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Food Deserts: Myth or Reality?

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Abstract

In 2010, the White House announced the goal of eradicating food deserts—low-income neighborhoods without nearby supermarkets—in seven years. The efficacy of this initiative is premised on the presumption, mostly untested in 2010, that food deserts significantly contribute to health disparities in low-resourced communities. We synthesize the post-2010 line of research that seeks to establish causality in the relationship between food access and nutrition/health. All things considered, there is so far little evidence that food deserts have a causal effect of meaningful magnitude on health and nutrition disparities. The causes of diet quality disparity lie more on the side of food demand than on supply. Therefore, from the public health perspective, policies that lower the relative price of healthy food or change the "deep parameters" of preferences in favor of healthy food would be more appealing than eliminating food deserts.

INTRODUCTION

> Where you live matters and it matters a lot (when it comes to obesity and related chronic diseases). Another way of putting this is: Does your ZIP code matter more than your genetic code?
>
> —Chaykin (2012, 21:35)

From 1999–2000 to 2017–2018, US adult obesity prevalence increased from 27.5% to 43.0% for men, and from 33.4% to 41.9% for women (Ogden et al. 2020). The same study finds that the prevalence of childhood obesity increased to a lesser degree in the same period but still stood at 19.3% among children aged 6–11 years and 20.9% among adolescents aged 12–19 years during 2017–2018. Between 1998 and 2006, the annual medical costs attributable to obesity are estimated to have risen from 6.5% to 9.1% of aggregate US medical spending or $1,429 per capita in 2008 dollars (Finkelstein et al. 2009). An instrumental variables estimate that corrects selection and measurement error in self-reported height and weight is even higher, at about twice the amount (Cawley & Meyerhoefer 2012).

Weight gain is a direct result of imbalance between energy intake and expenditure. Overweight and obesity are extensively documented to be inversely associated with diet quality in cross-sectional studies (Guo et al. 2004, Tande et al. 2010), retrospective cohorts (Wolongevicz et al. 2010, Fung et al. 2015), and randomized controlled trials (Epstein et al. 2008, Ebbeling et al. 2012). The diet of most Americans is suboptimal in nutritional quality when compared to the *Dietary Guidelines for Americans 2015–2020* set forth by the US Department of Health and Human Services and US Department of Agriculture (US HHS/USDA 2015). During 2007–2010, about 75% of the population did not meet the recommended daily amounts of vegetables, fruit, dairy, and oils; and regarding nutrients that should be consumed in limited amounts, over 70% of individuals exceeded the daily limits on added sugars, saturated fats, and sodium (US HHS/USDA 2015, figure 2-1). Although adherence is higher for total grains at close to 60%, far fewer Americans met the recommendation that whole grains account for at least half of total grain consumption (US HHS/USDA 2015, figure 2-5).

In addition to the poor diet quality of the overall population, disparities exist by socioeconomic status (SES). Adherence to recommendations is higher among higher-income consumers than low-income consumers for most but not all food groups. For example, during 2001–2004, larger percentages of high-income adults met the minimum recommendations for fruit, vegetables, whole grains, meat and beans, dairy, and oils; but for discretionary calories from solid fats and added sugars, there is little income gradient in the proportion of people consuming below the limit (Kirkpatrick et al. 2012). When assessed holistically using summary indexes, diets of lower-SES individuals receive lower overall scores, on average, than those of higher-SES individuals. For example, Zhang et al. (2018) report means of 32.1, 36.8, and 39.6 in American Heart Association diet scores (0 = least healthy, 80 = healthiest) for participants in the Supplemental Nutrition Assistance Program (SNAP), low-income nonparticipants, and higher-income individuals during 2013–2014, respectively.

Given that poor diet is etiologically linked to obesity and other health conditions and diseases, such as diabetes, cardiovascular diseases, and nutrition-related cancers (Danaei et al. 2009), there is a perpetual interest among policy makers and researchers in policy options that might improve the diet quality of the overall population and eliminate disparities among subpopulations. Healthy eating policies can be categorized based on the mechanisms through which a policy is designed to modify dietary behaviors. There are at least three types: information provision, pricing, and access policies. Information provision is perhaps the least contentious among stakeholders and, hence, more likely than the other two to become law. Two examples include the Nutrition Labeling and Education Act of 1990 that mandated nutrition facts labels on most packaged foods and the 2016

US Food and Drug Administration (FDA) revisions that made calories per serving size more salient and required added sugars on the label. If successful in changing dietary behaviors, information provision can be welfare enhancing, even before accounting for future health benefits, because it changes the "deep parameters" of consumer preferences (Teisl et al. 2001).

Pricing policies seek to change the relative shelf price of healthy and unhealthy foods through targeted taxes and subsidies. One of the most recognizable pricing strategies related to healthy eating is the ≥1 penny/ounce excise taxes on sweetened beverages currently collected in seven US cities. Although evidence is mixed on the extent to which these beverage taxes reduce sugary drink intake (Falbe et al. 2016, Silver et al. 2017), there are associated market inefficiencies such as the deadweight loss and cross-border shopping (Cawley et al. 2019).

Access policy aims to raise (lower) the effective price of unhealthy (healthy) foods by making them less (more) accessible. One such example is the removal of sugar-sweetened beverages from school vending machines. There is more evidence that compensatory behaviors such as increased home consumption negated much of the direct effect of a school ban (Fernandes 2008, Fletcher et al. 2010, Lichtman-Sadot 2016) than there is indicating a lack of compensation (Huang & Kiesel 2012).

One particular food access issue that stimulated enormous academic and policy interests and resulted in policy actions from the highest levels of the US government concerns the existence and consequences of food deserts. Food deserts refer to neighborhoods where access to affordable healthy foods is restricted by a lack of large food retailers within a convenient traveling distance. Although there are many ways to designate an area as a food desert, the two common chief criteria are population income and distance to the nearest large food store. In its first iteration of the Food Access Research Atlas, the USDA Economic Research Service (ERS) identified 6,529 low-income[1] census tracts with a combined 2006 population of 13.6 million as food deserts, where at least 500 people or 33% of the tract population live more than 1 mile from a large food store in urban areas or more than 10 miles in rural areas (Dutko et al. 2012). By this definition, the estimated number of food deserts increased to 8,894 census tracts in the contiguous United States in 2010 largely owing to the increase in low-income tracts following the Great Recession (USDA ERS 2014, p. 40).

It is hypothesized that the existence of food deserts has a causal adverse effect on the diet quality of residents. If the hypothesis is supported with evidence, the elimination of food deserts may help reduce the nutrition and health disparities between lower- and higher-SES population subgroups. It is these potential health and other social benefits (e.g., job creation, economic development) of tackling inequitable access that excited US politicians and policy makers. In 2010, First Lady Michelle Obama's *Let's Move!* campaign announced the goal of eradicating food deserts by 2017 (Croft 2010). A centerpiece of this policy push was the 2011 launch of the Healthy Food Financing Initiative (HFFI) at the Department of the Treasury and HHS. The 2014 Farm Bill officially established HFFI at the USDA. The program provides grants and technical assistance to community development organizations to construct new grocery and other types of retail food stores or renovate them to increase the availability of healthy foods in underserved, low-resourced communities. **Figure 1** illustrates trends in HFFI investments since its inception.

[1]The classification of a low-income tract follows the eligibility thresholds of the US Department of the Treasury's New Market Tax Credit program: a poverty rate of 20% or higher, or a median family income at 80% or less of the metropolitan area's median family income or the statewide median family income if the tract is in a nonmetropolitan area.

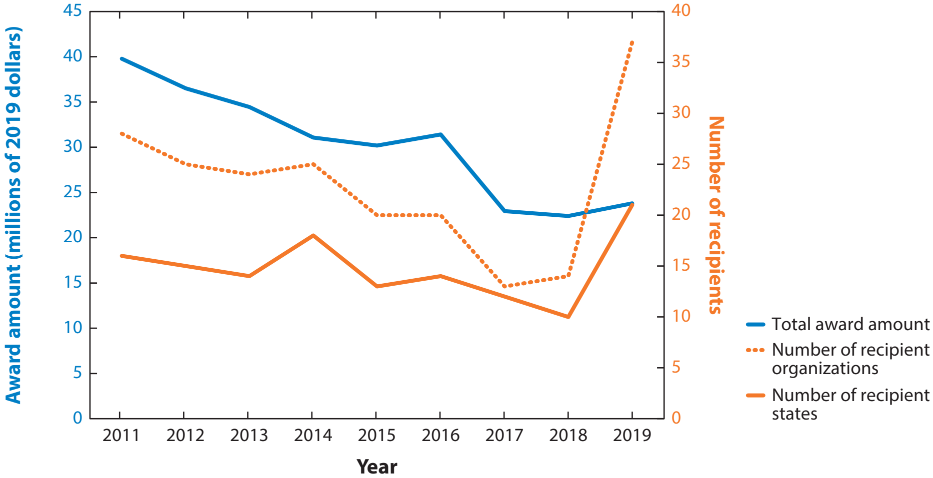

Figure 1

Trends in Healthy Food Financing Initiative investments (grants and technical assistance). Data from **http://www.healthyfoodaccess.org; http://www.cdfifund.gov; http://www.investinginfood.com/impact/**.

The level of investment steadily declined from the peak of $39.8 million (in 2019 dollars) in 2011 to $23.8 million in 2019.[2] There is also indication of a decreased focus on healthy food access in the evolution of federal public health goals. In 2010, Healthy People 2020 included an objective to "increase the proportion of Americans who have access to a food retail outlet that sells a variety of foods that are encouraged by the Dietary Guidelines for Americans" (US HHS 2010, objective NWS-4). By 2020, retail food access was no longer mentioned among the nutrition and healthy eating objectives of Healthy People 2030 (US HHS 2020). These trends are consistent with the growing empirical evidence that food deserts have little to no causal effect on the diet quality of residents within the time frame that can be practically tracked for an objective evaluation.

The objective of this review is to provide a detailed, in-depth discussion of findings from the small but critical branch of US food access research aimed at drawing causal inferences. This article is not meant to be a systematic review of the vast literature documenting the cross-sectional correlation between nutrition/health and the food environment. The latter literature has been influential in stimulating initial policy discussions and actions toward nutrition and health disparities and motivating causal research. Interested readers may refer to articles by Black & Macinko (2008), Larson et al. (2009), Cobb et al. (2015), and others for excellent reviews of the correlational studies, with the caveat that correlation does not imply causation and evidence-based policy making demands causal evidence.

The remainder of this article is organized as follows. The next section discusses some of the economic issues related to food deserts. This is followed by an in-depth look into the causal studies that rely on longitudinal data with individual fixed effects, quasi-experiments, or instrumental variables for identification. The penultimate section discusses limitations and potential extensions of current research. The last section concludes.

[2]The 2019 surge in the number of recipient organizations and states was a result of awarding small grants and providing technical assistance to a number of organizations.

BACKGROUND

Although the public health and medical journals publish the overwhelming majority of food desert studies, economics, which studies the allocation of scarce resources, can play an important role in the quest to understand the causal effects of food deserts on nutrition and health disparities. Bitler & Haider (2011) examine the pre-2010 food desert literature through the economic lens. They raise conceptual and methodological issues that need to be addressed before researchers can recommend sound policies. Some are measurement issues, such as profiling foods into healthy and less unhealthy products, defining the geographic scope of a food desert, and assessing the quality of food establishment registries. The authors note that because retail food outlets are often substitutes, focusing on accessibility of one establishment type or store format, as is often done, may lead to erroneous conclusions. Importantly, they challenge food desert researchers to identify the underlying causes of food deserts, if these areas exist in the first place. If food deserts and their effects on public health are primarily supply driven, then public policies such as zoning, tax relief, and financing might be effective. But if food deserts are caused largely by the lack of demand for healthy foods, then health promotion and benefit increases in food assistance (e.g., SNAP and the Special Supplemental Nutrition Program for Women, Infants, and Children) that change preferences or relax the budget constraints are more likely to work.

People with lower preferences for healthy food or with worse health conditions may self-select into food deserts for reasons unobservable to researchers. The possibility that health and the neighborhood food environment are endogenously determined can be illustrated in the results of Chen et al. (2016). The authors use IRI Consumer Network household scanner data to examine the associations of obesity with a number of social and environmental factors. They calculate the 5-year average adherence of household retail purchases to the USDA recommended food compositions. The measure, called USDAScore, is used as a covariate in the regression of household members' obesity status on the USDA food desert indicator and a number of other food establishment density variables. If the causal pathway for food deserts to affect obesity is through retail food purchases, inclusion of USDAScore should attenuate the food desert coefficient toward zero. To the contrary, the food desert status is associated with an increase of 30% (19%) in the odds of being obese (overweight). In fact, this association is the strongest and most consistent among all neighborhood food environment measures examined by the authors.[3]

It is well documented that most people do not use the closest food store as their primary store (Ver Ploeg et al. 2017). However, if the store where a consumer shops can be moved closer to home, there is likely gain in consumer welfare. Taylor & Villas-Boas (2016) use a multinomial mixed logit model and the USDA's National Household Food Acquisition and Purchase Survey (FoodAPS) to estimate households' preferences for store types and willingness to pay for shorter travel distance. Consistent with a priori expectations, the authors find that travel distance is inversely related to mean utility. To account for the endogeneity of travel distance with demand, travel distance to the store of choice is instrumented by distance to the nearest store of each type, which is arguably predetermined. Across all retail formats and household types, supercenters and supermarkets command the highest willingness to pay. This result is not surprising given households' strong preferences for the two formats, as reflected in their 58% share of total food

[3]USDAScore is an imperfect of measure of the home food environment. It is possible that the causal effect of food deserts on obesity through retail purchases is not entirely captured by variations in USDAScore. This would mean that the food desert indicator would still be statistically significant even with USDAScore as a covariate. Thus, Chen et al.'s (2016) results should be viewed as suggestive evidence of self-selection.

expenditures in FoodAPS. An average FoodAPS household is willing to pay about $2.5 per week to bring a supercenter or supermarket 1 mile closer to home. The estimates are about twice as high for SNAP households. The higher preferences for supercenters and supermarkets are likely due to the lower prices and greater varieties at these stores compared to other retail formats.

Retail scanner data offer the specificity and granularity required to compare variety and price differences between food deserts and other neighborhoods. Chenarides & Jaenicke (2019) examine the association between retail fruit/vegetable assortment in the IRI InfoScan retail scanner data and food access at the census tract level. They measured food access by the proportion of housing units without access to a vehicle and living ≥0.5 miles from the nearest supermarket. After controlling for county fixed effects and a number of time-varying county- and tract-level sociodemographic features, they find that a one-standard-deviation decrease in access is associated with a 0.07-standard-deviation decrease in fruit/vegetable assortment in 2010. The magnitude of this association steadily increased to 0.1 standard deviations by 2015.

Fan et al. (2018) unify the price-variety comparison by calculating an exact price index (EPI) for food-at-home items in food deserts and other neighborhoods. Feenstra (1994) develops the EPI for comparing the cost of living that accounts for differences in product availability across areas. The costs of unavailable products are set to their virtual prices—the price levels that drive demand to zero. So in areas where more products are unavailable, their EPI will be higher than in areas with greater product variety. If the unavailable products have a smaller budget share overall or are highly substitutable with products that are available, their unavailability in some local markets will have a lesser impact on the local EPI. Intuitively, both cases suggest that the unavailable products are not essential in consumer preferences and, hence, their absence should have no major impact on the cost of living. A key metric for constructing the EPI is the elasticities of substitution among hundreds of thousands of products. Broda & Weinstein (2010) estimate barcode-level elasticities of substitutions within a brand and across brands within a product group for about 122 product groups in scanner data, including most groups of foods for home consumption. Fan and coauthors use these elasticities and IRI InfoScan to calculate the EPI for FoodAPS counties. They create two alternative definitions of the marketing area for a census tract: the local market and own tract-only market. The local-market EPI assumes that residents of a census tract shop in their own and contiguous tracts. The own tract-only EPI assumes that residents shop within the boundary of their own tract. Under the local market definition, without controlling for sociodemographic variations across tracts, the prices of commonly available food items in food deserts are about 2% lower than in other types of tracts. When combined with the variety effect, the local market EPI in food deserts is 2.6% lower than in low-income high-access tracts, but 3.1% and 2.3% higher than in high-income low-access and high-income high-access tracts, respectively. The variety difference between low- and high-income tracts is primarily driven by lower variety within food groups, rather than missing food groups in low-income tracts. Controlling for observable between-tract sociodemographic differences that may be correlated with demand, low access is associated with a 3.5% higher local market EPI, where 74% of this cost difference is driven by the variety effect. Fan and coauthors argue that for people with the means to shop beyond the boundary of their own tract, the effect of food deserts on food choices appears to be trivial. However, residents of food deserts who are mobility constrained to shop within their own tracts pay significantly higher prices: The own tract-only EPI in food deserts is 9.2–15.5% higher than other tracts, overwhelmingly driven by the variety effect.

Assuming that healthy food is a normal good, through the income effect, the higher overall food price in food deserts may lower healthy food demand of those constrained to shop within their own tracts. But for people who travel further to shop at a supermarket, theoretical prediction about the effect of bringing a full-service food store closer to home on diet quality is ambiguous.

If the relative price of healthy food is lower at the new supermarket, healthy food demand will increase. However, if the new supermarket prices its products to local demand and if local demand is higher for unhealthy food, then unhealthy food may be promoted more frequently than healthy food (Chevalier et al. 2003). This would lead to a decrease in diet quality. Furthermore, travel cost is like a per unit tax vis-à-vis the grocery cart (Cowen & Tabarrok 1995). If consumers perceive a positive relationship between healthfulness and quality, the Alchian & Allen (1964) theorem[4] predicts that traveling further increases the proportion of healthy food purchased. This is because more people will acquire the higher-quality good than the lower-quality good when a per unit tax is added to both goods. Conversely, when a consumer no longer needs to travel far because of a new nearby supermarket, the mix of food in their grocery cart may become less healthy. In the end, the effect of food deserts on diet and health is an empirical question.

CAUSAL STUDIES

We group these studies by identification strategy: panel data with individual fixed effects, instrumental variables, and quasi-experiments with comparison groups. Each approach has its advantages and limitations. Fixed effects models control for time-invariant unobserved individual heterogeneity, but bias may occur if the unobserved heterogeneity varies over time. Instrumental variables could overcome this limitation, but identification relies on the exclusion assumption that cannot be directly tested. Quasi-experiments hold the promise of clear identification but often entail costly primary data collection. The resulting infrequent collection of preintervention data makes it difficult to test for the assumption of common trends between the treatment and comparison groups. As is well known, parallel trends prior to the policy intervention are required for residents of the comparison site representing a valid counterfactual of the experiences of residents of the treatment site. In addition, when the true effect of food deserts is small, the smaller sample size in quasi-experimental studies makes it more difficult to detect an effect than fixed effects and instrumental variables models, which can often count on extremely large secondary data sets. Thus, the extent to which these groups of studies are able to draw causal inferences varies despite each's effort in attaining clear identification.

We include several influential restaurant studies even though restaurants play no role in the designation of food desert status. When food at home and food away from home are substitutes, measuring both retail food stores and restaurants may be important (Bitler & Haider 2011). Food swamp is a metaphor for situations where availability of energy-dense food such as fast food overwhelms that of healthier options (Rose et al. 2010). There is evidence that when pitted against food desert measures, food swamp measures have a higher predictive power on adult obesity rates (Cooksey-Stowers et al. 2017).

Panel Data with Individual Fixed Effects

Currie et al. (2010) examine the effect of fast food restaurant supply on the obesity status of ninth graders and pregnant women. The authors obtain the exact location of each restaurant, school, and mother to estimate the effects of small increments in distance (0.1, 0.25, and 0.5 miles) on obesity. The school sample consists of more than 8,000 school-grade-year observations aggregated from more than 3 million cross-sectional observations of ninth graders in California over an 8-year period. Because ninth graders just transitioned from a middle school to a high school,

[4]The Alchian and Allen theorem is colloquially known as the "shipping good (Washington) apples out" problem.

they are more likely to experience a change in food environment around the school than students in other grades. In the authors' sample of schools, there is a 30-week period between the time they entered high school and when the fitness assessment is taken. The authors' lower-bound estimate (Currie et al. 2010, table 2, column 2) points to a 1.7-percentage-point increase (baseline obesity prevalence of 32.9%) in obese ninth graders due to the presence of fast food restaurants within a 0.1-mile radius of the school. Fast food restaurants at 0.25 and 0.5 miles have no effect on adolescent obesity, consistent with the expected nonlinear effect of distance on ninth graders who are not allowed to drive. Credibility of these causal estimates would be in doubt if fast food restaurants self-select into the immediate vicinity of a high school where demand is trending upward. The authors find no discernible effect on obesity of past and future fast food restaurant exposure conditional on current availability. This provides assurance that self-selection of fast food restaurants is not a problem in their data set.

From a sample of pregnant women representing more than 3.5 million births in Michigan, New Jersey, and Texas, Currie et al. (2010) examine the effects of fast food restaurant availability within a 0.1-, 0.25-, and 0.5-mile radius from the mother's exact residential address on weight gain during pregnancy. The observation of multiple pregnancies per mother allows the use of mother fixed effects. Changes in fast food restaurant proximity are due to moves to new addresses and restaurant entry and exit between pregnancies. Availability of fast food within 0.5 miles is estimated to lead to an average weight gain of 0.049 kg or 0.4% compared to the overall weight gain of 13.7 kg during pregnancy. The effect increases linearly from 0.5 miles to 0.1 miles, although the gradient is not statistically significant. This is consistent with the expectations that travel costs for adults are lower than ninth graders. There are substantial variations across race and education attainment. The effect on African Americans is more than three times the average, and on pregnant women with a high school degree or less, the effect is close to twice the average. Fast food restaurant availability has no effect on non-Hispanic whites and mothers with at least some college education. For both the teenage sample and the pregnant women sample, nonfast food restaurants have no effect at any distance, suggesting that either the lower price or the temptations of fast foods to people with self-control problems or both are at work.

Fitzpatrick et al. (2016) use the 2006 and 2010 waves of the Health and Retirement Study to examine the effect of food deserts on food insufficiency and material hardship of older adults. The authors rely on individual fixed effects and temporal changes in census tracts' food desert status per ERS definition to establish causation. They find that living in a food desert does not have significant effects on older adults' food insufficiency and material hardship. In addition, no food desert effects are found for older SNAP recipients. They do find, however, that living in a food desert increases the probability of being food insufficient by 12 percentage points for those not owning a vehicle. This segment of respondents represented 36% of respondents in food deserts in their sample. This is consistent with the view that car ownership alleviates food access difficulty for residents of food deserts. In a follow-up study, Fitzpatrick et al. (2019) add the 2015 wave of the Health and Retirement Study and 2015 ERS food desert measure to examine the effects of food deserts on overweight, heart disease, diabetes, high blood pressure, and self-reported overall health. The authors find little evidence that food deserts had a causal effect on the health of older adults.

In 2003, Arkansas became the first US state to surveil childhood obesity among public-school children. Thomsen et al. (2016) leverage 7 years of panel data on Arkansan children in kindergarten through fourth grade to examine the association between food deserts and the age-gender-specific body mass index (BMI) z-score controlling for individual fixed effects. Their analysis is distinguished from the rest of the literature in that it has the exact geographic coordinates of children's residences and focuses on long-term (≥2 years) exposure to food deserts. The authors find that being in a food desert is associated with a 0.04-standard-deviation increase in BMI z-score. To put this number into context, the authors calculate that a one-half-pound weight increase for an average-weight ten-year-old boy would be enough to increase his BMI z-score by 0.04 standard deviations. They note that this magnitude is comparable to the effects of school-based nutritional, physical, and educational interventions targeted to this age group. However, the authors are cautious not to attribute causation for two reasons. First, the estimated association is largely driven by children whose families relocated into or out of a food desert. There is reason to believe that the decision to move may be endogenous with BMI changes owing to lifestyle changes unrelated to food access. Among their nonmover samples, the authors find little evidence that switching of food desert status caused by the entry or exit of supermarkets, which are plausibly exogenous, has any effect on weight change. Second, recall that the ERS definition of food deserts consists of two necessary conditions: low access to supermarkets and residence in a low-income neighborhood. Once Thomsen et al. substitute distance to the nearest supermarket for the food desert indicator in their regression analysis, there is no longer any association between BMI z-score and this alternative measure of access. This leads to their suspicion that food deserts are obesogenic in ways other than being far from supermarkets.

Allcott et al. (2019) provide arguably the most comprehensive examination of the causal effects of food deserts on health disparities based on observational data. The study subsumes two independently developed working papers by Handbury and colleagues (2015) and Allcott, Diamond, and Dubé (2018). Allcott et al. use the Nielsen Homescan (~60,000 households) for household purchases of packaged foods, Nielsen Retail Measurement Services for store-level sales, and a data set of the opening dates and exact locations of all 6,721 new supermarkets in the United States during 2004–2016. Household diet quality and retailer healthfulness are measured by a lightly modified Healthy Eating Index calculated from household purchases and store-level sales of packaged foods, respectively. Three complementary approaches are pursued to increase robustness. First, an event study design is used to estimate the effect of supermarket entry within a 15-min drive from the centroid of a household's census tract. They find that in the first eight quarters of its opening, a new supermarket increases food desert household diet quality by 0.021 standard deviations (Allcott et al. 2019, table 2, panel B, column 3). The point estimate implies that local food access explains 1.5% of the dietary quality disparity between people in the lowest income quartile and those in the highest income quartile. The reason for such a small effect is that even Homescan households in food deserts were already shopping for groceries in supermarkets before the opening of a local supermarket. The entry simply shifted purchases from stores farther away to the new nearby store.

The authors employ a second event study to estimate the effects of changing retail environment healthfulness on diet quality for movers. The mover-based approach should provide an upper bound on the effect of a healthier retail environment. People that become more health conscious may self-select into a healthier neighborhood, and the changes in social network and peer groups may create a peer effect that amplifies the direct effects of improved food access. Even with these potential upward biases built in, moving a low-income household into a high-income neighborhood can explain no more than 3.2% of the diet quality disparity between low- and high-income populations.

The third approach of Allcott et al. (2019) is a structural estimation of food demand using a hybrid functional form for preferences over individual products, food groups, and nutrients. Using the parameter estimates, the authors simulate the effects of setting the prices and nutritional quality of food products available to low-income households to the same levels as those available to high-income households. This equalization of food access was found to only close 10% of the diet quality disparity, with the remaining 90% accounted for by preference heterogeneity. Based on the estimated price elasticities of demand, the authors predict that the entire diet quality disparity can be closed by subsidizing healthy foods for low-income households at a cost to government equal to 15% of the SNAP budget.

Instrumental Variables

The literature has also used instrumental variables to control for simultaneity of food demand and supply. To identify the causal effects of food access on nutrition outcomes, the instruments need to be (*a*) correlated with the supply of retail stores or restaurants (the relevance requirement), and (*b*), conditional on other covariates, uncorrelated with nutrition outcomes other than through their effects on food access (the exclusion restriction). Several studies exploit the variation in distance to Bentonville, Arkansas, where Walmart is headquartered, to instrument access to Walmart or supercenters in general. The relevance condition is easily met by a strong inverse correlation between Walmart penetration in a local market and the market's distance to Bentonville. This statistical association is created by Walmart's unique expansion strategy of saturating nearby markets before moving outward to more distant markets (Neumark et al. 2008). The validity of the exclusion restriction is more delicate and context dependent. Strategies for testing and meeting this condition are discussed below with the specific studies. On the food-away-from-home side, most studies use proximity to highways to instrument restaurant supply to local residents. The rationale is that restaurants, especially those serving fast food, tend to congregate near highway exits to serve travelers, and preferences of local residents are plausibly independent of those of travelers. Unfortunately, the instruments presented by the literature so far lack the specificity needed to separate the effect of fast food restaurants from that of full-service restaurants.

Courtemanche & Carden (2011) estimate the effect of Walmart supercenters on body weight from a sample of more than 1.6 million adult respondents from the 1996–2005 waves of the cross-sectional Behavioral Risk Factor Surveillance System (BRFSS) data. Respondents are linked with Walmart supercenter density (the authors' measure of Walmart accessibility) at the county level. There is a concern that the county distance to Bentonville be correlated with unobserved determinants of obesity. Obesity prevalence is higher on average in states closer to Arkansas than states farther away in the 1995–2005 period (CDC 2006). While Walmart's spatial and temporal patterns of expansion may explain some of this difference, it cannot account for all. To improve the plausibility of the exclusion restriction, the authors instrument Walmart supercenter presence with the interactions between year fixed effects and distance from the respondent's county of residence to Bentonville. This creates yearly within-county variations in the value of the instruments, which allow county fixed effects to be included as covariates to control for the time-invariant unobserved determinants of between-county obesity prevalence. To further strengthen the case for identification, among other respondent- and county-level observed characteristics, the authors also use interactions between county population density and year fixed effects to control for time-varying unobserved factors that are correlated with population density. They estimate that a one-standard-deviation increase in Walmart supercenter density (standard deviation of 0.79 supercenters/100,000 residents) leads to a 0.19-point increase in average BMI. A supercenter sells every food available at a full-service grocery store as well as nonfood items such as clothing, toys, and electronics. There are multiple pathways by which supercenter presence can impact obesity: by offering lower food (both healthy and unhealthy) prices and lower prices on inputs to both active (e.g., sporting goods) and sedentary (e.g., video games) lifestyles. Courtemanche & Carden's estimates reflect the net effect of supercenter accessibility on obesity.

Volpe et al. (2013) look into one of the potential pathways for supercenters to affect health: the effect of supercenters on the healthfulness of the grocery cart. Retail food purchases come from the Fresh Foods panelists of the 1998–2006 Homescan, who reported both barcoded and random-weight foods until 2006 (inclusive). Supercenter accessibility is measured by the market share of all supercenters, which are predominantly Walmart supercenters. The authors experiment with several strategies to control for the endogeneity of supercenter market share with household purchase healthfulness. In the specification with household fixed effects and interactions of year fixed effects with distance to Bentonville as instruments, the authors estimate that a 10% increase in supercenter share leads to a 1% decrease in the healthfulness of grocery purchases. This supports the hypothesis that supercenters increased obesity prevalence by reducing the nutritional quality of retail food purchases over the period examined by Courtemanche & Carden (2011). Volpe et al. also find that the magnitude of the supercenter effect has declined over time, presumably because of the improved healthfulness of foods sold at supercenters or changes in consumer preferences toward healthier foods. What remains unclear is whether the reduction in healthfulness is caused by price differences between healthy and unhealthy foods within supercenters or between supercenters and traditional supermarkets, or differences between supercenters and traditional supermarkets in the healthfulness of foods stocked.

Anderson & Matsa (2011) instrument the supply of fast food and full-service restaurants in rural areas to estimate the effect of restaurants on adult obesity. They use the restricted-access version of the BRFSS, in which respondents' locations are identified to the telephone area code and exchange level (i.e., the first 6 digits of the 10-digit phone number). Restaurant supply is measured by establishment counts at the ZIP (postal) code level. The reason for focusing on the rural population is to generate substantial variation in restaurant access. They argue that this is not possible in urban areas where restaurants are easily accessible for all residents. The instrument for the endogenous supply of restaurants is the straight-line distance to an interstate highway. Because restaurants, especially fast food outlets, tend to congregate near highway exits to capture traveler business and the highways lower travel costs to major cities for rural residents, towns closer to highways face lower effective prices for restaurant foods than towns farther away. To avoid invalidating the exclusion restriction because highway access can affect regional economic outcomes, the authors focus on comparing obesity between towns adjacent (0–5 miles) to a highway and towns slightly farther away (5–10 miles). Although the exclusion condition cannot be directly tested, diagnostic tools are available to provide suggestive evidence of its plausibility. To this end, the authors conduct a number of tests for covariate balance to show that the observed determinants of BMI are (*a*) similarly distributed between towns adjacent and nonadjacent to interstate highways and (*b*) uncorrelated with the instrument. Anderson & Matsa find that restaurant supply has no effect on BMI derived from self-reported height and weight. To explain the mechanism of the null finding, they analyze food intake data from the USDA Continuing Survey of Food Intakes by Individuals. They find that (*a*) individuals who dine out more frequently also consume more calories at home than others; and (*b*) controlling for individual unobserved heterogeneity, on days of eating out, individuals largely compensate for the large restaurant portions by eating less at home. The first result suggests that individuals with higher caloric needs self-select into restaurant meals, and the second is evidence of rational consumers optimizing their food decisions. Taken together, restraining restaurant supply is unlikely to meaningfully affect obesity in their rural sample consisting of mostly non-Hispanic whites.

Dunn (2010) uses the public-use 2004–2006 BRFSS data to examine the causal effect of fast food restaurants on adult obesity. This study differs from that of Anderson & Matsa (2011) by examining heterogeneous effects by gender, race/ethnicity, and urbanicity. Unlike Anderson & Matsa's approach of measuring accessibility in distance to the nearest restaurant of any type, Dunn uses county-level fast food establishment counts and counts per 100,000 county residents. He argues that distance from home to the nearest establishment is unlikely to dominate count or density as the best measure because there exist a variety of ways for people to come in contact with fast food (e.g., near work or along their commute). The number of interstate highway exits is used to instrument fast food availability. Dunn's covariate balance test results suggest that the instrument is associated with the exercise behavior of some demographic groups. However, he shows that the bias from this violation of the exclusion restriction is small and economically insignificant. Consistent with the results of Anderson & Matsa, Dunn finds that fast food restaurant availability has no effect on obesity in rural America. In counties populated between 90 and 400 people per square mile, a one-standard-deviation increase in the number of fast food restaurants increases the BMI of females (African Americans and Hispanics) by a statistically significant 0.7 (2.1) points. The estimates for high-population-density counties are again statistically insignificant. Dunn speculates that the significant effects for females and minorities in medium-density counties may reflect their more elastic demand for fast foods, which in turn may be associated with differences in the role of gender in household production and economic resources by race/ethnicity.

Qian et al. (2017) use the same data source as Thomsen et al. (2016) but focus on the effect of fast food restaurants in the home food environment on childhood obesity. To exploit the benefits of fixed-effects modeling, the authors limit their sample to schoolchildren who moved at least once while in the sample. This creates within-student variation in their instrument—distance from the student's residence to the nearest highway. Following Anderson & Matsa (2011), the authors conduct a series of balance tests and find no significant evidence against the exclusion condition. They find that an additional fast food restaurant within a 0.5-mile radius (base mean of 0.5 fast food restaurants) increases the BMI z-score by an average of 0.08 standard deviations. The effects are larger for white, rural, female, and higher-income school children. These results are somewhat inconsistent with those of Currie et al. (2010), Dunn (2010), and Anderson & Matsa (2011). Qian et al. caution that their results on heterogeneity may be specific to Arkansas and not readily generalizable to the nation.

Chen et al. (2013) use the percentage of land within a 0.5-mile radius of a respondent's residence that is zoned for commercial use and distance to a primary arterial road as instruments for fast food availability. Because fast food restaurants are only allowed in commercial-use zones and tend to locate near arterial roads where demand is not constrained to local residents, both instruments are supply-side variables. Chen et al. point out that density of fast food restaurants is really a proximity for effective fast food prices because competition reduces prices and proximity lowers travel cost. The authors use cross-sectional respondent-level data from Indianapolis, Indiana. They estimate that removing one fast food restaurant from an overserved area (defined as more than six fast food restaurants per square kilometer) reduces the BMI by 0.18 points (~0.03 standard deviations) for those within a 0.5-mile buffer. Because their model is overidentified, they test and conclude that the density of chain grocers, which is used as a control variable, is not endogenous with BMI. Although the authors do not elaborate their results related to chain grocer density, our calculation based on their preferred specification (Chen et al. 2013, table 6, column 3) indicates that a one-standard-deviation increase in supermarket density (SD = 0.722 chain grocers/0.5-mile radius) would be 36% more effective in reducing BMI than a one-standard-deviation reduction in fast food restaurant density (SD = 2.860 establishments/0.5-mile radius). Of course, this does not mean that building a new supermarket is more cost-effective in reducing obesity, because it depends on the cost of building a new supermarket relative to that of eliminating a fast food restaurant.

Quasi-Experiments

Elbel et al. (2015) evaluate the effects on children of opening a supermarket subsidized by New York City's Food Retail Expansion to Support Health program. The supermarket is in

Morrisania, a 1.6-square-mile community with majority low-income African American and Hispanic residents. Street-intercept surveys and follow-up telephone 24-h dietary recalls were used to collect food frequency, in-home food availability, and intake data for children ages 3–10 in Morrisania and a comparison community. Their difference-in-differences (DID) models find no consistent evidence of the new supermarket influencing food availability and consumption. Their survey finds that almost 90% of Morrisania already shopped at other supermarkets before the new supermarket opened. This leaves little value for the new store to add.

Cummins et al. (2014) evaluate the effects of a new supermarket in a low-income Philadelphia neighborhood that was classified as a food desert by the ERS's Food Access Research Atlas before the intervention. The supermarket is supported by the Pennsylvania Fresh Food Financing Initiative, after which the federal HFFI is modeled. The authors report that 26.7% of the intervention neighborhood residents adopted the new supermarket as their primary store, and 51.4% used it at least once for food shopping. The study finds that 6–11 months after the opening there was no significant change in fruit and vegetable intake or self-reported BMI. The only statistically significant positive change is an increase in perceived food access at the intervention site compared to the comparison neighborhood.

Researchers at the RAND Corporation conducted a series of studies to evaluate the effects of an HFFI-supported supermarket in the Hill District in Pittsburgh, Pennsylvania. The project, known as the Pittsburgh Hill/Homewood Research on Neighborhood Change and Health (PHRESH), continues to provide important quasi-experimental insights. Before the intervention, both the Hill District and the comparison site, Homewood (also in Pittsburgh), met the USDA definition of food deserts: Each had an annual mean household income below $15,000 compared to the median 2012 household income of $50,489 for the Pittsburgh metro area (US Census Bur. 2013), and average distance to the nearest supermarket was 1.73 and 1.45 miles for Hill District and Homewood residents, respectively. Dubowitz et al. (2015) report that at baseline, 99.1% of respondents reported shopping at a full-service supermarket at least occasionally. The team collected one wave of preintervention data 22–29 months before the supermarket opening and another wave 7–14 months after at the intervention and comparison neighborhoods. Respondents completed two 24-h dietary recalls 7–14 days apart in each wave. Their DID regressions point to changes in the expected directions: a 10% reduction in dietary energy, 23% reduction in added sugars, 9% decline in share of energy from solid fats, alcohol, and added sugars, and 5% increase in the Healthy Eating Index. In addition, the study observes a 17% improvement in neighborhood satisfaction and substantial increases (37–375%, depending on measures) in perceived access to healthy food. However, these dietary improvements are not, in fact, associated with regular use of the new supermarkets. In other words, there are no differential diet improvements between regular users and infrequent/never users in the intervention neighborhood. The only outcome significantly associated with the regular use of the new supermarket is increased perceived access to healthy food. In addition, there is no observed reduction in measured BMI following the supermarket opening. The authors speculate that the positive nutrition changes are related to the overall improvement in lifestyle following the community reinvestment.

Under the PHRESH project, Richardson et al. (2017) examine the effects of the new supermarket on the distal measures of economic and health conditions in the Hill District using DID regressions. They find 12-percentage-point declines in SNAP participation and the proportion of households experiencing low/very low food security at the intervention site relative to the comparison site. However, they find no statistically significant difference in employment or proportion of households below the federal poverty line. Although they detect a significant improvement in per capita annual income at the intervention site, it may not be attributed to the new supermarket because their 2014 follow-up survey asks for household income in the previous year and the supermarket opened in October 2013. In terms of health outcomes, they observed between 3.6- and 9.6-percentage-point declines in the prevalence of self-reported diabetes, arthritis, and high cholesterol.

Between 2013 and 2016, the 10,219 residents of the Hill District (1.37-square-miles) received a total investment of $194 million. In addition to the new $11-million supermarket, the investments included several public housing developments, a community center, and an energy innovation center for workforce development and business incubation. The comparison neighborhood, Homewood, received only $48 million for the same period. Using 2013 baseline and 2016 follow-up data on 676 households from the two communities, Dubowitz et al. (2019) examine the effects of neighborhood investments on physical activity, psychological distress, and perceptions of the neighborhood. They hypothesize that these investments promoted positive lifestyle changes. If supported by the data, it may help rationalize the team's earlier results (Dubowitz et al. 2015) that the new supermarket is associated with better diet quality but the improvements are not associated with regular use of the new supermarket. Physical activity is objectively measured by an accelerometer worn on the nondominant wrist of the respondent over 7 days. The team also measures the psychological precursors to physical activity. These include social norms, intentions, and self-efficacy to engage in physical activity, as well as the barriers to and expected outcomes of physical activity. Using a DID approach, the authors do not find that the neighborhood changes (e.g., housing, landscaping, sidewalks, streetscapes) in the Hill District led to short-run improvements in physical activity or its psychological precursors, neighborhood perceptions or mental distress. The null results are robust to the alternative specification of classifying all respondents within a 0.1-mile radius of any neighborhood investment in the Hill District and Homewood communities as the treatment group and everyone else in the control group. These results and those of Dubowitz et al. (2015) leave the causes of the observed diet improvements in the Hill District after the opening of the supermarket unexplained.

Cantor et al. (2020) repeat the analyses in Dubowitz et al. (2015) using the subsample of SNAP participants in the PHRESH project. Their results largely confirm those of Dubowitz et al. that the new supermarket opening is associated with positive changes in diet outcomes. The authors add food security as an additional outcome and find that it improved after the opening. Like Dubowitz et al., Cantor et al. do not find that the regular use of the new supermarket explains the positive changes.

In 2015, the city of Minneapolis, Minnesota, implemented the nation's first Staple Foods Ordinance requiring food stores to stock minimum quantities and varieties of ten categories of healthy foods and beverages. Laska et al. (2019) evaluate the compliance rates at small food stores, including corner stores, convenience stores, small groceries, dollar stores, food-gas marts, and pharmacies; the nutritional quality of foods purchased from these stores; and the home food environment of their regular customers. Compared with the control city of nearby Saint Paul, the authors find that few changes in purchases and home food environments can be attributed to the ordinance two years after its implementation. In addition, full compliance was as low as 10% in 2017 at small food stores even though the city strived to make the standards reasonable and attainable by even the smallest food retailers.

CAVEATS AND AREAS FOR IMPROVEMENT

In the previous section, we synthesized some of the most important US studies that seek to infer the causal effects of food access on nutrition and health. As methodical and extensive as they have been, there are areas for (difficult) future research. Measurement is one such area. To paint a complete picture of the effects of food deserts on diet, data on intake or acquisition of foods for both at-home and away-from-home consumption are required. The 24-h dietary recall is the gold standard in nutritional epidemiology for tracking changes in dietary quality (Subar et al. 2003). However, multiple recalls from the same respondent are necessary to estimate usual intakes (Tooze et al. 2006), which are long-term average intakes. Budgetary considerations often limit the sample size in the number of repeated 24-h dietary recalls collected from the same respondent and the number of respondents, or they force the substitution of the more error-laden food frequency questionnaires (Subar et al. 2003) for 24-h dietary recalls. These factors make detecting statistical significance less likely when, based on available evidence, the effect of food deserts is expected to be small.

The use of the Nielsen and IRI household scanner data largely addresses the sample size concerns. However, the household scanner panel does not collect purchases of foods consumed away from home, nor does it collect quantity or detailed expenditure information on random-weight foods after 2006. If at-home and away-from-home calories are substitutes (Anderson & Matsa 2011), focusing on food at home may overestimate the positive effect of eliminating food deserts. To fill the data gap, it may be possible to use a two-sample instrumental variables estimator (Inoue & Solon 2010) to join scanner data with another data set that tracks away-from-home but not at-home food purchases. The missing random-weight foods are more concerning. Allcott et al. (2019) argue that focusing on packaged foods with a barcode should not significantly bias their results because the caloric share of packaged produce in total produce purchased is about 60% and does not vary statistically with income. Their caloric shares are based on a subset of the Homescan panel (known as the Fresh Foods panel) that reported detailed data on both packaged and random-weight food purchases until 2006. The 60%, however, is likely an overestimate of packaged produce share because random-weight foods are known to be severely underreported in household scanner data (Zhen et al. 2009). It is even possible that concerns about the low quality of random-weight food data contributed to their discontinuation in 2006. The 2012 FoodAPS collects household purchase and acquisition data on all foods over a 7-day period. The short data collection period alleviates concerns about reporting burden responsible for the underreporting of purchases in scanner data. In FoodAPS, about 21% (in expenditure share) of produce purchases came with a barcode. This is alarming, because if the primary effect of a supermarket entry to a food desert is to increase produce purchases, analyzing packaged food purchase patterns in household scanner data will lead to misleading conclusions. More research is needed to examine whether and the extent to which this bias exists.

Another measurement issue relates to the time horizon over which the full effect of a new supermarket takes place. Existing causal studies aim to identify the immediate to medium-run effects ranging from several months to a few years post-intervention. However, if healthy eating habits take much longer to develop, current estimates may underestimate the long-run effect of eliminating a food desert. Hut (2020) combines eleven years of Nielsen household and retail scanner data with a retrospective survey of Nielsen respondents' relocation history dating back several decades. The household and retail scanner data are used to measure the healthfulness of the home and retail food environments, respectively. Using an event study design with household fixed effects, he finds that at most 3% of the nutrition quality difference between the origin and destination food environments was closed within the first 24 months of the move. Hut assumes that the spatial difference in retail food environment did not change on average over time and provides evidence consistent with this assumption. This allows him to use contemporaneous food environment measures of a location to approximate the food environment of a person who moved out of this location decades ago. Based on this assumption, he provides suggestive evidence that 30 to 40 years after a move, a person closes about one-half of the retail healthfulness difference between the origin and destination. This indicates that an abrupt change in the retail environment does not induce a meaningful immediate change in purchase habits. The sizable long-run effect is possibly effectuated through peer effects, social interactions, or habit formation induced by the permanent change in retail food environment. Instead of waiting several decades for the full effect to present itself, it may be possible to use structural models of habit formation and peer effects to simulate the long-run effects.[5] The complication is that a structural model may not be able to econometrically distinguish a slowly evolving habit stock from the individual fixed effect. The consequence is an underestimation of the degree of habits and the long-run effects of the intervention.

CONCLUSION

The enormous burden of obesity and nutrition-related noncommunicable diseases and the pervasive SES-related health disparities continue to push researchers and policy makers to search for answers. In the last decade, perhaps no other healthy eating strategies have received more attention and support from the highest levels of the US government than the initiative to eradicate food deserts from inner cities and rural America. The efforts culminated in 2011 with the launch of the HFFI to provide financial and technical assistance to healthy food access projects. As a federal program, HFFI continues to receive government support, as evidenced by its reauthorization in the 2018 Farm Bill.

By 2011, there had been an ocean of correlational studies on associations of diet and health outcomes with the local food environment. Many, but not all, document statistical correlations between diet and health disparities and inequitable access to healthy foods. Interestingly, the food access literature that seeks to identify causality through credible identification strategies was nearly nonexistent before 2010. The heightened academic interest post-2010 is partly responding to policy makers' requests for causal evidence. Projects supported by the HFFI and other state and local initiatives also provide rare quasi-experiments to researchers for causal inferencing.

Overall, few studies reviewed here detect statistically significant effects of supermarkets on obesity or diet outcomes that can be plausibly interpreted as causal. Among those that do find statistical significance, the economic significance of the effect estimates is small (mostly <0.05 standard deviations of diet quality or obesity measures). This conclusion is hardly surprising, given that most residents of a food desert have access to a car and obtain groceries from large food stores outside of the food desert. Building a supermarket closer to a food desert will only reduce travel cost but not necessarily the relative price of healthy foods for those already shopping at other supermarkets. Available evidence points to a largely demand-driven thesis for poor diet quality in food deserts. For example, in addition to the aforementioned studies, Chen et al. (2020) find that the well-documented diet quality disparities by SNAP participation status (Zhang et al. 2018) are no longer significant after controlling for differences in nutritional attitude. Coincidentally, the supermarket in Pittsburgh that enabled the RAND Corporation's PHRESH project closed its doors in 2019 for reasons unknown to the public. It is not unreasonable, however, to suspect that a lack of demand is one driver behind the closure. A follow-up study is planned to evaluate the impact of the closure on the community (Cantor et al. 2020).

[5] See, for example, Zhen et al. (2011) for an application of a rational habit formation model with static expectations to predict the long-run effect of sugar-sweetened beverage taxes.

It is worth emphasizing that the lack of sufficient evidence of population-level impact does not mean that some individuals are not adversely affected by the inequitable physical access to healthy foods. It does suggest that other approaches to improving healthy food access should be explored and evaluated for the subgroup of consumers to whom physical distance poses a real barrier to healthy eating. For example, the SNAP Online Purchasing Pilot that expands SNAP benefits to online orders for pickup and delivery (USDA FNS 2019) may be a promising approach to healthy eating for the subgroup of less-mobile low-income households. Studies of shelf nutrition labels (Rahkovsky et al. 2013, Nikolova & Inman 2015, Melo et al. 2019, Zhen & Zheng 2020), including natural experiment-based studies (Li et al. 2021) in brick-and-mortar supermarkets have shown effectiveness in promoting healthy food purchasing. Online grocery shopping opens the possibility of personalized shelf nutrition labeling, which customizes the summary nutrition score of a food product to the preferences and health conditions of each shopper.

The studies reviewed in this article focus on the nutrition and health effects of improved access to healthy food. Aside from the health impacts, bringing full-service grocery stores to low-access communities may have social and economic benefits that elevate the SES of the residents. In addition, a small community-level improvement in diet quality could create a "keeping up with the Joneses" (showing that one is as good as their neighbors by buying and doing the same things) peer effect on household purchases that multiplies the initial direct effect of removing a food desert. These, in turn, could change consumer preferences through habit formation that ultimately lead to meaningful increases in diet quality in the long run ($\geq$10 years). Nevertheless, these long-run effects are merely theoretical possibilities that have not been tested empirically. The difficulty lies in the long time frame required to track behavioral changes and the credible separation of the long-run effect of improved access from the effects of other economic, social, cultural, and environmental determinants of health that also evolve over time.

To conclude, there is little evidence that food deserts significantly contribute to diet quality disparities at the population level. A full-service supermarket sells both healthy and unhealthy foods. Because most residents of a food desert already shop outside of the food desert, bringing a supermarket closer to the residents alone does not provide a significant enough incentive for consumers to increase the healthfulness of the grocery cart. A large body of evidence (not reviewed here) shows that information provision and price (dis)incentives can be effective in promoting healthy eating. Information disclosure, such as the nutrition facts labels and shelf nutrition labels, works by changing the underlying consumer preferences. Price (dis)incentives, such as sugar-sweetened beverage taxes and fruit and vegetable subsidies, significantly shift the economic constraints in favor of healthier foods. Supermarkets with these point-of-purchase interventions are more helpful for reducing diet disparities than those without.

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