THE EFFECT OF FOOD DESERTS ON THE BODY MASS INDEX OF ELEMENTARY SCHOOLCHILDREN

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Families in low-income neighborhoods sometimes lack access to supermarkets that provide a broad range of healthy foods. We investigate whether these so called "food deserts" play a role in childhood obesity using a statewide panel data set of Arkansas elementary schoolchildren. We use fixed-effects panel data regression models to estimate the average food desert effect. We next compare children who left (entered) food deserts to children who were always (never) in food deserts and homogenize samples for those whose food desert status changed as a result of a change in residence and those whose status changed only as a consequence of the entry or exit of a supermarket. We present evidence that exposure to food deserts is associated with higher z-scores for body mass index. On average, this is in the neighborhood of 0.04 standard deviations. The strongest evidence and largest association is among urban students and especially those that transition into food deserts from non-deserts. Our food desert estimates are similar in magnitude to findings reported in earlier work on diet and lifestyle interventions targeting similarly aged schoolchildren. That said, we are unable to conclude that the estimated food desert effect is causal because many of the transitions into or out of food deserts result from a change in residence, an event that is endogenous to the child's household. However, there is evidence that food deserts are a risk indicator and that food desert areas may be obesogenic in ways that other low-income neighborhoods are not.

Key words: BMI z-score, built environment, childhood obesity, difference-in-differences, food deserts, panel data estimation.

JEL codes: I140, I190, Q180.

Obesity among children is a major public health issue facing the United States. Ogden et al. (2010) found that from 1980 to 2008, obesity rates among children aged 6 to 11 years nearly tripled—from 6.5% to 19.6%, respectively. This means that a greater proportion of U.S. children are at risk for health problems throughout life (Guo and Chumlea 1999). While the rise in childhood obesity is the combination of many factors (Anderson and Butcher 2006), one that has been receiving a great deal of attention is the extent to which the commercial food environment provides a broad assortment of affordable foods that underpin a healthy diet (Beaulac, Kristjansson, and Cummins 2009; Chung and Myers 1999; Hendrickson, Smith, and Eikenberry 2006; Karpyn, Young, and Weiss 2012; Larson, Story, and Nelson 2009). The concern is that many families live in what are termed food deserts: areas where healthy food choices are not readily available and may thereby place children at greater risk for obesity.

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Food deserts have caught the imagination of policy makers, with large-scale interventions and healthy financing initiatives targeting food desert areas, usually in large cities (Gentile 2008; Karpyn et al. 2010), and food access has been a central theme in First Lady Michelle Obama's "Let's Move" campaign.[1] The criteria used to define food deserts have varied in the literature (Walker, Keane, and Burke 2010), but there is broad agreement that a food desert is an area where residents have limited access to healthy foods because: *a*) there is a dearth of establishments that provide such foods, primarily supermarkets, within a reasonable geographic proximity, and *b*) residents lack the resources to obtain healthy foods from more distant locales. For example, the Food, Conservation, and Energy Act of 2008 (Public Law 110-234 Sec. 7527) defines a food desert as "…an area in the United States with limited access to affordable and nutritious food, particularly such an area composed of predominantly lower-income neighborhoods and communities."[2] Thus, the concept of a food desert, as it has emerged, is a reflection of two environmental features, the extent to which residents have access to establishments that sell healthy foods, and the economic status of the community in which they live.

Concerns have been raised that evidence on the role of food access and other environmental causes of obesity is not sufficient to support and defend policy interventions (Karpyn, Young, and Weiss 2012; National Research Council 2010). As explained below, there is fairly clear evidence that areas exist where residents would face meaningful constraints to securing healthy foods at reasonable time and monetary costs. However, estimating the impact of living in these areas on body weight has been challenging for several reasons. One is that constraints to food access cannot be easily separated from socioeconomic status. Another is that access to large food retailers could affect lower-income neighborhoods differently than higher-income neighborhoods. Finally, weight change is a gradual process and the effects of living in a food desert are only likely to be observed over time.

In this article we examine whether food deserts are associated with childhood obesity through a unique, individual-level, panel data set containing body mass index (BMI) measurements of Arkansas elementary schoolchildren. Specifically, we track three cohorts containing children for whom BMI z-scores are available in kindergarten, first, second, and fourth grades. We use these grade ranges for two reasons. One is opportunity; the data we use contain three complete cohorts over these grades with at least four BMI observations per child. Another reason is that these grades follow the developmental stage known as adiposity rebound, which is a period of increasing BMI after early childhood. It usually occurs at about six years of age and has been shown to be a key stage in the weight-gain trajectory over the human life span (Boonpleng, Park, and Gallo 2012). Understanding the role of the food environment at this point in life may be of particular importance.

In this article we classify schoolchildren into food deserts along lines similar to those used by the U.S. Department of Agriculture's (USDA) Economic Research Service (ERS) (Dutko, Ver Ploeg, and Farrigan 2012). The main difference is that certain members of our study team had access to the exact geographic coordinates of the children's residences and could therefore classify whether each residence was located in a food desert or non-desert. To be classified as located in a food desert, our first criterion is that the distance between a child's residence and the nearest supermarket was more than one mile (ten miles) for urban (rural) areas. The second criterion is that the child's residence was located in a low-income neighborhood. We define neighborhoods at the level of a census-block group and define low-income block groups as those with median household incomes less than 80% of the statewide median income or poverty rates in excess of 20%.

Given this definition, two things can trigger a change in the child's food desert status. One is a physical move from a non-desert to a food desert or vice versa. The other is the entry or exit of a supermarket that causes the child's residence to be reclassified from a non-desert to a food desert or vice versa. One might expect a different response depending on what caused the change in food desert status. In our analyses, we first estimate the food desert effect regardless of the reason. We then estimate the effect separately for samples containing children who

[1] See http://www.letsmove.gov/about.
[2] See http://www.gpo.gov/fdsys/pkg/PLAW-110publ234/pdf/PLAW-110publ234.pdf.

moved and did not move. Secondly, because a change in food desert status may also entail a change in neighborhood income status, we estimate the food desert effect using all children without regard to neighborhood income status, and then again using subsamples containing only the children who were always observed in low-income block groups. Finally, the criterion used to define a food desert depends on whether the residence is urban or rural, and we also estimate the food desert effect separately from subsamples of rural and urban children.

In what follows we first provide a general background on food deserts and the mechanisms by which food deserts may contribute to poor diets and weight gain. Next, we describe our data sets, the classification of children into food deserts or non-deserts, the selection of our analysis sample, and our empirical methods. Our results follow. We show an average food desert effect on BMI z-score of about 0.04 standard deviations. The magnitude of this food desert estimate is similar to estimates reported in earlier studies of school-based interventions aimed at improving diet and increasing physical activity among elementary schoolchildren. Using a difference-in-differences framework, we next examine children who transition into food deserts separately from children who transition out of food deserts. Again, there is evidence of a food desert effect. However, alternative measures of food access have no measurable effect on BMI z-scores. Thus, we find evidence that food deserts matter, but we cannot rule out the possibility that food deserts are obesogenic in ways that are independent of food access. We conclude the article by discussing these findings and what they mean about food access and its importance to body weight during childhood.

Food Deserts and Body Weight Outcomes

The main issue at hand is that life in a food desert raises the costs of accessing a healthy diet and thereby contributes to weight gain. Retail inventories show that residents of disadvantaged neighborhoods face higher prices and may otherwise face a retail food environment with lower assortments and quality in healthy food categories such as fresh fruits and vegetables (Chung and Myers 1999; Hendrickson, Smith, and Eikenberry 2006). Hamrick and Hopkins (2012) find that lower income households spend considerably more time traveling to the grocery store and shop for groceries less frequently than do higher income households. While it is possible that non-chain retailers, including fresh fruit and vegetable markets, provide fairly diverse and affordable food options in some lower income neighborhoods (Raja, Ma, and Yadav 2008), recent and comprehensive literature reviews conclude that geographic areas where residents face meaningful monetary and non-monetary barriers to sourcing healthy foods do exist within the United States (Cummins and Macintyre 2006; Beaulac, Kristjansson, and Cummins 2009; Larson, Story, and Nelson 2009). Evidence that the presence of retailers meaningfully affects food accessibility or food purchase behaviors is provided in before/after studies that examine contexts where food stores have expanded into disadvantaged areas (Wrigley, Warm, and Margetts 2003; Sadler, Gilliland, and Arku 2013; Weatherspoon et al. 2013).

While there is little doubt as to the existence of food deserts, findings on whether food deserts meaningfully contribute to weight gain are mixed. In studies on children, Schafft, Jensen, and Hinrichs (2009) find a significant link between a community's food desert status and childhood obesity rates among fifth to seventh graders in rural Pennsylvania. Similarly, Booth, Pinkston, and Poston (2005) find that high levels of neighborhood deprivation are associated with higher BMI rates, especially in children. However, Alviola, Nayga, Jr., and Thomsen (2013) find no relationship between a school district's food desert status and school-district obesity rates in Arkansas. Studies on adults also provide conflicting results (Rundle et al. 2009; Budzynska et al. 2013; Macdonald et al. 2011; Stafford et al. 2007). In sum, the evidence linking food access to BMI is inconclusive. This is also the conclusion of Black and Macinko (2008) in their review of the earlier literature.

Establishing a link between food deserts and BMI is challenging for several reasons. First, food-store access is difficult to separate from neighborhood economic conditions (Black and Macinko 2008; Walker, Keane, and Burke 2010). The effects of living in a food desert are confounded with other risk factors. Specifically, socioeconomic status is correlated with obesity in both adults and children (McLaren 2011). Furthermore, the food environment may simply be symptomatic of economically disadvantaged areas. For instance, if healthier foods are normal goods, one would expect to find fewer "healthy" stores in lower income areas (Bitler & Haider 2011).

A second, but related, challenge is that the effect of improved food access may depend on neighborhood context. In one context, a new supermarket may meaningfully expand healthy food options for residents and facilitate healthy dietary choices. In another, the increased competition that a new supermarket provides may have the opposite effect by lowering prices on less healthy foods (Courtemanche and Carden 2011). Work by Chen et al. (2010) suggests that context is important. These authors find that residents in lower income neighborhoods with better access to chain grocers had lower BMIs. The opposite was true for residents in higher income neighborhoods.

Finally, weight gain is a gradual process and length of exposure to a poor food environment is likely an important factor in determining weight gain. Wang et al. (2006) estimate that the rise in childhood obesity rates can be explained by an excess consumption of 110 to 165 calories per day, roughly equivalent to the energy contained in a single twelve-ounce sweetened beverage. Because modest energy imbalances accumulate into weight change, the length of time spent in a food desert is a key, albeit often neglected, dimension of the food environment. Identifiable changes in body weight resulting from a poor food environment are likely to manifest themselves only after some time has passed. Indeed, earlier work shows that economic changes that can be expected to affect diet or exercise behaviors take long periods of time to translate into weight gain (Goldman, Lakdawalla, and Zheng 2009; Courtemanche 2011). Most of the earlier studies have been unable to control for the length of time spent in a food desert, yet this is probably key to understanding the impact of living in a food desert on body weight.

Data and Methods

The Arkansas childhood BMI dataset provides a unique opportunity to examine the impact of food deserts on childhood body weight while addressing the empirical issues described above. It is to the characteristics of these data that we now turn.

BMI Screenings of Arkansas Public Schoolchildren

Due to concerns over the rise in childhood obesity, the Arkansas General Assembly passed Act 1220 of 2003. This legislation was aimed broadly at reducing rates of childhood obesity and included a requirement that schools conduct annual BMI screenings. With the implementation of this act, Arkansas became the first state to systematically screen public schoolchildren for unhealthy weight status. The BMI assessments began in the 2003/2004 school year and have continued annually since that time. The Arkansas Center for Health Improvement (ACHI) oversaw the development of protocols, training materials, and training programs to facilitate the statewide BMI assessment program. The BMI measurements are taken by trained personnel within the public schools and the statewide protocols involve uniformity not only in procedure but also in the equipment (scales and stadiometers) used for weight and height measurement (Justus et al. 2007). After measurement, BMI is calculated as a ratio ([weight in pounds / (height in inches)^2] $\times$ 703) and is then converted to age-gender specific z-scores according to the Centers for Disease Control and Prevention guidelines (CDC 2015). Justus et al. (2007) report that school participation rates range from 94–99%. Among the years included in our analysis, valid BMI measurements were obtained from at least 82% of students in each year.[3] For the first four years of the screening program, the 2003/2004 through 2006/2007 academic years, all public school children were measured for BMI. Since that time, only children in even-numbered grades, kindergarten through tenth grade, have been measured.

Our analysis is based on a balanced panel of BMI z-scores drawn from this BMI database over the 2003/2004 through 2009/2010 academic years (seven years total). In addition to the BMI z-score, these data include the child's gender, age in months,

[3] The leading reason for a child not having a valid measure is his or her absence on the day of measurement, followed by parent refusal, child refusal, and incomplete enrollment data (Arkansas Center for Health Improvement 2010).

race and ethnicity, and school meal status (whether the child qualified for free or reduced price school lunches). We focus specifically on children in early elementary grades. As noted earlier, weight changes around this point in life may be important predictors of obesity later in life. Moreover, the food desert effect should be easier to detect among younger children because their diets are more likely to be dictated by the adults in their lives. In general, these children will be more dependent on foods provided in the home, whereas older children have greater ability to obtain foods of their own choosing outside of the home and school. Young children are also predominantly housed in elementary schools and the food environment within the school will be more uniform for students across the state. In comparison to middle schools, junior high, or high schools, elementary schools present students with fewer meal options and we are aware of none that provide access to on-campus vending machines.

Determining Children's Food Desert Status

As is common in earlier studies, we obtained food store location data from Dun and Bradstreet (D&B; Powell et al. 2007; Zick et al. 2009; Bader et al. 2010b). Business lists compiled by D&B or similar vendors have been shown to contain errors, but accuracy rates are high for supermarkets and grocery stores, the types of stores of primary interest here, and there is evidence that inaccuracies are random and do not vary by neighborhood characteristics (Han et al. 2012; Bader et al. 2010a). Because the BMI z-scores are observed repeatedly with time, we obtained archival data showing year-by-year locations of Arkansas food stores beginning in 2004 and extending through 2010. These provide snapshots of the commercial food environment that are synchronous with the time periods reflected in the BMI data.

To define food deserts, our goal was to distinguish between food stores that provide a broad array of foods including healthy food options from those that offer more limited food selections. For our convention, we refer to these "healthier stores" as supermarkets and, as a decision rule, we classified food stores as supermarkets if we were reasonably certain they contained a fresh produce department. Unfortunately, the existence of a fresh produce department could not be determined based only on the standard industrial classification (SIC) codes contained in the D&B data. In fact, it was clear that many of the establishments with SIC codes for supermarkets or grocery stores were smaller, convenience-type stores that offered a limited range of food options. Consequently, we examined the name and trade-style fields in the D&B data to identify chain stores and affiliated grocers that we knew provided fresh produce.[4] In questionable cases, where type of store could not be ascertained based on establishment name or trade style, we placed calls to the telephone number provided in the D&B database and/or used street-view images in the Google search engine to verify store formats.

Another relevant issue is that supercenters operated by mass merchandisers have emerged as key players in the retail food market. Walmart Stores, Inc., in particular, is an important feature of the commercial food landscape throughout Arkansas. These stores were consistently classified in D&B records as discount department stores. Consequently, we used the SIC code for discount department stores but verified that non-supercenter formats were excluded from stores used in the definition of food deserts. In the case of Walmart, we also made a point of counting the company's Neighborhood Market formats because these are similar to traditional supermarkets and include large fresh produce departments.

To match the food environment data to the BMI records, ACHI personnel geocoded the residential addresses of schoolchildren represented in the BMI data and interfaced these addresses with data on food store locations. The ACHI personnel also identified whether each address fell into an urban or rural census block and matched schoolchildren by census-block group to the 2009 American Community Survey (ACS) summary files. The 2009 ACS represents neighborhood-level demographic and economic characteristics over the 2005 through 2009 period and so is centered on the time period covered by the BMI data available for use in this study.

Residences were classified as food deserts if they met both of the following two

[4] The name field sometimes contained a legal name for the company that obscured the type of business facing the public, for example, "MARVINS INC A KANSAS CORP," in which case the trade style field provided the trade name of the establishment as "MARVINS IGA."

Table 1. Grade Levels for which BMI z-Scores are used by Kindergarten (K) Cohort and by Year

Cohort	2003/04	2004/05	2005/06	2006/07	2007/08	2008/09	2009/10
2003/04	K	1st Grade	2nd Grade		4th Grade		
2004/05		K	1st Grade	2nd Grade		4th Grade	
2005/06			K	1st Grade	2nd Grade		4th Grade

Note: BMI measurement began in the 2003/04 school-year. Public schoolchildren in Arkansas were measured for BMI in all grades through 2006/07. Afterwards, only children in even grades K through 10th grade were measured.

conditions: *a*) The residence was located in low-income census block-group. Low-income block groups are defined as those with median household income less than or equal to $28,273, or with at least 20% of the population below poverty;[5] *b*) The residence was more than one mile (ten miles) from the nearest supermarket and located in an urban (rural) census block.

Our classification of residences as food deserts is similar in spirit to the classification methods used by USDA for the nationwide food environment atlas (ERS USDA 2015).

Empirical Analyses

After assigning residences to food deserts or non-deserts, we pulled a sample consisting of all children for whom BMI measures are available in kindergarten, first, second, and fourth grades. Specifically, we included the 2003/04, 2004/05, and 2005/06 kindergarten cohorts as shown in table 1. Given the switch from every-year measurement to even-grade measurement in 2008, third-grade BMI z-scores are available only for the 2003/04 cohort. To maintain a consistent set of measurements across the three cohorts, we do not include these third grade BMI scores in our analysis. From this panel, we then selected our analysis sample as children who fell into one of the following categories: *a*) Children who were always observed in a food desert. These children's addresses of record were classified as a food desert during kindergarten, first, second, and fourth grades; *b*) Children who were never observed in a food desert residence. These children's addresses of record were classified as non-deserts in kindergarten, first, second, and fourth grades; *c*) Children who were observed two consecutive times in food desert residences (in both kindergarten and first grade), followed by two consecutive times in non-desert residences (in second grade and fourth grade); *d*) Children who were observed two consecutive times in non-desert residences (in kindergarten and first grade), followed by two consecutive times in food desert residences (in second and fourth grade).

These inclusion criteria assure at least two consecutive time-series observations inside (outside) food deserts and thereby provide some assurance that a child's food desert status was not simply a transitory phenomenon. Secondly, for children who change food desert status, this design assures some degree of uniformity regarding the stage of life when that change occurred.

At this juncture, it is useful to clarify a few points about our classification of children into food deserts and non-deserts. The child's food desert status measures two dimensions. The first is the income status of her census block group. The second is the distance from her residence to the nearest grocery store containing a fresh produce department. This distance depends on whether her residence is rural or urban. In our data, the income status of any given neighborhood is from the 2009 ACS, which represents an average over 2005–2009 and does not vary over the years included in our study. A change in food desert status can therefore be the result of only two events: *a*) The child moved to a new residence and that move caused her food desert status to change, and *b*) the child did not move, but the food environment surrounding her residence changed. That is, a store opened or closed and this caused the child's residence to be reclassified from a food desert to a non-desert or *vice versa*.

Because of these issues, we analyze several subsamples. First, given that food deserts are, by definition, low-income areas, a move to a food desert from a non-desert residence

[5] The $28,273 median block-group income threshold is denominated in 2009 dollars and is 80% of $35,341, which is the median of Arkansas census block-groups reported in the 2009 ACS five-year estimates.

will often represent a move into a worse neighborhood. It stands to reason that such moves may be caused by deteriorating household economic conditions or a family crisis that might affect weight gain regardless of food access. To address this issue, we analyze a sample that is further homogenized by neighborhood income status. Second, food deserts may pose different constraints to rural and urban residences and their effects on bodyweight may differ. We assess this by analyzing subsamples comprised of only rural and only urban children. Finally, we consider it important to test whether the food desert effect depends on the underlying reason for reclassification, and so we estimate the food desert effect once for the entire sample and then again for samples of "movers" and "non-movers," which are homogenized by whether the child changed residential location between the first and second grades.

We first estimate the food desert effect within a two-way fixed-effects regression model for all observations and for subsamples that have been homogenized by movers, non-movers, and neighborhood income status. The model is:

$$Y_{it} = \alpha_0 + \alpha_1 D_{it} + \boldsymbol{\theta}\mathbf{X}_{it} + \gamma_i + \phi_t + \varepsilon_{it} \tag{1}$$

where Y_{it} is the BMI z-score of child i at time t, D_{it} is a binary variable taking the value of 1 if the child's residence was classified as a food desert, $\mathbf{X}_{it}$ is a vector of control variables, γ_i is an individual fixed effect, ϕ_t is a year fixed effect, and ε_{it} is the error term.

The control variables include the child's school lunch status (free and reduced), the number of fast food restaurants within a two-mile radius of the child's home, and neighborhood characteristics from the 2009 ACS indicating educational attainment, vehicle ownership rate, and household/family structure as presented in table 2. The ACS measures represent a five-year average and so do not vary by neighborhood over time. However, they are not linear combinations of the child fixed effects due to the fact that some children change neighborhoods during the period over which they are observed.[6]

We use a two-way model because there is reason to believe that year effects may be important. First, childhood obesity rates have trended upwards over time, and year effects can account for this trend. Second, the period under study does include the onset of the recent recession. Arkansas fared comparatively well relative to the rest of the nation, but unemployment rates rose from 5.4% in 2008 to 7.5 and 7.9% in 2009 and 2010, respectively (Bureau of Labor Statistics 2014).

Because a food desert classification reflects both food access and neighborhood income, we re-estimate these models using measures of food access alone. First, we use a low-access measure. This takes a value of one if there were no supermarkets within one and ten miles of the child's residence for urban and rural residences, respectively. As shown in table 2, the number of children who lived in residences with low access during the study period is nearly five times the number of children who lived in food desert residences. For the low-income subsamples, the low-food access measure corresponds to the food desert measure and thereby provides no additional insight. Consequently, we also estimate the models replacing the food desert measure with the distance in miles to the nearest supermarket.

The model in equation 1 estimates an average food desert effect, but it is possible that switching into a food desert affects body weight differently than switching out. To address this, we follow up with difference-in-differences (DID) panel estimations, which is a common approach in program and policy evaluations (Khandker, Koolwal, and Samad 2010) and has become a common strategy to estimate effects of programs that could impact nutrition, weight, or health outcomes (Datar and Sturm 2004; Variyam 2008; Belot and James 2011; Goetzel et al. 2010; Racine et al. 2012; Angelucci and Attanasio 2013). The standard DID regression is augmented by individual and year fixed effects as follows:

$$\begin{aligned} Y_{it} = {} & \beta_0 + \beta_1 (T_{it} \times \tau_{it}) + \beta_2 T_{it} + \beta_3 \tau_{it} \\ & + \gamma_i + \phi_t + \varepsilon_{it} \end{aligned} \tag{2}$$

where Y_{it}, γ_i, ϕ_t, and ε_{it} are as defined above. Further, T_{it} is a binary variable taking the value of 1 for children in the transition group (those that switched desert status), and τ_{it} is

[6] This is true even among the sample of non-movers. Inclusion in this sample requires that no move occurred between the first- and second-grade BMI observations, the point at which we examine transitions into or out of food deserts. It is possible for a child in this sample to have changed residence between kindergarten and first grade or between the second grade and fourth grade, provided that the change in residence did not also trigger a change in food desert status.

Table 2. Number of Observations, Means, and Standard Deviations by Sample

Measure	Unit	All	Low-income[a]	Rural[b]	Urban[b]	Movers[c]	Non-movers[c]
Observations (N)		110,384	27,504	36,892	62,256	16,336	94,048
Panel (a): Student-level information							
BMI	z-score	0.652	0.709	0.632	0.666	0.664	0.650
		(1.048)	(1.085)	(1.050)	(1.051)	(1.022)	(1.053)
Food Desert	Binary	0.063	0.234	0.049	0.079	0.062	0.063
		(0.243)	(0.423)	(0.216)	(0.269)	(0.241)	(0.243)
Low Food Access	Binary	0.300	0.234	0.153	0.396	0.278	0.304
		(0.458)	(0.423)	(0.360)	(0.489)	(0.448)	(0.460)
Nearest Supermarket	Miles	3.005	2.690	6.013	1.209	2.372	3.115
		(3.566)	(3.995)	(4.035)	(1.588)	(3.030)	(3.640)
African American	Binary	0.210	0.421	0.055	0.322	0.282	0.198
		(0.407)	(0.494)	(0.228)	(0.467)	(0.450)	(0.398)
Hispanic	Binary	0.076	0.101	0.031	0.106	0.082	0.075
		(0.265)	(0.301)	(0.174)	(0.308)	(0.274)	(0.263)
Female	Binary	0.496	0.497	0.491	0.499	0.517	0.492
		(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Rural	Binary	0.384	0.292	1.000	0.000	0.293	0.400
		(0.486)	(0.455)	(0.000)	(0.000)	(0.455)	(0.490)
Age	Months	91.587	91.560	91.864	91.382	91.663	91.574
		(18.066)	(18.110)	(18.101)	(18.039)	(18.098)	(18.061)
Free School Lunch	Binary	0.398	0.600	0.308	0.448	0.540	0.373
		(0.489)	(0.490)	(0.462)	(0.497)	(0.498)	(0.484)
Reduced Price Lunch	Binary	0.099	0.103	0.115	0.090	0.091	0.101
		(0.299)	(0.304)	(0.319)	(0.286)	(0.288)	(0.301)
Fast Food	Count[d]	5.113	6.893	0.356	8.221	6.296	4.908
		(6.274)	(6.803)	(1.077)	(6.41)	(6.614)	(6.19)
Panel (b): Community-level (census block group) information							
No Vehicle	Proportion[e]	0.063	0.116	0.041	0.078	0.071	0.061
		(0.076)	(0.105)	(0.042)	(0.090)	(0.082)	(0.075)
High School	Proportion[f]	0.352	0.381	0.399	0.322	0.352	0.352
		(0.111)	(0.105)	(0.09)	(0.113)	(0.11)	(0.111)
Some College	Proportion[f]	0.275	0.250	0.265	0.280	0.276	0.275
		(0.086)	(0.093)	(0.076)	(0.092)	(0.089)	(0.085)
College Degree	Proportion[f]	0.189	0.116	0.148	0.214	0.180	0.190
		(0.139)	(0.088)	(0.091)	(0.159)	(0.132)	(0.141)
Working Mother	Proportion[g]	0.246	0.351	0.181	0.289	0.273	0.241
		(0.198)	(0.239)	(0.140)	(0.219)	(0.212)	(0.195)
Married HH	Proportion[g]	0.684	0.516	0.775	0.624	0.644	0.691
		(0.242)	(0.277)	(0.169)	(0.266)	(0.256)	(0.239)
Single Female HH	Proportion[g]	0.256	0.414	0.169	0.314	0.291	0.250
		(0.230)	(0.274)	(0.151)	(0.255)	(0.244)	(0.227)

Note: Standard deviations are in parentheses. (a) Children in low-income block groups during all four periods of observation; (b) Rural (urban) subsamples include children in rural (urban) census blocks in all four periods of observation; (c) Movers (non-movers) changed (did not change) location of residence between the 1st and 2nd grades; (d) Count within a two-mile radius of the child's residence; (e) Proportion of occupied housing units; (f) Proportion of population over age 25; (g) Proportion of all children under 18.

a binary variable taking the value of 1 for all children during the transition periods (periods when children were in second and fourth grade).[7] The coefficient of interest is β_1. This is the difference in BMI z-score attributed to the change in food desert status after differencing out any transition-period effect from the stable (those that did not switch desert status) and transition groups.

[7] Given the two-way fixed effects specification, the effect of membership in the transition group is captured in the fixed effect (transition group membership is a linear combination of the fixed effects and so β_2 cannot be estimated uniquely). The effect of the transition period, however, can be estimated despite the inclusion of year effects. While the post-transition period for all members of the transition group occurs in second and fourth grades, these grades occur in different calendar years for the different cohorts.

We estimate two sets of DID regressions but acknowledge that assigning children to the stable or transition groups is not random as would be desired. Still, the DID framework presents an opportunity to analyze the effects of transitioning into food deserts separately from the effects of transitioning out of them. Again, we estimate DID models for samples containing all children and for subsamples that have been homogenized by neighborhood income status, urbanity of residence, movers, and non-movers. In the first set of regressions, the transition group includes children who were observed in food deserts in second and fourth grades after having been observed in non-deserts in kindergarten and first grade. These are compared to children who were never observed in a food desert. In the second set of regressions, the transition group includes children who were observed in non-deserts in second and fourth grades after having been observed in food deserts during kindergarten and first grade. These are compared to children who were always observed in a food desert residence.

An assumption in DID analysis is that the counterfactual trend in the BMI z-scores of transitioning children would have been the same as the stable children had there not been a change in food desert status. We assess the validity of this assumption by performing placebo DID regressions using only the periods prior to the change in desert status, when the children were in kindergarten and first grade. Because all children faced the same food desert status during these two periods, significant DID interaction terms in these placebo regressions would be evidence against the parallel trends assumption. These placebo regressions are specified as in equation 2 except that individual fixed effects are excluded because the regressions conform to the standard two-period DID model.

Results

Table 3 presents estimates of the fixed-effects models by sample. The food desert coefficient is 0.0384 standard deviations in the sample of all children. With the exception of urban children, estimates from each of the subsamples reported in table 3 are within one standard error on either side of this estimate. However, food desert estimates from the subsamples are estimated with less precision. Coefficients from the subsamples of rural children and non-moving children are not statistically different from zero.

There are few consistent patterns in the estimates for the control covariates across the different samples reported in table 3. One limitation is that controls for vehicle ownership, educational attainment, and household structure are measured at the block-group of the child's residence from the 2009 ACS five-year estimates. As a consequence, temporal variation by child is limited and these covariates may not contribute much information beyond that which is already contained in the individual fixed effects. This is especially true in subsamples containing non-movers.[8]

Inclusion of School Fixed Effects

As noted above, elementary schools are generally more uniform in school food options but unobserved school characteristics may still be important. Schools are a significant source of calories; some children receive two meals per day at the school. For this reason, even small differences among schools could affect weight gain over time. School meal receipts are affected by student income status, which may also confound estimates of the food desert effect. Differences in the implementation of physical activity standards, playground equipment, or the amount of space suitable for vigorous play could also affect weight gain.

The issue of unobserved school characteristics applies mainly to the movers. Among the non-movers, time-invariant school-level characteristics that affect weight gain will be captured by the individual fixed effects because the vast majority of elementary schools house kindergarten through fourth grade and very few children would change schools as part of the natural progression through the public school system over these grades.

When school fixed-effects are included, the food desert from the sample of all children is similar in magnitude to that reported

[8] School meal status and fast food counts are measured at the individual level but tend to be stable over time and similarly may not contribute much beyond the individual fixed effects. Among the movers, there will be substantially more time series variation in the neighborhood controls because changes in residence will generally mean a change in neighborhood. All movers changed residences between the first and second grade.

Table 3. Fixed-effects Estimates by Sample

Measure	All	Low-Income[a]	Rural[b]	Urban[b]	Movers[c]	Non-movers[c]
Observations (N)	110,384	27,504	36,892	62,256	16,336	94,048
Food Desert	0.0384**	0.0388*	0.0312	0.0594***	0.0482**	0.0230
	(0.0153)	(0.0227)	(0.0300)	(0.0212)	(0.0202)	(0.0269)
Age	−0.0001	0.0034	−0.0004	−0.0006	0.0009	−0.0002
	(0.0013)	(0.0022)	(0.002)	(0.0016)	(0.0021)	(0.0014)
Free School Lunch	0.0061	−0.0014	0.0045	0.0035	0.0150	0.0043
	(0.0066)	(0.0148)	(0.0116)	(0.0090)	(0.0144)	(0.0074)
Reduced Lunch	0.0103	0.0085	0.0146	0.0028	0.0324*	0.0065
	(0.0067)	(0.0149)	(0.0109)	(0.0094)	(0.0176)	(0.0071)
Fast Food	−0.0002	0.0002	−0.0078	−0.0018**	−0.0002	−0.0002
	(0.0007)	(0.0021)	(0.0054)	(0.0008)	(0.0010)	(0.0010)
No Vehicle	−0.0115	0.0435	−0.6056**	0.0343	0.0177	−0.0430
	(0.0550)	(0.0762)	(0.2539)	(0.0579)	(0.0785)	(0.0646)
High School	−0.0331	−0.0476	0.0641	0.0033	0.0294	−0.0882
	(0.0428)	(0.0770)	(0.1358)	(0.0523)	(0.0600)	(0.0600)
Some College	0.0395	0.0946	−0.0765	0.1045*	0.0995	−0.0037
	(0.0431)	(0.0982)	(0.1345)	(0.0545)	(0.0612)	(0.0623)
College Degree	−0.0071	−0.0522	−0.1271	0.0326	0.0906*	−0.0978*
	(0.0395)	(0.1136)	(0.1476)	(0.0495)	(0.0525)	(0.0547)
Working Mother	−0.0036	0.0356	0.0198	0.0086	0.0386	−0.0453
	(0.0244)	(0.0449)	(0.0899)	(0.032)	(0.0317)	(0.0378)
Married HH	−0.0863***	−0.0922	−0.1109	−0.1314***	−0.1354***	−0.0275
	(0.0318)	(0.0719)	(0.0842)	(0.045)	(0.0449)	(0.0476)
Single Female HH	−0.0570	−0.1543**	−0.0609	−0.1172**	−0.1529***	0.0435
	(0.0377)	(0.0767)	(0.101)	(0.0542)	(0.0492)	(0.0570)
2004/05	0.0317**	−0.0001	0.0285	0.0400**	0.0184	0.0341**
	(0.0153)	(0.0280)	(0.0221)	(0.0190)	(0.0255)	(0.0163)
2005/06	0.0509*	0.0133	0.0518	0.0674*	0.0269	0.0553*
	(0.0296)	(0.0520)	(0.0422)	(0.0359)	(0.0460)	(0.0320)
2006/07	0.0679	0.0189	0.0686	0.0988*	0.0202	0.0768
	(0.0441)	(0.0738)	(0.0649)	(0.0526)	(0.0691)	(0.0478)
2007/08	0.0976	−0.0143	0.1198	0.1224*	0.0401	0.1082*
	(0.0602)	(0.0999)	(0.0891)	(0.0713)	(0.0948)	(0.0652)
2008/09	0.1319*	0.0217	0.1607	0.1658*	0.0489	0.1478*
	(0.0753)	(0.1251)	(0.1112)	(0.0894)	(0.1172)	(0.0818)
2009/10	0.1535*	−0.0002	0.2057	0.1786*	0.0622	0.1701*
	(0.091)	(0.1524)	(0.1353)	(0.1071)	(0.1445)	(0.0983)
Intercept	0.6602***	0.4741***	0.7204***	0.7156***	0.6095***	0.6636***
	(0.0975)	(0.1580)	(0.1720)	(0.1181)	(0.1415)	(0.1130)

Note: Asterisks *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. Robust standard errors are in parentheses. Standard errors are clustered at the school level. (a) Children in low-income block groups in all four periods of observation; (b) Rural (urban) subsamples include children in rural (urban) census blocks in all four periods of observation; (c) Movers (non-movers) changed (did not change) location of residence between the 1st and 2nd grades.

in table 3 at 0.0302 standard deviations (p-value = 0.051). Among the subsample of movers, the food desert coefficient after including school fixed effects is 0.0571 standard deviations with a p-value of 0.011. Overall, this is evidence that the food desert effect is robust to the inclusion/exclusion of school fixed effects.

Alternative Measures of Food Access

In table 4, the food desert coefficient is compared to alternative measures, including low food access and distance in miles from the child's residence to the nearest supermarket. In the case of the low-income sample, the low-food-access measure corresponds exactly to the food desert measure. For all other samples, the low-food-access measure will encompass all the food desert children and additional children from higher-income neighborhoods that lacked a nearby supermarket. The fact that the low-food-access measure is not statistically different from zero across these samples is evidence that a lack of nearby supermarkets itself is not

Table 4. Effect of Food Desert, Low Food Access, and Distance to Supermarket on BMI z-score by Sample

Measure	All	Low-income[a]	Rural[b]	Urban[b]	Movers[c]	Non-movers[c]
Observations (N)	110,384	27,504	36,892	62,256	16,336	94,048
Panel (a): Fixed effects estimate from regressions of BMI z-score on food-desert status (same as in table 3)						
Food Desert	0.0384**	0.0388*	0.0312	0.0594***	0.0482**	0.0230
	(0.0153)	(0.0227)	(0.0300)	(0.0212)	(0.0202)	(0.0269)
Panel (b): Fixed effects estimate from regressions of BMI z-score on the measure of low food access						
Low Food Access	0.0020	NA[d]	−0.0220	0.0020	−0.0088	0.0077
	(0.0070)		(0.0167)	(0.0095)	(0.0102)	(0.0088)
Panel (c): Fixed effects estimate from regressions of BMI z-score on distance to the nearest supermarket						
Nearest Supermarket	−0.0009	0.0012	−0.0018	0.0018	−0.0035	−0.0003
	(0.0012)	(0.0026)	(0.0018)	(0.0018)	(0.0022)	(0.0013)

Note: Asterisks *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. Robust standard errors are in parentheses. Standard errors are clustered at the school level. Each coefficient is from a separate regression model that includes all covariates reported in table 3. (a) Children in low-income block groups in all four periods of observation; (b) Rural (urban) subsamples include children in rural (urban) census blocks in all four periods of observation; (c) Movers (non-movers) changed (did not change) location of residence between the first and second grades; (d) When the sample is restricted to low-income children, the food desert measure is identical to the low food access measure.

associated with increases in BMI across the population of children. The absence of supermarkets manifests differently in low-income communities and in ways that are associated with weight gain. Thus, food desert areas are associated with BMI in ways that food access alone is not.

Nevertheless, the distance to the nearest supermarket is not statistically different from zero in any of the samples, including the sample of low-income children. Given the strong associations between food desert status and BMI, it is a little surprising that distance to the nearest supermarket is not also associated with BMI in the lower-income subsample. This raises the possibility that food desert areas may be obesogenic in ways other than food access.

DID Estimates of the Food Desert Effect

The DID measures of the food desert effect are presented in tables 5 and 6. As described above, we use the two periods of data prior to the transition into or out of food deserts in a series of placebo DID regressions to assess whether the counterfactual trend in BMI z-scores among transitioning children is likely to be parallel to those whose desert status remained stable. Findings of these placebo regressions are summarized in the supplementary appendix online and show no statistical difference in the transitioning and stable children prior to the change in desert status.

Among those who transitioned into food deserts, the DID estimate from the sample of all children is 0.064 standard deviations (table 5). Although there is a substantial degree of overlap in the confidence intervals of all estimates reported in table 5, the largest point estimate is from the sample of urban children, while the smallest is in the sample of low-income children. The latter is not significantly different from zero, which is in contrast to the fixed-effects estimate of the food desert effect for the low-income children reported above in table 3.

In table 6, the effect of a transition out of a food desert residence on BMI z-score across all children is −0.045 standard deviations and is significant at the 10% level ($p - \text{value} = 0.07$). With the exception of the sample of non-movers, the point estimates from the subsamples in table 6 fall within one standard error of this estimate. However, estimates from the subsamples are not different from zero at conventional critical values. A comparison of standard errors with those reported in table 5 indicates that estimates from the samples of all children, low-income children, and urban children are as precisely estimated as the analogous coefficients reported in table 5, despite having much smaller sample sizes. This could reflect the fact that, in the table 6 samples, stable children are from low-income neighborhoods

Table 5. DID Estimates of the Food Desert Effect for Children Switching into Food Deserts

Sample	Food Desert Effect	Robust SE	Number of Observations		
			Transitioned[a]	Stable[b]	Total
All	0.0640***	0.0248	1,088	102,276	103,364
Low-income[c]	0.0260	0.0333	748	20,412	21,160
Rural[d]	0.0566	0.0379	344	34,808	35,152
Urban[d]	0.0958***	0.0347	628	56,668	57,296
Movers[e]	0.0789**	0.0366	616	14,576	15,192
Non-movers[e]	0.0651**	0.0302	472	87,700	88,172

Note: Asterisks *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. Standard errors are clustered at the school level. Each coefficient is from a separate regression model. (a) Observations from children who switched into food deserts from non-deserts between first and second grades; (b) Observations from children who were always observed to be in non-desert residences; (c) Children in low-income block groups in all four periods of observation; (d) Rural (urban) subsamples include children in rural (urban) census blocks in all four periods of observation; (e) Movers (non-movers) changed (did not change) location of residence between the 1st and 2nd grades.

Table 6. DID Estimate of the Food Desert Effect for Children Switching Out of Food Deserts

Sample	Food Desert Effect	Robust SE	Number of Observations		
			Transitioned[a]	Stable[b]	Total
All	−0.0450*	0.0249	1,236	5,784	7,020
Low-income[c]	−0.0328	0.0336	560	5,784	6,344
Rural[d]	−0.0418	0.0530	220	1,520	1,740
Urban[d]	−0.0430	0.0314	744	4,216	4,960
Movers[e]	−0.0652	0.0703	876	268	1,144
Non-movers[e]	0.0066	0.0470	360	5,516	5,876

Note: Asterisks *, **, and *** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. Standard errors are clustered at the school level. Each coefficient is from a separate regression model. (a) Observations from children who switched into non-desert areas from food desert areas between first and second grades; (b) Observations from children who were always observed to be in a food desert residence; (c) Children in low-income block groups in all four periods of observation; (d) Rural (urban) subsamples include children in rural (urban) census blocks in all four periods of observation; (e) Movers (non-movers) changed (did not change) location of residence between the first and second grades.

and are therefore similar to the transitioning children. Differences between the stable and transitioning children have implications for whether the food desert estimates yield a causal interpretation. We address this further below.

Limits to Causality

The results presented above provide evidence that food deserts are associated with higher BMI z-scores among elementary-aged schoolchildren, but how strong of a case is there that a food desert is a causal factor in childhood weight gain? Many of the changes in food desert status are a consequence of the child's family having moved into a food desert from a non-desert or *vice versa*. In these cases, the change in desert status is clearly not exogenous, and the food desert effect would be confounded with factors that led to the change in residence. This prevents us from placing a causal interpretation on our estimates from samples that include movers.

Among the non-movers, however, the change in desert status is due entirely to the entry or exit of a neighborhood supermarket. An argument can be made that such occurrences are exogenous to the households of children in our sample. No single household is important enough to meaningfully affect store locations, and in most cases, households would lack sufficient foreknowledge to base their choice of residential location on future store openings or closings.

Given this argument, let us focus specifically on the samples of non-movers. The effect of a transition into food deserts by reason of supermarket closure is 0.0651 standard deviations and is statistically different from zero (non-moving sample in table 5). However, there is no significant effect among those who transition out of a food desert by reason of a new supermarket (non-moving sample in table 6). Moreover, the fixed-effects estimate of the average food desert effect from non-movers is not statistically different from zero (table 3).

In short, the only evidence that food-deserts matter among non-movers is from children who transition into food deserts from non-deserts (table 5). It is possible that effects are asymmetric and the exit of a supermarket contributes more to weight gain than entry facilitates weight loss. However, the table 5 results could reflect income differences between the transitioning and stable children. The non-moving children in the stable column of table 5 were never in food deserts and it is clear from the reported sample sizes that most of these stable children were not from low-income areas. The non-moving children in the transitioning column of table 5, on the other hand, were in a low-income area both before and after the closure of the supermarket.[9]

To address this concern, we further homogenized the sample of non-movers in table 5 so that the stable children were also drawn only from low-income neighborhoods. We then re-estimated the DID regression on this restricted sample ($N = 18,292$). The resulting DID coefficient is −0.0027 with a standard error of (0.0320), and so there is no longer evidence of a food desert effect among non-movers once the sample is further restricted to low-income children.

In sum, there is little evidence of a causal effect based on what can arguably be considered exogenous entry and exit decisions of supermarkets during our sample period. This is not the same as concluding that there is no causal effect. Rather, the evidence does not convincingly establish causality because the strongest associations between BMI and food desert status depends on the inclusion of children who changed desert status as a consequence of an endogenous change in residence.

Still, there is evidence of an association between food deserts and weight gain among schoolchildren. The samples of movers were constructed so that both transitioning and stable students experienced a change in residence between the first and second grades. This should partially account for moving-related stress that could impact weight gain. Students moving into food deserts would still generally be moving into worse neighborhoods, which may be symptomatic of hardships within the child's household that affect weight gain in ways not experienced by other children who changed residences. To explore this concern, samples of movers are restricted to include only low-income children. The fixed-effects estimate of the food desert coefficient from the sample of low-income movers ($N = 2,928$) is 0.0492. This is virtually identical to that reported in table 3, but it is estimated with less precision. The standard error increases to 0.0376. Similarly, when movers in table 5 are restricted to be from low-income neighborhoods ($N = 2,120$), the DID estimate of the food desert effect is 0.0730 and is nearly identical to the point estimate reported in the table. Again, the estimate is less precise with a standard error of 0.0630. Given the decrease in precision, neither of these estimates is statistically different from zero, but the similarity of the point estimates provides evidence that the food desert effect is not simply a function of the income differential between first- and second-grade neighborhoods. There appears to be something unique to food deserts that contributes to weight gain even after homogenizing the moving samples by neighborhood income status.

Food Deserts Facing Urban and Rural Children

Our results provide evidence that food deserts are more strongly associated with BMI increases among urban children than among rural children. In fact, there is no case where an estimate from the rural subsample is statistically different from zero. Food desert estimates from the urban subsamples are significant at the 1% level in all but table 6. While there is a substantial degree of overlap between the confidence intervals of estimates from the urban and rural subsamples, it is noteworthy that point estimates of the food desert effect are largest in the urban subsamples across tables 3 through 5.

[9] As described above, we test the assumption of parallel trends and find no difference between transitioning and stable students for the non-movers in table 5. However, the parallel trends assumption involves an unobserved counterfactual, and it is possible that there are differences between the stable and transitioning students despite our failure to reject the hypothesis of parallel trends.

One possibility is that the food desert measure more accurately reflects the constraints facing the families of urban children. Table 2 shows that, on average, the families of rural children are 6 miles from the nearest supermarket. Given this distance, the typical rural family will depend on automobile transportation. Indeed, in comparison to urban neighborhoods, the proportion of households without a vehicle is smaller in rural neighborhoods.[10] Focus groups of rural parents reveal that automobiles provide flexibility to shop for deals at multiple food stores during trips to urban centers (Yousefian et al. 2011). Rural households may have other ways to alleviate a lack of nearby supermarkets. For example, Yousefian et al. (2011) also find that rural parents rely on home freezers to store foods bought in volume on shopping trips and to preserve seasonal foods obtained locally.

To the extent that these findings apply similarly to our sample, families in rural food deserts may have more flexibility in accessing healthy foods than families in urban food deserts. Urban food deserts could also be more obesogenic given a greater density of alternative store types with less healthy options. For example, the average number of fast food restaurants surrounding the urban subsample is six times greater than the average for the rural subsample (table 2).

On the other hand, if the food-access constraints facing rural households are more complex, it could simply be more difficult to detect a statistically meaningful association between rural food deserts and childhood weight gain. For example, gasoline prices are likely to have a larger impact on the rural poor. The income effects from fluctuations in gasoline prices could be influential to food purchases or could affect physical activity by restricting a low-income family's ability to pay for a child's participation in organized sports.

Importance of the Association between Food Deserts and BMI

Findings presented above indicate that food deserts are associated with increases in BMI z-scores of roughly 0.04 standard deviations on average, and may approach 0.10 standard deviations for urban children who transition into food deserts. To put this in context, a ten-year old boy measuring four feet and six inches in height with a weight of 75.4 pounds would have a BMI z-score of 0.65, which is near the sample average reported in the first column of table 2. Keeping height constant, a weight gain of only one-half pound would raise his z-score 0.04 standard deviations to 0.69. A weight gain of just over one pound would raise his z-score 0.10 standard deviations from 0.65 to 0.75. In short, the food desert effect we find is fairly small in terms of body weight. However, we were careful to control for length of exposure in constructing our analysis sample, and food deserts could be of greater importance to body weight over longer periods of exposure than those we examine here.

Nonetheless, the associations between food deserts and body weight that we find are not trivial when compared to the effects of school-based interventions targeting similarly-aged children. Shirley et al. (2015) provide a review of recent studies that measure the effects of school-based nutrition, education, and/or physical activity interventions aimed at children aged 6 to 12 years. Eight of the studies provide evidence that the interventions were effective in addressing childhood obesity, and five reported significant effects in terms of BMI (kg/m^2), BMI z-scores, or BMI percentiles, each of which can be contrasted, more or less, to the estimates we report here.

It turns out that the associations between food desert exposure and weight gain that we estimate are comparable to the estimated intervention effects. Speroni, Earley, and Atherton (2007) and Hendy, Williams, and Camise (2011) find intervention effects of -2.3 to -2.6 BMI percentiles. At our sample average BMI z-score of 0.65, percentile reductions of this magnitude would translate into about -0.06 standard deviations. Hollar et al. (2010) find that a school-based intervention emphasizing physical activity, improvements in school meals, and nutrition education reduced the BMI z-scores of elementary schoolgirls by 0.03 standard deviations. Barbeau et al. (2007) and Howe, Harris, and Gutin (2011) examine after-school physical activity programs and show larger impacts on BMI z-scores of 0.10 to 0.12 standard deviations among third to

[10] The difference in vehicle ownership between the urban and rural subsamples in table 2 is significant based on a t-test for differences in the sample means (p-value < 0.01).

fifth grade African American girls and boys, respectively.[11]

One important take-away here is that school-based interventions may be sufficient to offset the weight gain that is associated with life in a food desert. One caution, however, is that the features of food deserts that contribute to weight would tend to persist over time, while some of the intervention effects summarized above reflect short-term outcomes. Moreover, lack of supermarket access or other features of food deserts could hinder changes in the behaviors being targeted by the intervention programs.

Summary and Conclusions

In this paper we examine the impact of food deserts on the BMI of elementary schoolchildren using a sample that has allowed us to control for children's exposure to food deserts in space and over time. While our results suggest that exposure to food deserts may facilitate weight gain, they do not show conclusively that food access is what matters, nor do they convincingly establish a causal link between food deserts and childhood weight gain. The key challenges of the analysis are twofold. One is in finding exogenous sources of change in the food desert statuses of children in our sample. The other is the fact that food deserts are, by definition, lower income areas, and obesity is inversely correlated with socioeconomic status.

Still, we do present evidence of a positive association between food desert exposure and childhood BMI. The strongest evidence is among children in urban areas. It is possible that physical distance is a more meaningful constraint facing urban households, but it is also possible that the food desert measure we use may not adequately reflect food access issues confronting rural families. This underscores the need to think carefully about measures of food accessibility and to develop improved measures of the food environment, as called for by Ver Ploeg, Dutko, and Breneman (2015).

At a minimum, there is evidence that living in a food desert is an additional risk factor and that it is reasonable to consider an area's food desert status among the criteria used to prioritize interventions aimed at childhood obesity. However, it is not entirely clear that food access is the only problem in these areas. In fact, once our samples were homogenized for neighborhood income, there was no evidence that nearby supermarket openings or closings affected childhood BMI in low-income areas. If food deserts are obesogenic in ways that simultaneously make them less profitable locations for supermarkets, then public-private financing programs, industry incentives, or other efforts designed to improve the food environment in lower-income neighborhoods might not result in meaningful reductions in childhood obesity. There is evidence that school-based interventions could offset the weight gain that is associated with living in a food desert, although more work is needed to demonstrate that school-based interventions are effective in areas like food deserts with chronically poor environmental conditions.

Supplementary Material

Supplementary online appendix is available at http://oxfordjournals.org/our_journals/ajae/online.

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