Food Access and Student Achievement: Evidence from Federal Summer Meal Programs

M. Cade Lawson *

Georgia State University

Abstract

Many of the 26 million U.S. children who rely on free or reduced-price meals at school lose reliable access to food when the summer begins. However, little evidence has emerged on how poor geographic food access affects student achievement in school. I study this question by leveraging within-child changes in distance to federally-funded summer meal sites and a student-level panel following 250,000 students per year in the metro-Atlanta area. I find that an additional mile of distance to the nearest meal site reduces student achievement by 1-3% of a standard deviation in reading and math, with the strongest effects for elementary-aged and low-income students. Approximately 80% of free/reduced-price meal eligible students live within 1 mile of a meal site, but reducing restrictions on where sites can locate does not affect access for the remaining 20%. Results point to summer meal programs as a cost-effective part of the U.S. social safety net.

*Corresponding author. Email: mlawson18@gsu.edu

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1 Introduction

Approximately 26 million school-aged children, just over 53% of all students enrolled in U.S. public schools, qualify for free or reduced-price school breakfast and lunch (National Center for Education Statistics, 2023). When school ends for the summer, many of these children abruptly lose access to reliable meals and face higher risk of food insecurity. This leaves children exposed to a host of negative health outcomes associated with food insecurity, including depression (Cain et al., 2022), chronic illnesses like asthma (Ke and Ford-Jones, 2015; Mangini et al., 2015), and a higher likelihood of foregoing medical care (Thomas et al., 2019).

Negative effects even follow children to the classroom: food insecurity is associated with lower in-school achievement and reduced social-emotional outcomes in students (Johnson and Markowitz, 2018; Grineski et al., 2018; Shankar et al., 2017; Gallegos et al., 2021). Associations are especially strong between food insecurity and skill formation, including academically in math and vocabulary (de Oliveira et al., 2020) and non-academically in self-control and relationship building (Howard, 2011). Despite the consequences, summer increases in child hunger have been allowed to persist for years (Nord and Romig, 2006). These seasonal patterns, coupled with evidence that food insecurity affects performance in school, imply that poor summer nutrition may play a role in widening achievement gaps between students. This includes through summer learning loss ("summer slide"), when student test scores regress over the summer (Kuhfeld, 2019).

To bridge the nutritional gap between school years, the U.S. Department of Agriculture (USDA) provides summer meals for children through two flagship initiatives: the Seamless Summer Option (SSO) and Summer Food Service Program (SFSP). Sites operated through these programs supply free meals to any child in attendance, most often breakfast and lunch. Meal sites are are overseen by organizations such as school districts and private non-profits, and operate in community points of interest like schools, community centers, and places of worship. During the summer of 2024, the Summer Food Service Program alone served over 160 million meals at a cost of nearly $700 million (USDA Food & Nutrition Service, 2025b).[1] Prior work finds that proximity to these sites is associated with decreased risk of very low food security for households with children (Miller, 2016) and improved dietary intake for children (Chen et al., 2025). Research on summer hunger finds that states which serve more summer meals experience smaller seasonal increases in food insecurity among children (Nord and Romig, 2006). However, limited evidence exists on how summer meal access affects students once they return to school.

I study the effect of summer meal access on student achievement using within-child changes in distance to the nearest meal site over time. I use the geocoded addresses of nearly 250,000 students per year in the metro-Atlanta area and a census of federally-funded meal sites in Georgia

[1] Cost estimates are only available for the Summer Food Service Program. Because the Seamless Summer Option is an extension of the National School Lunch Program, separate funding amounts for the program are not reported.

to calculate every student's annual distance to a meal site over an 8-year period. Using a fixed effects instrumental variables (FE-IV) strategy, find that the first mile of distance to a meal site reduces math and reading performance by 1-3% of a standard deviation, with the the strongest effects for elementary and free/reduced-price meal (FRPM) eligible students. In addition, I provide student-level evidence from administrative data on whether federal meal programs are accessible to low-income students in an urban setting. I find that over 80% of FRPM eligible students live within 1 mile of a meal site, compared to just 45% of non-FRPM eligible students. My results also suggest that meal sites have very short lifespans, posing additional challenges for families who rely on them for summer food access.

This paper makes three key contributions. First, I provide what I believe to be the first student-level, causal evidence on how access to summer meal sites affects student achievement in school. The dearth of evidence on how these programs affect children is explicit called out in prior studies, such as Turner and Calvert (2019). Similarly, advocacy groups have highlighted the need to understand whether access to summer programs such as meal sites may drive existing achievement gaps (Food Research & Action Center, 2025).

Second, I provide evidence from administrative data on the reach of federally-funded summer meal sites. This provides a complement to existing estimates of program accessibility, which rely on survey data (Vericker et al., 2021). Because I observe each student's address and the universe of summer meal sites in Georgia at an annual frequency, I can estimate measures of site access that are fully contemporaneous and study changes over time. In contrast, prior estimates rely on a combination of decennial census counts, five-year American Community Survey (ACS) poverty rates, and annual site locations, which are inherently measured at different times. This results in estimates that could be confounded by population change. The tradeoff is that my estimates have lower external validity, particularly for rural areas.

Third, this paper contributes to the ongoing policy debate around the value of federal spending on food programs. I provide additional empirical evidence that federal nutrition programs can improve the well-being of children. This finding is timely, considering recent cuts to the Supplemental Nutrition Assistance Program (SNAP) that could result over 800,000 children losing both SNAP and access to free school meals (Gutierrez, 2025). Moreover, 13 states recently declined to participate in the new federal Summer EBT ("SUN Bucks") program, which targets childhood food insecurity over the summer by providing additional SNAP benefits to families with children (USDA Food & Nutrition Service, 2025d). These trends affirm the need for further research on the payoffs from these federal programs, of which improved academic outcomes for children is one.

The rest of the paper is structured as follows. In Section 2, I provide a brief review of the introduce the details of the USDA's summer meal programs and the literature linking food access to student wellbeing. In Section 3, I discuss the datasets used in my study. In Section 4, I introduce

my instrumental variables estimation strategy. In Section 5, I discuss my results. In Section 6, I conclude and discuss the policy implications of this work at the federal, state, and school district levels.

2 Background

In this section I review the literature on how food access and federal meal programs influence student outcomes, then discuss the institutional context of the USDA's summer food programs. In the latter section I focus on how and where federal summer meal sites operate, who they are designed to serve, and how effective they are at reaching students in their target demographic.

2.1 The Effects of Food Access and Meal Programs

I. Geographic Food Access and Student Wellbeing

While the effects of food insecurity on students and children are well-understood, there are fewer studies focused on the relationship between food access – the geographic subcomponent of food insecurity – and student wellbeing. Prior work studying school-aged children in Arkansas has established that nearby grocery store access may reduce body mass index (BMI) in children (Thomsen et al., 2016; Zeng et al., 2019a) while nearby convenience store access may actually increase BMI through the provision of less nutritious food (Zeng et al., 2019b). Health-related outcomes such as BMI provide plausible mechanisms through which food access could affect student achievement in school. The relationship between food access and student performance in school has not yet been studied using student-level data. School district-level evidence from New York finds a strong negative correlation between the share of population living in a food desert and student performance but highlights the need for additional analyses at the student-level (Frndak, 2014).

II. Federal Meal Programs and Student Outcomes

There is a long-standing literature on how federal food programs affect the outcomes of students and children, including first-order effects on food insecurity and second-order effects on health and educational measures (Ralston et al., 2017). Effects on childhood food security are seen across the suite of federal food programs. Access to the summer meal programs studied here have measurable negative associations with the risk of food insecurity among children (Miller, 2016). Similarly, the National School Lunch Program has been found to have a mitigating effect on food insecurity for participating children (Kabbani and Kmeid, 2005; Huang and Barnidge, 2016). The School Breakfast Program is shown to reduce both marginal (Bartfeld and Ahn, 2011) and overall (Fletcher and Frisvold, 2017) food insecurity among children.

Federal food programs also have effects on children that extend beyond food security. Evidence suggests that the availability of breakfast through the School Breakfast Program increases student achievement (Frisvold, 2015), and that effects on students are comparable whether the free breakfasts are universal or eligibility-based (Ribar and Haldeman, 2013). Similarly, universal free school lunches have been shown to improve student performance in both math and reading (Gordanier et al., 2020) and boost the overall health of students (Gundersen et al., 2012). Such programs have also been found to moderately reduce both obesity (Localio et al., 2024) and the likelihood of suspension (Gordon and Ruffini, 2021) for students. Effects of free lunches on educational attainment have been shown to persist into adulthood (Hinrichs, 2010).

2.2 Federal Summer Meal Programs for Children

I. Institutional Details

To bridge the nutritional gap between school years, the USDA funds two flagship summer meal programs that provide prepared meals to children at community-based sites: the Summer Food Service Program (SFSP) and the Seamless Summer Option (SSO). Both programs are federally-funded and administered by states, with local sponsors operating free meal sites at locations such as schools, camps, churches, parks, and community centers (Billings and Aussenberg, 2024). SSO sites must be sponsored by school food authorities (SFAs; usually schools or school districts) participating in the National School Lunch Program (NSLP), while SFSP sponsors can be SFAs, universities, camps, faith-based organizations, and other public or private non-profit organizations.

From the perspective of a child or family, the two programs are essentially identical. Any child under 18 can receive a free meal from any open[2] site operated under either program, and most sites report offering other programming for children such as organized games or arts and crafts in addition to the free meals (Gordon et al., 2003). Their differences are largely administrative. Aside from differing rules on who can sponsor sites, the other primary difference is that the SFSP and SSO have slightly difference reimbursement rates for meals served.[3] I treat the two as perfect substitutes in my analysis.

A key aspect of the SFSP and SSO is that sites must be operated in either school attendance zones or census-designated area where 50% or more of children qualify for free or reduced price lunch[4]

[2]Some sites operate as either "closed enrolled", where service is restricted to enrolled children, or under a "camp" status reserved for either residential or nonresidential summer camps. Approximately 91% of SSO sites in metro-Atlanta were designated as "open" in 2023.

[3]The SFSP has its own reimbursement scheme, while meals served at SSO sites are reimbursed at the standard rate for meals served under the NSLP. For a full comparison of the two programs, see (USDA Food & Nutrition Service, 2025a).

[4]Income thresholds are set at 130 percent of the federal poverty level for free meals and 185 percent of the federal poverty level for reduced-price meals (USDA Food & Nutrition Service, 2024a). Children in households that

(USDA Food & Nutrition Service, 2024b). This directs federal funds towards children from low-income households, but does not necessarily guarantee equitable access to sites for all low-income students. Demand for these programs is strong; the SFSP alone served over 2.2 million children during the summer of 2023 (Jones and Toossi, 2024).

II. Changes During the COVID-19 Pandemic

The USDA made a series of changes to its summer meal programs in response to the COVID-19 pandemic.[5] First, the rules on where sites could locate were temporarily waived. For the summers of 2020 through 2022, meal sites could operate in any community without the need to meet the usual requirement that 50% of local children be eligible for free/reduced-price school meals. Second, USDA permitted "non-congregate" meal service during these years. This allowed students or parents to take meals off-site and for sites with appropriate resources to arrange home delivery of meals.[6]

III. Prior Research on Site Accessibility

Whether federally-funded meal sites are geographically accessible to the children they are designed to serve remains an important policy issue. Prior research indicates that households with children experience elevated rates of food insecurity during the summer months (Nord and Romig, 2006), implying an important role for programs like the SSO and SFSP in mitigating child hunger – one which cannot be filled if sites are too far from children. In addition, there is direct evidence that children who rely on the NSLP for meals during the school year experience higher rates of food insecurity during the summer (Huang et al., 2015). Despite such facts affirming the need for summer meal sites as part of the greater food access landscape, recent estimates suggest that only around 11% of students eligible for free school meals receive lunch through the USDA's summer nutrition programs (Food Research & Action Center, 2023).

Studies using American Community Survey and Census counts of children in low income households estimate that approximately 80% of children live within 1 mile of a site in urban areas or within 10 miles in rural areas (Vericker et al., 2021). While such estimates provide reliable aggregate approximations of site access, they must also tolerate years-long lags between survey response and meal site locations and lack access to exact child address data. Other work has doc-

receive Supplemental Nutrition Assistance Program (SNAP) benefits are categorically eligible.

[5] I discuss the changes most relevant to this study here. See Washburn et al. (2025) for a summary of all waivers issued by USDA during this period.

[6] The districts studied here offered meal delivery via school bus routes through federal programs during summer of 2020 only. In Georgia, any SFSP site seeking to deliver meals directly to children was required to submit a USDA Waiver Request Addendum and 1) obtain written consent from the household, 2) confirm the number of children being served, 3) confirm the household's contact information, and 4) ensure that meals were only delivered to students living in area eligible locations (if an open site) or to students who were FRPM eligible themselves (if a closed site). For additional information, see: https://www.decal.ga.gov/Nutrition/Covid19Resources.aspx.

umented that 63.9% of public urban high schools were located near a summer meal site, a figure which is higher when examining only large, high-poverty schools (Turner et al., 2019).

An important question around federal meal programs is whether site restrictions based on local shares of free/reduced-price meal-eligible (FRPM) students prevent needy children from being reached. An opportunity to test this concern came in 2020, when the USDA issued "area eligibility waivers" allowing SSO and SFSP sites to operate in any location (USDA Food & Nutrition Service, 2020). Research on the effect of these waivers in Texas finds that removing site location restrictions resulted in a greater share of state census tracts hosting a meal site and a higher number of sites operating overall (Toossi, 2023). In addition, site sponsors in states which did not apply for a statewide federal waiver reported increased difficulty operating free meal sites (Harper et al., 2022).

3 Data

My analysis uses a combination of restricted-use, student-level data from three large school districts in the metro-Atlanta area and state-provided data on meal sites operated under the SSO and SFSP programs in Georgia. I describe each of these datasets in turn.

3.1 Student data

Student data used in this analysis comes from three large school districts in the metro-Atlanta area with a cumulative annual enrollment of over 250,000 students. This data includes standard demographic information for each student such as grade level, race, and free/reduced-price meal eligibility as well as annual test scores and measures of attendance and disciplinary incidents. It is is unique in the scope of student-level education data because I observe the exact address of each student during each school year, allowing me to determine exact distances to meal sites.[7] From the address data, I extract student zip codes and census tracts to be used as fixed effects in different model specifications. I also supplement my dataset with data on the nearest grocery store to each student, allowing me to control for the nearest large grocery store to each student during each year.

Each district, which I number 1 through 3 to preserve anonymity, administers assessments in math and reading to its students every fall. School begins in early August, and the assessments are administered in late August or early September. As such, they provide soonest opportunity to identify effects of summer meal sites after the school year begins. Other assessments in the district are administered in the winter and spring, long after summer meal sites have stopped operat-

[7] Address data is unavailable for SYs 2016-17 and 2017-18. For these two years, I impute a student's addresses based on where they lived during either SY 2015-16 (the year before the gap) or SY 2018-19 (the year after the gap).

ing for the year. The years of available test score data vary across the three districts in my study. District 2 has administered fall tests every year since SY 2017 each subject. Districts 1 and 3 began administering fall exams during subsequent school years, with District 3 beginning during SY 2018 and District 1 beginning during SY 2019.[8] My analysis covers SY 2017 through SY 2024.[9] Test scores are standardized with respect to 2020 national means and standard deviations within each grade level and subject (math and reading).[10]

In Table 1, I provide descriptive statistics for each student-year observation in my data broken down by how far each student lives from the nearest meal site during that year. I divide students into three groups: those who live within 0.5 miles of a meal site, those who live between 0.5 and 1 mile of a meal site, and those who live further than 1 mile from a meal site. As expected, students living closer to meal sites are significantly more likely to be eligible for free and reduced-price school meals: 77% of students within 0.5 miles of a site are FRPM eligible, compared to just 40% of students greater than a mile away. Students nearest to meal sites are also more likely to be both Black (63% within 0.5 miles vs. 38% further than a mile) and Hispanic (23% vs. 12%). They are also marginally more mobile than students living 1+ mile away, moving addresses 4 percentage points (p.p.) more often in a given year.

Average in-school achievement rises with distance to the nearest site, which could be driven in part by slightly (2 p.p.) lower attendance rates for students within 0.5 miles. The closest students to meal sites have average z-scores of -0.48 and -0.51 in reading and math respectively, which increase to -0.26 and -0.31 for students between 0.5 and 1 mile and then to 0.25 (in both subjects) for students 1+ mile away. This clearly illustrates why simple ordinary least squares is assuredly biased upward in a simple regression of achievement on site distance.

Table 1 also reveals that test scores are more sparsely-reported than other measures. While all other variables are required by the state to be collected for every student during every year, administering fall assessments is completely voluntary on the part of individual districts. The lack of a state reporting mandate creates more situations where students (for example, those in a particular grade or school) have no reported score in a given year. Furthermore, districts can decide whether to test all students ("universal" testing) or particular subgroups of students at their discretion. I discuss issues around test score missingness and universal testing at length in a later robustness check.

[8] District 3 had a limited rollout of testing in math during SY 2018, which I omit from my analysis sample.

[9] While I only have test score outcomes since SY 2017 at the earliest, I have student address data for all districts dating back to SY 2015 and data on student demographics and in-school outcomes like attendance and disciplinary incidents dating back to SY 2004.

[10] These are the most recent publicly-released benchmarks as of the time of writing. Means and standard deviations are not reported annually by the testing vendor.

3.2 Treatment Timing

In my panel, a student's nearest meal site in year t is determined by the address they live at during school year t. For example, a student who reports living at 123 Main St. at the beginning of SY 2018 will be assigned treatment (distance) based on the closest site to them during summer of 2018, which is at the end of SY 2018. However, the relevant test score for that student is taken during fall of SY 2019 (immediately after summer 2018 concludes). As such, a student's test score in year t is a function of meal site access at school year $t-1$ (the summer immediately before the school year begins). To clarify this timing, I provide a timeline in Figure 7. This means that while my panel ranges from SY 2017 to SY 2024, it uses site distances and student addresses ranging from SY 2016 to SY 2023.

3.3 Summer meal site data

In Georgia, SSO sites are administrated by the Georgia Department of Education (GADOE) while SFSP sites are administrated by the Georgia Department of Early Care and Learning (DECAL). I obtained site-level location data including exact site address, dates of operation, sponsoring organization, and types of meals served for each SFSP and SSO site by submitting Freedom of Information Act (FOIA) requests to each agency. This data covers every site in the state of Georgia from summer 2015 until summer 2024. It also indicates whether a site was "open" (usable by any child) or "closed" (open only to children enrolled at the site). I observe numbers of meals served by month and reimbursement amounts paid by site for SSO sites only.

While site data comes with a site's address and a unique ID for each sponsor (sponsors being organizations that generally host many sites each summer), it does not come with a unique geographic ID for a site to monitor openings and closings. For example, a site at 123 Main St. which is sponsored by a church in one year and the local school system in the next would have two site IDs despite being in the same location. I address this by defining a "site" as a specific geographic location hosting a meal site, regardless of who operates the site.[11]

I contrast the types of locations that host summer meal sites between the SSO and SFSP programs in Figure 1, which includes every site operated in metro-Atlanta under both programs between SY 2016 and SY 2024. Because SSO sites can only be operated by SFAs that participate in the National School Lunch Program, the majority of SSO sites are located at schools. In contrast, most SFSP sites – which do not need to be operated by an SFA – are located in apartment complexes or other local points of interest.

[11] I generate my ID by extracting the latitude and longitude of each site and applying the Density-Based Spatial Clustering of Applications with Noise (DBSCAN) algorithm to generate site IDs. In short, this algorithm groups latitudes and longitudes into "clusters" that expand to include sites no more than 0.1 miles away from sites currently in the cluster. This is superior to generating an ID based purely on latitude and longitude coordinates because coordinates are sensitive to how addresses are entered by site operators.

3.4 Identifying nearby meal sites

I identify the nearest meal site for each student by geocoding every home address, finding the linear distance between that home address and all operational meal meal sites, and taking the minimum across distances in that year. I calculate Haversine distance, which (unlike Euclidian distance) implicitly accounts for the curvature of earth.[12] Geocoding was done in ArcGIS Pro using a custom geocoder that combines address point data collected from county GIS offices in each metro-Atlanta county. Among sites that were closest to at least 1 student during SY 2024, the average number of students for which that site was the nearest option was 570. This suggests that the sites I study are each accessible to large numbers of students.

4 Methods

I estimate relationship between food access and student achievement in school using individual-level variation in distance to the nearest meal site with two quasi-experimental designs: a fixed effects-instrumental variables (FE-IV) approach and a standard two-way fixed effects (TWFE) approach. The former accounts for endogenous changes in distance to a meal site driven by students moving homes. The latter allows me the to more flexibly include fixed effects and controls that may be related to student moves. I discuss each approach in this section.

4.1 Fixed effects-instrumental variables (FE-IV) approach

There are two reasons that a student can experience a change in distance to the nearest meal site between one year and the next: either a meal site opens or closes near a student's home, or a student moves addresses. Variation in distance to the nearest site driven by a student moving homes is likely to be endogenous the student's performance in school. For example, a student may move following a divorce and experience a decline in performance at the same time his distance to the nearest meal site changes, biasing results. On the other hand, variation in distance coming from sites opening and closing (due to administrative burden or staffing shortages, for example) is likely exogenous to the student's in-school performance. The FE-IV approach isolates this more desirable type of variation.

To handle endogeneity in the relationship between meal site distance and achievement, I instrument for meal site distance using a student's hypothetical distance to the nearest meal site had they never moved. This approach closely follows that of Currie and Rossin-Slater (2013) and Rossin-Slater (2013). For any student who never moves addresses, this instrument is identical

[12] While driving distance or drive time may be relevant proxies for accessibility of meal sites, data sharing agreements prohibit channeling data through the usual APIs and other online options used for calculating such metrics. As such, I use Haversine distance as my primary measure of distance.

to the student's regular distance to the nearest site each year. For a student who moves, the instrument fixes the student at his first observed address and estimates the distance from that address to the nearest site. This satisfies the relevance condition because hypothetical distance to the nearest site correlates highly with true distance. To satisfy the exclusion restriction, one must control for the fact that a family's selection into an initial address may reflect preferences on how far to live from a meal site. This can be accomplished by including a student fixed effect in the model if such preferences are assumed to be time-invariant. When constructed in this manner, the instrument measures changes in site distance driven by openings and closing of sites conditional on a first choice of location. In other words, it isolates the plausibly exogenous variation in a student's distance to the nearest meal site that comes from site openings and closings (rather than the endogenous portion that comes from moving) while controlling for selection into the initial address. I estimate the model using two-stage least squares as follows.

$$D_{ig,t-1} = \alpha_1 HDS_{ig,t-1} + \gamma_t + \nu_g + \psi_i + \eta_{igt} \tag{1}$$

$$Y_{igt} = \tau \hat{D}_{ig,t-1} + \delta_t + \sigma_g + \pi_i + \epsilon_{igt} \tag{2}$$

Equation (1) regresses the true nearest site distance of student i in grade t during time $t-1$, $D_{ig,t-1}$, on hypothetical distance HDS and its square[13] as well as time γ_t, grade ν_g, and student ψ_i fixed effects with idiosyncratic error term η_{igt}. In Equation (2) Fall achievement Y for student i and grade g during school year t is regressed on predicted meal site distance from last school year $\hat{D}_{ig,t-1}$ and the same set of fixed effects. The vector τ contains coefficients for distance and its square to allow for nonlinearities in the relationship between meal site access and achievement. To illustrate how much within-student variation is created by sites opening and closing, Figure 2 displays the annual share of students whose distance to the nearest site experiences each possible outcome: no change, a change due to a site opening or closing, and a change due to the student moving homes. It reveals that only a small share of all students experience a change in distance to nearest meal site driven by moving homes, while nearly half experience a year-over-year change due to site openings or closings. Around 30-45% of students see no change in distance to the nearest site between any two years.

4.1.1 Identifying and Interpreting the Causal Parameter

In this section, I discuss and interpret the target causal parameter $\hat{\tau}$ in my FE-IV analysis. Particularly because distance is itself a first stage for true use of a meal site, the interaction between observed distance and my "hypothetical distance" instrument in a LATE framework warrants fur-

[13] Including the square of distance allows me to capture possible nonlinearities in the relationship between distance to a meal site and student performance, as one would not expect the marginal association between the 1st and 2nd mile of added distance to equal the marginal association between the 11th and 12th miles, for example.

ther discussion.

I first discuss whether the "hypothetical distance" instrument is likely to satisfy the monotonicity condition of Angrist and Imbens (1995), which is required to recover causal estimates of a local average treatment effect (LATE). Under the "complier/defier" framework of Angrist et al. (1996), monotonicity requires that there are no defiers – those whose dose of the instrument causally changes their true distance to a meal site in the opposite direction of the compliers. In my setting, a complier is any student whose true distance decreases (increases) when a site opens (closes) nearby. Any student who remains at their original address is therefore a complier. Because the instrument only varies when sites open and close, a defier would be any student who chooses to (i) move further away from the nearest site because a site opened closer to his initial address or (ii) move closer to a site because the site nearest to their initial address closed. Meeting either condition requires advanced notice of site locations (which are generally announced at the beginning of summer) and willingness to relocate one's family based incremental changes in distance to the nearest site, making the existence of defiers unlikely.[14] Students who move closer to sites regardless of their hypothetical distance are always-takers, while students who move further away regardless of their hypothetical distance are never-takers. These groups reduce the precision of my estimates but do not introduce bias.

4.2 Two-way fixed effects (TWFE) with continuous distance

I also model fall student test scores (in standard deviation units) as a function of a student's distance from the nearest meal site (in miles) as described in Equation (3).

$$Y_{igzt} = \tau D_{igz,t-1} + \beta X_{igz,t-1} + \gamma_t + \delta_i + \pi_g + \eta_z + \epsilon_{igzt} \tag{3}$$

In Equation (3), I express student test score Y_{igzt} (in either reading or math) in terms of distance from meal site during the prior summer $D_{i,t-1}$, unit (student) and time (year) fixed effects δ_i and γ_t, grade fixed effects π_g, student zip code fixed effect η_z, student covariates $X_{i,t-1}$, and idiosyncratic error term ϵ_{igzt}. The contents of $X_{i,t-1}$ vary by model specification include factors such as whether the student moved addresses in the prior year and distance to the nearest grocery store during the prior summer. τ estimates the relationship between an additional mile of distance to the nearest meal site and student performance; in most specifications, $D_{i,t-1}$ is a vector of distance to the nearest site and its square for each student. My TWFE approach allows me to include sets of fixed effects that would re-introduce endogeneity from students who move addresses were they to be included in my FE-IV specification. For example, zip code fixed effects can only be identified off of students who change zip codes in a model with student fixed effects.

[14] Even if there are some defiers, $\hat{\tau}$ is likely still a policy-relevant parameter in that it captures the aggregate effect of summer meal sites rather than just effects coming through the accessibility channel (Heckman and Vytlacil, 2005).

5 Results

I present my results in two parts. In the first, I provide evidence on the accessibility of summer meal sites to students in my sample. In the second, I provide evidence on the relationship between food access and student achievement in school.

5.1 Summer meal site accessibility

This section focuses on the extent to which students in the study's three metro-Atlanta districts have nearby access to federally-funded meal sites and documents the trends underlying site access over time. Figure 3 plots the annual average distance to nearest meal site for students in my sample over time, in aggregate and by free/reduced-price meal status. The average student lived nearly 1.5 miles from the nearest meal site in 2017, which fell to just under 1 mile by 2021 when COVID-era site location waivers were still in effect. The expiration of site location vouchers in 2023 brought a decline in the number of operational sites and a corresponding 0.5 mile increase in average student distance to nearest meal site.

Consistent with the goal of the programs, meal sites locate closer to the homes of FRPM eligible students than non-FRPM eligible students. FRPM-eligible students live an average of just over half a mile from the nearest site, substantially closer than higher-income students. Notably, nearly all of the reduction in average site distance over the past decade is driven by higher-income students – there is no noticeable downward trend for lower-income students. The lack of downward movement in average distance for FRPM-eligible students during the waiver period suggests that removing site location restrictions alone is not sufficient to improve access for the neediest children.

While this view indicates that the average student now lives further from a meal site than at any point since 2018, annual averages say little about within-year differences in site accessibility for children. Figure 4 addresses this by displaying the cumulative density function (CDF) of site distance by FRPM status during SY 2024. It reveals that approximately 80% of FRPM-eligible students live within a mile of the nearest meal site, compared to around 45% of non-FRPM students. This suggests that federal meal programs are, on average, accessible to the students they are designed to serve.

I compare the access distribution between FRPM eligible and non-FRPM eligible students between 2021 (the year during which operational sites were at a high and student distances to sites were at a low) and 2024 (the most recent summer in my data) in Figure 4 to illustrate the effect of COVID location waivers. I include markers at the 1/2-mile and 1-mile thresholds because the USDA uses these distances to define food deserts in urban areas (USDA Food & Nutrition Service, 2025c) and because 1 mile is considered the "walkable" range for younger students by many metro-Atlanta school districts. A comparison of the CDFs for FRPM students in 2021 vs.

2024 reveals no noticeable differences: the expiration of COVID-era site location waivers hardly changed the distance distribution for low-income children. On the other hand, removal of the waivers resulted in around 15% of higher-income students losing access to sites at a 1-mile radius and 20% losing access at a 2-mile radius.

5.2 Food access and student achievement

In this section, I present the results of my empirical analysis on the effect of site access on in-school achievement. All analyses measure the marginal effect of a one-mile increase in distance to the nearest meal site on fall assessment performance, measured in standard deviation units. As such, a negative coefficient on distance should be interpreted as proximity to meal sites *improving* academic achievement: as the nearest site becomes further away, test scores fall.

I present the second-stage results from my primary FE-IV analysis in Table 2. I find that increasing distance to the nearest meal site decreases academic achievement modestly. Column (I) reports results from my preferred model specification, which includes student, year, and grade level fixed effects as well as the square of site distance. The first mile of distance to a meal site decreases math achievement by 3.2% of a standard deviation and reading achievement by 1.8% of a standard deviation, effects which taper off as distance increases. This finding is intuitive: distance matters, but the effects are nonlinear because a 1-mile increase from 0 to 1 miles imposes a larger access barrier than the same 1-mile increase from a further initial distance where sites may already be inaccessible. In Column (II) I remove the quadratic term from the FE-IV regression and find effects that are significant but attenuated, an expected result given that they represent an average over the u-shaped effect curve implied by Column (I).

In Columns (III) and (IV) of Table 2, I vary the fixed effects included in my model specification. In Column (III) I demonstrate that while grade fixed effects are important in theory (to avoid conflating grade progression effects with treatment effects), they make only a modest difference for my point estimates. In Column (IV) I include school fixed effects to purge time-invariant unobserved factors like school quality that may correlate with site distance. Doing so does not change the interpretation of my results, but I omit the school fixed effects from my preferred specification since they are identified off of students who move schools (in the presence of student fixed effects) and may therefore re-introduce endogeneity from address movers to my FE-IV analysis.

I report the first stage joint F-statistics for each 2SLS regression in the final row of Table 2 to provide evidence that my instrument meets the "relevance" condition of an instrumental variables analysis. These statistics are generally in the range of $1,500$, suggesting that the hypothetical distance instrument is quite strong. This is expected given that a student's hypothetical distance and true distance are equivalent until they move addresses, but is worth exploring to rule out the possibility that high student mobility breaks the relationship too early in the panel. I also present results from the first stage regressions of true distance on hypothetical (instrument) distance and

fixed effects in Table 3. Because my preferred specification includes two endogenous regressors (distance and its square), Table 3 reports coefficients from four regressions: separate regressions of 1) distance and 2) distance squared on the full set of instruments (hypothetical distance and its square) and exogenous fixed effects, for each of math and reading.

5.3 Effect Heterogeneity

I now study whether treatment effects differ along two important margins: by income level and age of students. In Table 4, I present results from my primary specification (Column I) alongside results from models where distance and its square are interacted with an ever-FRPM eligible indicator[15] in Column II and with an indicator for middle/high school (grades 6-12) in Column III. I find that effects in reading are driven by lower-income students, but that both lower and higher-income students see returns to math scores from proximity to meal sites. Benefits of meal site access also diminish much more quickly for low-income students than for high-income students, as seen in the differences between the coefficients on the quadratic term. This may suggest that access barriers are especially pronounced for FRPM-eligible students, who suffer greater losses after the first mile of increased distance to the nearest site.

Because the quadratic term makes differences between groups hard to visualize for larger distances, I present the average marginal effect of distance from each mile from 0 to 3 in Figure 5. The figure plots the average marginal effect of a 1-mile increase from each mile indicated on the x-axis: the effect of increasing distance from 0 to 1 miles, 1 to 2 miles, 2 to 3 miles, and so on. For both FRPM eligible and non-FRPM eligible students, the effect of marginal distance increases fades out substantially by mile 3. While there are no statistically significant differences between students from lower and higher income families, the results are suggestive that the first two miles of distance affect test scores more for FRPM eligible students than for non-FRPM eligible ones.

Column III suggests that effects are driven almost entirely by students in grades K-5, whose scores fall 5.8% of a standard deviation in math after the first mile of distance to the nearest site. Meanwhile, middle and high school students (grades 6-12) see virtually no effect of distance on their academic achievement. This result is expected due to documented trends in summer meal site usage by children of different ages. The most comprehensive study of USDA summer meal sites to date finds that just 14% of all meals were served to high school students, compared to 55% to elementary school students and 21% to middle school students (Vericker et al., 2021). Distance is a first-stage for actual meal site use. As such, it is logical for the effect of distance to attenuate as

[15] Due to within-student variation in FRPM eligibility over time (particularly during the pandemic years), I adopt "ever-FRPM eligible" and "never-FRPM eligible" labels for this analysis. An "ever-FRPM eligible" student is one who was designated as FRPM eligible during any single year, whereas "never-FRPM eligible" students were never eligible for free/reduced-price school meals.

students with lower underlying propensities to use the sites are added to the sample.

6 Robustness & Sensitivity Checks

In this section, I conduct a series of robustness and sensitivity checks for my findings. I begin by estimating a series of two-way fixed effects (TWFE) models as an alternative specification to my FE-IV strategy. Sensitivity checks include approaches to handling non-random test score missingness in my sample and robustness checks that omit effects of the COVID-19 pandemic and associated remote learning. I conclude with a placebo test of my main findings.

6.1 Two-Way Fixed Effects

I estimate traditional two-way fixed effects (TWFE) models under a variety of different specifications as an alternative to my FE-IV approach. I directly regress achievement on a student's true distance rather than on the predicted values from a first stage as under the FE-IV framework. While the TWFE approach leaves my estimates more exposed to bias introduced by students who move addresses, it also allows me to include additional fixed effects that would require instruments themselves were they to be included in the FE-IV model. I present the results in Table 5. Columns I of Table 5 keeps the same set of fixed effects as my main specification (student, grade, and year). When endogeneity from student moves is not handled, results attenuate slightly in both subjects relative to the FE-IV approach. I then check whether the point estimates are robust to the inclusion of student zip code, student census tract, and school fixed effects. In the presence of student fixed effects, these additional effects are identified off of students who move schools, homes, or both and therefore re-introduce endogeneity. As such, they are not appropriate to include in the FE-IV analysis.

Column VI provides the most stringent test of my empirical strategy by including both school and census tract fixed effects. This results in slight attenuation of the point estimates for both math and reading, but does not substantively change the interpretation of my results. Census tracts are the smallest geographies available in the data; particularly in urban areas like metro-Atlanta, they have small average radii and therefore severely restrict the possible variation in site distance when included as a fixed effect. On the other hand, they are compelling way to control for possible "neighborhood change" effects. It is reassuring that effects remain detectable after they are included in the model, particularly alongside both student and school fixed effects.

6.2 The COVID-19 Pandemic and Remote Instruction

My panel is bisected by the COVID-19 pandemic, during which both district testing protocols and operating procedures for summer meal sites changed substantially. Students took fall assess-

ments at home rather than at school during fall 2020 (of SY 2020-21), resulting in both higher-than-usual variance in scores and grade inflation (Sass and Goldring, 2021). This, coupled with disruptions to meal site operations and public health concerns, make the setting potentially problematic for my analysis.[16]

Table 6 displays the results from my preferred model specification (student, year, and grade fixed effects) estimated using 1) the full analysis sample, 2) a subsample from before the COVID-19 pandemic, and 3) a subsample of all years except for SY 2021. Results in Columns III and IV reveal that my effects are not driven by systemic changes brought on by the COVID-19 pandemic; the point estimates barely change even when the model is estimated using only pre-2021 data (SY 2016-17 to SY 2019-20). Columns V and VI conduct a similar exercise, but only drop data from SY 2021 to omit any scores from exams taken at home. As such, the underlying analysis sample includes only exams taken in school. The point estimates are once again nearly unchanged.

6.3 Test Score Missingness

Because fall testing is voluntary on the part of school districts (as opposed to the mandatory state exams given in the spring), there is variation year-over-year in which grades are tested and whether testing is universal for a given grade. This results in many cases where students do not have test score in every year of my panel. A related concern is that a student's probability of having a missing score is in some way correlated with their site distance (or more precisely, correlated with within-student changes in distance over time). For example, a district may choose to test only under-performing students in a particular grade and year. This could create a spurious relationship between site distance and test score missingness, potentially biasing my results.

I first examine whether there is evidence of selective testing in my data. I do so by calculating the testing rate for each district-grade-year cell (the testing rate for 2nd graders in District 1 in 2019, etc.) by summing the number of student test scores in that cell and dividing by the total number of students enrolled in the relevant district-grade-year. I then generate a histogram of the district-grade-year testing rates, which I display in Figure 6.

While there is evidence that districts are not attempting to test all students in all grades during each year, the majority of district-grade-year cells cluster at around 80% to 90% exam participation. That range is broadly consistent with a universal testing policy after accounting for the expected absence rate of the day of the test (~10%) plus some additional score missingness due to factors like technical errors with score reporting.

After calculating the district-year-grade testing rates, I generate a test score missingness indicator and regress it on site distance, its square, and the set of fixed effects from my preferred model specification. I then test whether the relationship between test score missingness and site distance

[16] Year fixed effects alone are not equipped to handle changes in variance of the outcome or effects that differ across subgroups along unmeasured dimensions, such as whether a child's parents were working from home.

changes as I restrict the sample just to district-year-grade cells with plausibly universal testing. The goals of this exercise are to 1) establish whether there is any systematic relationship between score missingness and meal site distance, 2) see whether any such relationship is still present when restricting the analysis sample just to grades with universal testing, and 3) check whether estimating models using subsamples of grades with universal testing materially changes my main results. I display the results of this analysis in Table 7.

I find that score missingness increases modestly with each mile of distance to the nearest meal site in both math and reading in the full sample. Although the magnitudes are small (~2-4% higher likelihood of having a missing score as distance increases by the first mile), this provides suggestive evidence that score missingness is somewhat non-random. However, the relationship between score missingness and site distance attenuates as I progressively restrict the sample to grades with higher testing rates. By the time I include only observations from grades with testing rates greater than 90% (as in the final column of Table 7), there is no relationship at all. In Panel B of Table 7, I estimate my primary FE-IV model for each sample. Even when restricting the analysis to students in grades with 90%+ testing rates, which throws out nearly 85% of the data, my point estimates remain quantitatively similar to those estimated off of the full sample.

6.4 Placebo Test

As a further robustness check, I test whether a student's test score this year is affected by next summer's hypothetical (instrument) site distance. In theory, how far a student lives from a meal site in the future should not affect current achievement. If a test of this nature picks up effects, it may suggest that my empirical strategy is picking up the effects of an unobserved confounder (such as local crime or neighborhood change) rather than true effects of the program. I regress achievement in each of math and reading on 1) the current year's instrument distance, 2) next year's instrument distance, and 3) both distances concurrently in Table 8.[17]

As expected, the reduced form regression of test scores on this year's instrument distance yield effects that are quantitatively similar to my main effects (see Columns I and IV of Table 8). Regressing test scores on next year's instrument distance yields null effects in math and reading for the linear term, though the quadratic term comes out statistically significant in both subjects (Columns II and V). When both current and future distance are included simultaneously in the regression, the coefficient on the linear term for future distance is null while that of current year distance remains nearly unchanged. The quadratic terms remain statistically significant but modest in magnitude for both current and future distance, across both subjects.

[17] Each regression also includes the full set of fixed effects from my preferred model specification: student, grade, and year.

7 Conclusion

In this paper, I use student-level data from three large school districts in metro-Atlanta to analyze the reach of federal USDA-funded summer meal programs and their effect on student performance. I find that approximately 80% of all free/reduced-price eligible students live within 1 mile of a free meal site during the summer, and that removing federally-mandated site location restrictions did little to reduce distances for the 20% with lower access. I also show that approximately 45% of students in any given year experience a change in where the nearest site is located compared to the prior year, speaking to inconsistency in where sites operate over time.
Finally, quasi-experimental evidence from an instrumental variables-fixed effects (IV-FE) approach reveals that K-12 students perform between 1-3% of a standard deviation worse on fall assessments when their distance to the nearest site increases from 0 to 1 mile, with effects strongest for FRPM-eligible students and elementary-aged students. I also find stronger effects in math than in reading, a result consistent with prior literature on the impact of federal food programs for children (Gordanier et al., 2020). My findings have policy implications at the federal level, where rules governing eligible summer meal site sponsors and locations are set, and at the local level for school districts and other community organizations calibrating participation in federal food programs.

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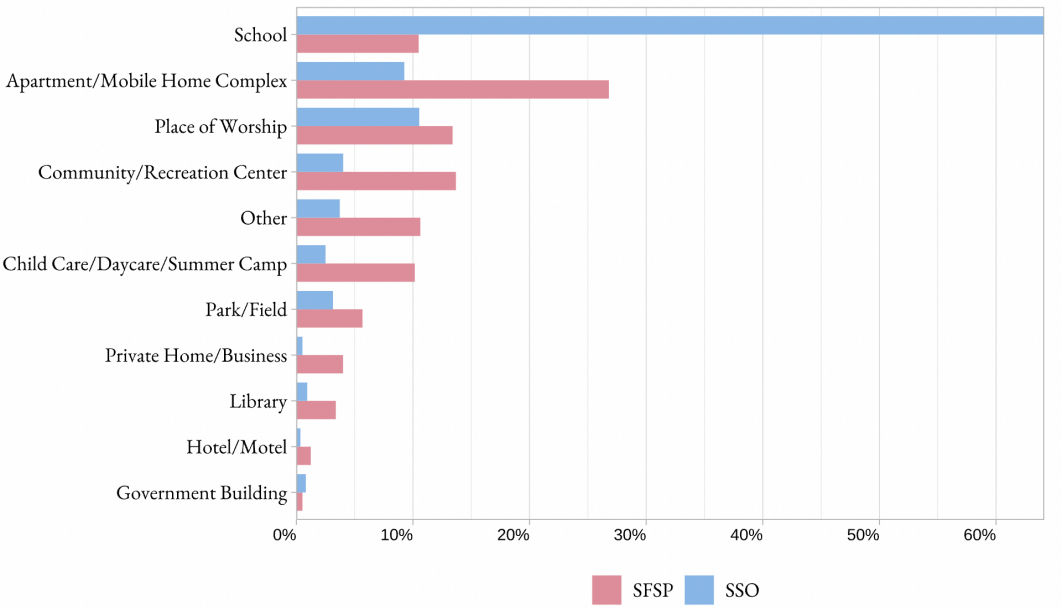
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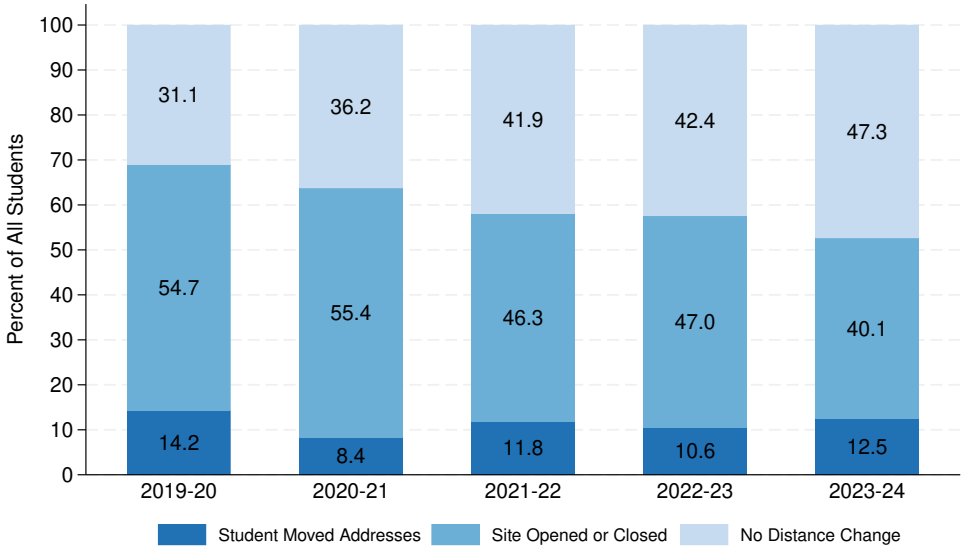
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Figure 1: Percent of metro-Atlanta meal sites by location type, SY 2016 - SY 2024



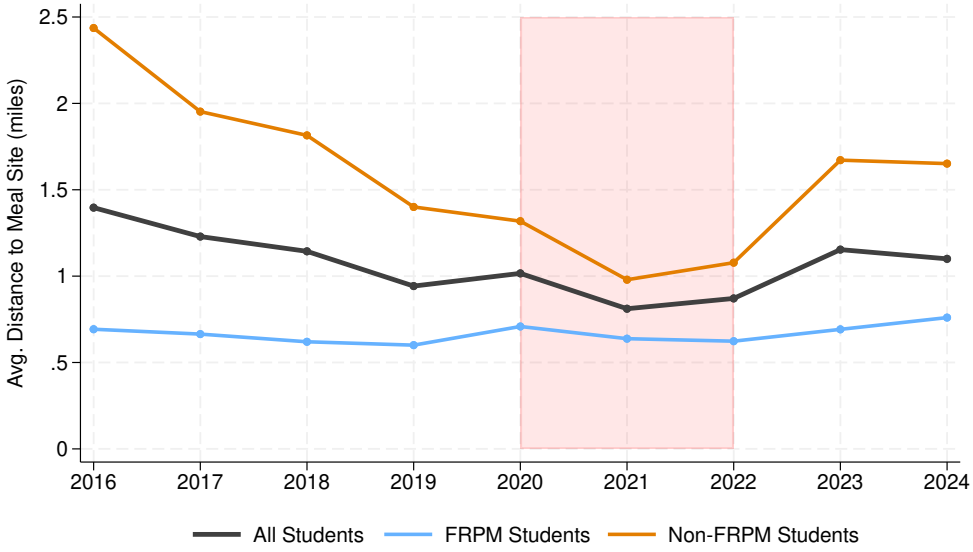
Notes. The figure above shows the percent of SSO and SFSP meal sites in metro-Atlata that operated at each type of host location from SY 2016 until SY 2024.

Figure 2: Percent of Students Experiencing Change in Meal Site Distance



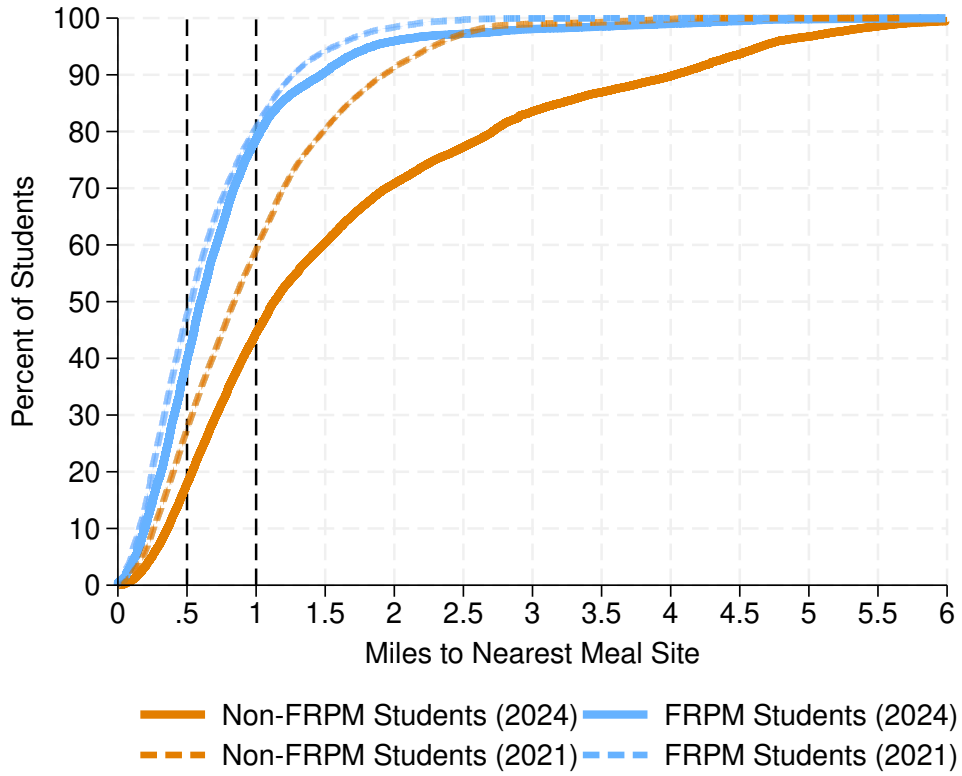
Notes. The figure above displays the share of students whose site distance either changed due a the student moving addresses, changed due to sites opening or closing near the student, or did not change at all between the year listed on the x-axis and the year prior. For example, 40.1% of all students across the three districts studied had their nearest site distance change due to site openings and closings between summer of SY 2023 and summer of SY 2024.

Figure 3: Annual Average Distance to Meal Site for FRPM vs. Non-FRPM Students



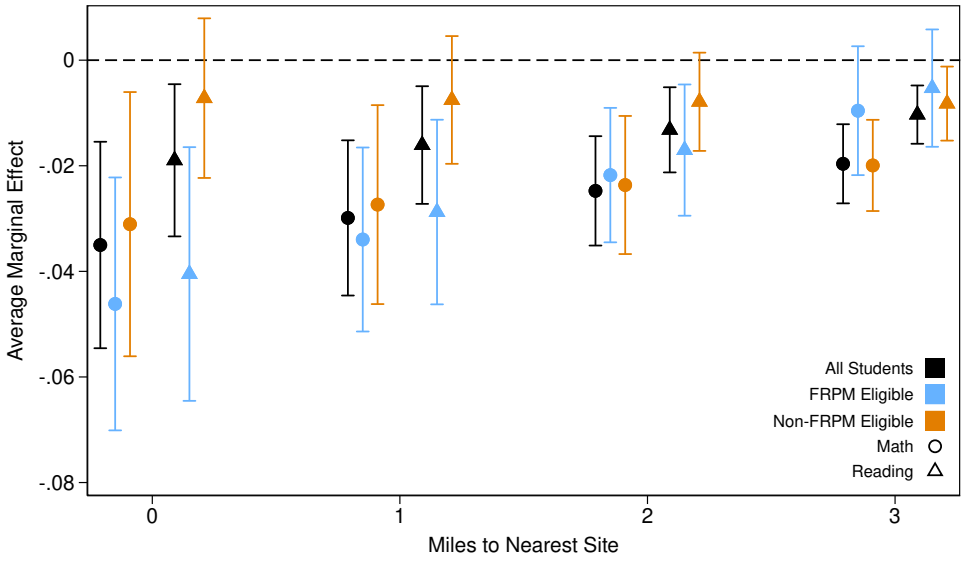
Notes. The figure above displays the average distance that students of the type indicated by the color of the line lived from their nearest meal site during the year specified on the x-axis. The red region indicates summers during which USDA waivers allowing sites to locate in any community were in place.

Figure 4: Percent of Students Living at Each Distance to Meal Site (SY 2024 vs. SY 2021)



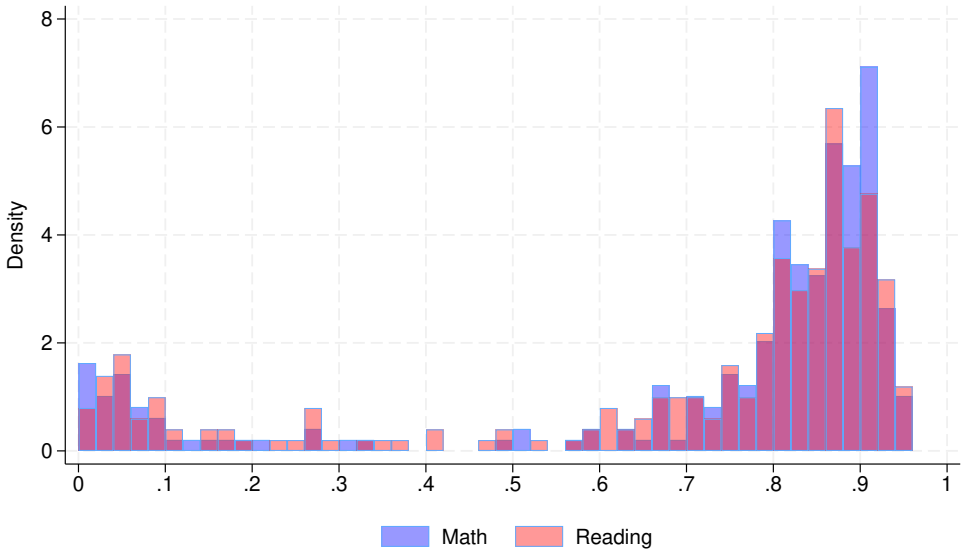
Notes. The figure above displays the cumulative density function (CDF) of meal site distance for students in the study's three partner school districts during the summers of 2021 and 2024. Markers are placed at 0.5 miles and 1 mile to mirror USDA food desert boundaries. One mile is also the designated "walking range" for younger children in many school districts in the metro-Atlanta area.

Figure 5: Average marginal effects of distance on student achievement by income level.



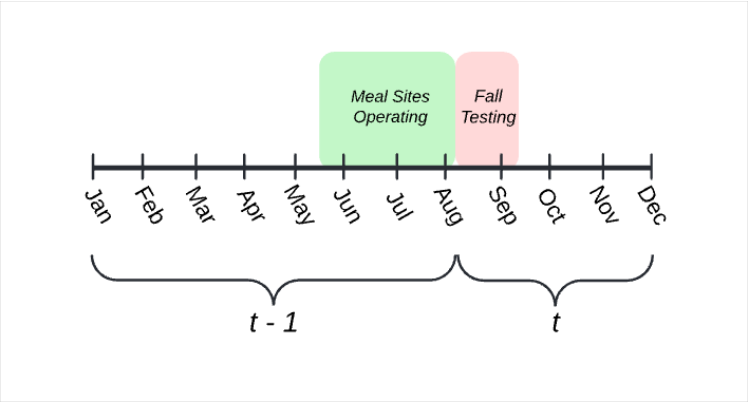
Notes. The figure above displays the marginal average effect and corresponding 95% confidence interval of increasing site distance 1 additional mile from the base distance indicated on the x-axis. For example, the far left cluster of estimates show the average marginal effect of increasing site distance from 0 to 1 miles for each subgroup of students in each subject. Results come from the primary FE-IV specification, which includes student, grade, and year fixed effects.

Figure 6: The distribution of district-grade-year testing rates by subject.



Notes. The figure above plots the distribution of fall testing rates across all district-subject-grade cells from SY 2016-17 to SY 2023-24, by subject. For example, one data point is the share of all students in grade 3 who took the math formative assessment in District 3 during fall of SY 2019.

Figure 7: Annual timing of meal site operation and fall formative testing.



Notes. The figure above clarifies the time structure of the panel. A fall exam taken by a student in year t is expected to be affected by meal sites in year $t-1$, the operation of which directly precedes the fall testing window.

Table 1: Descriptive statistics by distance to the nearest meal site.

	Nearest Site < 0.5 Miles			Nearest Site 0.5 - 1 Miles			Nearest Site > 1 Miles		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Demographics									
FRPM Eligible	0.77	0.42	750,638	0.68	0.46	633,185	0.40	0.49	573,656
Female	0.49	0.50	749,437	0.49	0.50	632,303	0.49	0.50	573,045
Black	0.63	0.48	749,437	0.62	0.49	632,303	0.38	0.49	573,045
White	0.06	0.23	749,437	0.11	0.31	632,303	0.31	0.46	573,045
Asian	0.05	0.22	749,437	0.05	0.21	632,303	0.15	0.36	573,045
Hispanic	0.23	0.42	749,437	0.20	0.40	632,303	0.12	0.33	573,045
Other / Multiple Races	0.03	0.16	749,437	0.03	0.17	632,303	0.04	0.19	573,045
Moved Addresses This Year	0.14	0.34	750,638	0.13	0.34	633,185	0.10	0.30	573,656
In-School Outcomes									
Number of Disciplinary Incidents	0.23	0.92	750,638	0.21	0.87	633,185	0.13	0.68	573,656
Any Disciplinary Incident	0.11	0.31	750,638	0.10	0.30	633,185	0.07	0.25	573,656
Attendance Rate	0.92	0.11	749,324	0.93	0.10	632,240	0.94	0.08	573,020
Fall Reading Score	-0.48	1.18	446,609	-0.26	1.20	367,680	0.25	1.19	316,384
Fall Math Score	-0.51	1.14	454,958	-0.31	1.17	373,693	0.25	1.26	313,169
Meal Site Outcomes									
Nearest Meal Site Distance	0.29	0.13	750,638	0.71	0.14	633,185	2.08	1.19	573,656
Hypothetical Site Distance (Instrument)	0.36	0.27	749,184	0.69	0.28	631,692	1.94	1.26	572,418

Notes: Descriptive statistics above are calculated at the student-year level across all years from SY 2016-17 to SY 2023-24.

Table 2: The effect of summer meal site distance on student achievement.

	I		II		III		IV	
	Math	Reading	Math	Reading	Math	Reading	Math	Reading
Site Distance	-0.035***	-0.019**	-0.021***	-0.011***	-0.039***	-0.025***	-0.027***	-0.019***
	(0.010)	(0.007)	(0.005)	(0.004)	(0.012)	(0.008)	(0.009)	(0.007)
Site Distance^2	0.003*	0.001			0.004**	0.003**	0.002	0.001
	(0.001)	(0.001)			(0.002)	(0.001)	(0.001)	(0.001)
School FE							✓	✓
Grade FE	✓	✓	✓	✓			✓	✓
Student, Year FE	✓	✓	✓	✓	✓	✓	✓	✓
N	902,453	881,430	902,453	881,430	902,453	881,430	902,450	881,425
First stage F-statistic	1,582	1,754	3,111	3,175	1,576	1,744	1,536	1,709

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

Notes. Standard errors are reported in parentheses. Clusters are defined as groups of students who share a common closest meal site, and standard errors are clustered at the nearest site ID accordingly. The outcome of each regression is a student's standardized fall formative assessment score. Effects are expressed in standard deviation units. Estimates come from the second stage of a 2SLS procedure where achievement is regressed on predicted distance, with predicted distance coming from a first stage regression of endogenous "true" distance on hypothetical distance had the student never moved. Regressions include the fixed effects listed in the corresponding column, in both the first and second stages.

Table 3: First stage results for main FE-IV analysis.

	Math		Reading	
	I	II	III	IV
	True Distance	True Distance^2	True Distance	True Distance^2
Hypothetical Distance	0.831***	-0.080	0.827***	-0.085
	(0.021)	(0.063)	(0.021)	(0.065)
Hypothetical Distance^2	0.016***	0.961***	0.016***	0.961***
	(0.003)	(0.017)	(0.003)	(0.019)
N	902,453	902,453	881,430	881,430

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

Notes. Standard errors are reported in parentheses. Clusters are defined as groups of students who share a common closest meal site, and standard errors are clustered at the nearest site ID accordingly. The outcome of each regression is either a student's true meal site distance or its square, depending on the column. Estimates come from the first stage regression of true distance on hypothetical (instrument) distance. All specifications include student, year, and grade fixed effects.

Table 4: Heterogeneous treatment effects by student age and family income level.

	Main Results		Effects by Income Level		Effects by Grade Bin	
	Math	Reading	Math	Reading	Math	Reading
Site Distance	-0.035***	-0.019**	-0.031**	-0.007	-0.058***	-0.013
	(0.010)	(0.007)	(0.013)	(0.008)	(0.012)	(0.012)
Site Distance^2	0.003*	0.001	0.002	-0.000	0.004**	0.000
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Ever FRPM-Eligible x Site Distance			-0.015	-0.033**		
			(0.016)	(0.014)		
Ever FRPM-Eligible x Site Distance^2			0.004*	0.006***		
			(0.002)	(0.002)		
Middle/High School x Site Distance					0.055***	-0.012
					(0.021)	(0.023)
Middle/High School x Site Distance^2					-0.005	0.002
					(0.003)	(0.004)
N	902,453	881,430	902,453	881,430	902,453	881,430
First stage F-statistic	1,582	1,754	916	970	430	459

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

Notes. Standard errors are reported in parentheses. Clusters are defined as groups of students who share a common closest meal site, and standard errors are clustered at the nearest site ID accordingly. The outcome of each regression is a student's standardized fall formative assessment score. Effects are expressed in standard deviation units. Estimates come from the second stage of a 2SLS procedure where achievement is regressed on predicted distance, with predicted distance coming from a first stage regression of endogenous "true" distance on hypothetical distance had the student never moved. All regressions include student, year, and grade fixed effects. Heterogeneous effects are estimated by instrumenting for the interaction of true distance and the relevant group indicator with the interaction of the instrument distance and the group indicator. Ever FRPM-Eligible takes value 1 if a student is eligible for free or reduced-price meals in any single year, and 0 otherwise. Middle/High School takes a value of 1 if a student is in grades 6-12 and 0 otherwise.

Table 5: Two-way fixed effects regression of fall achievement on site distance.

	I		II		III		IV		V		VI	
	Math	Reading	Math	Reading	Math	Reading	Math	Reading	Math	Reading	Math	Reading
Site Distance	-0.025***	-0.009	-0.017***	-0.008**	-0.024***	-0.009	-0.024***	-0.010*	-0.020***	-0.011*	-0.019***	-0.011*
	(0.008)	(0.006)	(0.004)	(0.003)	(0.008)	(0.006)	(0.008)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)
Site Distance^2	0.001	0.000			0.001	0.000	0.001	0.000	0.001	0.001	0.001	0.000
	(0.001)	(0.001)			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Zip Code FE					✓	✓						
Census Tract FE							✓	✓			✓	✓
School FE									✓	✓	✓	✓
Grade FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Student, Year FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	902,453	881,430	902,453	881,430	902,453	881,430	902,439	881,415	902,450	881,425	902,436	881,410

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Notes. Standard errors are reported in parentheses. Clusters are defined as groups of students who share a common closest meal site, and standard errors are clustered at the nearest site ID accordingly. The outcome of each regression is a student's standardized fall formative assessment score. Effects are expressed in standard deviation units. Estimates come from a regression of achievement directly on a student's distance to the nearest meal site. The fixed effects included in each regression are indicated by check marks in the appropriate columns and rows.

Table 6: Treatment effects estimated for periods without COVID-19, virtual testing.

	Full Sample		Pre-SY 2021		Omit SY 2021	
	I	II	III	IV	V	VI
	Math	Reading	Math	Reading	Math	Reading
Site Distance	-0.035***	-0.019**	-0.027**	-0.028*	-0.036***	-0.021***
	(0.010)	(0.007)	(0.013)	(0.015)	(0.009)	(0.008)
Site Distance^2	0.003*	0.001	0.007***	0.005*	0.002	0.001
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)
N	902,453	881,430	341,506	315,271	781,756	756,972

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

Notes. Standard errors are reported in parentheses. Clusters are defined as groups of students who share a common closest meal site, and standard errors are clustered at the nearest site ID accordingly. The outcome of each regression is a student's standardized fall formative assessment score. Effects are expressed in standard deviation units. Estimates come from the second stage of a 2SLS procedure where achievement is regressed on predicted distance, with predicted distance coming from a first stage regression of endogenous "true" distance on hypothetical distance had the student never moved. All regressions include student, year, and grade fixed effects. Headers indicate the school years (subsample) used to estimate the coefficients listed below.

Table 7: Analysis of non-random test score missingness.

	Full Sample		> 80% Testing Rate		> 85% Testing Rate		> 90% Testing Rate	
	Math	Reading	Math	Reading	Math	Reading	Math	Reading
Panel A: Score Missingness Indicator								
Site Distance	0.029***	0.042***	0.008***	0.012**	0.004**	0.004*	0.003	-0.001
	(0.010)	(0.014)	(0.003)	(0.006)	(0.002)	(0.002)	(0.003)	(0.004)
Site Distance^2	-0.004**	-0.003*	-0.001*	0.000	0.000	-0.000	0.000	0.001
	(0.002)	(0.002)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
N	1,309,997	1,309,997	767,369	703,825	508,992	440,514	156,458	123,888
Panel B: Test Score								
Site Distance	-0.035***	-0.019**	-0.037***	-0.025***	-0.039***	-0.015	-0.037**	-0.045**
	(0.010)	(0.007)	(0.010)	(0.009)	(0.015)	(0.012)	(0.017)	(0.021)
Site Distance^2	0.003*	0.001	0.003**	0.003***	0.005***	0.003**	0.003	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)
N	902,453	881,430	674,767	616,982	454,370	393,093	143,023	113,686

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

Notes. Standard errors are reported in parentheses. Clusters are defined as groups of students who share a common closest meal site, and standard errors are clustered at the nearest site ID accordingly. All regressions include student, year, and grade fixed effects. The outcome of each regression in Panel A is a an indicator for whether a student-year observation has a missing test score in the indicated subject. The outcome of each regression in Panel B is a student's standardized fall formative assessment score. Effects in Panel B are expressed in standard deviation units. Estimates presented in Panel A come from a reduced form regression of test score missingness directly on a student's true site distance. Estimates in Panel B come from the second stage of a 2SLS regression of achievement on predicted distance, where predicted distance comes from a first stage regression of endogenous "true" distance on hypothetical distance had the student never moved.

Table 8: Placebo regressions of achievement on future hypothetical (instrument) site distance.

	Math			Reading		
	I	II	III	IV	V	VI
Site Distance (Instrument)	-0.029***		-0.036***	-0.016***		-0.020***
	(0.008)		(0.007)	(0.006)		(0.006)
Site Distance (Instrument)^2	0.002		0.003***	0.001		0.002**
	(0.001)		(0.001)	(0.001)		(0.001)
Next Year Site Distance (Instrument)		-0.001	0.003		0.000	0.003
		(0.008)	(0.009)		(0.006)	(0.006)
Next Year Site Distance (Instrument)^2		0.004***	0.004***		0.002**	0.002**
		(0.001)	(0.001)		(0.001)	(0.001)
N	902,453	902,453	902,453	881,430	881,430	881,430

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

Notes. Standard errors are reported in parentheses. Clusters are defined as groups of students who share a common closest meal site, and standard errors are clustered at the nearest site ID accordingly. All regressions include student, year, and grade fixed effects. The outcome of each regression is a student's standardized fall formative assessment score. Effects are expressed in standard deviation units. Estimates come from a reduced form regression of the current year's test score on current year's hypothetical (instrument) site distance, next year's hypothetical (instrument) site distance, or both.