	313,848		132,367	345,871	
Observations	57,938	831,675		464,033	942,756	
Schools	90	770		882	858	

Note: Descriptive statistics pooled across AY 2014-2018. Columns 1 and 4 present descriptive statistics students in the stable school and full samples, respectively, that are ever exposed to a new menu. Columns 2 and 5 do so for students never exposed to a new menu. Differences in characteristics across the ever and never new menu students in each sample are presented in columns 3 and 6.

Table 3. Variation in Analytic Sample, AY 2014-2018

	Analytic Samples	
	Full	Stable School
VARIABLES	(2)	(4)
New Menu	.33	.07
New School	.37	-
New Principal	.33	.28
Years Observed	3.24	2.88
Students	478,238	334,126
Observations	1,406,789	889,613
Schools	885	787

Note: Table shows percent of students across all years that experienced a menu, school, or principal different than one they had the previous AY.

Table 4. Distribution of Menus across Schools

Menu Categories	AY 2014	AY 2015	Ay 2016	AY 2017	AY 2018
Standard	226	535	612	691	672
Non-Standard	14	32	49	58	64
Express	*5*	*8*	*15*	*23*	*20*
Alternative	*9*	*24*	*34*	*35*	*34*
Food Court	*0*	*0*	*0*	*0*	*10*
School Adopt A New Menu	-	5	19	26	53
Adopt a Standard	-	*3*	*10*	*16*	*34*
Alternative	-	*2*	*5*	*6*	*8*
Express	-	*0*	*4*	*4*	*1*
Food Court	-	*0*	*0*	*0*	*10*
Switch from					
Standard to Standard	-	*3*	*9*	*15*	*28*
Standard to Non-Standard	-	*2*	*9*	*10*	*18*
Non-Standard to Non-Standard	-	*0*	*0*	*0*	*1*
Non-Standard to Standard	-	*0*	*1*	*1*	*6*
Total Schools	240	567	661	749	736

Note: Values in cells represent the number of schools serving a specific menu type or adopting a new menu in each academic year and are not cumulative.

Table 5. Results for EverLP & LPrate

VARIABLES	$EverLP_1$ (1)	$EverLP_{10}$ (2)	$EverLP_{20}$ (3)	LPrate (4)
Panel A: Overall Effect				
Post New Menu	0.032**	0.042**	0.033	2.45*
	(0.013)	(0.017)	(0.020)	(1.42)
Panel B: Effect Over Time				
Post New Menu 1st AY	0.027**	0.041**	0.034*	2.60*
	(0.014)	(0.017)	(0.020)	(1.43)
Post New Menu gt. 1 AY	0.067*	0.049	0.022	1.27
	(0.037)	(0.032)	(0.031)	(2.34)
Average EverLP	.85	.71	.64	39.8
Observations	889,613	889,613	889,613	889,613
R-squared	0.688	0.719	0.708	0.77

Note: Stable school sample including grade 6 through 12 traditional public school students who do not change schools and attend schools serving a single menu. All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school UFM status, new principal, and enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table 6. Results for EverLP & LPrate, UFM

	$EverLP_1$	$EverLP_{10}$	$EverLP_{20}$	LPrate
VARIABLES	(1)	(2)	(3)	(4)
UFM	0.088***	0.080***	0.081***	4.476***
	(0.007)	(0.009)	(0.010)	(0.929)
Post New Menu*No UFM	0.015	0.030	0.009	0.572
	(0.020)	(0.019)	(0.024)	(1.577)
Post New Menu*UFM	0.039**	0.047**	0.043*	3.264**
	(0.017)	(0.020)	(0.022)	(1.608)
Average EverLP/LPrate	.85	0.71	0.64	39.8
Observations	889,613	889,613	889,613	889,613
R-squared	0.688	0.719	0.708	0.774

Note: Stable school sample including grade 6 through 12 traditional public school students who do not change schools and attend schools serving a single menu. All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school principal change and enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table 7. Results for New Non-Standard Menu

VARIABLES	$EverLP_1$ (1)	$EverLP_{10}$ (2)	$EverLP_{20}$ (3)	LPrate (4)
Non-Standard Menu	-0.004	-0.005	-0.011	-2.64
	(0.025)	(0.025)	(0.032)	(3.03)
Post New Menu*Standard Menu	0.009	0.024	0.014	0.34
	(0.008)	(0.015)	(0.022)	(1.83)
Post New Menu*Non-Standard Menu	0.062*	0.067*	0.062	6.41*
	(0.033)	(0.037)	(0.043)	(3.53)
Average EverLP/LPrate	.85	.71	.64	39.8
Observations	889,613	889,613	889,613	889,613
R-squared	0.69	0.72	0.71	0.77

Note: Stable school sample including grade 6 through 12 traditional public school students who do not change schools and attend schools serving a single menu. All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school UFM status, principal change, and enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table 8. Results for Other Outcomes

VARIABLES	Attendance Rate (1)	Underweight (2)	Overweight or Obese (3)	zBMI (4)	ln(BMI) (5)
Panel A: Overall Effect					
Post New Menu	0.041	0.004	-0.010**	-0.026	-0.005*
	(0.373)	(0.004)	(0.004)	(0.017)	(0.002)
Panel B: Effect Over Time					
Post New Menu 1st AY	0.118	0.005	-0.011**	-0.029*	-0.005**
	(0.364)	(0.004)	(0.005)	(0.017)	(0.002)
Post New Menu gt. 1 AY	-0.559	-0.003	-0.006	-0.001	0.000
	(0.708)	(0.004)	(0.007)	(0.020)	(0.004)
Observations	889,613	777,745	777,745	777,745	777,745
R-squared	0.770	0.714	0.851	0.925	0.942

Note: Stable school sample including grade 6 through 12 traditional public school students who do not change schools and attend schools serving a single menu. All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school UFM status, principal change, and enrollment. The sample for columns 2 through 5 are restricted to students with weight data in years AY2014-2017. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Appendix

Table A1. Distribution of Schools with POS Systems by Academic Year

	AY 2014	AY 2015	AY 2016	AY 2017	AY 2018
POS School	484	672	772	888	881
Non-POS School	489	295	183	41	40
Total	**973**	**967**	**955**	**929**	**921**

Note: Table shows the distribution of schools with and without POS systems across years among those serving grades 6 through 12. A POS school is a school with a point-of-service system and a Non-POS school is one without the system.

Table A2. Descriptive Statistics for Schools with and Without POS Systems

		By School POS Status		Analytic Samples	
	All Students	No	Yes	Full	Stable School
Variable	(1)	(2)	(3)	(4)	(5)
Hispanic	.40	.44	.39	.39	.39
Asian/Other	.19	.14	.20	.21	.20
Black	.26	.30	.25	.25	.26
White	.15	.13	.15	.15	.15
Non-English at Home	.47	.43	.47	.47	.46
Female	.50	.51	.49	.50	.49
Ever FRPL	.91	.93	.91	.91	.91
SWD	.14	.15	.14	.14	.14
Attendance Rate	.83	.84	.83	.85	.84
In UFM School	.45	.33	.47	.49	.42
Grade 6	.13	.18	.12	.10	.12
Grade 7	.13	.19	.13	.15	.15
Grade 8	.14	.19	.13	.14	.09
Grade 9	.17	.12	.17	.17	.12
Grade 10	.16	.12	.17	.17	.18
Grade 11	.14	.10	.14	.15	.18
Grade 12	.13	.10	.14	.11	.16
New School	.45	.50	.44	.37	-
New Principal	.37	.38	.37	.33	.28
Students	844,216	228,800	789,870	478,238	334,126
Student-Year	2,331,963	327,588	2,004,375	1,406,789	889,613
Schools	1,013	513	951	885	787

Note: Table presents descriptive statistics pooled across AY 2014 through AY 2018. Columns 1, 2 and 3 presents summary statistics for all NYC traditional public-school students in grades 6 through 12, those in schools without POS systems, and those in school with POS systems, respectively. Columns 4 and 5 present descriptive statistics for the full and stable school analytic samples for reference. The full analytic sample is smaller than the sample of all students in POS schools because of the requirement that students be observed at least twice and attend schools serving a single menu.

Table A3. Results for EverLP & LPrate, Full Sample

VARIABLES	$EverLP_1$ (1)	$EverLP_{10}$ (2)	$EverLP_{20}$ (3)	LPrate (4)
Panel A: Overall Effect				
Post Menu Change	0.020*	0.030**	0.025	2.50**
	(0.010)	(0.013)	(0.016)	(1.25)
Panel B: Effect Over Time				
Post New Menu 1st AY	0.020*	0.032**	0.027	2.48*
	(0.011)	(0.015)	(0.017)	(1.32)
Post New Menu gt. 1 AY	0.018*	0.023*	0.018	2.57**
	(0.011)	(0.012)	(0.015)	(1.19)
Average EverLP/LPrate	.86	.73	.66	40.5
Observations	1,406,789	1,406,789	1,406,789	1,406,789
R-squared	0.642	0.676	0.666	0.74

Note: Full sample includes all grade 6 through 12 students in traditional public schools serving a single menu. All models include student, school, grade, and year fixed effects, and control for student primary language at home, special education services, and school change, as well as school UFM status, principal change, and enrollment. Panel A presents the estimates for the overall effect of a new menu adoption. Panel B shows the estimates for the effect over time. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A4. Results for EverLP & LPrate, Restricted Stable School Sample

VARIABLES	$EverLP_1$ (1)	$EverLP_{10}$ (2)	$EverLP_{20}$ (5)	LPrate (6)
Panel A: Overall Effect				
Post New Menu	0.013	0.024	0.016	2.18
	(0.024)	(0.017)	(0.018)	(1.41)
Panel B: Effect Over Time				
Post New Menu	0.019	0.020	0.008	1.68
	(0.021)	(0.018)	(0.020)	(1.57)
Post New Menu gt. 1 AY	0.046	0.006	-0.029	-0.58
	(0.036)	(0.046)	(0.051)	(3.82)
Average EverLP/LPrate	.87	.72	.65	40.10
Observations	57,938	57,938	57,938	57,938
R-squared	0.644	0.701	0.692	0.76

Note: Stable school sample restricted to students who ever experience a new menu. All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school UFM status, principal change, and enrollment. Panel A presents the estimates for the overall effect of a new menu adoption. Panel B shows the estimates for the effect over time. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A5. Results for LPrate, Students with History of Lunch Participation

	Sample	
	Stable School	Full
VARIABLES	(1)	(2)
Panel A: Overall Effect		
Post New Menu	2.60*	2.69**
	(1.55)	(1.34)
Panel B: Effect Over Time		
Post New Menu 1st AY	2.75*	2.64*
	(1.56)	(1.41)
Post New Menu gt. 1 AY	1.27	2.91**
	(2.48)	(1.30)
Average LPrate	45.8	45.4
Observations	763,784	1,238,703
R-squared	0.74	0.71

Note: Stable (full) school sample including grade 6 through 12 traditional public school students who do not (who do) change schools and attend schools serving a single menu with a history of lunch participation ($EverLP_1$ in period t-1). All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school UFM status, new principal and enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A6. Results for EverLP & LPrate, School Fixed-Effects, Stable School Sample

	$EverLP_1$	$EverLP_{10}$	$EverLP_{20}$	LPrate
VARIABLES	(1)	(2)	(5)	(6)
Panel A: Overall Effect				
Post New Menu	0.029*	0.035*	0.023	1.52
	(0.017)	(0.019)	(0.021)	(1.44)
Panel B: Effect Over Time				
Post New Menu 1st AY	0.023	0.035*	0.026	1.99
	(0.017)	(0.019)	(0.021)	(1.45)
Post New Menu gt. 1 AY	0.057	0.034	0.009	-0.66
	(0.037)	(0.032)	(0.030)	(2.00)
Average EverLP/LPrate	.85	0.71	0.64	39.8
Student-Year Obs.	888,837	888,837	888,837	888,837
R-squared	0.220	0.246	0.245	0.29

Note: Stable school sample including grade 6 through 12 traditional public school students who do not change schools and attend schools serving a single menu. All models include school, grade, and year fixed effects, and control for student race/ethnicity, gender, primary language at home, and special education services, as well as school UFM status, new principal, and enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A7. Results for EverLP & LPrate, School Fixed-Effects, Full Sample

	$EverLP_1$	$EverLP_{10}$	$EverLP_{20}$	LPrate
VARIABLES	(1)	(2)	(5)	(6)
Panel A: Overall Effect				
Post New Menu	0.024	0.035	0.026	1.96
	(0.018)	(0.021)	(0.023)	(1.67)
Panel B: Effect Over Time				
Post New Menu	0.020	0.035	0.029	2.24
	(0.020)	(0.023)	(0.025)	(1.71)
Post New Menu gt. 1 AY	0.042	0.033	0.014	0.90
	(0.030)	(0.030)	(0.030)	(2.24)
Average EverLP/LPrate	.87	.72	.66	40.57
Student-Year Obs.	1,404,624	1,404,624	1,404,624	1,404,624
R-squared	0.206	0.231	0.228	0.28

Note: Full sample includes all grade 6 through 12 students in traditional public schools serving a single menu. All models include student, school, grade, and year fixed effects, and control for student primary language at home, special education services, and school change, as well as school UFM status, principal change, and total enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A8. Results for EverLP & LPrate, Weight Controls, Stable School Sample

	$EverLP_1$	$EverLP_{10}$	$EverLP_{20}$	LPrate
VARIABLES	(1)	(2)	(5)	(6)
Panel A: Overall Effect				
Post New Menu	0.020*	0.037**	0.028	1.74
	(0.012)	(0.017)	(0.021)	(1.46)
Panel B: Effect Over Time				
Post New Menu 1st AY	0.015	0.035**	0.029	1.89
	(0.013)	(0.017)	(0.021)	(1.49)
Post New Menu gt. 1 AY	0.060	0.052*	0.022	0.45
	(0.039)	(0.030)	(0.031)	(2.30)
Average EverLP/LPrate	.86	.72	.66	40.4
Observations	777,745	777,745	777,745	777,745
R-squared	0.698	0.728	0.717	0.78

Note: Stable school sample restricted to students with weight and height data. All models include student, grade, and year fixed effects, and control for student primary language at home, special education services, underweight, overweight, and obese, as well as school UFM status, principal change, and enrollment. Panel A presents the estimates for the overall effect of a new menu adoption. Panel B shows the estimates for the effect over time. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A9. Results for EverLP & LPrate, Excluding Food Court Menu, Stable School Sample

	$EverLP_1$	$EverLP_{10}$	$EverLP_{20}$	LPrate
VARIABLES	(1)	(2)	(3)	(4)
Post New Menu	0.028**	0.026	0.011	1.01
	(0.014)	(0.017)	(0.020)	(1.44)
Observations	877,985	877,985	877,985	877,985
R-squared	0.689	0.720	0.708	0.77

Note: Stable school sample including grade 6 through 12 traditional public school students who do not change schools, attend schools serving a single menu, and are not exposed to the food court menu. All models include school, grade, and year fixed effects, and control for student race/ethnicity, gender, primary language at home, and special education services, as well as school UFM status, new principal, and enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A10. Predicting New Menu Adoptions, School Level

VARIABLES	(1)	(2)
Average LPrate	-0.001	-0.001
	(0.001)	(0.001)
% EverLP_1	0.001	0.001
	(0.001)	(0.003)
% EverLP_{10}	-0.001	-0.000
	(0.001)	(0.002)
% EverLP_{20}	0.001	0.000
	(0.001)	(0.002)
% Female	-0.002	-0.001
	(0.002)	(0.003)
% Asian	-0.001	0.004
	(0.004)	(0.005)
% Black	-0.001	0.001
	(0.003)	(0.003)
% White	0.005	0.009
	(0.005)	(0.008)
% Ever FRPL	-0.004	-0.011
	(0.006)	(0.010)
% SWD	-0.003	-0.006**
	(0.003)	(0.003)
% Non-English at Home	-0.002	0.000
	(0.002)	(0.003)
School-Year Obs.	2,070	1,198
Schools	665	502
R-squared	0.402	0.472
F-Statistic	0.94	0.83

Note: Schools in the stable school sample. All models include school and year fixed-effects, the estimates for which are typically small and statistically insignificant. Sample is further restricted to observations with no new menu in period t for the analysis in column 2. The last observation of each school is dropped for the analysis in column 1, and the first and last for that in column 2. The dependent variable in each specification is an indicator variable for adopting a new menu in time t+1, and all left-hand side variables are school characteristics in time t. Standard errors in parentheses and clustered at the school level (*** $p<0.01$, ** $p<0.05$, * $p<0.1$).

Table A11. Predicting New Menu Exposures, Student Level

VARIABLES	(1)	(2)
LPrate	-0.000	0.000
	(0.000)	(0.000)
$EverLP_1$	0.011	0.008
	(0.008)	(0.012)
$EverLP_{10}$	0.008	0.003
	(0.005)	(0.007)
$EverLP_{20}$	-0.002	0.005
	(0.004)	(0.006)
SWD	-0.014**	-0.017
	(0.006)	(0.013)
Non-English at Home	0.019	0.029
	(0.015)	(0.042)
Observations	399,626	99,767
Students	172,817	48,091
R-Squared	0.491	0.504
F-Statistic	1.20	0.74

Note: Stable school sample including grade 6 through 12 traditional public school students who do not change schools and attend schools serving a single menu. Sample is further restricted to observations with no new menu in period t for the analysis in column 2. All models include student, grade, and year fixed-effects. The last observation of each student is dropped for the analysis in columns 1, and the first and last observations for that in column 2. The dependent variable in each specification is an indicator variable for a menu change in time *t+1*, and all right-hand side variables are time-varying student characteristics in time *t*. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A12. Falsification Test: Results for EverLP & LPrate, Stable School Sample

VARIABLES	$EverLP_1$ (1)	$EverLP_{10}$ (2)	$EverLP_{20}$ (3)	LPrate (4)
Panel A: Overall Effect				
Early Post New Menu	0.028	0.029	0.010	-0.252
	(0.018)	(0.025)	(0.027)	(1.977)
Panel B: Over Time				
Early Post New Menu 1st AY	0.015	0.010	-0.007	-1.679
	(0.017)	(0.022)	(0.024)	(1.834)
Early Post New Menu gt. 1 AY	0.028**	0.040***	0.035*	2.910**
	(0.012)	(0.014)	(0.018)	(1.299)
Student-Year Obs.	889,613	889,613	889,613	889,613
R-squared	0.688	0.719	0.708	0.773

Note: Stable school sample including grade 6 through 12 traditional public school students in schools serving a single menu who do not change schools. All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school UFM status, principal change, and total enrollment. Standard errors in parentheses and clustered at the school level. (*** p<0.01, ** p<0.05, * p<0.1).

Table A13. Falsification Test, Breakfast Participation, Stable School Sample

VARIABLES	$EverBP_1$ (1)	$EverBP_{10}$ (2)	$EverBP_{20}$ (5)	BPrate (6)
Post New Menu	0.017	0.006	-0.005	0.17
	(0.025)	(0.023)	(0.020)	(1.51)
Student-Year Obs.	889,613	889,613	889,613	889,613
R-squared	0.649	0.684	0.671	0.74

Note: Full sample includes all grade 6 through 12 students in traditional public schools serving a single menu. All models include student, grade, and year fixed effects, and control for student primary language at home, special education services, and school change, as well as school UFM status, principal change, and total enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A14. Results for EverLP & LPrate by Student Characteristics

	Race/Ethnicity		Gender		FRPL Status		Grade	
	$EverLP_1$	LPrate	$EverLP_1$	LPrate	$EverLP_1$	LPrate	$EverLP_1$	LPrate
VARIABLES	(1)	(2)	(5)	(6)	(7)	(8)	(9)	(10)
Post New Menu*Asian/Other	0.018	0.514						
	(0.011)	(1.681)						
Post New Menu*Hispanic	0.013	0.757						
	(0.011)	(1.387)						
Post New Menu*Black	0.032**	4.154**						
	(0.014)	(2.056)						
Post New Menu*White	0.086**	5.251***						
	(0.034)	(1.938)						
Post New Menu*Male			0.021**	3.062**				
			(0.010)	(1.519)				
Post New Menu*Female			0.043**	1.799				
			(0.018)	(1.456)				
Post New Menu*Never FRPL					0.138***	7.535***		
					(0.047)	(1.697)		
Post New Menu*Ever FRPL					0.018*	1.777		
					(0.010)	(1.446)		
Post New Menu*Grades 6-8							-0.011	0.795
							(0.012)	(1.844)
Post New Menu*Grades 9-12							0.055***	3.335*
							(0.019)	(1.926)
Average EverLP/LPrate	.848	39.8	.848	39.8	.848	39.8	.848	39.8
Observations	883,437	883,437	889,482	889,482	889,613	889,613	889,613	889,613
R-squared	0.688	0.773	0.688	0.774	0.688	0.774	0.688	0.774

Note: Stable school sample including grade 6 through 12 traditional public school students who do not change schools and attend schools serving a single menu. All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school UFM status, principal change, and enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A15. Results for EverLP & LPrate by Student Characteristics, Full Sample

	Race/Ethnicity		Gender		FRPL Status		Grade	
	$EverLP_1$	LPrate	$EverLP_1$	LPrate	$EverLP_1$	LPrate	$EverLP_1$	LPrate
VARIABLES	(1)	(2)	(5)	(6)	(7)	(8)	(9)	(10)
Post New Menu*Asian/Other	0.028***	3.216**						
	(0.010)	(1.298)						
Post New Menu*Hispanic	0.020**	2.104*						
	(0.010)	(1.191)						
Post New Menu*Black	0.016	1.855						
	(0.010)	(1.412)						
Post New Menu*White	0.014	3.804***						
	(0.016)	(1.350)						
Post New Menu*Male			0.029***	4.362***				
			(0.010)	(1.294)				
Post New Menu*Female			0.010	0.561				
			(0.011)	(1.247)				
Post New Menu*Never FRPL					0.012	4.247***		
					(0.020)	(1.294)		
Post New Menu*Ever FRPL					0.021**	2.312*		
					(0.010)	(1.245)		
Post New Menu*Grades 6-8							-0.004	2.148
							(0.009)	(1.674)
Post New Menu*Grades 9-12							0.035**	2.720*
							(0.015)	(1.638)
Student-Year Obs.	1,398,446	1,398,446	1,406,572	1,406,572	1,406,789	1,406,789	1,406,789	1,406,789
R-squared	0.642	0.736	0.642	0.736	0.642	0.736	0.642	0.736

Note: Full sample includes all grade 6 through 12 students in traditional public schools serving a single menu. All models include student, school, grade, and year fixed effects, and control for student primary language at home, special education services, and school change, as well as school UFM status, principal change, and enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A16. Results for EverLP & LPrate, UFM, Full Sample

	$EverLP_1$	$EverLP_{10}$	$EverLP_{20}$	LPrate
VARIABLES	(1)	(2)	(3)	(4)
UFM	0.085***	0.072***	0.070***	3.534***
	(0.008)	(0.009)	(0.010)	(0.978)
Post New Menu*No UFM	-0.003	0.009	0.007	1.791
	(0.010)	(0.012)	(0.015)	(1.204)
Post New Menu*UFM	0.033***	0.042***	0.035**	2.900**
	(0.011)	(0.014)	(0.017)	(1.321)
Average EverLP/LPrate	0.86	0.73	0.66	40.5
Observations	1,406,789	1,406,789	1,406,789	1,406,789
R-squared	0.642	0.676	0.666	0.736

Note: Full sample includes all grade 6 through 12 students in traditional public schools serving a single menu. All models include student, school, grade, and year fixed effects, and control for student primary language at home, special education services, and school change, as well as school UFM status, principal change, and total enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A17. Results for New Non-Standard Menu, Full Sample

	$EverLP_1$	$EverLP_{10}$	$EverLP_{20}$	LPrate
VARIABLES	(1)	(2)	(3)	(4)
Non-Standard Menu	0.001	-0.006	0.003	-2.38
	(0.023)	(0.021)	(0.026)	(3.07)
Post New Menu*Standard Menu	0.004	0.011	0.013	1.38
	(0.006)	(0.011)	(0.015)	(1.20)
Post New Menu*Non-Standard Menu	0.051*	0.070**	0.049	5.86
	(0.029)	(0.035)	(0.042)	(3.57)
Average EverLP/LPrate	.86	.73	.66	40.5
Observations	1,406,789	1,406,789	1,406,789	1,406,789
R-squared	0.642	0.676	0.666	0.74

Note: Full sample includes all grade 6 through 12 students in traditional public schools serving a single menu. All models include student, school, grade, and year fixed effects, and control for student primary language at home, special education services, and school change, as well as school UFM status, principal change, and total enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A18. Results for EverLP and LPrate, Menu Type

VARIABLES	$EverLP_1$ (1)	$EverLP_{10}$ (2)	$EverLP_{20}$ (3)	LPrate (4)
MS Standard	0.018	0.025	0.023	0.411
	(0.012)	(0.017)	(0.018)	(1.491)
K-8 Standard	0.019	0.035	0.037	-0.174
	(0.014)	(0.026)	(0.036)	(2.947)
Alternative	0.044	0.015	0.003	0.210
	(0.027)	(0.032)	(0.038)	(2.366)
Express	-0.016	-0.037	-0.061	-6.813
	(0.047)	(0.045)	(0.045)	(4.600)
Food Court	0.057*	0.125***	0.138***	8.605***
	(0.030)	(0.033)	(0.035)	(2.724)
Observations	889,613	889,613	889,613	889,613
R-squared	0.688	0.719	0.708	0.773

Note: Stable school sample including grade 6 through 12 traditional public school students in schools serving a single menu who do not change schools. All models include student, grade, and year fixed effects, and control for student primary language at home and special education services, as well as school UFM status, principal change, and total enrollment. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

Table A19. Results for Other Outcomes, Full Sample

VARIABLES	Attendance Rate (1)	Underweight (2)	Overweight or Obese (3)	zBMI (4)	Ln(BMI) (5)
Panel A: Overall Effect					
Post New Menu	0.555*	0.002	-0.005	-0.013	-0.002
	(0.296)	(0.006)	(0.006)	(0.027)	(0.004)
Panel B: Effect Over Time					
Post New Menu 1st AY	0.643**	0.003	-0.006	-0.017	-0.003
	(0.287)	(0.006)	(0.006)	(0.027)	(0.004)
Post New Menu gt. 1 AY	0.187	-0.000	-0.001	0.004	0.000
	(0.338)	(0.005)	(0.005)	(0.020)	(0.003)
Student-Year Obs.	1,406,789	882,037	882,037	882,037	882,037
R-squared	0.760	0.706	0.847	0.920	0.939

Note: Full sample includes all grade 6 through 12 students in traditional public schools serving a single menu. All models include student, school, grade, and year fixed effects, and control for student primary language at home, special education services, and school change, as well as school UFM status, principal change, and total enrollment. The sample for columns 2 through 5 are restricted to students with weight data in years AY2014-2017. Standard errors in parentheses and clustered at the school level (*** p<0.01, ** p<0.05, * p<0.1).

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Chapter 2

Incentivizing healthy eating in children: An investigation of the "ripple" and "temporal" effects of a reward-based intervention

Introduction

According to the Centers for Disease Control (CDC), healthy eating promotes the optimal growth and development of children while also reducing their risk for developing obesity and other illnesses (CDC, 2015). The consumption of fruits and vegetables is of particular importance, as they are not only key sources of fiber and essential micronutrients, but also help to mitigate weight gain (Rolls et al., 2004; Ledikwe et al., 2006; Vioque et al., 2008). Most American children ages two years and older do not, however, meet the United States Department of Agriculture's (USDA) recommendations for a diet rich in fruits and vegetables. In contrast, their intake of sodium is more than the recommended maximum daily allowance and 40% of their daily caloric intake comes from added sugars and solid fats, approximately half of which are acquired through the consumption of various junk foods (CDC, 2015). Research also suggests that diet during childhood is a significant predictor of diet in adulthood, and that pediatric obesity has negative implications for adult health outcomes (Hingle, 2010; Nicklaus, 2009; Birch, 1999).

The latest figures available from the CDC indicate that, in the United States, 8.4% of 2- to 5-year-olds, 17.7% of 6- to 11-year-olds, and 20.5% of 12- to 19-year-olds are obese, a problem more acute among black, Hispanic, and low-income children (CDC, 2016). Given their adverse effects on normal development, the associated costs, and influence on long-term eating habits, the targeting of pediatric obesity and children's unhealthy dietary choices are particularly important. Preventative measures designed to induce better eating behaviors earlier in the

lifecycle may therefore yield maximum health benefits and establish dietary habits that may persist into adulthood.

A growing body of research examines the impacts of various interventions on encouraging healthy eating habits in school-aged children. These range from various *non-remunerative* methods—used here to mean those in which participants are not provided a tangible, material reward in return for the performance of a particular behavior—to *remunerative* approaches—defined here as those in which participants receive some form of a tangible, material reward in exchange for behaving in a desired manner. Although the former have been studied extensively, the latter have generally been avoided due to concerns that their use may "*crowd out*" intrinsic motivation for healthy eating behaviors and result in worse outcomes after their removal (Horne et al., 2010), a phenomena sometimes also referred to as the "overjustification effect" or the "negative rebound effect" (Just and Price, 2012). There exists, however, scant evidence in favor of such an adverse effect in the context of fruit and vegetable consumption (Horne et al., 2010), and the studies employing remunerative incentives find them to have no impact on children's intrinsic motivations (Raju et al., 2010; Corsini et al., 2011; Just & Price 2013; Belot, James, & Nolen, 2013; Loewenstein, Price, and Volpp, 2016; List & Samek, 2015a, 2015b).

Existing studies also suggest that remunerative interventions may be more cost-effective than their non-remunerative counterparts, which tend to be costly, time-consuming, and labor-intensive to put into practice (Hendy et al., 2005; Evans et al., 2012) while producing little, if any, changes in dietary behaviors. Such interventions often involve changes to school curricula, time-intensive involvement of everyone involved (e.g., teachers, staff, parents, or children), costly materials (e.g., equipment or educational and informational materials), or the alteration of

the physical aspects of school, home, or community environments (Cauwenberghe et al., 2010; Evans et al., 2012; Hendrie et al., 2016). In contrast, remunerative interventions employing small rewards worth 50 cents or less—even as low as a nickel (USD $0.05)—have been shown to produce large changes in the choice and consumption of fruits and vegetables (Raju et al., 2010; Just & Price 2013; Belot, James, & Nolen, 2013; Loewenstein, Price, and Volpp, 2016; List & Samek, 2015a, 2015b). Simply affixing such small rewards to an item has also been shown to increase their selection (List & Samek, 2015b), implying that such incentives may be effective at little additional burden, financial or otherwise.

Given the promise of these initial studies, the impact of such rewards over time and their influence on behaviors outside of experimental settings warrant further study. Regarding the former, it may be that the effectiveness of rewards as a motivator in influencing dietary behaviors diminishes over time as the novelty of their introduction wears off. If so, this may imply that the frequency of their use, and the types of rewards used, may matter in designing a long-term effective remunerative intervention. Studies that have attempted to investigate the temporal dimensions of such interventions either suffer from significant data collection issues, employ complex intervention schemes, or use designs that combine multiple treatments. Raju, Rajagopal, and Gilbride (2010) find effects that fluctuated over the course of their study but recommend a cautious interpretation of their findings as they failed to collect data on 62% of their sample. The interventions tested in Belot, James, and Nolen (2013) consisted of a piece-rate scheme and a competition scheme in which children had to collect a certain number of stickers to be eligible for a prize at the end of each school week. While the piece rate scheme was found to be ineffective, the competition scheme produced sizeable effects that diminished with time overall but persisted for the subset of students who had some margin for improvement. Lastly,

Loewesnstein, Price, and Volpp (2014) found effects that did not fade out over time but used rewards in conjunction with a verbal prompt, making it difficult to attribute any effects to the presence of rewards themselves.

As for the influence of rewards on behaviors in other contexts, no attempts have yet been made to discern their impact outside of intervention settings. Health outcomes will ultimately depend on whether any positive impacts on food choice within the intervention setting are off-set or out-weighed by poor eating behaviors in other settings, behaviors that may be exacerbated by the introduction of such incentives (Ransley et al., 2007; Evans et al., 2012; List and Samek, 2015). The introduction of rewards may have three potential effects. They may increase fruit choice and consumption outside of the intervention setting if, for example, children, develop a taste or habit for them. It may also be that such an intervention has no effect on dietary behaviors outside of the setting in which it is administered. Lastly, the intervention may reduce the choice and consumption of fruits if children compensate for foregoing junk food earlier in the day by eating more of it in another setting. Of particular importance are behaviors at home where most habits are learned (De Bourdeaudhuij, 1997; Campbell et al., 2007; Dowda et al., 2001), the external setting of interest in this study.

This study seeks to add to the small body of literature on remunerative approaches targeting children's eating habits by shedding light on these issues in the context of fruit choice. It employs a removed treatment within-subject design in conjunction with parent surveys and presents the results of a month-long field experiment in which 23 low-income children ages 5 to 8 attending a summer program were offered a small prize for choosing a fruit cup for dessert after lunch in lieu of cookies. The contributions are threefold. First, this study adds to the contexts in which such experiments have been conducted and, in conjunction with previous

studies, serves to bolster the case for the generalizability of existing findings. Second, by surveying parents about their children's dietary behaviors at home, this study attempts to identify the potential impact of reward-based incentives on children's eating behaviors outside of the intervention setting—labeled here as "*ripple*" effects. Third, this study gauges the efficacy of such interventions over time—labeled here as "*temporal*" effects—both between weeks and within weeks.

Materials and Methods

Experimental Design

This study employs a removed treatment within-subjects design. In within-subjects designs, participants serve as their own controls, thereby reducing the amount of error arising from natural variance between individuals. Such designs are, however, susceptible to various threats to internal validity. The plausibility of these threats is significantly diminished in the context of a treatment removal design (Shadish, Cook, & Campbell, 2001). In such a design, pre-treatment observations are first recorded, after which the treatment is introduced and post-treatment observations recorded. This is then followed by the removal of the treatment and further observation. If it can be demonstrated that the outcome of interest rises and falls with the presence or absence of the intervention, it becomes highly implausible that observed changes could be the result of alternative factors or extraneous events, thereby facilitating causal inferences.

Location

The field experiment was conducted at a Boys and Girls Club (BGC) location in central New York, and was approved by Syracuse University's Institutional Review Board (IRB) as well as the local branch of the BGC. The site serves low-income children ages 5 to 12 throughout the

year with an after-school program when school is in session and an all-day program during the summer months. During the summer, children are served breakfast and lunch, both of which are provided by the local school district and are standard meals that are also served in school cafeterias during the school year. These meals did not include dessert, which was introduced for the first time as a part of this experiment.

School-like settings such as this serve as ideal testing grounds for interventions targeting eating habits among children since schools—the likely hosts of any large-scale intervention—are in a unique position to promote healthy eating as they offer opportunities for targeting large numbers of children while also providing up to half of their daily caloric intake (USDA, 2017; Briefel, Wilson, & Gleason, 2009). Additionally, summer programs may offer greater access to children most likely to benefit from interventions targeting dietary choices to the extent that they serve those from lower socioeconomic backgrounds.

Participants

Parents with children ages 5 to 8 were asked in person if they wished to participate in the study, and allow for their children to participate, as they arrived to pick up their children from the program. Mothers were targeted as research indicates that they are the most accurate source of information about the behavior patterns of their children (Hendy et al. 2005)—an important consideration since parent surveys are an integral component of this study. The age range was selected to fit the theme of early dietary interventions as well as to facilitate the investigation of "ripple effects" using parent surveys, which requires that children be old enough to express their preferences but still young enough to need their parents help in obtaining access to food.

Of eligible parents who personally picked up their child from the site and indicated that they intended to enroll, or had enrolled, them in the summer program, 25 were solicited and only

one refused to participate. Of those who initially agreed to participate, 19 ultimately had their children attend the summer program. Upon consenting, assent was also obtained from their children, on whom socioeconomic and demographic information was collected. As summarized in **Table 9**, participants were predominantly Black and most children were part of low-income, single-parent households.

In sum, 29 children and their parents were recruited for the study, of which 23 were present on at least one day during the field experiment. This sample comprises approximately 61% of all children ages 5 to 8 that were enrolled in the summer program, but likely more on any given day since some children attended sporadically.

Timeline & Procedure

On-site enrollment for the experiment began in mid-June of 2016. The recruitment period lasted three weeks, two of which occurred while school was still in session. The third week of recruitment took place during the first week of the site's summer program. The four-week field experiment commenced immediately after the recruitment period. The first week was composed of pre-intervention baseline observations, the intervention was implemented the following two weeks, and the final week consisted of post-intervention removal observations.

During the first week of the experiment, children were told that they could choose between a fruit cup or a cookie cup for dessert. During the following intervention period, children were told that they could again choose between a fruit cup and a cookie cup for dessert, but that they would receive a prize for choosing the former and nothing for choosing the latter. The final post-intervention week mimicked the first week of the experiment. Children also had the option of choosing neither dessert in each period.

After lunch, a tray of desserts was set out lined with white napkins displaying 24 translucent plastic cups containing fruits on the left-hand side and 24 identical cups containing cookies on the right-hand side. Given that the site served between 45 and 60 children on any given day, additional fruit and cookie cups were prepared and set to the side in order to replenish the supply on the tray if necessary. The site director or, in some instances, another staff member would first ask all 5 to 8-year olds to line up by the food counter. As they approached the dessert tray, they were provided the appropriate information for that particular week and asked to make a choice. Once all 5 to 8-year olds had been served, the site director or staff member would ask the 9 to 12-year-old children to line up by the food counter, after which the process was repeated. After the 9 to 12-year olds had been served, the 5 to 8 year olds were once again asked to line up by the counter so that the consumption of participating children could be recorded. All of the children were instructed not to throw away their cups until their consumption was recorded. The cups of participating children were examined to see how much they had eaten, and consumption was recorded as either a quarter, half, three quarters, all of a cup, or none.

Prizes

Prizes included small notepads, pencils, pencil sharpeners, rubber balls, rings, airplanes, and finger lights, each worth on average roughly 10 cents. These prizes were chosen since the reward value of similar items were established in List & Samek (2015). Prizes also varied in color and design in order to ensure that children would continue to value them throughout the experiment, as per List and Samek (2015). Children choosing a fruit cup during the intervention period were allowed to choose one prize from among the options listed, which were set on the countertop next to the dessert tray within sight.

Parent Pre-Survey and Post-Surveys and Daily Logs

A novel component of this study is its attempt to provide insights on the ripple effects of remunerative interventions—that is to say, in this context, the effect of rewards introduced in the experimental setting on children's eating behaviors at home. To do so, this study employs both pre- and post-intervention surveys and daily logs, to be completed by participating parents. The purpose of incorporating both surveys and the daily logs is twofold. First, including both allows for the pre- and post-survey responses to not only be compared with one another to identify any differences in a child's eating behavior, but also to be compared with the results from the daily logs to corroborate their veracity. If, for example, the trend in the daily logs and survey responses are positively correlated, confidence in the accuracy of the responses is enhanced. Second, in the event that one of the methods produces unusable responses, perhaps due to respondent related issues, the other may be used as a substitute. In both cases, parents were motivated to complete the surveys and daily logs with a cash incentive.[34]

Parents were asked to complete the first survey upon enrollment prior to the implementation of the experiment in order to establish a baseline and the second a week after its conclusion. To reduce the burden on parents and minimize respondent fatigue, the surveys were comprised of six questions adapted from the Child Eating Behavior Inventory.[35] Similarly, parents were also asked to maintain a daily log—including weekends—of their child's behavior and preferences regarding fruits for the duration of the experiment. To minimize burden and

[34] Parents could earn up to $25 per child. Parents were given $3 for completing the first survey, $5 per completed daily log, and $2 for the final survey.

[35] The Child Eating Behavior Inventory comprises 40 items that are rated on a 5-point scale with response options being "never," "seldom," "sometimes," "often," and "always." It is a parent-report instrument designed to assess childhood eating and mealtime problems. Six of the 40 questions were selected and modified to fit the context in which they were used. The questions used for this study, and how they were adapted, are available in **Figure A1** of the Appendix.

respondent fatigue for the daily logs, parents were asked to answer six short "yes or no" questions each night of the week.

Desserts

The healthy dessert in this experiment consisted of a fruit cup, while the unhealthy dessert consisted of a cookie cup. Fruits are nutrient-dense and are therefore recommended by the USDA for their health benefits. In fact, the USDA recommends that individuals increase their fruit consumption as part of a healthy eating pattern (USDHHS & USDA, 2015). In contrast, cookies provide little nutrients and are high in sugar content. Fruit cups weighed approximately 85 grams, 5 grams more than the minimum serving size recommended by any governmental agency in the OECD (Evans et al., 2012).[36] In an effort to ensure equal familiarity with both desserts, fruits and cookies were chosen such that they would be universally recognizable. Apples, pears, grapes, and bananas were served as the fruit options and chocolate chip and Oreo cookies were served as the cookie options. Combinations of fruits served varied by type and color, as did cookies, in order to mitigate the risk of children losing interest in the food items.[37]

Data Analysis

The analysis employs standard experimental methods, supplemented by econometric analysis. Changes in children's dessert choice between weeks are first analyzed using two-tailed paired sample t-tests. The longitudinal nature of the data collection produced 358 participant-day observations, thereby also facilitating the use of regression analysis. The comparison of means tests in the proportion of fruit cups chosen by participating children are therefore supplemented

[36] USDA recommendations differ by type of fruit and how it is served. Consequently, no one standard applies to an assortment of fruits. Therefore, the minimum OECD requirement, in grams, was used for each serving.
[37] Additional information on dessert combinations and dessert cup composition are provided in **Figure A2** of the Appendix.

by logistic regressions with dessert choice as the dependent variable—where dessert choice equals 1 if a child chose a fruit cup and 0 if they chose a cookie cup or neither—and a treatment dummy as the independent variable of interest, conditional on attendance. The results of a linear probability model (LPM) are also presented to facilitate the reporting and interpretation of marginal effects. All regressions include individual fixed effects to account for any time-invariant participant-specific characteristics and standard errors clustered at the individual level.

Results

Attendance, Consumption, & Prize Selection

Ideally, participating children would have been observed on each of the 20 weekdays of the study. Attendance for some children was, however, sporadic. As a result, roughly 78% of the potential 460 child-day observations were those for which the children were present on site. In sum, participating children made a total of 358 decisions during the four weeks of the experiment, of which 50.84% resulted in the choice of a fruit cup and 46.65% that of a cookie cup. Neither dessert option was chosen in only nine out of the 358 instances, constituting 2.33% of total decisions. Among those who chose a dessert, consumption was near universal, with the contents of fruit and cookie cups being consumed in their entirety 95% of the time, as shown in **Table 10**. Cheating was not observed by myself or by any members of the staff. Also depicted is prize selection. Descriptive statistics suggest that non-school related prizes were the most desirable, particularly the finger-lights and the rubber balls, which constituted approximately 44% and 34% of all prize selections, respectively.[38]

[38] Prize selection was calculated by taking the total number of rewards ordered prior to the beginning of the study and subtracting from that what remained after its conclusion. IRB, however, required that prizes and desserts be made available to all children present on site, regardless of their participation status. Since all children present were afforded an opportunity to choose a prize, the proportions are only close approximations of the selections made by participating children.

Baseline, Treatment, and Post-Treatment Week Comparisons on Selection

The change in dessert choice among participants who chose a dessert, averaged across days for each week, is depicted in **Figure 8**. There were large changes between Week 1 and the intervention weeks, and between the intervention weeks and week 4. The statistical significance of these differences is assessed using paired sample t-tests. The proportion of children choosing fruit cups increased from 32% in Week 1 to 81% in Week 2 ($p < .001$), decreased from 81% to 64% between Weeks 2 and 3 ($p = .023$), and fell further from 64% to 29% between Weeks 3 and 4 ($p = .003$). A comparison of children's pre- and post-intervention choices also showed no change, as indicated by the statistical insignificance of the difference in the proportion choosing fruits between Weeks 1 and 4, which were 32% and 29% respectively ($p = .33$).

The results from the fixed-effect logistic and linear probability model regressions, which corroborate these results, are depicted in **Table 11**. Models 1 and 3 are fixed-effect logistic models whereas Models 2 and 4 are linear probability models. To identify the effect of the reward-based incentives in inducing the choice of fruit cups over that of cookie cups, the sample is restricted to observations collected in Weeks 1, 2, and 3 for models 1 and 2, and the treatment dummy is a binary variable that takes the value of 0 if the intervention was absent and 1 if present. To ascertain the effect of their removal, the sample used for models 3 and 4 is restricted to observations collected in Weeks 2, 3, and 4, and the treatment dummy takes the value of 1 if the intervention is absent and 0 if present. The coefficients on the variables of interest are statistically significant in each specification. The linear probability models show that the effects of the intervention are large in magnitude, with its introduction increasing the likelihood of a child choosing a fruit cup by 37 percentage-points and its removal reducing that likelihood by 43 percentage-points.

Separating the treatment effect by week also highlights the waning effect of the incentives over time. As shown in **Table 12**, the introduction of incentives increased the likelihood of a fruit cup being chosen by 45 percentage-points in the first week of the intervention (Week 2). By the second week (Week 3), this effect dropped to 28 percentage-points. Further analysis also indicates that the effect of the incentives wane not only between intervention weeks but within intervention weeks as well, as depicted in **Figure 9**. The effect of the incentives are strongest during the first half of the first intervention week (Week 2: Monday, Tuesday, and Wednesday), in which they increase the likelihood of choosing a fruit cup by 49 percentage-points. The effect then declines steadily to 26 percentage-points by the second half of the second intervention week (Week 3: Thursday and Friday). In both the between-week and within-week cases, the null hypothesis of equality between the aforementioned coefficients was rejected ($p = .017$ and $p = .051$, respectively).[39]

Robustness Checks

If children's absence was in some way related to their propensity to choose fruits or cookies, the sporadic attendance of some children may be a concern. However unlikely this might be, the sensitivity of the results to attendance were checked by restricting the sample to only those students who were present at least one day during each of the given periods of interest (i.e. the sample was restricted so that each child contributes to the analysis in each period). For the overall and between week effects, participating children must have attended at least once during each week to be included in the analysis. For the within-week effects, each child must have attended at least once during each week half to be included in the analysis. Accounting for

[39] The null hypotheses of equality between all of the coefficients shown in **Table 12** were tested and the *p*-values are shown in **Table A20** of the Appendix.

attendance and re-conducting the analysis did not change the result, which remained almost identical to the original estimates. These results are presented in **Table A21** of the Appendix.

Alternatively, it may be that the results of the within-week analysis may be sensitive to how the days are grouped. To investigate this possibility, the first half of each week was redefined to comprise of Monday and Tuesday, while the second half was redefined to include Wednesday, Thursday, and Friday. Doing so did not change the coefficient estimates, which again remained nearly identical to the original estimates, but led to an increase in the statistical significance of the difference between coefficients. These results are presented in **Table A22** and **Table A23** of the Appendix, respectively. Finally, three of the participating children had parents that were employed on site. Dropping these children and conducting the same analysis did not change the results. These results are depicted in **Table A24** of the Appendix.

Effect of Intervention on Children's Preferences at Home

Survey responses did not produce evidence of either a positive or negative "ripple" effect. Both pre-intervention and post-intervention Child Eating Behavior surveys were obtained for 16 of the 23 children, constituting a response rate of approximately 70%. Cronbach's alpha coefficients for the pre-intervention and post-intervention surveys were .84 and .89, respectively. To get a sense of how parents perceptions may have changed overall, the survey scores across individuals for each period were aggregated (i.e. the sum of the total survey score for each child; minimum score possible = 5, maximum score possible = 30). The post-intervention aggregate score of 389 declined relative to the pre-intervention aggregate score of 422. This could suggest that—as a whole—parents felt worse about their children's eating behaviors with regards to fruits. However, a two-tailed paired sample t-test indicates that the null hypothesis of no difference between pre- and post-survey responses cannot be rejected ($p = .13$). Consequently,

there is no statistical evidence that children's eating behaviors outside of the experimental setting were affected due to the intervention.

While the response rate to the surveys did allow for some inference, that for the daily logs was not sufficiently large for any meaningful analysis or comparison with the survey results. Although they did not contradict the survey findings, daily logs for each week were obtained for only seven of the children—constituting a 30% response rate. This was, however, a contingency that was planned for by having both pre- and post-surveys and daily logs. Although the results of the survey response analysis cannot be fully corroborated by a secondary measure, they nonetheless do offer some insight on preferences outside of the experimental setting.

Internal Validity

Treatment Effects

In the absence of randomized assignment into treatment and control groups, potential threats to internal validity in this context include "history," "maturation," and "experimenter" effects. The research design employed reduces the plausibility of these threats, however. History effects may be present if factors external to the experiment occurred concurrent to the intervention being introduced and removed that could also have affected fruit cup choice in the directions observed. There is no indication this was the case, however, since there were no changes in the site's operations or in the school districts provision of meals during this time. Since participating children were eight years old and younger, any confounding extraneous factors would have had to occur at home, but there is nothing to suggest that anything changed significantly over the course of the four-week experiment.

Given the length of the study and the ages of the participants involved, it is also unlikely that any observed effects could be attributed to the maturation of the participants. Furthermore,

that fruit cup choice increased with the introduction of the rewards and declined back to pre-intervention levels after their removal suggests that no such maturation occurred. Experimenter effects may have biased the fruit cup selection and consumption upward if the presence of the researcher motivated the children to do so at higher rates than they otherwise would. To the extent possible, such an effect was mitigated with the inclusion of a baseline observation week, which would have accounted for any upward bias, and by restricting interactions with the subjects to the bare minimum necessary to execute the study.

In addition to these potential confounders, there may exists a threat unique to the removed treatment within-subjects design that must be addressed. It may be that the decline in fruit cup choice may not be due to the removal of rewards but rather the result of a declining time trend in the desire to choose fruit cups. This study does, in fact, suggest that there may exist such a trend. To test whether the decline between the second half of Week 3 and the first half of Week 4, as seen in **Figure 9**, is a consequence of treatment removal, the sample was restricted to corresponding observations and a linear probability model regression was used. Dessert choice was regressed on a dummy variable representing the first half of Week 4 (Monday, Tuesday, and Wednesday), with a dummy representing the second half of Week 3 (Thursday and Friday) as the comparison. The analysis finds a 25-percentage point drop in the probability of choosing fruit cups associated with the removal of rewards ($p = .08$). The sharp decline in fruit cup choice between the second half of Week 3 and the first half of Week 4—from 61% to 35%—in conjunction with the overall sharp drop between Weeks 3 and 4—from 64% to 29%—implies that, though declining, the trend itself could not be the cause at the time the intervention was removed.

Finally, like all such studies, randomized or not, "peer effects" are unavoidable. Peer effects may be present if, for example, the introduction of rewards induced popular children to choose fruits, which in turn influenced the decision of their peers to do so as well. However, to the extent that peer effects bias the results upwards, they can be thought of as a desirable source of bias if they serve to increase fruit cup selection and consumption, which is the desired objective. Insofar as their presence has biased the results of this study downward, the magnitude of the intervention effect implies that such an impact is not large enough to nullify the estimated effects.

Survey Results

It is possible that parents were not entirely blind to the intervention and that their responses to the surveys may have been affected as a result. Of particular concern would be "social desirability" bias, whereby parents may have answered questions pertaining to their children's preferences for fruits so as to be viewed more favorably. The decline in aggregate scores between the pre-survey and post-survey suggests that this may not be the case, but it may also be that the introduction of the first survey made parents more conscious of their children's dietary behaviors such that their responses in the second survey were more indicative of their true preferences.

To minimize such risks, parents were only provided the minimum amount of information about the experiment as required by IRB. They were informed that their children would be served desserts and would also have the opportunity to win prizes, but nothing was said about the motivation for the experiment, its objectives, or how it was to be implemented. While it is certainly possible that parents may have been aware of the aims of the study to some extent, and that survey responses reflected that knowledge, ethical and moral concerns would not allow the

nature of the experiment to be completely hidden from potential participants. However, this issue affects any study involving human participants and which requires their explicit consent.

Discussion

The introduction of small reward-based incentives increased the proportion of children choosing a fruit cup in lieu of a cookie cup for dessert after lunch, thereby corroborating the findings of existing studies. Also as in previous studies, no evidence was found to suggest that the introduction of rewards crowded out children's intrinsic motivation. Consumption was also high, with nearly all children consuming the contents of their chosen dessert cup in its entirety. Though the presence of small rewards in general appeared to excite and motivate the children, non-school related rewards appeared to be the most popular among the options available for those that chose a fruit cup. Further analysis also suggests that the effect of reward-based incentives wane over time, not only between weeks but also within weeks. The effect of the incentives on the likelihood of choosing a fruit cup declined by 37% between Weeks 2 and 3—the first and last of the intervention weeks. Similarly, between the first half of Week 2 and the second half of Week 3, the effect of the incentives declined by nearly half.

Together, all of the above-mentioned findings suggest that—at least in this context—small reward-based incentives are effective for low-income children, though their effect diminishes over time and depends on the types of rewards offered. Future investigations of reward-based interventions administered daily are encouraged to provide more insight on the former. If the findings here are corroborated, it may suggest that any reward-based intervention should be administered intermittently to maintain its effect over time. The latter suggests that rewards that excite students should be chosen for maximum effect and that there should be variation in the types of rewards available so as to maintain children's interest in them. To the

extent that the negative trend observed in the intervention effect is a byproduct of children losing interest in the incentives being offered, then the introduction of new prizes at optimal intervals may have an offsetting effect, thereby potentially facilitating the continuous provision of rewards. Future research is therefore also needed to ascertain the types of prizes likely to elicit the greatest response and the requisite variation necessary to maintain interest.

As for the evidence pertaining to "ripple" effects, the findings presented here are likely not definitive. Although the survey results suggest that there may be no external effects related to the introduction of reward-based incentives in school or school-like settings, the response rate and the lack of a second measure to corroborate the accuracy of the responses leave much to be desired. Further research is therefore necessary to assess the effect of rewards-based incentives outside of intervention-settings.

With that said, the presence of negative "ripple" effects may be less of a concern among children from low-income families, as studies suggest that such children consume fewer fruits and vegetables at home (Krebs-Smith et al. 1996; Munoz et al. 1997). Anecdotal evidence collected as part of conversations with various staff members and non-staff familiar with the community that the site served also suggest that the children in this study—nearly all of whom were from low-income households—did not consume many fruits and vegetables at home, if any healthy food at all.

Finally, given this study's sample selection process and size, the generalizability of the results presented here are limited. Nevertheless, by adding yet another context in which remunerative interventions have been tested, the results of this study together with those of others bolsters the case for their efficacy among young children and provides suggestive evidence for the absence of any adverse "ripple" effects. Furthermore, the study population is

one of particular interest since low-income children are likely to benefit the most from interventions targeting their dietary behaviors.

Conclusions

Corroborating existing studies, the introduction of small reward-based incentives were found to increase the number of children choosing the healthy dessert options after lunch but disaggregating the results by week and days suggests that their impact may diminish over time. Attempts to ascertain their effect outside of experimental settings did not indicate that the introduction of rewards had any adverse effects, but also did not provide definitive results. Consequently, further research is needed in this regard. There is also a greater need for long-term studies, not only to assess the temporal effects of reward-based interventions but also habit formation. If children were to develop a habit for healthier eating behaviors within intervention settings, this may then translate to better dietary choices in other environments as well.

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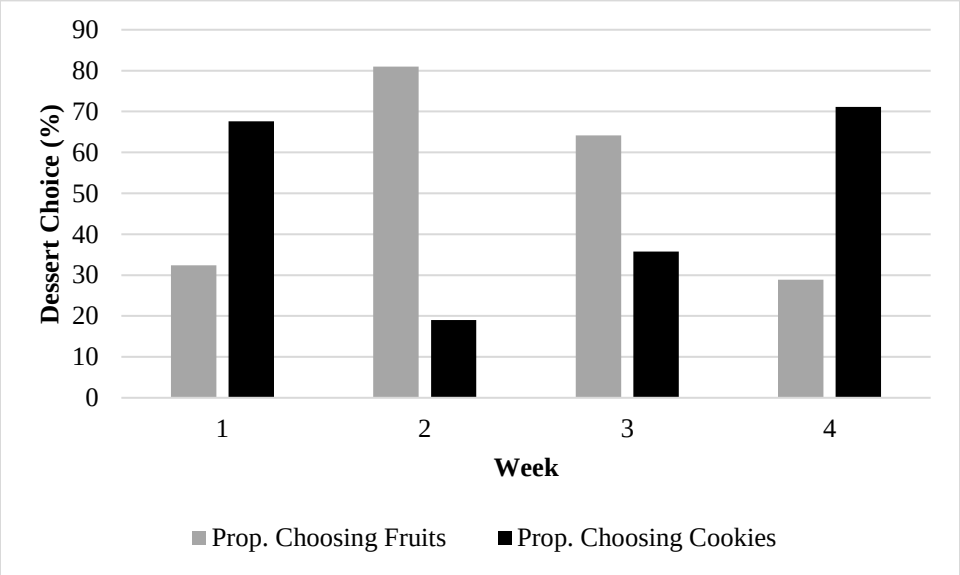
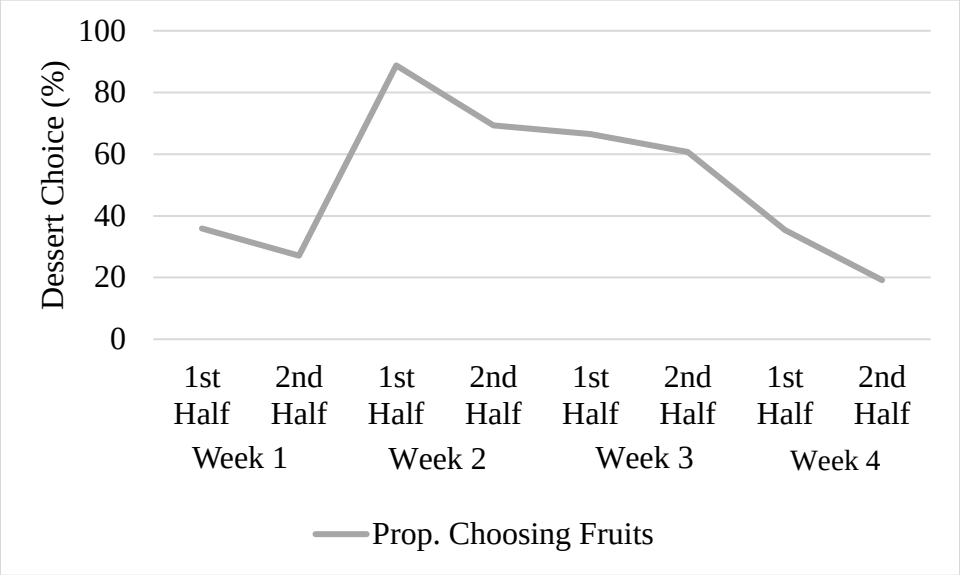
Tables & Figures

Figure 8. Proportion Choosing Fruits – Between Weeks



Note: This table depicts the change in dessert choice among participants who chose a dessert, averaged across days for each week.

Figure 9. Proportion Choosing Fruits – Within Weeks



Note: This graph depicts within-week changes in the proportion of participants choosing fruits. The first half of each week denotes Monday, Tuesday, and Wednesday, and the second half denotes Thursday and Friday.

Table 9. Descriptive Statistics

Characteristics	Frequency	Percent of Total
AGE		
5	5	22%
6	3	13%
7	10	43%
8	5	22%
Gender		
Female	9	39%
Male	14	61%
Race		
Black	17	74%
White	1	9%
Mixed (Black & White)	2	13%
Other	3	4%
Ethnicity		
Hispanic	2	9%
Non-Hispanic	21	91%
Household Type		
Single Mother	15	65%
Both Parents	7	30%
Alternate Custody	1	4%
Household Income		
<10,000	11	48%
10,001 – 20,000	8	35%
20,001 – 30,000	1	4%
40,001 – 50,000	3	13%
Number of Siblings		
0	6	26%
1	9	39%
2	2	9%
3	3	13%
4	3	13%
Free Lunch	23	100%

Note: This table provides socioeconomic and demographic information for participating children. The percentages are rounded to the nearest whole number.

Table 10. Consumption and Prize Selection

	Consumption (Weeks 1 – 4)							
	¼ Cup	½ Cup	¾ Cup			All		Total
Amount Consumed	2	2	12			332		348[†]
Proportion	0.57%	0.57%	3.45%			95.40%		100%
	Prize Selection (Weeks 2 & 3)							
	Pencil Sharpeners	Pamphlets	Pencils	Rings	Gliders	Rubber Balls	Finger Lights	Total
Proportion	3.42%	2.48%	2.48%	3.12%	11.49%	33.54%	43.48%	100%[‡]

Note: The top panel depicts consumption of fruits and cookies throughout the experiment and the bottom panel depicts prize selection in weeks 2 and 3. The total number of prizes ordered were as follows: 72 pencil sharpeners,72 pamphlets, 144 plastic rings, 100 pencils, 72 gliders, 144 rubber balls, and 140 finger lights. [†] Of 358 decisions, 349 resulted in the choice of a dessert. However, total consumption observations sum to 348 as one child dropped their fruit cup and did not ask for it to be replenished. This child had dropped the contents of their chosen dessert cups several times during the duration of the experiment but had asked for it to be replenished each time, with this time being the sole exception. [‡] Refer to footnote 5 for details.

Table 11. Intervention and Post-Intervention Effects

	Treatment Effect		Removal of Treatment Effect	
	(1)	(2)	(3)	(4)
	Logit Model	LPM	Logit Model	LPM
Intervention	2.090***	0.372***		
	(0.410)	(0.0732)		
	[1.287, 2.894]	[.229, .516]		
Post-Intervention			-2.507***	-0.429***
			(0.471)	(0.0768)
			[-3.429, -1.585]	[-.579, -.279]
Constant		-0.196***		0.209***
		(0.0549)		(0.0236)
		[-.303, -.088]		[.163, .255]
Observations	257	275	264	266

Note: Models (1) & (3) are logistic models with fixed-effects and clustered standard errors (both at the individual level); Models (2) & (4) are linear probability models with fixed-effects and clustered standard errors (both at the individual level). Standard errors in parentheses; 95% confidence intervals in brackets; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 12. Week and Day Intervention Effects

Week 1 (Omitted)	(1) LPM - Week Effects	(2) LPM - Day Effects
Week 2	0.451***	
	(0.0734)	
	[.307, .595]	
Week 3	0.283**	
	(0.0885)	
	[.109, .457]	
Week 2 – *first half*		0.494***
		(0.0860)
		[.325, .662]
Week 2 – *second half*		0.387***
		(0.0845)
		[.222, .553]
Week 3 – *first half*		0.296**
		(0.0920)
		[.115, .476]
Week 3 – *second half*		0.264*
		(0.114)
		[.039, .488]
Constant	-0.199***	-0.198***
	(0.0544)	(0.0545)
	[-.305, -.092]	[-.305, -.091]
Observations	275	275

Note: Model's (1) & (2) are linear probability models with fixed-effects and clustered standard errors (both at the individual level). The baseline week is omitted in model (1) and, similarly, the baseline days are omitted in model (2). Standard errors in parentheses; 95% confidence intervals in brackets; * $p < 0.05$, ** $p < 0.01$, ***$p < 0.001$.

Appendix

Figure A1. Survey Questions

How often does this happen?	Never 1	Seldom 2	Sometimes 3	Often 4	Always 5	Are you Satisfied With this?
My child enjoys eating fruits	1	2	3	4	5	Yes No
My child asks for fruits	1	2	3	4	5	Yes No
My child feeds him/her-self fruits	1	2	3	4	5	Yes No
I feel confident my child eats enough fruits	1	2	3	4	5	Yes No
My child asks for fruits between meals	1	2	3	4	5	Yes No
My child eats fruits	1	2	3	4	5	Yes No

These questions were adapted form the Child Eating Behavior Inventory (CEBI), a parent-report instrument designed to assess childhood eating and mealtime problems. These questions were adapted in the following way:

1. Question 7 of CEBI: "My child enjoys eating"
    a. Adapted to read: "My child enjoys eating fruits"

2. Question 8 of CEBI: "My child asks for food which he/she shouldn't have"
   Question 25 of CEBI: "My child asks for food between meals"
    a. Adapted to read: "My child asks for fruits"

3. Question 9 of CEBI: "My child feeds him/her self as expected for his/her age"
    a. Adapted to read: "My child feeds him/her-self fruits"

4. Question 11 of CEBI: "I feel confident my child eats enough"
    a. Adapted to read: "I feel confident my child eats enough fruits"

5. Question 25 of CEBI: "My child asks for food between meals"
    a. Adapted to read: "My child asks for fruits between meals"

6. Question 17 of CEBI: "My child eats quickly"
   Question 27 of CEBI: "My child eats chunky food"
    a. Adapted to read: "My child eats fruits"

Figure A2. Dessert Combinations

	Dessert Combination							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Week 1†								
Mon. (7/11)	X							
Tues. (7/12)	X							
Wed. (7/13)	X							
Thurs. (7/14)		X						
Fri. (7/15)	X							
Week 2*								
Mon. (7/18)			X					
Tues. (7/19)				X				
Wed. (7/20)					X			
Thurs. (7/21)						X		
Fri. (7/22)		X						
Week 3*								
Mon. (7/25)							X	
Tues. (7/26)								X
Wed. (7/27)					X			
Thurs. (7/28)				X				
Fri. (7/29)			X					
Week 4‡								
Mon. (8/1)		X					X	
Tues. (8/2)								
Wed. (8/3)						X		
Thurs. (8/4)	X							
Fri. (8/5)								X

Notes: Dessert combinations are as follows: (1) Apples, bananas, purple grapes, and chocolate chip cookies; (2) Apples, bananas, green grapes, and chocolate chip cookies; (3) Pears, bananas, purple grapes, and chocolate chip cookies; (4) Apples, bananas, green grapes and Oreo cookies; (5) Apples, bananas, purple grapes, and Oreo cookies; (6) Pears, green grapes, purple grapes, and chocolate chip cookies; (7) Pears, purple grapes, bananas, and Oreo cookies; (8) Pears, green grapes, bananas, and chocolate chip cookies.

† Baseline week

* Intervention Week

‡ Post-intervention week

Additional Information: Each fruit cup contained roughly four diced pieces of an apple or pear, five grapes, and four pieces of banana, in that order (~85 grams). Each cookie cup consisted of one and a half cookies (if chocolate chip) or two cookies (if Oreos). That the fruit cups may appear more full than the cookie cups may be a potential source of bias. Some children may have, for example, preferred the cookie cups for dessert simply because they are satiated and do not wish to eat a lot for dessert. This would be a source of downward bias for the effect of the intervention. On the other hand, children may wish to choose the cup they believe offers the most food. If so, then this would bias the results upwards. It's hard to imagine that this is the case though, as children are unlikely to do such cost-benefit analysis and, after

just having eaten lunch, it's unlikely that they are still so hungry that they would choose fruit cups simply because they contain more food. In fact, children were sometimes able to get a second serving of lunch if they so desired, and some did. The inclusion of a baseline observation week in the analysis should, however, account for such effects.

Table A20. Comparison of Coefficients

		Week 2		Week 3	
	Week 2	*1st half*	*2nd half*	*1st half*	*2nd half*
Week 1	.017	–	–	–	–
Week 2					
1st half	–	–	0.231	0.035	0.051
2nd half	–	–	–	0.107	0.282
Week 3					
1st half	–	–	–	–	0.749
2nd half	–	–	–	–	–

Note: These *p*-values correspond to tests of equality between the coefficients in Table 4. *t*-tests were used to compare differences in coefficients for statistical significance and the *p*-values are reported. The first half of each week is comprised of Monday, Tuesday, and Wednesday. The second half is comprised of Thursday and Friday.

Table A21. Robustness Check: Attendance

	(1) Intervention Introduction	(2) Intervention Removal	(3) Week Effects	(4) Day Effects
Intervention	0.365***			
	(0.0746)			
Post-Intervention		-0.429***		
		(0.0768)		
Week 2			0.454***	
			(0.0762)	
Week 3			0.275**	
			(0.0895)	
1st half of Week 2				0.476***
				(0.0881)
2nd half of Week 2				0.373***
				(0.0911)
1st half of Week 3				0.282**
				(0.0985)
2nd half of Week 3				0.254*
				(0.116)
Constant	-0.190***	0.209***	-0.198***	-0.187**
	(0.0560)	(0.0236)	(0.0559)	(0.0579)
Observations	267	263	264	252

Note: Columns 1 through 4 present the results from linear probability models with fixed-effects and clustered standard errors (both at the individual level). The sample is restricted so that each individual attends at least once in any given period of interest and therefore contributes to the analysis. Coefficients denote treatment effects. Standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A22. Robustness Check: Alternative Days

	(1) Day Effects
Week 2 – *first half*	0.500***
	(0.0955)
Week 2 – *second half*	0.416***
	(0.0767)
Week 3 – *first half*	0.389***
	(0.0972)
Week 3 – *first half*	0.210*
	(0.0973)
Constant	-0.189***
	(0.0542)
Observations	275

Note: These are the results from a linear probability model with fixed-effects and clustered standard errors (both at the individual level). The baseline days are omitted. Standard errors in parentheses; * p < 0.05, ** p <0.01,***p<0.001.

Table A23. Comparison of Coefficients

		Week 2		Week 3	
	Week 2	*1st half*	*2nd half*	*1st half*	*2nd half*
Week 2					
1st half	–	–	0.321	0.311	0.006
2nd half	–	–	–	0.691	0.017
Week 3					
1st half	–	–	–	–	0.043
2nd half	–	–	–	–	–

Note: These *p*-values correspond to tests of equality between the coefficients in Table A3. *t*-tests were used to compare differences in coefficients for statistical significance and the *p*-values are reported. The first half of each week is comprised of Monday and Tuesday. The second half is comprised of Wednesday, Thursday, and Friday.

Table A24. Robustness Check: Excluding Children with Parents on Site

	Treatment Effect		Removal of Treatment Effect	
	(1)	(2)	(3)	(4)
Intervention	2.343***	0.386***		
	(0.430)	(0.0765)		
Post-Intervention			-2.763***	-0.437***
			(0.587)	(0.0895)
Constant		-0.206***		0.211***
		(0.0573)		(0.0275)
Observations	214	232	223	225

Note: Model's (1) & (3) are conditional logit with fixed-effects and clustered standard errors (both at the individual level); Model's (2) & (4) are linear probability models with fixed-effects and clustered standard errors (both at the individual level). Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Chapter 3

Think of the Children? The Effect of Children on Public Support for the Supplemental Nutrition Assistance Program

Introduction

Public opinion of government programs is often group-centric in that it is shaped by attitudes towards the social or demographic groups perceived to be their beneficiaries (Nelson & Kinder, 1996; Converse, 2006; Winter, 2008). This is particularly true for welfare programs, which are highly politicized and racialized and often the target of reforms that limit their scope and coverage. Motivating these reforms are perceptions of benefits recipients as undeserving of aid—predicated on the idea that many are able to work but choose not to—and (or) the association of welfare with black Americans (e.g., Gamson & Lasch, 1983; Gilens, 1999). The Supplemental Nutrition Assistance Program (SNAP), the largest food assistance program in the United States, is no exception (Goren, 2008).

Growth in caseloads and costs in the decades since welfare reform have once again made the program a subject of scrutiny (Dickert-Conlin et al., 2016). At the federal level, the Trump administration seeks to cut spending on SNAP by $230 billion over ten years by restricting eligibility, raising administrative burdens, imposing more stringent program rules, reducing benefits, and limiting access to food (Rosenbaum & Nuerberger, 2020).[40] If adopted, these proposals would deny or reduce benefits to millions of adults as well as millions of children, who make up 44 percent of all SNAP beneficiaries (Cronquist, 2019; Rosenbaum & Nuerberger,

[40] Sonny Purdue, the United States Secretary of Agriculture under the Trump administration and head of the United States Department of Agriculture, which administers SNAP, has referenced the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (colloquially referred to as "welfare reform") and arguments motivating its passage in defending proposals to reform the program (e.g., press release no. 0025.19, available at: https://bit.ly/2VUQ0w4).

2020).[41] This despite evidence that current benefits are exhausted quickly upon receipt, forcing many households to reduce their caloric intake towards the end of their benefits cycle (Shapiro, 2005; Todd, 2015; Smith et al., 2016).

To the extent that attitudes towards SNAP are group-centric and rooted in misconceptions about the composition of its beneficiaries, highlighting the children it serves may make the program more appealing to the public and politically sustainable. Highlighting the child beneficiaries of the program—who presumably cannot be underserving of aid and draw more empathy than adults—may induce greater support for it (Schneider & Ingram, 1993). Alternatively, however, an emphasis on children may be "self-undermining" (e.g. Levine, 2015) if it evokes thoughts of irresponsible parenting or out of wedlock births, thereby reinforcing existing stereotypes or creating new ones, leading to a reduction in expressed support for the program or no change at all. Whether accentuating children might alter support for SNAP is therefore an empirical question.

This study investigates whether highlighting the child beneficiaries of SNAP can increase support for the program and overcome racial antipathies using a large-scale survey experiment. I first examine whether any frame evoking children—with and without imagery and regardless of their race—is effective. I then investigate whether this effect varies by the type of frame used. More specifically, I test whether imagery of children moderates the effect of simply mentioning children, and whether the race of the children shown matters. I also examine differential responsiveness to these frames across participants from politically influential social and demographic groups. In several extensions, I explore whether these frames can alter how strongly participants feel about their position on the program and their underlying motivations. I

[41] These reforms may also have the secondary effect of depriving hundreds of thousands of children access to free and reduced-price meals through the National School Lunch Program (Rosenbaum & Nuerberger, 2020).

also assess the generalizability of these effects to another welfare program, the Temporary Assistance for Needy Families (TANF), which provides cash assistance to families with children.

This study makes three important contributions. First, while the literature on the political and racial determinants of support for welfare programs is extensive, much less is known about the conditions under which these programs may become "de-politicized" or "de-racialized" (Winter, 2006). This study helps to fill this gap in the literature. In so doing, it also expands on the group-centric model of public opinion by considering how different characteristics of a group—in this case age and race—interact with deservingness to shape perceptions. Lastly, it presents evidence that black children may not induce the same level of empathy among some demographic and social groups as compared to white children.

To preview the main results, I find that highlighting the child beneficiaries of SNAP induces greater support for the program overall, and that this effect is smaller when children are characterized as black as compared to white. I also find that support increases among whites and Republicans, and high- and middle-income households, though, again, less so when the child beneficiaries are characterized as black. Only when race is not primed, or when children are characterized as white or diverse, is support for TANF increased. In sum, while highlighting children likely taps into ideas of deservingness that boost support for SNAP and TANF, these beliefs are tempered by attitudes towards blacks.

Background on SNAP

In 2019, SNAP provided $56 billion in benefits to nearly 36 million Americans. Though the primary aim of the program is to reduce food insecurity—defined as the lack of reliable access to food necessary for a healthy, active lifestyle—it also serves as an anti-poverty program

and an "automatic stabilizer" during economic downturns (Tiehen, Jolliffe, & Smeeding, 2015; Blinder & Zandi, 2015; Keith-Jennings & Rosenbaum, 2015).

According to federal guidelines for SNAP, eligibility for the program requires that household income not exceed 130% of the federal poverty level (FPL) and net income (gross income less certain deductions) not exceed 100% of the FPL, as well as satisfying two asset tests. Benefits are determined by subtracting 30% of net income from the maximum benefit guaranteed, which varies by household size, and are distributed via a debit card system known as Electronic Benefits Transfer. These benefits can only be used to purchase select foods at authorized retailers. Beginning in the late 1990s and accelerating in the 2000s, states have been afforded discretion over many aspects of the program's implementation, and can relax eligibility criteria, increase benefits generosity, and waive restrictions on the types of foods that can be purchased with benefits.[42] States are also responsible for setting the program's rules, outreach efforts, and sanctioning policies (Gabor et al., 2003).[43,44]

Additionally, ten states further delegate implementation of SNAP to county governments. Although counties in these states are required to comply with specific federal or state mandates, they are afforded considerable flexibility in implementing the program otherwise (National Association of Counties, 2019). Street-level bureaucrats, such as case workers in local offices, can also influence program outcomes by implementing the programs in ways that may encourage or deter participation among prospective or current beneficiaries. For example, workers in local offices can deter participation by creating an unwelcoming or stigmatizing atmosphere,

[42] Though benefits generosity can be increased by adding to allowable deductions in the calculation of net income, doing so does not significantly lower net income and benefits levels do not vary substantially across states.
[43] These practices can include whether and what kind of information is disseminated about the program, whether or not applicants can apply online, unannounced home visits to detect fraud, how often households need to recertify, and the kind of sanctions that are imposed on households who fail to meet the program's requirements.
[44] For a more detailed overview of SNAP and its implementation, see Courtmanche et al. (2019).

dedicating less time and fewer resources to outreach, or exerting little effort in guiding prospective beneficiaries through the application process (Lopez-Landin, 2013; Kogan, 2017).

The devolution of the program has produced a wide array of eligibility criteria and program rules that have contributed to large differences in participation rates among eligible households across and within states (Kogan, 2017; Call & Shimada, 2018; Cunnyngham, 2019). The lack of uniformity in the implementation of SNAP has implications for social equity and economic inequality, expanding its reach and impact in some contexts while limiting them in others (Dickert-Conlin, et al., 2016; Shaw, 2009). In fact, participation rates have been found to be lower and application denial rates higher in jurisdictions with higher minority populations and where support for redistributive government policies is lower (Kogan, 2017). Consequently, a black household with children is more likely than a similar white household to live in a state or county with more stringent eligibility criteria and program rules that might impede their access to benefits.

To the extent that local political and social considerations reflect misconceptions about SNAP beneficiaries and manifest in state and local policies through elected officials and bureaucrats, adjusting these perceptions may bolster support for the programs and help to minimize or eliminate disparities in its implementation.

Theoretical Framework and Relevant Literature

Public opinion influences policy through elected and appointed officials (Burstein, 2003; Stimson, 2004). State and local lawmakers and bureaucrats may be responsive to their political and social contexts for several reasons. First, they may be subject to political processes (e.g., elections or referrals) that require them to adhere to the policy preferences of their electorate. Second, social pressures exerted by local interest groups or important constituencies (e.g.,

wealthier residents) may constrain their behavior. Third, as members of the communities they live or work in, they likely share the same preferences and attitudes as those of the majority in their jurisdiction.

Public opinion can, in turn, be influenced by how those policies are presented, or framed (Chong & Druckman, 2007). Government programs are often framed in ways that shape public perceptions of the issues they are meant to address, thereby influencing attitudes towards the policies themselves.[45] These frames serve as heuristics that help individuals conceptualize complex issues (e.g., poverty), suggest explanations for them (e.g. who bears responsibility), standards for judging them (e.g. their consequences), and viable solutions (e.g. government intervention) (Nelson & Kinder, 1996).

The effects of framing are particularly apparent with respect to welfare programs. Although Americans consistently express support for greater assistance to the poor when asked in abstract terms, many also consistently oppose government spending on aid to the poor when questions about assistance are framed in terms of specific policies (Rasinski, 1989; Green & Kern, 2012; Berinsky et al., 2012). The lack of support for welfare programs has been attributed to racial bias against their presumed recipients and, more generally, perceptions of their beneficiaries as undeserving of government aid.

In their study cataloging frames associated with welfare in the 1960s and 1970s, Gamson & Lasch (1983) identified the "freeloader frame"—which advances the notion that many of the poor are simply taking advantage of government assistance programs—as particularly pervasive. This frame is also closely linked to racial attitudes towards blacks. As documented in detail in

[45] Framing effects have been shown to alter support for Social Security (Winter, 2006), school vouchers (Brewer & Gross, 2005), affirmative action (Kinder & Sanders, 1990), and government spending (Jacoby, 2000), among many others. For more on framing theory and a review of the literature, please see Chong and Druckman (2007).

Gilens (1999), decades of negative media coverage of poverty associated welfare, and its abuse, with blacks lacking in work ethic.[46] In fact, the relationship between opinions on welfare and attitudes towards blacks is so strong as to suggest the two are nearly synonymous: mentioning welfare, even absent any racial cues, elicits thoughts of blacks and their "moral failings." Research has consistently found antipathy towards blacks to be among the most important determinants of weak support for welfare programs (Jacoby, 2000; Winter, 2006; Goren, 2008; Ellis & Faricy, 2019). A separate strand of literature finds that states in which blacks constitute a larger share of welfare caseloads have narrower eligibility criteria, more stringent program rules, and less generous benefits (Soss et al., 2001; Fellowes & Rowe, 2004).

Public opinion on government programs is often group-centric in that it is influenced by attitudes citizens possess toward the social or demographic groups perceived to be their beneficiaries (Nelson & Kinder, 1996; Converse, 2006; Winter, 2008). Group-centric thinking is not immutable, however. Just as public opinion has been conditioned by decades of media coverage and political discourse to associate welfare programs with the undeserving, blacks, or both, these same programs can be re-framed in ways that divert attention away from these groups or accentuate other groups perceived as more deserving, thereby shifting opinion in favor of such programs. Children, as among the main beneficiaries of welfare programs, may serve as one such group, though studies evaluating the efficacy of frames using children are scarce, outdated, and produce mixed results.[47]

[46] Welfare is not as strongly associated with other minority groups. Whites generally have a far more favorable opinion of Asians (who are often stereotyped as a "model minority") and Hispanics as compared to blacks, Studies examining the link between attitudes towards Hispanics and support for, or implementation of, welfare programs have found mixed or null results (Soss et al., 2001; Schram et al., 2009; Hussey & Pearson-Merkowitz, 2013). In contrast, support for welfare programs and their implementation has consistently been linked to attitudes about blacks. For this reason, this study focuses primarily on whites and blacks, rather than Hispanics or Asians.

[47] Using the typology developed by Schneider and Ingram (1993), perceptions of "dependents," like children, are shaped by positive frames and they are considered more deserving of aid.

In a lab experiment, Iyengar (1990) finds that while participants exposed to a treatment mimicking news coverage of children affected by poverty expressed more sympathy than those in a similar treatment using adults, the mostly white participants expressed less sympathy when the coverage involved black children as compared to white children. Supplementing these results with data from a nationally representative survey, Iyengar (1990) also found greater support for families described as having larger numbers of children, though the vignettes used did not vary descriptions of the families by race.

In another lab experiment, Nelson and Oxley (1999) tested support for a family cap limiting welfare benefits for families with children across groups of participants exposed to an article emphasizing the potential consequences it would have for child poverty together with a picture of a black boy and an article emphasizing the importance of personal responsibility and the need to disincentivize child birth among those who could not support their families together with a picture of a black woman and four children. Although no difference was found between the two groups in their support for the family cap, those presented with the picture of the child were more likely to believe in the importance of protecting poor children.

This study updates and improves upon this literature. In addition to the mixed findings of these previous studies, decades of social, economic, and political developments leave the question of whether framing welfare programs as benefiting children can alter public support for them unresolved.[48] I improve on previous work by conducting a survey experiment using a larger and more diverse sample as opposed to a lab experiment relying on small samples made up of local white (or predominantly white) volunteers. The use of a survey experiment has the added

[48] One particularly noteworthy development has been the treatment of undocumented children crossing into the United States, which has garnered significant media attention in recent years and become a highly politicized and polarizing issue.

advantage of testing frames in the real-world settings in which individuals formulate their opinions, and my more diverse sample allows for heterogeneity analysis across participants from diverse backgrounds. Additionally, this study extends the literature in several ways.

Nelson and Kinder (1996) find that the addition of an image to a frame can have a substantial impact on its efficacy. I assess the moderating effect that imagery of children can have on a frame only mentioning children. They also show that group-centric thinking can be shaped by multiple features of a single target group in ways that may compound or counteract each other. I therefore also explore the interaction of age and race with perceived deservingness. While children may be considered more deserving in general, their characterization as black may evoke negative stereotypes that negate perceptions of their deservingness. In contrast, white children may evoke positive stereotypes that raises perceptions of their deservingness.

Existing research also finds consistent links between being white, high income, or politically conservative with lower support for cash and food assistance (e.g., Jacoby, 2000; Soss et al., 2001; Fellowes & Rowe, 2004; Winter, 2006). I leverage the diversity of my sample to investigate how framing SNAP as benefiting children can boost support for the program across these politically influential groups. Going beyond gauging changes in support for SNAP, I also document how different frames highlighting children affect the motivations underlying expressed preferences. Specifically, I track changes in beliefs commonly tied to support for welfare, including deservingness, egalitarianism, and attitudes towards government (Ellis & Faricy, 2019).

Finally, I explore the efficacy of a frame highlighting children in the context of TANF.[49] Unlike SNAP, which provides benefits that can only be used for the purchase of food, TANF

[49] The aims of TANF include the provision of aid to needy families with children, promoting employment, preventing and reducing out of wedlock pregnancies, and encouraging marriage. Total federal and state TANF

provides cash-assistance. States also have far more discretion in implementing TANF, resulting in even starker differences in participation rates and benefits levels across the country that reinforce social inequity and economic inequality (Floyd, 2020; Thompson, 2018). How programs are designed can also influence public opinion, and direct cash payments are particularly unpopular (Shaw, 2009; Ellis & Faricy, 2019). As such, effects may vary across the two programs.

A priori, relative to no mention or imagery of children, I expect that frames highlighting children will induce greater support for SNAP, that the use of imagery will boost this effect, and that induced support will be lower when children are characterized as black as compared to white. I also expect whites, Republicans, and those from middle- and high-income household to be responsive, though less responsive than their black, Democrat, and low-income counterparts. I have no a prior expectations as to how effects might vary across SNAP and TANF.

Experimental Design, Data, and Sample

Experimental Design

I employ a survey experiment in which participants are randomized into a control condition or one of four treatment conditions. A randomized survey experiment is ideal as it allows for an evaluation of the causal impact of the treatments as they might be employed in real world settings. In each condition, participants are presented nine statements pertaining to the spending activities of various organizations, including governments, non-profits, and for-profit

expenditures amounted to $31.3 billion in 2018, only $6.7 billion of which was dedicated to cash assistance. federal guidelines only require that benefits receipt be tied to work activities, and that states not provide cash assistance from federal funds to families that include an adult recipient for longer than 60 months (with some exceptions) and to legal immigrants who have not lived in the country for at least five years. Illegal immigrants are barred from any assistance through either federal or state TANF funds. Otherwise, states can choose what services to provide and can set their own eligibility criteria, outreach efforts, program rules, sanctioning policies, and benefits levels. The overall effect of devolution on cash assistance has been particularly stark. Participation rates vary from 4 percent in Louisiana to 68 percent in California (Floyd, 2020), and benefits vary from $170 per month for a family of three with no income in Mississippi to $923 for the same family in Alaska (Thompson, 2018).

businesses. In the order that they appear in the survey, these are the state government of California (California), Johnson & Johnson (J&J), the federal government (for SNAP), Susan G. Komen (Komen), Amazon, government of New York City (NYC), Walmart, the National Football League (NFL), and state and federal governments (for TANF). After being presented with a statement, participants are asked whether they think spending on the specified activity should be increased, decreased, or remain unchanged, how strongly they feel about their spending preference, and the reasons motivating their choice. Asking about spending preferences in this way is frequently used to gauge support for government programs and taps into general sentiments about them (Stimson, 2004; Winter, 2006). The survey concludes with an attention check question followed by a questionnaire collecting data on participant's socioeconomic and demographic characteristics.[50]

Participants in all conditions are presented the same set of statements, the exceptions being those pertaining to SNAP and TANF, which are altered to mention children in the treatment conditions. These statements are shown in **Figure 10**. In the *Control* condition, participants are provided a statement that explains what SNAP and TANF are and how much they cost with no mention of the children served by the programs.[51] In the first treatment condition, *Text Only* (*TO*), both statements are altered to mention children. Participants in the remaining three conditions are presented the same statements as those in the *Text Only* condition together with an image.

[50] The attention check questions present a statement about a for-profit company's spending on an activity but also instructs participants to ignore the statement and select "remain unchanged."

[51] This is commonly how public opinion polls ask such questions. For example, from a 2018 Politico-Harvard poll: *"Another part of the US Farm Bill is support for SNAP, the Supplemental Nutritional Assistance Program, formerly known as food stamps. This program provides government-supported food purchasing assistance to millions of low-income people in the US. In the new Farm Bill, do you think federal spending on the SNAP or food stamp program should be increased, decreased or kept about the same?"*

These images are shown in **Figure 11** The images accompanying the SNAP and TANF statements for those in the *Text & Diverse Image* (*TDI*) condition show children of varying racial and ethnic backgrounds so as to avoid contaminating this condition with the race-specific images those in the *Text & Black Image* (*TBI*) and *Text & White Image* (*TWI*) are exposed to. The images accompanying the SNAP statement in the latter two treatment conditions depict a black and white boy-girl pair, respectively. As three quarters of all child beneficiaries of SNAP are below the age of eleven, the images were chosen so as to depict young children.[52,53] While the TANF statements in the *Text & Black Image* and *Text & White Image* conditions are accompanied by the same racially and ethnically diverse image that appears in the *Text & Diverse Image* condition, the racial framing is primed by the race-based images accompanying the preceding SNAP statement. In other words, given the sequence in which statements appear, participants in the image treatments are exposed to a single image when asked about spending on SNAP, and two images when asked about spending on TANF (the earlier SNAP image as well as the TANF image).

After participants indicate their preferred spending position in response to each statement, a follow-up question asks them to indicate how strongly they feel about their position by choosing a number on a continuous scale that ranges from 0 ("Not that strongly") to 3 ("Very strongly").[54] Depending on their spending preference (increase, decrease, or remain unchanged), they are then presented with a list of reasons that may have motivated their position, covering the: (1) assistance beneficiaries of an activity are receiving, (2) extent of an entity's involvement

[52] In 2018, 29.4% of child beneficiaries of SNAP were between 0-4 years old, 17.9% were between 5-7, 24.3% were between 8-11, and 19.9% were between 12-15 years (Cronquist, 2019).
[53] These images were shown to 100 participants in a separate survey conducted using Lucid. Participants were asked to indicate what age they believed the children in the images to be. The average age for the SNAP images were 5.6, 7.6, and 8.9 across the Text & Diverse Image, Text & Black Image, and Text & White Image conditions, respectively. The average age for the TANF image was 9.6.
[54] On this scale, 1 and 2 indicate "Somewhat strongly" and "Strongly," respectively.

an activity, (3) use of resources for an activity, (4) desirability of an activity, (5) and an entity's wastefulness in performing the activity. For each reason, they are asked to specify a number on a continuous scale ranging from 0 ("Not important") to 3 ("Very important").[55] These are presented in **Figure 12.** In the context of SNAP and TANF, these motivations are used to capture beliefs about deservingness (1), government involvement (2), egalitarianism (3 & 4), and government efficiency (5).

To mask the objective of the experiment, the SNAP and TANF statements are embedded among statements covering other activities carried out by various organizations that are all charitable in nature and which benefit individuals. These statements are also accompanied by images in the treatment conditions involving imagery. The images associated with each statement were chosen to be relevant to the activity specified, and are shown in **Figure A3** of the Appendix.

In order to minimize any priming or anchoring effects, participants are first exposed to two statements covering the activities of a state government (California) and a for-profit firm (Johnson & Johnson) and two varying levels of expenditures ($75 billion and $9 billion, respectively) so as to assuage suspicions that the experiment is intended to gauge support for government spending on a particular program and avoid having respondents base their evaluations relative to the lower or higher spending levels of other organizations. Participants are then presented the statement pertaining to SNAP. The survey then continues with five more statements covering the activities and expenditures of other entities, with the statement about TANF appearing last in the sequence. The survey statements are presented in **Figure A4** of the Appendix.

[55] On this scale, 1 and 2 indicate "Somewhat important" and "Important," respectively.

Data

The survey experiment was administered by Lucid between February 20th and March 6th of 2020 and involved 3,878 participants. Lucid recruits adult participants from a large and diverse pool of respondents and uses a quota sampling procedure to construct samples demographically similar to the general population as reflected in the United States Census across age gender, ethnicity, and region.[56] Samples collected using Lucid have been shown to closely mirror probability-based samples of the broader population and have been used to successfully replicate survey experiments relying on both convenience and probability-based samples (Coppock and McClellan, 2019).[57]

As mentioned above, all participants were asked questions pertaining to their socioeconomic and demographic characteristics, including their gender, age, race, ethnicity, household income, nativity, marital status, parental status, employment status, utilization of social assistance programs, political party affiliation, and political ideology. This information was supplemented with data collected by Lucid on educational attainment and region of residence.[58]

I used this information to create a set of indicator variables for whether a participant: is female; is young, middle aged, or elderly; is Hispanic, white (*white*), black (*black*), or other

[56] See Coppock and McClellan (2019) for additional details about Lucid and its sampling procedure.
[57] Lucid offers several advantages over Amazon's MTurk services, which has been widely used to test social scientific experiments (until recently, Lucid catered primarily to market research firms). First, Lucid taps into a larger and more diverse pool of respondents. Second, samples collected using Lucid better reflect the demographic, political, and psychological profiles of the general population. Third, critics argue that MTurk samples include respondents that are "overfished," "professionalized," and potentially fraudulent (e.g., bots). In contrast, 375,000 unique respondents pass through Lucid's "marketplace" each day and there has been no evidence of fraudulent responses to date.
[58] Lucid also provides data on respondents gender, age, race, ethnicity, household income, and political party affiliation. Comparing the data on these characteristics provided by Lucid to those collected as part of the survey reveals few discrepancies. Whenever possible, I use the variables collected as part of the survey in my analysis as they are more comprehensive. As a robustness check, I use the data provided by Lucid. Results are substantively similar and available upon request.

(Asian, Native American, or Pacific Islander); has a bachelor's degree or higher; comes from a low, middle, or high income household; was born in the United States; is married; is a parent; is employed; is on social assistance; is a Democrat (*dem*), Republican (*repub*), or Independent/other (Green Party, Libertarian Party, or other); is liberal, conservative, or moderate; or resides in the South, Northeast, Midwest, or West. I also create variables for whether a participant is non-white (*nonwhite*), non-black (*nonblack*), non-Republican (*nonrepub*), and non-Democrat (*nondem*), and non-high- (*nonhighinc*), non-middle- (*nonmiddleinc*), and non-low income (*nonlowinc*).

My key measures of interest include a set of binary variables indicating whether a participant was assigned to the *Control*, *Text Only*, *Text & Diverse Image*, *Text & Black Image*, or *Text & White Image* condition. These variables take a value of 1 if the participant was assigned to the condition and 0 otherwise. Similarly, I create two additional binary variables indicating assignment to any of the treatment conditions (*AnyTreat*) and any of the image treatment conditions (*AnyImageTreat*). My primary outcome measure, *Increase SNAP*, captures support for SNAP and takes a value of 1 if a participant indicated a preference for increased spending on the program and 0 if their preference was for spending to remain unchanged or be decreased. I also create two alternative sets of outcome measures. To gauge shifts in preferences away from decreased spending and allow for a more expansive definition of support, *Increase or No Change SNAP* takes a value of 1 if a participant preferred spending to remain unchanged or be increased, and 0 otherwise. The second is a categorical variable, *Spending SNAP*, that takes a value of 0, 1, or 2 if a participant preferred spending to decrease, remain unchanged, or increase,

respectively. I create variables *Increase TANF*, *Increase or No Change TANF*, and *Spending TANF* similarly to capture support for TANF.[59]

To represent how strongly participants feel about their indicated spending preference, I create continuous variables *Strength SNAP* and *Strength TANF* ranging from 0 ("Not that strongly") to 3 ("Very strongly"). I create continuous variables corresponding to each of the reasons that may have motivated participants' chosen spending preferences similarly. These are *Assistance*, *Involvement*, *Resources*, *Desirability*, and *Wastefulness*. Finally, I create a set of indicator variables capturing preferences for increased spending on activities carried out by the other entities included in the survey. These include *California*, *J&J*, *Komen*, *Amazon*, *NYC*, *Walmart*, and *NFL*, and take a value of 1 if participants indicated a preference for increased spending and 0 otherwise.

Analytic Sample

The analytic sample includes 3,106 that passed the attention check embedded in the survey. Column 1 of **Table 13** presents summary statistics for this sample. The majority of respondents are between the ages of 35 and 64. A majority also report having children, household incomes between $25,000 and $74,999, being female, and being employed. The sample is ethnically and racially diverse, with 13 percent being Hispanic, 74 percent white, 12 percent black, and 14 percent "other" (Asian, Native American, or Pacific Islander).[60] A third of the sample has a bachelor's degree or higher, 44 percent are married, and 26 percent report

[59] It should be noted that it is possible for a participant to support SNAP or TANF but also believe that existing spending levels on the programs are adequate or perhaps higher than what is optimal. If so, they may express a preference for spending on the programs to remain unchanged or be decreased and the *Increase SNAP* and *Increase TANF* measures will underestimate the extent of support. This problem is somewhat ameliorated by the more expansive *Increase or No Change SNAP* and *Increase or No Change TANF* measures. These measures are not likely to result in an overestimation of support, however, as it is unlikely that a participant opposing the programs would prefer for their spending to be increased or remain unchanged.

[60] A participant may identify as both ethnically Hispanic and racially white, black, or other.

receiving some social assistance (e.g., unemployment insurance). Nearly the entire sample reports being born in the United States. With regards to political party affiliation, a plurality of the sample identifies as Democrat, with equal shares identifying as Republican and independent or other (Libertarian Party or Green Party). Ideologically, the sample is roughly divided equally across liberal, conservative, and moderate.

For comparison with the analytic sample, columns 2, 3, and 4 of **Table 13** present profiles of the general population as captured by the American Community Survey (ACS) for 2017, the General Social Survey (GSS) for 2018, and Gallup for February 2020.[61] Along most dimensions, the analytic sample appears very similar to either the ACS sample, the GSS sample, or both, even across characteristics that are not used by Lucid in its quota sampling procedure as seen in Panel B. Minor discrepancies are likely attributable to differences in the year data was collected and sampling variability inherent to probability-based samples. Some differences are large, however.

The analytic sample is younger, with 32 percent of participants between the ages of 18 to 34, compared to 23 percent in the ACS and 27 percent in the GSS. A larger fraction of the analytic sample, 51 percent, report a household income of between $25,000 and $74,999, as compared to 39 percent in the ACS. As such, the analytic sample has fewer respondents reporting a household income of $75,000 or greater than the ACS, 24 percent relative to 40 percent. As previously mentioned, 95 percent of the analytic sample reports being born in the United States as compared to 86 and 87 percent in the ACS and GSS, respectively. Perhaps most notably, as compared to Gallup polling, more of the analytic sample identifies as Democrat and a smaller share identifies as independent or other.

[61] The 2017 ACS, 2018 GSS, and 2020 Gallup data are the most recent releases available.

Unsurprisingly, these discrepancies may cast doubt on the representativeness of any treatment effects identified in the analytic sample as estimates of their true value in the general population. Nevertheless, absent any theoretical reason to the contrary, this study can still provide insights about the causal relationships between the treatments tested and their outcomes in the general population.[62] Given the diversity of the analytic sample on important characteristics and its similarity to nationally representative probability-based samples, there is no reason to believe that participation in the survey is correlated with opinions towards spending on SNAP and TANF in a way that might skew the results. As such, any observed shift in preferences for spending on SNAP and TANF in response to the treatments in the analytic sample can help inform our understanding of how such opinions might be expected to change in response to similar treatments in the general population.[63]

Table 14 presents summary statistics across the control and treatment conditions. Balancing tests reveal no meaningful differences across the treatment conditions relative to the control condition.[64] Comparing the spending preferences of those in the analytic sample assigned to the control condition to that found by a similar survey using a probability-based sample—noting sampling variability and differences in data collection years—further boosts the case for the generalizability of this study's finding to the broader population. Overall, in the case of

[62] Under the "fit-for-purpose" framework recommended by the American Association of Public Opinion Research (AAPOR), the choice between using a probability-based sample and a convenience sample depends on a study's objective (Baker et al., 2013). While descriptive work necessarily requires a probability-based sample, a convenience sample may be appropriate for research focused on documenting relationships between variables. See Baker et al. (2013) and Coppock and McClellan (2019) for additional details about the "fit-for-purpose" framework.
[63] In fact, many relationships between treatments targeting public opinion and their outcomes identified using convenience samples recruited through online platforms (e.g., Facebook, Qualtrics, MTurk, and Lucid) have been found to map onto the general population (Mullinix et al., 2015; Levay et al., 2016; Coppock et al., 2018; Coppock, 2019; Coppock and McClellan, 2019; Boas et al., 2020).
[64] The observed discrepancies across the treatment conditions relative to the control condition are not unusual given the small sample sizes involved, even with randomization. F-tests for joint significance did not indicate any statistically significant differences across the treatment conditions relative to the control condition, indicating balance across the conditions. Only 7 of 112 t-tests on difference-in-means relative to the control condition were statistically significant at conventional levels (p-value$\leq$0.10), less than what might be expected by chance.

SNAP, 19.7, 37.7, and 42.6 percent of those in the control condition preferred that spending be decreased, remain unchanged, or increased, respectively. Quite similarly, a July 2018 Politico-Harvard T.H. Chan School of Public Health survey found the share of respondents preferring a decrease, no change, or increase in spending to be 19, 39, and 37 percent, respectively. Breaking down spending preferences across the two by political party affiliation also shows that the analytic sample is remarkably representative. Among Republicans in the control condition, 35.4, 39.2, and 25.4 percent preferred that spending be decreased, remain unchanged, or increased, compared to 39, 44, and 13 percent in the probability-based sample. Among democrats, these figures are 9.1, 35.3, and 55.6 percent, and 7, 27, and 63 percent, for decreased spending, no change, or increased spending, respectively, across the two surveys.[65]

Analysis

For ease of interpretation, I use a baseline linear probability model for the main analysis. This model takes the following form:

$$Outcome_{it} = \beta_0 + \delta \boldsymbol{Treatment\ Condition_{it}} + \lambda \boldsymbol{Covariates_{it}} + \varepsilon_{it}$$

where subscripts *i* and *t* represent participant and control or treatment condition, respectively. Depending on the specification, $Outcome_i$ is either *Increase SNAP* or *Increase or No Change SNAP*. When examining the effect of being assigned to any of the treatment conditions, $\boldsymbol{Treatment\ Condition_{it}}$ is *AnyTreat* (with *Control* as the reference group). In regressions

[65] An analogous survey gauging public opinion on TANF or cash assistance by asking about spending preferences could not be found. However, the results from a poll conducted by Pew Research in September of 2019 found that, among those who agreed that there was too much income inequality in the United States, the vast majority preferred that the government invest in education and job training for the poor rather than provide direct assistance in the form of cash or tax credits overall and across income and political party affiliation (Horowitz, Igielnik, & Kochhar, 2020). Differences in survey design and subsampling aside, this is consistent with the preferences of those in the analytic sample assigned to the control condition, the majority of whom favored that spending on TANF be decreased or remain unchanged overall, across political party affiliation (with the exception of Democrats), and household income.

disentangling the effect by type of treatment, $\boldsymbol{Treatment\ Condition_{it}}$ is either a vector of binary variables indicating whether a participant is in the *Text Only* or *AnyImageTreat* conditions, or the *Text Only*, *Text & Diverse Image*, *Text & Black Image,* or *Text & White Image* conditions (with *Control* as the reference group). To increase the precision of my estimates, $\boldsymbol{Covariates_{it}}$ is a vector of variables capturing participant characteristics, including gender, age, race, ethnicity, educational attainment, household income, nativity, marital status, parental status, employment status, utilization of social assistance programs, political party affiliation, and region. I test the sensitivity of the results to the choice of a linear probability model by supplementing the analysis with logit and generalized ordered logit models. For the latter, I substitute *Spending SNAP* in place of the binary outcomes. When examining support for TANF, the outcome measures are *Increase TANF*, *Increase or No Change TANF*, or *Spending TANF*.[66]

To investigate differential responsiveness to the treatments across race, political party affiliation, and household income, I run separate regressions for each characteristic using the baseline model amended to include interactions as follows:

$$Outcome_{it} = \beta_0 + \delta \boldsymbol{Treatment\ Condition_{it}} * \boldsymbol{Characteristic_{it}} + \lambda \boldsymbol{Covariates_{it}} + \varepsilon_{it}$$

For example, when investigating the effect of the treatments on whites, I interact *white* with *Text Only*, *Text & Diverse Image*, *Text & Black Image*, and *Text & White Image*, and *nonwhite* with *control*, *Text Only*, *Text & Diverse Image*, *Text & Black Image*, and *Text & White Image* such that the reference group is whites assigned to the control condition. I do this similarly with *black* and *nonblack*, *repub* and *nonrepub*, *dem* and *nondem*, *highinc* and *nonhighinc*, *middleinc* and

[66] To further verify that results are not sensitive to the coding scheme used to measure the outcomes of interest, two other sets of "continuous" measures are also used as outcomes. For the first set, preferences for decreased spending, no change, and increased spending take values of -1, 0, and 1, respectively. For the second set, these preferences take on values of 0, 0.5, and 1, respectively. Results remain consistent and are available upon request.

nonmiddleinc, and *lowinc* and *nonlowinc* to investigate the effect on blacks, Republicans, and Democrats, and those from high-, middle-, and low- income households.

As extensions, I also explore how the treatments affect how strongly participants feel about their spending preference and the reasons motivating their decision. To assess changes in the strength of participants' spending preferences, the baseline model is estimated with *Strength SNAP* and *Strength TANF* as the outcomes. To examine their motivation, the outcomes used are *Assistance, Involvement, Resources, Desirability,* and *Wastefulness*. I run this analysis on subsamples stratified by spending preference. Finally, to check that any observed treatment effects are the result of the interventions employed and not some unintended manipulation, I assess the effect of assignment to the treatment conditions on preferences for increased spending on other activities. This is done by substituting *California, J&J, Komen, Amazon, NYC, Walmart,* and *NFL* for the outcome measures in the baseline model.

Main Results

Treatment Effects on Support for SNAP

Figure 13 depicts the percent of participants in each condition indicating a preference for increased spending on SNAP. The dashed horizontal line represents the share in the control condition. Participants expressed a greater preference for increased spending on SNAP across all treatment conditions relative to the control condition, though the difference is smaller and not statistically significant in the *Text & Black Image* condition. **Figure 14** presents the regression results.

Beginning with Panel A, the overall effect of highlighting the child beneficiaries of SNAP is to increase support for the program relative to the control condition. Assignment to any of the treatment conditions raises the likelihood that participants express a preference for

increased spending on the program by 7.9 percentage points. In panel B, the effect is allowed to vary across the different treatments. Interestingly, mentioning children with and without imagery has the same effect on support for SNAP, increasing the likelihood of preferring increased spending on the program by 7.8 and 7.9 percentage points relative to the control condition. However, disaggregating further reveals qualitatively meaningful differences in the efficacy of the different treatments employing imagery, though the estimates are not statistically distinguishable from one another. Accompanying the statement mentioning children with an image of a diverse group of children has a moderating effect, raising the likelihood of preferring increased spending to 10.2 percentage points. When the image is that of black children, however, the combined effect of mentioning children and imagery is lower than either only mentioning children or doing so together with an image of diverse children, increasing the likelihood by only 5.8 percentage points. Mentioning children together with an image of white children raises the likelihood by 7.8 percentage points.

Results from logit and generalized ordered logit models are consistent with these findings, revealing similar patterns as seen in Appendix **Table A25**. Relative to the share of participants indicating a preference for increased spending on SNAP (42.6 percent) in the control condition, these constitute large effects. The effect of the *Text Only, Text & Diverse Image, Text & Black Image,* and *Text & White Image* treatments is to increase preference for more spending on SNAP by 18 to 24 percent.

Shifting Preferences Away from Decreased Spending

Since SNAP is the target of reforms intended to reign in its scope and coverage, whether attitudes towards children can help to maintain at least existing levels of expenditures on the programs may also be important. **Figure 14** also presents the estimated treatment effects on the

likelihood of preferring that spending be increased or remaining unchanged. Using this more expansive measure of support for the program reveals the same patterns as before. As seen in Panel A, highlighting children is effective for increasing support for SNAP, raising the likelihood that participants prefer spending on the program to be increased or remain unchanged by 6.4 percentage points relative to the control condition. Letting the effect vary by the type of treatment in Panel B again shows the effects of mentioning children with and without imagery to be similar, raising the likelihood by 6.1 and 6.6 percentage points, respectively. Further disaggregating the results reveals qualitatively meaningful and statistically distinguishable differences, however.

These effects are 6.1, 9.1, 4.8, and 5.8 percentage across the *Text Only, Text & Diverse Image, Text & Black Image*, and *Text & White Image* treatments, respectively. The effect of the *Text & Diverse Image* treatment is statistically distinguishable from the others. The effect of only mentioning children is moderated by the addition of an image of a diverse group of children (*p-value*=0.095). An image of a diverse group of children is also more effective than when the children are characterized as black (*p-value*=0.022) or white (*p-value*=0.082). Also notable is that the gap between effects when images of black and white children are displayed is now smaller.

As before, results from logit and generalized ordered logit models are consistent with these findings, as seen in Appendix **Table A25**. To get a better sense of how preferences are changing, **Figure 15** shows the distribution of spending preferences for SNAP across the control and treatment conditions. Comparing the distribution of spending preferences in the treatment conditions to that of the control condition suggests that preferences are being shifted away from decreased spending and towards increased spending.

Differential Responsiveness Across Participants

Panel A of **Figure 16** shows preferences for spending on SNAP among participants in the control condition by race, political party, and income. Non-whites, Democrats, and low-income participants express support for increased spending at higher levels than whites, Republicans, and those from middle- and high-income households. This variation suggests that participants from diverse backgrounds may be differentially responsive to the treatments. **Table 15** presents the regression results by participant characteristics. The same pattern as before emerges across whites, Republicans, and those from high- and middle-income households, with greater increases in the likelihood of participants preferring more spending in response to the *Text Only*, *Text & Diverse Image*, and *Text & White Image* treatments as compared to the *Text & Black Image* treatment.

Among whites, the *Text Only*, *Text & Diverse Image*, *Text & Black Image*, and *Text & White Image* treatments raise the likelihood of preferring increased spending on SNAP relative to the control condition by 6.2, 13.7, 5.7, and 11.3 percentage points, respectively. Notably, the estimated effect is smaller when children are characterized as black as compared to white or diverse, differences that are statistically significant.[67] Similarly, among Republicans, the estimated coefficients are 10.2, 8.6, 3.3, and 7.3 percentage points, though the latter two are statistically insignificant. Relative to the share of Republicans in the control condition preferring more spending on the program (25 percent), the effects of the *Text Only* and *Text & Diverse Image* treatments constitute increases of 40.8 and 34.4 percent, respectively. The magnitude of the estimates do not vary substantially across the different treatments for those from high-income

[67] These effects are statistically distinguishable from one another at conventional levels (*p-value*≤0.10), with the exception of the *Text Only* & *Text & Black Image*, *Text Only* & *Text & White Image*, and *Text & Diverse Image* & *Text & White Image* conditions.

households, hovering around 7 and 9 percentage points, and are statistically significant only when children are mentioned together with an image of a diverse group of children.[68] Participants from middle-income households are more responsive than their higher-income counterparts.[69]

Among blacks, simply mentioning children has a large effect, raising the likelihood of preferring increased spending on SNAP by 16.7 percentage points, a 32 percent increase over the share of blacks preferring more spending in the control condition (53 percent).[70] Interestingly, blacks are not responsiveness to treatments that include imagery of a diverse group of children or white children, the estimates for which are negative, qualitatively small, and statistically insignificant. Although the effect when children are characterized as black is statistically insignificant, it is a qualitatively large 10.6 percentage points. Democrats, are roughly similarly responsive across all treatments. Considering that support for SNAP among democrats is high at nearly 56 percent in the control condition, these effects suggest that there is still room for bolstering support for the program among this demographic. Preferences for increased spending on SNAP are not altered among those from low-income households, who already support the program at higher levels than any another group.[71]

These results suggest that highlighting the children that benefit from SNAP can boost support for the program among whites, Republicans, and high- and middle-income households, key social and demographic groups with substantial political influence. Who the child

[68] Effects across treatment conditions among Republicans and those from high-income households are not statistically distinguishable from one another.
[69] The effects for the *Text Only* and *Text & Diverse Image* treatments are statistically distinguishable from that of the *TIB* treatment at conventional levels ($p\text{-}value \leq 0.10$).
[70] The effect of *Text Only* is statistically distinguishable from that of the *Text & Diverse Image* and *Text & White Image* conditions at conventional levels ($p\text{-}value \leq 0.10$).
[71] Effects across treatments conditions among Democrats and those from low-income households are not statistically distinguishable from one another.

beneficiaries are matters, however. The induced support for SNAP is smaller when the children are characterized as black as compared to white or racially and ethnically diverse. These results also suggest that highlighting child beneficiaries can also increase support for the program among groups that already lean towards supporting the program, such as Democrats and blacks.

Extensions

Strength of Preference and Motivation

Table 16 shows how the strength of participant's spending preference for SNAP was affected by the different treatments relative to the control condition, separately across participants who expressed a preference for spending to be decreased, remain unchanged, or increased. Among those that preferred decreased spending, the coefficient estimates are mostly negative—indicating that attitudes toward lower spending may be less firmly held than before—though none are statistically significant. This result is in line with the finding that shifts in attitudes towards increased spending on the program may be occurring primarily among those who would have otherwise preferred decreased spending. The *Text & Black Image* and *Text & White Image* treatments strengthened attitudes towards spending preferences among those that preferred spending to remain unchanged. These effects constitute 8.9 and 7.5 percent increases in strength of preference, respectively, relative to an average score of 1.7 among those in the control condition. Among participants who indicated a preference for increased spending, only the *Text & White Image* treatment altered the strength of attitudes, by 3.7 percent relative to an average score of 2.46 in the control condition.

The results for participants motivation among those who preferred spending to be decreased, remain unchanged, or increased are shown in **Table 17**. Beginning with the results in Panel A, participants who indicated a preference for decreased spending scored the statement

that program beneficiaries could be "doing more to help themselves" (*Assistance*) lower in terms of importance in response to the *Text Only* and *Text & Black Image* treatments. These effects are qualitatively large, constituting decreases of 9.6 and 8.1 percent relative to an average score 2.41 among those in the control condition, and suggest that participants may be more likely to view SNAP beneficiaries, including black beneficiaries, as deserving of aid when children are highlighted.

In Panel B, among those preferring no change in spending, the *Text & Black Image* treatment led to increases in how important participants felt about the statements that beneficiaries are "getting the assistance they need" (*Assistance*) and that the "government is as involved as it should be in food assistance" (*Involvement*). Relative to the average score in the control condition, these effects constituted increases of 10.3 and 9.7 percent, respectively. Lastly, among those that preferred increased spending in Panel C, only the *Text & White Image* treatment altered motivations, increasing the importance that participants put on the statements that spending on food assistance is a "desirable thing to do" (*Desirability*) by 3.3 percent relative to the average score in the control condition, respectively. This suggests that egalitarian sentiments are stronger when children receiving benefits are characterized as white.

Treatment Effects on support for TANF

As mentioned above, SNAP and TANF differ in their design, with the former providing benefits that can only be spent on food and the latter providing direct cash assistance. As such, the effect of highlighting children on support may differ across the two programs. As seen in **Figure 13**, preferences for increased spending on TANF is generally lower relative to that for SNAP. Regression results for TANF are presented in **Table 18**. Beginning with column 1 and Panel A, highlighting children raises the likelihood of preferring increased spending on TANF by

4.1 percentage points relative to the control condition. As seen in Panel C, however, the effect is driven by the priming of white children, which raises the likelihood of preferring increased spending by 9 percentage points, an increase of 21.4 percent relative to the share of participants preferring increased spending in the control condition.[72]

Expanding the definition of support to include preferences for spending to remain unchanged in column 2 reveals similar patterns as in the case of SNAP, though the estimated effects are less pronounced. Focusing on Panel C, the *Text Only*, *Text & Diverse Image*, and *Text & White Image* conditions raise the likelihood of support for preferring spending to be increased or remain unchanged by 4.1, 5.5, and 3.5 percentage points relative to the control condition, respectively. The effect when black children are primed is qualitatively smaller and statistically insignificant.[73,74] **Figure 17** shows that preferences for spending on TANF are being shifted away from decreased spending to no change, with the exception of the *Text & White Image* condition, which shifts preferences away from decreased spending and towards increased spending.

As in the case of SNAP, support for increased spending on TANF varies across participants in a similar manner, as shown in Panel B of **Figure 7**. **Table 19** presents the regression results across race, political party, and household income. Among whites, Republicans, Democrats, and those from high, middle, and low-income households, only when priming white children are the estimates meaningfully large, though only that for whites and

[72] The effect of *Text & White Image* is statistically different than each of the effects of *Text Only*, *Text & Diverse Image*, and *Text & Black Image* at conventional levels (*p-value*≤0.10)
[73] The effects of *Text & Diverse Image* and *Text & Black Image* are statistically different from one another at conventional levels (*p-value*≤0.10).
[74] As in the case of SNAP, results from logit and generalized ordered logit models are consistent with these findings, revealing similar patterns for as seen in Appendix Table A25.

Republicans is statistically significant. Those from middle-income households and Republicans are also responsive when children are mentioned without any racial cues or characterized as racially and ethnically diverse, respectively. In the case of TANF, the treatments did little to alter how strongly participants felt about their preferred spending on the program, except to soften attitudes among those who preferred decreased spending in the *Text Only* condition. Results are shown in Appendix **Table A26**.

The *Text Only* and *Text & Black Image* treatments lower the importance that participants preferring decreased spending placed on the statements that the "government should not be involved in cash assistance" (*Involvement*) and that spending on cash assistance is "not good a use of resources" (*Resources*) by 13.2 and 11.3 percent, respectively, relative to average scores in the control condition. The former result suggests that aversion to government intervention was less forceful when children were highlighted, while the latter suggests that the depiction of black children activates more egalitarian sentiments. There were no statistically significant changes in motivations among participants preferring spending to remain unchanged or be increased. Results are shown in Appendix **Table A27.**

Probing the Results

Sample Composition

I test the sensitivity of the main results to sample composition in two ways. First, I expand the analytic sample to include all participants, regardless of whether they passed the attention check question. Unsurprisingly, and as seen in **Table A28** of the Appendix, those who did and did not pass the attention check differ markedly. Using this more inclusive sample does not substantively alter the conclusions, however. Panel A of **Table A29** in the Appendix presents

the results, showing the same patterns as observed using the analytic sample.[75] Again, this bolsters the case for the generalizability of the results. Second, I restrict the analytic sample to only include participants who spent at least 5 minutes and no more than 25 minutes on the survey so as to exclude potentially anomalous responses from participants who may have spent too little or too much time answering the questions. Again, the results are substantively similar, as seen in Panel B of **Table A29** in the Appendix.

Manipulation Checks

If the shifts in spending preferences are in response to the specific interventions employed and not some unintended manipulation, assignment to the treatment conditions would not be expected to have much of an effect, if any, on spending preferences for activities conducted by other entities. As seen in Panel A of **Table A30** in the Appendix, with a few exceptions, assignment to the treatment conditions had no effect on preferences for increased spending on other activities. Most estimates are qualitatively small and statistically insignificant. Given that these results are based on comparisons with the control condition, the use of images could be a source of variation that may account for the observed statistically significant estimates. Panel B presents the effect of assignment to the *Text & Black Image* and *Text & White Image* conditions as compared to the *Text & Diverse Image* condition only. As before, estimates are mostly qualitatively small and statistically insignificant.

[75] These regressions include controls for gender, race and ethnicity, educational attainment, age, household income, region, political party affiliation, and whether participant passed the attention check question. Since survey participants had to pass the attention check to be shown the questions inquiring about their socioeconomic and demographic characteristics, only data provided by Lucid could be used in the analysis. As such, controls for nativity, marital status, parental status, employment, and social assistance could not be included.

Discussion

Welfare programs are often the subject of scrutiny among the public and targets for reform among legislators. Motivating these attitudes and efforts are perceptions of beneficiaries as undeserving of aid, disproportionately black, or both. Yet many of these beneficiaries are children, who presumably cannot be undeserving. To the extent that views on welfare programs are shaped by the groups perceived to be their beneficiaries, highlighting their more sympathetic demographics may make them more appealing. This study presents the results from a large survey experiment testing whether highlighting the child beneficiaries of SNAP—the largest food assistance program in the country and one of the largest welfare programs—can boost support for the program.

Overall, I find that accentuating the child beneficiaries of SNAP can boost support, narrowly defined, for the program by nearly 19 percent. This is a meaningfully large effect. I also find that this effect varies depending on how children are highlighted. Although the estimates were not statistically different from one another, the effects of simply mentioning children and mentioning children with an image of diverse children, black children, or white children, increased support by roughly 18, 24, 14, and 18 percent, respectively. The same pattern was observed when using a more expansive definition of support. In this context, the effect of mentioning children together with an image of a diverse group of children was statistically distinguishable from that of only mentioning children or doing so together with imagery of black or white children.

These effects reveal a disturbing pattern, however. Although characterizing the children benefitting from SNAP as black still leads to increases in support for the program, the effect is less pronounced as compared to when children are characterized as diverse or white. This finding

conforms with research revealing disparities across white and black children and youth in criminal sentencing and school disciplinary sanctions, which may in part be due to negative perceptions of blacks (Bridges & Steen, 1998; Monroe, 2005; Gregory, Skiba, & Noguera, 2010). To the extent that support for SNAP are driven by beliefs about the perceived deservingness of children, this effect is somewhat counteracted by racial antipathies. Black children do not elicit the same level of sympathy as their white counterparts.

I also find that highlighting children can boost support for SNAP among whites, Republicans, and participants from high- and middle-income households, key political constituencies that typically express lower support for welfare programs as compared to other social and demographic groups. Given the association of more rigid and restrictive eligibility criteria and program rules with political conservatism and racial attitudes, boosting support among these groups may help to mitigate disparities in the implementation of SNAP across jurisdictions, thereby helping to promote social equity and economic equality. Support is also bolstered among Democrats and blacks, suggesting that that there is room for improvement on this margin even among groups that typically favor welfare programs.

Qualitatively, the responsiveness of whites, Republicans, and those from middle-income households reveals the same pattern as before. Among whites, these responses are statistically distinguishable: estimated effects are smaller when children are characterized as black as compared to white or diverse. Recalling that the image of diverse children includes a white child and considering the latter two effects together suggests in-group bias—white adults are more likely to favor white children. The disparity therefore suggests out-group aversion—white adults are less likely to identify with black or minority children. Alternatively, however, depicting children as diverse may also signal that SNAP is a universal program in that it serves people

from all backgrounds rather than only minorities or a specific group (e.g., blacks), thereby making it more appealing to whites.

In several extensions, I find that highlighting children also shifted underlying motivations. Perceptions of beneficiaries as undeserving were softened when children were mentioned among those that did not support the program, even when those children were black. Characterizing children as white elicited more egalitarian sentiments among those who supported the program. Finally, highlighting the children benefitting from TANF also bolsters support for that program, though more so with regards to maintaining its current level of benefits than increasing its expenditures—the exception being when children are characterized as white. Whereas characterizing the children benefiting as black still raised support for SNAP overall, no such effect was found in the context of TANF. In fact, disaggregating the results by key political constituencies revealed that only when children were characterized as white or diverse, or not racialized at all, was support for the program raised. In line with previous work suggesting that public opinion on government programs is in part influenced by how those programs are designed, participants may have made a distinction between SNAP, which offers vouchers that can only be used for the purchase of food, and TANF, which offers assistance in the form of cash

In sum, highlighting the child beneficiaries of SNAP can boost public support for the program and can be effective in the context of other welfare programs as well, though how the child beneficiaries are characterized and in what way that information is conveyed matters. Of concern is that black children do not elicit the same feelings of deservingness as white children. Furthermore, considering that participants were told that SNAP expenditures on benefits amounted to $66 billion in 2016—among the highest ever—shifts in support for the programs

suggest that the public may be willing to spend far more than current levels on providing households in need with assistance.

Acknowledgements

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Figures & Tables

Figure 10. Experimental Conditions

Panel A. SNAP Statements

Control	The federal government provides food and nutrition assistance to people living in the United States through the Supplemental Nutrition Assistance Program (formerly the Food Stamp Program). In 2016, the government provided $66 billion in food assistance to program participants.
Treatment Conditions	
Text Only Text & Image Text & Black Image Text & White Image	The federal government provides food and nutrition assistance to people – **including 20 million children** – living in the United States through the Supplemental Nutrition Assistance Program (formerly the Food Stamp Program). In 2016, the government provided $66 billion in food assistance to program participants.

Panel B. TANF Statements

Control	The federal and state governments provide cash assistance to families living in the United States through the Temporary Assistance for Needy Families program. In 2016, state governments and the federal government provided a combined $7 billion in cash assistance to program participants.
Treatment Conditions	
Text Only Text & Image Text & Black Image Text & White Image	The federal and state governments provide cash assistance to families **with children** living in the United States through the Temporary Assistance for Needy Families program. In 2016, state governments and the federal government provided a combined $7 billion in cash assistance to program participants.

Note: The framing manipulations are bolded in the figure for clarity.

Figure 11. Treatment Condition Images

Panel A. SNAP Images

Panel B. TANF Image

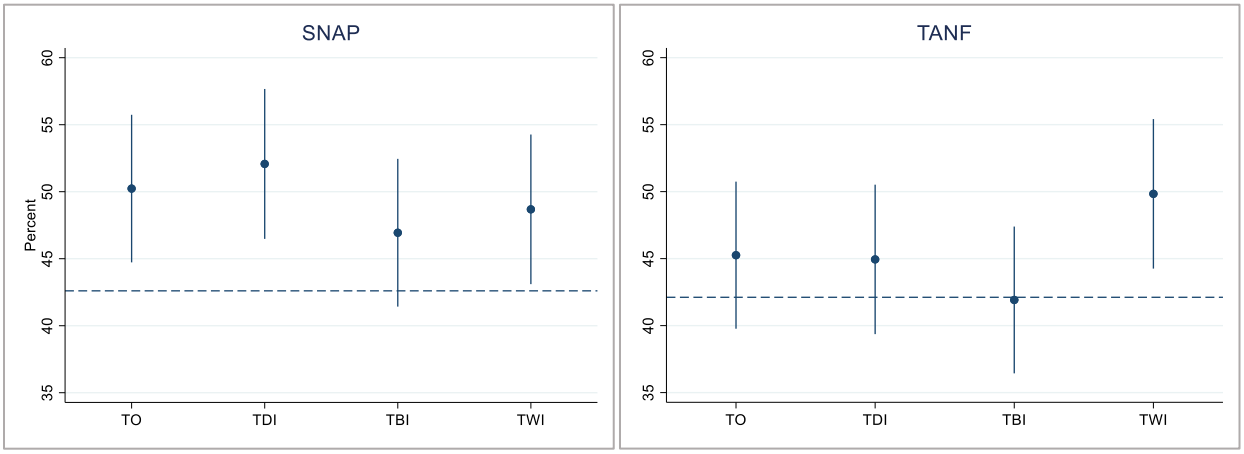
Note: From left to right in Panel A, image accompanying the SNAP statement in the *Text & Image*, *Text & Black Image*, and *Text & White Image* conditions. Panel B shows image accompanying the TANF statement in all treatment conditions involving images.

Figure 12. Follow-up questions

Spending preference	I believe that…
Increased	…those who benefit from spending on [activity] could use more assistance. …that the [entity] should be more involved in [activity]. …spending on [activity] is a good use of resources. … spending on [activity] is a desirable thing to do.
Remain Unchanged	…those who benefit from spending on [activity] are getting the assistance they need. …the [entity] is as involved as it should be in [activity]. …spending on [activity] is neither a good nor bad use of resources. …spending on [activity] is neither a desirable nor undesirable thing to do.
Decreased	…those who benefit from spending on [activity] could be doing more to help themselves. …the [entity] should not be involved in [activity]. …spending on [activity] is wasteful in how it spends on [activity]. …spending on [activity] is not a good use of resources. …spending on [activity] is not a desirable thing to do.

Note: Figure displays the list of reasons participants are asked to choose from to indicate their underlying motivation for their indicated spending preference.

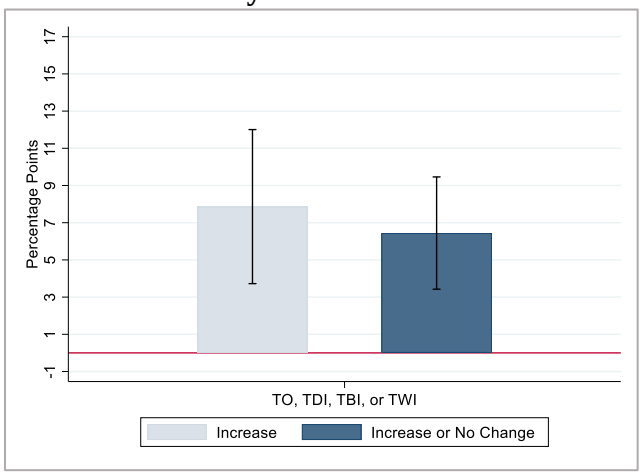
Figure 13. Percent Favoring Increased Spending across Control and Treatment Conditions



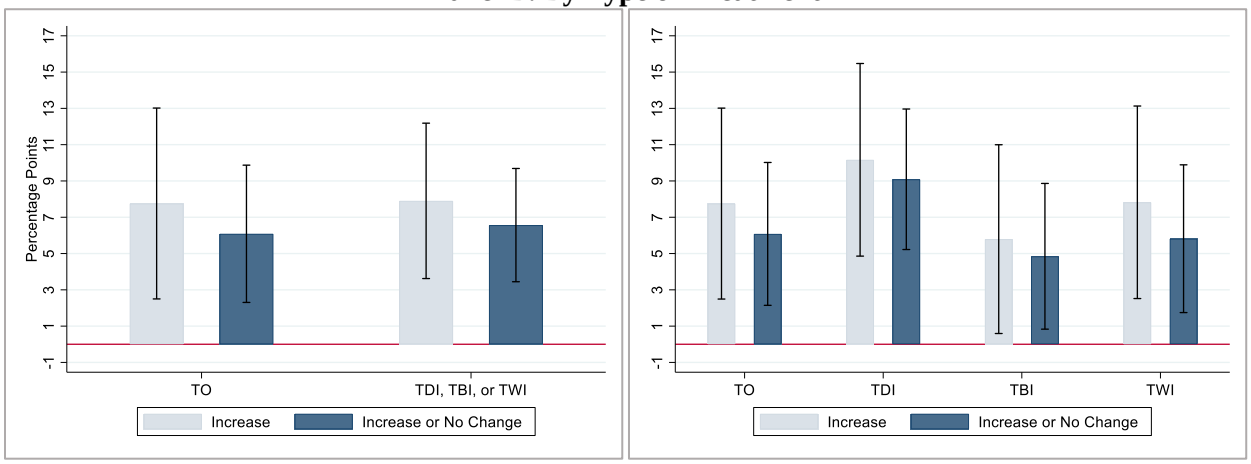
Note: Analytic sample includes all participants who passed the attention check. Figures depict the percent of participants in each treatment condition indicating a preference for increased spending, with the percent indicating a preference for increased spending in the control condition represented by the dashed horizontal line. The percent of participants favoring increased spending in each treatment condition was separately compared to that for the control condition using t-tests. Robust standard errors with confidence intervals at the 95% level.

Figure 14. Treatment Effects on Spending Preferences, SNAP

Panel A. Any Treatment Condition

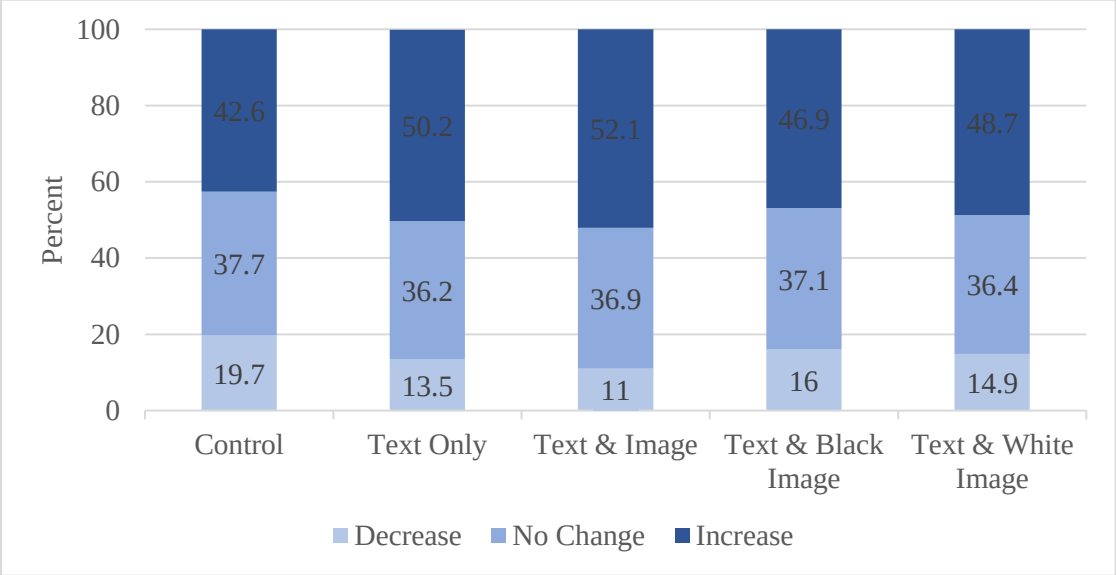


Panel B. By Type of Treatment



Note: Analytic sample includes all participants who passed the attention check. Figures depict coefficient estimates from three separate regressions with *Increase SNAP* or *Increase or No Change SNAP* as the dependent variables. Panel A presents the effect of being assigned to any of the treatment conditions. Panel B presents the effect of assignment to the *Text Only* condition or any of the image treatment conditions (left) and assignment to the *Text Only*, *Text & Diverse Image*, *Text & Black Image*, and *Text & White Image* conditions (right), relative to the control. Each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors with confidence intervals at 95% level.

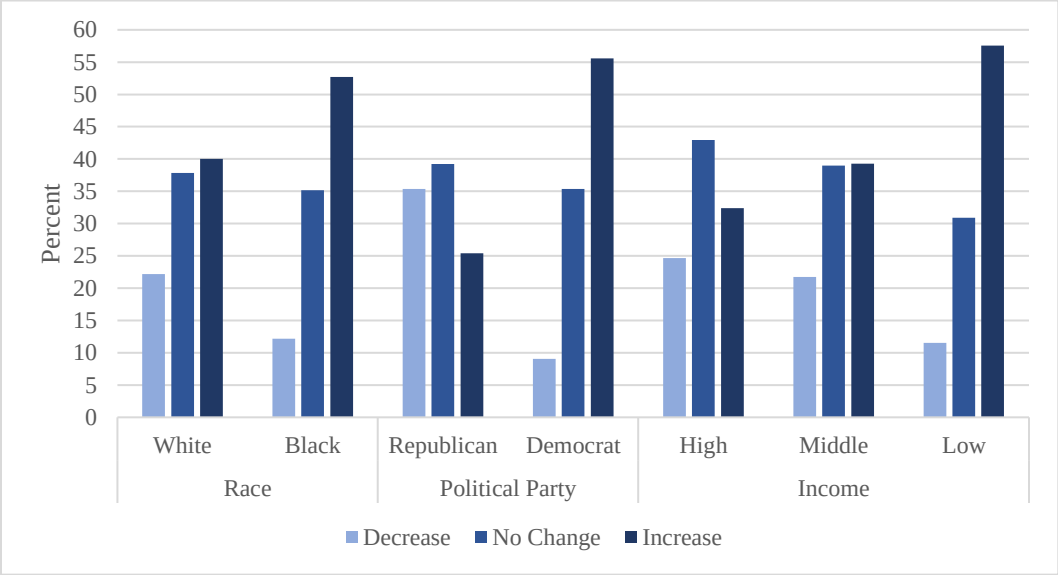
Figure 15. Distribution of Spending Preferences Across Conditions, SNAP



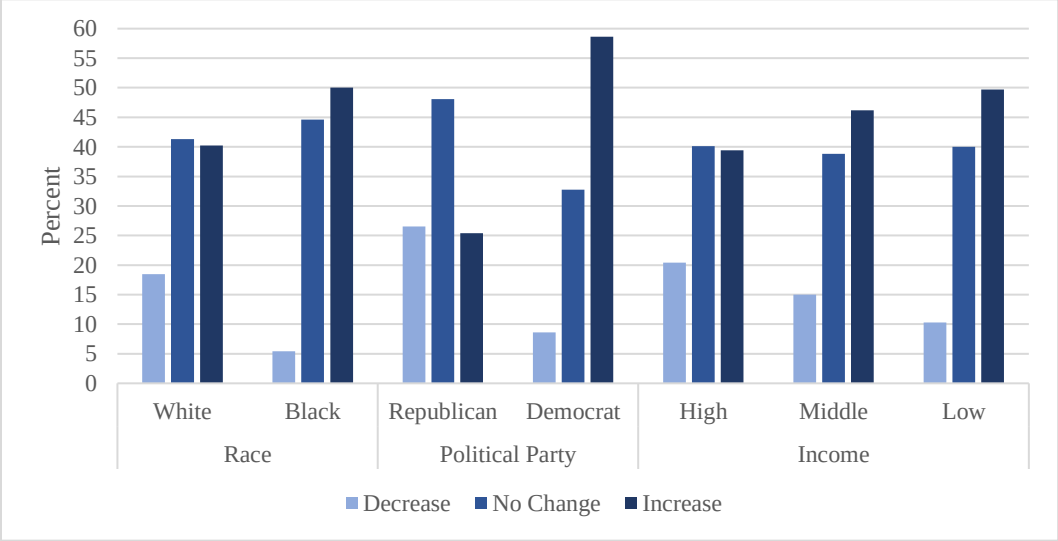
Note: Analytic sample includes all participants who passed the attention check. Figure shows the distribution of spending preferences on SNAP across the control and treatment conditions.

Figure 16. Spending Preferences in Control Condition by Race, Political Party, and Income

Panel A. Preferences for SNAP Spending

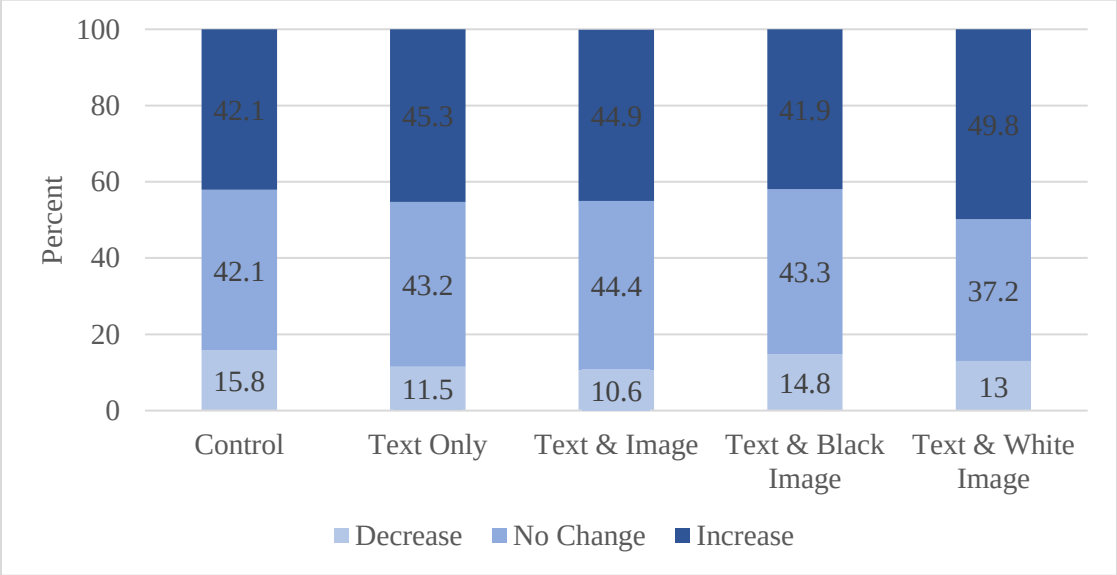


Panel B. Preferences for TANF Spending



Note: Analytic sample includes all participants who passed the attention check. Figure shows preferences for spending on SNAP and TANF among participants in the control group separately by race, political party, and income.

Figure 17. Distribution of Spending Preferences Across Conditions, TANF



Note: Analytic sample includes all participants who passed the attention check. Figure shows the distribution of spending preferences on TANF across the control and treatment conditions.

Table 13. Summary Statistics for Analytic Sample

Variables	Analytic Sample (1)	2017 ACS (2)	2018 GSS (3)	2020 Gallup (4)
Panel A: Characteristics used by Lucid to match the US Census				
Female	53	51	55	-
Hispanic	13	18	15	-
White	74	72	72	-
Black	12	13	16	-
Other	14	15	12	-
Bachelors or higher	34	32	33	-
Age				
Young (18-34)	32	23	27	-
Middle (35-64)	52	39	59	-
Elderly (65+)	16	16	23	-
Household Income				
Low ($0-24,999)	25	20.3	35	-
Middle ($25,000-74,999)	51	39.4	-	-
High ($75,000 or more)	24	40.3	-	-
Region				
Northeast	20	17	15	-
Midwest	19	21	22	-
South	37	38	41	-
West	24	24	22	-
Panel B: Other characteristics				
Born in USA	95	86	87	-
Married†	44	48	43	-
Parent	56	-	72	-
Employed††	55	60	59	-
Social assistance	26	-	-	-
Democrat‡	38	-	32	29
Republican‡	31	-	23	30
Independent/other‡	32	-	46	39
Liberal‡‡	32	-	29	26
Conservative‡‡	34	-	33	35
Moderate‡‡	35	-	38	35
Minutes	14.02			
Observations	3106			

Note: Analytic sample includes all participants who passed the attention check. All figures for socioeconomic and demographic characteristics in percentages and rounded to the nearest integer. Columns 1 presents summary statistics for the analytic sample. Sample collected through an online survey experiment conducted by Lucid between February 20, 2020 and March 7th, 2020. Columns 2, 3, and 4 present national estimates for select variables for comparison. Column 2 presents data from the 2017 American Community Survey (ACS) 1-year estimates. Column 3 presents data from the 2018 General Social Survey. Column 4 presents data from Gallup poll inquiring about political party affiliation and political ideology.

† ACS Data for those 15 and older.

†† ACS Data for those 16 and older.

‡ Gallup data from February 2020 polling.

‡‡ Gallup data from 2018 Gallup polling.

Table 14. Summary Statistics by Control and Treatment Conditions

Variable	Control (1)	Text Only (2)	Text & Image (3)	Text & Black Image (4)	Text & White Image (5)
Female	51	53	51	54	54
Hispanic	13	11	12	15	13
White	75	71	73	75	74
Black	12	15	12	8	11
Other	13	14	14	16	14
Bachelors or higher	35	33	34	35	35
Age					
Young (18-34)	32	34	33	29	33
Middle (35-64)	53	48	55	56	50
Elderly (65+)	15	18	12	15	17
Household Income					
Low ($0-24,999)	27	25	24	23	25
Middle ($25,000-74,999)	50	53	51	52	49
High ($75,000 or more)	23	22	25	25	26
Born in USA	97	94	95	92	95
Married	41	44	44	46	44
Parent	54	56	53	59	58
Employed	52	54	56	56	56
Social assistance	28	27	25	25	25
Region					
Northeast	20	20	17	22	23
Midwest	19	19	19	18	18
South	38	37	39	34	37
West	23	24	24	26	23
Democrat	38	40	38	36	36
Republican	29	28	30	33	33
Independent/other	33	32	32	31	31
Liberal	31	34	32	31	31
Conservative	34	32	33	35	36
Moderate	35	35	36	34	33
Minutes	19.19	12.43	11.94	14.36	12.19
Observations	615	643	603	637	608

Note: Analytic sample includes all participants who passed the attention check. All figures for socioeconomic and demographic characteristics in percentages and rounded to the nearest integer.

Table 15. Differential Responsiveness, SNAP, by Race, Political Party, and Income

	Race		Political Party		Household Income		
	Whites	Blacks	Republican	Democrat	High	Middle	Low
VARIABLES	(1)	(2)	(3)	(4)	(6)	(7)	(8)
Text Only	0.062**	0.167**	0.102**	0.083*	0.080	0.125***	-0.018
	(0.031)	(0.076)	(0.047)	(0.044)	(0.055)	(0.038)	(0.053)
Text & Diverse Image	0.137***	-0.004	0.086*	0.113**	0.093*	0.134***	0.049
	(0.031)	(0.081)	(0.047)	(0.045)	(0.054)	(0.038)	(0.055)
Text & Black Image	0.057*	0.106	0.033	0.102**	0.066	0.062*	0.049
	(0.030)	(0.088)	(0.044)	(0.045)	(0.052)	(0.038)	(0.053)
Text & White Image	0.113***	-0.039	0.073	0.092**	0.083	0.110***	0.012
	(0.031)	(0.086)	(0.046)	(0.046)	(0.054)	(0.039)	(0.054)
Avg. in Control	0.40	.53	0.25	0.56	0.32	0.39	0.58
Observations	3,106	3,106	3,106	3,106	3,106	3,106	3,106
R-squared	0.112	0.111	0.107	0.107	0.108	0.108	0.108

Note: Analytic sample includes all participants who passed the attention check. Each column represents a separate regression with the characteristic corresponding to the column title interacted with *Text Only*, *Text & Diverse Image*, *Text & Black Image*, and *Text & White Image*. Across columns 1-8, the reference groups are whites, blacks, Republicans, and Democrats, and those from high-, middle-, and low-income households, in the control condition, respectively. The dependent variable is *Increase SNAP*. Each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 16. Strength of Spending Preference, SNAP

	SNAP		
	Decreased	Remain Unchanged	Increased
VARIABLES	(1)	(2)	(3)
Text Only	-0.164	0.090	0.059
	(0.124)	(0.077)	(0.052)
Text & Diverse Image	0.079	-0.000	0.016
	(0.117)	(0.080)	(0.055)
Text & Black Image	-0.154	0.152**	0.042
	(0.120)	(0.076)	(0.053)
Text & White Image	-0.048	0.127*	0.091*
	(0.119)	(0.077)	(0.052)
Avg. Control Score	2.21	1.70	2.46
Observations	456	1,115	1,480
R-squared	0.062	0.042	0.066

Note: Analytic sample includes all participants who passed the attention check. Each column represents a separate regression on a subsample stratified by participant's spending preference corresponding to the column title. The dependent variable in each is *Strength SNAP*. Each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 17. Motivation for Spending Preference, SNAP

VARIABLES	Assistance (1)	Involvement (2)	Resources (3)	Desirability (4)	Wastefulness (5)
Panel A: Among Participants Preferring Decreased Spending					
Text Only	-0.231*	-0.116	-0.228	-0.092	-0.119
	(0.124)	(0.143)	(0.143)	(0.150)	(0.125)
Text & Diverse Image	-0.048	-0.001	-0.111	0.135	0.038
	(0.117)	(0.144)	(0.147)	(0.139)	(0.116)
Text & Black Image	-0.195*	0.084	0.053	0.032	-0.025
	(0.107)	(0.132)	(0.132)	(0.134)	(0.118)
Text & White Image	-0.159	0.117	0.028	0.075	0.023
	(0.106)	(0.131)	(0.137)	(0.138)	(0.116)
Avg. Control Score	2.41	1.48	1.64	1.55	2.34
Observations	459	451	449	443	459
R-squared	0.090	0.053	0.041	0.043	0.112
Panel B: Among Participants Preferring No Change in Spending					
Text Only	0.064	0.123	0.078	0.034	
	(0.071)	(0.077)	(0.080)	(0.078)	
Text & Diverse Image	0.095	0.014	-0.039	-0.087	
	(0.075)	(0.079)	(0.082)	(0.083)	
Text & Black Image	0.185**	0.168**	0.028	-0.069	
	(0.073)	(0.076)	(0.080)	(0.080)	
Text & White Image	0.056	0.049	0.018	0.003	
	(0.073)	(0.078)	(0.079)	(0.078)	
Avg. Control Score	1.79	1.73	1.51	1.51	
Observations	1,125	1,117	1,102	1,094	
R-squared	0.038	0.051	0.043	0.057	
Panel C: Among Participants Preferring Increased Spending					
Text Only	-0.029	0.021	0.001	0.012	
	(0.052)	(0.051)	(0.048)	(0.049)	
Text & Diverse Image	-0.054	-0.039	-0.048	-0.025	
	(0.055)	(0.054)	(0.051)	(0.049)	
Text & Black Image	-0.022	-0.038	-0.024	0.022	
	(0.053)	(0.055)	(0.049)	(0.050)	
Text & White Image	0.055	0.030	0.069	0.085*	
	(0.050)	(0.051)	(0.046)	(0.046)	
Avg. Control Score	2.53	2.55	2.61	2.59	
Observations	1,491	1,485	1,485	1,485	
R-squared	0.062	0.051	0.070	0.064	

Note: Analytic sample includes all participants who passed the attention check. Table presents the results form separate regressions with the dependent variables corresponding to the column titles. Samples in panels A, B and C include only observations indicating a preference for spending to be decreased, remain unchanged, or increased, respectively. Each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 18. Treatment Effects on Spending Preferences, TANF

	Increase	Increase or No Change
VARIABLES	(1)	(2)
Panel A: Any Treatment		
AnyTreat	0.041*	0.037**
	(0.022)	(0.015)
R-squared	0.073	0.048
Panel B: By Type of Treatment		
TO	0.033	0.041**
	(0.027)	(0.019)
AnyImageTreat	0.043*	0.036**
	(0.022)	(0.015)
R-squared	0.074	0.048
Panel C: By Type of Treatment		
Text Only	0.033	0.041**
	(0.027)	(0.019)
Text & Diverse Image	0.031	0.055***
	(0.028)	(0.019)
Text & Black Image	0.010	0.018
	(0.027)	(0.020)
Text & White Image	0.090***	0.035*
	(0.027)	(0.020)
R-squared	0.076	0.049
Avg. in Control	.421	.842
Observations	3,106	3,106

Note: Analytic sample includes all participants who passed the attention check. Table depicts coefficient estimates from six separate regressions with *Increase TANF* or *Increase or No Change TANF* as the dependent variables. Panel A presents the effect of being assigned to any of the treatment conditions (*AnyTreat*). Panel B presents the effect of assignment to the *Text Only* condition or any of the image treatment conditions (*AnyImageTreat*). Panel C presents the effect of assignment to the *Text Only*, *Text & Diverse Image*, *Text & Black Image*, or *Text & White Image* conditions. Each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors in parenthesis (*** p<0.01, ** p<0.05, * p<0.1).

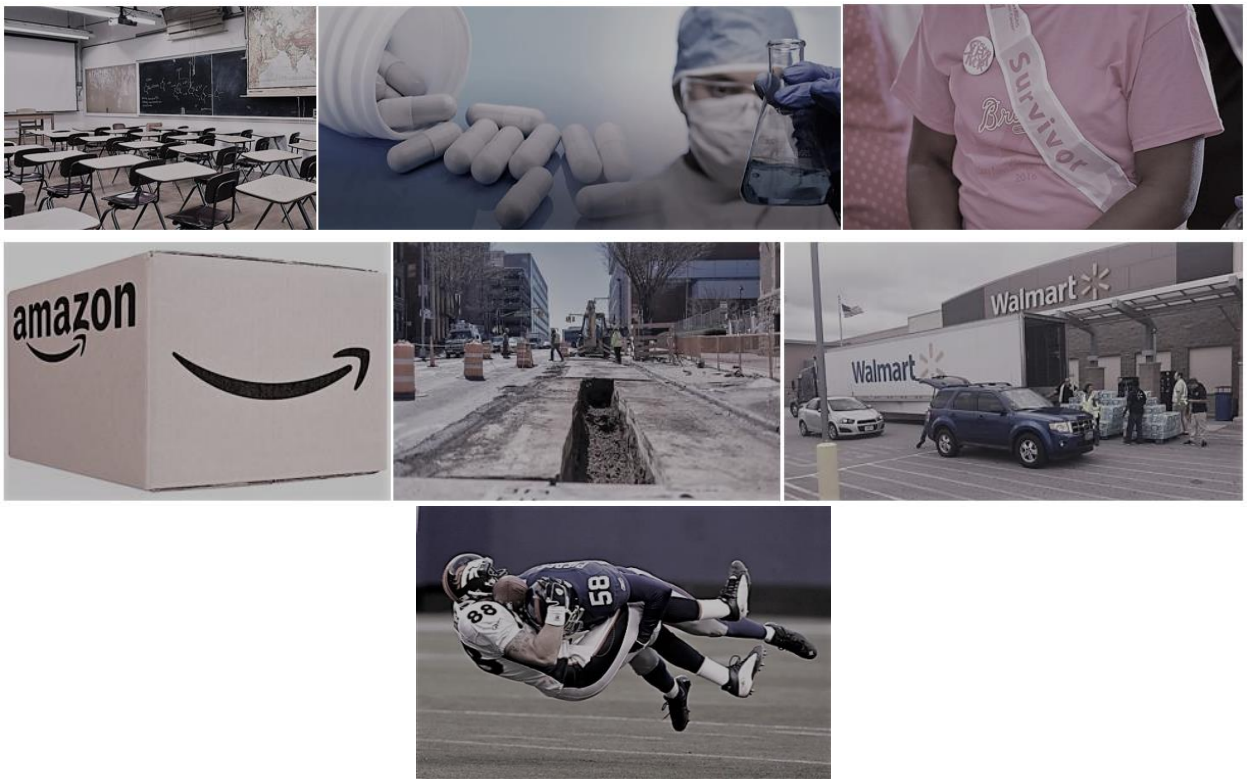
Table 19. Differential Responsiveness, TANF, by Race, Political Party, and Income

	Race		Political Party		Household Income		
	Whites	Blacks	Republican	Democrat	High	Middle	Low
VARIABLES	(1)	(2)	(3)	(4)	(6)	(7)	(8)
Text Only	0.032	0.042	0.037	0.052	-0.051	0.077**	0.017
	(0.031)	(0.078)	(0.046)	(0.044)	(0.054)	(0.037)	(0.053)
Text & Diverse Image	0.038	0.014	0.095**	-0.064	-0.001	0.055	0.012
	(0.032)	(0.083)	(0.048)	(0.046)	(0.055)	(0.039)	(0.057)
Text & Black Image	0.005	0.050	0.028	-0.040	-0.024	0.018	0.030
	(0.031)	(0.089)	(0.045)	(0.046)	(0.054)	(0.038)	(0.055)
Text & White Image	0.096***	0.048	0.084*	0.072	0.064	0.113***	0.068
	(0.031)	(0.084)	(0.047)	(0.046)	(0.054)	(0.039)	(0.055)
Avg. in Control	0.40	0.50	0.25	0.59	0.39	0.46	0.50
R-squared	0.077	0.077	0.081	0.081	0.078	0.078	0.078
Observations	3,106	3,106	3,106	3,106	3,106	3,106	3,106

Note: Analytic sample includes all participants who passed the attention check. Each column represents a separate regression with the characteristic corresponding to the column title interacted with *Text Only*, *Text & Diverse Image*, *Text & Black Image*, and *Text & White Image*. Across columns 1-8, the reference groups are whites, blacks, Republicans, and Democrats, and those from high-, middle-, and low-income households, in the control condition, respectively. The dependent variable is *Increase TANF*. Each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Appendix

Figure A3. Statement Images



Note: Images associated with the statements (excluding statements pertaining to SNAP and TANF). Images in top row pertain to the state of California, Johnson & Johnson, Susan G. Komen, Amazon, New York City, Walmart, and the NFL.

Figure A4. Survey Statements

All Conditions:

1) The state government of California administers and finances numerous programs pertaining to the education of its residents. In 2017, the state's legislature approved a budget that allocated $75 billion toward spending on K-12 education and community colleges.

2) Johnson & Johnson is a for-profit medical device, pharmaceutical, and consumer packaged goods manufacturing company based in the United States. In 2016, the company spent $9 billion on research and development, much of it directed toward medications for rare conditions.

Control Condition:

3) The federal government provides food and nutrition assistance to people living in the United States through the Supplemental Nutrition Assistance Program (formerly the Food Stamp Program). In 2016, the government provided $66 billion in food assistance to program participants.

Treatment Conditions:

3) The federal government provides food and nutrition assistance to people - including 20 million children - living in the United States through the Supplemental Nutrition Assistance Program (formerly the Food Stamp Program). In 2016, the government provided $66 billion in food assistance to program participants.

All Conditions:

4) Susan G. Komen is a not-for-profit organization that promotes breast cancer awareness, research, health services, and social support programs in the United States. In 2016, $21 million of the organization's expenditures were directed toward fundraising efforts.

5) Amazon is a for-profit online retailer and cloud-computing company based in the United States. In 2015, the company donated $13 million to various charities via the AmazonSmiles foundation.

6) The New York City municipal government provides numerous services involving transportation, sewage, and power for its residents. In 2015, the city approved a budget that allocated about $28 billion for infrastructure spending.

7) Walmart is a for-profit retailer based in the United States. In 2016, the company donated $300 million to various causes via the Walmart Foundation.

8) The National Football League (NFL) is a professional sports league representing 32 for-profit teams based in the United States. In 2016, the league pledged to spend $100 million over five years on research and projects to reduce the risk of head trauma among its athletes.

Control Condition:

9) The federal and state governments provide cash assistance to families living in the United States through the Temporary Assistance for Needy Families program. In 2016, state governments and the federal government provided a combined $7 billion in cash assistance to program participants.

Treatment Conditions:

9) The federal and state governments provide cash assistance to families with children living in the United States through the Temporary Assistance for Needy Families program. In 2016, state governments and the federal government provided a combined $7 billion in cash assistance to program participants.

Attention Check:

10) Tesla is a for-profit automotive and energy company based in the United States. In 2018, the company increased its research and development budget by $90 million dollars.

Note: Figure presents the statements in the order they are presented to participants across all conditions.

Table A25. Treatment Effects on Spending Preferences, Alternative Models

	Logit Models				Generalized Ordered Logit Models			
	SNAP		TANF		SNAP		TANF	
VARIABLES	Increase (1)	Increase or No Change (2)	Increase (3)	Increase or No Change (4)	Increase (5)	Increase or No Change (6)	Increase (7)	Increase or No Change (8)
Text Only	0.351***	0.480***	0.145	0.358**	0.350***	0.451***	0.137	0.383**
	(0.121)	(0.163)	(0.117)	(0.172)	(0.120)	(0.161)	(0.116)	(0.172)
Text & Diverse Image	0.457***	0.775***	0.137	0.488***	0.453***	0.739***	0.129	0.453**
	(0.122)	(0.176)	(0.121)	(0.178)	(0.122)	(0.176)	(0.121)	(0.177)
Text & Black Image	0.264**	0.377**	0.046	0.144	0.254**	0.383**	0.025	0.128
	(0.120)	(0.156)	(0.119)	(0.162)	(0.119)	(0.155)	(0.119)	(0.162)
Text & White Image	0.355***	0.446***	0.394***	0.292*	0.352***	0.407**	0.373***	0.271
	(0.122)	(0.163)	(0.120)	(0.168)	(0.121)	(0.161)	(0.119)	(0.167)
Observations	3,106	3,106	3,106	3,106	3,106	3,106	3,106	3,106

Note: Analytic sample includes all participants who passed the attention check. Columns 1 and 2, and 3 and 4, present the results from separate regressions where the dependent variables are *Increase SNAP* and *Increase or No Change SNAP*, and *Increase TANF* and *Increase or No Change TANF*, respectively. Columns 5 and 6 present the output at the cutoffs from a single generalized ordered logit regression with *Spending SNAP* as the dependent variable. Columns 7 and 8 present the same with *Spending TANF* as the dependent variable. Each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table A26. Strength of Spending Preference, TANF

	TANF		
	Decreased	Remain Unchanged	Increased
VARIABLES	(1)	(2)	(3)
Text Only	-0.195*	0.097	-0.021
	(0.117)	(0.073)	(0.055)
Text & Diverse Image	-0.000	0.000	0.045
	(0.132)	(0.075)	(0.054)
Text & Black Image	-0.096	0.091	0.027
	(0.117)	(0.075)	(0.054)
Text & White Image	0.003	0.012	0.030
	(0.118)	(0.077)	(0.052)
Avg. Control Score	2.25	1.71	2.48
Observations	450	1,265	1,378
R-squared	0.061	0.044	0.041

Note: Analytic sample includes all participants who passed the attention check. Each column represents a separate regression on a subsample stratified by participant's spending preference corresponding to the column title. The dependent variable in each is *Strength TANF*. Each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table A27. Motivation for Spending Preference, TANF

VARIABLES	Assistance (1)	Involvement (2)	Resources (3)	Desirability (4)	Wastefulness (5)
Panel A: Among Participants Preferring Decreased Spending					
Text Only	-0.029	-0.252*	-0.213	0.111	-0.155
	(0.122)	(0.142)	(0.155)	(0.151)	(0.128)
Text & Diverse Image	-0.106	0.071	-0.036	0.132	-0.061
	(0.136)	(0.150)	(0.145)	(0.156)	(0.141)
Text & Black Image	-0.171	-0.026	-0.234*	-0.028	-0.012
	(0.123)	(0.141)	(0.134)	(0.141)	(0.119)
Text & White Image	0.048	-0.011	-0.097	-0.040	-0.016
	(0.113)	(0.143)	(0.140)	(0.153)	(0.124)
Avg. Control Score	2.34	1.91	2.08	1.89	2.30
Observations	404	398	403	399	395
R-squared	0.136	0.072	0.126	0.087	0.072
Panel B: Among Participants Preferring No Change in Spending					
Text Only	0.030	0.101	0.065	0.086	
	(0.067)	(0.069)	(0.073)	(0.071)	
Text & Diverse Image	-0.028	0.052	-0.024	-0.033	
	(0.069)	(0.073)	(0.076)	(0.075)	
Text & Black Image	0.109	0.111	0.013	-0.036	
	(0.068)	(0.072)	(0.076)	(0.074)	
Text & White Image	-0.055	0.003	0.104	0.092	
	(0.073)	(0.075)	(0.078)	(0.076)	
Avg. Control Score	1.77	1.76	1.55	1.57	
Observations	1,277	1,276	1,263	1,252	
R-squared	0.044	0.046	0.029	0.032	
Panel C: Among Participants Preferring Increased Spending					
Text Only	-0.049	-0.056	-0.017	-0.045	
	(0.053)	(0.052)	(0.052)	(0.054)	
Text & Diverse Image	-0.011	-0.011	0.018	-0.025	
	(0.051)	(0.052)	(0.053)	(0.056)	
Text & Black Image	-0.031	-0.032	-0.033	-0.043	
	(0.051)	(0.053)	(0.053)	(0.055)	
Text & White Image	-0.074	-0.048	-0.032	-0.063	
	(0.052)	(0.050)	(0.053)	(0.054)	
Avg. Control Condition	2.54	2.55	2.53	2.55	
Observations	1,381	1,376	1,378	1,371	
R-squared	0.053	0.045	0.047	0.045	

Note: Analytic sample includes all participants who passed the attention check. Table presents the results form separate regressions with the dependent variables corresponding to the column titles. Samples in panels A, B and C include only observations preferring for spending to be decreased, remain unchanged, or increased, respectively. Each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table A28. Summary Statistics by Attention Check Response

	Attention Check	
Variable	Failed	Passed
Female	40	52
Hispanic	20	11
White	56	72
Black	21	12
Other	19	14
Bachelors or higher	32	34
Age		
18-34	51	32
35-64	45	52
65+	4	16
Household Income		
$0-24,999	43	33
$25,000-74,999	36	46
$75,000 or more	21	21
Region		
Northeast	20	20
Midwest	17	19
South	38	37
West	25	24
Democrat	43	37
Republican	37	38
Independent/other	20	25
Minutes	12.48	14.10
Observations	812	3066

Note: All figures for socioeconomic and demographic characteristics in percentages and rounded to the nearest integer.

Table A29. Preferences for Increased Spending, Alternative Samples

	SNAP		TANF	
VARIABLES	Increase (1)	Increase or No Change (2)	Increase (3)	Increase or No Change (4)
Panel A: All Observations				
Text Only	0.073***	0.048***	0.031	0.023
	(0.025)	(0.018)	(0.024)	(0.017)
Text & Diverse Image	0.089***	0.069***	0.023	0.037**
	(0.025)	(0.017)	(0.025)	(0.017)
Text & Black Image	0.064***	0.030*	0.016	-0.001
	(0.024)	(0.018)	(0.024)	(0.017)
Text & White Image	0.063**	0.041**	0.058**	0.024
	(0.025)	(0.018)	(0.025)	(0.017)
Observations	3,878	3,878	3,878	3,878
R-squared	0.075	0.072	0.080	0.040
Panel B: 5 ≤ Minutes ≤ 25				
Text Only	0.079***	0.067***	0.033	0.041*
	(0.030)	(0.024)	(0.031)	(0.022)
Text & Diverse Image	0.067**	0.101***	0.010	0.062***
	(0.030)	(0.023)	(0.031)	(0.022)
Text & Black Image	0.034	0.059**	-0.010	0.025
	(0.030)	(0.024)	(0.030)	(0.023)
Text & White Image	0.065**	0.071***	0.070**	0.036
	(0.030)	(0.024)	(0.031)	(0.023)
Observations	2,460	2,460	2,460	2,460
R-squared	0.125	0.109	0.077	0.057

Note: Table presents the results form separate regressions with the dependent variables corresponding to the column titles. In Panel A, the sample includes all observations, regardless of whether the attention check was passed, and each regression control for age, gender, race and ethnicity, educational attainment, and income. In Panel B, the sample includes only those that spent at least 5 minutes and no more than 25 minutes on the survey, and each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table A30. Manipulation Check

VARIABLES	California (1)	J&J (2)	Komen (3)	Amazon (4)	NYC (5)	Walmart (6)	NFL (7)
Panel A: Relative to *Control* condition							
Text Only	0.028	-0.010	0.034	-0.002	0.004	-0.032	0.011
	(0.027)	(0.028)	(0.027)	(0.027)	(0.027)	(0.028)	(0.028)
Text & Diverse Image	0.017	0.017	0.057**	0.025	-0.006	-0.005	0.014
	(0.028)	(0.029)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Text & Black Image	0.024	0.011	0.031	0.067**	0.040	0.014	0.030
	(0.027)	(0.028)	(0.027)	(0.027)	(0.028)	(0.028)	(0.028)
Text & White Image	0.016	-0.036	0.052*	0.042	0.003	0.023	0.024
	(0.028)	(0.029)	(0.028)	(0.027)	(0.028)	(0.028)	(0.028)
Observations	3,106	3,106	3,106	3,106	3,106	3,106	3,106
R-squared	0.063	0.014	0.026	0.059	0.036	0.041	0.042
Panel B: Relative to *Text & Image* condition							
Text & Black Image	0.006	-0.007	-0.025	0.039	0.045	0.019	0.014
	(0.028)	(0.028)	(0.028)	(0.027)	(0.028)	(0.028)	(0.028)
Text & White Image	-0.000	-0.054*	-0.006	0.015	0.007	0.027	0.010
	(0.028)	(0.029)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Observations	1,848	1,848	1,848	1,848	1,848	1,848	1,848
R-squared	0.072	0.021	0.030	0.067	0.035	0.048	0.045

Note: Analytic sample includes all participants who passed the attention check. Table presents the results form separate regressions with the dependent variables corresponding to the column titles. In Panel B, the analytic sample includes observations in either the *Text & Image*, *Text & Black Image*, or *Text & White Image* conditions. Each regression includes controls for age, gender, race and ethnicity, educational attainment, income, political party, region, nativity, marital status, parental status, employment status, and receipt of social assistance. Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

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REFEREED JOURNAL ARTICLES

Saied Toossi and Pengju Zhang. 2019. "Video Gambling Adoption and Tax Revenues: Evidence from Illinois." *Public Budgeting & Finance*, 39(1): 67-88.

Saied Toossi. 2017. "Incentivizing Healthy Eating in Children: An Investigation of the Ripple and Temporal Effects of a Reward-based Intervention." *Appetite*, 117(1): 58-66.

WORKING PAPERS

Saied Toossi and Amy Ellen Schwartz. "What's for Lunch? The Relationship between School Menus and Student Lunch Participation."

Saied Toossi. "Think of the Children? The Effect of Children on Public Support for the Supplemental Nutrition Assistance Program"

RESEARCH GRANTS

Spencer D. Parratt Summer Research Award (2018, 2019)

Center for Policy Research Graduate Student Research Grant (2018)

Roscoe Martin Research Award (2018, 2019)

Summer Research Grant, The Maxwell School, Syracuse University (2016)

CONFERENCE PRESENTATIONS

American Society of Health Economists (2019)

Association for Public Policy Analysis & Management (2016, 2017, 2018, 2019)

Association for Budgeting & Financial Management (2017)

Association of Education Finance & Policy (2017)

Association of Municipal Management Assistants (2015)

TEACHING EXPERIENCE

Instructor, The Maxwell School, Syracuse University

Introduction to Stata, MPA (2017, 2018)

Teaching Assistant, The Maxwell School, Syracuse University

Public Administration and Democracy, MPA (2017, 2019)

Quantitative Methods for the Social Sciences, Undergraduate (2019)

Introduction to Statistics, MPA (2018)

Quantitative Analysis: Program Evaluation, MPA (2018)

Public Budgeting, MPA (2017)

Economics for Public Decisions, MPA (2016)

Teaching Assistant, Northern Illinois University

International Economics, Undergraduate (2014)

Principles of Microeconomics, Undergraduate (2013, 2014)

ACADEMIC AWARDS AND FELLOWSHIPS

Research Excellence Doctoral Funding Fellowship, Syracuse University (2019)

Graduate Student Organization Travel Grant, Syracuse University (2016, 2017, 2018, 2019)

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