

**SUPERMARKET ACCESS AND CHILDHOOD BODYWEIGHT:  
EVIDENCE FROM STORE OPENINGS AND CLOSINGS**

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Working Paper  
October 2016

# **SUPERMARKET ACCESS AND CHILDHOOD BODYWEIGHT: EVIDENCE FROM STORE OPENINGS AND CLOSINGS**

**Abstract:** The food environment is increasingly hypothesized to cause obesity. This study investigates the impacts of access to supermarkets, the primary source of healthy foods, on the bodyweight outcomes of children. The empirical analysis uses a statewide individual-level panel data set covering health screenings of public schoolchildren along with annual georeferenced business lists and examines the effect of supermarket openings and closings. There is little overall impact in either case, but supermarket openings are found to moderately reduce the bodyweight of low-income children. (*JEL* Q18 I18)

## I. INTRODUCTION

There is a clear need for an improved understanding of childhood obesity in many developed countries, especially the causal factors of weight gain. In the United States, nearly one in five children is now obese (Ogden et al. 2014), facing increased health risks that extend into adulthood (Serdula et al. 1993; Biro and Wien, 2010) as well as associated medical costs (Trasande and Chatterjee 2009; Cawley and Meyerhoefer 2012). While scholarly investigations have been active for decades, obesity prevalence has been inadequately explained by individual-level factors (Garner and Wooley 1991; Cummins and Macintyre 2006). Researchers have become increasingly aware that the complex social and physical contexts that play a role in individual decision making could affect health outcomes (Diez-Roux 1998). The commercial food environment, in particular, is receiving growing attention (see Walker et al. 2010; Caspi et al. 2012 for recent reviews). It is hypothesized that access to healthy foods, or exposure to unhealthy foods, would affect bodyweight and so the impacts of food retail outlets on bodyweight are increasingly the topic of study. To date, studies have emphasized supermarkets (Lopez 2007; Schafft et al. 2009; Thomsen et al. 2016), fast-food restaurants (Currie et al. 2010; Dunn 2010; Alviola et al. 2014), and convenience stores (Morland et al. 2006; Bodor et al. 2010).

As the dominant feature of the commercial food environment, supermarkets are the most important provider of daily foods (United States Census Bureau 2011). They are the primary source of healthy foods such as fresh fruits and vegetables. Therefore, easy access to these stores may contribute to better diets and lower bodyweight. For this reason, residents with limited access to supermarkets may presumably procure a larger proportion of daily foods from alternative food retailers such as convenience stores that carry energy-dense, less healthy foods

(Morland et al. 2006). Bereft of healthy foods, these people are believed to face increased risk of weight gain. This argument has led to concerns that some regions are “food deserts” where limited access to supermarkets could adversely impact diet quality. Food deserts have received increasing scholarly attention, yet empirical findings are mixed (Schafft et al. 2009; Walker et al. 2010; Caspi et al. 2012; Alviola et al. 2013, Thomsen et al. 2016).

On the other hand, it is argued that rising obesity in the United States could be a result of reductions in real food prices that are partly induced by logistical innovations of major supermarkets (Chou et al. 2004; Hausman and Leibtag 2007). Hence, better access to supermarkets can be associated with lower food prices, thereby increasing food consumption and energy intake. Evidence in favor of this argument is provided by Courtemanche and Carden (2011), who find that Walmart Supercenters lead to increases in the bodyweight of nearby residents. Moreover, in addition to healthy foods such as fresh fruits and vegetables, supermarkets also stock an abundance of processed, energy-dense foods at price points lower than most alternative food retailers, which may stimulate the consumption of such foods. In this regard, there is empirical evidence that an increased market share of supercenters reduces purchase healthfulness for groceries (Volpe et al. 2013).

These competing arguments suggest that the impact of supermarkets on bodyweight is complicated and further evidence is needed to understand the linkage between the commercial food environment and health. There have been recent calls for experimental methods and causal evidence linking environmental factors to health outcomes (Institute of Medicine 2010). However, such evidence is largely absent in the existing commercial food environment literature due to data limitations. In an attempt to fill this void, we extend this literature by providing some population-wide evidence of the weight impact of supermarkets on children. In contrast to

existing studies focusing on food deserts alone (e.g., Schafft et al. 2009; Alviola et al. 2013, Thomsen et al. 2016), we consider the broader issue of supermarket access, and identify its causal impacts on the bodyweight of children by exploiting variation of store openings and closings.

This article extends a recent study by Thomsen et al. (2016) who specifically investigate the impacts of food deserts on the bodyweight of young children (kindergarten, grades one, two and four). They show that exposure to food deserts is positively associated with the bodyweight of those children, but are unable to conclude causality because changes in food desert status are mainly a result of home relocation, an endogenous event. This article differs from the Thomsen et al. (2016) study in important ways. Instead of focusing narrowly on food deserts, which are defined as low-income areas that lack access to supermarkets, we focus more broadly on the issue of supermarket access and do not impose the additional neighborhood income tests. Second, our sample is restricted to children that were in stable residential locations so that the only source of variation in supermarket access is due to the opening or closing of a nearby supermarket.

We utilize an individual-level panel dataset that covers the body mass index (BMI) measures of public schoolchildren in Arkansas, United States and that has been georeferenced by year to different types of food retail establishments. As compared to Thomsen et al. (2016), our analysis covers a much broader range of children from pre-kindergarten through grade twelve, yet focuses on a narrower time span during which children were subject to annual as opposed to biennial BMI screenings. The BMI measures are based on measured heights and weights and this is a clear advantage as errors in self reporting BMI can be significant and can substantially affect empirical findings (Cawley et al. 2015). We focus on supermarket openings and closings that

affect only a portion of the population and those children that remained in stable residences, which enables us to employ a difference-in-differences (DID) strategy to identify the causal relationship. Two DID models are estimated. The first compares the BMI z-scores of children who experienced supermarket openings and therefore started to have supermarket access (the treatment group) with the BMI z-scores of children who never had access to supermarkets (the control group). The second compares the BMI z-scores of children who experienced supermarket closings and therefore lost supermarket access (the treatment group) with the BMI z-scores of children who always had access to supermarkets (the control group).

We find little overall impact of supermarket openings and closings at the population level. However, in contrast to Thomsen et al. (2016), there is evidence that supermarket openings slightly reduce the BMI z-scores of low-income children in this broader sample. To minimize concerns about the validity of our identification strategy, we further conduct a series of robustness checks to rule out alternative mechanisms that could cause estimation biases. These exercises consistently support the main results.

The rest of the article is structured as follows. Section 2 describes the data. Section 3 discusses our empirical methods. Section 4 presents baseline results as well as robustness checks. Section 5 discusses our findings in context. Section 6 finally concludes.

## **II. DATA**

Annual BMI screening of public schoolchildren in Arkansas started in the 2003/2004 academic year in partial fulfillment of the requirements of Act 1220, passed in 2003 by the Arkansas General Assembly. Children were screened annually through the 2006/2007 school year and biennially in subsequent years. The screenings are conducted by trained personnel in

public schools across the state, and data are maintained by the Arkansas Center for Health Improvement (ACHI), who worked with us to match children's residences to the locations of food retail establishments and to the 2009 American Community Survey (ACS) by census block group.<sup>1</sup> Additional information about the Arkansas BMI dataset is provided by Justus et al. (2007) and Thomsen et al. (2016). Our analysis of these data took place on a secure computer in ACHI's offices in Little Rock, Arkansas.

The outcome variable of interest is the age- and gender-specific BMI z-scores computed according to the guidelines of the United States Centers for Disease Control and Prevention. The use of BMI z-scores rather than direct BMI values helps to control for possible confounding factors, such as age-related adiposity rebound, puberty, and gender differences in growth trajectories. In addition to the BMI measures, the age, gender, race, school, and school lunch qualification (whether the child was eligible for free or reduced-price school lunch) of each child are also present in the data.

We analyze a sample covering from 2003/2004 to 2006/2007 academic years, during which all public schoolchildren were screened annually. By restricting the sample to the 2003/2004 and 2006/2007, we are able to construct a balanced panel containing four consecutive BMI observations per child. We further restrict the sample to only include children that were observed in the same residential location during all four years of observation. This helps to minimize the confounding impacts that may have given rise to home relocation decisions. The available cohort-grade structure is depicted in Table 1.

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<sup>1</sup> Annual food store location data were obtained from Dun and Bradstreet (D&B), which are commonly used in the commercial food environment literature (e.g. Powell et al. 2007; Zick et al. 2009; Bader et al. 2010).

The accessibility of supermarkets is then defined by a binary variable indicating the existence of a supermarket within a given radius of the residence of that child.<sup>2</sup> The United States Department of Agriculture (USDA) uses the cutoff of one (ten) mile(s) for urban (rural) areas and define low-access communities as those census tracts without supermarkets within such distances.<sup>3</sup> This pair of radii, however, appears to be less appropriate in our finer-scale data. Specifically, our data show that in urban areas, 21.03%, 56.93% and 86.86% of the observations have access to supermarkets within one half mile, one mile and two miles, while in rural areas, 13.06%, 46.80% and 83.80% of the observations have access to supermarkets within two miles, five miles and ten miles. Hence, one mile for urban areas and five miles for rural areas appear to be best available midpoints for the sake of variation. Accordingly, we define a residential address as one with supermarket access if the exact distance to the nearest supermarket is less than one (five) mile(s) for urban (rural) areas, and as one without supermarket access if the distance is larger than those cutoffs. That said, we conduct robustness checks using the USDA cutoffs.

Given the four-year study period (from 2003/2004 to 2006/2007 school years), we specifically consider supermarket openings and closings that occurred between 2004/2005 and 2005/2006 school years, and exclude children who experienced supermarket openings or closings in the other years during the study period. This design ensures two periods of observation before the change in supermarket access, and another two periods of observation after the change. The goal is to provide sufficient time for any impact of supermarket openings or closings to manifest itself in terms of BMI z-scores. Furthermore, access to two observations

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<sup>2</sup> We define supermarkets as food stores that contain a fresh produce department. We examined the names and trade styles in the store location data to identify chain stores and affiliated grocers that provide fresh produce, and further used phone calls and Google Street-view images to verify store formats in questionable cases.

<sup>3</sup> See <http://apps.ams.usda.gov/fooddeserts/fooddeserts.aspx>.



prior to the change allows us to assess the parallel trend assumption essential to DID identification, which we will further address in Section 5.

While our emphasis is on the relationship between supermarket access and childhood bodyweight, we acknowledge that other features of the commercial food environment could also play a role in bodyweight production. Analogous to our measurement of supermarket access, we further account for access to convenience stores and fast-food restaurants around the residence of each child. A dummy access indicator for each type of store is based on a half mile radius for urban residences and a two mile radius for rural residences because these are the best available midpoints for variation. Formal consideration of these additional store types will also be discussed in Section 5.

Table 2 presents the summary statistics for our samples. Between the 2004/2005 and 2005/2006 school years, supermarket openings resulted in the reclassification of 1,064 children that previously had no supermarket access, while supermarket closings resulted in the reclassification of 1,210 children that previously had access to a nearby supermarket to no access. The pairwise t-tests suggest that there are few statistical differences in the average BMI z-scores by treatment status. In comparison to the control group, children who experienced supermarket openings or closings are less likely to qualify for free school lunch and are less likely to be of Hispanic ethnicity. Moreover, children who experienced supermarket openings are more likely to be from more affluent communities than children who never had supermarket access, while children who experienced supermarket closings are more likely to be from less affluent communities and rural communities than children who always had supermarket access. Despite statistical significance, most of these differences are small in magnitude, implying that the children have fairly similar characteristics regardless of treatment status.

### III. ANALYTICAL FRAMEWORK

Two DID models are estimated using the supermarket access measure for each child. First, we compare the BMI z-scores of children who were affected by supermarket openings, and therefore started to have supermarket access, to those of children who never had access to nearby supermarkets during the study period. Second, we compare the BMI z-scores of children who were affected by supermarket closings, and therefore lost access to a nearby supermarket, to those of children who always had access to nearby supermarkets during the study period. According to our data, supermarket openings and closings that change the store access status almost always involved only one supermarket.<sup>4</sup>

#### A. Identification Strategy

In either the case of supermarket openings or closings, the baseline DID regression model can be specified as:

$$(1) W_{it} = \beta_0 + \beta_1 D_i P_t + \beta_2 D_i + \beta_3 P_t + \mathbf{X}'_{it} \beta_4 + \epsilon_{it}$$

where  $W_{it}$  is the weight outcome of child  $i$  in school year  $t$ , measured by BMI z-score.  $D_i$  is a binary indicator of the treatment status, which equals one if child  $i$  belongs to the treatment group, or zero if he/she belongs to the control group.  $P_t$  is a binary indicator of the timing of observation, which equals one for post-treatment observations, or zero for pre-treatment observations.  $\mathbf{X}_{it}$  is a vector of covariates, and  $\epsilon_{it}$  is the idiosyncratic error. In the empirical estimation,  $\mathbf{X}_{it}$  includes the age, gender, whether the child qualifies for free school lunch, and two dummy variables indicating whether the child is African American or of Hispanic ethnicity.

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<sup>4</sup> The opening or closing of a supermarket around the residence of the child would not affect his/her access status if an alternative supermarket was also accessible. The openings or closings of multiple supermarkets at the same time are very rare.

The coefficient of primary interest is  $\beta_1$ , which identifies the respective impact of supermarket openings and closings. Empirical estimates of  $\beta_1$ , however, can be underestimated since households without nearby access to supermarkets in the given radii may still purchase foods from supermarkets at a longer distance, which may attenuate any possible impacts. This gives rise to a broader issue concerning the intensity of treatment, where the behavioral responses of households induced by the openings or closings of a supermarket can be less meaningful if alternative supermarkets are also accessible with only minor changes in travel distance and/or travel time.<sup>5</sup> To address this, we extend the baseline models and consider two additional analyses where the treatment group is restricted to children who observed supermarket openings or closings that involved the only available supermarket within larger radii of two miles (urban) and ten miles (rural), while keeping the control group the same (this is referenced hereafter as the “extended model”). This design would largely control for possible confounding impacts from alternative supermarkets and therefore reduce the downward estimation biases.

Since we have four rounds of observations for each child, the empirical estimation of Eq. (1) is subject to serial correlations that can result in estimation bias, which needs to be formally addressed. Several possible empirical strategies to correct for serial correlations are discussed in Bertrand et al. (2004), including block-bootstrap error correction, arbitrary variance-covariance matrix correction, empirical variance-covariance matrix correction, and aggregating the time series information. In a series of Monte Carlo simulations with placebo treatments inserted into real data, Bertrand et al. (2004) further show that all these methods perform well if the number of groups (i.e. clusters of individuals that observe the same policy treatment) is sufficiently large,

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<sup>5</sup> In fact, we have also examined cases where the opening or closing of supermarkets do not change the access status of children. Specifically, we investigated whether the increases or decreases in the number of accessible supermarkets, occurring between 2004/2005 and 2005/2006 school years, would affect BMI z-scores. However, we find little evidence that these changes in the number of stores matter.

while only the last method performs satisfactorily for a small number of groups. Unlike many policy interventions targeted at certain regions, the “groups” are difficult to define in our case because there is no natural cluster within the treatment and control groups. Even children in the same community might have different treatment status because the exact distance from each residence to the nearest supermarket utilized that we use in the analysis varies across households. We therefore opt to aggregate the time series information to correct for serial correlation over time. Specifically, we average the data before and after the treatment and estimate Eq. (1) on the averaged BMI z-scores in a panel of length two.<sup>6</sup>

### *B. Treatment exogeneity*

The estimate of  $\beta_1$  can be interpreted as causal if two identification assumptions hold. First, the openings and closings of supermarkets are exogenously determined for each household and therefore each child  $i$  (treatment exogeneity). Second, the counterfactual trend in the BMI z-scores of the treated children, were the treatment not received, would have been the same as the untreated children (the parallel trend assumption). Although we will empirically test the parallel trend assumption in Section 5, treatment exogeneity is not directly testable, and is worth some more discussion.

At the household level, it is unlikely that any single household is important enough to meaningfully affect store locations. Moreover, few households possess sufficient foreknowledge of future supermarket openings or closings that could factor into their residential location decisions. Therefore, treatment exogeneity at the household level should be reasonably satisfied. Still, there is a possibility that residents from a community could exhibit some common features

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<sup>6</sup> Impact results are dubiously more statistically significant without the aggregation of the time series information. This pattern is discussed in Bertrand et al. (2014) as a result of underestimated standard errors of the DID estimator.

that result in both supermarket opening/closing and weight changes of children. It could specifically be the case if the opening/closing of a supermarket occurs as a result of catering/failure to cater the specific food preferences of consumers in certain communities. To minimize this concern, we re-estimate Eq. (1) using a series of homogenized subsamples that may maintain different consumption behaviors. Specifically, we assess samples homogenized by neighborhood income, vehicle ownership status, and urbanity.

We subsample by income because income is an important socioeconomic indicator and low income is a feature of food deserts (Lopez 2007; Schafft et al. 2009; Thomsen et al. 2016). Therefore, we consider the median household income of each census-block group (from 2009 ACS), and define low (high) income communities as census-block groups with median household incomes below (above) the statewide median income. We also evaluate community economic situation using vehicle ownership rate, as the distance to supermarkets may be more of an issue for households without a vehicle (Fitzpatrick et al. 2016). The proportion of residents without vehicles are also retrieved from the 2009 ACS, and the full sample is broken into two subgroups divided by the statewide median vehicle ownership rate. We further differentiate children living in rural and urban residences, as rural and urban households may exhibit varying food procurement practices as well as differences in vehicle dependence. The subsample estimations will also serve the dual purpose of testing any impact heterogeneity among different population groups. In Section 5, we further apply alternative DID regression as well as matching methods to establish our main results.

#### IV. BASELINE RESULTS

Table 3 presents the BMI impact estimates of supermarket openings and closings for all children as well as different subsamples. All children in the full sample are assigned to subsample classified by neighborhood income, vehicle ownership rate, and urbanity. Robust standard errors are reported in each case.

At the population level, the overall impacts are small and statistically insignificant, but the subsample estimates do exhibit some interesting patterns. Specifically, supermarket openings are found to significantly reduce the BMI z-scores of children from communities with low household income and low vehicle ownership rates, or the low-income children in general (as these two subsamples largely overlap). In the baseline model, a new supermarket reduces the BMI z-scores of low-income children by 0.090-0.096 standard deviations. The extended model further confirms this impact, yielding slightly larger estimates. These findings provide some support to the argument that better access to supermarkets (and therefore healthy foods) may reduce the bodyweight of children. It is possible that the nearby supermarket openings could have substantially shifted the food purchase and consumption patterns of the low-income population towards healthy foods, and therefore reduced the bodyweight of children.

Unlike the subsample impacts of supermarket openings, few subsamples show significant impacts of supermarket closings. The only exception is that in the extended model, supermarket closings lead to an increase of BMI z-score by 0.044 standard deviations for low-income children. Nevertheless, this impact is nonexistent in the baseline model. We speculate that, unlike the opening of a new supermarket that can induce an increase in demand for relatively healthier food given the improved shopping convenience, the closing of an existing supermarket

may less likely alter established food purchasing patterns, especially those based on food traditions, tastes and preferences of food healthfulness.

In sum, although the overall BMI impacts of supermarket openings or closings are small and statistically insignificant, there is evidence that supermarket access matters among low-income children. These findings imply that healthy food access could contribute to bodyweight reduction. We now turn to robustness check procedures to further establish the validity of these estimates.

## V. ROBUSTNESS CHECKS

In addition to subsample estimation, we further apply two strategies to address concerns about endogeneity of supermarket openings and closings. In the first strategy, we estimate our baseline model and extended model with an alternative DID specification augmented by community and year fixed effects. The community fixed effects control for unobserved time-invariant community characteristics (e.g. similar lifestyles). The year fixed effects further takes into account any macroeconomic dynamics that could affect the bodyweight of children (e.g. changes in employment opportunities). The augmented DID regression model is specified as:

$$(2) \quad W_{it} = \gamma_0 + \gamma_1 D_i P_t + \mathbf{X}'_{it} \gamma_2 + \eta_c + \phi_t + \epsilon_{ict}$$

where  $\eta_c$  is the community fixed effects and  $\phi_t$  is the year fixed effects, with other terms defined in Eq. (1). Concerns about any systematic differences among communities that could have contributed to the estimated BMI impacts should be minimized if our main results can be replicated by the estimation of Eq. (2).

In the second strategy, we alternatively use difference-in-differences matching (DID matching) techniques to estimate the impacts of supermarket openings and closings. The DID

matching estimator compares the conditional before-after outcomes of the treatment group with those of the control group (Heckman et al. 1997). It relaxes the linear functional form required in regression, and it re-weights the observations in the control group using semiparametric methods (Smith and Todd 2005). Changes in BMI z-scores before and after the treatment are then matched using a set of observed characteristics. Using subscripts  $\tau'$  and  $\tau$  to denote the (aggregated) before-treatment period and after-treatment period, the DID matching estimator is computed as:

$$(3) \hat{\Delta} = \frac{1}{N} \sum_i \{W_{it}^1 - \hat{E}(W_{it'}^0 | \Pr(\mathbf{X}_{it}), D_i = 0)\} - \frac{1}{N} \sum_i \{W_{it}^0 - \hat{E}(W_{it'}^0 | \Pr(\mathbf{X}_{it}), D_i = 0)\}$$

where the superscript 1 or 0 indicates that child  $i$  belongs to the treatment group or control group, respectively;  $\hat{E}(\cdot)$  is a nonparametric weighting function that can be estimated with matching methods such as nearest neighbor or kernel density; and  $\Pr(\cdot)$  is the propensity score function which estimates the probability that child  $i$  observes either supermarket opening or closing based on observed characteristics. We specifically consider propensity score matching because it is equivalent to directly matching observed characteristics in all their dimensions while it avoids the curse of dimensionality that prevents successful matching (Rosenbaum and Rubin 1983). Under the identification assumption that the BMI z-scores of children are independent of supermarket openings or closings conditional on the observed characteristics, the DID matching estimator unbiasedly identifies the average treatment effect on the treated. If supermarket openings and closings can be considered as random, it further identifies the population average treatment effect. Therefore, DID matching should be able to replicate our regression-based impact estimates if treatment exogeneity actually holds (i.e. unobservables do not affect treatment status).



Table 4 presents the impact estimates from these procedures, namely alternative DID regressions with community and year fixed effects, DID nearest neighbor matching and DID kernel matching for a total of three procedures. In general, these estimates are very similar to our main results in terms of impact magnitudes and statistical significance. There is little impact at the population level. For supermarket openings, it is seen again that improvements in store access reduce the BMI z-scores of low-income children. In the extended model, even the overall impact estimated by DID nearest neighbor matching is statistically significant at the 5% level (middle panel). In a few cases, the impacts on children from communities with low vehicle ownership rates lose statistical significance at the 5% level (upper and lower panels), but their magnitudes change little and both remain significant at 10% level. On the other hand, the impacts of supermarket closings again appears to be less robust. Despite the few discrepancies, the results suggest that there is little evidence against treatment exogeneity, providing fairly strong support to our main results.

The second threat to the validity of our results concerns whether the parallel trend assumption is satisfied, which is essential to DID regression identification. The parallel trend assumption asserts that the counterfactual trend in the BMI z-scores of children who observed supermarket openings (closings) would have been the same as the children who never (always) had supermarket access had there not been supermarket openings (closings). Violation of this assumption can lead to biased estimates, which might have resulted from the intrinsic differences that could have produced different BMI trends of children in the treatment and control groups over time rather than from supermarket openings or closings felt by the children in the treatment group.

We assess the validity of the parallel trend assumption through the performance of placebo DID regressions with the two rounds of observations, namely 2003/2004 and 2004/2005 school years, before the supermarket openings or closings that we consider (between 2004/2005 and 2005/2006 school years). Without time series aggregation (as applied to our main DID regressions), this pre-treatment panel provides richer information about any possible discrepancies in BMI trends before treatment and control groups. Each placebo DID regression is specified exactly the same as in Eq. (1), while we consider a placebo treatment between 2003/2004 and 2004/2005 school years to the children in the real treatment group who finally observed such changes a year later, and redo the analysis. Since our samples do not include supermarket openings and closings between these two years by construction, the DID interaction terms should be zero. Statistical significance in these placebo DID regressions, if any, would imply the violation of the parallel trends assumption. Impact estimates are presented in Table 5. In all cases, we see no statistically significant difference between the BMI z-scores between children in the treatment and control groups, suggesting that the parallel trend assumption is reasonably satisfied, and lending credence to our identification strategy and main results.

We further perform two additional robustness exercises to check the possible existence of alternative mechanisms that could have contributed to the above estimated BMI impacts. The first procedure investigates possible confounding impacts of the school food environment. School meals are an important food source for youth, and possible differences across schools may confound the estimated impacts of supermarket openings and closings. To address this issue, we estimate an alternative DID regression model which is similar to that specified in Eq. (2) but is now augmented by school and grade fixed effects instead of community and year fixed effects, in hope of capturing any time-invariant heterogeneity in school food practices.

In addition to supermarkets, other features of the commercial food environment could also play an unrecognized role in affecting the bodyweight of children. We formally control for access to convenience stores and fast food restaurants as both have received growing attention in recent literature (e.g. Morland et al. 2006; Dunn 2010; Alviola et al. 2014). Similar to the supermarket access measure, we construct two binary access indicators that equals one if there exist(s) one or more stores of either type within a half-mile (two-mile) radius in an urban (rural) setting. These cutoffs are the best available midpoints that allow maximal variation in the store access measures in both cases. They enter Eq. (1) as additional covariates which vary not only across individuals but also over time (with the openings and closings of these stores as the source of variation).

Table 6 reports the impact estimates from these two procedures. Most estimates with statistical significance are also numerically close to our main results. Although the impact of supermarket openings on children from communities with low vehicle ownership rates loses statistical significance at the 5% level in the baseline model estimation, there are only trivial changes in their magnitudes and both estimates are still significant at the 10% level. The impacts of supermarket openings in the extended model exhibit even higher similarities to our main findings. On the other hand, the impacts of supermarket closings are less robust.

The final robustness check concerns the distance used to define supermarket access. As noted above, our definition of supermarket access differs from that of USDA where residences one (ten) mile(s) away from the nearest supermarket in an urban (rural) area are labeled as low access. We further check if our results hold with an alternative supermarket access measure defined using the USDA radius cutoffs. Table 7 presents the impact estimates. Although the magnitudes of most estimates with statistical significance are much smaller than our main

results, they still appear to be consistent enough to our main results, especially with greatly reduced variation in the supermarket access measure.

## **VI. DISCUSSION**

We find little overall impact of supermarket access on the bodyweight of children. However, at the subsample level, supermarket openings are found to have small weight-reducing impacts for low-income children as measured by both community-level income status and vehicle ownership rates. These results hold across a series of robustness checks, and consistently suggest that appropriate access to healthy foods does play a desirable role in the bodyweight production of these children.

Further insights can be obtained by placing the significant subsample impacts in context. Our main results suggest that supermarket openings can reduce BMI-z-scores for low-income children by 0.090 to 0.096 standard deviations. Consider a boy of 11.5 years (138 months, which is close to the average age of our full sample). A boy 4 feet 10 inches in height with a weight of 92.5 pounds would have a BMI z-score of 0.684, which is near our sample average. A decrease in BMI z-score of 0.090-0.096 standard deviations would translate into a weight reduction of roughly 1.3 to 1.4 pounds with no change in height. Therefore, the BMI impacts are not large in terms of bodyweight, especially given the prominent role of supermarkets in the commercial food environment.

Going into this study, we do not have strong a priori hypotheses on the signs of the BMI impacts of supermarket openings and closings. On one hand, appropriate access to supermarkets may reduce bodyweight by providing access to healthy foods. On the other hand, lower price points and the abundance of unhealthy foods in large package sizes in supermarkets could

contribute to increases in bodyweight. These mechanisms could come into play simultaneously; hence our estimates could be the net impacts of multiple conflicting mechanisms. While our analysis does not directly address these possible underlying mechanisms (which is neither within the scope of the study nor feasible given our current data), our subsample results suggest that access to healthy foods can matter more on balance. Moreover, since our primary finding is mainly about the desirable impacts of supermarket openings, these estimates are likely the lower bounds of the true impacts because the alternative mechanisms regarding income effects and provision of unhealthy foods in supermarkets would unambiguously increase bodyweight, and thus could possibly offset the true impacts of supermarket openings.

It is noteworthy that most consistent findings come from the subsample of lower-income children. This is in contrast to Thomsen et al. (2016) who found no BMI impact among children switching out of food deserts by reason of supermarket openings. Although our estimated BMI impacts are small, they are still important given the vulnerability of these children. National-level evidence shows that from 2003 to 2007, obesity prevalence increased by 10% for all children in the United States but increased by 23% for children from low-income households (Singh et al. 2010). In another nationally representative sample, the rates of severe obesity were approximately 1.7 times higher among poor children and adolescents aged 2 to 19 years (Skelton et al. 2009). Low-income communities are often food deserts (Beaulac et al. 2009), where available healthy foods are more expensive (Drewnowski 2010), and are of poorer quality (Andreyeva et al. 2008). It is for these reasons that these communities are increasingly found to observe higher childhood obesity rates (Lopez 2007; Schafft et al. 2009; Ghosh-Dastidar et al. 2014). Regarding supermarket openings, there is evidence that new supermarket entries and increased supermarket competition reduce food prices (Basker and Noel 2009), and improve

food quality (Matsa 2011). On the food demand side, it is further found that supermarket access is associated with increased daily consumption of fruits and vegetables among food stamp recipients (Rose and Richards 2004). Hence, improvements in supermarket access in low-income communities could enhance healthy food access in all these aspects, which could then potentially reduce the bodyweight of children in those communities.

Despite the moderate magnitudes of BMI impacts on low-income children, they still provide some reliable information for policy decisions. There is no simple answer to dealing with childhood obesity, which is certainly a joint product of numerous factors. Admittedly, access to healthy foods is only one possible factor that could affect the bodyweight of children. That said, our study does confirm its role for low-income children, who are among the neediest members of society for health improvement.

## **VII. CONCLUDING REMARKS**

We investigate the possible relationship between supermarket access and childhood bodyweight using variation of supermarket openings and closings. At the population level, we see little evidence that supermarket access affects the BMI z-scores of children. However, supermarket openings are found to have moderate weight-reducing impacts on low-income children. Such impacts are highly significant and robust across a series of robustness checks. The estimated BMI impacts finally translate into a one or two pound bodyweight reduction which, though not large, is likely a conservative estimate and still of policy significance when specifically felt by low-income children.

Our findings that improvements in healthy food access may have desirable impacts on the bodyweight of low-income children could further justify the consideration of the commercial

food environment as an added dimension in public policy designs for childhood obesity prevention. However, as the benefits may not be equally shared among different socioeconomic groups, we do not directly see the cost effectiveness of broadly stimulating new supermarket operations from the social welfare perspective. Rather, our results could point to the need for better access to healthy foods specifically targeting low-income children. Also, educational interventions targeting both children and parents should keep receiving policy attention, especially for low-income communities and those with larger presence of young children. While our findings do not speak directly to the merits of these strategies, they could still contribute to more informed policy decisions.

As one of the first studies assessing the weight impacts of supermarket access on children, our study has several limitations. Given data constraints, we are only able to estimate the net impacts and are unable to disentangle the possible confounding mechanisms of supermarket access. We are also prevented from assessing impact heterogeneity among racial/ethnic groups given their limited presence in the treatment groups. That said, we do provide some reliable findings that supermarket access does matter for certain vulnerable children.

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**TABLE 1**  
**Cohort-Grade Structure of Constructed Panel Data**

Year	Number of observation	2003/2004	2004/2005	2005/2006	2006/2007
Cohort 1	1,988	Pre-K <sup>1</sup>	K <sup>1</sup>	1	2
Cohort 2	34,564	K <sup>1</sup>	1	2	3
Cohort 3	34,548	1	2	3	4
Cohort 4	36,648	2	3	4	5
Cohort 5	32,468	3	4	5	6
Cohort 6	30,884	4	5	6	7
Cohort 7	29,084	5	6	7	8
Cohort 8	27,832	6	7	8	9
Cohort 9	26,416	7	8	9	10
Cohort 10	22,420	8	9	10	11
Cohort 11	16,272	9	10	11	12

<sup>1</sup> Pre-K denotes pre-kindergarten, K denotes kindergarten.

**TABLE 2**  
**Summary Statistics<sup>1</sup>**

	All observations	Supermarket opening analysis sample		Supermarket closing analysis sample	
	(n=293,124) <sup>3</sup>	Treatment group <sup>2</sup> (n=4,256) <sup>3</sup>	Control group <sup>2</sup> (n=126,836) <sup>3</sup>	Treatment group <sup>2</sup> (n=4,840) <sup>3</sup>	Control group <sup>2</sup> (n=143,144) <sup>3</sup>
BMI z-score	0.684 (1.042)	0.671 (1.051)	0.694 (1.061)	0.688 (1.047)	0.678 (1.064)
Age (month)	137.874 (33.962)	135.672 (34.905)	137.835 (36.718)	137.463 (34.460)	137.986 (36.969)
Gender (male=1; female=0)	0.519 (0.500)	0.511 (0.500)	0.524 (0.499)	0.521 (0.500)	0.514 (0.500)
Free lunch (qualified=1; otherwise=0)	0.322 (0.424)	0.297 (0.429)	0.313 * (0.452)	0.305 (0.464)	0.331 ** (0.480)
African American (dummy indicator)	0.135 (0.318)	0.146 (0.353)	0.137 (0.341)	0.125 (0.319)	0.133 (0.435)
Hispanic (dummy indicator)	0.044 (0.163)	0.035 (0.157)	0.042 * (0.202)	0.033 (0.180)	0.049 ** (0.254)
Median household income (thousand USD) <sup>4</sup>	42.443 (15.221)	43.231 (17.299)	41.844 ** (15.143)	41.548 (14.416)	42.903 ** (17.912)
Median vehicle ownership rate (%) <sup>4</sup>	93.814 (5.032)	95.487 (5.146)	95.029 (6.192)	90.819 (5.458)	92.641 ** (8.725)
Rural residence (dummy indicator) <sup>5</sup>	0.433 (0.418)	0.436 (0.442)	0.447 (0.425)	0.455 (0.437)	0.421 ** (0.422)

<sup>1</sup> All measures are averaged over the four years of study. Standard deviations are represented in parentheses. \* and \*\* indicate statistical significance at 5% and 1% levels, respectively, from pairwise *t*-tests between treatment and control groups within each subsample.

<sup>2</sup> The treatment group in the supermarket opening analysis subsample consists of children who had no supermarket access in the first two periods of observation while gained such access in the last two periods of observation due to supermarket openings. The control group in the supermarket closing analysis subsample consists of children who never had supermarket access during the study period. The treatment group in subsample 2 consists of children who previously had supermarket access in the first two periods of observation while lost such access in the last two periods of observation due to supermarket closings. The control group in subsample 2 consists of children who always had supermarket access during the study period.

<sup>3</sup> The number of children is one fourth the number of observations presented here as each children is observed in four consecutive years.

<sup>4</sup> These census-block group level statistics are from the 2009 ACS.

<sup>5</sup> Rural-urban classification are based on 2009 ACS.

**TABLE 3**  
**Baseline BMI Impact Estimates of Supermarket Openings and Closings<sup>1,2</sup>**

	Baseline model		Extended model	
	Openings	Closings	Openings	Closings
Full sample	-0.038 (0.032)	0.001 (0.030)	-0.051 (0.031)	0.004 (0.027)
High income ( $\geq$ statewide median)	-0.024 (0.056)	-0.003 (0.044)	-0.029 (0.043)	-0.015 (0.058)
Low income ( $<$ statewide median)	-0.096 (0.034) **	-0.021 (0.034)	-0.113 (0.036) **	0.044 (0.021) *
High vehicle ownership rate ( $\geq$ statewide median)	-0.012 (0.046)	0.019 (0.048)	-0.008 (0.025)	0.011 (0.055)
Low vehicle ownership rate ( $<$ statewide median)	-0.090 (0.045) *	0.001 (0.039)	-0.096 (0.039) *	-0.004 (0.047)
Rural children	-0.083 (0.046)	-0.065 (0.041)	-0.064 (0.053)	-0.012 (0.037)
Urban children	-0.005 (0.045)	0.075 (0.044)	-0.033 (0.026)	0.023 (0.096)

<sup>1</sup> Each cell represents the BMI impact estimate from a separate DID regression. Coefficient estimates of covariates are not reported.

<sup>2</sup> Robust standard errors are represented in parentheses. \* and \*\* indicate statistical significance at 5% and 1% levels, respectively.

**TABLE 4**  
**BMI Impact Estimates: Robustness Checks for Treatment Exogeneity<sup>1,2</sup>**

	Baseline model		Extended model	
	Openings	Closings	Openings	Closings
<i>DID regression with community and year fixed effects</i>				
Full sample	-0.016 (0.041)	0.009 (0.037)	-0.031 (0.020)	0.012 (0.054)
High income ( $\geq$ statewide median)	-0.001 (0.072)	-0.017 (0.050)	-0.013 (0.043)	0.003 (0.081)
Low income ( $<$ statewide median)	-0.069 (0.034) *	0.027 (0.046)	-0.077 (0.038) *	0.023 (0.014)
High vehicle ownership rate ( $\geq$ statewide median)	0.001 (0.053)	0.014 (0.071)	-0.005 (0.047)	0.018 (0.073)
Low vehicle ownership rate ( $<$ statewide median)	-0.066 (0.036)	0.011 (0.045)	-0.082 (0.042) *	0.007 (0.054)
Rural children	-0.035 (0.056)	-0.012 (0.052)	-0.040 (0.073)	0.021 (0.049)
Urban children	-0.008 (0.077)	0.036 (0.074)	0.011 (0.015)	0.008 (0.117)
<i>DID matching: nearest neighbor</i>				
Full sample	-0.023 (0.025)	0.003 (0.023)	-0.053 (0.024) **	0.010 (0.049)
High income ( $\geq$ statewide median)	-0.007 (0.055)	0.001 (0.027)	-0.007 (0.073)	-0.015 (0.058)
Low income ( $<$ statewide median)	-0.092 (0.032) **	0.017 (0.031)	-0.094 (0.032) **	0.033 (0.024)
High vehicle ownership rate ( $\geq$ statewide median)	0.006 (0.062)	-0.008 (0.051)	-0.011 (0.048)	0.004 (0.021)
Low vehicle ownership rate ( $<$ statewide median)	-0.093 (0.036) *	0.013 (0.041)	-0.089 (0.037) *	0.029 (0.026)
Rural children	0.042 (0.025)	-0.030 (0.054)	-0.056 (0.037)	0.002 (0.040)
Urban children	-0.013 (0.040)	0.033 (0.073)	-0.027 (0.039)	0.013 (0.078)
<i>DID matching: kernel</i>				
Full sample	-0.028 (0.031)	0.002 (0.025)	-0.045 (0.024)	0.004 (0.052)
High income ( $\geq$ statewide median)	-0.014 (0.043)	0.001 (0.035)	-0.021 (0.050)	-0.015 (0.045)
Low income ( $<$ statewide median)	-0.089 (0.026) **	0.011 (0.028)	-0.137 (0.042) **	0.047 (0.019) *
High vehicle ownership rate ( $\geq$ statewide median)	-0.002 (0.057)	-0.006 (0.035)	-0.005 (0.066)	0.007 (0.068)
Low vehicle ownership rate ( $<$ statewide median)	-0.078 (0.046)	0.017 (0.038)	-0.103 (0.034) **	-0.003 (0.044)
Rural children	-0.067 (0.048)	-0.057 (0.078)	-0.048 (0.042)	-0.010 (0.033)
Urban children	0.004 (0.060)	0.063 (0.059)	-0.038 (0.029)	0.016 (0.066)

<sup>1</sup> Each cell represents the BMI impact estimate from a separate DID regression or DID matching procedure.

<sup>2</sup> Standard errors are represented in parentheses. Robust standard errors are reported in the upper panel. \* and \*\* indicate statistical significance at 5% and 1% levels, respectively.



**TABLE 5**  
**Placebo DID Regression Results: Testing the Parallel Trend Assumption<sup>1,2</sup>**

	Baseline model		Extended model	
	Openings	Closings	Openings	Closings
Full sample	-0.012 (0.027)	0.023 (0.029)	-0.043 (0.051)	-0.019 (0.028)
High income ( $\geq$ statewide median)	-0.039 (0.063)	-0.011 (0.056)	-0.053 (0.088)	-0.037 (0.069)
Low income ( $<$ statewide median)	0.020 (0.048)	0.038 (0.037)	-0.001 (0.092)	0.014 (0.035)
High vehicle ownership rate ( $\geq$ statewide median)	-0.055 (0.056)	-0.007 (0.063)	-0.058 (0.139)	0.017 (0.065)
Low vehicle ownership rate ( $<$ statewide median)	0.027 (0.061)	0.063 (0.054)	0.002 (0.045)	-0.044 (0.037)
Rural children	0.033 (0.101)	-0.009 (0.055)	0.021 (0.066)	-0.030 (0.056)
Urban children	-0.059 (0.047)	0.026 (0.038)	-0.053 (0.071)	0.006 (0.086)

<sup>1</sup> Each cell represents the BMI impact estimate from a separate DID regression. Coefficient estimates of covariates are not reported.

<sup>2</sup> Robust standard errors are represented in parentheses. \* and \*\* indicate statistical significance at 5% and 1% levels, respectively.

**TABLE 6**

**BMI Impact Estimates: Robustness Checks for Other Food Environment Features<sup>1, 2</sup>**

	Baseline model		Extended model	
	Openings	Closings	Openings	Closings
<i>DID regression with school and grade fixed effects</i>				
Full sample	-0.011 (0.025)	-0.007 (0.033)	-0.037 (0.021)	0.001 (0.014)
High income ( $\geq$ statewide median)	-0.005 (0.026)	0.016 (0.052)	0.029 (0.043)	-0.015 (0.058)
Low income ( $<$ statewide median)	-0.077 (0.035) *	-0.032 (0.026)	-0.127 (0.049) *	0.037 (0.020)
High vehicle ownership rate ( $\geq$ statewide median)	0.024 (0.048)	0.031 (0.059)	-0.004 (0.024)	-0.015 (0.066)
Low vehicle ownership rate ( $<$ statewide median)	-0.082 (0.054)	-0.010 (0.047)	-0.074 (0.031) *	0.030 (0.044)
Rural children	-0.114 (0.079)	-0.032 (0.027)	-0.055 (0.064)	-0.042 (0.072)
Urban children	0.047 (0.052)	-0.002 (0.067)	-0.033 (0.026)	0.050 (0.088)
<i>DID regression controlling for unhealthy food store access</i>				
Full sample	-0.035 (0.026)	0.003 (0.024)	-0.039 (0.033)	0.011 (0.028)
High income ( $\geq$ statewide median)	0.022 (0.048)	-0.019 (0.045)	-0.029 (0.043)	-0.015 (0.058)
Low income ( $<$ statewide median)	-0.103 (0.037) **	0.025 (0.038)	-0.087 (0.034) *	0.055 (0.026) *
High vehicle ownership rate ( $\geq$ statewide median)	0.054 (0.052)	0.011 (0.045)	0.021 (0.037)	0.000 (0.043)
Low vehicle ownership rate ( $<$ statewide median)	-0.107 (0.060)	0.001 (0.027)	-0.100 (0.041) *	0.034 (0.034)
Rural children	-0.055 (0.047)	-0.037 (0.073)	-0.067 (0.105)	0.002 (0.032)
Urban children	0.001 (0.032)	0.058 (0.049)	0.013 (0.026)	0.022 (0.068)

<sup>1</sup> Each cell represents the BMI impact estimate from a separate DID regression. Coefficient estimates of covariates are not reported.

<sup>2</sup> Robust standard errors are represented in parentheses. \* and \*\* indicate statistical significance at 5% and 1% levels, respectively.

**TABLE 7**  
**BMI Impact Estimates with USDA Radius Definitions of Supermarket Access<sup>1,2</sup>**

	Baseline model		Extended model	
	Openings	Closings	Openings	Closings
Full sample	-0.007 (0.025)	0.004 (0.026)	-0.020 (0.018)	0.001 (0.011)
High income ( $\geq$ statewide median)	0.003 (0.035)	-0.022 (0.049)	0.004 (0.057)	-0.012 (0.040)
Low income ( $<$ statewide median)	-0.059 (0.029) *	0.036 (0.038)	-0.046 (0.021) *	0.014 (0.027)
High vehicle ownership rate ( $\geq$ statewide median)	0.000 (0.037)	0.008 (0.031)	0.020 (0.023)	0.004 (0.044)
Low vehicle ownership rate ( $<$ statewide median)	-0.052 (0.030)	0.003 (0.053)	-0.049 (0.022) *	0.006 (0.038)
Rural children	-0.011 (0.048)	-0.023 (0.066)	-0.017 (0.054)	0.003 (0.015)
Urban children	-0.005 (0.045)	0.075 (0.044)	-0.033 (0.026)	0.023 (0.096)

<sup>1</sup> Each cell represents the BMI impact estimate from a separate DID regression. Coefficient estimates of covariates are not reported.

<sup>2</sup> Robust standard errors are represented in parentheses. \* and \*\* indicate statistical significance at 5% and 1% levels, respectively.