Summer 7-16-2021

Three Essays on Public Housing and Social Inequality

Jeehee Han
Syracuse University

Abstract

This dissertation consists of three essays that examine the impact of housing policies and the surrounding neighborhoods on socioeconomically disadvantaged populations living in public housing.

In the first chapter, titled "The Spillover Effects of Source of Income Anti-Discrimination Laws on Public Housing," I examine whether and to what extent source of income (SOI) anti-discrimination laws affect the sociodemographic composition of households living in public housing. SOI laws make it illegal for landlords to discriminate against the source of rent payment, including housing choice vouchers. Landlord discrimination is a major barrier to voucher utilization, disproportionately affecting extremely low-income families and racial minorities among all voucher holders. Thus, improvements in voucher utilization through SOI laws may affect the pool of applicants and recipients of public housing that operate within the same local public housing authority service areas. I use nationwide public housing authority level data and examine the changes in the composition of households living in public housing before and after SOI laws. I use a difference-in-difference approach, exploiting the variation in the precise timing that state and local jurisdictions enact SOI laws. I find that SOI laws significantly reduce the share of extremely poor households and minority residents in public housing, along with a decline in new entries to public housing. The results suggest potentially positive spillover effects of SOI laws, alleviating "concentration of poverty" in public housing as a consequence of a policy attempt to improve accessibilities to an alternative housing program.

The second chapter is titled "Are Public Housing Good for Kids After All?" and revisits the popular belief that public housing residency harms rather than helps children's development and academic achievement. Critics charge that public housing projects concentrate poverty and

create neighborhoods with limited opportunities, including low-quality schools. However, whether the net effect is positive or negative is theoretically ambiguous and likely to depend on the characteristics of the neighborhood and schools compared to origin neighborhoods. In this paper, I draw on detailed individual-level longitudinal data on students moving into New York City public housing and examine their standardized test scores over time. Exploiting plausibly random variation in the precise timing of entry into public housing, I estimate credibly causal effects of public housing using both difference-in-differences and event study designs. Further, I explore the role of schools by estimating the effects on school mobility and the quality of the school attended. I find credibly causal evidence of positive effects of moving into public housing, with larger effects over time. Stalled academic performance in the first year of entry reflects, in part, potentially disruptive effects of residential and school moves. I find neighborhood matters: impacts are larger for students moving *out* of low-income neighborhoods or *into* higher-income neighborhoods, and these students move to schools with higher average test scores and lower shares of economically disadvantaged peers.

The final chapter, titled “Does Proximity to Fast Food Cause Childhood Obesity? Evidence from Public Housing,” examines the causal link between local food environments and childhood obesity. Using individual-level longitudinal data on students living in New York City public housing linked to restaurant location data, I exploit the naturally occurring within-development variation in distance to fast food restaurants to estimate the impact of proximity on obesity. Since the assignment of households to specific buildings is based upon availability at the time of assignment to public housing, the distance between student residence and retail food outlets is plausibly random. The study results suggest that childhood obesity increases with proximity to fast food, with larger effects for younger children who attend neighborhood schools.

THREE ESSAYS ON PUBLIC HOUSING AND SOCIAL INEQUALITY

by

Jeehee Han

M.A., Columbia University, 2016

B.A., Northwestern University, 2014

DISSERTATION

Submitted in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Public Administration

Syracuse University

July 2021

Acknowledgements

Working towards my doctoral degree and my dissertation for the past five years has been an experience of growth both as a researcher and as a person. I learned the importance of having good people around me who can push me to find my strongest self and support me at my weakest moments. I dedicate this section to my mentors, colleagues, friends, and family, who made this dissertation possible by helping me grow as a researcher and also a better person.

First and foremost, I thank my advisor Amy Schwartz for being a firm supporter whenever most needed. I am also extremely grateful to my committee members, Michah Rothbart, Bob Bifulco, Johnny Yinger, and Ying Shi, for their invaluable guidance and support. It was truly a privilege to learn from and work closely with leading scholars of my field that I can look up to and wish to emulate. I thank Alfonso Flores-Lagunes for serving as my committee chair and teaching the courses that became the methodological ground of my dissertation.

At Syracuse University, the Center for Policy Research has been the research home to me. I thank Len Lopoo, Peggy Austin, Katrina Fiacchi, Emily Minnoe, Laura Walsh, and Candi Patterson for the making of this home (especially Katrina for helping me throughout the administrative procedures of my job search). I would walk into my grad bay every day to be greeted by Emily Gutierrez, Mattie Mackenzie-Liu, and Stephanie Coffey, whom I dearly missed while working from home during the pandemic. I also became friends with some wonderful colleagues at CPR – Laura Rodriguez, Yusun Kim, Saied Toossi, Iuliia Shybalkina, David Schwegman, Jay Kwon, Chris Rick, Joanna Bailey Rae, and Sam Trajkovski – who would regularly stop by our grad bay to share questions and insights about research. CPR also provided me the opportunity to work with exceptional faculty members, and I thank David Popp, Saba Siddiki, Kathy Michelmore, Sarah Hamersma, and Colleen Heflin for their support during my job search. I also thank other faculty members, administrative staff, and colleagues at the Maxwell School, including Julia Carboni, Kristi Vega, Ken Dwyer, Suyeon Jo, and Nara Yoon. Finally, thank you, Hannah Patnaik and Raghav Puri, for all the fun journeys we had and for providing me a safe space for my sanity, and Ziqiao Chen and Yoon-Jung Choi for staying with me at my best and worst times yet always accepting me as who I am.

People I met outside Syracuse University have also been a tremendous support to my dissertation. I thank the group at New York University, including Meryle Weinstein, Zac McDermott, Jeremy Sze, and Brian Elbel, for data and support. I also thank Jeff Zabel, Rob Collinson, David Mundel, and other conference discussants and anonymous referees for providing invaluable feedback for what became my dissertation chapters.

Last but not least, words can never explain my love and gratitude for my family in Korea, who supported me every step of the way. I am always happy to be your pride and joy.

Table of Contents

List of Tables

List of Figures

Essay I

The Spillover Effects of Source of Income Anti-Discrimination Laws on Public Housing

1. Introduction

Public housing was the federal government's first major housing assistance program for low-income families. Despite its intention to assist low-income households, public housing has long been criticized for creating "concentrations of poverty" and placing its residents and children in neighborhoods with limited opportunities (Massey & Kanaiaupuni, 1993). As a result, federal housing policies shifted away from public housing to alternative housing assistance programs (Collinson et al., 2015). Housing choice vouchers are among the preferred alternative forms of housing assistance, as they provide opportunities for the housing-subsidy recipients to find rental units in the private market, not limited to those located in high-poverty neighborhoods. Voucher recipients, however, often face difficulties finding rental units before their vouchers expire, partly due to high search costs and landlord discriminations. In response to address the low utilization rates of housing choice vouchers, a set of related statutory requirements has been enacted at the state and local levels. These include housing counseling programs, statutes against the source of income discrimination, and small area fair market rents.

While most policy efforts have focused on improving low-income households' utilization of housing choice vouchers, little attention has been paid to their spillover effects on public housing. However, potential recipients of vouchers and public housing are likely to overlap within a given local jurisdiction. Statutes that aim to improve the utilization of vouchers may not only change the type of households that successfully lease up using vouchers but also the type of households that could have been in public housing. Do the statutes help the neediest households utilize vouchers and transition out from the limited choice of public housing? Despite its decline, public housing continues to serve more than two million low-income populations nationwide; thus, it is important to understand the consequences and implications of the emerging state and local policies that limitedly focus on other housing assistance programs. This paper examines how the sociodemographic composition of the households living in public housing change before and after local jurisdictions and states pass the source of income (SOI) anti-discrimination laws.

SOI laws make it illegal for landlords to discriminate against tenants' source of income to pay rent, including housing vouchers, and are advocated by fair housing groups to lower the barriers of voucher utilization in the private housing market. Landlord discrimination is a major barrier to voucher utilization, disproportionately affecting extremely poor families and racial minorities among all voucher holders (Finkel & Buron, 2001). While an abundance of studies documents the role of SOI laws on improving voucher utilization rates, no studies to date have examined the impact of SOI laws on public housing.

I use nationwide public housing authority (PHA) level data, in years 2009-2018, to examine the share of low-income households (50% below the local area median income (AMI)), extremely low-income households (30% below the local AMI), minority households, and female-headed households with children in public housing. I use a difference-in-difference (DD) approach, exploiting the variation in SOI law enactment years by PHA, and examine the changes in the composition of households living in public housing before and after SOI laws.

To preview results, I find statistically significant reductions in the share of socioeconomically disadvantaged households in public housing for PHAs with SOI laws. More specifically, I find up to a 4.51 percentage point decrease in the share of extremely low-income households and a 1.9 percentage point decrease for low-income households in public housing after passing SOI laws. I also find some evidence of a decrease in the share of minority households, primarily driven by the decrease in black households. These changes are accompanied by a relative decline in new entries into public housing (by 2.31 percentage points) and an increase in the share of low-income, black households using vouchers. The results suggest positive spillover effects of SOI laws, alleviating concentrations of poverty in public housing.

This paper is organized as follows. I first explain the differences between public housing and housing choice vouchers. One difference is the barriers that voucher holders face when searching for housing units in the private market. I explain how SOI laws may address such difficulties, followed by existing empirical evidence on the effects of the laws on voucher utilization rates and locational outcomes. In Section 3, I explain the conceptual framework and hypotheses on how SOI laws may affect households in public housing, along with this paper's contribution to the existing literature. Then, I present the PHA-level data, measures, and the analytic sample in Section 4. In Section 5, I provide the estimating equations for the empirical strategy. I present my results and a series of robustness checks in Section 6 and conclude with discussion and policy implications in Section 7.

2. Background

2.1. Public Housing, Housing Choice Vouchers, and SOI Laws

The Housing Act of 1937 first established local public housing authorities (PHAs) to develop public housing projects with the goal of providing "decent, safe, and sanitary dwelling for families of low-income" (p.888). Public housing projects mostly consisted of one or more concentrated blocks of standardized high-rise and sometimes low-rise apartment buildings (Von Hoffman, 1996). Tenants in public housing projects would typically pay 30% of their income towards rent, with some variation across local PHAs, which are well below the market rate. For cost-saving purposes, city governments commonly built projects in areas already occupied by poor, minority residents (Schill & Wachter, 1995). For example, out of 33 projects constructed in Chicago between 1950 and 1970, all but one project was built in neighborhoods that were at least 85% black (Hirsch, 1983). As a place-based program, public housing offers rent subsidies for units in the public housing projects; in other words, public housing recipients cannot choose rental units outside the projects. The program recipients, based on the eligibility criteria, also come from a pool of low-income households that are also predominantly racial minorities. The design of project-based public housing, as a result, has been often criticized for creating neighborhoods of "concentrated poverty," or isolated geographic areas that are disproportionately poor and racially black.

In the landmark case of Gautreaux in 1976, the Supreme Court ruled that the Chicago Housing Authority (CHA) and the U.S. Department of Housing and Urban Development (HUD) discriminated against black tenants by concentrating them in large-scale developments located in poor, black neighborhoods. As a result, more than 250,000 public housing units were demolished across the nation, including the projects in Chicago. The Housing and Community Development Act of 1974 was passed to create the housing voucher program as an alternative to the traditional projects. Housing voucher recipients also typically pay 30% of their income towards rent like public housing tenants, and the federal government would subsidize the difference between the payment and the rent. The main difference between the two programs is that the housing vouchers are tenant-based, subsidizing the rental units in the private market chosen by the tenants rather than limiting the subsidized units to those in the projects. The housing voucher program was often preferred by policymakers as the means to alleviate the concerns around public housing's concentration of poverty.[1]

---

[1] For example, families that had to move out of demolished CHA public housing were provided with vouchers to move to other low-poverty neighborhoods. Later in the 1990s, HUD's Moving to Opportunity experiment provided

Voucher recipients, however, are not guaranteed to successfully lease up in the private housing market. The two often-discussed barriers of voucher utilization are high search costs and landlord discrimination (Weicher, 1990). First, low-income households may not have the time and resources to find eligible housing units in the private market that meet both program requirements and personal preferences (DeLuca et al., 2013). While rent cannot exceed the maximum payment standard set by local PHAs, the rental units should also satisfy the basic housing quality standards set by the federal government (HUD, 2020).[2] Voucher recipients typically have 60 days to find housing with some variation by local PHAs. A second barrier is landlord reluctance to accept vouchers. This may relate to landlords' perception of administrative burdens, assuming PHAs are slow and bureaucratic and not wanting to comply with inspections on the housing quality standards. Another reason behind landlord reluctance may be due to viewing voucher holders as undesirable tenants. Landlord refusal to rent to voucher recipients may also mask racial discrimination, as voucher programs are disproportionately non-white compared to the broader population of households in rental units (Galvez, 2011).

Due to high search costs and landlord discrimination, voucher recipients may fail to use vouchers before they expire. Extremely poor families and minority households may be disproportionately affected by these barriers, as they may face more discrimination in the private housing market (Finkel & Buron, 2001). Although only descriptive, an early evaluation shows that almost half (48 percent) of minorities failed to find housing using vouchers, which is a significantly higher failure rate compared to 28 percent of white voucher households failing to

---

housing vouchers to low-income households for the purpose of examining whether moving into low-poverty neighborhoods improved household outcomes. Descriptive studies show that voucher recipients are less likely to live in high-poverty neighborhoods than general low-income population and households in public housing (Newman & Schnare, 1997; Pendall, 2000; Turner, 1998).

[2] If rent exceeds the maximum payment standard, tenants have to pay the difference in addition to 30% of their adjusted income, and some PHAs only allow the rent to exceed the maximum payment standard for a limited amount of time.

use vouchers (President's Commission on Housing, 1982). Another consequence is voucher recipients leasing up in similarly poor-quality neighborhoods instead of moving to lower-poverty neighborhoods (Jacob, 2004).

Since 1971, an increasing number of states and localities have passed SOI laws to address discrimination problems, reaching 80 local jurisdictions and 12 states in 2019 (see Figure 1). These laws make it illegal for landlords to discriminate against voucher recipients solely based on the source of income, including housing choice vouchers and other welfare assistance. Many advocates and fair housing groups have been pushing for the passage of SOI laws as a policy response to address the discrimination against voucher holders (Macdonnell & Kahn, 2005). In the following sections, I provide existing evidence on whether SOI laws effectively address these two barriers and how the suggested impact may change the composition of households living in public housing.

2.2. Literature Review: Empirical Evidence on the Impact of SOI Laws

A body of previous empirical studies focuses on evaluating the effects of SOI laws on (1) voucher utilization rates and (2) the locational outcomes of voucher recipients. Finkle and Buron (2001) are the earliest to examine the effects of SOI laws on voucher utilization rates. They use data from telephone interviews of 2,609 voucher holders across 48 PHAs in 2000. They find that voucher utilization rates are 12 percentage points higher in SOI jurisdictions. A more recent study by Freeman (2012) use voucher data from the HUD administrative data (HUD-50058) in 1995-2008 and use a difference-in-differences approach to compare PHAs in jurisdictions with SOI laws and those in adjacent jurisdictions. He finds that voucher utilization rates are 5 to 12 percentage points higher in SOI jurisdictions.

Another set of studies examines the locational outcomes of voucher holders as a result of enacting SOI laws. A cross-sectional study by Galvez (2011) uses restricted-use HUD data and finds that neighborhood poverty rate is 1 percentage point lower for voucher holders in metropolitan statistical areas with SOI laws. Freeman and Li (2014), using HUD administrative data (HUD-50058) in 1995-2008 and a difference-in-difference approach, find that neighborhood poverty rate is 3 percentage points lower for voucher holders in SOI jurisdictions. To summarize, previous studies find that voucher recipients are less likely to fail at finding landlords that accept vouchers and more likely to move into low-poverty neighborhoods in the presence of SOI laws.

While previous studies – naturally – focus on voucher outcomes, no studies to date discuss the impact of SOI laws on public housing. It is, however, critical to understand whether recent policy attention on the housing voucher program is creating any blind spots for public housing. Not all eligible households can receive housing vouchers, and despite improved utilization through SOI laws, not all voucher recipients end up successfully leasing up in the private market. Public housing remains an important stream of housing assistance for needy families, currently serving more than 2 million low-income individuals across the nation. This paper examines whether SOI laws have any unintended consequences on public housing. In the next section, I discuss how SOI laws might affect the type of households that sort into public housing due to the changes in voucher utilization rates and locational outcomes within the same PHA service area.

3. Conceptual Framework: How Might SOI Laws Affect Public Housing?

This section outlines the two sets of hypotheses on how SOI laws may change the type of households that enter or exit public housing based. Public housing and housing choice voucher programs are federally funded housing assistance programs that are administered by local public housing authorities (PHAs).[3] For example, CHA would receive federal funds to manage and administer public housing projects located in Chicago and also administer housing choice vouchers for households in Chicago. Each PHA has its own waiting list, if any, and a systemized tenant selection process for each program. PHAs in large cities typically have long waiting lists that easily surpass years of waiting time and sometimes temporarily close the application to control the volume of applicants. Other PHAs may have substantially shorter waiting lists. As long as the application is open, eligible households can apply for both programs at the same time and even apply for one program while receiving the other type of housing assistance.

Figure 2 depicts a household's application and admission process for public housing and housing choice vouchers. In this process, vouchers are different from public housing in that reaching the top of the waiting list does not guarantee admission into the program. If voucher holders are unable to find landlords willing to accept their vouchers within a given time frame, they may lose their vouchers before they ever use vouchers to pay the rent.[4] Based on the application and admission processes for both programs, I describe how passing SOI laws might affect households seeking housing assistance in a given PHA service area.

Switching from public housing to vouchers

First, SOI laws may reduce the share of socioeconomically disadvantaged households in public housing by improving voucher utilization rates. Racial minorities and extremely low-income households are more likely to face landlord discrimination in the absence of anti-discrimination laws and may become less likely to "fail to lease up" using vouchers *with SOI laws* (see Figure 2). In the short run, the pool of disadvantaged households affected by SOI laws

[3] As mentioned above, tenants for both programs typically pay 30% of their adjusted household income towards rent and the federal government subsidizes the difference between the rent and the payment.
[4] As mentioned above, households typically have 60 days to find a unit.

is most likely to come from households that had already applied for the housing choice vouchers. These households may include those waiting for both programs – vouchers and public housing – and those already living in public housing but had been waiting for vouchers. As a result, improved voucher utilization among particularly disadvantaged households may reduce their likelihood of staying on the waiting list for public housing or continue living in public housing, thus leading to the first hypothesis:

> **H1:** SOI laws increase voucher utilization rates for disadvantaged households and reduce the share of disadvantaged households in public housing.

However, as an opposing hypothesis, SOI laws may not improve voucher utilization rates for racial minorities or extremely low-income households. Furthermore, SOI laws may work in favor of relatively better-off voucher holders with market readiness. Relatively better-off households with incomes right below the eligibility threshold may have the ability to take advantage of SOI laws and become more successful leasing up using vouchers in the private housing market. In these cases, we may see null effects or increases in the share of disadvantaged households in public housing as a result of SOI laws.

Crowded out of vouchers into public housing

In the longer run, SOI laws may increase competition for vouchers and crowd out racial minorities and extremely low-income households to public housing. SOI laws are expected to improve voucher utilization overall and utilization in "better" neighborhoods. This aspect of SOI laws may congest the waiting list in two ways. First, improved voucher utilization may paradoxically lengthen the waiting time for vouchers, as fewer vouchers expire and fewer households newly receive vouchers. Relatively better-off applicants may have higher tolerance for longer wait times, as they can afford to stay on the waiting list without having to resort to other forms of housing assistance. Second, improved locational outcomes may increase these better-off households' willingness to wait. The share of relatively better-off applicants "placed on the waiting list" for vouchers (in Figure 2) may increase as a result of SOI laws. On the contrary, worse-off households' perceived value of vouchers is less likely to depend upon the potential locational outcomes of vouchers. Instead, they are more likely to stay in public housing or take up the offer of public housing units instead of taking the chance to wait longer for vouchers. As a result, socioeconomically disadvantaged households are more likely to be crowded out from vouchers due to increased competition. This leads to the second hypothesis on the longer-term effects of SOI laws:

> **H2:** SOI laws increase competition for vouchers, crowd out households with lower tolerance for the wait time, and increase the share of disadvantaged households in public housing.

Whether the hypothesized long-term effect exists and how long it would take for the effect to appear are both empirical questions. If the waiting lists are long, it may take more time for the overall household composition of public housing to change. The hypothesized effects may appear faster in PHAs with shorter waiting lists. In later sections, I explain how I stratify the sample into PHAs with short and long waiting lists to explore heterogeneity in the timing of the effects and employ an event study approach to examine possible dosage effects of SOI laws over time. Of course, households may not adjust their waiting behavior in response to SOI laws. As an opposing hypothesis for H2, there may be null effects of SOI laws on the share of disadvantaged households in public housing, at least through the mechanism of increased competition for vouchers.

4. Data Sources, Measures, and Sample

I use longitudinal PHA-level data from three administrative data sources in years 2009-2018. First, I use annual PHA- and program-level data from HUD Picture of Subsidized Households. These data include the household compositions for public housing in each PHA, such as the percent of households that are *poor* (income lower than 50% of area median income (AMI)), *extremely poor* (income lower than 30% of AMI), *black*, racial *minority*, and *female-headed with children* in public housing. I use these five measures, respectively, as the outcome variable. I have a set of identical variables for households on housing choice vouchers. Other annual PHA-level data include the total number of public housing units available, the percent of public housing units *occupied*, and the percent of households that newly entered public housing (*move-in*). The annual average waiting time (in months) for public housing is also reported but only for households that newly move into public housing in a given year; therefore, years with no new public housing entries are missing average waiting time. I instead calculate the aggregate average waiting time for each PHA (thus not varying across years).[5]

I link PHA-level data with local jurisdiction- and state-level SOI law status based on the geographical boundaries of PHA service areas. The Poverty and Race Research Action Council database (2020) contains each jurisdiction and state's statute against SOI discrimination, their enactment year, and whether an enforcement agency is identified by the state or the local jurisdiction. PHA service areas are not always coterminous with jurisdictions that enact SOI laws.[6] In this paper, I spatially match the estimated PHA service areas from the HUD Office of

---

[5] For example, a PHA with an average waiting time of 12 months for every year between 2009 and 2018 would have an average waiting time of 12 months, and a PHA with an average waiting time of 24 months but for every other year between 2009 and 2018 (thus missing five years of average waiting time data) would have an average waiting time of 24 months instead of 12 months.

[6] Among previous studies that examine the impact of SOI laws on voucher outcomes, no studies to my knowledge specify how treatment of SOI laws were identified at the PHA level.

Policy Development and Research with state and jurisdiction boundaries to identify PHAs with SOI laws. I consider a PHA to be affected by SOI laws if its service area either: 1) exactly match the state or locality that enacted SOI laws, 2) is nested within the state or locality that enacted SOI law, or 3) include smaller localities that enacted SOI law.

This study focuses on 4,020 observations of 402 "Ever SOI" PHAs that were continuously operating their public housing program in 2009-2018. I restrict the sample to include PHAs that were ever affected by SOI laws up to date. In our sample, 307 PHAs had SOI laws before the sample period ("Always SOI"), 48 PHAs cover states or jurisdictions that enact SOI laws between 2009 and 2018 ("Sometimes SOI"), and 47 PHAs are considered future recipients of SOI laws ("Future SOI") that eventually pass SOI laws but after the sample period. While jurisdictions that enact SOI laws may be systematically different from those that do not, it is plausible that the precise timing of enactment is exogenous among ever SOI PHAs. Table 1 provides the variable means for Ever SOI and Never SOI PHAs in 2009. Baseline characteristics suggest that Ever SOI PHAs, on average, provide larger numbers of public housing units.

5. Empirical strategy

5.1. Testing pre-trends

I exploit staggered enactment of SOI laws over time to estimate the effect of SOI on public housing composition based on the assumption that the precise timing of SOI enactment year is plausibly random. The event study results in Figures 4-6 show the coefficients for this study's main outcome variables by years since PHA's first exposure to SOI laws, using PHA fixed effects and year fixed effects. The event study results clearly show that PHAs that enact SOI laws do not have significant trends in the composition of households in public housing prior to passing SOI laws. This confirms that household characteristics of public housing in pre-SOI years are determined unrelated to the SOI enactment decisions.

5.2. Estimating equations

I use a difference-in-differences approach based on the assumption that the precise timing of SOI enactment year is plausibly random. I identify the effects by comparing PHAs across time, as well as comparing early adopters to late adopters. This paper's baseline model contains the following elements:

$$Y_{it} = \beta_0 + \beta_1 \mathit{SOI}_{it} + \gamma X_{it} + \delta_i + \tau_t + \varepsilon_{it} \tag{1}$$

where Y_{it} represents the set of sociodemographic characteristics of the households living in public housing in PHA i in year t. I also examine the share of *occupied* units and the share of new *move-in* units to explore the possibilities of whether the estimated effects driven by households exiting public housing to receive vouchers or changes in the inflow of new applicants. SOI_{it} captures the SOI law enactment status. A vector of time-varying PHA characteristics is included in the equation as X_{it}. PHA fixed effects, δ_i, control for underlying PHA characteristics, and year fixed effects, τ_t, control for secular trends.

I also examine potential dosage effects, using an event study specification:

$$Y_{it} = \beta_0 + \beta\, \mathit{SOI\ Year}_{it} + \gamma X_{it} + \delta_i + \tau_t + \varepsilon_{it} \tag{2}$$

where $\mathit{SOI\ Year}_{it}$ captures a vector of the years since a PHA's first exposure to SOI laws. The event study analyses can shed light on whether the long-term effects of SOI laws, discussed in the second hypothesis (H2), change the trends observed in the shorter term.

I also interact the average wait time for public housing with SOI_{it} in model (1) and $\mathit{SOI\ Year}_{it}$ in model (2) to explore whether the length of the waiting list moderates the timing that the longer-term effects appear. Finally, I also examine the household composition outcomes for the housing choice voucher program to further shed light on mechanisms. The estimated impact of SOI laws on the households in public housing can be explained in relation to the changes in the households that successfully utilize vouchers after the enactment of SOI laws.

6. Results

Baseline results in Table 2 show the estimated impact of SOI laws on the sociodemographic composition of public housing households as well as the percent of occupied units and the percent of households that moved into public housing. The coefficients in columns 1 and 2 suggest statistically significant declines in the share of poor and extremely poor households by 1.44 and 2.83 percentage points after the enactment of SOI laws. The statistically significant reduction in the share of new households (see column 7) may explain the decline in the share of poor and extremely poor households in public housing. In other words, fewer economically disadvantaged households are entering public housing after the passage of SOI laws. Appendix Figure A.3. provides a descriptive trend in the increasing share of new public housing households in PHAs that ever enact SOI laws; however, increasing trends are steeper for Future SOI and Always SOI groups. These suggest the mechanism behind the decline in the share of poor and extremely poor households for Sometimes SOI group can be potentially explained through the relative decline in entries to public housing post-SOI laws.

In Table 3, we examine whether the effects of SOI laws grow or diminish over time. The estimates in columns 1 and 2 suggest that, except for the initial year, there is a general trend of the negative estimates growing over time. The decline in the percent of poor and extremely poor households ranges up to 1.9 and 4.51 percentage points in the fifth year. While not statistically significant at the conventional level, we see immediate positive changes in the shares of black households and female-headed households with children, but their shares decline after the second year (starting from SOI 3). It is important to note that I hypothesize in the longer term (see H2), socioeconomically disadvantaged households might be crowded out from the competition for vouchers, which would increase their share in public housing, as opposed to H1. The estimates suggest that the hypothesized longer-term effects do not dominate the hypothesized effects of H1 even in later years. In other words, socioeconomically disadvantaged households appear to be taking advantage of SOI laws to opt out of public housing without any evidence for being crowded out in the competition for vouchers.

I estimate whether the effects are different by PHA's length of the waiting list for public housing. Results in Table 4 columns 1 and 2 suggest that the direction and the statistical significance of the estimated impact of SOI laws on the share of poor and extremely poor households are consistent with those in Table 3; the impact does not vary by the PHA's average waiting time for public housing. The estimates in column 3, however, suggest that SOI laws increase the share of black households in public housing but are moderated by the waiting time. For example, PHAs with no waiting time would experience an increase in the share of black households in public housing by almost 2.3 percentage points; yet, PHAs with an average waiting time of more than 24 months would see no effects in the change in black households. Similarly, PHAs with longer waiting times would experience declines in black households. It is likely that we observe only the short-term effects of SOI laws (or the hypothesized effects of H1) for PHAs with longer waiting times. In other words, it takes a shorter time for the long-term effects to appear in PHAs with short waiting lists. This suggests that black households are – similar to poor households, extremely poor households – more likely to switch from public housing to vouchers as a result of SOI laws but are also likely to be crowded out from the competition for vouchers.

This trend is more prominent in Table 5. The decline in the share of black households in public housing is only evident in PHAs with longer wait times. However, the share of black households significantly increases over time for PHAs with shorter waiting times, suggesting black households are being crowded out from the competition for vouchers and are more likely sorting into public housing as a result of SOI laws. Estimates in columns 1 and 2 show that the waiting time mostly does not moderate the effects of SOI laws for poor and extremely poor households in public housing.

Results for voucher households are shown in Tables 6 and 7. The estimates suggest that the share of poor and black households increase after the enactment of SOI laws. Event study results further suggest that that the positive effects grow over time for the share of poor, extremely poor, black, and minority households. In other words, these results support the hypothesized mechanism of H1 that socioeconomically disadvantaged households are more likely to utilize vouchers in PHAs with SOI laws and rule out H2 that they are crowded out from the potential increase in competitions for vouchers.

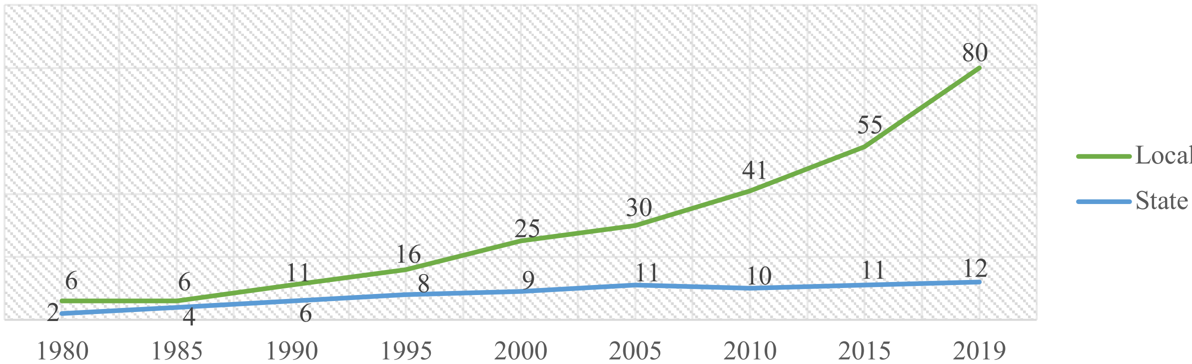
7. Discussions

Fair housing advocates suggest that anti-discrimination laws would help low-income families find housing in the private market, including households that use housing choice vouchers to pay rent. A recent body of research has provided evidence that voucher holders are more likely to successfully find housing – and also find housing in low-poverty neighborhoods – after the enactment of SOI laws. However, little is known about the spillover effects of SOI laws on alternative housing assistance programs, such as public housing, that are operated by the same local PHAs. Enactment of SOI laws and the subsequent effects on vouchers may change the pool of households in need of public housing in a given locality, changing the sociodemographic composition of households living in public housing. This paper aims to provide credibly causal estimates of the effect of SOI laws on the composition of households living in public housing and shed light on the potential mechanisms.

I find that SOI laws significantly reduce the share of disadvantaged households in public housing. Specifically, the shares of poor and extremely poor households in public housing reduce up to 1.9 and 4.51 percentage points. These reductions in disadvantaged households are coupled with the increase in poor, extremely poor, and minority households in vouchers, as well as the reduction in the share of new entries to public housing, suggesting that SOI laws help socioeconomically disadvantaged households' transition out from the limited choice of public housing. This paper provides evidence that will likely assuage critic's worries, demonstrating that SOI laws help socioeconomically disadvantaged households benefit from vouchers without leaving public housing behind in exacerbated concentrations of poverty.

Tables and Figures

Figure 1. The number of state and local jurisdictions with SOI laws over time, 1971-2019



Notes: Data from Poverty & Race Research Action Council (2020).

Figure 2. Application process for public housing and housing choice vouchers

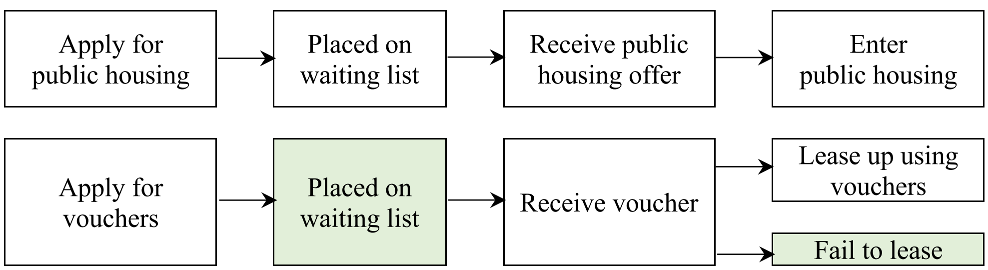


Figure 3. PHA service areas by SOI enactment status, 2009-2019

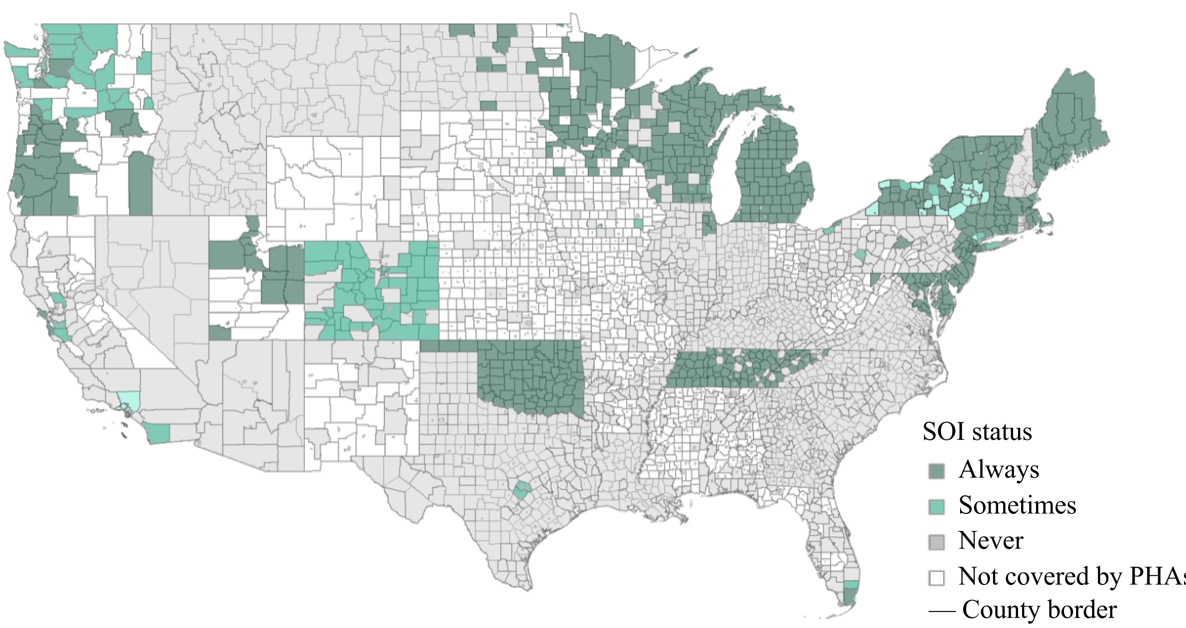


Table 1. Summary statistics – variable means by SOI enactment status

	Never SOI (1)	Total (2)	Always (3)	Sometimes (4)	Future (5)
Total units	274.02 (498.69)	580.3 (1357.76)	524.73 (1318.49)	1007.99 (1753.49)	516.92 (1056.30)
Total people	548.89 (1006.86)	1097.65 (2705.86)	966.01 (2534.94)	1919.53 (3126.82)	1134.82 (3148.32)
% poor	87.90 (8.74)	89.41 (8.15)	89.06 (8.73)	92.07 (5.00)	89.04 (6.05)
% extremely poor	63.41 (14.39)	66.82 (14.39)	66.42 (15.06)	72.40 (9.98)	63.95 (12.08)
% black	32.30 (35.10)	34.77 (36.32)	21.34 (26.46)	18.78 (24.63)	37.88 (33.46)
% minority	45.41 (36.50)	40.24 (32.21)	36.95 (30.82)	64.70 (30.15)	37.26 (32.63)
% female head with children	36.67 (17.05)	29.09 (16.51)	27.90 (16.53)	37.47 (15.56)	28.55 (14.86)
% occupied	94.91 (6.24)	95.32 (5.78)	95.15 (6.16)	95.46 (5.03)	96.25 (3.33)
% move in	17.23 (8.57)	13.64 (9.01)	14.56 (9.67)	9.71 (5.20)	11.65 (5.62)
Average wait time (months)	13.10 (27.27)	11.91 (29.07)	18.40 (16.08)	17.69 (16.18)	25.61 (15.59)
N	17,870	4,020	3,070	470	480

Figure 4. Event study results for % poor (left) and % extremely poor (right) households

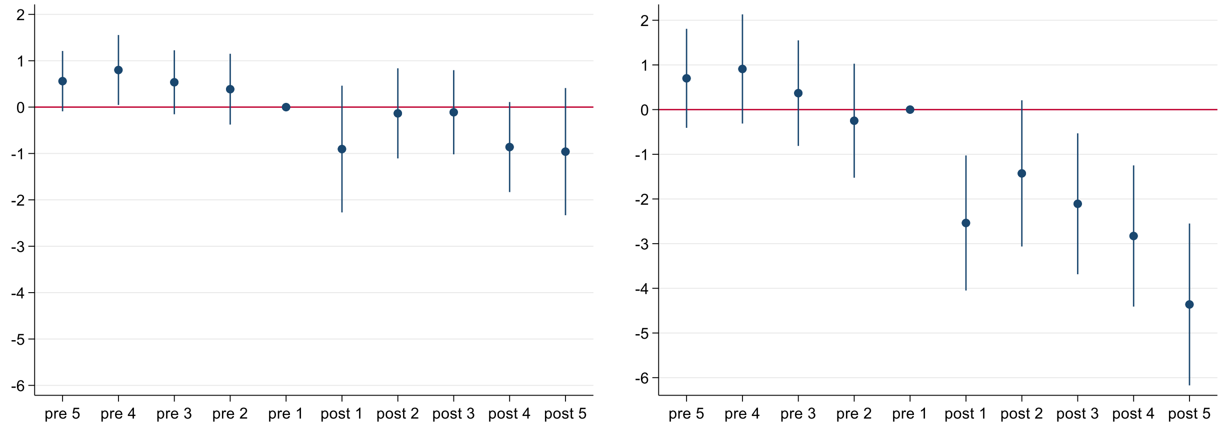


Figure 5. Event study results for % black (left) and % minority (right) households

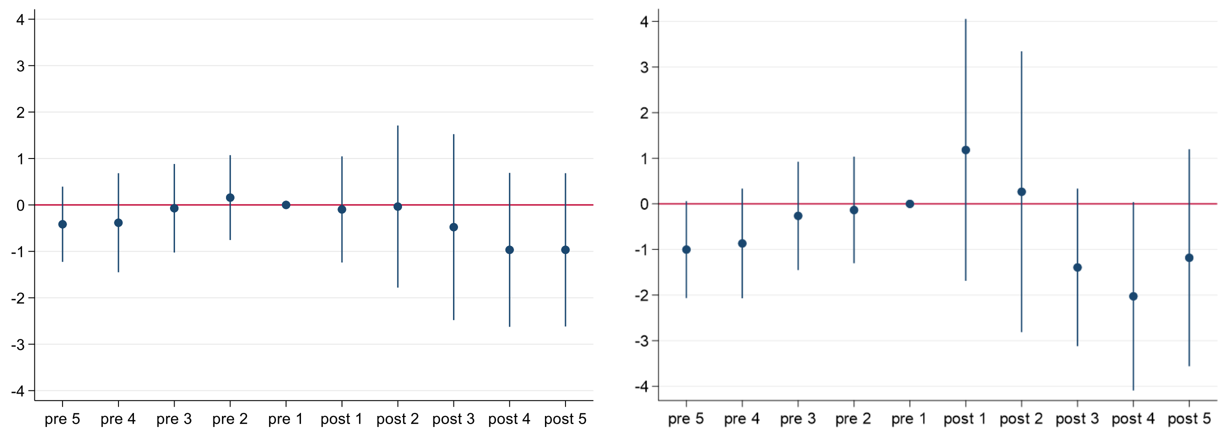


Figure 6. Event study results for % occupied units (left) and % new move-ins (right)

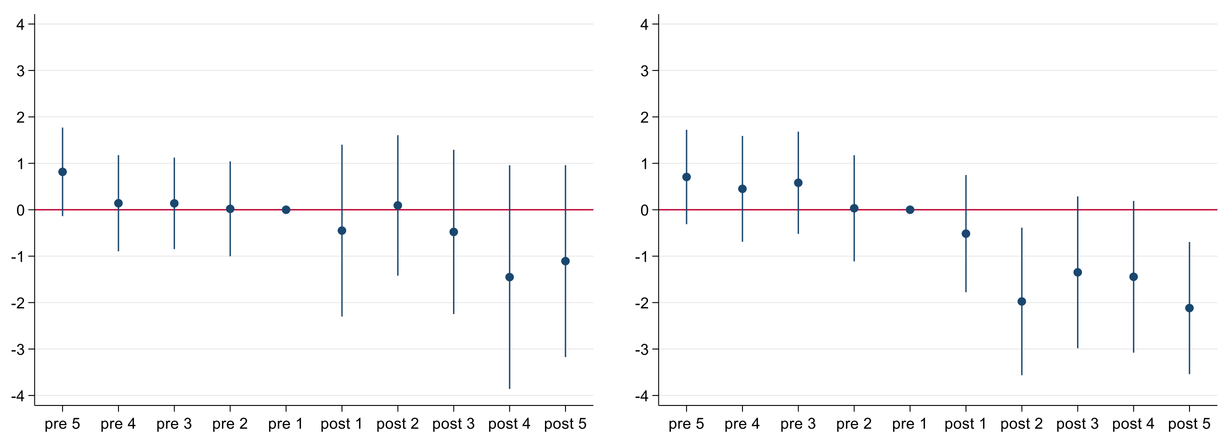


Table 2. Baseline regression results for public housing households

Dependent variable (%):	poor	extremely poor	black	minority	female head with child	occupied	move in
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SOI	-1.443*** (0.401)	-2.827*** (0.515)	-0.242 (0.508)	-0.430 (0.369)	0.372 (0.922)	-0.726 (0.586)	-1.507*** (0.411)
PHA FX	Y	Y	Y	Y	Y	Y	Y
Year FX	Y	Y	Y	Y	Y	Y	Y
Constant	91.125*** (0.353)	68.912*** (0.484)	21.399*** (0.440)	38.393*** (0.359)	29.502*** (0.739)	94.224*** (0.512)	12.679*** (0.384)
R2	0.801	0.869	0.986	0.987	0.944	0.525	0.676
N observations	4,020	4,020	4,020	4,020	4,020	4,020	4,020

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 3. Event study results for public housing households

Dependent variable (%):	poor	extremely poor	black	minority	female head with child	occupied	move in
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SOI 1	-1.697*** (0.614)	-2.885*** (0.695)	0.059 (0.550)	-0.409 (0.455)	1.655 (1.406)	-0.718 (0.882)	-0.876 (0.543)
SOI 2	-0.753* (0.394)	-1.716** (0.763)	0.071 (0.868)	-0.306 (0.481)	0.642 (1.489)	-0.066 (0.692)	-2.282*** (0.727)
SOI 3	-0.862** (0.392)	-2.361*** (0.730)	-0.401 (0.989)	-0.797 (0.576)	-1.072 (0.778)	-0.572 (0.824)	-1.617** (0.740)
SOI 4	-1.726*** (0.457)	-3.036*** (0.741)	-0.914 (0.816)	-0.302 (0.730)	-1.752* (0.960)	-1.526 (1.179)	-1.693** (0.749)
SOI 5+	-1.904*** (0.525)	-4.512*** (0.870)	-0.947 (0.811)	-0.403 (0.598)	-0.974 (1.127)	-1.123 (1.006)	-2.312*** (0.643)
PHA FX	Y	Y	Y	Y	Y	Y	Y
Year FX	Y	Y	Y	Y	Y	Y	Y
Constant	91.471*** (0.420)	70.161*** (0.699)	21.927*** (0.637)	38.371*** (0.489)	30.515*** (0.874)	94.528*** (0.790)	13.270*** (0.521)
R2	0.801	0.870	0.986	0.987	0.944	0.525	0.676
N observations	4,020	4,020	4,020	4,020	4,020	4,020	4,020

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 4. Regression results interacted with average waiting time for public housing households

Dependent variable (%):	poor	extremely poor	black	minority	female head with child	occupied	move in
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SOI	-2.063***	-3.900***	2.308*	-0.281	0.622	0.589	0.624
	(0.765)	(1.124)	(1.184)	(0.660)	(1.242)	(1.078)	(0.758)
SOI x Wait time	0.023	0.040	-0.096**	-0.006	-0.009	-0.049*	-0.080***
	(0.020)	(0.032)	(0.039)	(0.016)	(0.030)	(0.026)	(0.022)
PHA FX	Y	Y	Y	Y	Y	Y	Y
Year FX	Y	Y	Y	Y	Y	Y	Y
Constant	91.283***	69.186***	20.748***	38.355***	29.438***	93.888***	12.134***
	(0.412)	(0.581)	(0.529)	(0.405)	(0.773)	(0.605)	(0.426)
R2	0.801	0.869	0.986	0.987	0.944	0.525	0.676
N observations	4,020	4,020	4,020	4,020	4,020	4,020	4,020

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 5. Event study results interacted with average waiting time for public housing households

Dependent variable (%):	poor	extremely poor	black	minority	female head with child	occupied	move in
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SOI 1	-2.675***	-4.207***	1.101	-0.742	1.413	0.136	0.494
	(1.007)	(1.355)	(1.018)	(0.773)	(1.799)	(1.382)	(0.928)
SOI 1 x Wait	0.038*	0.051	-0.042	0.013	0.010	-0.034	-0.054**
	(0.020)	(0.036)	(0.029)	(0.017)	(0.037)	(0.027)	(0.023)
SOI 2	-1.690**	-2.757	4.126**	-0.206	-0.787	1.926	0.201
	(0.672)	(1.704)	(1.904)	(0.902)	(2.051)	(1.437)	(1.410)
SOI 2 x Wait	0.034	0.038	-0.146***	-0.004	0.050	-0.072*	-0.090**
	(0.021)	(0.053)	(0.046)	(0.023)	(0.069)	(0.037)	(0.036)
SOI 3	-0.443	-2.687	3.517*	0.491	-1.991	-0.380	-0.658
	(0.795)	(1.794)	(1.985)	(1.410)	(1.850)	(3.209)	(1.602)
SOI 3 x Wait	-0.014	0.013	-0.144***	-0.016	0.021	-0.074	-0.108***
	(0.026)	(0.059)	(0.046)	(0.028)	(0.041)	(0.045)	(0.031)
SOI 4	-1.067	-2.961*	2.841*	0.491	-1.991	-0.380	-0.658
	(0.767)	(1.537)	(1.692)	(1.410)	(1.850)	(3.209)	(1.602)
SOI 4 x Wait	-0.020	0.003	-0.137***	-0.029	0.004	-0.044	-0.044
	(0.024)	(0.046)	(0.046)	(0.033)	(0.054)	(0.084)	(0.039)
SOI 5+	-2.704**	-7.413***	4.752**	1.514	2.932	1.151	2.497
	(1.051)	(2.044)	(1.971)	(1.237)	(2.782)	(2.203)	(1.567)
SOI 5+ x Wait	0.026	0.093*	-0.191***	-0.061**	-0.120*	-0.077	-0.158***
	(0.025)	(0.052)	(0.050)	(0.028)	(0.071)	(0.052)	(0.040)
PHA FX	Y	Y	Y	Y	Y	Y	Y
Year FX	Y	Y	Y	Y	Y	Y	Y
Constant	91.736***	71.121***	20.149***	37.725***	29.137***	93.829***	11.719***
	(0.544)	(0.986)	(0.923)	(0.650)	(1.304)	(1.095)	(0.751)
R2	0.801	0.870	0.986	0.987	0.945	0.526	0.677
N observations	4,020	4,020	4,020	4,020	4,020	4,020	4,020

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 6. Regression results for voucher households

Dependent variable (%):	poor	extremely poor	black	minority	female head with child
	(1)	(2)	(3)	(4)	(5)
SOI	1.501***	0.572	3.183*	-0.052	-0.240
	(0.501)	(0.533)	(1.836)	(0.566)	(0.474)
PHA FX	Y	Y	Y	Y	Y
Year FX	Y	Y	Y	Y	Y
Constant	95.308***	75.029***	23.862***	44.149***	44.133***
	(0.569)	(0.578)	(1.430)	(0.538)	(0.479)
R2	0.336	0.509	0.990	0.972	0.842
N observations	2,850	2,850	2,850	2,850	2,850

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 7. Event study results for voucher households

Dependent variable (%):	poor	extremely poor	black	minority	female head with child
	(1)	(2)	(3)	(4)	(5)
SOI 1	1.392**	0.665	3.329	-0.044	-0.605
	(0.593)	(0.645)	(2.027)	(0.639)	(0.639)
SOI 2	1.630***	0.522	3.194*	-0.620	-0.248
	(0.558)	(0.782)	(1.710)	(1.008)	(0.558)
SOI 3	1.712***	-0.001	3.765*	-0.350	0.639
	(0.582)	(0.646)	(2.101)	(1.237)	(0.659)
SOI 4	2.087***	1.086	5.535***	1.404	0.820
	(0.698)	(0.832)	(2.129)	(0.888)	(0.767)
SOI 5+	2.821***	1.661*	5.825***	2.098**	-0.019
	(0.920)	(0.975)	(2.207)	(0.935)	(0.857)
PHA FX	Y	Y	Y	Y	Y
Year FX	Y	Y	Y	Y	Y
Constant	89.432***	70.081***	19.799***	42.897***	46.473***
	(3.624)	(3.127)	(2.652)	(1.756)	(2.143)
R2	0.338	0.509	0.990	0.972	0.842
N observations	2,850	2,850	2,850	2,850	2,850

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Appendix

Figure A.1. Trends in the share of poor households in public housing by enactment year

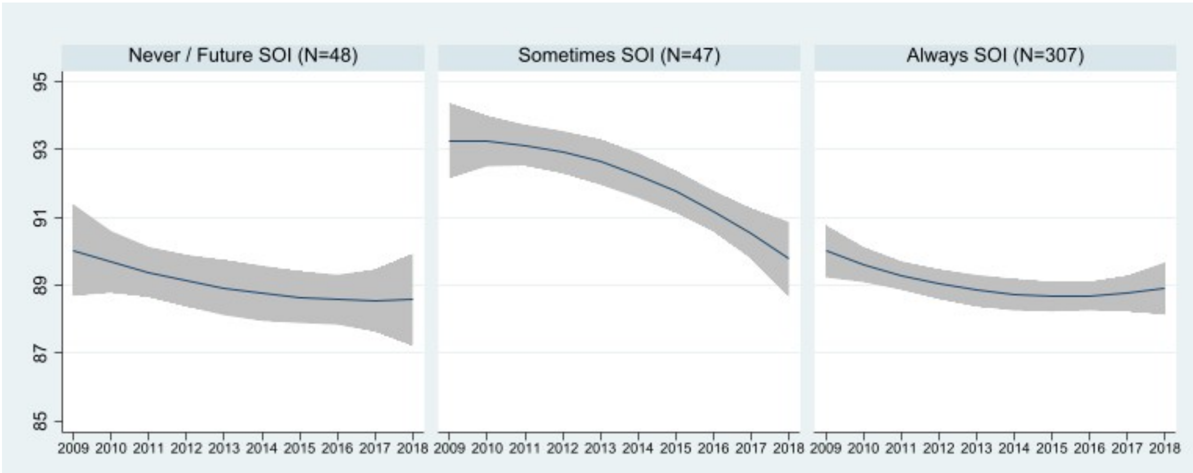


Figure A.2. Trends in the share of extremely poor households in public housing by enactment year

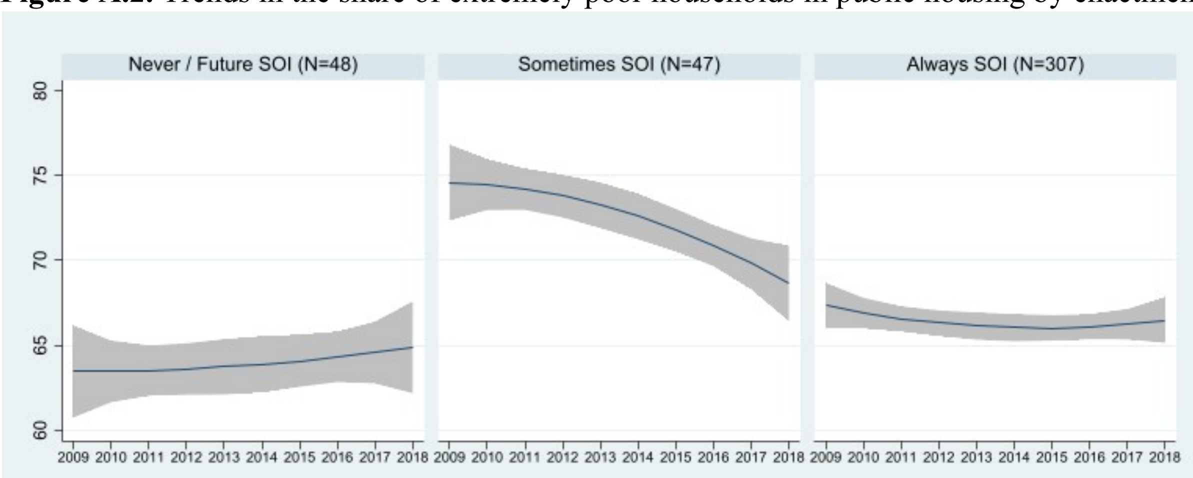
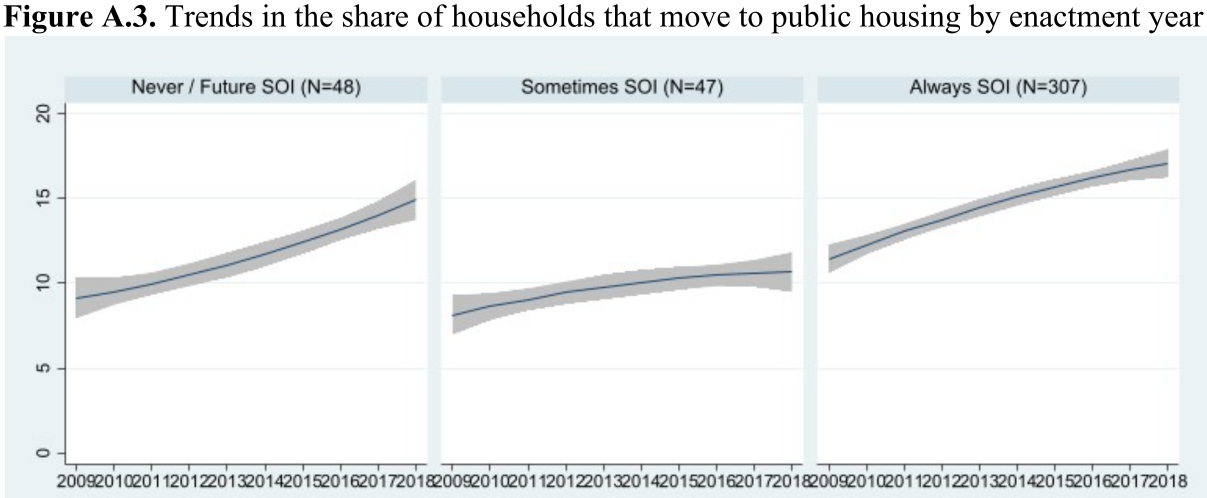


Figure A.3. Trends in the share of households that move to public housing by enactment year



References

Collinson, R., Ellen, I. G., & Ludwig, J. (2015). Low-income housing policy. In *Economics of Means-Tested Transfer Programs in the United States, Volume 2* (pp. 59-126). University of Chicago Press.

DeLuca, S., Garboden, P. M., & Rosenblatt, P. (2013). Segregating shelter: How housing policies shape the residential locations of low-income minority families. *The ANNALS of the American Academy of Political and Social Science*, *647*(1), 268-299.

Finkel, M., & Buron, L. (2001). Study on Section 8 voucher success rates. Volume I. Quantitative study of success rates in metropolitan areas. *prepared by Abt Associates for the US Department of Housing and Urban Development*, 2-3.

Freeman, L. (2012). The impact of source of income laws on voucher utilization. *Housing Policy Debate*, *22*(2), 297-318.

Freeman, L., & Li, Y. (2014). Do source of income anti-discrimination laws facilitate access to less disadvantaged neighborhoods? *Housing Studies*, *29*(1), 88-107.

Galvez, M. M. (2011). *Defining "choice" in the Housing Choice Voucher program: The role of market constraints and household preferences in location outcomes*. New York University.

Jacob, B. A. (2004). Public housing, housing vouchers, and student achievement: Evidence from public housing demolitions in Chicago. *American Economic Review*, *94*(1), 233-258.

Macdonnell, J. G., & Kahn, B. E. (2005). In search of decent housing in the DC metropolitan area: the affordable housing crisis for section 8 voucher holders. *Washington DC*.

Massey, D. S., & Kanaiaupuni, S. M. (1993). Public housing and the concentration of poverty. *Social Science Quarterly*, *74*(1), 109-122.

McKenna, W. F., & Hills, C. A. (1982). *The report of the President's Commission on housing* (Vol. 81). President's Commission on Housing.

Newman, S. J., & Schnare, A. B. (1997). "… And a suitable living environment": The failure of housing programs to deliver on neighborhood quality. *Housing Policy Debate*, *8*(4), 703-741.

Pendall, R. (2000). Why voucher and certificate users live in distressed neighborhoods. *Housing Policy Debate*, *11*(4), 881-910.

Schill, M. H., & Wachter, S. M. (1995). The spatial bias of federal housing law and policy: Concentrated poverty in urban America. *University of Pennsylvania Law Review*, *143*(5), 1285-1342.

Turner, M. A. (1998). Moving out of poverty: Expanding mobility and choice through tenant-based housing assistance. *Housing policy debate*, *9*(2), 373-394.

United States Department of Housing and Urban Development (2020). Public Housing Fact Sheet. Retrieved from https://www.hud.gov/topics/rental_assistance/phprog

United States Housing Act of 1937, Pub. L. No. 75-412, 50 Stat. 888 (1937)

Von Hoffman, A. (1996). High ambitions: The past and future of American low-income housing policy. *Housing Policy Debate*, *7*(3), 423-446.

Weicher, J. C. (1990). How poverty neighborhoods are changing. *Inner-city poverty in the United States*, *68*, 70.

Essay II

Are Public Housing Projects Good for Kids After All?

(Co-authored with Amy Ellen Schwartz)

1. Introduction

Is public housing good for children? Despite the best intentions, the benefits of public housing – such as improved affordability, better housing units, or greater residential stability – may be outweighed by detrimental effects of high-poverty neighborhoods, low-quality schools, neighborhood crime, or other (dis)amenities. Whether the net effect is positive or negative is theoretically ambiguous and likely to depend on the characteristics of the housing and its associated schools and neighborhood and their quality compared to origin neighborhoods. While the limited previous literature is discouraging, much of this work exploits *exits* from relatively undesirable public housing – comparing outcomes of children who left to those who remained (Katz et al., 2001; Jacob, 2004; Sanbonmatsu et al., 2006; Ludwig et al., 2013; Chetty et al., 2016; Chyn, 2018). Two notable exceptions, Weinhardt (2014) and Carlson et al. (2019), focus on *entries* into oversubscribed social housing in England and public housing in Wisconsin, respectively. While both studies report mostly null effects, limitations in sample size or variation in the public housing stock may have masked positive (or negative) effects under some circumstances. Further, they do not control for the independent effects of residential and school mobility.

In this paper, we draw on detailed individual-level longitudinal data on public school students in New York City (NYC) to examine the effects of *entries* into public housing, including some projects located in gentrifying neighborhoods. Exploiting plausibly random variation in the precise timing of entry into public housing, we estimate credibly causal effects of public housing on academic and weight outcomes using both difference-in-differences and event study designs. We exploit heterogeneity across the neighborhoods surrounding the 147 public housing developments and myriad origin residences and leverage data on public schools to explore the extent to which neighborhoods and schools shape the effects of public housing.

More specifically, we use administrative student-level data from the NYC Department of Education on NYC public school students, in grades 3-8, between academic years 2009 and 2017. We focus on 35,456 observations and 7,832 students who enter public housing in grades 5, 6, or 7 and thus have at least one year of standardized test scores both before and after moving into public housing. We use an expanded sample for attendance and weight analyses, adding students in grades K-2 and 9-12. We begin with a difference-in-differences identification strategy, using parsimonious models that link test scores and attendance to public housing residency and student fixed effects. We control for residential and school moves, estimate separate effects for the first post-move year and subsequent years, and explore heterogeneity across neighborhood types (both origin and destination), race/ethnicity, and gender. We further explore school characteristics of students moving into and out of different neighborhoods to understand the role of public schools. We investigate the empirical support for our identification assumption of plausibly random variation in the precise timing of student entry; we explore sociodemographic predictors of timing of entry and find little evidence that student characteristics are significant predictors of the grade of entry. Event study analyses also reveal little evidence of significant pre-trends.

To preview the results, we find that moving into public housing has positive and statistically significant effects on student outcomes, most prominently after the initial adjustment year. Small changes in test scores in year one are followed by larger improvements in subsequent years – both reading and math scores increase by roughly 0.1 standard deviations (sd). Event studies show smaller immediate changes in test scores post-move with steady improvement over time. As for heterogeneity, we find positive effects for all subgroups with larger effects for girls than boys (0.15 vs. 0.06) and for Asians and Whites (0.31 and 0.18 respectively) than Hispanics or Blacks (0.10 and 0.08 respectively). We find no significant effects for attendance or weight outcomes overall, but reductions in probability of being obese and overweight for boys.

Further, our results reveal the importance of neighborhoods – we see larger effects among students moving into higher-income neighborhoods (up to 0.13 increase) than those moving into lower-income neighborhoods (roughly 0.09 increase) after the first year in public housing. We also find improvement in attendance rate and reduction in chronic absenteeism for students moving into higher-income neighborhoods. Results suggest school matters, as moving out of low-income neighborhoods or into high-income neighborhoods is associated with attending better schools – with lower shares of economically disadvantaged schoolmates (that is, the share of students eligible for free and reduced-price lunch) and higher average test scores.

Taken together, our results suggest that differences in the neighborhood and school contexts of public housing studied in previous research underlie the differences in results. Put simply, the null effects in previous studies may reflect the poor quality of the schools or neighborhoods associated with public housing, while our positive effects may be driven by improvements in schools and neighborhoods. Bottom line, our results refute the popular belief that public housing *per se* is bad for kids and call for future work probing the circumstances under which public housing works to improve academic outcomes for low-income students.

2. Background and Literature Review

2.1. The Promises and the Problems of Public Housing

Public housing was the federal government's first major housing assistance program for low-income households. The Housing Act of 1937 established local housing authorities to develop public housing projects with the goal of providing "decent, safe, and sanitary dwelling for families of low-income" (p.888). While federally funded, public housing projects are administered, managed, and operated by local public housing authorities. Some examples of local housing authorities include Chicago Housing Authority and NYC Housing Authority (NYCHA). There are approximately 3,300 public housing authorities nationwide that serve around 1.2 million households living in public housing projects (HUD, 2020). Each public housing authority sets the eligibility criteria for public housing based on household income and has its own waiting list (if any) and tenant selection and assignment process. Households living in public housing typically pay 30 percent of their adjusted household income towards rent, which is in most cases well below the market rate.

Despite the best intentions to provide subsidized housing units for needy families, public housing has long been criticized for its creation of "concentration of poverty" (Massey & Kanaiaupuni, 1993). Public housing projects, typically consisting of one or more concentrated blocks of high-rise apartment buildings, were often sited in neighborhoods occupied by poor, minority residents (Von Hoffman, 1996; Schill & Wachter, 1995). Public housing tenants also come from low-income and minority households because of the program's eligibility criteria, bringing in a large number of poor, minority families to the neighborhood. Critics blamed the design of public housing projects created isolated geographic areas that are disproportionately poor and racially Black.

Concerns about the concentration of poverty in and around public housing motivated federal housing policies to shift towards alternative housing assistance programs (Collinson et al., 2015). In the landmark case of Gautreaux in 1976, the Supreme Court ruled that the Chicago Housing Authority and the U.S. Department of Housing and Urban Development (HUD) discriminated against Black tenants by concentrating them in large-scale projects located in poor, Black neighborhoods. In 1992, Congress passed a new public housing funding program, HOPE VI, to replace public housing projects in distressed neighborhoods across the nation with privately-owned, mixed-income projects (Schwartz, 2014). Tenants living in public housing buildings subject to demolition were given vouchers to move out of public housing and into other rental units in the private market. The housing choice voucher program was first created in 1974 as an alternative to the project-based approach of public housing and allowed tenants to find rental units in the private housing market. These alternative programs were often viewed by policymakers as the preferred form of housing assistance as the means to alleviate the concerns around the concentration of poverty, compared to public housing that limits the choice of residential locations to poor neighborhoods.[7]

However, not all eligible households can receive housing choice vouchers.[8] Waiting lists for vouchers are long; the average waiting time for vouchers easily exceeds a year, with substantially longer waits of up to multiple years in large cities (Maney & Crowley, 2000). Even after many months on the waiting list, households are not guaranteed to successfully find housing in the private market. Low-income households – most likely under time and resource

---

[7] As another alternative, the government subsidized private developers to ensure low- and moderate-income households could afford units in private projects. Low-Income Household Tax Credit (LIHTC) program is the most popular form of privately-owned, mixed-income projects. State allocating agencies would award tax credits to developers if at least 20 percent of their tenants have incomes below 50 percent of the area median income (AMI) or at least 40 percent have incomes below 60 percent of AMI.
[8] In the case of mixed-income projects, it is mostly moderate-income households that occupy the projects, often not affordable for low-income households without other forms of subsidy (Desai et al., 2010; O'Regan & Horn, 2013).

constraints – typically have 60 days to find adequate housing units that meet the federal housing quality standards and are also below the rent limits. Although prohibited by law in many states and municipalities to discriminate against the source of income to pay rent, landlords may also decline to accept vouchers (Galvez, 2011). Low-income households and minorities, in particular, may further face discrimination by landlords in the private housing market (President's Commission on Housing, 1982; Freeman, 2012; Tighe et al., 2017). As a result, a substantial number of voucher recipients fail every year to successfully utilize their housing vouchers or find housing in neighborhoods not different from where they used to live.

Public housing, therefore, remains an important stream of housing assistance for low-income families that may face multiple barriers in obtaining and utilizing housing vouchers. As of 2020, public housing serves more than 2 million residents and 1 million households in need of housing assistance across the nation (HUD, 2020). The all-too-common long waiting lists for most public housing authorities attest the demand remains high. Furthermore, while previous studies and popular press often focus on the negative effects of poor neighborhoods surrounding public housing, there is limited causal evidence that public housing of this negative impact on children. It is crucial to understand the effects of living in public housing and the mechanisms through which it may further help or harm the residents and their children in need of housing assistance.

2.2. How Might Public Housing Affect Student Outcomes?

We identify four key channels through which public housing may affect student outcomes. First, moving into public housing may have *income effects*. Subsidized rents for public housing units, which are well below the market price, may effectively reduce rent burdens and increase disposable income. Public housing tenants typically pay 30 percent of their adjusted income towards rent, with some variation by local housing authorities (HUD, 2020).[9] Increased income is likely to improve children's academic outcomes, especially for low-income households. Previous studies suggest families in affordable housing are more likely to increase their expenditure on necessities and enrichment of their children yet experience reduced parental stress, which are all associated with improvements in children's cognitive skills, physical, social, and emotional health (Harkness & Newman, 2005; Newman & Holupka, 2016).

Second, moving into public housing may mean *improved housing*. That is, public housing may provide housing units of "decent, safe, and sanitary dwelling." Improved housing conditions may include more reliable heat, water, and other utilities, as well as more space and improved privacy. Early studies, including Currie and Yelowitz (2000), suggest that public housing units provide better housing conditions, as they should meet federal housing quality standards. Currie and Yelowitz (2000) use the sex composition of children as an instrument for the relationship between families' likelihood of living in public housing and their housing conditions. Families with two children of the opposite sex are eligible for an extra bedroom than those of the same sex and are thus more likely to apply for public housing. They find that public housing children are less likely to live in overcrowded units or high-density complexes and less likely to repeat grades. However, anecdotal evidence from the popular press suggests otherwise; dilapidated housing conditions of NYC public housing recently called attention from the popular press and nearly resulted in a federal takeover of NYCHA (Benfer, 2019; Weiser & Goodman, 2018). More generally, previous research suggest housing conditions are closely related to children's

---

[9] Voucher recipients typically pay 30 percent of their adjusted income towards rent but must pay any additional amount if rents are above the payment standard set by local public housing authorities. In case of privately-owned mixed-income projects, rents are often not affordable for low-income households without other forms of subsidy. Put differently, public housing may bring larger income effects than other housing assistance programs.

physical and psychological development and academic performance (Leventhal & Newman, 2010; Coley et al., 2013).

Third, public housing may improve *residential stability* over time. Unlike families in private rental housing, public housing tenants are at lower risk of eviction or rent hikes at lease renewal.[10] Students in public housing may be less likely to experience multiple moves to new schools and communities, which are shown to have disruptive effects on their academic performance (Newman & Harkness, 2002; Crowley, 2003; Cordes et al., 2016; Cordes et al., 2019). Improved residential stability provided by public housing may therefore have positive academic impacts on students.

Finally, there may be *neighborhood effects* if moving into public housing means *different neighborhoods* – either or worse better than the alternative (or counterfactual) neighborhood. A broad literature documents the importance of neighborhood resources on children's development and academic outcomes (Leventhal & Newman, 2010; Chetty et al., 2016; Schwartz et al., 2017). While early descriptive studies show kids in public housing live in worse neighborhoods than those of welfare households living elsewhere (Newman and Schnare, 1997), more recent studies on NYC public housing – the setting of this study – suggest substantial variation in public housing neighborhoods. Han et al. (2020) document substantial variation in the micro-neighborhood food environment among students living in NYC public housing, which has consequences for childhood obesity. Dastrup and Ellen (2016) also find that most NYC public housing, originally built decades ago in low-income areas, is now surrounded by relatively in high-income neighborhoods, and Schwartz et al. (2010) do not find NYC public school students living in public housing attend worse schools than otherwise similar peers living elsewhere.

[10] This includes housing voucher recipients who would need to find housing in the private market.

2.3. Quasi-Experimental Evidence on the Impacts of Public Housing Exits and Entries

Existing research identifying the causal effect of public housing is limited. Most of these prior studies leverage exogenous exits from public housing programs driven by the building demolition or policy shifts towards the housing choice voucher program. The earliest studies include research on the Gautreaux mobility program, in which selected households living in Chicago Housing Authority's inner-city projects received housing vouchers to move to suburban neighborhoods as part of the Gautreaux litigation in 1976. The demand for the program exceeded its supply, and program participation was primarily determined by whether the applicant's telephone call went through on the registration day. Leveraging this variation, Rosenbaum and Popkin (1991) and Rosenbaum (1995) find improved academic performance among children who moved out of public housing located in extremely distressed inner-city neighborhoods and into private housing (using vouchers) in predominantly White, suburban communities.

Jacob (2004) exploits the exogenous timing of the demolition of public housing buildings in Chicago to examine the effect of public housing exits. He finds no significant differences in the test scores between children who move out earlier and those that stay in public housing. Jacob (2004) suggests that the null effects may be due to students moving to neighborhoods and schools that closely resemble the public housing neighborhoods they had left. Chyn (2018), however, examines the longer-run impacts and finds that three years after demolition, displaced households live in lower-poverty neighborhoods with lower crime rates, which leads to improved employment and wage outcomes.

The reported gains from the Gautreaux relocation program and lasting concerns about the concentrated poverty motivated the Moving to Opportunity (MTO) experiment in 1992. The purpose of MTO, the largest randomized controlled experiment on housing mobility, was to test whether providing housing vouchers would improve the life trajectories of the poorest families in public housing (Orr et al., 2003). Households with children living in public housing in census tracts with a poverty rate of at least 40 percent were eligible to participate in the experiment. The experimental group was randomly chosen to receive vouchers and mobility counseling, along with a requirement to move out of public housing and into low-poverty neighborhoods while the control group received no vouchers but could continue to live in public housing. Results are disappointing. A series of studies find no evidence of test score gains (or any effects on physical health, including incidences of childhood obesity); however, depending on the subgroup, some find evidence of mental health gains and reduction in risky behaviors for girls (but not boys) and longer-term positive effects on college attendance and earnings for children who moved at younger ages (Katz et al., 2001; Sanbonmatsu et al., 2006; Kling et al., 2007; Ludwig et al., 2013; Chetty et al., 2016).

To be clear, all of these studies focused on families leaving public housing in very poor, troubled neighborhoods. Whether results generalize to public housing in better neighborhoods – perhaps with better schools – is unclear, and the estimated results are most likely underestimated in previous studies.

Two recent quasi-experimental studies exploit exogenous timing of entry to public housing, yet they find mostly null effects of public housing on student outcomes. Weinhardt (2014) exploits plausibly random timing of entry into social housing *neighborhoods* in England.[11] His identification relies on the long waiting lists that give tenants little control over the precise timing they move into social housing neighborhoods. He compares two key stage

[11] Weinhardt (2014) is unable to precisely identify housing subsidy recipients, but instead identifies households moving into high-density social housing neighborhoods based upon the percentage of households that rent from the council (local authority), a registered social landlord or housing association and calculates the percentage of households living in social housing.

exam test scores of early movers who in between the two exams and late movers or "future recipients" who move in after the two exams. He finds that earlier movers into social housing neighborhoods do not experience any detrimental effects on their test scores compared to later movers. Carlson et al. (2020) also examine the impact of entry into public housing in Wisconsin, comparing test scores for 841 students who enter public housing to 604 future recipients (and other welfare recipients). They find that some evidence that public housing leads to declines in math scores but null effects on reading scores.

While these recent studies represent important contributions, empirical limitations may have obscured any positive impact of public housing residency. First, neither accounts for the potentially disruptive effects of residential and/or school mobility, as distinct from moving into public housing *per se.* Thus, their estimates of impacts immediately post move may well be moderated by short term adjustment costs. Second, neither of the studies has sufficient sample size or variation to explore heterogeneity across student subgroups and/or neighborhoods. Carlson et al. (2020) have a small public housing sample and do not explore any variation within public housing sample in terms of demographic subgroups and neighborhoods.[12] A large majority of the students in the Weinhardt (2014) sample were White (more than 80 percent), while public housing residents in the US include large populations of Blacks, Hispanics and Asians. The significant discrimination in housing markets suggests impacts may vary across racial/ethnic groups. As the neighborhood effects literature find differential effects by gender, we may find further differences in the impact of public housing residency across other student demographic subgroups.

[12] Carlson et al. (2020) do not specify the racial composition of their sample of students living in public housing, but their overall housing assistance sample (in public housing and on housing choice vouchers) are 40 percent Black and 44 percent White. They find that housing assistance (for the overall sample) benefits black students, while they find null results for whites. They find no meaningful differences between girls and boys.

In this paper, we build upon these two papers and exploit plausibly random variation in the timing of student entry to NYC public housing created by its application process and the waiting list. We use data on individual-level test scores following students up to four years after they move into public housing and attendance and weight outcomes for up to eight years. The rich longitudinal dataset, which includes indicators of school attended, allows us to parse the potentially disruptive effects of residential and school mobility followed by entry to public housing. In addition to exploring heterogeneity by racial/ethnic subgroups and gender, we capture the differences in neighborhoods surrounding public housing projects, including their school peer characteristics, to probe potential mechanisms. Leveraging variation in student and neighborhood characteristics and controlling for residential and school moves may help reconcile findings from previous research.

3. NYC Public Housing and the Waiting List

NYCHA – the setting of our study – is the nation's largest public housing system. As of 2020, the NYC public housing system contains 169,820 households in 2,252 buildings and 139 projects dispersed across the city's five boroughs (NYCHA, 2020).[13] Roughly over 400,000 residents occupy NYC public housing, of which approximately 26 percent are under age 18. Large projects located in different parts of the city are likely to provide diverse living and learning environments for children living in NYC public housing. Dastrup and Ellen (2016) document that 54 public housing projects in NYC were surrounded by high-income neighborhoods, while 49 projects were in low-income neighborhoods in 2010. They find other neighborhood amenities, including public school quality, are correlated with neighborhood

[13] The five boroughs include Manhattan, Bronx, Brooklyn, Queens, and Staten Island. The maximum rent for NYC public housing is 30 percent of the household's income, with HUD subsidizing the remainder of the rent. The average NYCHA public housing family's annual income is $25,502. The average monthly rent is $548, which is way below the market rate.

income, which may explain the finding in Schwartz et al. (2010) that public school students in NYCHA buildings do not systematically attend worse public schools than otherwise similar peers living elsewhere in the city.

Like most public housing authorities in major cities, NYCHA is oversubscribed and has its own tenant selection and assignment process, creating extensive uncertainty in the precise timing that eligible households can apply and receive an offer for public housing. First, NYC residents have little control over the *timing of their application*, because NYCHA closes its waiting list for public housing from time to time to control the volume of the applications it receives. When applications for public housing open, eligible households that apply are placed on the waiting list based on their family size, income, needs (e.g., emergencies), and date of application. Second, applicants have limited control over the *timing of receiving offers* due to the long waiting list. As of 2020, 176,646 families are on the waiting list for NYC public housing (NYCHA, 2020). In the past five years, the average time between "date entered waiting list" and "admission date" for NYC public housing has been more than 38 months (HUD, 2019).[14] Third, applicants are unlikely to manipulate their *timing of entry* through waiting and rejecting offers. When applicants reach the top of the waiting list, they must select one preferred project within 30 days, conditional on containing an anticipated vacancy. NYCHA randomly assigns applicants to vacant units in the selected project. Applicants can reject the initial offer and can receive up to two offers, but their application will be closed if they fail to accept or reject the offers within 60 days (NYCHA, 2020).[15] Previous research suggests few households reject the initial offer for

---

[14] Note that this average waiting time does not include the time spent on the waiting list for households that have not received the offer yet (or those that withdraw from the list) and may underestimate the average waiting time to receive a public housing offer.

[15] In exceptional cases, "emergency" applicants – such as households with children that are homeless, victims of domestic violence, or intimidated witnesses – may be prioritized in the tenant selection process. The timing of entry to public housing for these emergency applicants will be also be affected by the limited availability, list closures, etc. that create randomness in the timing of entry. Emergency applicants are only allowed to select a preferred

housing assistance programs with long waiting lists, as it may entail a substantial wait for and uncertainty regarding the availability of another unit (Coley et al., 1997; Rosenbaum, 1995; DeLuca & Rosenbaum, 2003). NYCHA closing its waiting list from time to time may further reduce households' likelihood of rejecting offers, as it would increase their uncertainty around the chance to create new applications to get back on the waiting list.

We exploit the resulting exogenous variation in the timing of household entry to NYC public housing to examine the causal impact of public housing residency on student outcomes. We explore the empirical support for the proposition that the timing and the grade in which students enter public housing are uncorrelated with pre-determined student characteristics in the following sections.

4. Data, Measures, and Sample

We use individual-level longitudinal data from the NYC Department of Education on NYC public school students, grades 3-8, in academic years (AY) 2009-2017. These data include scores on state tests in English Language Arts and mathematics standardized by grade (*z-ELA* and *z-Math* respectively), attendance, height and weight measures (from an annual FitnessGram®), student residential and school location, and sociodemographic variables, such as gender, race/ethnicity, grade, educational program participation (e.g., students with disability and English language learners), and economically disadvantaged students.[16] In addition to the attendance rate, *Attendance*, we construct an indicator for chronic absenteeism, *ChrncAbsent*, which identifies students absent for ten or more days in an academic year. We calculate body mass index (BMI) using student height and weight and follow Centers for Disease Control and

---

borough rather than a particular project but are also allowed to reject the initial offer. The application will be closed if they reject the second offer.

[16] Economically disadvantaged students are defined by whether they were ever eligible for free or reduced-price lunch (household incomes below 185 percent of the federal poverty level) in AY 2001-2017.

Prevention guidelines in constructing an indicator for obesity, *Obese,* if BMI is at or above the 95th percentile for their age and sex and overweight, *Overweight*, if BMI is at or above the 85th percentile. Two indicator variable captures school and residential mobility: *NewSchool* equals 1 in time t if a student attends a different school in t than t-1 and *NewAddress* equals 1 in time t if student address differs between t and t-1. We also link our student-level data to longitudinal school-level data on enrollment, standardized test scores, and demographic characteristics, such as percentage Black, Hispanic, Asian, White, eligible for free or reduced-price lunch from the New York State Annual School Report and the School Report Card.

Critically, we identify students living in NYCHA housing using student residential location and address data for NYCHA buildings. Specifically, an indicator for public housing residency, *PH*, equals 1 if student i lives in public housing in t or any previous period – that is, *PH* is 1 in the first year a student lives in public housing and all following years.[17] We also create an indicator *EntryPH,* which equals 1 in the first year a student lives in public housing, and *PostPH,* which equals 1 in all subsequent years. In this way, we parse the immediate effects of moving into public housing – which may include potential disruptive effects of school and residential mobility – from effects in subsequent years. Similarly, we create a set of pre- and post-public housing year indicators for our event study specifications.

We identify two neighborhood types surrounding the public housing projects. Following Dastrup and Ellen (2016) classification of NYC public housing neighborhoods, we define "high-opportunity" projects as those surrounded by census block groups with average median household incomes at or above the city median in 2010 and "low-opportunity" projects if below the median. We assign time-invariant indicator variables for students who move into high-

[17] We adopt this "intent-to-treat" definition because household decisions to exit public housing are likely endogenous. Thus, our impact estimates include the effects in all years post moving into public housing.

opportunity projects (*HighOpp*) and for students who move into low-opportunity neighborhoods (*LowOpp*). We construct similar indicators based on student "origin" neighborhoods – that is, their residential neighborhoods in time t-1 when they moved into public housing in time t. *HighOrigin* equals 1 for students if their census block group of the origin residence is surrounded by census block groups with average median household incomes at or above the city median, and *LowOrigin* equals 1 for other students.

Since our identification strategy relies upon comparing student outcomes before and after entering public housing, we focus on three cohorts of students that enter public housing in grades 5, 6, or 7 and have at least one year of standardized test scores before and after entering public housing. We call these cohorts G5, G6, and G7, respectively (to be concrete, G5 are students who enter public housing in grade 5). These include 7,832 students with 35,456 pre- and post-public housing observations in grades 3-8. Our extended sample for attendance and weight analyses include 40,086 pre- and post-public housing observations of students in grades K-12.

Table 1 provides summary statistics of our analytic sample by public housing entry cohort in AY 2009, a "pre-treatment" year for all of the students (none live in public housing yet), by construction. As shown, our three entry cohorts are quite similar in baseline characteristics. All cohorts are slightly overrepresented by female students (52 to 55 percent female) and approximately 40 percent are Black and 50 percent Hispanic, with 8 to 9 percent being Asian and 2 percent White. Virtually all are economically disadvantaged (rounded up to 100 percent) with standardized test scores below NYC average in both reading (ranging between -0.31 and -0.28) and math (between -0.33 and -0.27). The earlier cohort has higher shares of chronically absent students (58 percent vs. 52 to 54 percent) but have approximately the same attendance rate as other cohorts (91 and 92 percent). Roughly one quarter are obese, and 43 percent are overweight across all cohorts. Time-invariant indicators for neighborhood characteristics before and after moving into public housing show that around 47 to 48 percent of these students eventually move into high-opportunity projects and 21 to 22 percent come from high-income neighborhoods prior to entering public housing. Overall, student characteristics appear to differ little across entry cohorts prior to entering public housing, and the shares of students that move into high-opportunity projects are strikingly similar across cohorts.

5. Empirical Strategy

5.1. Regression Models

The centerpiece of our empirical work is a regression model linking student academic outcomes to *PH*, our public housing indicator, along with a set of time-varying student characteristics and student fixed effects to capture any unobserved time-invariant differences between students:

$$Y_{it} = \beta_0 + \beta_1 PH_{it} + \gamma X_{it} + \delta_i + \tau_t + \varepsilon_{it} \quad (1)$$

where Y_{it} is the standardized test scores and attendance outcomes for student i in grade t, including *z-ELA*, *z-Math*, *Attendance*, and *ChrnAbsent*. X_{it} is a vector of other time-varying student characteristics, including disability status and English language learner status. Student and grade fixed effects are δ_i and τ_t, respectively. Our coefficient of interest is β_1, which captures the impact of living in public housing. In this formulation, β_1 will warrant a causal interpretation if the precise timing of entry public housing is random. We provide empirical evidence in support of this below.

We parse the first year and subsequent effects with the following:

$$Y_{it} = \beta_0 + \beta_1 EntryPH_{it} + \beta_2 PostPH_{it} + \gamma X_{it} + \delta_i + \tau_t + \varepsilon_{it} \quad (2)$$

where β_1 captures the first-year effect of living in public housing– including any disruptive effects of residential and school moves – and β_2 captures the effect of living in public housing in subsequent years. Alternative specifications add controls for *NewSchool* and *NewAddress* to isolate the disruptive effects of school and residential mobility from the effect of living in public housing after an adjustment period.

We investigate the potential mechanisms underlying the effect of public housing in two ways, first, by exploring heterogeneity in impacts by neighborhood income and student demographic characteristics and, second, by exploring intermediary variables, such as school peer characteristics. To do so, we introduce a series of interaction terms between public housing variables and neighborhood (or demographic) indicators to estimate separate coefficients for those moving into low- and high-opportunity neighborhoods, for those moving out of low- and high-origin neighborhoods, and for different demographic subgroups (by gender, race/ethnicity). For instance, we estimate separate coefficients for high- and low-opportunity destination neighborhoods with the following:

$$Y_{it} = \beta_0 + \beta_{l1}\, LowOpp \; x \; EntryPH_{it} + \beta_{l2}\, LowOpp \; x \; PostPH_{it} \qquad (3)$$

$$+ \beta_{h1}\, HighOpp \; x \; EntryPH_{it} + \beta_{h2}\, HighOpp \; x \; PostPH_{it} + \gamma X_{it} + \delta_i + \tau_t + \varepsilon_{it}$$

where other models would substitute *LowOpp* and *HighOpp* with a set of indicators for origin neighborhoods (*LowOrigin* and *HighOrigin*), for gender (*Female* and *Male*), and for race/ethnicity (*Asian*, *Black*, *Hispanic*, and *White*) to be interacted with the indicators for public housing residency (*EntryPH* and *PostPH*).

We then probe the intermediary factors driving the differences in the effect of public housing between high- and low-opportunity neighborhoods (and high- and low-income origin neighborhoods) by exploring the effects on school characteristics:

$$\text{School}_{it} = \beta_0 + \beta_{l1}\, \textit{LowOpp x EntryPH}_{it} + \beta_{l2}\, \textit{LowOpp x PostPH}_{it} \qquad (4)$$

$$+ \beta_{h1}\, \textit{HighOpp x EntryPH}_{it} + \beta_{h2}\, \textit{HighOpp x PostPH}_{it} + \gamma X_{it} + \delta_i + \tau_t + \varepsilon_{it}$$

where $School_{it}$ is a set of school-level characteristics including enrollment, share of economically disadvantaged peers, and average z-scores of math and reading exams that student i attends in grade t. Again, we substitute *LowOpp* and *HighOpp* with a set of indicators for origin neighborhoods, gender, and race/ethnicity.

Finally, we conduct a series of robustness checks. We examine whether our results are robust to limiting our sample to students who do not exit public housing during our study period. We explore heterogeneity by entry cohort (G5, G6, and G7) to examine alternative specifications of our difference-in-differences approach. We use an extended sample of students in grades K-12 for attendance outcomes to examine whether the longer-term results are robust to our baseline results focusing on students in grades 3-8. We also examine other non-academic student outcomes, including student obesity outcomes, that we have data for students in grades K-12.

5.2. Timing of Moving into Public Housing: Testing Key Assumptions

As described earlier, the key to a causal interpretation of our impact estimates is that the precise timing of entering public housing is effectively random – specifically, that the "assignment" of entry into public housing in grade 5 rather than grade 6 or 7 is effectively random – and unrelated to outcomes or salient student characteristics. Although a formal test of this hypothesis is not possible – since salient characteristics may be unobservable – the similarity of the mean characteristics of cohorts, shown in Table 1, bolsters our confidence in this assumption. We probe the empirical support for this claim further in two ways. First, we estimate a linear probability model linking an indicator for "early mover" (G5 rather than G6 or G7) or "late mover" (in G7 rather than G5 or G6) to student baseline characteristics including demographics and test scores in AY 2009.[18] This way, we shed light on the extent to which baseline characteristics predict the timing of entry. As shown in Table 2, all coefficients are statistically insignificant with one exception of being black in column 2. However, all measures of academic performance prior to public housing entry, including standardized test scores and attendance outcomes, do not predict entry cohort.

Second, we explore the trajectory of student outcomes prior to entry into public housing using an event study specification of our baseline model. We use event study analyses to gain insight into whether outcomes were improving (or falling) in the pre-period – suggesting a trend that might have continued after entry. As shown in Figures 1a and 1b, test scores in previous years (pre 2 and pre 3-) are statistically indistinguishable from the reference year's test score (pre 1 or the year prior to entering public housing or pre 1) – with an exception for *zMath* in pre 2, for which we cannot reject the null hypothesis that all pre-public housing test scores are distinguishable from zero in a joint F-test at the 0.1 level. On the contrary, reading and math scores gradually increase after public housing residency and are statistically distinguishable from the reference year (see post 2 and post 3+). To summarize, event study analyses show no statistically significant pre-trends, again, suggesting a causal interpretation of our impact estimates is warranted.

[18] Borough fixed effects are included in the model to reflect the NYC public housing assignment process, in which households are required to indicate preferred borough on their application to be placed on the waiting list (see Section 3). Results in Table 2 are robust to the alternative model that does not include borough fixed effects. Main regression models do not include borough fixed effects, because student fixed effects are likely to absorb borough fixed effects unless students move across boroughs. We, instead, stratify our sample by borough and re-estimate our baseline model as a robustness check.

6. Results

6.1. Moving into Public Housing Improves Student Academic Outcomes

As shown in Table 3, estimates from our parsimonious model suggest living in public housing increases reading and math scores by 0.03 to 0.04 sd, respectively. Adding controls for residential and school mobility increases the estimated effects to 0.06 and 0.07 and yields negative coefficients for *NewSchool* and *NewAddress* (-0.03 and -0.02, respectively). Thus, the naïve estimates in the parsimonious model may reflect, in part, the academic adjustment costs of residential and school mobility. As for attendance, naïve estimates suggest a deleterious effect of public housing – attendance falls by 0.6 percentage points (pp) and chronic absenteeism increases by 2.7 pp. However, controlling for school and residential mobility suggests no effect of moving into public housing, per se, although moving to a new school or new address has a negative effect.

To probe the timing of these effects, we estimate separate effects for the first year and the subsequent years. As shown in Table 4, we see positive test score effects in the first year of 0.04 (*z-ELA*) and 0.05 (*z-Math*), followed by larger effects of 0.10 (*z-ELA*) and 0.11 (*z-Math*) in later years. In this formulation, we see no separate negative effect of residential mobility, although the negative effect of school mobility remains. While there is no statistically significant effect of public housing per se on attendance outcomes, the effects of residential and school mobility remain negative and statistically significant.

Overall, our results in Tables 3 and 4 suggest that moving into public housing yields considerable improvements in student test scores with larger positive effects in later years and does not harm, if not benefit, attendance outcomes conditional on residential and school mobility.

6.2. Are Effects Larger for Public Housing in Better Neighborhoods?

Results in Table 5 show that neighborhoods matter. For test score outcomes, public housing has similarly positive effects in the initial year of around 0.04 for reading and slightly larger improvement in math for students moving into high-opportunity projects (0.06) than those in low-opportunity projects (0.04). The differences in test score improvements become larger in later years between students moving into different public housing neighborhoods, where students in high-opportunity projects improve reading and math scores by 0.11 and 0.13 and other students in low-opportunity projects improve by 0.09 and 0.10. These are all statistically meaningful differences at the 0.1 level. We also find statistically significant improvement in attendance rates by 0.4 pp and reduction in incidence of chronic absenteeism by 2.1 pp for students who move into high-opportunity projects after the initial year in public housing. We see no statistically significant changes in attendance outcomes for students who move into low-opportunity projects, yet the directions of the coefficients still remain positive for attendance rates and negative for chronic absenteeism. These results suggest that the positive impact of moving into public housing on test scores persists regardless of neighborhood, but improvements in neighborhood quality may enhance the positive impacts and also improve student attendance outcomes.

As for heterogeneity by origin neighborhood, we find less pronounced differences in the impact of moving into public housing. In Table 6, in the initial year in public housing, we find that the improvements in reading (0.04) and math (0.05) scores are substantially similar and statistically indistinguishable between students from different origin neighborhoods. However, in later years, students moving from lower-income neighborhoods experience larger increases in reading scores than students moving from higher-income neighborhoods (0.10 vs. 0.08). Yet again, improvements in math scores are not statistically different between students from lower-income and higher-income neighborhoods in later years (0.11). For attendance outcomes, we find students from higher-income neighborhoods are likely to experience larger and statistically significant improvement in attendance rates (by 0.06 pp) and reduction in chronic absenteeism (by 2.9 pp). Students from lower-income neighborhoods do not experience any statistically significant changes in attendance outcomes after moving into public housing.[19] While we find generally positive impact of moving into public housing regardless of neighborhood quality, the magnitude of the effects may depend upon the extent to which moving into public housing delivers a better or worse neighborhood.

6.3 Heterogeneity by Demographic Subgroups

We explore heterogeneity in impact by other student demographic characteristics. In Table 7, we find the positive effects on test scores are primarily driven by female students. The estimated increase in test scores range up to 0.15 sd for female students in their later years in public housing. Male students do not experience statistically significant increases in reading scores in their first year in public housing; however, both their reading and math scores appear to improve after the initial year (by 0.04 and 0.06, respectively). For attendance outcomes, in Table 7 columns 3 and 4, we find the estimated impacts of moving into public housing are not statistically significant for both boys and girls.

In Table 8, we find that Asian and White students experience larger increases in test scores following entry to public housing – ranging up to 0.31 increase for Asians and 0.18 increase for Whites. While our analytic sample includes diverse demographic groups of students, note that 8 percent are Asian and 2 percent are White; the largest positive effects are represented

[19] In future work, we plan to explore heterogeneity by changes in neighborhood median income or by whether students move to a better public housing neighborhood compared to their neighborhood of origin residence.

by small shares of Asian and White students. However, we still find meaningful improvements in test scores among Hispanic (up to 0.10) and Black students (up to 0.08) after they move into public housing. For attendance outcomes, we find generally positive impact except for White students.

6.4. Probing Mechanisms: Moving to Better Schools

We examine whether the differences in the estimated impact by public housing neighborhoods are attributable to the quality of public schools that students attend. In Table 9, we find that school peer characteristics change in different directions depending on the surrounding neighborhood. Students who move into low-opportunity projects attend schools with lower school-level standardized test scores than their previous school (around 0.02 to 0.03 sd reduction for both reading and math), but students who move into high-opportunity projects attend schools with higher standardized test scores (between 0.02 and 0.03 increase). Regarding the share of economically disadvantaged peers, although the magnitudes are small (of roughly 1.4 pp differences), we also find that changes are in different directions by the type of public housing neighborhood, where students moving to high-opportunity neighborhoods experience a reduction in the share of poor peers at school. School-level enrollment reduce for students in both types of neighborhoods.

While moving into public housing consistently have positive impacts on student academic performance regardless of their surrounding neighborhoods, students in higher-income neighborhoods may benefit more in the transition partly due to the changes in school peer characteristics and the resulting learning environment.

Similarly, we examine the changes in school peer characteristics for students moving out of low-income versus high-income neighborhoods in Table 10. We find that students from lower-income neighborhoods are likely to move to schools with higher average reading scores of around 0.02 after their initial year in public housing, but we find no statistically significant changes in school-level math scores. On the contrary, we find reductions in both school-level reading and math scores for students moving from higher-income neighborhoods (around 0.04 to 0.05). These changes in peer test scores may explain the larger positive gains in reading scores for students moving from lower-income neighborhoods yet statistically indistinguishable gains in math scores found in Table 6. We also find that students moving from lower-income neighborhoods are likely to attend schools with lower shares of economically disadvantaged peers (by 0.6 pp), while students moving from higher-income neighborhoods experience increase in the share of economically disadvantaged peers of around 2 pp after moving into public housing. Unlike students moving from lower-income neighborhoods, those from higher-income neighborhoods do not attend smaller schools than before.

We further explore whether school characteristics vary by student demographic subgroups and find meaningful differences. In Table 11, we find that female and male student experience different changes in school characteristics after they move into public housing. Female students attend schools with higher standardized test scores for both reading and math by 0.02 after the initial year they move into public housing, while male students attend schools with lower scores by 0.02 in the initial year and around 0.01 for reading to 0.02 for math in later years. These changes in school peer characteristics may explain larger improvements in test scores and attendance outcomes among female students found in Table 7. In Table 12, we find that Asian students experience the most dramatic changes in peer characteristics after moving into public housing; they attend schools with peer test scores that are around 0.05 higher in the initial year and 0.14 higher for reading and 0.11 for math in later years than their pre-treatment years. On the contrary, Hispanic and Black students experience a drop in school-level standardized test scores by around 0.01 to 0.03 in the initial year but the changes become statistically insignificant in later years. These may explain positive yet smaller improvements in student test scores for Hispanic and Black students found in Table 8. Students of different demographic subgroups may leverage changes in housing and neighborhood in disparate ways, including the choice of public schools to attend. Our results suggest that the resulting characteristics of public schools attended by demographic subgroups may be driving the differences in the magnitude of our estimated impact of moving into public housing.

6.4. Robustness Checks and Other Outcomes

We conduct a series of robustness checks and find that the results are not sensitive to alternative samples and specifications. First, results are robust to measuring the outcomes excluding public housing exiters in Table A.1. Our intent-to-treat approach in identifying public housing residency considers students who exit public housing after their first year to still be "treated" by public housing. We exclude students who ever leave public housing from our sample and still find consistently positive and statistically significant impacts of public housing residency on student academic performance.

Second, we examine whether the effects are different by entry cohort – G5 (Figures A.1a and A.1b), G6 (Figures A.2a and A.2b), and G7 (Figures A.3a and A.3b). Event study results suggest that if we stratify our analytic sample by students who enter public housing in the same grade, we still identify no significant pre-trends in their standardized test scores for both reading and math. We also examine statistically significant and steady improvements in student test scores across all cohorts. These results suggest that our study results are not sensitive to alternative difference-in-differences estimations.

Finally, we extend our sample of students to all K-12 students who entered public housing in grades 5, 6, or 7 to examine the longer-term impact on student attendance and weight outcomes. Similar to our results for students in grades 3-8, we find that public housing residency has no significant impacts on student attendance outcomes (see Table A.2). We also examine student weight outcomes and find no significant changes in their likelihood of being obese or overweight. Examining heterogeneity in weight outcomes by sex, we find some evidence of statistically significant reductions in the probability of being obese and overweight for boys after their initial year in public housing (see Table A.3).

7. Conclusion and Discussion

Our study provides credibly causal evidence that public housing residency improves academic outcomes for NYC public school students. In a study observing students a similar setting in NYC but in *voucher households,* students were found to perform 0.05 sd better, on average, in both reading and math scores after receiving vouchers (Schwartz et al., 2019). We find comparable improvements of around 0.03 to 0.04 sd in student reading and math scores after moving into public housing. After the initial year in public housing, the year in which students make residential moves, by definition, and are highly likely to make school moves, we find student performance in reading and math exams increases by 0.09 and 0.11 sd. Our results suggest steady improvements over time, with little evidence of significant pre-trends in test scores. Stalled academic performance on the first year of entry may reflect potentially disruptive effects of residential and school moves in addition to any benefits of public housing. Residential and school mobility may play a key role in reconciling previous evidence on the null effects of moving into public housing from Weinhardt (2014) and Carlson et al. (2020), as they do not parse out the effects of disruptive moves from the impact of entering to public housing.

Moreover, our results highlight the importance of context. Our data include diverse public housing projects, and we find statistically significant and meaningful differences in impacts in different types of public housing neighborhoods. We find the strongest treatment effects of around 0.11 and 0.13 sd increase in reading and math scores for students who move into public housing sited in neighborhoods with higher average household incomes. These results are not only statistically significant but also substantively important. These results particularly reconcile findings from previous studies on the Gautreaux relocation program, mass public housing demolitions in Chicago, and the MTO experiment that focus on exits from extremely distressed public housing neighborhoods. Public housing, when supported by sufficient neighborhood resources, may have substantial positive impacts on student academic performance, given its income effects and potential improvements in housing conditions and residential stability. However, note that we find positive effects of public housing persist regardless of neighborhoods.

Our data also include a diverse set of student bodies that may provide more nuanced evidence on the impact of public housing by student demographic subgroups. We find larger improvement in test scores for girls (0.15 sd) than boys (0.06 sd). These results are similar to the findings from the MTO studies that girls benefit more from transitions to low-poverty neighborhoods relative to boys. For reference, Weinhardt (2014) and Carlson et al. (2020) do not find any statistically meaningful differences between girls and boys. We also explore heterogeneity by racial and ethnic groups and find that public housing residency has positive impacts not only on Black and White students but also on Hispanic and Asian children, who are little represented in previous literature. Previous studies on public housing mostly focus in geographic areas with less diverse body of students. For example, public housing kids in Chicago included in Jacob (2004) and Chyn (2018) are around 96 percent and 98 percent Black, and the sample of students in England's social housing neighborhoods from Weinhardt (2014) is roughly 84 percent White. As an exception, the sample of students on housing assistance in Wisconsin from Carlson et al. (2020) is 40 percent Black and 44 percent White; however, they do not specify the demographic breakdown between students on housing choice vouchers and in public housing and – due to the small public housing sample – are unable to explore heterogeneity by race. Our sample of NYC public housing children are 41 percent Black and 49 percent Hispanic, the remaining being 8 percent Asian and 2 percent White.

In summary, our results provide compelling evidence that public housing, unlike popular beliefs, may improve educational outcomes for low-income students. Public housing serves as an important source of housing assistance, especially for a particularly disadvantaged subset of populations that may face multiple barriers in utilizing alternative housing programs. Our study suggests that moving into public housing may provide low-income children with living environments beneficial to their academic outcomes when given the time to offset the disruption in the initial adjustment period. The potential positive effects of living in public housing are larger for projects located in high-opportunity neighborhoods. Future research includes understanding the role of differences in housing quality across public housing units to further understand what we can improve the current public housing system.

Tables and Figure

Table 1. Baseline variable means by grade of entry to public housing, AY 2009.

	Entered public housing in:		
	Grade 5 "G5" (1)	Grade 6 "G6" (2)	Grade 7 "G7" (3)
Student characteristics			
Female	0.55	0.52	0.54
Asian	0.08	0.09	0.08
Black	0.42	0.43	0.38
Hispanic	0.48	0.46	0.52
White	0.02	0.02	0.02
Economically disadvantaged	1.00	1.00	1.00
Student with disabilities	0.14	0.13	0.12
English language learner	0.18	0.17	0.19
Grade	3.57	4.36	4.89
Student outcomes			
z-ELA	-0.28	-0.28	-0.31
z-Math	-0.27	-0.33	-0.32
Attendance	0.91	0.92	0.92
ChrnAbsent	0.58	0.52	0.54
Obese	0.27	0.25	0.23
Overweight	0.43	0.43	0.43
Neighborhood characteristics			
HighOrigin	0.21	0.22	0.21
HighOpp	0.47	0.48	0.47
N	1,048	1,179	1,443

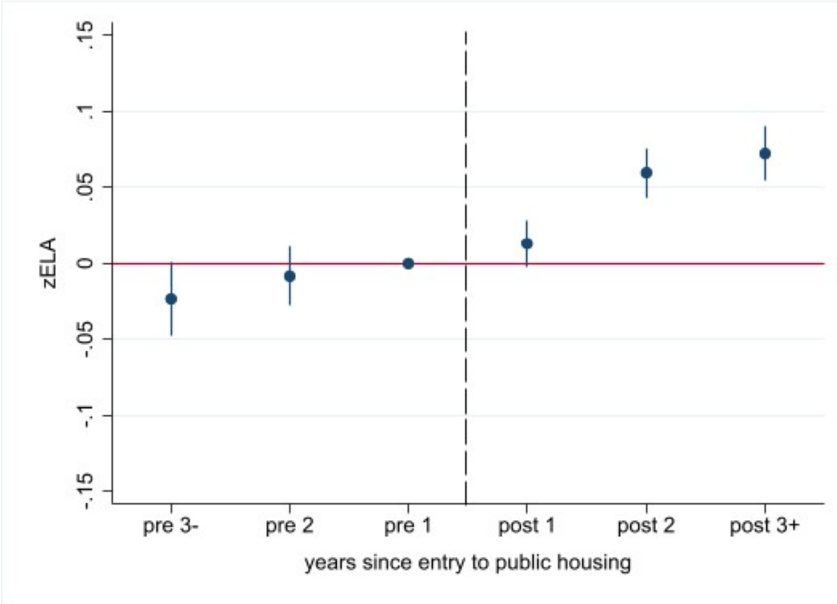
Notes: By construction, all observations in AY 2009 for this study's analytic sample are pre-public housing observations.

Table 2. Probability of moving into public housing in earlier and later grades, AY 2009.

	G5 "early mover" (1)	G7 "late mover" (2)
Female	0.013	0.011
	(0.014)	(0.014)
Asian	0.006	-0.033
	(0.027)	(0.028)
Black	0.024	-0.036**
	(0.015)	(0.016)
White	-0.071	0.027
	(0.051)	(0.053)
Economically disadvantaged	0.030	-0.163
	(0.104)	(0.109)
Student with disabilities	0.019	-0.035
	(0.021)	(0.022)
English language learner	-0.004	0.031
	(0.019)	(0.020)
z-ELA	0.008	-0.011
	(0.010)	(0.010)
z-Math	0.004	0.005
	(0.009)	(0.010)
Attendance	-0.021	0.196
	(0.132)	(0.137)
ChrnAbsent	0.035	-0.006
	(0.029)	(0.020)
Obese	0.025	-0.003
	(0.021)	(0.022)
Overweight	0.002	-0.015
	(0.018)	(0.019)
Grade FX	Y	Y
P-value for joint F-test	0.110	0.021
R2	0.298	0.349
N students (obs)	3,670	3,670

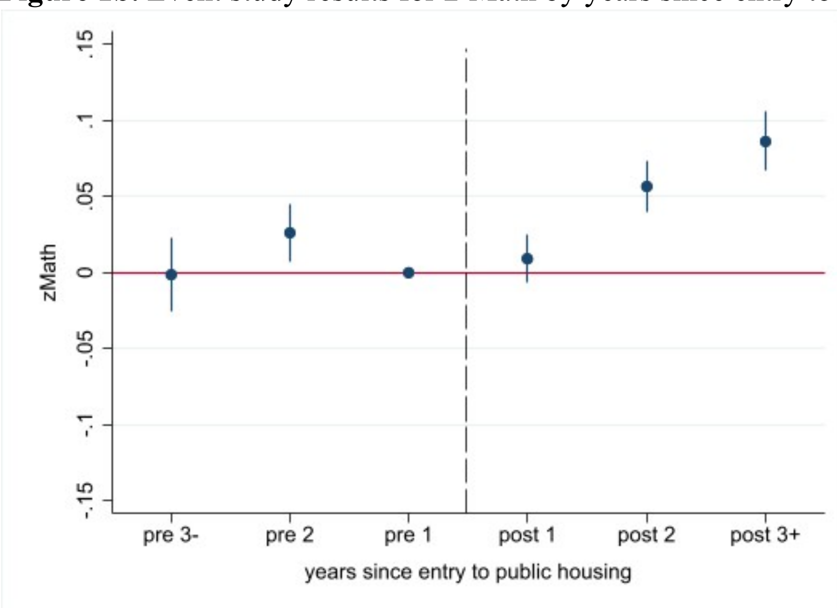
Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Hispanic is the reference category for race/ethnicity.

Figure 1a. Event study results for z-ELA by years since entry to public housing



Notes: N=36,560. Sample consists of NYC public school students who ever moved into NYCHA public housing in grades 5, 6, or 7, AY 2009-2017. Reference year is the year prior to entry (pre 1). Student fixed effects are included in the models.

Figure 1b. Event study results for z-Math by years since entry to public housing



Notes: N=36,560. Sample consists of NYC public school students who ever moved into NYCHA public housing in grades 5, 6, or 7, AY 2009-2017. Reference year is the year prior to entry (pre 1). Student fixed effects are included in the models.

Table 3. Parsimonious regression results for student outcomes

Dependent variable:	z-ELA (1)	z-ELA (2)	z-Math (3)	z-Math (4)	Attendance (5)	Attendance (6)	ChrnAbsent (7)	ChrnAbsent (8)
PH	0.028***	0.060***	0.041***	0.070***	-0.006***	0.002	0.027***	-0.007
	(0.010)	(0.013)	(0.011)	(0.013)	(0.001)	(0.002)	(0.007)	(0.009)
NewSchool		-0.033***		-0.031***		-0.007***		0.019***
		(0.009)		(0.009)		(0.001)		(0.006)
NewAddress		-0.023***		-0.021***		-0.006***		0.030***
		(0.008)		(0.008)		(0.001)		(0.006)
Student FX & Grade FX	Y	Y	Y	Y	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y	Y	Y	Y	Y
R2	0.727	0.727	0.745	0.745	0.695	0.697	0.627	0.628
N obs	35,456	35,456	35,456	35,456	35,456	35,456	35,456	35,456

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Time-varying student characteristics include gender, race/ethnicity, poverty status (eligibility for free and reduced-price lunch), special education, and limited English proficiency status. Sample consists of NYC public school students who ever moved into NYCHA public housing in grades 5, 6, or 7, AY 2009-2017.

Table 4. Baseline regression results for student outcomes

Dependent variable:	z-ELA (1)	z-Math (2)	Attendance (3)	ChrnAbsent (4)
EntryPH	0.041***	0.049***	0.001	-0.004
	(0.013)	(0.014)	(0.002)	(0.009)
PostPH	0.097***	0.110***	0.002	-0.013
	(0.015)	(0.016)	(0.002)	(0.011)
NewSchool	-0.034***	-0.033***	-0.007***	0.020***
	(0.009)	(0.009)	(0.001)	(0.006)
NewAddress	0.002	0.006	-0.005***	0.026***
	(0.010)	(0.010)	(0.001)	(0.007)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.727	0.745	0.697	0.628
N obs	35,456	35,456	35,456	35,456

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). EntryPH and PostPH are statistically different at the 0.01 level for columns 1 and 2. See notes in Table 3 for variable and sample descriptions.

Table 5. Regression results for student outcomes by public housing neighborhood

Dependent variable:	z-ELA	z-Math	Attendance	ChrnAbsent
	(1)	(2)	(3)	(4)
EntryPH x LowOpp	0.041***	0.040**	0.002	-0.004
	(0.015)	(0.016)	(0.002)	(0.011)
PostPH x LowOpp	0.088***	0.095***	0.001	-0.005
	(0.016)	(0.018)	(0.002)	(0.012)
EntryPH x HighOpp	0.041***	0.061***	0.001	-0.004
	(0.015)	(0.016)	(0.002)	(0.011)
PostPH x HighOpp	0.108***	0.126***	0.004**	-0.021*
	(0.017)	(0.018)	(0.002)	(0.012)
NewSchool	-0.034***	-0.033***	-0.007***	0.020***
	(0.009)	(0.009)	(0.001)	(0.006)
NewAddress	0.001	0.005	-0.005***	0.026***
	(0.010)	(0.010)	(0.001)	(0.007)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.727	0.745	0.697	0.628
N obs	35,456	35,456	35,456	35,456

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
PostPH x HighOpp and PostPH x LowOpp are statistically different at the 0.1 level for columns 1-4.
See notes in Table 3 for variable and sample descriptions.

Table 6. Regression results for student outcomes by origin neighborhood

Dependent variable:	z-ELA	z-Math	Attendance	ChrnAbsent
	(1)	(2)	(3)	(4)
EntryPH x LowOrigin	0.041***	0.049***	0.001	-0.003
	(0.014)	(0.015)	(0.002)	(0.010)
PostPH x LowOrigin	0.103***	0.111***	0.002	-0.008
	(0.016)	(0.017)	(0.002)	(0.011)
EntryPH x HighOrigin	0.036**	0.051***	0.002	-0.007
	(0.018)	(0.018)	(0.002)	(0.014)
PostPH x HighOrigin	0.076***	0.106***	0.006**	-0.029**
	(0.019)	(0.020)	(0.002)	(0.014)
NewSchool	-0.035***	-0.033***	-0.007***	0.019***
	(0.009)	(0.009)	(0.001)	(0.006)
NewAddress	0.002	0.006	-0.005***	0.026***
	(0.010)	(0.010)	(0.001)	(0.007)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.727	0.745	0.697	0.628
N obs	35,456	35,456	35,456	35,456

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
PostPH x HighOrigin and PostPH x LowOrigin are statistically different at the 0.1 level for columns 1, 3, and 4.
See notes in Table 3 for variable and sample descriptions.

Table 7. Regression results for student outcomes by sex

Dependent variable:	z-ELA (1)	z-Math (2)	Attendance (3)	ChrnAbsent (4)
EntryPH x Female	0.067***	0.067***	0.001	-0.005
	(0.015)	(0.015)	(0.002)	(0.010)
PostPH x Female	0.148***	0.153***	0.002	-0.007
	(0.016)	(0.018)	(0.002)	(0.012)
EntryPH x Male	0.012	0.030*	0.001	-0.002
	(0.015)	(0.016)	(0.002)	(0.011)
PostPH x Male	0.043**	0.064***	0.003	-0.019
	(0.017)	(0.018)	(0.002)	(0.012)
NewSchool	-0.036***	-0.034***	-0.007***	0.020***
	(0.009)	(0.009)	(0.001)	(0.006)
NewAddress	0.002	0.006	-0.005***	0.026***
	(0.010)	(0.010)	(0.001)	(0.007)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.728	0.745	0.697	0.628
N obs	35,456	35,456	35,456	35,456

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
PostPH x Female and PostPH x Male are statistically different at the 0.01 level for columns 1 and 2.
See notes in Table 3 for variable and sample descriptions.

Table 8. Regression results for student outcomes by race/ethnicity

Dependent variable:	z-ELA	z-Math	Attendance	ChrnAbsent
	(1)	(2)	(3)	(4)
EntryPH x Hispanic	0.039**	0.040**	0.001	-0.003
	(0.015)	(0.016)	(0.002)	(0.011)
PostPH x Hispanic	0.091***	0.095***	0.002	-0.001
	(0.017)	(0.018)	(0.002)	(0.012)
EntryPH x Black	0.018	0.033**	-0.000	-0.004
	(0.015)	(0.016)	(0.002)	(0.011)
PostPH x Black	0.063***	0.084***	0.002	-0.030**
	(0.017)	(0.019)	(0.002)	(0.012)
EntryPH x Asian	0.189***	0.172***	0.005**	0.003
	(0.033)	(0.026)	(0.002)	(0.014)
PostPH x Asian	0.297***	0.316***	0.010***	-0.012
	(0.029)	(0.029)	(0.002)	(0.014)
EntryPH x White	0.011	0.126***	0.006	0.001
	(0.045)	(0.046)	(0.004)	(0.034)
PostPH x White	0.162***	0.178***	-0.005	0.073**
	(0.044)	(0.044)	(0.005)	(0.033)
NewSchool	-0.033***	-0.032***	-0.007***	0.020***
	(0.009)	(0.009)	(0.001)	(0.006)
NewAddress	0.000	0.005	-0.005***	0.025***
	(0.010)	(0.010)	(0.001)	(0.007)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.727	0.745	0.697	0.628
N obs	36,560	36,560	36,560	36,560

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See notes in Table 3 for variable and sample descriptions.

Table 9. Regression results for school characteristics by public housing neighborhood

Dependent variable:	Enrollment (1)	% ED (2)	z-ELA (3)	z-Math (4)
EntryPH x LowOpp	-52.646***	0.888***	-0.025***	-0.029***
	(6.255)	(0.256)	(0.006)	(0.006)
PostPH x LowOpp	-57.918***	0.736**	-0.016**	-0.019**
	(8.572)	(0.341)	(0.008)	(0.009)
EntryPH x HighOpp	-45.621***	-0.264	0.003	-0.004
	(6.758)	(0.276)	(0.006)	(0.007)
PostPH x HighOpp	-43.123***	-0.787**	0.029***	0.022**
	(8.867)	(0.358)	(0.008)	(0.009)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.616	0.615	0.701	0.718
N obs	35,456	35,456	35,456	35,456

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
See notes in Table 3 for variable and sample descriptions.

Table 10. Regression results for school characteristics by origin neighborhood

Dependent variable:	Enrollment (1)	% ED (2)	z-ELA (3)	z-Math (4)
EntryPH x LowOrigin	-63.106***	-0.084	-0.003	-0.009
	(5.672)	(0.229)	(0.005)	(0.006)
PostPH x LowOrigin	-69.437***	-0.629*	0.017**	0.013
	(8.149)	(0.324)	(0.007)	(0.008)
EntryPH x HighOrigin	-3.295	1.849***	-0.044***	-0.046***
	(9.060)	(0.386)	(0.008)	(0.009)
PostPH x HighOrigin	13.213	2.248***	-0.035***	-0.043***
	(10.625)	(0.446)	(0.010)	(0.011)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.618	0.616	0.701	0.718
N obs	35,456	35,456	35,456	35,456

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
See notes in Table 3 for variable and sample descriptions.

Table 11. Regression results for school characteristics by sex

Dependent variable:	Enrollment (1)	% ED (2)	z-ELA (3)	z-Math (4)
EntryPH x Female	-53.026***	0.072	-0.003	-0.011
	(6.489)	(0.265)	(0.006)	(0.007)
PostPH x Female	-61.732***	-0.044	0.023***	0.017**
	(8.710)	(0.348)	(0.008)	(0.009)
EntryPH x Male	-46.216***	0.648**	-0.021***	-0.025***
	(6.487)	(0.266)	(0.006)	(0.007)
PostPH x Male	-39.340***	0.078	-0.014*	-0.018**
	(8.709)	(0.349)	(0.008)	(0.009)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.617	0.615	0.701	0.718
N obs	35,456	35,456	35,456	35,456

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See notes in Table 3 for variable and sample descriptions.

Table 12. Regression results for school characteristics by race/ethnicity

Dependent variable:	Enrollment (1)	% ED (2)	z-ELA (3)	z-Math (4)
EntryPH x Hispanic	-71.668***	0.356	-0.011*	-0.019***
	(6.613)	(0.266)	(0.006)	(0.007)
PostPH x Hispanic	-89.428***	0.463	-0.009	-0.014*
	(8.735)	(0.344)	(0.008)	(0.009)
EntryPH x Black	-35.863***	0.936***	-0.024***	-0.028***
	(6.602)	(0.274)	(0.006)	(0.007)
PostPH x Black	-36.020***	0.795**	-0.005	-0.005
	(8.808)	(0.356)	(0.008)	(0.009)
EntryPH x Asian	1.640	-3.275***	0.047***	0.047***
	(14.836)	(0.637)	(0.015)	(0.015)
PostPH x Asian	62.516***	-7.880***	0.141***	0.111***
	(15.328)	(0.638)	(0.016)	(0.016)
EntryPH x White	11.381	2.522**	-0.002	-0.022
	(31.122)	(1.144)	(0.024)	(0.027)
PostPH x White	107.635***	5.255***	0.029	0.039
	(31.133)	(1.173)	(0.025)	(0.027)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.620	0.620	0.702	0.719
N obs	35,456	35,456	35,456	35,456

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See notes in Table 3 for variable and sample descriptions.

Appendix

Table A.1. Regression results for student outcomes without exiters

Dependent variable:	z-ELA	z-Math	Attendance	ChrnAbsent
	(1)	(2)	(3)	(4)
EntryPH	0.041***	0.049***	0.001	-0.004
	(0.013)	(0.014)	(0.002)	(0.009)
PostPH	0.097***	0.110***	0.002	-0.013
	(0.015)	(0.016)	(0.002)	(0.011)
NewSchool	-0.034***	-0.033***	-0.007***	0.020***
	(0.009)	(0.009)	(0.001)	(0.006)
NewAddress	0.002	0.006	-0.005***	0.026***
	(0.010)	(0.010)	(0.001)	(0.007)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.727	0.727	0.745	0.745
N obs	29,453	29,453	29,453	29,453

Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
EntryPH and PostPH are statistically different at the 0.01 level for columns 1 and 2.
See notes in Table 3 for variable and sample descriptions.

Table A.2. Regression results for student outcomes (including weight), grades K-12

Dependent variable:	Attendance	ChrnAbsent	Obese	Overweight
	(1)	(2)	(3)	(4)
EntryPH	0.001	-0.001	-0.001	-0.004
	(0.001)	(0.009)	(0.007)	(0.008)
PostPH	0.002	-0.005	-0.007	-0.004
	(0.002)	(0.011)	(0.008)	(0.009)
NewSchool	-0.005***	0.027***	-0.008*	-0.003
	(0.001)	(0.007)	(0.005)	(0.006)
NewAddress	-0.004***	0.014	-0.004	0.007
	(0.001)	(0.006)	(0.004)	(0.005)
Student FX & Grade FX	Y	Y	Y	Y
Time-varying characteristics	Y	Y	Y	Y
R2	0.670	0.600	0.734	0.726
N obs	38,812	38,812	38,812	38,812

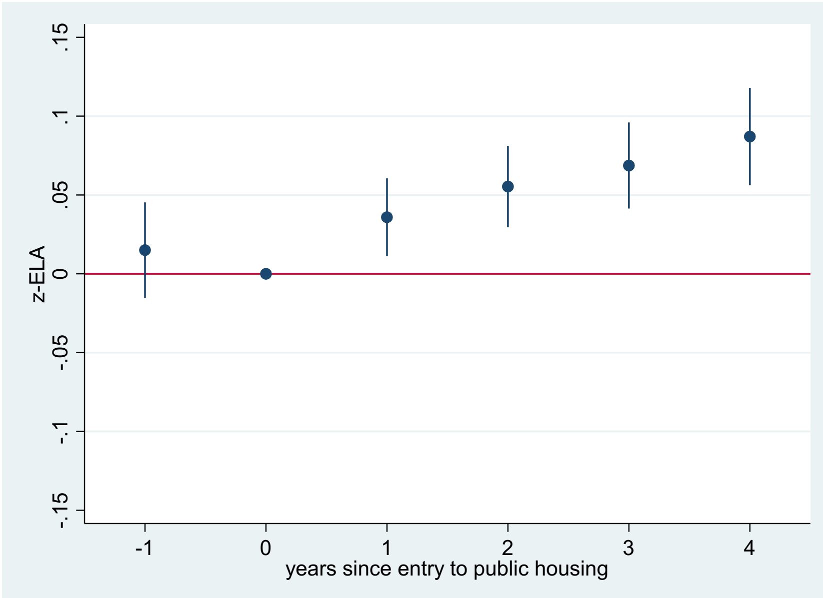
Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
See notes in Table 3 for variable and sample descriptions.

Table A.3. Regression results for weight outcomes by sex, grades K-12

Dependent variable:	Obese (1)	Overweight (2)
EntryPH x Female	-0.001	0.001
	(0.007)	(0.009)
PostPH x Female	0.001	0.014
	(0.008)	(0.010)
EntryPH x Male	-0.000	-0.010
	(0.008)	(0.009)
PostPH x Male	-0.015*	-0.024**
	(0.005)	(0.010)
NewSchool	-0.008*	-0.002
	(0.005)	(0.006)
NewAddress	0.004	0.007
	(0.004)	(0.005)
Student FX & Grade FX	Y	Y
Time-varying characteristics	Y	Y
R2	0.726	0.726
N obs	38,812	38,812

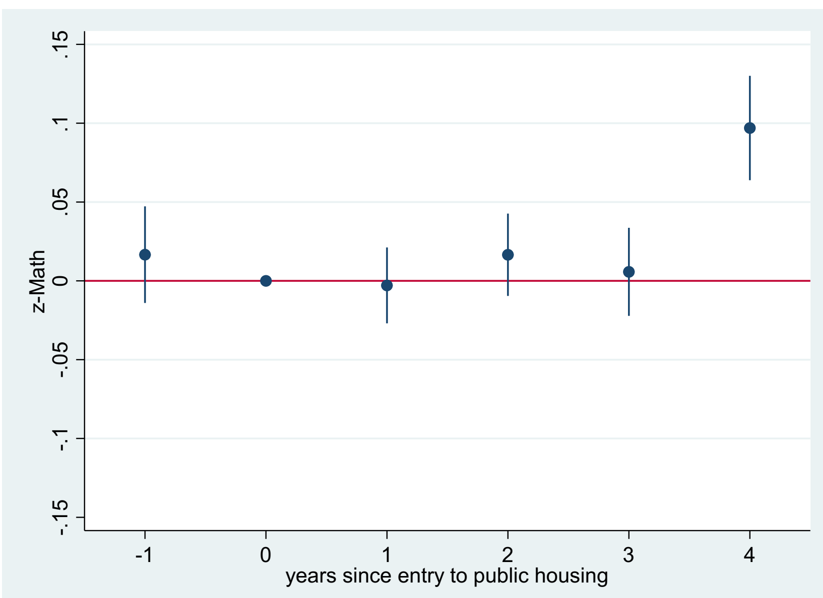
Note: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
PostPH x Female and PostPH x Male are statistically different at the 0.01 level for columns 1 and 2.
See notes in Table 3 for variable and sample descriptions.

Figure A.1a. Event study results for z-ELA, *G5*



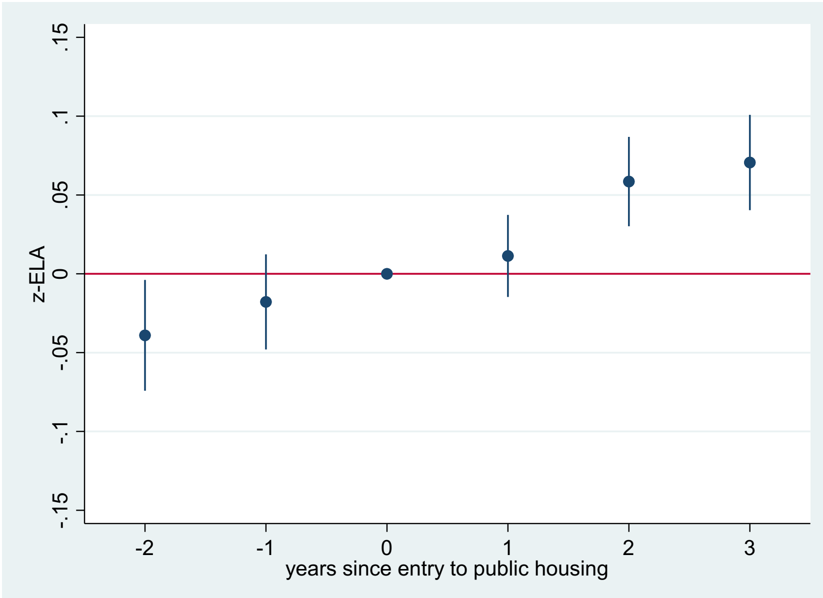
Notes: N=13,697. Sample consists of NYC public school students who ever moved into NYCHA public housing in grades 5, AY 2009-2017. Reference year is the year prior to entry (pre 1). Student fixed effects are included in the models. Joint F-test on pre-entry years in public housing p=0.330 and on post-entry years in public housing p=0.004.

Figure A.1b. Event study results for z-Math, *G5*



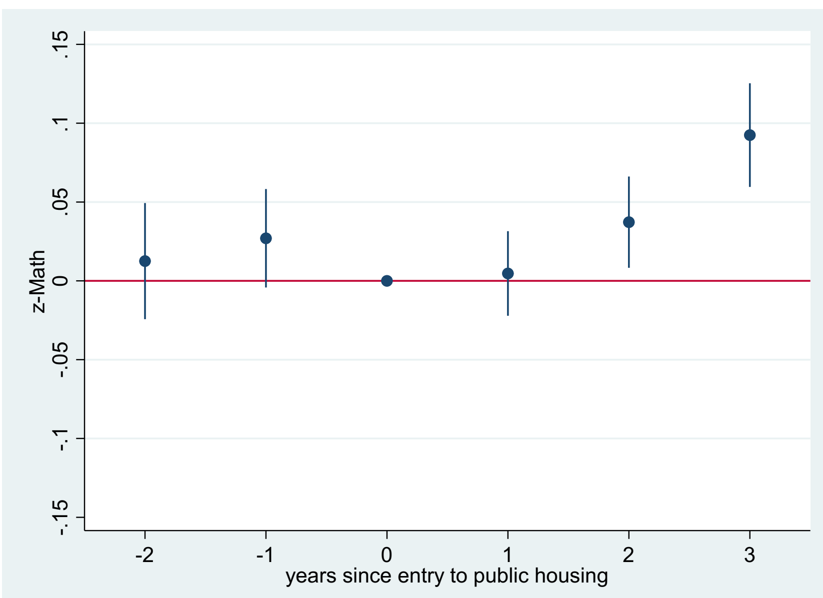
Notes: N=13,697. Sample consists of NYC public school students who ever moved into NYCHA public housing in grades 5, AY 2009-2017. Reference year is the year prior to entry (pre 1). Student fixed effects are included in the models. Joint F-test on pre-entry years in public housing p=0.288 and on post-entry years in public housing p=0.000.

Figure A.2a. Event study results for z-ELA, *G6*



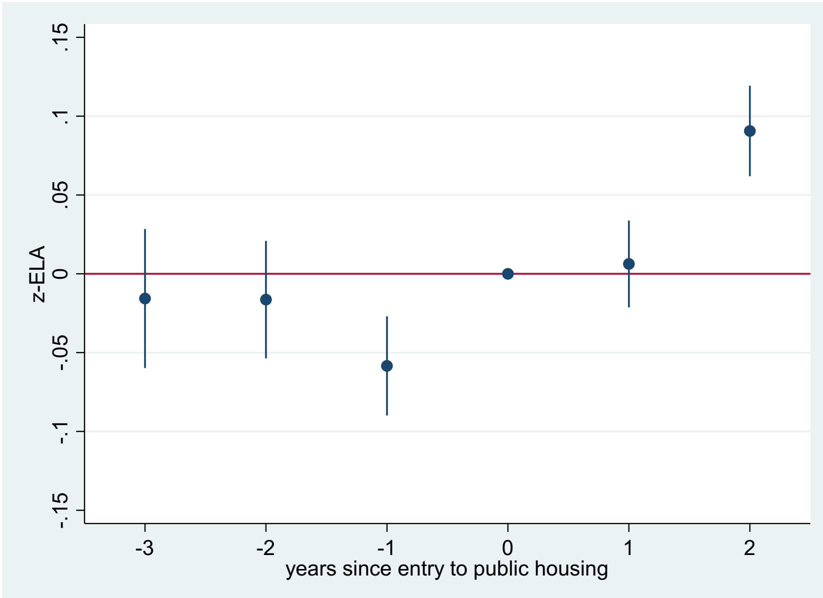
Notes: N=12,605. Sample consists of NYC public school students who ever moved into NYCHA public housing in grades 5, AY 2009-2017. Reference year is the year prior to entry (pre 1). Student fixed effects are included in the models. Joint F-test on pre-entry years in public housing p=0.243 and on post-entry years in public housing p=0.000.

Figure A.2b. Event study results for z-Math, *G6*



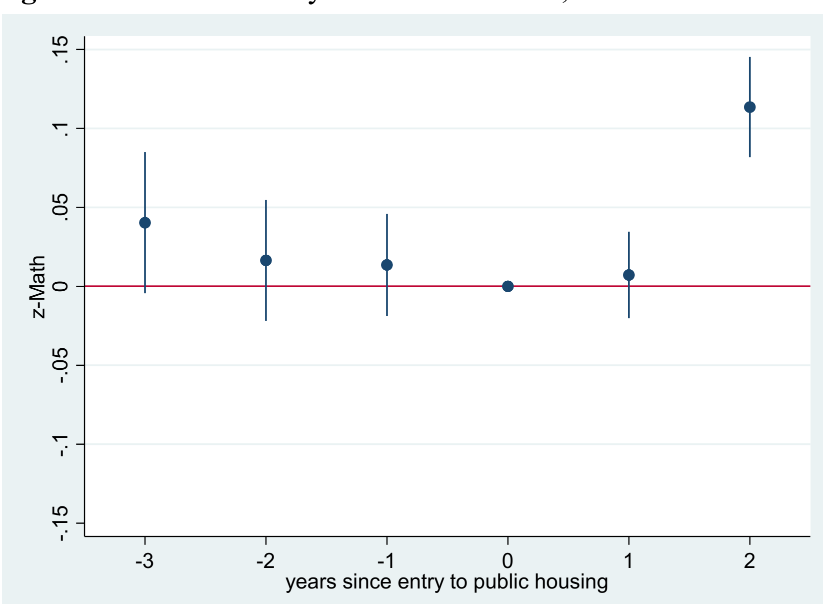
Notes: N=12,605. Sample consists of NYC public school students who ever moved into NYCHA public housing in grades 5, AY 2009-2017. Reference year is the year prior to entry (pre 1). Student fixed effects are included in the models. Joint F-test on pre-entry years in public housing p=0.450 and on post-entry years in public housing p=0.000.

Figure A.3a. Event study results for z-ELA, *G7*



Notes: N=10,258. Sample consists of NYC public school students who ever moved into NYCHA public housing in grades 5, AY 2009-2017. Reference year is the year prior to entry (pre 1). Student fixed effects are included in the models. Joint F-test on pre-entry years in public housing p=0.003 and on post-entry years in public housing p=0.000.

Figure A.3b. Event study results for z-Math, *G7*



Notes: N=10,258. Sample consists of NYC public school students who ever moved into NYCHA public housing in grades 5, AY 2009-2017. Reference year is the year prior to entry (pre 1). Student fixed effects are included in the models. Joint F-test on pre-entry years in public housing p=0.363 and on post-entry years in public housing p=0.000.

References

Benfer, E. A. (2019, Feb 11). New York's public housing system is the size of a city. It's failing children. The Washington Post.

Carlson, D., Miller, H., Haveman, R., Kang, S., Schmidt, A., & Wolfe, B. (2019). The effect of housing assistance on student achievement: Evidence from Wisconsin. *Journal of Housing Economics*, *44*, 61-73.

Chetty, R., Hendren, N., & Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment. *American Economic Review*, *106*(4), 855-902.

Chyn, E. (2018). Moved to opportunity: The long-run effects of public housing demolition on children. *American Economic Review*, *108*(10), 3028-56.

Coley, R. L., Leventhal, T., Lynch, A. D., & Kull, M. (2013). Relations between housing characteristics and the well-being of low-income children and adolescents. *Developmental psychology*, *49*(9), 1775.

Collinson, R., Ellen, I. G., & Ludwig, J. (2015). Low-income housing policy. In *Economics of Means-Tested Transfer Programs in the United States, Volume 2* (pp. 59-126). University of Chicago Press.

Cordes, S. A., Schwartz, A. E., & Stiefel, L. (2019). The effect of residential mobility on student performance: Evidence from New York City. *American Educational Research Journal*, *56*(4), 1380-1411.

Cordes, S. A., Schwartz, A. E., Stiefel, L., & Zabel, J. (2016). Is neighbourhood destiny? Exploring the link between neighbourhood mobility and student outcomes. *Urban Studies*, *53*(2), 400-417.

Crowley, S. (2003). The affordable housing crisis: Residential mobility of poor families and school mobility of poor children. *Journal of Negro Education*, 22-38.

Currie, J., & Yelowitz, A. (2000). Are public housing projects good for kids? *Journal of Public Economics*, *75*(1), 99-124.

Dastrup, S., & Ellen, I. G. (2016). Linking residents to opportunity: gentrification and public housing. *Cityscape*, *18*(3), 87-108.

Desai, M., Dharmapala, D., & Singhal, M. (2010). Tax incentives for affordable housing: the low-income housing tax credit. *Tax Policy and the Economy*, *24*(1), 181-205.

Freeman, L. (2012). The impact of source of income laws on voucher utilization. *Housing Policy Debate*, *22*(2), 297-318.

Galvez, M. M. (2011). *Defining "choice" in the Housing Choice Voucher program: The role of market constraints and household preferences in location outcomes*. New York University.

Han, J., Schwartz, A. E., & Elbel, B. (2020). Does proximity to fast food cause childhood obesity? Evidence from public housing. *Regional science and urban economics*, *84*, 103565.

Harkness, J., & Newman, S. J. (2005). Housing affordability and children's well-being: Evidence from the national survey of America's families. *Housing Policy Debate*, 16(2), 223-255.

Hirsch, A. R. (1983). *Making the second ghetto: Race and housing in Chicago 1940-1960*. University of Chicago Press.

Jacob, B. A. (2004). Public housing, housing vouchers, and student achievement: Evidence from public housing demolitions in Chicago. *American Economic Review*, *94*(1), 233-258.

Katz, L. F., Kling, J. R., & Liebman, J. B. (2001). Moving to opportunity in Boston: Early results of a randomized mobility experiment. *The Quarterly Journal of Economics*, *116*(2), 607-654.

Kling, J. R., Liebman, J. B., & Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, *75*(1), 83-119.

Kling, J. R., Ludwig, J., & Katz, L. F. (2005). Neighborhood effects on crime for female and male youth: Evidence from a randomized housing voucher experiment. *The Quarterly Journal of Economics*, *120*(1), 87-130.

Leventhal, T., & Newman, S. (2010). Housing and child development. *Children and Youth Services Review*, *32*(9), 1165-1174.

Ludwig, J., Duncan, G. J., Gennetian, L. A., Katz, L. F., Kessler, R. C., Kling, J. R., & Sanbonmatsu, L. (2013). Long-term neighborhood effects on low-income families: Evidence from Moving to Opportunity. *American Economic Review*, *103*(3), 226-31.

Maney, B., & Crowley, S. (2000). Scarcity and success: perspectives on assisted housing. *Journal of Affordable Housing & Community Development Law*, 319-368.

Massey, D. S., & Kanaiaupuni, S. M. (1993). Public housing and the concentration of poverty. *Social Science Quarterly*, *74*(1), 109-122.

New York City Housing Authority (2020). Tenant Selection and Assignment Plan. Retrieved from https://www1.nyc.gov/assets/nycha/downloads/pdf/TSAPlan.pdf

Newman, S. J., & Harkness, J. M. (2002). The long-term effects of public housing on self-sufficiency. *Journal of Policy Analysis and Management: The Journal of the Association for Public Policy Analysis and Management*, *21*(1), 21-43.

Newman, S., & Holupka, C. S. (2017). The effects of assisted housing on child well-being. *American Journal of Community Psychology*, *60*(1-2), 66-78.

O'Regan, K. M., & Horn, K. M. (2013). What can we learn about the low-income housing tax credit program by looking at the tenants? *Housing Policy Debate*, *23*(3), 597-613.

Orr, L., Feins, J., Jacob, R., Beecroft, E., Sanbonmatsu, L., Katz, L. F., Liebman, J. B., & Kling, J. R. (2003). Moving to opportunity: Interim impacts evaluation. Washington, DC: US Department of Housing and Urban Development, Office of Policy Development and Research.

President's Commission on Housing. (1982). Report of the President's commission on housing. Washington, DC: U.S. Government Printing Office.

Rosenbaum, J. E. (1995). Changing the geography of opportunity by expanding residential choice: Lessons from the Gautreaux program. *Housing Policy Debate*, *6*(1), 231-269.

Rosenbaum, J. E., & Popkin, S. J. (1991). Employment and earnings of low-income blacks who move to middle-class suburbs. *The urban underclass*, *342*, 356.

Sanbonmatsu, L., Kling, J. R., Duncan, G. J., & Brooks-Gunn, J. (2006). Neighborhoods and academic achievement results from the Moving to Opportunity experiment. *Journal of Human Resources*, *41*(4), 649-691.

Schill, M. H., & Wachter, S. M. (1995). Housing market constraints and spatial stratification by income and race. *Housing Policy Debate*, *6*(1), 141-167.

Schwartz, A. E., Horn, K. M., Ellen, I. G., & Cordes, S. A. (2020). Do housing vouchers improve academic performance? Evidence from New York City. *Journal of Policy Analysis and Management*, *39*(1), 131-158.

Schwartz, A. E., McCabe, B. J., Gould Ellen, I., & Chellman, C. C. (2010). Public schools, public housing: The education of children living in public housing. *Urban Affairs Review*, *46*(1), 68-89.

Schwartz, A. E., Stiefel, L., & Cordes, S. A. (2017). Moving matters: The causal effect of moving schools on student performance. *Education Finance and Policy*, *12*(4), 419-446.

Schwartz, A. F. (2014). *Housing Policy in the United States*. Routledge.

Tighe, J. R., Hatch, M. E., & Mead, J. (2017). Source of income discrimination and fair housing policy. *Journal of Planning Literature*, *32*(1), 3-15.

United States Department of Housing and Urban Development (2019). A picture of subsidized households. Retrieved from https://www.huduser.org/picture2019

United States Department of Housing and Urban Development (2020). Public Housing Fact Sheet. Retrieved from https://www.hud.gov/topics/rental_assistance/phprog

United States Housing Act of 1937, Pub. L. No. 75-412, 50 Stat. 888 (1937)

Von Hoffman, A. (1996). High ambitions: The past and future of American low-income housing policy. *Housing Policy Debate*, *7*(3), 423-446.

Weinhardt, F. (2014). Social housing, neighborhood quality and student performance. *Journal of Urban Economics*, *82*, 12-31.

Weiser, B. & Goodman, J. D. (2018, June 11) New York City Housing Authority, Accused of Endangering Residents Agrees to Oversight. The New York Times.

Essay III

Does Proximity to Fast Food Cause Childhood Obesity? Evidence from Public Housing

(Co-authored with Amy Ellen Schwartz and Brian Elbel)

1. Introduction

Does proximity to fast food outlets and "unhealthy" food increase obesity among low-income children? In the United States, nearly one fifth of low-income children are obese, facing elevated risks of adult obesity, hypertension, and cardiovascular disease, among other serious complications that may lead to premature death (Bridger, 2009; Ogden et al., 2018; Sahoo et al., 2015). One commonly proffered culprit is the high density of fast food and relative scarcity of "healthy" food outlets in low-income neighborhoods, which facilitate consumption of high-calorie, low-nutrient food and spur obesity. Despite persuasive evidence of a correlation between obesity and the food environment in low-income neighborhoods, there is a dearth of credible evidence on the *causal* effects of proximity to fast food for two key reasons. First, both individual weight and residential location are likely to reflect a set of common underlying individual or family characteristics, such as income or educational attainment, making it difficult to isolate the effects of proximity to fast food per se. Second, individual-level data linking weight measures to proximity to fast food are scarce, typically only available for small samples or in limited detail. In this paper, we leverage the plausibly random within-development location of families living in public housing – a novel strategy – and use longitudinal individual-level data on weight, residential location, and neighborhood food outlets for New York City (NYC) public school students to derive credibly causal estimates of the impact of proximity to fast food on weight outcomes.

The key to our identification strategy is the plausibly random within-development variation in food environment driven by the tenant selection and assignment process of NYC public housing. First, NYC public housing applicants cannot indicate their preference for precise residential location in the assignment process. Second, it takes approximately 38 months on average to get to the top of the waiting list for NYC public housing, and the long wait time discourages applicants from rejecting offers. Thus the unit assignment within a development depends upon the vacancies at the time of assignment and generates the plausibly random variation in proximity to food outlets that we leverage to isolate causal effects of proximity to fast food on weight outcomes.

We draw on a rich individual-level longitudinal dataset on 143,859 K-12 NYC public school students who lived in public housing at some point between academic years 2009 and 2016. In addition to socio-demographic characteristics, the data include residential location and annual height and weight measures. Using data on the locations of food outlets citywide, we then calculate the distance from each student's residential location to the nearest fast food restaurant (and to other food outlets). The large sizes of public housing developments in NYC yield substantial within-development variation in proximity to food outlets, and we find no statistically significant evidence of selection.

To preview our results, we find that, indeed, proximity to fast food increases the probability a child is obese. More specifically, the probability of being obese increases 0.6 percentage points for every 0.1 mile closer a student lives to the nearest fast food restaurant, and the probability of being overweight (which includes obesity) increases by 1.1 percentage points. In contrast, we find no evidence that proximity to other types of food outlets – wait-service restaurants, supermarkets, or corner stores – has any impact on weight outcomes. Stratifying by grade level reveals the largest effects are among students in grades 3-8, where the incidence of obesity increases 1.4 percentage points and the incidence of overweight increases up to 1.9 percentage points for every 0.1 mile closer to the nearest fast food. Effects are even larger among older elementary and middle school students attending neighborhood schools (located less than a half-mile from home) who are likely to patronize neighborhood food outlets: the incidence of obesity increases 1.7 percentage points, and the incidence of overweight increases 2.1 percentage points for every 0.1 mile closer to the nearest fast food. An average city block is 0.05 mile in NYC, and these estimates respectively represent a 6 percent and a 4.7 percent increase in the obesity and overweight rates from a two-block difference in distance to fast food. Effects among younger students (grades K-2) and older students (grades 9-12) are close to zero and not statistically significant. Results are robust to alternative measures of the food environment, such as variables capturing the presence or number of fast food restaurants within different radiuses from home. In short, we find credibly causal evidence that proximity to fast food increases obesity and overweight among low-income children, and students in grades 3-8 (typically ages 8-13) are the most vulnerable. Our results suggest that place-based interventions to limit access to or consumption of fast food may be effective in reducing the obesity rates among low-income children in urban areas.

This paper is organized as follows. We first review prior literature and the theoretical motivation that inform our approach to examine the relationship between proximity to fast food and childhood obesity. In Section 3, we describe the institutional setting of public housing in NYC that provides plausibly exogenous variation in student proximity to fast food. Section 4 presents the individual-level weight outcome data, food environment measures, and descriptive statistics of our sample. Most importantly, we show within-development variation in proximity to fast food among students in public housing and test whether the variation in the local food environment is uncorrelated with student demographic characteristics. In Section 5, we provide the estimating equations for our empirical strategy. We present results and a series of robustness checks in Section 6 and conclude with discussion and implications for policy in Section 7.

2. Literature review

2.1. The Link between Obesity and Proximity to Fast Food

A large body of descriptive studies, from a range of settings, provide compelling evidence on the association between unhealthy food environment and the prevalence of obesity. National studies linking individual weight outcomes with the food environment data at the county-level (Mehta & Chang, 2008), zip-code level (Gibson, 2011), and census tract-level (Chen et al., 2016; Dubowitz et al., 2012) find that higher density of fast food restaurants and lower density of supermarkets are associated with increased probability of obesity for individuals in the neighborhood. Studies focusing on southern states also find that individuals in census tracts with more supermarkets are less likely to be obese, while those in census tracts with more fast food restaurants and corner stores are more likely to be obese (Morland et al., 2006; Morland & Everson, 2009).

Studies on children further document that *micro-neighborhood* food environments are associated with obesity risks. A school-level analysis by Davis and Carpenter (2009) finds having a fast food restaurant within a half-mile of school is associated with higher probabilities of being obese among middle and high school students. A more recent study by Elbel et al. (2020) examines the weight outcomes of children attending NYC public schools and the individual-level variation in distance to fast food *within census tracts.* They find that living more than 0.025 mile (about half of a city block) from the nearest fast food restaurant is associated with lower obesity and overweight risks.

Why might access to fast food affect obesity? Put simply, closer proximity to a fast food outlet lowers the relative travel cost of purchasing fast food, which may lead to increased fast food consumption and, in turn, a high-calorie diet. Previous research on consumption decisions, including McCarthy (1999) and Bellettini and Kempf (2013), suggests longer distance and travel time increase the opportunity cost and the effective price the consumers pay. The food environment literature (see Anderson & Matsa, 2011; Cutler et al., 2003; Dunn, 2010) also builds upon the theory that food purchasing decisions are based on a function of the monetary price of the meal and other disutility, including the value of forgone time used to access or prepare the meal. As the distance to fast food decreases, the time and the cost spent on traveling to purchase fast food decrease, yet the relative time and cost of purchasing groceries and preparing home-cooked meals increase. Thus, among two individuals (if all else equal but their distances to fast food), the person living closer to a fast food restaurant is more likely to purchase fast food on a given day. Athens, Duncan, and Elbel (2016) provide evidence to support the hypothesis that proximity to fast food outlets and supermarkets are predictors of fast food dining frequency.[20]

Residential proximity to fast food, however, may be correlated with weight outcomes through other avenues. Families with a higher propensity for obesity – say a taste for fast food – may choose residential locations closer to fast food. More generally, there may be underlying individual or family characteristics that determine both residential location and propensity for obesity. While we are unaware of direct evidence on the underlying factors, a variety of existing studies have documented differences in the demographic characteristics of neighborhoods with

[20] Yet a recent empirical study finds that exposing low-income households to the same produces and prices available to high-income households do not change their demand for healthy groceries (Allcott et al., 2019).

different food environments. For example, Lewis et al. (2005) and Galvez et al. (2008) find that neighborhoods with higher density of fast food restaurants are also likely to have higher concentrations of racial minorities and low-income households (see Berger et al., 2019; Block et al., 2004; Powell et al., 2007 for more). Studies also document that areas with greater access to supermarkets are predominantly white (Powell et al., 2007; Richardson et al., 2012). Disparities in weight outcomes across residential locations may reflect underlying differences in individual- or household-level characteristics, apart from the effects of the food environment per se. The key empirical challenge is to isolate the impact of the food environment from these underlying differences.

2.2. Quasi-Experimental Evidence

Despite the abundant descriptive evidence linking obesity to fast food availability, relatively few papers focus on estimating the causal effects. Four key papers use access to highways as an instrument for fast food locations to identify the causal relationship between food environment and obesity outcomes. First, Anderson and Matsa (2011) use distance to interstate highways as an instrument to examine the effect of distance to the nearest fast food restaurant, focusing on rural areas in 11 states. They employ a two-sample instrumental variable technique, using ZIP code centroids for restaurant data and telephone area code centroids for obesity data. They find no significant relationship between distances to the nearest restaurant (both fast food and full-service) and weight outcomes.

Two other studies by Dunn (2010) and Dunn, Sharkey, and Horel (2012), however, find detrimental effects of living near fast food restaurants on obesity outcomes for racial minorities and female populations. Dunn (2010) uses the number of interstate highway exits in each county as an instrument for county-level variation in the number of fast food restaurants. He categorizes counties into urban, rural, and medium-density counties across the nation and finds that obesity risks increase with the number of fast food restaurants, specifically among non-whites and females in medium-density counties. Dunn, Sharkey, and Horel (2012) focus on households in central Texas and use distance to a major highway – including not only interstates but also Texas highways – as an instrument to identify the effects of distance to the nearest fast food and the number of fast food restaurants near home on obesity outcomes. They find that both living closer to the nearest fast food and having more fast food restaurants within 1 mile and within 3 miles from home results in a statistically significant increase in the probability of being obese for non-white residents.

Potential explanations for heterogeneity in the effects of living near fast food across racial and gender subgroups include differential preferences and travel costs. First, the distance elasticity of fast food demand may be higher for minority groups due to differences in preferences. For example, ethnic cuisines differ in key ingredients that may require further travel to particular food outlets (Bitler & Haider, 2011). Easier access to fast food restaurants increases the opportunity cost of traveling further distances to purchase products for ethnic cuisines. The above studies, however, do not provide evidence for the effects of the availability of other food retails, such as large supermarkets, which is a common data limitation for past studies. Further, Dunn et al. (2012) explain that the travel cost may be higher for racial minorities because they are less likely to own vehicles than their white counterparts. If white residents are highly mobile, their exposure to the food environment near home will only take up a small portion of their total food environment exposure. Dunn (2010) also provides a potential explanation that females may respond differently to the presence of fast food due to differences in household responsibilities. In other words, their opportunity cost for traveling further distances may be higher.

Alviola, Nayga, Thomsen, and Smartt (2014) use distance to a major highway as an instrument to examine the causal relationship between school-level food environment and obesity rates. They examine the number of fast food restaurants near high schools in Arkansas and find each additional fast food restaurant within a mile from school increases school-level obesity rates by 1.23 percentage points.

Another notable study by Currie, DellaVigna, Moretti, and Pathania (2010) uses two different approaches to investigate the impact of proximity to fast food on weight outcomes. First, they examine school-level obesity rates for fifth, seventh, and ninth graders in California and compare schools that have any fast food restaurants within a tenth of a mile, a quarter-mile, and a half-mile from school. Here, the identification assumption is that small differences in proximity do not correlate to unobservable differences between the groups. They find that having a fast food restaurant within one tenth of a mile rather than a quarter of a mile from school increases school-level obesity rates by 1.7 percentage points for ninth graders. They find small and statistically insignificant effects for fifth and seventh graders. Second, using birth certificate data in Michigan, New Jersey, and Texas, they examine the impact of living near fast food on weight gain between pregnancies among women who have at least two children. They find smaller yet significant effects of having a fast food restaurant within a half-mile of a residence.

Currie et al. (2010) also suggest the difference in the magnitude of the results between students and mothers is driven by relative travel costs. If traveling the same distance incurs lower travel costs for adults, students will be more affected by fast food restaurants in the immediate proximity. Following this logic, travel costs are likely to be lower for older students than younger students, implying that younger students will be more sensitive to proximity, conditional on autonomy in food consumption decisions. Put differently, among students old enough to purchase their own food, younger students might be more responsive to fast food availability nearby due to difficulties driving, walking, or using public transportation to travel further distances for food. However, the two existing studies on children – Alviola et al. (2014) and Currie et al. (2010) – focus on high school grade students with little attention on *younger children*.

Previous studies also do not provide evidence on the causal impact of the *residential* food environment on childhood obesity. While past studies on student obesity focus on the school food environment, many elementary and middle school students are not allowed to leave school for lunch and are less likely to be affected by the food environment surrounding their school (Mirtcheva & Powell, 2009). This could potentially explain the null effects of fast food restaurants around school on obesity outcomes for younger students in Currie et al. (2010). After school hours, students can substitute home-prepared meals with fast food near home or consume fast food in addition to home-prepared meals. Especially for younger students, who are more likely to attend a school close to home, fast food near home may take up a larger part of their total food environment exposure. Thus, residential food environment has important implications for student food consumption decisions, and its estimated effects for younger students are likely to differ from that of older students.

Finally, previous quasi-experimental studies typically lack data on *non-restaurant food outlets* that may also affect food consumption decisions and obesity outcomes. In particular, supermarkets or corner stores may be alternatives to fast food restaurants, and proximity to these food outlets will shape the cost of purchasing fast food. Furthermore, distance to fast food may be correlated with distance to corner stores with low-quality food sources and inversely correlated with distance to high-quality supermarkets, complicating the interpretation of the coefficients on fast food. In this study, we have data on individual-level weight outcomes for school-aged children in all grades and their food environment around home, including distances to different types of food outlets, which we use to estimate the effects of proximity to fast food on childhood obesity.

3. Public Housing and Residential Location

We focus on students living in public housing, because the institutional setting of public housing – and that of NYC public housing, in particular – provides plausibly random variation in individual proximity to fast food within a development. Public housing is a federally funded housing assistance program, administered and managed by local housing authorities like the NYC Housing Authority (NYCHA). A public housing development typically consists of one or more concentrated blocks of standardized high-rise (and sometimes low-rise) apartment buildings. NYCHA is the nation's largest public housing system, containing 2,418 buildings in 149 developments dispersed across the city's five boroughs – Manhattan, Bronx, Brooklyn, Queens, and Staten Island (NYCHA, 2019). With roughly 174,000 households living in NYC public housing, an average NYCHA development has more than 16 buildings and approximately 150 residents per building.

To be clear, public housing is a place-based housing assistance program, in which program recipients are assigned to specific units that they can either "take or leave." It differs from other tenant-based programs like the housing choice vouchers, which allow households to choose their neighborhoods and housing units in the private market. The assignment process into NYC public housing units makes it difficult for public housing applicants to choose the precise residential location of their preference. Furthermore, most inner-city public housing developments are oversubscribed, requiring local housing authorities to have long waiting lists and systemized processes of assigning tenants to public housing units. There is also a long waiting list to get into NYCHA public housing units. In this section, we describe NYCHA's tenant assignment process that provides tenants little control over their choice of specific buildings, although they can specify some preference over locations.

More specifically, households can list up to two preferred boroughs on the application but are not permitted to list any preference for individual developments or buildings. After receiving applications, NYCHA assigns priority codes to eligible households, based on family size, income, needs (e.g., emergencies), and date of application (NYCHA, 2020). NYCHA then conducts interviews to place households on its waiting list. While the details of the process differ by priority code, all households have limited choice of housing units.

Applicants can select one preferred development during the process, conditional on the development containing an anticipated vacancy.[21] A computer matches applicants to vacant units in the selected development. Applicants can receive up to two offers (i.e., applicants are permitted to reject the initial offer), but applications will be closed if applicants fail to choose a development within 30 days or if applicants reject the second offer (NYCHA, 2020). "Emergency applicants," while prioritized in the tenant selection process, may only select a preferred borough rather than a particular development.[22] They are matched to vacant units in the selected borough "without regard to any preference by the applicant for a particular development in that borough" (NYCHA, 2020). Emergency applicants can also reject their initial offer, but their application will be closed if they reject the second offer. To summarize, the choice of

[21] Development selection should be from one of the two boroughs listed in their initial application form.
[22] Emergency applicants are households with children that are either homeless, victims of domestic violence, or intimidated witnesses, and borough selection should also be from one of the two boroughs listed on their initial application form.

development is constrained by anticipated vacancies around the time of the initial offer, and the choice of particular units or buildings within a development is more explicitly restricted.

A city-wide oversubscription for NYCHA public housing is likely to further discourage applicants from rejecting offers. From time to time, NYCHA closes its waiting list to control the volume of the applications it receives. Therefore, rejecting the second offer would increase households' uncertainty around whether they can create new applications to get back on the waiting list. Previous research suggests only a few households turn down the initial set of offers for housing assistance programs with long waiting lists, since starting over the application may entail a substantial wait for and uncertainty regarding the availability of another unit (Coley et al., 1997; Rosenbaum, 1995; DeLuca & Rosenbaum, 2003).[23] In the past five years, the average time between "date entered waiting list" and the "admission date" for NYCHA public housing has been more than 38 months (HUD, 2019). This process creates random variation in the precise location of a student's residence (and the subsequent food environment) within a public housing development, which we leverage to isolate the causal impact of proximity to fast food on children.

A small number of previous studies exploit the assignment process in public housing to identify causal estimates of neighborhood effects on individual outcomes. Two are particularly relevant. Oreopoulos (2003) focuses on the Toronto public housing program, in which applicants cannot specify development preferences, to examine the effects of neighborhoods on long-run labor market outcomes. Goux and Maurin (2007) focus on public housing in France, where public housing managers have a very limited set of units to offer each year to eligible families, to

[23] Drawing on student-level data in England, Weinhardt (2014) finds that precise timing of moving to neighborhoods with oversubscribed social housing is uncorrelated with any observable individual characteristics, suggesting households that apply for housing assistance programs with long waiting lists are likely to accept available offers regardless of their individual taste.

estimate neighborhood effects on academic success. Thus, both studies leverage the resulting quasi-random assignment to a particular public housing development and, therefore, neighborhood to isolate causal estimates of neighborhood effects. We employ a similar methodology but also exploit the within-development variation in proximity to neighborhood (dis)amenities. To summarize, we exploit the institutional setting of public housing that assigns children in different micro-neighborhood food environments to derive credibly causal estimates of living near fast food restaurants.

4. Data and Sample

4.1. Student-Level Data

Our analyses draw on a rich set of longitudinal, student-level data for NYC public school students, K-12, in AY 2009-2016. Administrative data from the NYC Department of Education (NYCDOE) include student residential location, school attended, socio-demographic variables, such as gender, race/ethnicity, grade, primary language spoken at home, and poverty status,[24] educational program participation (e.g., students with disabilities and English language learners), and critically, student height and weight measures from an annual FitnessGram®. The FitnessGram® measures provide weight and height of students every year, which we use to calculate student body mass index (BMI). We follow the Centers for Disease Control and Prevention guidelines and define students as *obese* if their BMI is at or above the 95th percentile of their age and sex group and *overweight* if their BMI is at or above the 85th percentile. In addition to the two binary weight outcome variables, we calculate the z-score of the BMI (*z-BMI*), standardized by age and sex group, to examine the estimated effects on the weight distribution for later robustness checks. We link student residential location to data on the

[24] Poverty status is defined by whether students were ever eligible for free or reduced-price lunch (household incomes below 185 percent of the federal poverty level) in AY 2001-2016.

locations of NYCHA developments to create an identifier for each public housing development, which we use to derive a set of development fixed effects.

We also link the student-level data to the locations of restaurants and supermarkets. We follow Elbel et al. (2020) to create four food retail outlet variables derived from two data sources. Specifically, we use data on NYC restaurants from the NYC Department of Health and Mental Hygiene, including information on locations and the type of service provided (fast food or wait-service). We calculate the straight-line distance (in miles) between student residential location and the nearest fast food restaurant (*DistFF*) and the nearest wait-service restaurant (*DistWaitService*).[25] We then link to data on the locations and characteristics of food stores from the New York State Department of Agriculture and Markets to calculate distances to the nearest large supermarket (store greater than 6,000 square feet) and the nearest corner store (less than 2,000 square feet), respectively *DistSupermarke*t and *DistCornerStore*. While our analyses focus on these continuous measures of student distance to food outlets, we create a set of binary variables indicating the presence of each food outlet type within 0.1 mile from home (e.g., *AnyFF*) as an alternate specification. We also create density measures by counting the total number of food outlets within 0.1 mile and within 0.25 mile from home (e.g., *NumFF10* and *NumFF25*).

Finally, we calculate the straight-line distance between home and school attended in miles (*DistSch*). Using this, we create an indicator variable, *SchNear,* that identifies those who attend schools within a half-mile from home. Students in kindergarten, first, and second grade who travel less than a half-mile for school do not qualify for district-provided school buses in NYC and are, therefore, more likely to walk to school in the neighborhood. We also create

[25] Street network distances were correlated with straight-line distances at more than 90 percent.

SchFar to identify those who live far away (half-mile or more) from school and are thus eligible for school buses in the early grades. A second set of variables, *SchNear36* and *SchFar36,* is similarly defined using a one-mile threshold, which determines school bus eligibility for students in grades 3-6.[26]

4.2. Sample and Descriptive Statistics

Our analysis focuses on the students living in NYCHA public housing developments. The sample consists of 486,178 observations of K-12 NYC public school students in public housing for AY 2009-2016. Students missing weight and height data or residential location are not included in the sample. We further exclude outliers of non-poor students, who comprise less than 2 percent of the students living in public housing. Table 1 provides summary statistics of our analytic sample in all grades and by grade level. In addition to stratifying by elementary (K-5), middle (6-8), and high school grades (9-12), we separate students in grades K-2 from those in grades 3-5 to explore plausible heterogeneity within elementary school grade kids.

Table 1 shows that 23.2 percent of students in our analytic sample are obese and 40.9 percent are overweight. Obesity rates differ across grade levels, where students in grades 3-5 are more likely to be obese (27 percent) and high school students are less likely to be obese (19.5 percent) than students in other grade levels. A majority of our analytic sample are either Hispanic (47.4 percent) or black (46.2 percent), and less than 10 percent, across all grade levels, are Asian or white. Approximately half of the students are female (51.5 percent). Moreover, students in

---

[26] Half-mile and one-mile thresholds are used by the NYCDOE to determine school bus eligibility (NYCDOE, 2020). K-2 students are eligible for school buses when they live further than half a mile from school, and students in grades 3-6 are eligible when they live more than a mile from school. Students in grades 7-12 are not eligible for school bus regardless of their distance between home and school; however, students in grades 7-8 living in Staten Island would be eligible for school buses at 1 mile. For other types of pupil transportation, students are eligible for half-fare and full-fare MetroCards. Students in grades K-2 are eligible for half-fare if they travel less than 0.5 mile for school and for full-fare if more than 0.5 mile; students in grades 3-6 are eligible for half-fare if they travel between 0.5 to 1 mile for school and for full-fare if they travel more than 1 mile; students in grades 7-12 are eligible for half-fare if they travel between 0.5 to 1.5 mile for school and for full-fare if they travel more than 1.5 mile.

higher grade levels are likely to travel further distances for school. While less than a quarter of elementary school students attend schools more than a half-mile from home, almost 90 percent of high school students attend schools outside a half-mile radius from home. However, there is less, if any, variation in distances to the nearest food outlets across grade levels. On average, students live approximately 0.1 mile (around two city blocks in NYC) from the nearest fast food restaurant regardless of grade level.

Critical to our study is the within-development variation in distances to fast food. To explore this, we plot the distribution of *DistFF* in each of the 139 public housing developments in Figure 1. In this figure, each line shows the range of student distance to the nearest fast food at 5% and 95% of the distribution in a given development. The first range plot presented in Figure 1, for example, shows that one student would have to travel 0.2 mile (around four city blocks) further to reach the nearest fast food restaurant from home compared to another student *in the same development*. The plotted range of *DistFF* within developments suggests the within-development distance between buildings can span multiple blocks and place children in substantially different micro-neighborhood food environments. A decomposition of the variation in *DistFF*, in a one-way analysis of variance (ANOVA), indicates that almost half of the variation (47.8 percent) is within developments and only slightly more (52.2 percent) is between developments.

4.3. Exploring the Within-Development Variation in Local Food Environments

Before turning to models, we explore the empirical support for the claim that the within-development variation in distance between residence and fast food is plausibly random. To do so, we estimate a series of regression models that examine the correlation between distance to the nearest food outlet and student characteristics, using a set of development and year fixed effects.

We use *DistFF*, *DistWaitService*, *DistSupermarket*, and *DistCornerStore* as the outcome and link them to a vector of student demographic variables, including gender, race/ethnicity (Asian, black, or white, using Hispanic as the reference group) and grade level (grades 3-5, grades 6-8, or grades 9-12, using grades K-2 as the reference group).

The results in Table 2 provide little evidence of a meaningful relationship between distance to food outlets and student characteristics. The magnitudes of *all* coefficients are substantively unimportant, ranging from -0.002 to 0.001, although some are statistically significant. The coefficient for *black*, for example, indicates that black students are 0.001 mile, or five feet, further away from the nearest fast food restaurant than Hispanic students living in the same development. This represents one fiftieth of a typical city block in NYC. Similarly, estimates suggest older students (in middle school and high school grades) live 0.001 mile further from the nearest fast food restaurant than younger kids (grades K-2) in the same development. These distances are not economically meaningful and bolster our confidence that the causal interpretation of our estimates is warranted.

5. Empirical Strategy

5.1. Regression Models

As described previously, we exploit the exogenous within-development variation in distance to fast food and identify the effects of proximity to fast food by comparing weight outcomes among students living in the same development but in different buildings (thus with different micro-neighborhood food environments). Our baseline model contains the following elements:

$$Y_{idt} = \beta_0 + \beta_1 \, DistFF_{idt} + \gamma X_{idt} + \delta_d + \tau_t + \varepsilon_{idt} \qquad (1)$$

where Y_{idt} represents the weight outcome (*obese* and *overweight*) of student i in development d in year t. $DistFF_{idt}$ captures student distance to the nearest fast food restaurant in miles. A vector of student characteristics (shown in Table 1) are included in the equation as X_{idt}, and year fixed effects, τ_t, control for secular trends. Finally, δ_d are development fixed effects, such that our coefficient of interest, β_1, is identified by the variation in *DistFF* within developments. An alternate specification includes and controls for student distance to other food outlets (*DistWaitService*, *DistSupermarket*, and *DistCornerStore*), which may also affect the relative travel cost for *DistFF* and child weight outcomes.

We first estimate this baseline model on our full analytic sample of students in all grades (K-12) and then stratify by grade levels to shed light on heterogeneity across grades, as discussed in earlier sections. We then explore differences in the estimated effects of *DistFF* between students who live near enough to school to be in the early grades "walk zone" of a half-mile and those who live farther away with the following model:

$$Y_{idt} = \beta_0 + \beta_1\, DistFF\text{x}SchNear_{idt} + \beta_2\, DistFF\text{x}SchFar_{idt}$$

$$+ \beta_3\, SchNear_{idt} + \gamma X_{idt} + \delta_d + \tau_t + \varepsilon_{idt} \qquad (2)$$

where we fully interact *DistFF* with binary indicators of student distance to school, *SchNear* and *SchFar*, to allow the estimated impact of *DistFF* to vary by student distance to school. Again, we first estimate this model on the full analytic sample and then stratify by grade level, with and without controlling for distances to other food outlets. Following previous studies that find stronger effects of proximity to fast food on obesity outcomes among minorities and women, we also examine heterogeneity by student race/ethnicity and gender.

We also explore the robustness of our results to alternative specifications and measures. First, we re-estimate our models using *z-BMI,* instead of the binary indicators *obese* and *overweight*. Second, we explore alternative ways of capturing the food environment, substituting continuous distance measures with binary indicators, such as *AnyFF*. We also control for the density of food outlets by within different radiuses from home, including *NumFF10* and *NumFF25*. Finally, we use alternative measures for distance to school, replacing *SchNear* and *SchFar* with *SchNear36* and *SchFar36*, constructed using a one-mile threshold, and using the continuous measure of student distance between home and school in miles, *DistSch*, instead of the indicator variables.

6. Results

6.1. Impact of Proximity to Fast Food by Grade Level

Baseline results in Table 3 show the estimated impact of proximity to fast food on student weight outcomes for K-12 students. Consistently negative and statistically significant coefficients for *DistFF* suggest that proximity to fast food increases student probability of being obese and overweight. Indeed, every additional 0.1 mile (or two city blocks) separating the nearest fast food restaurant from a student's residence decreases the probability of being obese by approximately 0.6 percentage points. The effects on overweight range between 0.93 to 1.11 percentage point increases, depending on the inclusion of distances to other food outlets in the model. We see little evidence that proximity to other food outlets matters. Coefficients on the distances to other food outlets (see full results in Table A.1) are small and statistically insignificant.

As described earlier, we estimate the impact of *DistFF* by student grade level and report separate coefficients for students in grades K-2, grades 3-5, grades 6-8, and grades 9-12. Estimates in Table 4 suggest that the baseline effects are largely driven by older elementary school students (in grades 3-5) and middle school students (in grades 6-8). For every 0.1 mile a student lives further away from the nearest fast food, the probability of being obese decreases by 1.39 to 1.42 percentage points and overweight decreases by 1.66 to 1.86 percentage points for students in grades 3-8 (see Table 4 Columns 2 and 4). To understand the magnitude of the effects, consider the group mean obesity rate of 27 percent for older elementary school students and 25.6 percent for middle school students (see Table 1). A 1.39 to 1.42 percentage point increase translates to approximately a 5.4 percent increase in obesity rate for living two blocks closer to the nearest fast food.

In contrast, the estimated effects on K-2 students and high school students are smaller in magnitude and, more importantly, statistically insignificant across all models using different weight outcomes and controls for distance to other food outlets. Students in grades K-2 may not be old enough to exercise independent food consumption decisions regardless of fast food locations near home. High school students, who tend to travel the furthest for school (see Table 1), may have exposure to food environment outside their residential neighborhood and, therefore, appear to be less sensitive to the micro-neighborhood food environments near home.

6.2. Does School Proximity Matter?

Estimates in Table 5 show that the impact of proximity to fast food near home differs by distance to school among students in all grade levels, except for K-2 students. Negative coefficients of *DistFF* for students attending neighborhood schools (coefficient on *DistFF*x*SchNear*) are always larger in magnitude than the corresponding coefficient for students attending schools farther away (*DistFF*x*SchFar*).[27] For example, among students in grade 3-5 (see Table 5 Panel B Columns 1 and 2), the estimated effects of *DistFF*x*SchNear* on obesity is larger by approximately 0.6 percentage points for every 0.1-mile increase compared to those of

[27] For each model in Table 5, we test whether the coefficients for the interaction terms are statistically different from each other and report the p-value of the joint F-test.

*DistFF*x*SchFar* (-0.144 vs. -0.081 and -0.158 vs. -0.095). The impact of living near fast food on obesity outcomes is almost 1.78 times larger for students attending schools nearby. We also find similar patterns for obesity outcomes among middle school students (see Table 5 Panel C). The impact of living 0.1 mile closer to fast food increases probability of obese by 1.43 to 1.68 percentage points for students attending neighborhood schools, approximately 0.5 percentage points larger than those attending schools farther away. These estimates imply that every 0.1 mile closer a student lives to fast food translates into approximately a 6 and 7 percent increase in obesity rates (and a 4.2 and 4.7 percent increase in overweight rates) respectively for older elementary students and middle school students that attend neighborhood schools.

As for high school students, in Panel D, the coefficient on *DistFF*x*SchNear* indicates statistically significant, negative effects on overweight. The estimates suggest living 0.1 mile closer to the nearest fast food increases high school students' probability of being overweight by approximately 1.3 percentage points, and the effect is statistically different from that of *DistFF*x*SchFar.* Thus, even for high school students, those attending neighborhood schools are affected by fast food near home. To understand the magnitude of the effects, consider the base overweight rate of 37 percent for high school students (see Table 1). An increase in the probability of being overweight by 1.3 percentage points represents a 3.5 percent increase in overweight rates.

Overall, the detrimental effects of living near fast food are largest among those students who are most likely to have meaningful autonomy in food decisions and, at the same time, are likely to spend a significant amount of their free time in their residential neighborhood. Results are robust to clustering standard errors at the development level (see Table A.2). Although standard errors are slightly larger, our key coefficients are still statistically significant at conventional levels.

6.3. Heterogeneity by Race and Gender and Robustness Checks

In Table 6, our analyses reveal considerable heterogeneity in impact across demographic groups, consistent with findings from previous research. First, we see negative and statistically significant effects of *DistFF* for black students, with similar evidence for Hispanic students. For black students, living 0.1 mile closer to fast food increases the probability of being obese by 0.9 to 1.05 percentage points and overweight by 1.08 to 1.17 percentage points. For Hispanic students the effects on overweight ranges between 0.8 to 1.28 percentage points. We see little evidence of the effects on weight outcomes among Asian and white students.[28] In Table 7, we report separate results by gender and find that boys are more sensitive to proximity to fast food near home than girls. While this may reflect greater autonomy granted to boys than girls, other underlying mechanisms are possible and warrant further research.

The findings from our series of robustness checks suggest the results are not sensitive to alternative measures and specifications. First, results are robust to measuring weight outcomes using *z-BMI,* rather than indicators for obese or overweight (see Table A.3). Living 0.1 mile closer to the nearest fast food increases student BMI by approximately 0.03 standard deviations, or 4.3 percent of the sample's mean *z-BMI*. Second, results are substantively unchanged by alternative measures of the food environment (see Table A.4). The probability of being obese is 0.6 percentage points higher (and overweight is 0.7 percentage points higher) for students who travel less than 0.1 mile to the nearest fast food restaurant. Third, results are substantively

[28] Note that the differences in effects by race/ethnicity may reflect income differences across racial and ethnic subgroups within our low-income populations. Unfortunately, our data do not include information on household income, but future research exploring the heterogeneity across income groups within public housing and the relationship across racial subgroups is clearly warranted.

unchanged by including controls for density of the fast food restaurants (see Table A.5). Finally, we examine whether alternative specifications for distance to school yield similar results. Models with interaction terms using *SchNear36* and *SchFar36* (see Table A.7) and *DistSch* (see Table A.8) in place of *SchNear* and *SchFar* consistently show that students who travel further distances to school are less likely to be affected by *DistFF*. We also see in Table A.6 that including *DistSch* as a control, instead of interacting *DistSch* with *DistFF*, does not change the coefficients for *DistFF*. In other words, the moderating effects of attending schools nearby on the relationship between proximity to fast food near home and weight outcomes are robust to different specifications for distance to school.

7. Discussion and Policy Implications

A wide range of policymakers, advocates, and "urbanists" blame the ease of access to unhealthy food outlets and particularly fast food as the culprit for high obesity rates among low income, minority children. There are, however, few credibly causal empirical findings on the effects of proximity to fast food on childhood obesity, mainly due to the endogenous nature of fast food locations and scarcity of the requisite micro-data linking children weight to the food environment. In this paper, we overcome the two key empirical obstacles using a detailed set of individual-level data on students living in public housing and exploiting their quasi-random assignment into micro-neighborhood food environments. Specifically, we use administrative data on NYC public school students living in public housing and link their weight outcomes and residential locations with all restaurant locations in NYC. We then leverage the plausibly random within-development variation in distance between residence and fast food generated by NYCHA's tenant assignment process to derive credibly causal estimates of the effects of living near fast food. To our knowledge, this is the first paper to use this particular identification strategy.

Our results suggest significant deleterious effects of proximity to fast food for student weight outcomes, with the largest effects among students in grades 3-8 attending neighborhood schools. Economic theory predicts that individuals with relatively higher travel costs are more sensitive to fast food availability in close proximity. Currie et al. (2010), for instance, find that proximity to fast food has larger effects on high school students than pregnant mothers, providing lower travel costs for adults as a potential explanation. In this paper, we exploit a detailed set of data on public housing students in all grades, and our results support the theory that living near fast food has larger impacts on younger students who are likely to have higher travel costs but old enough to make independent food purchasing decisions. In addition to heterogeneity across grade levels, students attending neighborhood schools are also likely to face higher costs traveling outside their residential neighborhood to purchase food, compared to students who attend schools far from home. We also find that students who travel shorter distances to school are more sensitive to fast food proximity in their micro-neighborhood environment. For students in grades 3-8 attending neighborhood schools, the probability of being obese (overweight) increases up to 1.7 (2.1) percentage points for every one tenth mile decrement in distance between home and fast food. These are sizable magnitudes, roughly representing a 12 percent increase in obesity rates (9.4 percent for overweight) for a four-block reduction in distance to the nearest fast food.

We note two key limitations of our study results. First, the location of fast food restaurants may be related to the availability of other neighborhood amenities that may affect student weight outcomes such that our estimates would reflect the combined effects of proximity to fast food and proximity to other unobserved amenities. However, it is reassuring – although not dispositive – that our results are robust to including controls for proximity to other food outlets. Second, our work focuses on public school children living in NYC public housing, a population disproportionately black, Hispanic, and urban. Investigating whether and how proximity to fast food affects higher-income students or those living in lower-density suburban and rural areas with greater reliance on cars remains for future research.

Our study results are particularly relevant to place-based interventions that attempt to limit unhealthy food outlets in an urban context to reduce the prevalence of obesity in low-income, minority neighborhoods. We suggest such interventions might include zoning regulations that restrict openings of fast food outlets in designated areas of a city. In a different vein, school policies regarding the quality or price of school lunch or "open-campus" policies governing student's ability to exit during school lunch periods might also be relevant. In summary, our findings suggest fast food locations near residence have sizable impacts on childhood obesity and warrant the attention of policymakers hoping to identify policy levers to reduce access to or consumption of fast food among poor, urban children.

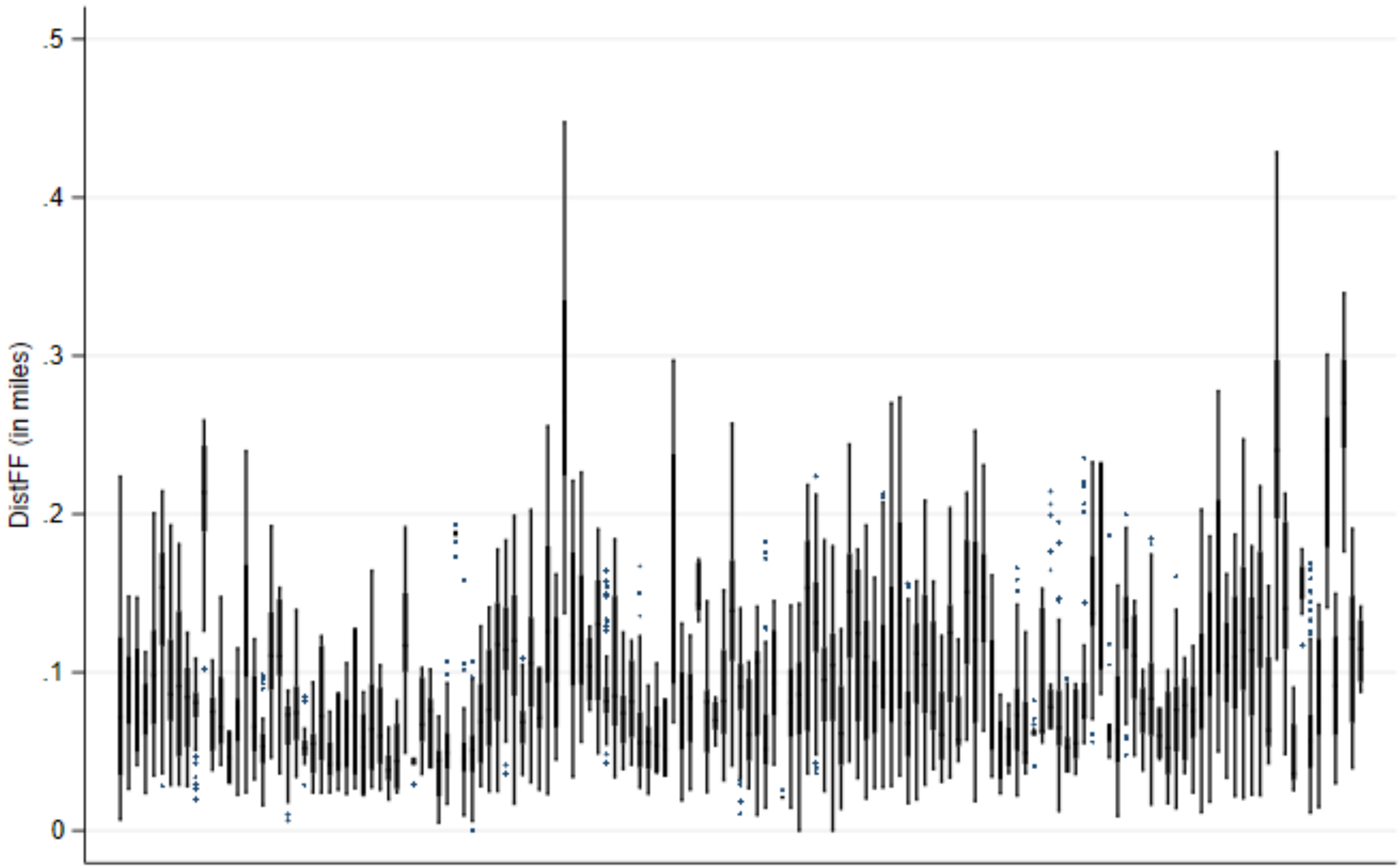
Tables and Figures

Table 1. Mean student characteristics for all students and by grade level

	All grades (1)	Grades K-2 (2)	Grades 3-5 (3)	Grades 6-8 (4)	Grades 9-12 (5)
Weight outcomes					
Obese	0.232	0.214	0.270	0.256	0.195
	(0.422)	(0.410)	(0.444)	(0.436)	(0.396)
Overweight	0.409	0.380	0.448	0.445	0.370
	(0.492)	(0.485)	(0.497)	(0.497)	(0.483)
Distance to the nearest food outlet and school attended					
DistFF	0.099	0.099	0.099	0.100	0.099
	(0.056)	(0.056)	(0.056)	(0.057)	(0.056)
DistWaitService	0.212	0.213	0.212	0.215	0.211
	(0.168)	(0.169)	(0.167)	(0.172)	(0.164)
DistSupermarket	0.187	0.189	0.187	0.188	0.186
	(0.145)	(0.147)	(0.144)	(0.146)	(0.143)
DistCornerStore	0.096	0.096	0.096	0.097	0.096
	(0.067)	(0.067)	(0.067)	(0.068)	(0.066)
DistSch	1.480	0.613	0.720	1.139	3.053
	(2.211)	(1.458)	(1.542)	(1.706)	(2.663)
SchNear	0.512	0.817	0.770	0.461	0.111
	(0.500)	(0.387)	(0.421)	(0.499)	(0.314)
SchNear36	0.641	0.893	0.854	0.685	0.235
	(0.480)	(0.309)	(0.353)	(0.465)	(0.424)
Student characteristics					
Female	0.515	0.508	0.520	0.520	0.513
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Hispanic	0.474	0.474	0.475	0.477	0.469
	(0.499)	(0.499)	(0.499)	(0.500)	(0.499)
Asian	0.047	0.039	0.041	0.047	0.059
	(0.213)	(0.194)	(0.199)	(0.211)	(0.236)
Black	0.462	0.467	0.466	0.461	0.456
	(0.499)	(0.499)	(0.499)	(0.499)	(0.498)
White	0.017	0.020	0.018	0.015	0.015
	(0.128)	(0.138)	(0.131)	(0.123)	(0.121)
Grade	5.895	1.029	3.993	7.035	10.269
	(3.605)	(0.808)	(0.818)	(0.817)	(1.093)
Student with disability	0.188	0.157	0.201	0.203	0.190
	(0.391)	(0.364)	(0.401)	(0.402)	(0.392)
English language learner	0.075	0.089	0.084	0.069	0.063
	(0.264)	(0.284)	(0.278)	(0.253)	(0.244)
N	486,178	111,477	113,070	119,687	141,944

Notes: Standard deviations are shown in parentheses. Sample consists of NYC public school students, ever eligible for free and reduced-price lunch and living in NYCHA public housing, for AY 2009-2016. All distances are in miles.

Figure 1. Range of student-level distance to the nearest fast food restaurant by public housing development



Notes: Each range plot shows student distance to the nearest fast food at the 5% and 95% of the distribution in a given development and the outliers through dots. Sample consists of NYC public school students, ever eligible for free and reduced-price lunch and living in NYCHA public housing, for AY 2009-2016.

Table 2. Relationship between student demographic characteristics and proximity to food outlets

Dependent variable:	DistFF (1)	DistWaitService (2)	DistSupermarket (3)	DistCornerStore (4)
Female	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Hispanic	-	-	-	-
Asian	0.000 (0.000)	-0.001* (0.001)	-0.002*** (0.001)	0.000 (0.000)
Black	0.001*** (0.000)	0.001** (0.000)	0.000 (0.000)	0.001*** (0.000)
White	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.000 (0.001)
Grades K-2	-	-	-	-
Grades 3-5	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Grades 6-8	0.001*** (0.000)	-0.000 (0.000)	0.001*** (0.000)	0.000* (0.000)
Grades 9-12	0.001*** (0.000)	-0.001 (0.000)	0.001*** (0.000)	0.000 (0.000)
N	486,178	486,178	486,178	486,178
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Each column is a different regression. Sample consists of NYC public school students, ever eligible for free and reduced-price lunch and living in NYCHA public housing, for AY 2009-2016.

Table 3. Baseline impact of proximity to fast food on weight outcomes, K-12

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
DistFF	-0.058*** (0.016)	-0.062*** (0.019)	-0.093*** (0.018)	-0.111*** (0.022)
N	486,178	486,178	486,178	486,178
Dist. to other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each column is a different regression. Distance to other food include mile-distances to the nearest wait-service restaurant, supermarket, and corner stores. Student characteristics include gender, race/ethnicity, grade, primary language spoken at home, special education, and limited English proficiency status. Sample consists of NYC public school students, ever eligible for free and reduced-price lunch and living in NYCHA public housing, AY 2009-2016.

Table 4. Baseline impact of proximity to fast food on weight outcomes *by grade level*

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
Panel A: Grades K-2 only				
DistFF	0.000 (0.032)	0.023 (0.039)	-0.050 (0.037)	-0.065 (0.046)
N	111,477	111,477	111,477	111,477
Panel B: Grades 3-5 only				
DistFF	-0.129*** (0.034)	-0.142*** (0.042)	-0.149*** (0.038)	-0.166*** (0.047)
N	113,070	113,070	113,070	113,070
Panel C: Grades 6-8 only				
DistFF	-0.115*** (0.032)	-0.139*** (0.040)	-0.153*** (0.037)	-0.186*** (0.045)
N	119,687	119,687	119,687	119,687
Panel D: Grades 9-12 only				
DistFF	-0.003 (0.027)	-0.001 (0.033)	-0.039 (0.033)	-0.044 (0.040)
N	141,944	141,944	141,944	141,944
For all Panels:				
Dist. to other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Each column in each panel is a different regression. See notes in Table 3 for variable and sample descriptions.

Table 5. Impact of proximity to fast food on weight outcomes *by grade level and whether a student attends a school within 0.5 mile from home*

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
Panel A: Grades K-2 only				
DistFFxSchNear	-0.004	0.019	-0.059	-0.074
	(0.032)	(0.039)	(0.038)	(0.046)
DistFFxSchFar	0.017	0.041	-0.014	-0.029
	(0.039)	(0.045)	(0.046)	(0.053)
P-value for joint F-test	0.451	0.446	0.179	0.180
N	111,477	111,477	111,477	111,477
Panel B: Grades 3-5 only				
DistFFxSchNear	-0.144***	-0.158***	-0.174***	-0.191***
	(0.035)	(0.043)	(0.039)	(0.047)
DistFFxSchFar	-0.081**	-0.095**	-0.073	-0.090*
	(0.040)	(0.047)	(0.045)	(0.053)
P-value for joint F-test	0.026	0.026	0.046	0.045
N	113,070	113,070	113,070	113,070
Panel C: Grades 6-8 only				
DistFFxSchNear	-0.143***	-0.168***	-0.175***	-0.208***
	(0.036)	(0.043)	(0.041)	(0.048)
DistFFxSchFar	-0.096***	-0.120***	-0.139***	-0.171***
	(0.034)	(0.041)	(0.039)	(0.047)
P-value for joint F-test	0.061	0.059	0.203	0.195
N	119,687	119,687	119,687	119,687
Panel D: Grades 9-12 only				
DistFFxSchNear	-0.031	-0.029	-0.126**	-0.129**
	(0.043)	(0.047)	(0.052)	(0.057)
DistFFxSchFar	-0.001	0.001	-0.032	-0.036
	(0.027)	(0.034)	(0.033)	(0.041)
P-value for joint F-test	0.389	0.390	0.031	0.031
N	141,944	141,944	141,944	141,944
For all Panels:				
Dist. to other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Each column in each panel is a different regression. See notes in Table 3 for variable and sample descriptions. We test whether the coefficients for DistFFxSchNear and DistFFxSchFar are statistically different from each other and present the p-value of the joint F-test.

Table 6. Impact of proximity to fast food on weight outcomes *by race/ethnicity*

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
Panel A: Asian only				
DistFF	-0.082 (0.060)	-0.058 (0.076)	-0.014 (0.080)	-0.016 (0.102)
N	22,912	22,912	22,912	22,912
Panel B: Hispanic only				
DistFF	-0.016 (0.024)	-0.045 (0.030)	-0.080*** (0.028)	-0.128*** (0.034)
N	230,252	230,252	230,252	230,252
Panel C: Black only				
DistFF	-0.105*** (0.022)	-0.090*** (0.027)	-0.117*** (0.026)	-0.108*** (0.031)
N	224,794	224,794	224,794	224,794
Panel D: White only				
DistFF	0.232* (0.121)	0.225 (0.146)	0.064 (0.141)	0.018 (0.170)
N	8,090	8,090	8,090	8,090
For all Panels:				
Dist. to other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Each column in each panel is a different regression. See notes in Table 3 for variable and sample descriptions.

Table 7. Impact of proximity to fast food on weight outcomes *by gender*

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
Panel A: Male only				
DistFF	-0.113*** (0.023)	-0.112*** (0.028)	-0.108*** (0.026)	-0.116*** (0.032)
N	235,740	235,740	235,740	235,740
Panel B: Female only				
DistFF	-0.004 (0.022)	-0.015 (0.026)	-0.074*** (0.025)	-0.104*** (0.031)
N	250,438	250,438	250,438	250,438
For all Panels:				
Dist. to other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Each column in each panel is a different regression. See notes in Table 3 for variable and sample descriptions.

Appendix

Table A.1. Full results for the impact of proximity to food outlets on weight outcomes

Dependent variable:	Obese (1)	Overweight (2)
DistFF	-0.062***	-0.111***
	(0.019)	(0.022)
DistWaitSerivce	0.004	-0.004
	(0.008)	(0.009)
DistSupermarket	-0.000	-0.002
	(0.009)	(0.011)
DistCornerStore	0.003	0.036*
	(0.017)	(0.020)
N	486,178	486,178
Student characteristics	Y	Y
Year FX	Y	Y
Development FX	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See notes in Table 3.

Table A.2. Impact of proximity to fast food on weight outcomes, K-12, clustered standard errors

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
DistFF	-0.058**	-0.062*	-0.093***	-0.111***
	(0.029)	(0.033)	(0.034)	(0.042)
N	486,178	486,178	486,178	486,178
Dist. to other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Standard errors are clustered at the development level and are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See notes in Table 3 for variable and sample descriptions.

Table A.3. Impact of proximity to fast food on z-BMI

Dependent variable:	z-BMI (1)	z-BMI (2)
DistFF	-0.266***	-0.245***
	(0.043)	(0.053)
N	486,178	486,178
Dist. to other food	-	Y
Student characteristics	Y	Y
Year FX	Y	Y
Development FX	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table A.4. Impact of proximity to fast food on weight outcomes *using binary distance measures*

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
AnyFF	-0.006*** (0.002)	-0.006*** (0.002)	-0.007*** (0.002)	-0.007*** (0.002)
N	486,178	486,178	486,178	486,178
Dist. to other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Each column is a different regression. See notes in Table 3 for variable and sample descriptions.

Table A.5. Impact of proximity to fast food on weight outcomes, *controlling for density measures*

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
Panel A: Baseline proximity measure controlling for density				
DistFF	-0.069*** (0.017)	-0.069*** (0.021)	-0.112*** (0.020)	-0.127*** (0.024)
NumFF10	-0.001** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
NumFF25	0.000* (0.000)	0.000 (0.000)	0.000* (0.000)	-0.000 (0.000)
N	486,178	486,178	486,178	486,178
Panel B: Alternative proximity measure controlling for density				
AnyFF	-0.008*** (0.002)	-0.008*** (0.002)	-0.009*** (0.002)	-0.009*** (0.002)
NumFF10	-0.001*** (0.000)	-0.001*** (0.000)	-0.001** (0.000)	-0.001*** (0.000)
NumFF25	0.000** (0.000)	0.000 (0.000)	0.000* (0.000)	0.000 (0.000)
N	486,178	486,178	486,178	486,178
For all Panels:				
Dist. to other food	-	Y	-	Y
Num. of other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Each column in each panel is a different regression. See notes in Table 3 for variable and sample descriptions.

Table A.6. Impact of proximity to fast food on weight outcomes *by grade level, controlling for distance to school*

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
Panel A: Grades K-2 only				
DistFF	0.001 (0.032)	0.024 (0.039)	-0.049 (0.037)	-0.065 (0.046)
DistSch	-0.001 (0.001)	-0.001 (0.001)	-0.002** (0.001)	-0.002** (0.001)
N	111,477	111,477	111,477	111,477
Panel B: Grades 3-5 only				
DistFF	-0.128*** (0.034)	-0.142*** (0.042)	-0.149*** (0.038)	-0.166*** (0.047)
DistSch	-0.002** (0.001)	-0.002** (0.001)	-0.001 (0.001)	-0.001 (0.001)
N	113,070	113,070	113,070	113,070
Panel C: Grades 6-8 only				
DistFF	-0.113*** (0.032)	-0.138*** (0.040)	-0.152*** (0.037)	-0.185*** (0.045)
DistSch	-0.002*** (0.001)	-0.002*** (0.001)	-0.001* (0.001)	-0.001* (0.001)
N	119,687	119,687	119,687	119,687
Panel D: Grades 9-12 only				
DistFF	-0.003 (0.027)	-0.001 (0.033)	-0.039 (0.033)	-0.044 (0.040)
DistSch	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
N	141,944	141,944	141,944	141,944
For all Panels:				
Dist. to other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Each column in each panel is a different regression. See notes in Table 3 for variable and sample descriptions.

Table A.7. Impact of proximity to fast food on weight outcomes *by grade level and whether a student attends a school within 1 mile from home*

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
Panel A: Grades K-2 only				
DistFFxSchNear36	0.003 (0.032)	0.026 (0.039)	-0.047 (0.038)	-0.063 (0.046)
DistFFxSchFar36	-0.021 (0.044)	0.002 (0.049)	-0.068 (0.052)	-0.083 (0.058)
N	111,477	111,477	111,477	111,477
Panel B: Grades 3-5 only				
DistFFxSchNear36	-0.134*** (0.035)	-0.148*** (0.043)	-0.161*** (0.039)	-0.179*** (0.047)
DistFFxSchFar36	-0.103** (0.044)	-0.116** (0.050)	-0.088* (0.049)	-0.105* (0.056)
N	113,070	113,070	113,070	113,070
Panel C: Grades 6-8 only				
DistFFxSchNear36	-0.115*** (0.034)	-0.139*** (0.041)	-0.153*** (0.038)	-0.186*** (0.046)
DistFFxSchFar36	-0.115*** (0.036)	-0.139*** (0.043)	-0.153*** (0.041)	-0.185*** (0.049)
N	119,687	119,687	119,687	119,687
Panel D: Grades 9-12 only				
DistFFxSchNear36	-0.039 (0.033)	-0.038 (0.038)	-0.086** (0.040)	-0.091* (0.047)
DistFFxSchFar36	0.007 (0.028)	0.008 (0.034)	-0.027 (0.033)	-0.031 (0.041)
N	141,944	141,944	141,944	141,944
For all Panels:				
Dist. to other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Each column in each panel is a different regression. See notes in Table 3 for variable and sample descriptions.

Table A.8. Impact of proximity to fast food on weight outcomes *by grade level and distance to school*

Dependent variable:	Obese (1)	Obese (2)	Overweight (3)	Overweight (4)
Panel A: Grades K-2 only				
DistFF	-0.002 (0.033)	0.021 (0.040)	-0.059 (0.039)	-0.075 (0.047)
DistFFxDistSch	0.004 (0.012)	0.004 (0.012)	0.016 (0.015)	0.016 (0.015)
DistSch	-0.001 (0.002)	-0.001 (0.002)	-0.004* (0.002)	-0.004* (0.002)
N	111,477	111,477	111,477	111,477
Panel B: Grades 3-5 only				
DistFF	-0.142*** (0.036)	-0.156*** (0.043)	-0.159*** (0.040)	-0.176*** (0.048)
DistFFxDistSch	0.018 (0.013)	0.018 (0.013)	0.013 (0.015)	0.013 (0.015)
DistSch	-0.004** (0.002)	-0.004** (0.002)	-0.002 (0.002)	-0.002 (0.002)
N	113,070	113,070	113,070	113,070
Panel C: Grades 6-8 only				
DistFF	-0.147*** (0.035)	-0.171*** (0.042)	-0.161*** (0.040)	-0.194*** (0.048)
DistFFxDistSch	0.027** (0.011)	0.027** (0.011)	0.007 (0.012)	0.007 (0.012)
DistSch	-0.006*** (0.001)	-0.006*** (0.001)	-0.002 (0.002)	-0.002 (0.002)
N	119,687	119,687	119,687	119,687
Panel D: Grades 9-12 only				
DistFF	-0.039 (0.034)	-0.038 (0.039)	-0.080* (0.041)	-0.085* (0.048)
DistFFxDistSch	0.011* (0.007)	0.011* (0.007)	0.012 (0.008)	0.013 (0.008)
DistSch	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
N	141,944	141,944	141,944	141,944
For all Panels:				
Dist. to other food	-	Y	-	Y
Student characteristics	Y	Y	Y	Y
Year FX	Y	Y	Y	Y
Development FX	Y	Y	Y	Y

Notes: Robust standard errors are shown in parentheses (*** p<0.01, ** p<0.05, * p<0.1).
Each column in each panel is a different regression. See notes in Table 3 for variable and sample descriptions.

References

Allcott, H., Diamond, R., Dubé, J. P., Handbury, J., Rahkovsky, I., & Schnell, M. (2019). Food deserts and the causes of nutritional inequality. *The Quarterly Journal of Economics, 134*(4), 1793-1844.

Alviola IV, P. A., Nayga Jr, R. M., Thomsen, M. R., Danforth, D., & Smartt, J. (2014). The effect of fast-food restaurants on childhood obesity: a school level analysis. *Economics & Human Biology*, *12*, 110-119.

Anderson, M. L., & Matsa, D. A. (2011). Are restaurants really supersizing America? *American Economic Journal: Applied Economics*, *3*(1), 152-88.

Athens, J. K., Duncan, D. T., & Elbel, B. (2016). Proximity to fast-food outlets and supermarkets as predictors of fast-food dining frequency. *Journal of the Academy of Nutrition and Dietetics*, *116*(8), 1266-1275.

Bellettini, G., & Kempf, H. (2013). Why not in your backyard? On the location and size of a public facility. *Regional Science and Urban Economics*, *43*(1), 22-30.

Berger, N., Kaufman, T. K., Bader, M. D., Rundle, A. G., Mooney, S. J., Neckerman, K. M., & Lovasi, G. S. (2019). Disparities in trajectories of changes in the unhealthy food environment in New York City: A latent class growth analysis, 1990–2010. *Social Science & Medicine*, *234*, 112362.

Bitler, M., & Haider, S. J. (2011). An economic view of food deserts in the United States. *Journal of Policy Analysis and Management*, *30*(1), 153-176.

Block, J. P., Scribner, R. A., & DeSalvo, K. B. (2004). Fast food, race/ethnicity, and income: a geographic analysis. *American Journal of Preventive Medicine*, *27*(3), 211-217.

Bridger, T. (2009). Childhood obesity and cardiovascular disease. *Pediatrics & Child Health, 14*(3), 177-182.

Chen, D., Jaenicke, E. C., & Volpe, R. J. (2016). Food environments and obesity: household diet expenditure versus food deserts. *American Journal of Public Pealth*, *106*(5), 881-888.

Coley, R. L., Sullivan, W. C., & Kuo, F. E. (1997). Where does community grow? The social context created by nature in urban public housing. *Environment and Behavior*, *29*(4), 468-494.

Currie, J., DellaVigna, S., Moretti, E., & Pathania, V. (2010). The effect of fast food restaurants on obesity and weight gain. *American Economic Journal: Economic Policy*, *2*(3), 32-63.

Cutler, D. M., Glaeser, E. L., & Shapiro, J. M. (2003). Why have Americans become more obese? *Journal of Economic Perspectives*, *17*(3), 93-118.

Davis, B., & Carpenter, C. (2009). Proximity of fast-food restaurants to schools and adolescent obesity. *American Journal of Public Health*, *99*(3), 505-510.

DeLuca, S., & Rosenbaum, J. E. (2003). If low-income blacks are given a chance to live in white neighborhoods, will they stay? Examining mobility patterns in a quasi-experimental program with administrative data. *Housing Policy Debate*, *14*(3), 305-345.

Dubowitz, T., Ghosh-Dastidar, M., Eibner, C., Slaughter, M. E., Fernandes, M., Whitsel, E. A., Bird, C. E., Jewell, A., Margolis, K. L., Li, W., Michael, Y. L., Shih, R. A., Manson, J. E., & Escarce, J. J. (2012). The Women's Health Initiative: The food environment, neighborhood socioeconomic status, BMI, and blood pressure. *Obesity*, *20*(4), 862-871.

Dunn, R. A. (2010). The effect of fast-food availability on obesity: an analysis by gender, race, and residential location. *American Journal of Agricultural Economics*, *92*(4), 1149-1164.

Dunn, R. A., Sharkey, J. R., & Horel, S. (2012). The effect of fast-food availability on fast-food consumption and obesity among rural residents: an analysis by race/ethnicity. *Economics & Human Biology*, *10*(1), 1-13.

Elbel, B., Tamura, K., McDermott, Z. T., Wu, E., & Schwartz, A. E. (2020). Childhood Obesity and the Food Environment: A Population-Based Sample of Public School Children in New York City. *Obesity*, *28*(1), 65-72.

Galvez, M. P., Morland, K., Raines, C., Kobil, J., Siskind, J., Godbold, J., & Brenner, B. (2008). Race and food store availability in an inner-city neighbourhood. *Public Health Nutrition, 11*(6), 624-631.

Gibson, D. M. (2011). The neighborhood food environment and adult weight status: estimates from longitudinal data. *American journal of public health*, *101*(1), 71-78.

Goux, D., & Maurin, E. (2007). Close neighbours matter: Neighbourhood effects on early performance at school. *The Economic Journal*, *117*(523), 1193-1215.

Lewis, L. B., Sloane, D. C., Nascimento, L. M., Diamant, A. L., Guinyard, J. J., Yancey, A. K., & Flynn, G. (2005). African Americans' access to healthy food options in South Los Angeles restaurants. *American Journal of Public Health*, *95*(4), 668-673.

McCarthy, P. S. (1999). Public policy and highway safety: a city-wide perspective. *Regional Science and Urban Economics*, *29*(2), 231-244.

Mehta, N. K., & Chang, V. W. (2008). Weight status and restaurant availability: a multilevel analysis. *American Journal of Preventive Medicine*, *34*(2), 127-133.

Mirtcheva, D. M., & Powell, L. M. (2009). Participation in the national school lunch program: Importance of school-level and neighborhood contextual factors. *Journal of School Health*, *79*(10), 485-494.

Morland, K. B., & Evenson, K. R. (2009). Obesity prevalence and the local food environment. *Health & Place*, *15*(2), 491-495.

Morland, K., Roux, A. V. D., & Wing, S. (2006). Supermarkets, other food stores, and obesity: the atherosclerosis risk in communities study. *American Journal of Preventive Medicine*, *30*(4), 333-339.

New York City Department of Education (2020). Transportation Eligibility. Retrieved from https://www.schools.nyc.gov/school-life/transportation/bus-eligibility.

New York City Housing Authority (2019). NYCHA 2019 Fact Sheet. Retrieved from https://www1.nyc.gov/assets/nycha/downloads/pdf/NYCHA-Fact-Sheet_2019_08-01.pdf

New York City Housing Authority (2020). Tenant Selection and Assignment Plan. Retrieved from https://www1.nyc.gov/assets/nycha/downloads/pdf/TSAPlan.pdf

Ogden, C. L., Carroll, M.D., Fakhouri, T. H., Hales, C. M., Fryar, C. D., Li, X., & Freedman, D. S. (2018). Prevalence of obesity among youths by household income and education level of head of household–United States 2011-2014. Morbidity and Mortality Weekly Report, 67(6), 186.

Oreopoulos, P. (2003). The long-run consequences of living in a poor neighborhood. *The Quarterly Journal of Economics*, *118*(4), 1533-1575.

Powell, L. M., Chaloupka, F. J., & Bao, Y. (2007). The availability of fast-food and full-service restaurants in the United States: associations with neighborhood characteristics. *American Journal of Preventive Medicine*, *33*(4), S240-S245.

Powell, L. M., Slater, S., Mirtcheva, D., Bao, Y., & Chaloupka, F. J. (2007). Food store availability and neighborhood characteristics in the United States. *Preventive Medicine, 44*(3), 189-195.

Richardson, Andrea S., Janne Boone-Heinonen, Barry M. Popkin, and Penny Gordon-Larsen. "Are neighbourhood food resources distributed inequitably by income and race in the USA? Epidemiological findings across the urban spectrum." *BMJ open* 2, no. 2 (2012): e000698.

Rosenbaum, J. E. (1995). Changing the geography of opportunity by expanding residential choice: Lessons from the Gautreaux program. *Housing Policy Debate*, *6*(1), 231-269.

Sahoo, K., Sahoo, B., Choudhury, A. K., Sofi, N. Y., Kumar, R., & Bhadoria, A. S. (2015). Childhood obesity: causes and consequences. *Journal of Family Medicine and Primary Care*, *4*(2), 187.

U.S. Department of Housing and Urban Development. (2019). A picture of subsidized households, 2015-2019 [Data file]. Retrieved from https://www.huduser.gov/portal/datasets/assthsg.html.

Weinhardt, F. (2014). Social housing, neighborhood quality and student performance. *Journal of Urban Economics*, *82*, 12-31.

Jeehee Han

426 Eggers Hall
Syracuse University
Syracuse, NY 13244

Phone: +1-847-912-9611
Email: jhan09@syr.edu
sites.google.com/view/jeeheehan

Education

Syracuse University, Syracuse, NY — Expected July 2021
Ph.D. in Public Administration (Fields: Social Policy, Public Finance)

Columbia University, New York, NY — Feb. 2016
M.A. in Quantitative Methods in the Social Sciences

Northwestern University, Evanston, IL — June 2014
B.A. in Economics and Sociology *with Honors*

Peer-Reviewed Publications

Han, J., Schwartz, A. E., & Elbel, B. (2020). "Does Proximity to Fast Food Cause Childhood Obesity? Evidence from Public Housing." *Regional Science and Urban Economics*, Vol. 84.

Awards and Fellowships

Horowitz Foundations for Social Policy ($7,500), 2021

Lincoln Institute Scholars Program ($550), Lincoln Institute of Land Policy, 2021

SKPA Student Paper Award ($300), American Society for Public Administration, 2021

Student-Led Paper Competition Award ($300), North American Regional Science Council, 2020

Larry D. Schroeder Award for Excellence in PhD Research ($1,350), Syracuse University, 2018

Senior Thesis with Distinction Award, Northwestern University, 2014

Research and Travel Funding

Spencer D. Parratt Summer Research Award ($4,800 in total), Syracuse University, 2018-2021

Roe L. Johns Student Travel Grants ($150), Association for Education Finance and Policy, 2020

Department Conference Travel Funding ($3,245 in total), Syracuse University, 2017-2020

Graduate Student Organization Travel Grant ($450 in total), Syracuse University, 2018-2019

Conference Matching Travel Award ($250), Columbia University, 2015

Policy Briefs

Han, J., Schwartz, A. E., & Elbel, B. (2020). Does Proximity to Fast Food Increase Incidences of Childhood Obesity? Research Brief #29. Lerner Center Public Health Research Brief Series.

Conference Presentations

"The Spillover Effects of the Source of Income Anti-Discrimination Laws on Public Housing."

The Ohio State University PhD Conference on Real Estate and Housing, Virtual, April 2021

American Society for Public Administration (ASPA), Virtual, April 2021

"Does Proximity to Fast Food Cause Childhood Obesity? Evidence from Public Housing."

Association for Public Policy Analysis and Management (APPAM), Virtual, November 2020

North American Regional Science Council (NARSC), Virtual, November 2020

Urban Economics Association (UEA), Philadelphia, October 2019

American Real Estate and Urban Economics Association, Washington, DC, May 2019

"The Impact of Public Housing on Student Outcomes."

Association for Education Finance and Policy (AEFP), Virtual, March 2020

APPAM, Denver, November 2019

"Stretched to the Limit? Shared Buildings, Overcrowded Schools, and Underserved Students."

AEFP, Kansas City, March 2019

Association for Budgeting and Financial Management, Denver, October 2018

Professional Experience

The Maxwell School Center for Policy Research (CPR), Syracuse, NY — 2016 - Present
Graduate Research Associate

NYC Department of Housing Preservation and Development, New York, NY — Summer 2015
Research Assistant, Housing and Neighborhood Studies

U.S. Department of Education, Washington, DC — Summer 2013
Policy Intern, Program Implementation and Management Services

Teaching Experience

Education Policy / Economics of Education (Elective MPA/MA/PhD Course)

Guest Lecturer, Spring 2018, Spring 2020, and Fall 2020 (Evaluation: **4.9/5**)

Fundamentals of Policy Analysis (Core Executive MPA Course)

Tutor (On-campus and Online), Fall 2018, Spring 2019, and Spring, 2020 (Evaluation: **4.7/5**)
Teaching Assistant and Guest Lecturer, Fall 2017 and Fall 2019 (Evaluation: **4.6/5**)

Public Budgeting (Core MPA Course)

Teaching Assistant, Summer 2018 and Summer 2019
Guest Lecturer, Fall 2018 (Evaluation: **4.3/5**)