The Impact of Dollar Stores on Food Access: A Machine Learning Approach to Predict Counterfactuals

Abstract

Since the early 2000s, dollar stores transitioned from predominantly regional discount stores to national chains with store locations in urban and rural markets and with an increasing share of sales in household goods and food products, including perishable food items. We evaluate the impacts of dollar store entry on food access in the United States from 2006 to 2020 using a machine-learning approach to estimate the impact of dollar store entry on food access status, measured by the presence of at least one grocery store within a given radius. We find minimal impact of dollar store entry on food access within a ten-mile drive of block-group population centers in rural areas. In contrast, we find small, statistically significant effects that grow with the time from treatment within a two-mile drive of urban-area populations. We also show that treatment effects in urban areas increase with rising neighborhood poverty rates, the share of vacant housing, and the share of the population that is Black and that the effects of additional dollar store entries over time are strongest in areas with only one or two supermarkets at the beginning of our study period. The null effect in rural areas supports the narrative that small-box dollar stores often locate in remote, rural communities with minimal retail activity, while in urban areas, the density of dollar stores potentially cause food retailers, particularly small, independent grocers to exit the market.

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1 Introduction

The proliferation of dollar stores across the United States has spurred considerable debate in the policy arena regarding the potential effects of dollar store growth on competition, food access, public health, and overall community economic development (Karpyn, Riser, Tracy, Wang, & Shen, 2019). Dollar stores may compete with local grocery stores or preempt the entry of new grocery stores in communities that already lack access to conventional food retailers (Aubrey, 2019; Hawks, 2022). The concern that dollar stores cause the exit or stymie the entry of local grocery stores has led policy advocacy groups and community policymakers to design and implement dollar store location ordinance regulations, and in many cases, outright bans (Smith, 2023b).

The rapid growth of dollar stores and their transition into food retail suggest that they may compete more directly with conventional food stores, and in turn, impact U.S. household food access. We study the impact of dollar store expansion on neighborhood food access at the census block-group level in the United States from 2006 to 2020, a period in which dollar stores grew exponentially and increased the number of household goods, and particularly, food products in their store inventories. To measure the effect of dollar store entry on block-group food access, we first use U.S. Census Bureau block-group population-weighted centroids to proxy household locations, and determine the number of grocery stores within a two- and ten-mile drive for urban- and rural-area block groups, respectively for each year of our study period.[1] We create low-access binary variables for population-weighted block-group centroids that indicate whether no grocery store is present within the two- and ten-mile driving distance thresholds for urban and rural areas, respectively. For each block group, we compute dollar store entries within the same driving distances for urban (two miles) and rural (ten miles) areas and over the same time period, allowing us to track the year of entry (i.e.,

[1] We use the U.S. Census definition of Urban Areas, grouping block groups in Urbanized and Urban-Cluster Areas in urban-area models. We include the remaining block groups in rural-area models (US Census Bureau, 2021).

the entry-event year) and relative time from treatment (i.e., dollar store entry) for treated block groups. As detailed in 3.1, dollar store and retail food store location information come from NielsenIQ's TDLinx. For grocery stores, we group retailers in the conventional supermarket, limited assortment supermarket, natural/gourmet grocery store, supercenter, and warehouse grocery store formats. These are the same store channels used in the USDA's food access measures (USDA, 2022). We combine the panel of low-access indicators and dollar store entry information with U.S. census demographic and socioeconomic predictors and geo-spatial features rooted in urban economics and economic geography to control for neighborhood conditions, such as spatial inequality, that are predictive of block-group low-access status.

We use a machine-learning method to estimate the effect of experiencing at least one dollar store entry on block-group level food access and investigate the heterogeneous effects of dollar store expansion on U.S. household food access over time and across block-group characteristics, including by race, income, and poverty status, for both urban and rural areas (Souza, 2019). The machine-learning approach avoids the biases associated with two-way fixed-effect (TWFE) regression models when treatment is staggered and the causal impacts vary over time and across treated units. If the method performs well, it should result in accurate out-of-sample predictions of food access status prior to the first dollar store entry. Thus, any differences between predicted and observed food access following the entry of a dollar store can be attributed to a treatment effect rather than estimation bias. In our empirical models, we find that the method yields highly accurate predictions using data prior to dollar store entry, suggesting that our identifying assumptions are valid.

For rural areas, we find no statistically significant effect of dollar store entry on block-group food access. The average treatment-effect-on-the treated (ATT), expressed as a percentage change in low-access relative to the counterfactual scenario in which dollar stores do not enter, is essentially zero ($\text{ATT}_{\text{rural}} = -0.97\%$; $\text{CI}^{\text{ATT}}_{\text{rural}} = [-4.21\%, 2.26\%]$).[2] Using

[2]As detailed in 4.2, statistical inference and confidence intervals are based on bootstrapped standard

an event-study design, we also observe no statistically significant effects since the time from treatment, and the point estimates are mostly centered around zero. In urban areas, we find evidence that dollar store entry has small, but statistically significant, positive effects on the share of low-access block groups. Moreover, the effects increase over time, beginning one year after the initial dollar store entry. Aggregating the treatment effects over all periods, our results imply that dollar store entry increased the share of low-access block groups by approximately 14% from 2006 to 2020 ($\text{CI}^{\text{ATT}}_{\text{urban}} = [9.88\%, 18.07\%]$).

In our analyses of treatment effect heterogeneity in urban areas, we find that the effects of dollar store entry on reducing food access are larger with increases in poverty rates, the share of vacant housing, and the share of the population that is Black. Treatment effects are smallest (i.e., negative) for rising population shares that are White and Hispanic, the share of the population with at least a bachelor's degree, and increases in income per capita. We find that treatment effects increase with additional dollar store entries over time for block groups with initially only one or two supermarkets, whereas we find near-zero effects when block groups begin the study period with three or more supermarkets. Finally, regional analyses indicate that treatment effects are largest for block groups in the Midwest, followed by the Northeast and South regions. Minimal effects are observed in the West. The Midwest and South are the regions in which the dollar store format originated (Joseph & Kuby, 2013).

We contribute to our understanding of the economic impacts of dollar store expansion. Our estimated treatment effects consider the competitive effects of dollar store expansion on both the exit of grocery stores and the deterrence of conventional grocery store entry. Dollar store entry may directly increase competition with incumbent food retailers and inhibit the opening of local grocery stores in otherwise viable market locations. Thus, while our research has implications for food access and U.S. food policy, we also inform how dollar store proliferation has affected supermarket and grocery store survival and viability.

errors.

Our results contribute to our understanding of the causal effects of dollar store entry on food access in the United States. Chenarides, Cho, Nayga, and Thomsen (2021) study the correlation between low-income and low-access (LILA) block groups and dollar store locations, finding that dollar stores tend to enter LILA block groups that remain LILA throughout their study period (2000 to 2017). However, this work is descriptive and does not estimate causal impacts of dollar store entry using an event-study framework. We build on the latter research by constructing counterfactual outcomes of block-group food access for the scenario in which dollar store entry had not occurred. This allows us to estimate the causal effects of entry by comparing actual food access with the counterfactual outcome.

We also contribute to the food access literature by studying changes in accessibility over time, as opposed to studies limited to cross-sectional data (Widener & Shannon, 2014). Because neighborhood low-access status is related to supply and demand factors (Bitler & Haider, 2011), our study analyzes the emergence of food deserts via dollar store entry, controlling for factors of demand, changes in economic development, and other neighborhood spatial inequalities.

We contribute to the emerging economic literature that finds mostly negative impacts from dollar store entry on competing grocery store viability (Caoui, Hollenbeck, & Osborne, 2022; Marchesi, Lopez, & Steinbach, 2023). Our finding that dollar store entry has little impact on food access in rural areas may be attributed to the dollar-store format's ability to locate in areas in which conventional supermarkets and grocery stores cannot viably operate. The effects of dollar store entry on food access in remote, low-population communities are minimal because grocery stores were not operating in these areas prior to or following dollar store entry. In rural towns that have large enough populations to support multiple grocery stores, the effect of dollar store entry on food access is not sufficiently strong to cause all grocery stores to exit the market.

In Section 2, we provide background information on the broad issues related to food access in the United States and the public debate regarding the extent to which dollar store proliferation since the early 2000s has impacted food access, and particularly, food deserts. We also provide an overview of the research specifically related to the effects of dollar store entry on competing food retailers and on market structure. We then discuss our data sources and provide details about the creation of block-group low-access indicators, dollar store entry information, and demographic, socioeconomic, and economic geography predictors used in empirical models. Next, we outline our machine-learning imputation method to estimate the causal effects of dollar store entry on food access. In Section 6 and Section 7, we summarize our findings and provide concluding remarks.

2 Background

Policymakers at the local, state and national levels are concerned that U.S. communities have adequate access to healthy food sources, particularly in under-served, low-income, and predominantly minority areas. The policy relevance of food access and food deserts was underscored by the U.S. Government's mandate in the Food, Conservation, and Energy Act of 2008 to study the extent of limited access in the United States, the health consequences of living in a low-access area, and the causes of food retail disparities over geographic space, particularly among income- and transportation-constrained populations (Ver Ploeg et al., 2009). The federal government has also increased funding of community initiatives to improve food access. For example, as part of the Affordable Care Act, the Community Transformation Grant Program allocated $103 million in support of healthy-living projects, which included bolstering food access in under-served communities (Centers for Disease Control and Prevention (CDC), 2017). The USDA Rural Development, in collaboration with the Reinvestment Fund, administer the Healthy Food Financing Initiative (HFFI), a program designed specifically to foster the development of grocery and other healthy food retail enterprises in low-income areas through financial support, training, and project planning, and in turn, boost local economic development (Reinvestment Fund, 2023). Individual states have also implemented programs to help finance research into food-access issues in local communities and provide funding opportunities and tax incentives to support local food retailers that sell healthy food products (Centers for Disease Control and Prevention (CDC), 2011; Kansas State University, 2022).

The primary impetus to public-health advocate demands for equitable food access is the observed link between access to healthy food stores, diet quality, and neighborhood or community socioeconomic status. Social science and public health research finds positive associations between the number of or proximity to full-service supermarkets and grocery stores, healthy eating habits and superior health outcomes (e.g., lower obesity rates) (Larson, Story, & Nelson, 2009). Perhaps the most acute form of inadequate food access is the food desert concept, which typically means that a geographic area (e.g., community, neighborhood, etc.) has no supermarket that sells affordable and healthy food within a specified distance threshold.[3]

The impact of food deserts on communities potentially has health and economic ramifications, as the lack of access to nearby healthy food sources increases food insecurity. Further, the reduced retail activity in the area has spillover effects on the local economy (e.g., employment, tax revenue, etc.) (Karpyn et al., 2019). However, with respect to diet quality, research has found minimal statistically significant causal effects of neighborhood low-access status on health outcomes, implying that consumer-demand factors may contribute most to differences in the consumption patterns of households across geographic

[3]There is considerable debate regarding how food deserts are defined (Ver Ploeg, Dutko, & Breneman, 2015; Wright, Donley, Gualtieri, & Strickhouser, 2016). Studies measuring community food access or food deserts often incorporate aspects of travel time or distance to the nearest food store (Allcott et al., 2019; Dai & Wang, 2011; Eckert & Shetty, 2011; Lytle & Sokol, 2017; McKinnon, Reedy, Morrissette, Lytle, & Yaroch, 2009; Pearce, Witten, & Bartie, 2006; Walker, Keane, & Burke, 2010). The USDA defines a low-access census tract if a threshold share or quantity of the population within the tract (i.e., one-third of the population or at least 500 people) resides further than a given distance from the nearest supermarket (i.e., one mile or ten miles in urban and rural areas) (Dutko, Ver Ploeg, & Farrigan, 2012; Rhone, Ver Ploeg, Dicken, Williams, & Breneman, 2017). The USDA identifies a "low-income and low-access" area when a census tract meets the one- or ten-mile low-access criterion and is low-income if its poverty rate is at least 20% or median family income is less than state or metropolitan area averages (USDA, 2022).

space (Zhen, 2021). In particular, (Allcott et al., 2019) show that the observed difference in healthy eating between low- and high-income households is primarily due to heterogeneous consumer preferences and education, as opposed to the availability of nearby supermarkets. In addition to the potential health consequences stemming from unequal access to healthy food stores, research underscores the spatial relationship between retail store locations and the demographic and socioeconomic characteristics of the neighborhood or community. Food deserts are correlated with racial segregation and income inequality (Ver Ploeg et al., 2009), increasing poverty rates and the share of minority population (Dutko et al., 2012). The latter study also finds some evidence that food deserts may be positively associated with areas experiencing population decline.

Other research investigating the spatial inequality of food access consider the relative presence of supermarkets and grocery stores across socioeconomic and demographic characteristics. These studies suggest that the empirical relationship between household access to retail food stores is nuanced, depending on whether food access is defined by proximity or the number and type of stores. In terms of proximity, Black, Hispanic, and economically-constrained populations (i.e., low-income, SNAP households, no own-vehicle access), tend to be closer to the nearest retail food store than White and middle- and high-income populations, a finding that could be attributed to higher population densities or the greater prevalence of small, independent food stores in low-income and ethnically diverse neighborhoods (Rhone, Ver Ploeg, Williams, & Breneman, 2019).

In a national study, Powell, Slater, Mirtcheva, Bao, and Chaloupka (2007) pair year 2000 U.S. Census zip-code level demographic and socioeconomic characteristics with store location information from Dun and Bradstreet (D&B) and find that, controlling for other factors, compared to middle-income and White zip codes, low-income, Black and Hispanic areas have fewer full-service, chain supermarkets but more independent, non-chain supermarkets and small grocery stores. Small-area studies, primarily in urban U.S. cities, similarly indicate that predominantly Black or ethnically diverse communities with high poverty rates, poor housing conditions, and transportation barriers are more (less) associated with small grocery stores (chain supermarkets) compared to majority White and higher-income neighborhoods (Moore & Diez Roux, 2006; Morland, Wing, Roux, & Poole, 2002; Powell et al., 2007; Walker et al., 2010; Zenk et al., 2005). In rural areas, the relationship between income, race, and healthy food store access may differ from urban contexts. For example, case studies in Texas suggest that more ethnically diverse and poorer populations have improved access to supermarkets, while the most economically-deprived communities have greater access to stores selling fruits and vegetables in terms of proximity and presence (Sharkey & Horel, 2008; Sharkey, Horel, & Dean, 2010). Nevertheless, because of sparser populations and heterogeneous geography, food access issues in rural communities may be more challenging compared to urban areas (Bitto, Morton, Oakland, & Sand, 2003). Stagnant population growth and dwindling economic activity may lead to a dearth of retail options in small, rural towns, implying that households incur higher transportation costs when shopping for household goods and food products. The increased travel distance may be particularly acute for low-income or elderly populations without private transportation (Bailey, 2010; Bitto et al., 2003; Lebel et al., 2016).

Food environment studies also tend to highlight that, relative to full-service grocery stores, non-traditional food stores, including convenience, drug, and dollar stores, are more prevalent in socioeconomically disadvantaged communities (Block & Kouba, 2006; Caspi et al., 2017; Larson et al., 2009; Walker et al., 2010). In particular, the expansion of dollar stores in low-income and under-served communities has garnered the attention of popular press, which reports that dollar stores may exacerbate health outcomes, as well as increase economic deprivation, inequality, and crime rates (MacGillis, 2020; Sainato, 2019; Siegel, 2019). Opponents of dollar store proliferation argue that their expansion across the United States negatively impacts local grocery store profitability, inducing their decline and exit from the market, and is a primary cause of food deserts (Donahue, 2018; Mitchell & Donahue, 2018; Mitchell, Smith, & Holmberg, 2023; Smith, 2023a; Thaxton, 2019). The major dollar store companies, such as Dollar General Corporation© and Dollar Tree, Inc.©, operate small-box business models but the size of their enterprises generates economies-of-scale, scope, and density, which affords them low per-unit operating costs.[4] The lack of scale economies in purchasing and distribution of smaller grocery stores suggests that their per-unit costs may be greater than those of chain retailers (Bailey, 2010; Vias, 2004). Differences in operating costs potentially provide a competitive advantage to dollar store chains over small, independent grocery stores.

The replacement of conventional grocery stores with dollar stores could adversely affect the local population's health because the variety and healthfulness of food products at dollar stores is purportedly inferior to the selection and quality of food sold at conventional supermarkets and grocery stores (Feng, Page, & Cash, 2023). In particular, public-health advocates and policymakers highlight the abundance of high-calorie and low-nutrient packaged food items sold at dollar stores contrasted with the minimal availability of fresh produce (Feng et al., 2023; Mitchell et al., 2023).

The negative perception surrounding dollar stores and their potential impact on community access to healthy food retailers, economic development, and social well-being motivated several city councils to restrict dollar store growth (e.g., Toledo, Oklahoma City, Tulsa, Fort Worth, Mesquite, Cleveland, Birmingham, and Dekalb County in Atlanta) (Birmingham City Council, 2019; Canfield, 2018; Capelouto, 2020; FOX 4 News Dallas-Fort Worth, 2018; Hart, 2019; Higgs, 2020; Howard & Fleming, 2019; Jimenez, 2019; Toledo City Plan Commisssion, 2020; Williams, 2021). McCarthy, Minovi, and Singleton (2022) qualitatively study twenty-five dollar store restriction policies of municipalities located in cities or counties across the United States. They find that policies designed to reduce the proliferation of dollar stores range from new-store moratoria, dollar-store density ordinances (e.g., no stores within 1 or 2 miles), and permanent dollar store bans. McCarthy et al. (2022) highlight

[4]Using store-characteristic information from TDLinx, the average store size of dollar stores is approximately 7,500 squared feet.

that over three-quarters of the twenty-five legal policies were implemented with the goal of improving community access to healthy food retailers and mitigating the competitive effects of dollar stores on local food retailers.

Research investigating the effect of dollar stores on food access has analyzed the correlation between the presence and entry of dollar stores and neighborhood food desert status and has compared the healthfulness of dollar store food products to alternative retailers. Chenarides et al. (2021) use store location information from NielsenIQ's TDLinx and U.S. Census demographic and socioeconomic data to study the association between dollar stores and food deserts from 2000 to 2017. They find that both dollar store presence and entry are negatively associated with block-group food desert status. Moreover, they indicate that dollar stores may have a positive impact on food access in some contexts by entering low-access areas and having lower exit rates in block groups that remained food deserts throughout their study period. While food environment studies reveal that dollar stores sell no fresh produce (e.g., fruits and vegetables), research shows that most dollar stores do sell canned or frozen fruits and vegetables, perishable foods, such as milk, and whole-grain products (Caspi, Pelletier, Harnack, Erickson, & Laska, 2016; Racine, Batada, Solomon, & Story, 2016; Sharkey et al., 2010).

The latter case studies, therefore, suggest that dollar stores potentially enhance food access in food deserts with no nearby retail food stores (Racine et al., 2016). Additionally, despite the absence of fresh produce, the healthfulness of food products offered at dollar stores are comparable or even superior to those offered at convenience, drug, and mass merchandiser stores (Caspi et al., 2016; Sharkey et al., 2010). While the modal dollar store does not sell fresh produce, specific chains (e.g., Dollar General©) are incorporating fresh produce items in their food inventory (Troy, 2021).

A handful of studies have assessed the impact of dollar store entry on the viability of independent grocery stores and on food retail market structure. Using USDA's SNAP Retailer Locator Data, demographic and socioeconomic controls from the U.S. Census Bureau, and market and year fixed effects, Caoui et al. (2022) find that dollar store entry negatively impacts the number of independent grocery stores within a two-mile radius of the census-tract centroid and that the effects grow with the number of entry events from one to three or more entries. In their model of dollar store entry in 846 isolated markets, they find that dollar stores negatively impact independent grocery and convenience store profits and that, absent of dollar store expansion since 2010, the number of independent grocery stores would be approximately 50% larger. Leveraging the National Establishment Time Series (NETS) database containing store location, employment, and sales information on retail food stores and dollar stores, Marchesi et al. (2023) find that dollar store entry increases the exit rate of independent grocery stores and decreases sales and employment. They indicate that the effects are strongest in rural-area census tracts. Focusing more broadly on dollar store impacts on market structure, Chenarides, Çakır, and Richards (2023) use NielsenIQ's TDLinx store location data and U.S. Census tract-level socioeconomic and demographic characteristics to estimate a spatial competition structural model of retail food store entry in the state of Texas from 2014 to 2019. In their counterfactual exercises, a 25% or 50% increase in Dollar General© store densities decreases store densities and profits of small-store formats (e.g., competing dollar and convenience stores and superettes), while the opposite effect is observed for big-box supercenters and supermarkets. In summary, research to date that studies the implications of dollar stores on food deserts has been descriptive and has not included causal analyses. Other work has estimated the impacts of dollar store entry on the number, sales, or employment of grocery stores and on retail market structure, but these analyses focus narrowly on independent grocers or were limited to a single state.

Economists, geographers, and sociologists have proposed multiple theories with respect to the causes of food deserts, and more generally, the variance in spatial access to supermarkets and grocery stores (Deener, 2017; Guy, Clarke, & Eyre, 2004; Vias, 2004). Bitler and Haider (2011) argue that the causes of food deserts involve factors of demand and supply. Household incomes, heterogeneity of preferences, prices, and education of healthy eating impact the level of demand, while variable and fixed costs effect the availability of supermarkets. To the extent that chain retailers benefit from economies-of-scale, scope and agglomeration, theory suggests that certain markets may be less profitable locations for conventional supermarkets (Bitler & Haider, 2011). Depressed macroeconomic conditions at the community level could also create a shortage of local grocery stores (Bitler & Haider, 2011). Another barrier to understanding the economic origins of food deserts is the dearth of research incorporating dynamic panel data sources to analyze changes over time in the food environment (Widener & Shannon, 2014).[5] In this respect, our analyses of the heterogeneous effects of dollar store entry on food access over time and across urban and rural areas contributes to our understanding of the causes of differential food access in the United States. While the economic causes of food deserts are undoubtedly multivariate and nuanced, our research examines the extent to which dollar stores contribute to the exit of conventional supermarkets and grocery stores.

3 Data

We outline our data sources and feature creation, including for the dependent and treatment variables and the exogenous covariates. We merge supermarket and grocery store location and dollar store entry information, with demographic, socioeconomic, and economic geography features to control for spatial inequalities of economic development, neighborhood amenities, and unobserved market-level conditions that are predictive of food deserts, based on the hypothesized causes of food deserts and the food access literature, described above.

[5] The food desert and food access literature mostly utilize cross-sectional data or case studies to assess correlations between an area's food desert status, the retail environment, and community or neighborhood socioeconomic and demographic characteristics. Using the USDA Economic Research Service's (USDA-ERS) Food Access Research Atlas (FARA) database, researchers have tracked changes in low-access, low-income areas (i.e., food deserts) over time, but these analyses describe temporal trends in food access and do not investigate the causal mechanisms of differential access (Alana, Williams, & Dicken, 2022; Rhone et al., 2017, 2019).

We apply these data to machine learning models to estimate the impacts of dollar store entry on food access.

3.1 Low-Access Status

Our information on retail store locations, including for supermarkets and dollar stores, comes from NielsenIQ's TDLinx database covering 2005 through 2020, one of the most comprehensive data sources of retail food store locations and characteristics in the United States (Cho, W McLaughlin, Zeballos, Kent, & Dicken, 2019; Levin et al., 2018). TDLinx provides detailed store-level information of each food retailer's opening date and location, including their geo-coded address, state, county, zip code, and street address. NielsenIQ categorizes stores in TDLinx using primary channel and sub-channel definitions employed in the food retailer industry or developed by NielsenIQ (Nielsen, 2022). We define supermarkets and full-service grocery stores as conventional supermarkets, limited assortment supermarkets, natural/gourmet grocery stores, supercenters, and warehouse grocery stores. This definition of supermarkets and grocery stores is applied by USDA-ERS researchers in defining adequate food access (USDA, 2022).

We define food access in terms of driving distance to the nearest supermarket or grocery store. Given that the dominant mode of transportation for U.S. households is using a private vehicle, a distance-based definition provides a realistic measure of accessibility, which serves as a proxy for the transportation costs of shopping. We use survey data, empirical facts on consumer shopping patterns, and dollar store chain annual reports to choose the threshold distance for defining low-access urban and rural areas, respectively. Appendix A.1 discusses our findings on average distances U.S. households travel when shopping for food and other everyday household items based on the 2017 National Household Travel Survey (NHTS). We also review other pertinent studies investigating how far consumers travel for groceries.

We define low-access urban areas as block-group populations without a supermarket within a two-mile drive of their population-weighted centroid, which spatially indicates the geographic population center for each block group (U.S. Census Bureau, 2021). We use the population-weighted block-group centroids to proxy household locations and demand (Ellickson, Grieco, & Khvastunov, 2020). Due to the heterogeneity of average drive times and longer overall drive times in rural areas, we use a conservative ten-mile driving distance threshold to define low-access rural communities. A ten-mile driving distance in rural areas is employed by the USDA-ERS in defining low-access rural census tracts and is also economically relevant because it nests the five-mile market area defined by Dollar General© and Family Dollar©, two of the largest dollar store chains with store presence in rural areas (Dollar General Corporation, 1999, 2015; Family Dollar Stores Inc., 2007, 2014). We use the 2010 U.S. Census definition and geographic delineation of urban areas to indicate whether block groups belong to urbanized, urban-cluster, or rural areas. In our analyses, we designate urbanized and urban-cluster areas as urban, while we call block groups located outside of urbanized and urban-cluster areas rural areas.[6]

After finding the number of supermarkets within a two- and ten-mile driving distance of urban and rural block-group population-weighted centroids, respectively, we create binary low-access outcomes at the block-group level from 2005 to 2020.[7] The low-access outcome measures whether block-group i in year t has zero stores within a given driving distance, $y_{it} = \mathbb{1}[s_{it} = 0]$, where s_{it} is the number of supermarkets and y_{it} indicates block-group low-access status. This measure allows for block groups to change their low-access status over time. In Appendix A.2, we provide tables that summarize our low-access measures in urban and rural areas and that compare our estimates of low access with the USDA's FARA low-access estimates at the national scale.

[6] See `https://www.census.gov/programs-surveys/geography/guidance/geo-areas/urban-rural/2010-urban-rural.html`

[7] We use the hereR package in R (Unterfinger, 2021) and Here Technologies' REST API to determine whether supermarkets are within a two- and ten-mile drive from each block-group population-weighted centroid.

3.2 Dollar Store Entry

Using TDLinx we create a panel of dollar store entries within a two-mile driving distance for urban areas and ten-mile distance for rural areas from 2006 to 2020 for each block group. Because many block groups already have one or more dollar store beginning in the year 2006, we use year 2005 dollar store counts within the two- and ten-mile driving distance bands as a predictor. This means that in our econometric models, we estimate the impact of dollar store entry beginning in the year 2006, controlling for each block-group's baseline count of dollar stores in the year 2005. Block groups are considered to be treated the year in which the first post-2005 dollar store entry occurs and in all subsequent years (i.e., 2006 to 2020), meaning that treatment does not switch on and off for cases in which dollar stores enter and exit block groups. We also create categorical variables indicating the number of and cumulative entries over time, which are used to assess the heterogeneous impacts of dollar store entry as dollar store densities rise over time.

In our panel of dollar store entries, block-groups are considered to be untreated or treated in a given year. There are two types of untreated block groups: yet-to-be treated units, which are untreated blocks groups that eventually receive at least one post-2005 dollar store entry, and never-treated units, which are block groups that never experience a post-2005 dollar store entry from 2006 to 2020. Yet-to-be treated units become treated units the year in which they experience their first post-2005 dollar store entry.

Our study period is appropriate for understanding the effects of dollar store entry on block-group low-access status. Dollar stores historically did not compete directly in the food retail space until the early 2000s. While the sales of consumables accounted for less than half of Dollar General's© sales in the year 2000, by the year 2005, the company accepted SNAP in all of its stores, signalling the rising importance of food and other household goods in the chain's business model (Dollar General Corporation, 2001, 2005). Following Dollar General©, Family Dollar© indicated in annual reports that in 2005, the company planned to increase cooler space for perishable foods and update its payment technologies to accept SNAP (Family Dollar Stores Inc., 2004). Although our store-level data do not indicate the extent to which dollar stores sell food or accept SNAP payments, both of which would increase competition with nearby food stores and impact food access, dollar stores opening after 2005 are likely to have modern food store formats, including a wide assortment of foods, cold storage for perishable food items, and provide customers with flexible SNAP payment.

Appendix A.4 contains summary tables of dollar store entry events. In urban (rural) areas, approximately 65% (80%) of block groups experienced at least one dollar store entry within a two-mile (ten-mile) drive of their population-weighted centroid.

3.3 Pre-Entry Retail Store Counts

Using the TDLinx store location database, we use as predictors in our causal machine-learning models baseline year 2005 store counts within two- and ten-mile driving distances of population-weighted block-group centroids for retail formats not included in the supermarket, grocery, or dollar store channels. Specifically, we compute year 2005 store counts for supermarket, convenience, drug, and wholesale club stores, respectively, as well as superettes (i.e., mom-and-pop grocery stores), and mass and general merchandisers.[8] Given that our outcome is a binary variable indicating the presence of a supermarket within a two- or ten-mile driving distance of block-group population-weighted centroids, we do not include year 2005 supermarket counts as a predictor since this would amount to controlling for the outcome. Because we model the impact of dollar store entry beginning in the year 2006, baseline store counts in 2005 are not outcomes of future dollar store entry.

[8] We merge the conventional drug store and Rx only and small independent drug store channels into a single drug store category.

3.4 Demographics and Socioeconomics

Given that socio-demographic characteristics, such as poverty, income, and race, are predictive of retail locations (Schuetz, Kolko, & Meltzer, 2012), we collect demographic and socioeconomic block-group level data from the year 2000 U.S. Decennial Census (DC) and Five-Year 2006-2010, 2011-2015, and 2016-2020 American Community Survey (ACS) period estimates from the U.S. Census Bureau. For consistent units over time, we use the IPUMS National Historical Geographic Information System (NHGIS) crosswalks to convert year 2000 and ACS 2016-2020 block-group data to year 2010 U.S. Census geographies (Manson, Schroeder, Van Riper, Kugler, & Ruggles, 2022). The demographic and socioeconomic variables, as well as the other variables presented in Section 3, are shown in Table 2. We pair block-group low-access and dollar store entry measures from 2005 to year 2000 DC demographic and socioeconomic data and low-access status and dollar store entry variables for the years ranging from 2006-2010, 2011-2015, and 2016-2020 to their corresponding five-year ACS period estimates.

3.5 Park Accessibility

Access to retail stores, such as supermarkets, may be a neighborhood amenity that is correlated with access to other neighborhood amenities, such as parks and green space. The latter are commonly hypothesized to impact neighborhood property values and the likelihood of renovation (Conway, Li, Wolch, Kahle, & Jerrett, 2010; Helms, 2003; Irwin, 2002). Further, research has investigated the relationship between park and green space access and neighborhood socioeconomic and demographic characteristics. For instance, in a national study, Wen, Zhang, Harris, Holt, and Croft (2013) found that, in urban-area census tracts, contrary to their hypothesis, the distance to parks was negatively associated with poverty and the shares of the population that were Black and Hispanic. However, controlling for other covariates, one-unit rises in the poverty rate, the population shares that were Black and Hispanic, respectively, were negatively associated with the percentage of green space area. In small towns and rural areas, the census-tract poverty rate was positively associated with both park distance and green space area, while the covariates associated with race were mostly negative. In a three-city study in New York, North Carolina, and Baltimore, Moore, Diez Roux, Evenson, McGinn, and Brines (2008) show that predominantly Black and Hispanic and low-income census tracts are less likely to have recreational facilities relative to White and wealthy neighborhoods. However, they find positive associations between these demographic and socioeconomic covariates and the presence of parks.

While nuanced, the literature suggests that in densely populated urban areas, a dearth of parks, recreation, or green spaces may be predictive of food access, to the extent that these open-space and natural-amenities are correlated with retail access and socioeconomic conditions. In sparsely populated rural communities, on the other hand, a community with increased proximity to parks or protected forests may be a less profitable retail location, and therefore, is more likely to lack access to a supermarket.

We create a park and green space accessibility measure that accounts for both the distance and the size of the most proximate parks or green spaces. Following Wen et al. (2013) and X. Zhang, Lu, and Holt (2011), we compute each block-group's park and green space access based on its seven nearest parks as $\sum_{j=1}^{7} P_{ij} * d_{ij}$, where $P_{ij} = \frac{S_j/d_{ij}}{\sum_{j=1}^{7} S_j/d_{ij}^2}$.[9] The S_j measures the size, in squared kilometers, of park or green space j and d_{ij} measures the distance between block-group i's population-weighted centroid and park or green space j's geographic centroid.[10] We leverage three national-scale park and green space databases. These are ESRI's USA Parks (ESRI, 2021), the Trust for Public Land's ParkServe (Trust for Public Land, 2022), and the National Conservation Easement Database (NCED). Combining the three databases yields 199,301 parks, recreation, and conservation areas in the

[9]We compute the park accessibility measure using each block-group's first, third, and fifth nearest parks. Their respective correlations with the park accessibility measure that uses the seven nearest parks are approximately 0.999.

[10]Parks and green spaces in this context refers to local (e.g., county or city), state, and federal parks, as well as recreation and conservation areas open to the public.

contiguous United States. Appendix A.3 provides further detail about each data source and how we merge them into a single park and recreation database.

3.6 Schools

Given that neighborhood amenities consist of a bundled set of goods (Bartik & Smith, 1987; Kane, Riegg, & Staiger, 2006), access to supermarkets and other retail activities may be correlated with the presence of schools in the area. For example, applying the food desert framework to schools, disparities in access to high-quality schools may be associated with the community's demographic and socioeconomic characteristics (Alexander & Massaro, 2020). Increased proximity to schools is generally preferred to more distant locations. School proximity reduces travel costs to school, increases neighborhood safety, and facilitates student and parent access to extracurricular activities, which in turn, is capitalized by increased house prices in the vicinity of schools (Chor & Wai, 2006; Owusu-Edusei, Espey, & Lin, 2007; Rosiers, Lagana, & Theriault, 2001; Sah, Conroy, & Narwold, 2016). Ceteris paribus, areas with a relatively larger number of public and private schools may be less likely to become a food desert, given the spatial bundling of neighborhood amenities.

We obtain censuses of public and private school locations from Homeland Infrastructure Foundation-Level Data (HIFLD). The databases are based on data collected by the US Department of Education, National Center for Education Statistics (NCES), Common Core of Data (CCD), and the Private School Universe Survey (PSS) (U.S. Department of Homeland Security, 2022). Both public and private school censuses contain school name, geographic information, including the geo-coded school location, school type (e.g., regular, vocational, special education, etc.), school status (e.g., open, closed, reopened, yet-to-open, etc.) and grade levels (e.g., prekindergarten, K-12, etc.).[11]. We include all open regular

[11] While we can determine whether schools are opened or closed, we cannot determine the exact date in which schools opened or closed. Therefore, our school presence variables are fixed over time, implying that they enter our models as fixed effects in predicting block-group low-access status.

elementary, middle/secondary, and high schools from the public schools databases and all elementary, secondary, and combined elementary and secondary schools using the private school database. To coincide with our spatial measures of food access and dollar store entry, we find the number of public and private schools within a two- and ten-mile driving distance from the census block-group population weighted centroids for urban and rural areas, respectively. We include these variables as predictors in our causal models.

3.7 Land Use and Development

The likelihood of a neighborhood becoming low access may be associated with the area's changing economic development over time (Bitler & Haider, 2011; Deener, 2017). For instance, Hamidi (2020) finds that urban sprawl increases the likelihood that census tracts are classified as food deserts, controlling for geographic factors and U.S. Census demographic and socioeconomic variables. Further, certain types of land use (e.g., vacant land) can be predictive of environmental inequalities (e.g., heat islands), which in turn, are associated with poverty, unemployment rates, and minority populations (see Pearsall (2017)). In turn, we hypothesize that spatial inequalities in economic activity are predictive of food deserts.

In our causal models, we proxy spatial economic inequality by computing land-use shares within a two- and ten-mile radius of each urban and rural block-group population-weighted centroid. Our land-use data come from the National Land Cover Database (NLCD) for the years 2004, 2006, 2009, 2011, 2013, 2016, and 2019 (Dewitz & Survey), 2021). The NLCD provides land-use characteristics at a 30-meter resolution for the extent of the United States. The NLCD uses eight general land-use categories (Water, Developed, Barren, Forest, Shrubland, Herbaceous, Planted/Cultivated, and Wetlands). Most of these land-use categories contain subcategories, allowing for the classification of land across sixteen land-use types. In our models, we include as predictors the share of land under each of the four developed-land subcategories (open space, low, medium, and high intensity) and the land-use shares for each of the other seven broad land-use categories.[12] To compute the shares for each land use type, we count the total number of 30x30 meter pixels within a two- and ten-mile radius of each urban and rural block-group population-weighted centroid, respectively. We then sum the number of pixels belonging to each land-use category or subcategory within the two-mile (ten-mile) radius of each urban (rural) block-group centroid and divide by the total pixel count to obtain land-use shares for the distinct land-use classes.

3.8 Block-Group Accessibility Measures

We create two accessibility variables that may be associated with the availability of supermarkets and grocery stores in a given market. We find the shortest travel distance, in miles, between each block-group population-weighted centroid and its nearest urban core, defined as the geographic centroid of urban areas and urban clusters. For rural block groups, we find the nearest urban- or urban-cluster area centroid, and then compute the driving distance between each block-group population and its nearest urban center. For urban-area block groups, we find the driving distance between the block-group population-weighted centroid and the urban or urban-cluster centroid to which the block group belongs. Increased distance between population centers and metropolitan areas may increase the local poverty rate, as distance may be correlated with factors of economic agglomeration, labor demand and supply (Partridge & Rickman, 2008). In turn, block groups further removed from urban centers may also be more likely to become low access due to their distance from the clustering of retail activity in urban areas.

In addition to distance and retail agglomeration, road accessibility may affect the profitability of retail locations over time (Kickert, vom Hofe, Haas, Zhang, & Mahato, 2020). For each block group, we compute the total length of road miles for multiple road types (U.S., state, and county highways, interstate, and all other roadways) using U.S. Census

[12] The Developed land-use category has four levels - open space, low, medium, and high intensity. Each Developed-land subcategory is defined by the percentage of impervious surface.

Bureau primary and secondary roads data. For urban areas, we compute the total amount of roadway, in miles, within a 2-mile drive of each population-weighted block group centroid, while for rural areas, we calculate the total roadway mileage for each road type within a ten-mile driving distance of the block-group centroid.

3.9 Feature-Engineered Fixed Effects

In conventional two-way fixed effects regression, unit, time, and unit-by-time fixed effects are used to control for unobserved unit- and time-specific heterogeneity. Failing to account for these unobserved factors will bias the estimation of causal parameters of interest if the variables are correlated. However, incorporating unit and time fixed effects as binary indicator variables in machine learning algorithms may not be optimal (Micci-Barreca, 2001), especially using tree-based ensemble methods, such as gradient boosting machines. When observations from a given group or time period are forced into the same leaf by splitting on a unit or time dummy variable, only within-group or- time variation can be explained, potentially restricting the predictive power of the algorithm. Also, the inclusion of a large vector of fixed effects (e.g., hundreds or thousands) in regression trees may be computationally infeasible (Jens, Page, & Reeder III, 2021). To efficiently incorporate fixed effects in our XGBoost algorithms, we feature engineer state-, year-, and state-by-year fixed effects by employing the method proposed by (Jens et al., 2021), which is particularly applicable in tree-based ensemble methods. Following their approach, rather than including binary state, year, and state-by-year indicators as predictor variables, we estimate these fixed effects using OLS regression and use the fixed effects estimates as features in our predictive models.

For each low-access indicator, we regress block-group low-access status on the demographic, socioeconomic, and market geography covariates described above, along with state, year, and state-by-year fixed effects. In these regressions, we use only the pre-treatment observations. We convert the state, year, and state-by-year fixed effects to three separate vectors, which we then use as predictors in our machine learning models. Using the estimated fixed effects as features in our models allows for greater flexibility in tree construction relative to trees built with binary indicators. The vectors of fixed effects provide a wide variety of potential splits for grouping similar observations across states or time, potentially increasing predictive accuracy, in addition to providing computational benefits (Jens et al., 2021).

Appendix A.5 contains summary statistics of the predictor variables in urban and rural block groups. We compute descriptive statistics for never-treated and treated units. The tables suggest that that many characteristics of the treated and never-treated block groups vary across socio-demographic, economic geography, and year 2005 retail predictors.

4 Methods

We use a machine-learning approach to examine the impact of dollar store entry on block-group low-access status and assess the heterogeneous effects of dollar store entry over time, market geography, and demographic and socioeconomic characteristics. Two-way fixed effects difference-in-differences (TWFE) in which the outcome is regressed on unit and time fixed effects, a treatment-by-time indicator, and a vector of controls is a common method to estimate the causal effect of an economic event. When treatment is staggered, i.e., occurring for units in different time periods, the TWFE model is frequently estimated using an event-study design by including lead and lag treatment indicators to analyze the degree to which treatment effects vary over time, rather than estimate a single average treatment-effect-on-the treated (ATT) parameter. However, the TWFE model may yield biased estimates of the ATT, particularly for settings in which treatment effects are heterogeneous over time or across groups (Borusyak, Jaravel, & Spiess, 2021; Callaway & Sant'Anna, 2021; De Chaisemartin & d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Imai & Kim, 2021; Sun & Abraham, 2021). Treatment effect heterogeneity can occur when treatment is staggered over time and the effects vary across treated cohorts or groups.

In our study, treatment effect heterogeneity may be present because, while dollar stores rapidly expanded during our period of analysis (2006 to 2020), the retail environment and economic conditions changed (e.g., the Great Recession from 2007-2009, growth of supercenters, etc.). Also, as noted above, in addition to opening new stores, dollar stores increased the variety of food items in their currently open stores, which can also impact the effect of treatment over time and across block groups. When the covariates across treated groups vary, each cohort may respond to treatment differently (Sun & Abraham, 2021). In turn, we might expect communities that experience a dollar store entry in different years to have heterogeneous outcomes in terms of their low-access status.

As a result of the problems associated with TWFE models for settings with staggered treatment and heterogeneous treatment effects over time or groups, numerous alternatives have been proposed.[13] Given our temporally and spatially dynamic database containing block-group level data on demographics, socioeconomics, land use, neighborhood amenities, the pre-entry retail environment, and block-group accessibility variables, we implement a machine learning imputation method to estimate the effects of dollar store entry proposed by (Souza, 2019). The ML-imputation approach allows for efficient, unbiased estimation of heterogeneous treatment effects while leveraging the flexibility of ML algorithms, which are designed to maximize prediction accuracy. Whereas the linear regression fixed effects imputation method involves the estimation of unit fixed effects and time trends using data from the never-treated and yet-to-be treated observations (Borusyak et al., 2021; Gardner, 2022), the ML-imputation approach does not require the estimation of fixed effects using the pre-treatment data, while allowing for the inclusion of a rich set of exogenous covariates to maximize the predictive accuracy of potential outcome counterfactuals (Souza, 2019).

[13]See, for example, Athey, Bayati, Doudchenko, Imbens, and Khosravi (2021); Borusyak et al. (2021); Callaway and Sant'Anna (2021); De Chaisemartin and d'Haultfoeuille (2020); Gardner (2022); Liu, Wang, and Xu (2022); Roth and Sant'Anna (2021); Souza (2019); Sun and Abraham (2021); Wooldridge (2021).

There are several advantages of machine learning imputation of counterfactual low-access outcomes relative to alternative imputation approaches based on fixed effects. Tree-based machine learning algorithms, such as XGBoost, excel in capturing nuanced, non-linear relationships between predictors and outcomes, allowing for more precise estimates of counterfactual outcomes Goulet Coulombe, Leroux, Stevanovic, and Surprenant (2022). As described below, model building and selection is conducted using out-of-sample cross-validation prediction errors, which helps ensure that predicting unobserved potential outcomes is more accurate. Finally, machine learning algorithms are data-driven, meaning that they perform automatic variable selection and can find complex interactions between covariates without the researcher needing to specify the exact functional form. Examples of applications of this method include studies that estimate the efficacy of energy efficiency programs in the United States (Christensen, Francisco, Myers, & Souza, 2021) and the impact of the COVID-19 pandemic on energy demand in Spain (Fabra, Lacuesta, & Souza, 2022). In general, when pre- and post-treatment data are available in panel data settings, machine learning techniques can be used with difference-in-difference designs to predict out-of-sample, counterfactual outcomes using pre-treatment observations as controls for the prediction of post-treatment potential outcomes (Varian, 2016).

Estimation of causal effects involves three steps. Using our flexible machine learning algorithm (i.e., XGBoost), the first step is to build predictive models of block-group level low-access status using pre-treatment data only, which include never-treated and yet-to-be treated observations. Our model takes the form:

$$\underbrace{y_{it}}_{\text{Low-access status}} = \underbrace{f\left(S_{it}, D_{it}, G_{it}, R_{it}, \hat{\alpha}_s, \hat{\lambda}_{st}\right)}_{\substack{\text{Socio-Demographic}\\ \text{Geographic}\\ \text{Retail}\\ \text{Est. Fixed Effects}}} + \underbrace{\varepsilon_{it}}_{\text{Error}}, \text{ for all } \underbrace{t < e_i}_{\text{Pre-treatment}} \tag{1}$$

In Equation 1, we model low-access status of block-group i at time t as a function $f(\cdot)$ containing socio-demographic variables, controls for economic-development and urban amenities, estimated fixed effects, and pre-entry retail-environment features described in Section 3. The ε_{it} is the idiosyncratic error unexplained by the predictors. The notation $t < e_i$ indicates that we subset the data by using untreated and yet-to-be-treated observations from the pre-treatment sample, where e_i is the period in which dollar store entry occurs for block-group i. We use a general model specification in Equation 1 because we leverage flexible machine-learning algorithms that can find complex, interactive relationships between predictors and outcomes, which are generally unknown to researchers.

Model training and selection involves fitting Equation 1 to a portion of the pre-treatment data and testing the fitted model's accuracy on the remaining held-out pre-treatment observations. We use k-fold cross-validation, described below, to choose the model with highest predictive accuracy in out-of-sample, cross-validated predictions. In addition, we utilize the cross-validated errors to confirm our model identification assumptions.

In the second step, we apply the optimal machine learning model, estimated in step one, to the post-treatment data to impute counterfactual low-access outcomes.

$$\underbrace{\hat{y}_{it}(0)}_{\text{Counterfactual}} = \underbrace{\hat{f}\left(S_{it}, D_{it}, G_{it}, R_{it}, \hat{\alpha}_s, \hat{\lambda}_{st}\right)}_{\text{ML Model}}, \text{ for all } \underbrace{t \geq e_i}_{\text{Post-treatment}} \tag{2}$$

In Equation 2, $\hat{y}_{it}$ represents the counterfactual potential outcome of block-group i at time t, which represents its predicted low-access status had dollar stores not entered the block group within a two- and ten-mile drive for urban and rural block groups, respectively. The $t \geq e_i$ indicates that we apply the predictive model estimated in step one to the treated block groups in the post-entry periods.

In step three, we estimate treatment effects for each treated observation and time period by subtracting each block-group's counterfactual low-access outcome from its actual low-access status:

$$\underbrace{\hat{\tau}_{it}}_{\text{Treatment Effect}} = y_{it} - \hat{y}_{it}(0) \ , \text{ for all } t \geq e_i \tag{3}$$

In Equation 3, $\hat{\tau}_{it}$ is the estimated treatment effect of dollar store entry in block-group i at time t.[14] Exploiting the granularity of the estimated causal effects, we summarise $\hat{\tau}_{it}$ by examining how treatment effects vary with the time from treatment, using an event-study framework. We also assess heterogeneous treatment effects across market geography and demographic and socioeconomic covariates of interest, such as the area poverty rate and vacant housing.

Following (Souza, 2019), we describe the three primary identification assumptions.[15] We assume no anticipatory effects on yet-to-be treated block-groups, $y_{it} = y_{it}(0), \text{ for all } t < e_i$, where y_{it} is block-group i's low-access status at time t, $y_{it}(0)$ represents the potential outcome (i.e., low-access status) in the block-group's untreated state, and e_i is the the dollar store entry event date for block-group i. This assumption implies that supermarkets do not exit markets in anticipation of dollar store entry and that the observed outcomes of yet-to-be treated units are not affected by future, anticipated treatment effects. Alternatively, from a demand perspective, we assume that residents of the block-group do not change their shopping behaviour shortly before dollar store entry, causing supermarkets to exit.

The second assumption states that our covariates related to demographics, socioeconomics, and market geography are exogenous and are not outcomes of dollar-store entry, $\mathbf{X}_{it} = \mathbf{X}_{it}(0) = \mathbf{X}_{it}(1)$, for all t. The small-box format of dollar stores suggests that their entry or anticipated entry is unlikely to systematically impact income, unemployment, housing conditions, land use, or neighborhood amenities in either pre- or post-treatment periods. It is also implausible that dollar stores effect population levels or demographics over time.

[14] Because our outcome is a binary variable (1/0), indicating whether the block group is low access, if dollar store entry decreases food access, $\hat{\tau}_{it} = 1$, whereas if dollar store entry has no effect $\hat{\tau}_{it} = 0$. Were dollar stores to have a positive impact on food access, $\hat{\tau}_{it} = -1$.

[15] Appendix B contains a formal exposition of the assumptions needed for identifying the treatment effects.

The third assumption maintains that, conditional on exogenous covariates, the function describing low-access status in the pre-treatment period is equal to the function that models counterfactual potential outcomes in post-treatment periods (i.e., low-access status had dollar store entry not occured).[16] The assumption implies that the machine learning models estimated in step one using pre-treatment data can be used to accurately predict counterfactual low-access outcomes for the treated block groups in post-treatment periods. To assess heterogeneous treatment effects across covariates, an additional assumption is that, conditional on specific values of covariates, the functional form in the pre-treatment period that relates covariates to outcomes is the same in the post-treatment period in the absence of treatment. When the assumptions described above hold, the ATT and conditional ATT are identified (Souza, 2019).

4.1 Cross Validation

In machine learning, building and selecting an optimal model is determined by cross-validation. The data are split into independent training and test subsets, predictive models are fit to the training data, and the model's average predictive accuracy is estimated by applying the trained model to the test data (Bates, Hastie, & Tibshirani, 2021; Hastie, Tibshirani, & Friedman, 2009). In k-fold cross validation, the data are split randomly into k subsets (i.e., folds). The predictive model is fit to k-1 folds and out-of-sample predictions are computed for observations in the holdout fold using the trained model. The process is repeated for each fold. Using the out-of-sample predictions, the expected test error is estimated by averaging.

Conventional k-fold cross-validation assumes that the data are independent and identically distributed (i.i.d.) (Arlot & Celisse, 2010). If the data are not i.i.d., due to spatial or temporal correlation, for example, the estimated cross-validation errors may be biased. Ran-

[16] As stated by (Souza, 2019), this assumption is similar to conditional parallel trends, which is a standard assumption in TWFE models, including in imputation methods such as (Borusyak et al., 2021) or aggregation schemes such as (Callaway & Sant'Anna, 2021) that are robust to treatment-effect heterogeneity.

domly partitioning temporally auto-correlated data into training and test folds may result in the underestimation of model errors, as the data used in model training may be temporally dependent with the test data (Roberts et al., 2017). Given the panel structure of our data, block-group low-access status may be correlated over time, implying that observations are not i.i.d. To obtain biased-reduced estimates of the model errors, we must modify our cross-validation approach to account for the temporal auto-correlation.

For panel data settings in which individual units are observed for consecutive years, stratified random sampling at the unit level (i.e., vertical cross-validation) or splitting observations by time (i.e., horizontal cross-validation) are common approaches to obtain unbiased out-of-sample cross-validated predictions (Athey, Bayati, Imbens, & Qu, 2019; Roberts et al., 2017; Souza, 2019). Because our goal is to accurately estimate counterfactual outcomes for treated units using the never-treated and yet-to-be treated observations, we perform cross-validation splitting by time, which provides the most realistic predictive environment for post-treatment counterfactual predictions of treated block-group low-access status in later time periods.

Specifically, we implement 7-fold blocked cross-validation (CV), whereby observations are placed into separate folds using blocks of time as data splits. When data are dependent, blocked cross-validation is shown to be a preferred alternative to standard cross-validation in time series and machine learning applications (Bergmeir & Benítez, 2012; Bergmeir, Costantini, & Benítez, 2014; Schnaubelt, 2019). While alternative cross-validation schemes for time series and panel data exist (e.g., rolling origin, rolling window, etc.) (see Tashman (2000) and Schnaubelt (2019)), blocked cross-validation is particularly suitable because we are able to use all pre-treatment data in the training and validation folds, thus improving predictive performance (Bergmeir & Benítez, 2012; Bergmeir et al., 2014). In our application, an additional advantage of blocked CV is that we obtain out-of-sample predictions and prediction errors for each pre-treatment observation, critical for verifying our model assumptions. If our empirical method is valid, out-of-sample predictions for untreated outcomes should closely match actual untreated outcomes. This would give us confidence that any difference between post-treatment outcomes and predictions obtained from our models reflect the impact of dollar store entry rather than bias. In addition, we can assess whether out-of-sample model prediction errors for untreated outcomes are uncorrelated with predictor variables, i.e., whether we tend to under or over predict low access status for particular values of the covariates. If we find no such correlations, correlations between covariates and estimated treatment effects should reflect impact heterogeneity rather than model bias.

To create the seven folds, we split the pre-treatment data into two-year blocks from 2007 to 2020 using the fold designations in Table 1 and Figure 1. We include pre-treatment observations from the years 2005 and 2006 in each trained cross-validation model. We include year 2005 observations in each training model because we estimate the effect of dollar store entry conditioned on year 2005 counts for dollar, convenience, drug, general and mass merchandiser, superette, and wholesale club stores and pre-treatment observations from 2006 to balance the holdout folds with respect to the number of years in each fold.[17]

We observe individual block-group low-access status for varying lengths of time, depending on each block-group's treatment timing. We have information on never-treated units' low-access status from 2005 to 2020 and track yet-to-be treated outcomes until their time of treatment, $r = 0$, where r indicates the block-group's relative-time-to-treatment. For block groups that experienced a dollar store entry in 2010, we have data on pre-treatment low-access status from 2005 to 2009. On the other hand, for block groups treated in 2020, we observe pre-treatment low-access outcomes for 15 consecutive years (2005 to 2019). As shown in Table 1, the number of observations in each validation fold decreases over time as the number of yet-to-be treated block groups declines in the pre-treatment data. The total number of observations used in each cross-validation model is obtained by summing the observations across the k-1 columns.

[17]The fold structure in our blocked-CV implementation implies that we do not make out-of-sample, cross-validated predictions for block groups treated in 2006 or 2007 during their pre-treatment years (i.e., 2005 and 2006), though we obtain counterfactual predictions for block groups treated in these years.

In Appendix C we provide details about our preferred machine-learning algorithm, XGBoost, to construct counterfactual predictions of block-group low-access status. We also discuss model tuning and selection in Appendix C. The XGBoost algorithm contains various hyperparameters which allow for a high level of customization and should be adjusted to improve the model's predictive accuracy. The optimal hyperparameter configuration depends on the characteristics of the data. We train the algorithm using the pre-treatment data and the blocked-CV design across different hyperparameter settings. We base our selection of the optimal XGBoost model and its associated hyperparameters on the cross-validation error estimated using the pre-treamtent data.

4.2 Inference

For statistical inference and constructing confidence intervals, we create bootstrapped standard errors by iteratively performing the three-step estimation procedure described in Section 4 using bootstrap samples. We sample block groups at the census-tract level with replacement, to account for spatially correlated errors. For a given randomly selected census tract, all block groups contained in that census tract are included in the bootstrap sample. Each bootstrap sample contains approximately the same number of pre- and post-treatment observations. For each bootstrap sample, we conduct all analyses, including the training of the machine-learning algorithm using the pre-treatment data, running regressions and assessing model diagnostics using the pre-treatment cross-validated predictions and errors, predicting counterfactual potential outcomes of low-access status with the post-treatment data, and performing analyses for treatment effect heterogeneity.[18] We conduct 499 bootstrap iterations of all analyses, which are combined to compute bootstrapped standard errors and confidence

[18] For each bootstrap sample, we use the optimal set of hyperparameters found during initial model training using the actual pre-treatment data, with the exception of the hyperparameter, *scale_pos_weight*, and the number of boosting iterations. The value of *scale_pos_weight* is dependent on the bootstrap sample and is computed as $\frac{\sum_i^N \mathbb{1}[y_{it}=0]}{\sum_i^N \mathbb{1}[y_{it}=1]}$. The number of optimal boosting iterations is determined by cross-validation. Appendix C contains greater detail regarding the hyperparameter settings used for analyses.

intervals. Throughout the paper, we report 99% confidence intervals, as they provide the most conservative measure of statistical significance for empirical point estimates.[19]

5 Results

We present our model findings. In Sections 5.1 and 5.2, we construct event-study plots to confirm or reject the model identifying assumptions and assess predictive accuracy. In Section 5.3, we analyze heterogeneous treatment effects over time, by the number of dollar store entries, and across block-group socio-demographic characteristics.

5.1 Actual and Counterfactual Low-Access Status

Figures 2 and 3 plot the actual and predicted shares of low-access block groups against relative time from dollar store entry in urban areas and rural areas.[20] Recall that urban-area (rural-area) block groups are designated as low-access if they have no supermarket within a two-mile (ten-mile) drive of their population-weighted block-group centroids. The vertical dashed line at $r = 0$ on the horizontal axis divides relative time from treatment into pre- and post-entry periods.

For the pre-treatment, relative time from treatment periods, $r = [-13, -1]$, the predicted shares of low-access block groups are based on out-of-sample, cross-validation predictions estimated by the optimal ML model. Actual and predicted low-access shares in the relative time from treatment period for $r = -13$ corresponds to block groups in the year 2006 that

[19]For a given parameter estimate or summary statistic, the 99% bootstrapped confidence intervals are computed as $CI_{0.99}^{\hat{\beta}} = \left[\hat{\beta} - 2.576 \times \hat{\sigma}, \hat{\beta} + 2.576 \times \hat{\sigma}\right]$ where $\hat{\sigma}$ is the bootstrap standard error. The confidence intervals imply that with repeated sampling, there is 99% probability that the interval contains the true, population value.

[20]The predicted out-of-sample, cross-validation (counterfactual) low-access shares are estimated by the following regression: $\hat{y}_{it}^{cv(cf)} = \sum_r \beta_r \mathbb{1}\left[r = t - e_i\right] + u_{i,t}$. The same specification is used for computing the actual share of low-access block-groups by replacing predicted low-access with actual low access, y_{it}^{actual}. The standard errors and confidence intervals are estimated by the bootstrap procedure outlined in Section 4.2.

receive their first post-2005 dollar store entry in 2020, while $r = -1$ combines low-access outcomes of all treated block groups one year prior to their first post-2005 dollar store entry. In the post-treatment relative time from treatment periods, $r = [0, 14]$, the predicted low-access shares are counterfactual, potential outcomes imputed by the optimal ML model. The actual and counterfactual low-access shares at the relative time from treatment period for $r = 14$ corresponds to outcomes from the year 2020 for block-groups that experienced their first post-2005 dollar store entry in 2006. The average actual and counterfactual low-access status at $r = 0$ aggregates treated block-group outcomes for the year in which they experience their first post-2005 dollar store entry. Figures 2 and 3 use observations that correspond to actual and predicted low-access shares at the block-group level from 2006 to 2020.

Figures 2 and 3 provide insights into the predictive accuracy of the optimal ML model and the degree to which the model identifying assumptions are satisfied. In the pre-treatment periods, the optimal ML models accurately predict the actual shares of low-access block groups in both urban and rural areas. The actual and out-of-sample predicted shares from cross-validation almost perfectly align in the pre-treatment period. The downward trend in low-access among the yet-to-be treated and treated block groups aligns with the decrease in low-access among all block groups.

Figures 2 and 3 suggest that there is no systematic anticipation of dollar store entry by yet-to-be treated block groups. This could arise if either households or grocery stores change behavior in anticipation of dollar store entry. We also observe that, conditional on model covariates, there is no gradual divergence between actual and predicted low-access shares in the pre-treatment period. Therefore, we have evidence that our identifying assumptions hold, and that there is no systematic bias in the estimated treatment effects in the post-treatment period. If block-group households or nearby grocery stores anticipate dollar store entry, actual and predicted low-access shares would diverge in time periods just prior to dollar store entry. If there were unobserved factors not accounted for in the models that would bias counterfactual predictions in the post-treatment periods, the difference between the actual and predicted share of low-access block groups in the pre-treatment period would increase or decrease as block groups approach treatment. However, the close fit between actual and predicted low-access shares from cross-validation suggests that, conditional on model predictors, the optimal model estimated in the pre-treatment period can be used to predict post-treatment potential outcomes.

Another concern may be that the timing of dollar store entry is correlated with block-group low-access status because, for example, dollar stores may systematically anticipate when supermarket entry or exit occurs, biasing the ML model predictions. However, in both urban and rural areas, the counterfactual share of low-access block groups at $r = 0$, imputed by the optimal ML model using the post-treatment data, is nearly identical to the actual low-access share, indicating that at the time of treatment, yet-to-be treated and treated block groups are on parallel trends. Overall, Figures 2 and 3 indicate that the yet-to-be treated observations can be used to predict counterfactual low-access status of treated block groups.

Figures 2 and 3 reveal heterogeneous treatment effects across urban and rural market geographies. The top panel indicates that, within a two-mile drive of the population-weighted block-group centroids, the impact of dollar store entry on treated block groups in urban areas grows over time following the initial treatment period. The difference between the actual and counterfactual, predicted low-access shares in urban areas represent the treatment effects on the treated block groups, which we will assess in Section 5.3. On the other hand, dollar store entry has little effect on block-group low-access status in rural areas within a ten-mile drive of the block group population-weighted centroids. The predicted, counterfactual and the actual share of low-access block groups overlap in the rural-area model, implying that dollar store entry has minimal effect within a ten-mile drive of population-weighted block-group centroids in rural areas.

5.2 Cross-Validated Prediction Errors

We evaluate the patterns of the out-of-sample, cross-validation prediction errors (i.e., the ε_{it} in Equation 1) with respect to the ML-model covariates.[21] If the cross-validation errors (CV errors) are strongly correlated with the predictive model's covariates, such as the socio-demographic predictors, then variation in treatment effects across demographic and socioeconomic characteristics may be caused by unobserved factors that are correlated with those predictors but not controlled for in the predictive model. In turn, we would erroneously attribute heterogeneous treatment effects to variation in effects over covariate attributes.

We plot the average CV errors from the urban- and rural-area models with respect to binned demographic and socioeconomic model covariates. The figures are shown in Appendix E. To construct each figure, we regress the out-of-sample, CV prediction errors on binned model covariates, using the following specification.

$$\hat{\varepsilon}_{it}^{cv} = \sum_{b} \beta_b \mathbb{1}\left[X_{it} \in b\right] + v_{it}, \text{ for all } t < e_i \tag{4}$$

In Equation 4, the index b indicates to which binned numerical category the value X_{it} belongs, where v_{it} is the idiosyncratic error. We create binned variables of each covariate using deciles as cut points. Note that the distributions of some covariates are skewed right, whereby many observations are assigned a zero value, which results in fewer unique bins.[22] In addition, for predictors with discrete values, such as the year 2005 store counts and urban-area dummy variables, we create categorical variables for each unique value and regress the

[21] In Appendix E, we plot the average CV errors by the relative time to treatment, which can also be used to visually inspect the model identifying assumptions discussed in Section 5.1. Cross-validation errors that are consistently different than zero and that have a clear positive or negative trend as the treatment date approaches may indicate violations of the no anticipatory effects and invalidate our use of the models estimated with the pre-treatment data to predict post-entry potential outcomes. We show that the average CV errors are mostly statistically insignificant at each pre-treatment time period and do not have a strong positive or negative pattern prior to dollar store entry.

[22] For example, because the dominant mode of transportation is private vehicle in the United States, the share of the population using public transportation is mostly zero in rural areas, resulting in two discrete numerical bins. There are six discrete numerical bins in urban areas.

CV errors on the categorical indicators.

The average CV errors for each binned socio-demographic covariate are small, concentrated around zero, and except for a few bins at the extreme portions of a covariate's distribution, are statistically insignificant. Given that we find growing treatment effects over time in urban areas but not in rural areas, it is reassuring that the patterns observed for the CV errors with respect to each binned covariate are similar across market geographies, suggesting that the impacts revealed in the urban ML model are not primarily due to prediction error.

5.3 Average Treatment Effects on the Treated

Table 4 gives the ATT and percentage change in the treatment effect (Δ ATT (%)) by market geography. Consistent with Figures 2 and 3, the ATT in rural areas is not statistically significant, as the 99% confidence intervals contain zero. On the other hand, in urban areas, the ATT is positive, and statistically significant ($\text{ATT}_{\text{urban}} = 0.0069$). Though the ATT is numerically small, we find that, absent of dollar store entry within a two-mile drive, food access among treated block groups would be approximately 14% higher, measured by having at least one supermarket or grocery store operating within the same distance.

Figures 4 and 5 show how treatment effects vary beginning in the initial dollar store entry period at $r = 0$. For each relative time period, we compute the ATT as a percentage change in the share of low-access block groups. Figures 4 and 5 illustrate that, while treatment effects are essentially zero throughout the post-treatment period in rural areas, treatment effects increase over time in treated urban-area block groups (i.e., block groups that experienced at least one dollar store entry). Following the initial dollar store entry event at $r = 0$, the share of low-access block groups increased by approximately 5.5% one year after treatment and rose to over 30% approximately ten years after treatment.

The gradual increase in treatment effects in urban areas indicates that the impact of dollar store entry on food access is not immediate. In urban areas, block-group households are low access if they have no supermarket or grocery store within a two-mile driving distance of the block-group population-weighted centroids. Therefore, dollar store entry may initially affect competing grocery store revenue, and it may take several years for dollar stores to cause supermarket exit. Further, while we condition treatment status on at least one dollar store entry, treated block groups often experience multiple dollar store entries within the two- or ten-mile driving distance thresholds over time. In Section 5.4, we assess the impact that multiple dollar store entries have on block-group food access and in Section 5.5 we analyze treatment effect heterogeneity across socioeconomic and demographic characteristics. Given the null effects in rural areas, we focus our analyses of treatment effect heterogeneity on the urban-area ML model results. We provide the rural model figures in Appendix D.3.

We also summarize the treatment effects at the regional level to study the variation in the impacts of dollar store entry on food access across regions for both urban and rural areas. Figures 2.D and 3.D in Appendix D.1 display the ATTs for urban (top panel) and rural areas (bottom panel) for each of the four U.S. geographic regions (East, West, Midwest, South). In urban areas, the ATTs are statistically significantly different than zero in three of the four regions. The ATT is largest in the Midwest. The dollar store format originated in the South and Midwest (Joseph & Kuby, 2013). In rural areas, the confidence intervals in the Midwest and West contain zero, while the ATTs in the South are small and negative. In the Northeast, there is a small, statistically significant negative impact of dollar store entry on block-group food access. The ATT indicates that, absent of dollar store expansion in the Northeast region, the share of low-access block groups in areas that received at least one post-2005 dollar store would be lower. Despite the statistical significance, the ATT point estimate is small and the lower bound of the confidence interval is approximately zero (i.e., 0.004).

5.4 Impact of Multiple Entries over Time

To assess the relationship between treatment effects and the cumulative number of dollar store entry events, we leverage our information on year 2005 block-group supermarket and grocery store availability.[23] Conditional on a given number of dollar store entries, block groups with fewer nearby supermarkets in the year 2005 are more likely to become low access than block groups with numerous supermarkets. If changes in food access are partly a function of retail competition, we expect for the competitive shocks of dollar store entry to be lower in markets with multiple grocery stores because the share of the retail food market is not concentrated in one or two grocery stores. The impact of dollar store entry will be distributed across each food retailer in the market. Even with large dollar store densities, supermarket exit rates may decline as the number of supermarkets in each area increases. Therefore, the relationship between treatment effects and dollar store densities likely depends on initial pre-dollar-store entry supermarket levels. To investigate these hypotheses, we regress the estimated treatment effects on categorical variables indicating the number of dollar store entry events (or gross entries) interacted with year 2005 supermarket and grocery store counts.

$$\hat{\tau}_{it} = \sum_{d=1}^{D} \sum_{s=1}^{S} \beta_{ds} \mathbb{1}[\text{ \#DS Entries } = d]_{it} \cdot \mathbb{1}[\text{ \#Grocery } = s]_{i(t=2005)} + v_{it}, \text{ for all } t \geq e_i \quad (5)$$

The estimates for β_{ds} measure the average treatment effect for a given number of dollar store entries and grocery stores within a two-mile driving distance of block-group populations in the year 2005. Figure 6 displays the estimated coefficients and 99% confidence intervals using bootstrapped standard errors from the regression in Equation 5.[24] Figure 6 shows

[23] As noted in Section 3.3, in addition to the initial dollar store entry events, we also track the cumulative number of entries and the gross number of dollar store entries for each treated block group.

[24] The set of categorical grocery store counts include #Grocery $\in [0, 1, 2, 3, 4, 5, (5, 7], (7, 10], > 10]$. Dollar store entry events are categorized by the set #DS Entries $\in [1, 2, 3, > 3]$. In Figure 6, we exclude the

that the average treatment effects grow with successive dollar store entry events, but the magnitude of the impacts are conditioned on the initial number of supermarkets and grocery stores in the year 2005. The effects are strongest in the block groups with one or two grocery stores. With one dollar store entry and one grocery store in 2005, the average treatment effect is approximately 0.03 and statistically different from zero. But, conditioned on one grocery store in 2005, the ATT rises to approximately 0.09 when the block group experiences three or more entry events. In contrast, when three or more grocery stores are present in 2005, the effects of a single and multiple dollar store entries are approximately zero and statistically insignificant. The heterogeneous treatment effects with respect to dollar store densities and initial pre-entry grocery store counts suggests that dollar store entry and densities mostly impact small, local markets with only one or two grocery stores.

5.5 Heterogeneity by Socio-Demographic Characteristics

We assess the degree of treatment effect heterogeneity across demographic and socioeconomic characteristics using the estimated treatment effects from the urban-area model. To compare the relative importance of each socio-demographic predictor on the treatment effect, we standardize the covariates to have mean of zero and standard deviation of one. Using the standardized predictors, we run the following bivariate regression using the estimated treatment effects and socio-demographic predictors:

$$\hat{\tau}_{it} = \alpha + \beta Z_{it} + v_{it}, \text{ for all } t \geq e_i \tag{6}$$

In the above Equation, $\hat{\tau}_{it}$ is the estimated treatment effect for block-group i at time t, Z_{it} is a standardized covariate, and v_{it} is a random error term. We run equation 6 for each

estimated coefficients including the grocery store count categories corresponding to $(0, 5, (5, 7], (7, 10], > 10)$, all of which are approximately zero and statistically insignificant. The regression results in which we interact gross dollar store entries with year 2005 grocery store counts are similar to those in Figure 6. The figure with gross dollar store entry estimates is shown in Appendix D.2. The set of categorical gross dollar store entries includes block groups with four or more gross dollar store entries (i.e., > 4).

socio-demographic covariate and rank the coefficient estimates in descending order. Each estimated coefficient corresponding to β is interpreted as the change in the treatment effect in terms of a one standard-deviation increase in the predictor. Standard errors are obtained from the bootstrap procedure outlined in Section 4.2. Figure 7 displays the coefficient estimates from each bivariate regression.

Because the features are standardized, the magnitude of the estimated coefficients measures the strength of the correlation between the estimated treatment effects and the predictors. Figure 7 indicates that a one standard-deviation increase in the population share that is Black, the poverty rate, and the share of vacant housing, respectively, increases the likelihood that the block-group population experiences decreased access to conventional supermarkets or grocery stores as a result of dollar store entry. Other predictors positively correlated with the treatment effects include the share of the population commuting with a private vehicle, the unemployment rate, and the share that receives public assistance. Alternatively, a one standard-deviation increase in the population share that is White (Hispanic), income per capita, and the population share with at least a bachelor's degree, respectively, are all negatively associated with the estimated treatment effects.

The estimates in Figure 7 mostly align with the narrative in popular press and from policy advocates promoting dollar-store growth bans, moratorium, or zoning ordinances (Corkery, 2023; MacGillis, 2020; Mitchell et al., 2023; Sainato, 2019). In urban areas, dollar store densities and their impact on food access are cited to be strongest in economically disadvantaged, Black neighborhoods with limited access to conventional supermarkets. We similarly find strong positive correlations between the estimated treatment effects and one standard-deviation increases in the population share that is Black, the poverty rate, and the share of vacant housing. In conjunction with our results in Section 5.4, we can further infer that, in urban areas, the effects of dollar store growth on food access is largest in low-income, predominantly Black communities, under-served by conventional chain supermarkets and grocery stores. Comparatively lower levels of food retail competition in these localized markets may allow dollar stores to compete more directly with grocery stores that serve economically disadvantaged households.

6 Discussion

Our results reveal heterogeneous treatment effects of dollar store entry on block-group level food access that vary over geographic space, community socio-demographic characteristics, and the food retail environment prior to dollar store entry. After controlling for a host of socio-demographic factors and neighborhood amenities, we find that dollar store entry within a ten-mile drive of rural-area block-group population-weighted centroids has no statistically or economically significant effect on household food access measured within the same area. Our results indicate that, relative to the counterfactual scenario in which dollar stores do not enter rural-area block groups, the share of low-access communities decreased by less than one percent in rural areas ($\mathrm{ATT}_{\mathrm{rural}} = -0.97\%; \mathrm{CI}^{\mathrm{ATT}}_{\mathrm{rural}} = [-4.21\%, 2.26\%]$). In analyses of treatment effect heterogeneity with respect to multiple dollar store entries, the estimated impacts of dollar store entry were also small and almost all statistically insignificant in rural-area block groups that experienced dollar store entry. For treated block groups in rural areas with one or two grocery stores in the year 2005, multiple dollar store entries had little impact on changes to food access (see Figure 5.D in Appendix D.3). The analyses of treatment effect heterogeneity with respect to socio-demographic factors and across geographic regions similarly showed mostly statistically insignificant or economically small relationships between treatment effects and block-group level characteristics.

The impacts of dollar store expansion on food access may be minimal in rural towns because access to conventional supermarkets is already constrained. Relative to urban areas, the share of low-access block groups is higher during the 2006 to 2020 study period due to the presence of fewer supermarkets and grocery stores in less-populated rural communities (see Tables 5 and 6). Therefore, the arrival of dollar stores in areas with little retail potential has no impact on block-group level food access. On the other hand, for rural towns in which conventional grocery stores can viably operate, the competitive effects of dollar store entry may be more localized than a ten-mile drive from block-group households. We used a ten-mile driving distance threshold because it aligns with actual shopping behavior of rural-area households and is consistent with the USDA's measure of low-access.

In contrast to our rural area results, we find that, absent of dollar store entry within a two-mile drive of urban-area block-group population-weighted centroids from 2006 to 2020, food access would be approximately 14% higher in treated block groups ($\text{CI}^{\text{ATT}}_{\text{urban}} = [9.88\%, 18.07\%]$). The event-study diagrams plotting the actual share of low-access block groups relative to the counterfactual shares predicted by the machine learning model show that the impacts of dollar store entry grow over time following the initial post-2005 dollar store entry. In the counterfactual scenario in which dollar store entry does not occur, our results indicate that block-group access to conventional supermarket and grocery stores would be approximately 30% greater in treated block groups ten years following their first post-2005 dollar store entry.

In assessing treatment effect heterogeneity, we find that the negative impacts of dollar store entry on block-group level food access are most associated with the share of the population that is Black, the poverty rate, and the share of vacant housing, while the effects are less correlated with the population shares that are White and Hispanic, income per capita, and the share of the population with at least a bachelor's degree. The latter associations imply that, in urban areas, dollar store entry most negatively impacts food access in economically-disadvantaged communities, which also tend to be the areas targeted by policymakers for allocating program funds that promote food access (see, for example, the Reinvestment Fund (2023)). Local governments implementing dollar-store restriction policies also tend to be poorer cities and towns and have a higher share of the population that is Black, relative to the national average (McCarthy et al., 2022).

Yet, it is important to note that, relative to other income strata, low-income households tend to spend a larger share of their food expenditures at dollar stores (Feng et al., 2023; Volpe, Kuhns, & Jaenicke, 2017). Other research similarly indicates that dollar store entry mostly impacts household food expenditures among low-income, transportation-constrained, and Black households (Caoui et al., 2022). Even in communities with a nearby grocery store, economically-constrained households may substitute purchases at the local grocery store for products at dollar stores due to consumer preferences for the products offered at dollar stores or their lower prices compared to conventional food stores. Further, the leading dollar store chains have gradually increased the amount of everyday household products, and especially perishable foods, in their stores (Zboraj, 2023), implying that the dollar store format may more commonly be viewed by consumers as a viable alternative to shopping at conventional grocery stores. Thus, while we find evidence that dollar store entry negatively impacts access to conventional supermarkets and grocery stores in certain urban communities, the net effect of dollar store entry on food access may be more nuanced. Research could investigate the motivations of households to shop at dollar stores relative to conventional grocery stores, particularly in communities with one or two grocery stores. Additional small-area studies could compare the food products at contemporary dollar stores and conventional food retailers across different retail landscapes (Caspi et al., 2016; Sharkey et al., 2010).

For treated block groups with one or two grocery stores within a two-mile drive in the year 2005, we show that the negative impacts of dollar store entry on food access increase with the number of dollar store entries, implying that community food access may decline as the density of dollar stores rises. On the other hand, in block-groups with three or more pre-entry grocery stores within a two-mile drive, we find that dollar store expansion has minimal or no effect on block-group household food access, even as dollar store densities increase. Markets that can only support one to two stores may be served predominantly by independent grocery stores, which tend to be smaller in scale and have higher operating costs, making them most vulnerable to dollar store entry. In contrast, markets large enough to support three or more grocery stores may have a variety of independent and chain supermarkets. The latter stores tend to benefit from economies-of-scale and scope and are larger in size, implying that dollar store entry is unlikely to drive their exit. Because our measure of access includes both independent and chain grocery stores, we do not determine the composition of stores located in the vicinity of each block group. Future research could investigate the characteristics of grocery stores across markets with varying levels of retail food store accessibility to better understand the diminishing marginal effects of dollar store entries as the total number of grocery stores increases.

While our data-driven machine learning imputation models suggest that, at least in urban areas, supermarket and grocery store access would be greater had additional dollar stores not opened, it is important to note several limitations of our findings. First, our results apply only to block groups that received at least one additional dollar store after the year 2005. Second, while many communities experienced dollar store entry during our period of analysis, the lack of an effect on food access does not imply that dollar stores had no impact on other aspects of food retail competition. Future research could study the effects of dollar store entry on retail food store counts, revenue, and employment by exploring the heterogeneity of effects across dollar store chain types, food retail channels (e.g., supermarkets, superettes, etc.), market geography, and market socio-demographic characteristics, building on the work of Caoui et al. (2022); Chenarides et al. (2023); Marchesi et al. (2023). Though our findings suggest that dollar store entry decreased food access in some communities, our results do not necessarily imply that households are worse off as a result of dollar store expansion. Though there may be economic and social costs from unequal access or the loss of local grocery stores, there may also be gains from dollar store entry in terms of lower prices, greater product variety, or reduced transportation costs in acquiring similar goods at more distant stores. Additionally, while the exit of grocery stores implies a loss of employment for the local population and a less economically dynamic retail environment, dollar stores also provide employment opportunities and retail services. Future research could comprehensively assess the welfare impacts of dollar store entry.

7 Conclusion

We investigate the impacts of dollar store entry on food access across the United States from 2006 to 2020, a period in which dollar stores expanded their store network from predominantly the South and Midwest to the Northeast and West regions. Contemporaneous with their proliferation in both urban and rural markets, dollar stores gradually increased the share of household goods and perishable food items in their store inventory, particularly following the Great Recession, as low- and middle-income households aimed to economize on everyday products. The rise in sales of household goods and food items at dollar stores has increased their competition with conventional supermarkets and small grocery stores and generated concern among policymakers and policy advocates regarding their potential impact on U.S. household food access.

We combine exact store-location information on supermarkets and grocery stores, dollar store entries, and market-level covariates containing demographic, socioeconomic, and economic geography characteristics at the block-group level to estimate the effects of dollar store entry on U.S. household food access. We leverage machine learning techniques to impute the counterfactual outcomes of block-group low-access status. We find little evidence that dollar store entry impacts the share of low-access block groups in rural areas within a ten-mile drive of block-group population-weighted centroids. In urban areas, we estimate that, absent of dollar store entry within a two-mile drive of block-group population-weighted centroids, the share of low-access block groups would be approximately 14% lower, implying that dollar store entry in these areas decreased household access to conventional supermarkets and grocery stores by this amount, on average. Further, we find statistically significant, heterogeneous effects of dollar store entry within a two-mile drive of urban-area households that vary over time, by geographic region, the neighborhood's socioeconomic and demographic characteristics, and pre-entry food retail environment.

Despite their potential negative effect on food access with respect to conventional supermarkets and grocery stores, our results do not necessarily imply that consumers and communities are worse off in terms of prices paid for household goods and food products, health outcomes, or economic development. Welfare analyses that incorporate estimates of consumer spending and retail profitability pre- and post-dollar store entry may provide a more complete assessment of the overall economic impact of dollar store expansion. By providing evidence that dollar store entry reduces food access in primarily economically disadvantaged urban markets, our study is a first step in this direction.

Table 1: Blocked Cross-Validation Folds by Year with Number of Observations per Fold

Folds	1	2	3	4	5	6	7
Years	2007	2009	2011	2013	2015	2017	2019
	2008	2010	2012	2014	2016	2018	2020
Urban*	266,575	234,114	201,092	167,977	145,191	128,865	116,989
Rural*	74,208	60,302	50,655	40,966	32,438	25,180	20,809

We include data from the years 2005 and 2006 in each trained cross-validation model.
* Indicates the number of observations by cross-validation fold for urban and rural models using the actual data. The bootstrap models contain approximately the number of observations observed in the actual data and varies by the bootstrap sample of block groups.

Table 2: Demographic, Socioeconomic, Housing, Mobility, Market Geography, and Retail Variables

Demographics	Socioeconomics	Housing and Household Mobility	Market Geography	Retail Counts (2005)
Total Population	Per-Capita Income ($)	Population shares:	Public and private schools	Convenience Stores
Population shares:	Population shares:	Vacant housing units	Park accessibility	Dollar Stores
White	Poverty rate	Population without vehicle	Land-Use and Development	Drug Stores
Black	Receiving public assistance		Roadway miles	Wholesale Club
Hispanic	Unemployed		Urban cluster (0/1)	Superettes
Asian	Education less than high school diploma		Distance to nearest urban area (Miles)	General Merchandisers
Age 18-34	Bachelor's degree or higher		State Fixed Effects (est.)	Mass Merchandisers
Age 35-65			Year Fixed Effects (est.)	Supermarkets/Grocery Stores
Age 65 and over			State-by-Time Fixed Effects (est.)	

Details for each variable are available in Section 3.
All nominal dollar values are converted to real 2020 dollars using the R-CPI-U-RS.
The state, year, and state-by-year fixed effects are separate vectors estimated by extracting the fixed-effect estimates from regressing the binary low-access outcomes on the model covariates and state, year, and state-by-year controls using only the pre-treatment data.
The year 2005 supermarkets and grocery store counts are not included in the ML predictive models. We use the supermarket and grocery store counts in exploring treatment effect heterogeneity as the number of dollar store entries increases.

Table 3: Optimal Hyperparameters and Cross-Validation Error

Hyperparameter	Bounds	Urban	Rural
eta	[0.025, 0.3]	0.0421	0.0675
max_depth	[12, 25]	20	24
gamma	[0.1, 0.3]	0.1026	0.2291
colsample_bylevel	[0.7, 0.9]	0.8984	0.7966
subsample	-	0.8	0.8
colsample_bytree	-	0.75	0.75
scale_pos_weight	$\frac{\sum_i^N \mathbb{1}[y_{it}=0]}{\sum_i^N \mathbb{1}[y_{it}=1]}$	4.0404	1.5018
Avg. CV Error		0.0161	0.0138

We implement 7-fold blocked-CV to find the optimal tuning parameter combination that minimizes the cross-validated prediction error.

The average cross-validation classification error is based on 500 bootstrap estimates using the optimal hyperparameter set.

To prevent model over-fitting, we stop the cross-validation procedure if the prediction error does not decrease after 10 boosting iterations.

Table 4: Average Treatment Effects of Dollar Store Entry on Low-Access Block Groups by Market Geography

Model	Treatment Effect	Estimate	Lower	Upper
Urban	ATT	0.0069	0.0050	0.0088
	Δ ATT(%)	0.1397	0.0988	0.1807
Rural	ATT	-0.0009	-0.0037	0.0020
	Δ ATT(%)	-0.0097	-0.0421	0.0226

The lower and upper 99% confidence intervals are based on bootstrapped standard errors.

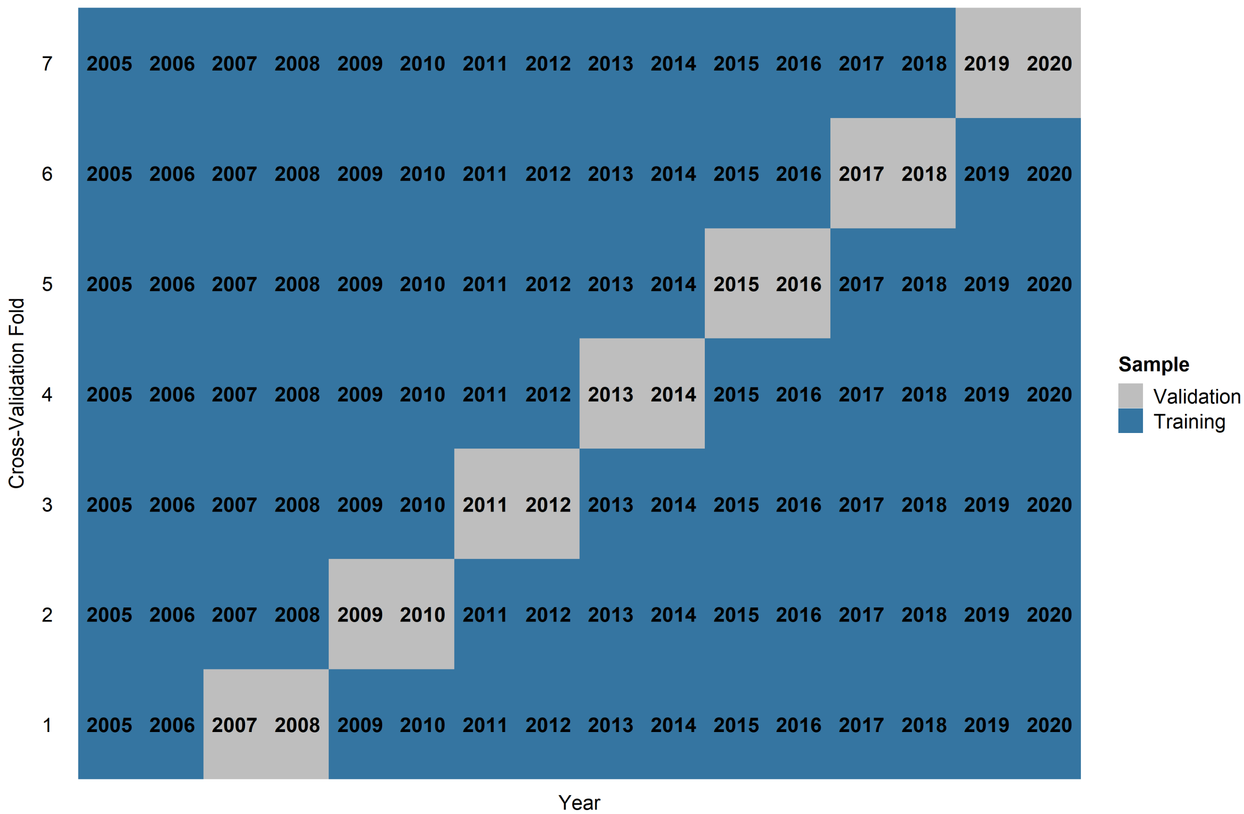

Figure 1: Blocked 7-fold Cross-Validation Design for Modeling Low-Access Status. Figure 1 shows the splitting of cross-validation folds by time, in two-year increments. Only pre-treatment observations (never-treated and yet-to-be-treated block groups) are used to build and select the optimal ML model that minimizes the out-of-sample cross-validation prediction errors. The model that achieves the highest predictive accuracy is applied to the post-treatment data to construct counterfactual outcomes of block-group low-access status.

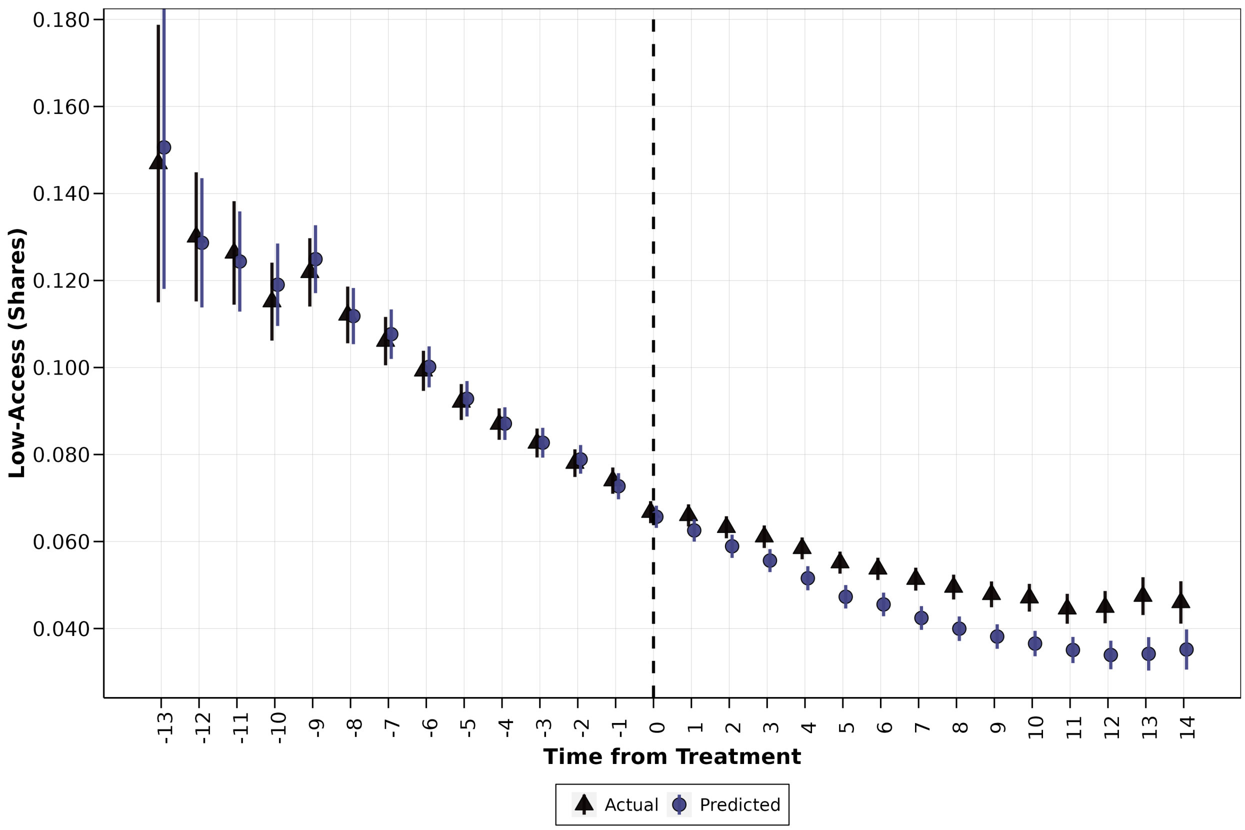

Figure 2: Actual vs Predicted Low-Access Status in Urban Areas. Figure 2 compares the actual and predicted low-access shares at the block-group level by relative time from dollar store entry in urban areas. The cross-validated predicted share of low-access block groups closely fits the actual shares of low-access block groups during the pre-treatment periods, with no indication of anticipatory effects or violation of parallel trends. In the post-treatment periods, we find that absent of dollar store entry, the counterfactual predicted share of low-access block groups in urban areas is estimated to be lower than the actual share, indicating that dollar store entry decreases food access within a two-mile driving distance of block-group centroids. The observed difference represents the estimated causal effect of dollar store entry on food access. The line-range associated with each point estimate represents 99% confidence intervals based on bootstrapped standard errors.

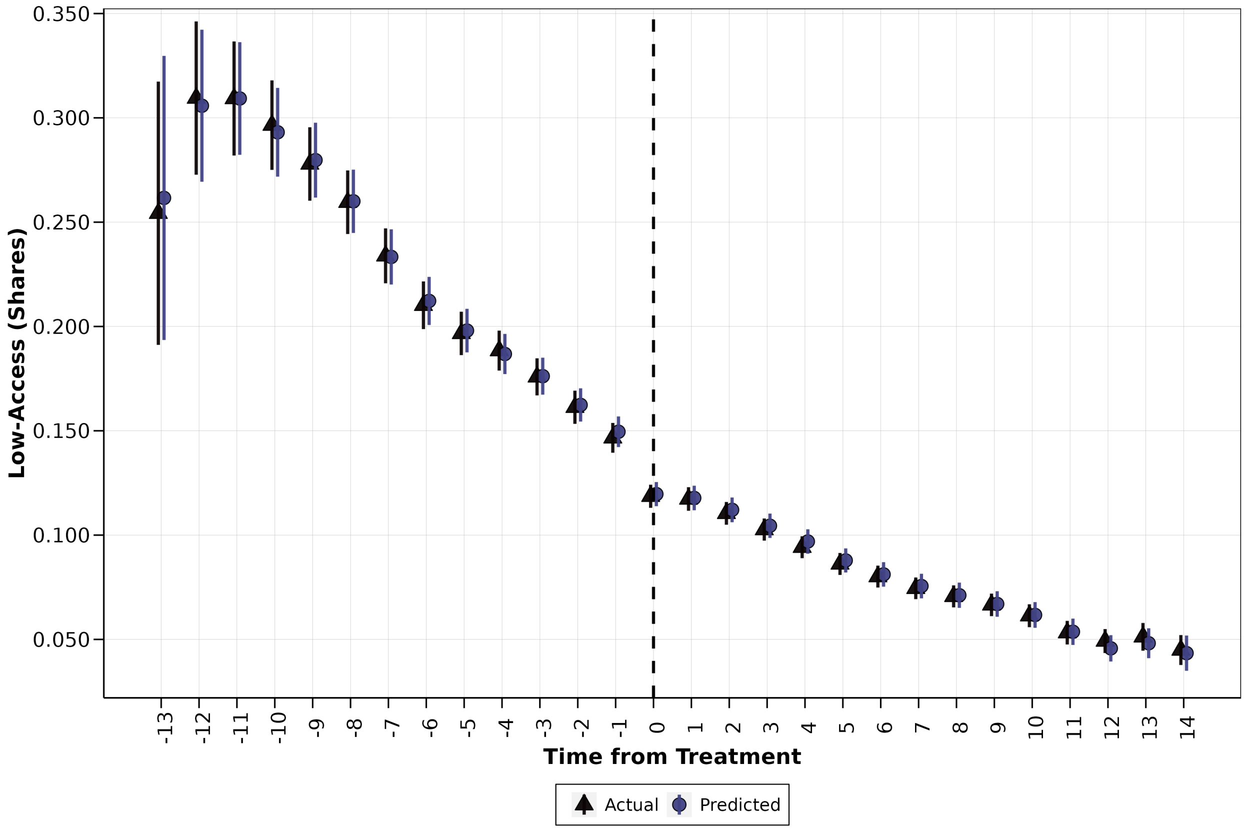

Figure 3: Actual vs Predicted Low-Access Status in Rural Areas. Figure 3 compares the actual and predicted low-access shares at the block-group level by relative time from dollar store entry in rural areas. The cross-validated predicted share of low-access block groups closely fits the actual shares of low-access block groups during the pre-treatment periods, with no indication of anticipatory effects or violation of parallel trends. In the post-treatment periods, the counterfactual predicted shares of low-access block groups align almost perfectly with the actual shares, suggesting that dollar store entry has minimal impact on food access within a ten-mile driving distance of block-group centroids. The line-range associated with each point estimate represents 99% confidence intervals based on bootstrapped standard errors.

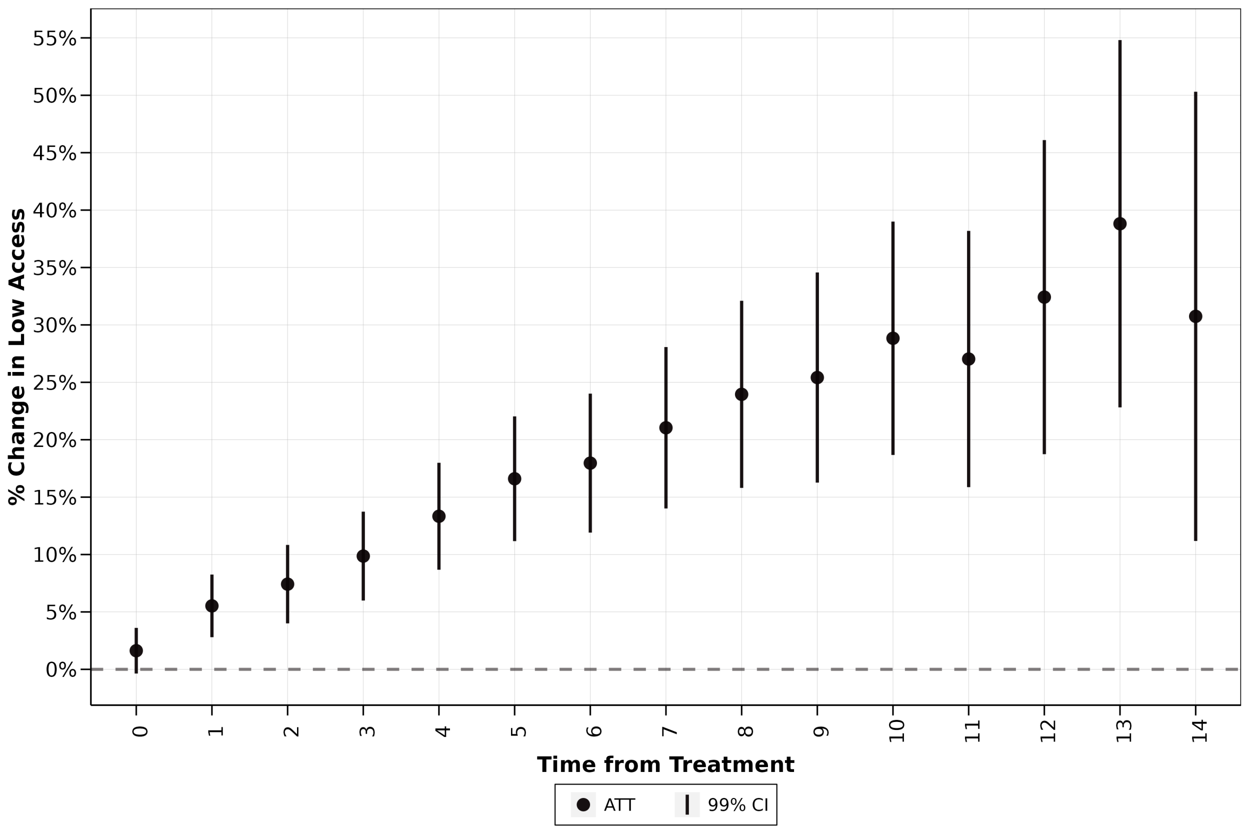

Figure 4: ATT (%) by Time from Treatment in Urban Areas. Figure 4 shows the percentage change in the share of low-access block groups with respect to the relative time from dollar store entry in urban areas. The 99% confidence intervals are computed from bootstrapped standard errors. Figure 4 illustrates that the impact of dollar store entry on food access grows over time in urban areas.

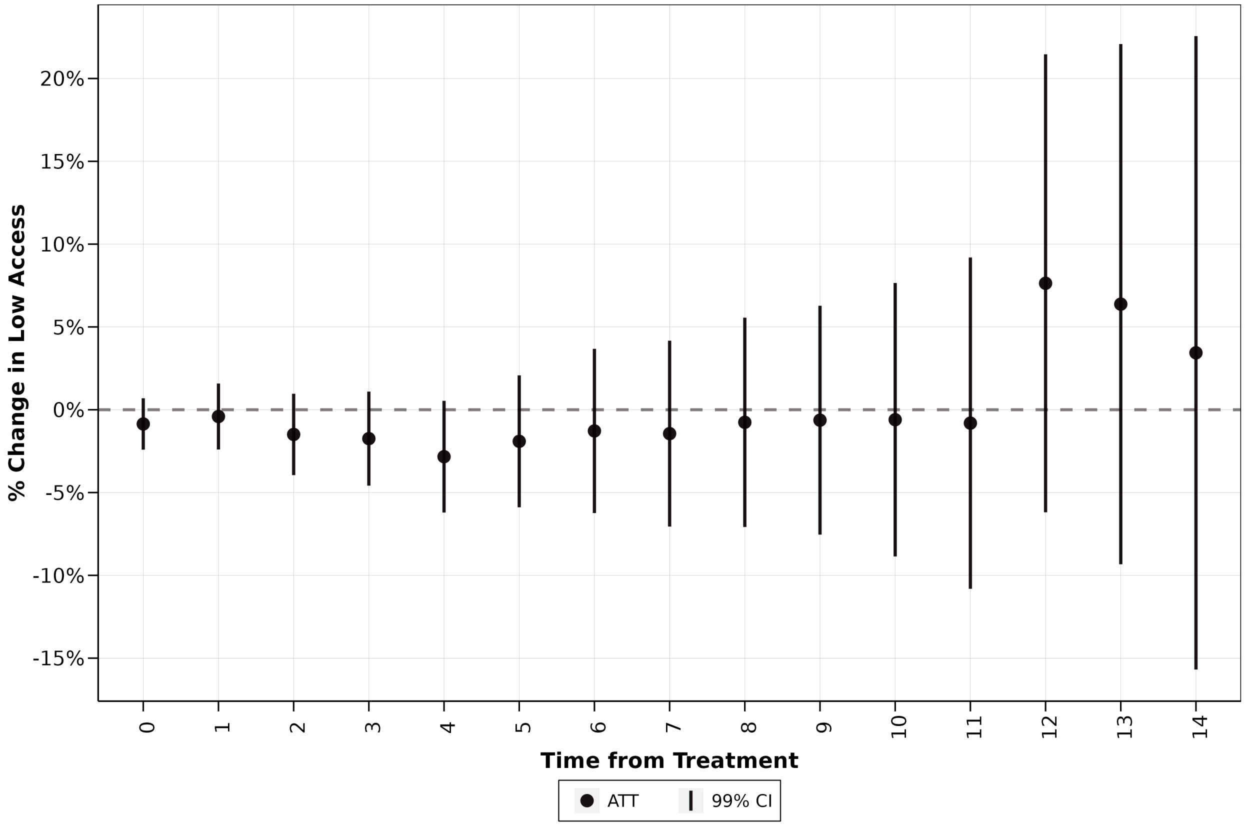

Figure 5: ATT (%) by Time from Treatment in Rural Areas. Figure 5 shows the percentage change in the share of low-access block groups with respect to the relative time from dollar store entry in rural areas. The 99% confidence intervals are computed from bootstrapped standard errors. Figure 5 indicates that there is no effect of dollar store entry on food access in rural areas.

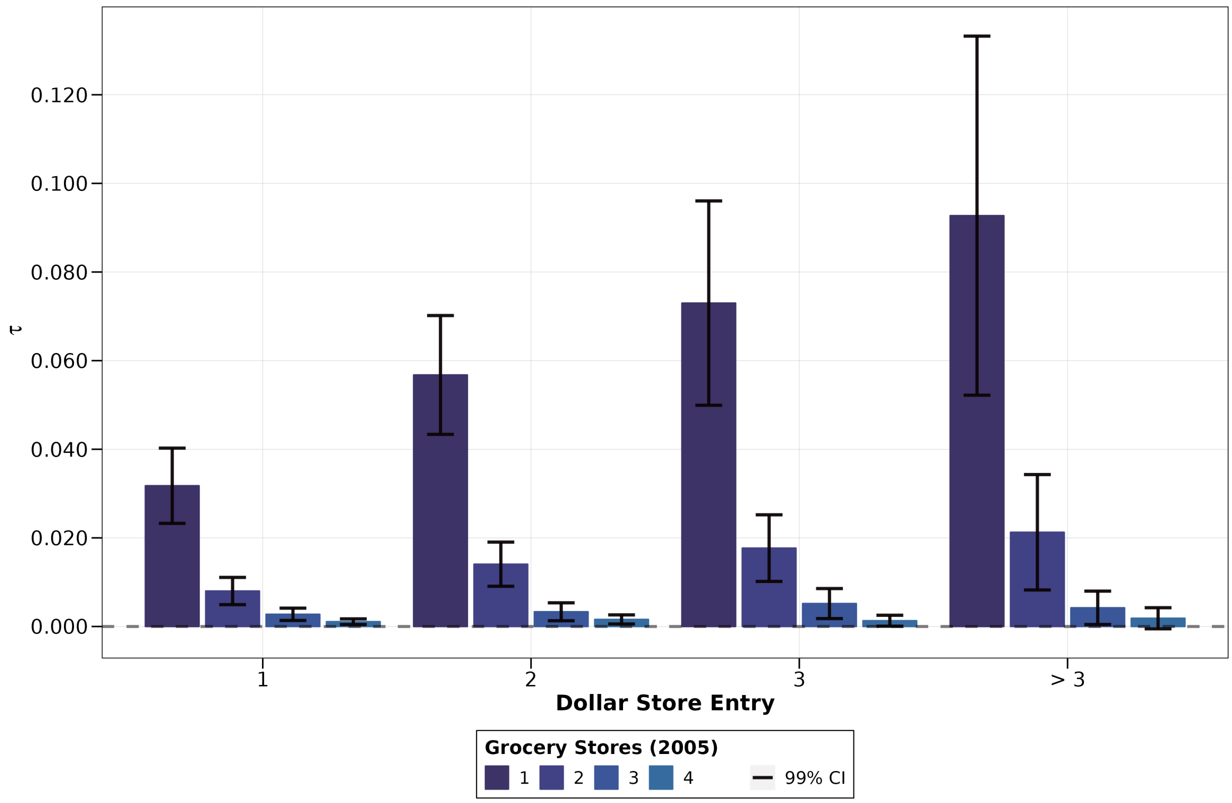

Figure 6: Impact of Multiple Dollar Store Entries by Pre-Entry Grocery Stores in 2005 (Urban Areas). Each bar represents the point estimate from the regression in Equation 5 using the estimated treatment effects and dollar store entries from the urban area model. The 99% confidence intervals corresponding to each estimated coefficient are derived from the bootstrapped estimation procedure described in Section 4.2. Figure 6 shows that the impact of multiple dollar store entries over time depends on the initial number of supermarkets and grocery stores in 2005.

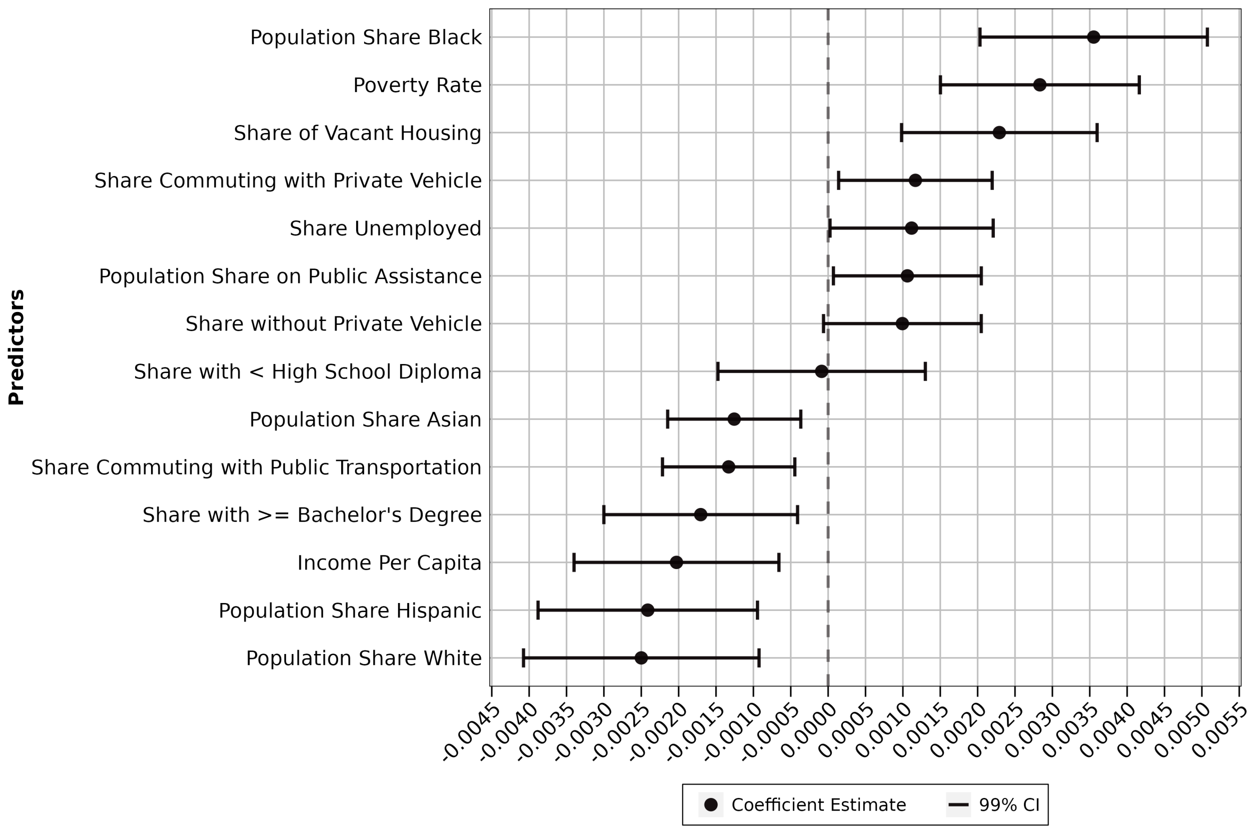

Figure 7: Treatment-Effect Heterogeneity by Socio-Demographic Characteristics (Urban Areas). Figure 7 contains each standardized predictor coefficient from estimating Equation 6 and the confidence intervals constructed from bootstrapped standard errors. The three socio-demographic characteristics that are most positively associated with increasing low-access status as a result of dollar store entry are the population share that is Black, the poverty rate, and the share of vacant housing. The 99% confidence intervals are derived from bootstrapped standard errors.

A Data: Supplementary Material

We provide further detail about our data sources and methods for feature creation. We present summary statistics on U.S. household travel distances to conduct shopping activities. We provide summary statistics for the shares of low-access block groups and compare our measure with the USDA's Food Access Research Atlas (FARA) census-tract level low-access estimates. We

A.1 Defining Low Access

Following Allcott et al. (2019), we use the 2017 National Household Transportation Survey (NHTS) (*National Household Travel Survey (NHTS)*, 2017), which collects detailed daily transport activities of approximately 130,000 U.S. households, including each surveyed household member's trip-level information regarding their origin and destination, means of transit (e.g., private vehicle or public transportation), transportation characteristics, and travel time and distance. The survey also includes household member demographic and socioeconomic information, including race, education, income, vehicle and home ownership. The NHTS indicates that approximately 94% of shopping trips (i.e., purchasing groceries, clothes, appliances, and gas) were conducted using a private vehicle (e.g., car, truck, van, SUV, motorcycle) and over one-third of shopping trips originated from home, the largest share of any other trip origin location.

Figure 1.A displays summary statistics of U.S. household average driving distances (in miles) while shopping for household goods. Figure 1.A includes the 25^{th} and 75^{th} percentiles, median and mean household driving distances at the national scale, as well as by income status (high- and low-income households), geography (urban and rural), and for households using private vehicles. We define high-income (low-income) households as those with incomes at or above (at or below) the 75^{th} (25^{th}) percentile of household incomes. The NHTS uses the U.S. Census Bureau definition of urban areas for determining whether households live in urban (urbanized or urban-cluster areas) or rural areas (US Census Bureau, 2021).

Figure 1.A indicates that the median one-way driving distance for shopping is approximately 2.5 miles nationally. In urban areas, median high-income household travel distance is 2.6 miles, but low-income households travel 1.9 miles. In rural areas, there is considerable variation in household shopping distance. Because of the geography of rural areas and sparse populations, median driving distance is greater for rural households compared to urban households. The unconditional median shopping travel distance for rural-area house-

holds is 3.455 miles. However, travel distance varies significantly by income and access to a private vehicle in rural areas. The mean ranges from approximately 10 miles for high-income, rural households with access to a private vehicle, to approximately 7.8 miles for rural, low-income households. Figure 1.A also illustrates that conditional on household geography (i.e., national, urban or rural), low-income households tend to drive shorter distances when shopping for everyday household goods. Access to own-transportation allows households to increase their driving distances.

Consistent with data from the NHTS, two- to three-mile distances, especially in urban areas, are cited as empirical distances that households are willing to travel to purchase groceries. Vitaliano (2022) investigates the appropriate distance threshold for defining food deserts and develops a structural model using spatial economic theory to estimate the minimum market radius for small supermarket viability in U.S. urban areas. (Vitaliano, 2022) applies the theoretical model to consumer- and retail-level data on shopping and costs to determine that the economically feasible circular market radius is approximately 3 miles, which equates to a two-mile driving distance. Based on the National Household Food Acquisition and Purchase Survey (FoodAPS), a nationally-representative survey collecting detailed information on household food purchases, including for diverse subgroups of the U.S. population (e.g., by demographics, socioeconomic status, participation in federal assistance food programs), Ver Ploeg et al. (2015) find that the average Euclidean distance that households traveled to the store from which which they purchased the majority of their food varied from 2.82 miles for food insecure to 3.98 miles for food secure households. The average distance for SNAP and WIC households was 3.26 and 3.10 miles, respectively. On average, the distance to the nearest SNAP-authorized supermarket, though not necessarily the store at which households shopped, was approximately 2 miles, regardless of income or participation in food assistance programs (Ver Ploeg et al., 2015).

In summary, statistics from the 2017 NHTS and empirical evidence from the literature suggest that most U.S. households shop for goods using a private vehicle, and that a significant proportion of shopping trips originate from household member homes, which implies that finding stores within a threshold drive distances from block-group population-weighted centroids provides a realistic measure of the actual household shopping environment. Additionally, the average travel distance that households incur to purchase groceries is approximately two miles in urban areas, while it varies from three to ten miles in rural areas. Ten miles is a conservative threshold driving distance for defining adequate food access in rural areas. While a ten-mile drive to the nearest grocery store overestimates the distance that many rural households drive to shop based on median driving distances in the NHTS, the mean one-way driving distance of rural high-income and rural high-income households with a private vehicle is approximately ten miles. A ten-mile distance threshold for determining adequate access is also conservative in that lower-income households in both urban and rural areas tend to shorten their distance traveled to economize on transportation costs, relative to high-income households.

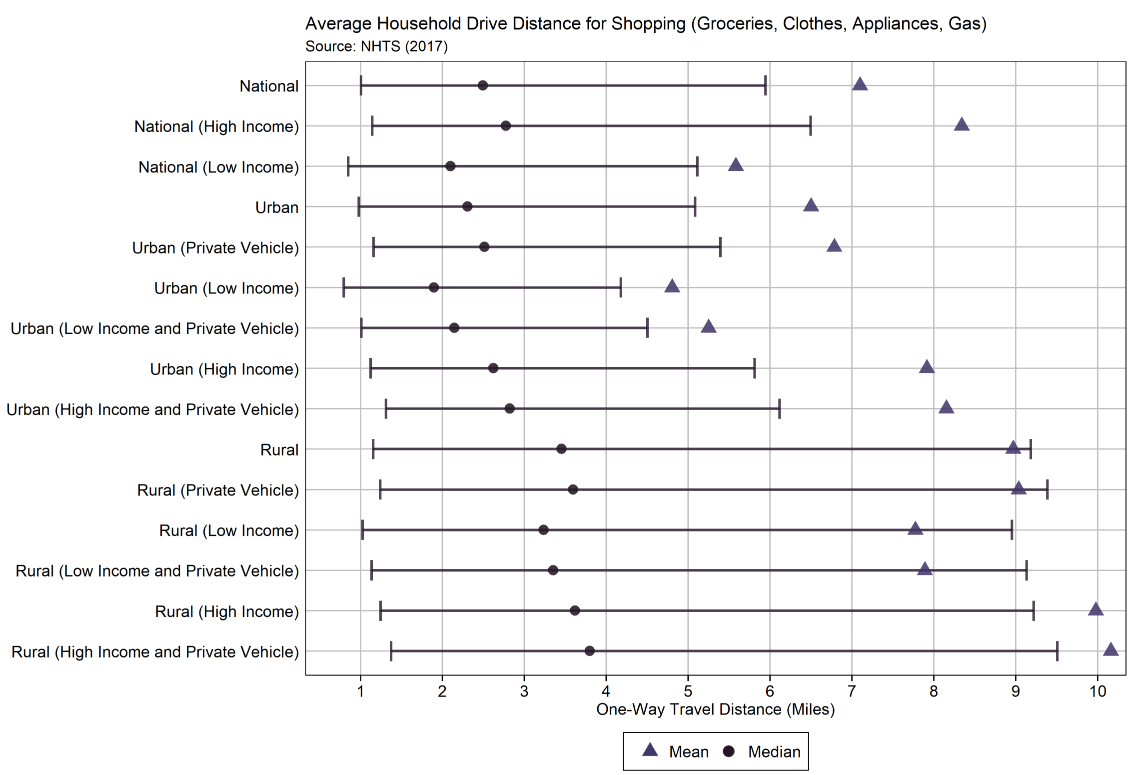

Figure 1.A: Average Household Drive Distance for Shopping (Groceries, Clothes, Appliances, Gas); Source: National Household Travel Survey (NHTS), 2017

A.2 Low-Access Shares over Time

Tables 5 and 6 show the total number and share of low-access block groups from 2005 to 2020 for all block groups (All), block groups that never experience a post-2005 dollar store entry (Never-Treated), and block groups that experience at least one post-2005 dollar store entry (Treated). Across all years, the share of low-access block groups in urban areas is lower than in rural areas. In both urban and rural areas, the share of low-access block-groups in treated units is lower than in never-treated units. Block groups in which dollar stores enter are likely better retail store locations, in general, and therefore, are less likely to not have a supermarket or grocery store within a two- or ten-mile drive. The share of low-access block groups in both urban and rural areas declines during most of our study period and increases slightly beginning in 2017 and 2018.

Because we measure low access across different units, use different distance thresholds and methods to determine whether an area is low access, we cannot directly compare our low-access estimates to the USDA's FARA. For example, the FARA's low-access is a census-tract based measure that denotes areas as low access if a sufficient number or share of the census tract population is further than a given distance from the nearest supermarket or grocery store. The low-access indicators created for our study are based on the driving distance between block-group population-weighted centroids and nearby supermarkets or grocery stores. Nevertheless, we can compare the low-access shares and trends of our block-group low-access measure with the USDA's FARA census-tract level measures of low access. The USDA creates low-access indicators for multiple distance thresholds (e.g., half- and one-mile in urban areas, ten- and twenty-miles in rural areas). Therefore, we compare our two- and ten-mile low-access measures at the block-group level to the FARA's one- and ten-mile low-access indicators at the census tract level because they are most similar.

Table 7 indicates that by using a two-mile rather than one-mile distance for determining low-access in urban areas, the share of low-access areas in our study is approximately half the low-access shares in the FARA. The shares of low-access census tracts and block groups both decrease from 2010 to 2019, indicating that a smaller share of the population lacks nearby access to a supermarket or grocery store over time. The share of low-access census tracts within a one- and ten-mile distance declines by approximately 0.014, while the share of low-access block groups decreases by approximately 0.002. Despite the differences in census geographies and methodologies for creating low-access indicators in our study and in the FARA, both low-access measures coincide in that the share of low-access households has fallen from 2010 to 2019.

Table 5: Number and Share of Low-Access Block-Groups in Urban Areas (2005-2020)

		Total			Share	
Year	All	Never-Treated	Treated	All	Never-Treated	Treated
2005	24,380	16,855	7,525	0.148	0.291	0.07
2006	24,211	16,676	7,535	0.147	0.288	0.07
2007	23,966	16,475	7,491	0.145	0.285	0.07
2008	23,650	16,284	7,366	0.143	0.281	0.069
2009	23,636	16,259	7,377	0.143	0.281	0.069
2010	23,300	16,161	7,139	0.141	0.279	0.067
2011	23,007	16,050	6,957	0.14	0.277	0.065
2012	22,978	15,984	6,994	0.139	0.276	0.065
2013	22,759	15,822	6,937	0.138	0.273	0.065
2014	22,614	15,740	6,874	0.137	0.272	0.064
2015	22,697	15,837	6,860	0.138	0.274	0.064
2016	22,553	15,758	6,795	0.137	0.273	0.064
2017	22,431	15,654	6,777	0.136	0.271	0.063
2018	22,873	15,705	7,168	0.139	0.272	0.067
2019	23,133	15,757	7,376	0.14	0.273	0.069
2020	23,736	15,998	7,738	0.144	0.277	0.072

Table 5 shows the total and share of low-access block groups in urban areas from 2005 to 2020. Low-access block groups in urban areas are defined by whether the population-weighted block-group centroid has a supermarket or grocery store within a two-mile drive.

Table 6: Number and Share of Low-Access Block-Groups in Rural Areas (2005-2020)

	Total			Share		
Year	All	Never-Treated	Treated	All	Never-Treated	Treated
2005	10,066	5,154	4,912	0.2	0.504	0.123
2006	10,051	5,132	4,919	0.201	0.503	0.123
2007	10,091	5,136	4,955	0.201	0.503	0.124
2008	9,959	5,120	4,839	0.199	0.502	0.121
2009	9,937	5,104	4,833	0.198	0.5	0.121
2010	9,880	5,107	4,773	0.197	0.5	0.12
2011	9,790	5,079	4,711	0.195	0.498	0.118
2012	9,799	5,051	4,748	0.196	0.495	0.119
2013	9,798	5,049	4,749	0.196	0.495	0.119
2014	9,788	5,043	4,745	0.195	0.494	0.119
2015	9,795	5,043	4,752	0.196	0.494	0.119
2016	9,689	4,991	4,698	0.193	0.489	0.118
2017	9,632	4,973	4,659	0.192	0.488	0.117
2018	9,656	4,958	4,698	0.193	0.486	0.118
2019	9,703	4,962	4,741	0.194	0.486	0.119
2020	9,885	4,996	4,889	0.197	0.49	0.123

Table 6 shows the total and share of low-access block groups in rural areas from 2005 to 2020. Low-access block groups in rural areas are defined by whether the population-weighted block-group centroid has a supermarket or grocery store within a ten-mile drive.

Table 7: Comparison with USDA-ERS FARA Low-Access (LA) Measures

	LA Census Tracts* One- and Ten-Miles			LA Block Groups+ Two- and Ten-Miles		
Year	Total	Shares	Change	Total	Shares	Change
2010	28,541	0.394	-	33,180	0.154	-
2015	27,527	0.380	-1,014	32,492	0.151	-688
2019	27,548	0.380	21	32,836	0.153	344

*The USDA-ERS summary statistics for low-access census tracts within a one- and ten-mile distance come from Rhone et al. (2017) and Alana et al. (2022).
+The block-group level low-access shares are based on the low-access indicators used in this study.

A.3 Compilation of Park and Green Space Databases

To create a comprehensive, national-scale database of park and green recreation space areas, we collect data from three sources, subsetting parks and green spaces in the contiguous United States coinciding with our data on retail store locations. First, we use ESRI's USA Parks database (ESRI, 2021), which contains a total of 58,146 parks, consisting of 48,806 local parks, 4,523 state parks, 2,676 regional parks, 1,312 county parks, and 829 national parks. We acquire additional park location information from the Trust for Public Land's ParkServe database (Trust for Public Land, 2022), which includes a census of local parks, recreation, and conservation areas across the United States. The Trust for Public Land uses the U.S. Census Urban-Area definitions in determining the communities from which to acquire local park or green space data. For a site to be included in the ParkServe database, the park, recreation, or conservation area must be designated as open access in terms of its availability to the general public. We remove sites already included in the ESRI USA parks database, leaving 127,934 parks, recreation, and conservation areas in the ParkServe data, of which approximately 78% are designated as local parks and 9% are local conservation areas. Our third and final data source from which we extract park and green space information comes from The National Conservation Easement Database (NCED), which contains location information for public and private lands with conservation easements in the United States. For each site, the data include the land-use purpose (e.g., forest, farming, recreation, environmental, etc.) and its access status (closed, open, or restricted access). For consistency with the ParkServe database, we include only the easements designated as open access, implying that they are open for public use. After removing sites overlapping the ESRI USA Parks and ParkServe databases, the NCED contains 13,221 sites, of which approximately 47% of the open-access lands are managed forests, while approximately 9% are recreational. Combining the three databases yields 199,301 parks, recreation, and conservation areas in the contiguous United States.

A.4 Dollar Store Entry Events

Tables 8 and 9 contain dollar store entry information in urban areas, while Tables 10 and 11 display dollar store entry statistics for rural areas. Tables 8 and 10 contain overall entry information in urban and rural areas, respectively, while Tables 9 and 11 summarize dollar store entry in urban and rural geographies by conditioning on the number of pre-entry grocery stores in the year 2005.

A.4.1 Urban Areas

Table 8: Dollar Store Entry Events (2006-2020, Urban Areas)

Entries	Count	Share
1	106,967	0.503
2	59,351	0.279
3	30,750	0.145
> 3	15,386	0.072

There are a total of 212,454 entry events in urban areas. The share of entry events indicates the proportion of events corresponding to the entry bin (e.g., one entry, two entries, etc.).

Table 9: Urban-Area Dollar Store Entries by Supermarkets and Grocery Stores (2005)

Supermarkets and Grocery Stores (2005)	Dollar Store Entries 1	2	3	>3
0	7,548	2,187	609	158
	(0.036)	(0.010)	(0.003)	(0.001)
1	15,419	5,924	1,860	496
	(0.073)	(0.028)	(0.009)	(0.002)
2	18,949	8,936	3,277	997
	(0.089)	(0.042)	(0.015)	(0.005)
3	17,109	9,406	4,163	1,471
	(0.081)	(0.044)	(0.020)	(0.007)
4	13,074	7,818	3,940	1,721
	(0.062)	(0.037)	(0.019)	(0.008)
5	9,269	5,963	3,297	1,476
	(0.044)	(0.028)	(0.016)	(0.007)
(5,7]	10,386	7,117	4,331	2,230
	(0.049)	(0.033)	(0.020)	(0.010)
(7,10]	6,431	4,736	3,356	2,179
	(0.030)	(0.022)	(0.016)	(0.010)
>10	8,782	7,264	5,917	4,658
	(0.041)	(0.034)	(0.028)	(0.022)

Table 9 contains the total number of dollar store entry events after the year 2005 conditioned on the number of year 2005 supermarket and grocery stores within a two-mile radius of population-weighted block-group centroids. Below the total number of unique entry events, we compute the share of events corresponding to the dollar store entry event and supermarket/grocery store count combination. Based on the shares, the most frequent entry-event occurs with one to two dollar store entries, especially in block groups with one to three initial, year 2005 supermarkets or grocery stores.

A.4.2 Rural Areas

Table 10: Dollar Store Entry Events (2006-2020, Rural Areas)

Entries	Count	Share
1	39,914	0.39
2	27,154	0.266
3	17,370	0.17
4	10,929	0.107
> 4	6,887	0.067

There are a total of 102,254 entry events in rural areas. The share of entry events indicates the proportion of events corresponding to the entry bin (e.g., one entry, two entries, etc.).

Table 11: Rural-Area Dollar Store Entries by Supermarkets and Grocery Stores (2005)

Supermarkets and Grocery Stores (2005)	Dollar Store Entries 1	2	3	4	>4
0	4,901	1,560	460	154	45
	(0.048)	(0.015)	(0.004)	(0.002)	(0)
1	6,348	2,832	1,044	340	100
	(0.062)	(0.028)	(0.01)	(0.003)	(0.001)
2	5,800	3,488	1,559	578	203
	(0.057)	(0.034)	(0.015)	(0.006)	(0.002)
3	4,787	3,413	1,767	795	300
	(0.047)	(0.033)	(0.017)	(0.008)	(0.003)
4	3,676	2,845	1,758	899	373
	(0.036)	(0.028)	(0.017)	(0.009)	(0.004)
5	2,877	2,369	1,649	909	444
	(0.028)	(0.023)	(0.016)	(0.009)	(0.004)
6	1,978	1,723	1,318	843	486
	(0.019)	(0.017)	(0.013)	(0.008)	(0.005)
(6,9]	3,982	3,607	2,965	2,164	1,420
	(0.039)	(0.035)	(0.029)	(0.021)	(0.014)
(9,13]	2,700	2,526	2,255	1,899	1,447
	(0.026)	(0.025)	(0.022)	(0.019)	(0.014)
> 13	2,865	2,791	2,595	2,348	2,069
	(0.028)	(0.027)	(0.025)	(0.023)	(0.02)

Table 11 contains the total number of rural-area dollar store entry events after the year 2005 conditioned on the number of year 2005 supermarket and grocery stores within a ten-mile drive of population-weighted block-group centroids. Below the total number of unique entry events, we compute the share of events corresponding to the dollar store entry event and supermarket/grocery store count combination.

A.5 Predictor Summary Statistics

A.5.1 Urban Areas

Table 12: Predictor Summary Statistics by Treatment Status (Urban Areas)

		Never-Treated		Treated	
Predictor	Category	Mean	SD	Mean	SD
Population Share Age 18-34	Socio-Demographic	0.221	0.130	0.252	0.120
Population Share Age 35-64		0.399	0.096	0.378	0.095
Population Share Age $\geq$ 65		0.157	0.114	0.141	0.098
Share Commuting with Private Vehicle		0.853	0.148	0.825	0.195
Share Commuting with Public Transportation		0.042	0.092	0.080	0.151
Share with $\geq$ Bachelor's Degree		0.388	0.222	0.263	0.197
Share with $<$ High School Diploma		0.097	0.108	0.170	0.144
Income Per Capita		4.196	9.141	2.960	10.447
Share without Private Vehicle		0.066	0.107	0.132	0.165
Population Share Black		0.078	0.152	0.195	0.278
Population Share Hispanic		0.124	0.176	0.211	0.258
Population Share Asian		0.060	0.104	0.050	0.099
Population Share White		0.786	0.209	0.643	0.294
Poverty Rate		0.108	0.123	0.180	0.158

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Table 12. Continued

Predictor	Category	Never-Treated Mean	Never-Treated SD	Treated Mean	Treated SD
Population Share on Public Assistance		0.021	0.037	0.035	0.053
Population		1.612	1.081	1.394	0.817
Share Unemployed		0.063	0.060	0.087	0.082
Share of Vacant Housing		0.085	0.104	0.103	0.104
Dollar Stores (2005)	Retail	0.625	1.047	1.639	1.732
Convenience Stores (2005)		7.059	12.309	17.494	24.001
Drug Stores (2005)		3.551	11.340	8.413	17.196
Superettes (2005)		1.364	6.474	5.558	16.943
Wholesale Club Stores (2005)		0.042	0.209	0.096	0.314
General Merchandisers (2005)		0.128	0.488	0.366	0.725
Mass Merchandisers (2005)		0.213	0.500	0.400	0.660
Distance To Urbanized Area	Economic Geography	14.292	12.270	12.092	10.475
Open Water		0.056	0.114	0.042	0.088
Developed Open Space		0.137	0.090	0.116	0.081
Developed Low Intensity		0.191	0.100	0.206	0.109
Developed Medium Intensity		0.185	0.127	0.258	0.147
Developed High Intensity		0.089	0.107	0.164	0.152

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Table 12. Continued

		Never-Treated		Treated	
Predictor	Category	Mean	SD	Mean	SD
Barren Land		0.011	0.047	0.013	0.063
Shrub Scrub		0.026	0.075	0.016	0.055
Herbaceous		0.028	0.073	0.016	0.052
Forest		0.129	0.152	0.074	0.110
Planted-Cultivated		0.082	0.132	0.051	0.109
Wetlands		0.066	0.109	0.043	0.087
Park Distance		1.553	2.026	1.169	1.663
U.S. Highway (Miles)		1.692	3.066	2.638	3.966
State Highway (Miles)		3.173	3.623	4.491	4.491
County Highway (Miles)		0.047	0.398	0.039	0.377
Other Highway (Miles)		4.590	4.982	7.065	6.170
Interstate Highway (Miles)		1.172	2.426	2.127	3.401
Public Schools		3.944	6.870	9.532	16.366
Private Schools		1.118	2.750	2.310	4.506
Urbanized Area		0.862	0.345	0.892	0.310
Urban Cluster Area		0.138	0.345	0.108	0.310
State Fixed Effects	Fixed Effects	-0.029	0.041	-0.022	0.041

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Table 12. Continued

		Never-Treated		Treated	
Predictor	Category	Mean	SD	Mean	SD
Year Fixed Effects		0.217	0.029	0.217	0.029
Year-x-State Fixed Effects		-0.018	0.024	-0.017	0.024

Table 12 contains summary statistics for the never-treated and treated block groups in urban areas, where treatment is defined by the block group experiencing at least one dollar store entry from 2006 to 2020.

A.5.2 Rural Areas

Table 13: Predictor Summary Statistics by Treatment Status (Rural Areas)

		Never-Treated		Treated	
Predictor	Category	Mean	SD	Mean	SD
Population Share Age 18-34	Socio-Demographic	0.169	0.072	0.182	0.071
Population Share Age 35-64		0.419	0.080	0.418	0.078
Population Share Age $\geq$ 65		0.194	0.091	0.174	0.085
Share Commuting with Private Vehicle		0.872	0.106	0.916	0.076
Share Commuting with Public Transportation		0.006	0.024	0.005	0.019
Share with $\geq$ Bachelor's Degree		0.208	0.138	0.203	0.135
Share with $<$ High School Diploma		0.135	0.102	0.144	0.103
Income Per Capita		3.138	9.570	3.032	17.821
Share without Private Vehicle		0.044	0.054	0.046	0.057
Population Share Black		0.036	0.118	0.070	0.153
Population Share Hispanic		0.060	0.121	0.059	0.124
Population Share Asian		0.006	0.017	0.008	0.026
Population Share White		0.885	0.190	0.875	0.174
Poverty Rate		0.135	0.107	0.129	0.105
Population Share on Public Assistance		0.023	0.034	0.021	0.030

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Table 13. Continued

Predictor	Category	Never-Treated Mean	Never-Treated SD	Treated Mean	Treated SD
Population		1.127	0.724	1.423	0.826
Share Unemployed		0.065	0.065	0.067	0.060
Share of Vacant Housing		0.226	0.179	0.144	0.130
Dollar Stores (2005)	Retail	0.654	1.107	3.184	3.849
Convenience Stores (2005)		4.571	5.430	20.117	22.125
Drug Stores (2005)		1.100	1.853	5.200	7.277
Superettes (2005)		0.400	0.794	1.224	3.432
Wholesale Club Stores (2005)		0.008	0.091	0.129	0.407
General Merchandisers (2005)		0.029	0.180	0.340	0.696
Mass Merchandisers (2005)		0.157	0.472	0.690	1.159
Distance To Urbanized Area	Economic Geography	20.833	16.136	12.569	8.925
Open Water		0.038	0.091	0.036	0.071
Developed Open Space		0.041	0.066	0.060	0.048
Developed Low Intensity		0.018	0.018	0.039	0.033
Developed Medium Intensity		0.008	0.012	0.020	0.025
Developed High Intensity		0.002	0.006	0.008	0.013
Barren Land		0.003	0.018	0.003	0.010

Continued on next page

Table 13. Continued

Predictor	Category	Never-Treated Mean	Never-Treated SD	Treated Mean	Treated SD
Shrub Scrub		0.085	0.171	0.039	0.104
Herbaceous		0.102	0.178	0.048	0.111
Forest		0.318	0.277	0.341	0.234
Planted-Cultivated		0.293	0.290	0.302	0.242
Wetlands		0.089	0.160	0.103	0.145
Park Distance		15.150	12.224	9.196	7.630
U.S. Highway (Miles)		12.060	16.000	24.246	23.733
State Highway (Miles)		23.572	20.800	46.942	31.502
County Highway (Miles)		1.134	5.231	1.353	6.122
Other Highway (Miles)		16.063	20.900	39.784	37.370
Interstate Highway (Miles)		4.286	10.500	11.491	17.330
Public Schools		3.162	3.960	10.749	13.032
Private Schools		0.496	1.624	2.038	4.184
State Fixed Effects	Fixed Effects	-0.026	0.044	-0.011	0.035
Year Fixed Effects		0.217	0.029	0.217	0.029
Year-x-State Fixed Effects		-0.014	0.027	-0.013	0.025

Table 13 contains summary statistics for the never-treated and treated block groups in rural areas, where treatment is defined by the block group experiencing at least one dollar store entry from 2006 to 2020.

B Model Assumptions

The identifying assumptions to estimate the causal effects of dollar store entry on block-group low-access status are outlined in this section. These are the same identifying assumptions developed by (Souza, 2019). We make only minor changes to the notation used by Souza (2019) in some places.

Assumption 1 (Random Sampling)**.**

$$\{y_{i,1}, y_{i,2}, \ldots, y_{i,T}, \mathbf{X}_{i,1}, \mathbf{X}_{i,2}, \ldots, \mathbf{X}_{i,T}, D_{i,1}, D_{i,2}, \ldots, D_{i,T}\}_{i=1}^{I} \tag{Ass. 1}$$

is independent and identically distributed (i.i.d.). This implies that we have access to panel data.

Assumption 2 (No Anticipatory Effects)**.**

$$y_{i,t} = y_{i,t}(0), \text{ for all } t < e_i \tag{Ass. 2}$$

where $t < e_i$ indicates pre-treatment periods only. Assumption 2 says that actual block-group i's low-access status in pre-treatment period t is equal to its potential outcome. Assumption 2 implies that supermarkets do not exit markets, while consumers do not alter their shopping behaviour in anticipation of dollar store entry.

Assumption 3 (Covariates are independent of dollar store entry)**.**

$$\mathbf{X}_{i,t} = \mathbf{X}_{i,t}(0) = \mathbf{X}_{i,t}(1), \text{ for all } t \tag{Ass. 3}$$

Assumption 3 indicates that covariates related to demographics, socioeconomics, housing, and market geography are exogenous and are not outcomes of dollar store entry. The small-box format of dollar stores suggests that their entry or anticipated entry is unlikely to systematically impact income, unemployment, housing conditions, land use, or neighborhood amenities. Dollar stores are not drivers of population or the composition of neighborhood demographics over time.

The relationship between untreated potential outcomes, $y_{i,t}(0)$, and the covariates $\mathbf{X}_{i,t}$ is:

$$y_{i,t}(0) = g\left(\mathbf{X}_{i,t}(0)\right) + \varepsilon_{i,t}, \quad \text{for all } t < e_i$$
$$\text{such that } \mathbb{E}\left[y_{i,t}(0) \mid \mathbf{X}_{i,t}, D_{i,t} = 0\right] = g\left(\mathbf{X}_{i,t}(0)\right).$$

The untreated counterfactual low-access potential outcome in post-treatment periods is:

$$y_{i,t}(0) = f\left(\mathbf{X}_{i,t}(0)\right) + \varepsilon_{i,t}, \quad \text{for all } t \geq e_i \text{ such that}$$
$$\mathbb{E}\left[y_{i,t}(0) \mid \mathbf{X}_{i,t}, D_{i,t} = 1\right] = f\left(\mathbf{X}_{i,t}(0)\right)$$

where $\varepsilon_{i,t}$ is the idiosyncratic error term. Because the equations for potential outcomes in treated and untreated states imply different functional forms, $g()$ and $f()$, the fourth assumption required for valid causal inference is:

Assumption 4 (Stability of counterfactual functions)**.**

$$f\left(\mathbf{X}_{i,t}(0)\right) = \mathbb{E}\left[g\left(\mathbf{X}_{i,t}(0)\right) \mid \mathbf{X}_{i,t}, D_{i,t} = 1\right] \text{ such that } \mathbb{E}\left[y_{i,t}(0) - g\left(\mathbf{X}_{i,t}(0)\right) \mid \mathbf{X}_{i,t}, D_{i,t} = 1\right] = 0 \tag{Ass. 4}$$

Assumption 4 indicates that the expected value of untreated potential outcomes, conditional on covariates $\mathbf{X}_{i,t}$ and treatment, $D_{i,t} = 1$, is equivalent to the pre-treatment function, evaluated using covariates from post-treatment periods. Assumption 4 implies that the machine learning models estimated in step one using pre-treatment data only can be used to predict counterfactual low-access outcomes for the treated block groups in post-treatment periods. When these assumptions hold, the ATT can be identified (Souza, 2019).

C Machine Learning Algorithm

Boosting is an ensemble machine learning method that combines a series of base learners (i.e., models slightly better than random guessing) in a stage-wise fashion (Ferreira & Figueiredo, 2012; Freund, Schapire, & Abe, 1999; Sagi & Rokach, 2018). In gradient boosting decision trees, the base learners (i.e., regression trees) are successively fit to the residuals generated from the previous base model predictions, where predictions are formed with the objective of minimizing a loss function, $\sum_{i=1}^{N} L\left(y_i, g\left(x_i\right)\right)$ (J. Friedman, Hastie, & Tibshirani, 2000; J. H. Friedman, 2001, 2002; Natekin & Knoll, 2013; Ridgeway, 2007). Common loss functions include the squared-error loss in the case of regression and log loss in the case of binary classification (Hastie et al., 2009; Natekin & Knoll, 2013). Whereas random forest combines multiple full-sized, independent regression trees to make predictions, reducing the model variance, boosting reduces model bias by successively adding simple regression trees to increase model accuracy, whereby each regression tree depends on previous predictions (Sagi & Rokach, 2018; Sutton, 2005; Yeturu, 2020). Each boosted model is multiplied by a small constant, $\eta = (0, 1]$ to shrink the contribution of each decision tree and control the model learning rate (Elith, Leathwick, & Hastie, 2008). Smaller values of η (e.g., 0.01) imply that a larger number of boosted iterations, B, are required to achieve strong prediction accuracy. However, higher B increases model complexity, which can lower the predictive performance on test data (Hastie et al., 2009). Therefore, in practice, optimal values of η and B are found via cross-validation. As in random forest, the predictive accuracy of gradient boosting can be further enhanced by adding randomness to each boosted iteration (J. H. Friedman, 2002). For each regression tree, a random sample, without replacement, of the training data is selected.

We implement the XGBoost version of gradient boosting (Chen & Guestrin, 2016), which is optimized for computational speed and includes an additional regularization term in the objective function to moderate tree complexity. XGBoost has multiple tuning parameters and allows for extensive customization to enahnce model training (Mienye & Sun, 2022). We use the xgboost package in R (Chen & Guestrin, 2016).

Machine learning algorithms contain hyperparameters that must be specified for estimation. An optimal combination of hyperparameters for a given machine learning model can increase the predictive accuracy (Lavesson & Davidsson, 2006). Hyperparameters also determine the complexity of the fitted model. More complex models reduce bias but tend to increase the variability of out-of-sample predictions. Less complex models, on the other hand, have greater bias but less variability in out-of-sample predictions (Hastie et al., 2009). Cross-validation is typically used to determine the set of hyperparameter values that increase predictive accuracy most for a given learning task, a process referred to as tuning (Lavesson & Davidsson, 2006). For many machine learning algorithms, tuning only a small subset of the total set of available hyperparameters tends to increase predictive power significantly. Default values are often recommended for the remaining hyperparameters (Van Rijn & Hutter, 2018; Weerts, Mueller, & Vanschoren, 2020).

Using the 7-fold blocked-CV scheme described in Section 4.1, we tune several XGBoost hyperparameters that impact the size, contribution, and number of boosted trees. To guide our tuning strategy and selection of other XGBoost parameter settings, we reference the recommended hyperparameter values from several machine learning model-comparison studies that include boosting (Bentéjac, Csörgő, & Martínez-Muñoz, 2021; Probst, Boulesteix, & Bischl, 2019; Van Rijn & Hutter, 2018).

The learning rate, *eta* (η), which determines the contribution of each boosted tree and *max_depth*, which controls the flexibility and the complexity of the regression trees in each boosting iteration are two of the most important hyperparameters in boosting algorithms that can be tuned to achieve higher prediction accuracy (Bentéjac et al., 2021; Probst et al., 2019; Van Rijn & Hutter, 2018; C. Zhang, Liu, Zhang, & Almpanidis, 2017). The learning rate and *max_depth* are inversely related whereby more complex trees require smaller learning rates, and both impact the optimal number of regression trees required to achieve good predictive performance (Elith et al., 2008). In turn, we tune each predictive model to find the optimal learning rate, *eta* (η), number of boosting iterations, *nrounds*, and depth of the regression trees in each boosting iteration, *max_depth*. While large values of *max_depth* allow for flexible interaction effects between covariates and the outcome, over-complex trees can lead to over-fitting in out-of-sample predictions. To prevent over-fitting, we specify *gamma* (γ), a regularization term in the XGBoost optimization function that controls tree complexity. Higher values of γ reduce model complexity by requiring that the reduction in model error from internal node splits is larger, whereas smaller values of γ allow for additional splits from small improvements in model performance.

XGBoost contains several hyperparameters that control the level of randomization during tree growing (Chen & Guestrin, 2016), including *subsample*, *colsample_bytree*, and *colsample_bylevel*. We use specific values for *subsample* and *colsample_bytree* based on the optimal default settings found in the literature (Bentéjac et al., 2021; Probst et al., 2019). We set *subsample* $= 0.8$, which controls the fraction of randomly selected (without replacement) training data observations to be used on each boosting iteration and *colsample_bytree* $= 0.75$, which determines the fraction of predictors randomly selected for each boosted tree. We tune *colsample_bylevel*, which determines the fraction of randomly chosen split-candidate variables at each tree level. An advantage of XGBoost is its ability to handle classification prediction tasks in which the outcome variable is imbalanced across groups. In our data, approximately 0.067 (6.7%) and 0.12 (12%) of treated block groups are low-access in urban and rural areas, respectively. To improve model predictive accuracy for imbalanced data, we set *scale_pos_weight* parameter $\frac{\sum_i^N \mathbb{1}[y_{it}=0]}{\sum_i^N \mathbb{1}[y_{it}=1]}$ (XGBoost Developers, 2022).

To find an optimal set of hyperparameter values, we utilize a Bayesian optimization algorithm, which uses a Gaussian process as a prior probability distribution and a pre-specified acquisition function to find candidate hyperparameter combinations that minimize the cross-validation error (Frazier, 2018; Snoek, Larochelle, & Adams, 2012; Wilson, 2021) . The advantage of the latter approach is that it allows us to more efficiently explore a wide range of hyperparameter combinations relative to a comprehensive grid search (Wu et al., 2019).

We perform 7-fold blocked-CV using different combinations of hyperparameters, selected based on the Bayesian optimization procedure. We choose the hyperparameter set that achieves the lowest cross-validated classification error. Using these optimal hyperparameters, we estimate a final set of cross-validated models and an optimized model trained on the full set of pre-treatment data, which is employed to predict counterfactual low-access status for the treated block groups.

D Summarizing the ATTs

The optimal ML model applied to the post-treatment data imputes counterfactual low-access outcomes, $y_{it}(0)$, for each block-group by post-treatment year. Subtracting the predicted counterfactual outcome from the actual low-access outcomes gives us the estimated treatment effect for block-group i at time t, denoted as $\hat{\tau}_{it}$.

$$\hat{\tau}_{it} = y_{it} - \hat{y}_{it}(0), \text{ for all } t \geq e_i \tag{7}$$

The ATT is estimated by averaging over the $\hat{\tau}_{it}$ using the post-treatment observations.

$$\widehat{ATT} = \frac{\sum_{i=1}^{I} \sum_{t=1}^{T} \hat{\tau}_{i,t}}{\sum_{i=1}^{I} (T - (e_i - 1))}, \text{ for all } t \geq e_i \tag{8}$$

The counterfactual share of low-access block groups in the post-treatment period is calculated using the imputed, counterfactual low-access status, in the absence of dollar store entry.

$$\bar{y}^{cf} = \frac{\sum_{i=1}^{I} \sum_{t=1}^{T} \hat{y}_{it}(0)}{\sum_{i=1}^{I} \left(T - (e_i - 1)\right)}, \text{ for all } t \geq e_i \tag{9}$$

Finally, combining the estimated ATT with the counterfactual share of low-access block groups, we compute the percentage change in the share of low-access block groups for the treated block groups.

$$\Delta \widehat{ATT}\,(\%) = \frac{\widehat{ATT}}{\bar{y}^{cf}} \tag{10}$$

ATTs by relative time from treatment are easily computed by conditioning each ATT on the relative time period, r.

D.1 ATTs by Region

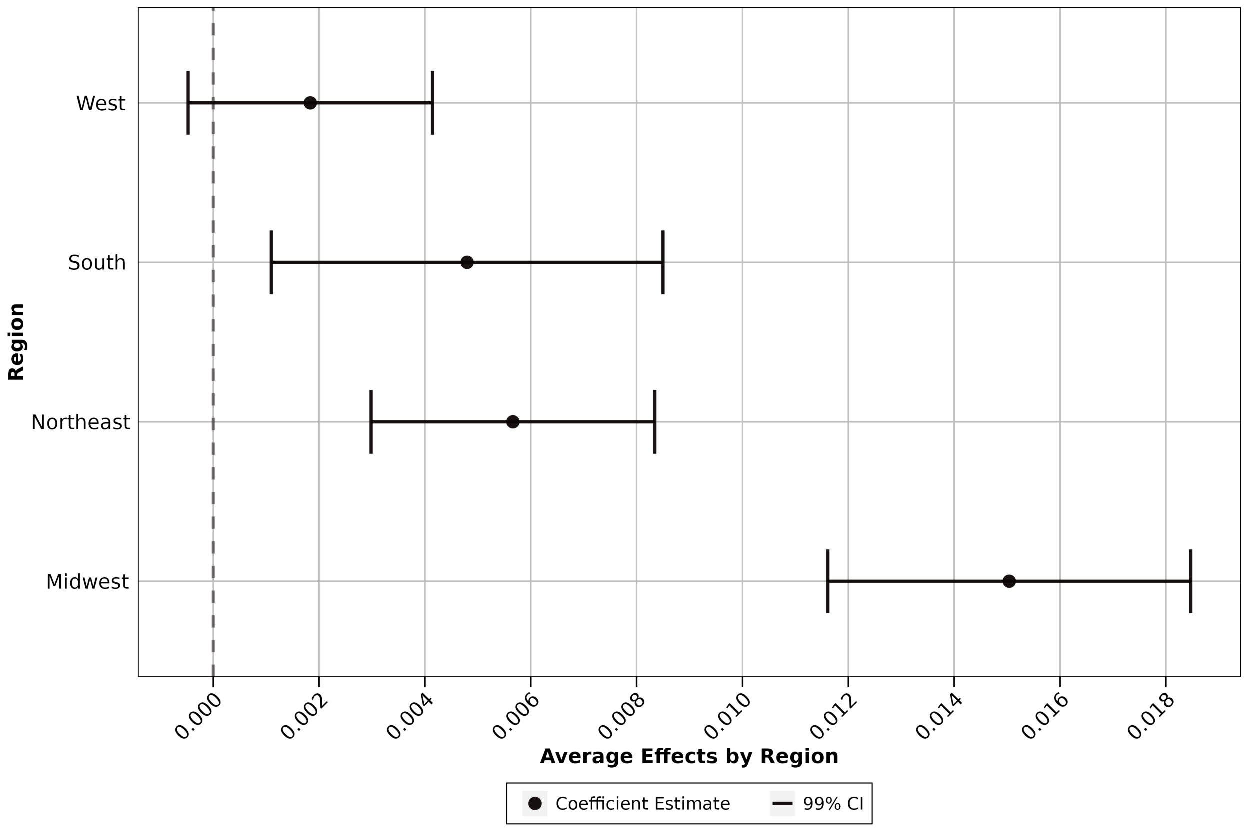


Figure 2.D: ATTs by Region in Urban Areas. Figure 2.D shows the ATTs by region in urban areas. The 99% confidence intervals are computed from bootstrapped standard errors. We find heterogeneous treatment effects across regions. In urban areas, dollar store expansion has the largest negative impact on food access in the Midwest, a region in which dollar stores originated.

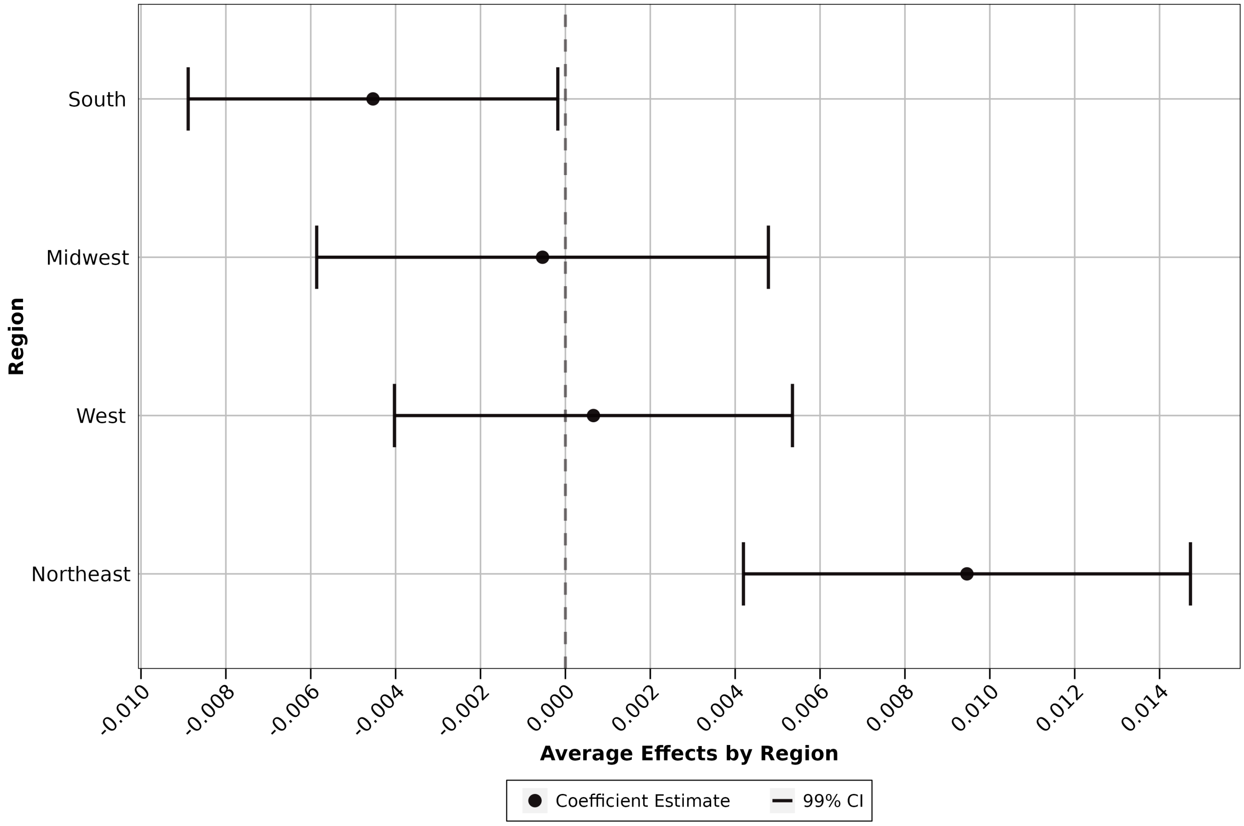


Figure 3.D: ATTs by Region in Rural Areas. Figure 3.D show the ATTs by region in rural areas. The 99% confidence intervals are computed from bootstrapped standard errors. Though the ATT is small and the 99% confidence intervals nearly contain zero, our results indicate that dollar store expansion in the Northeast has decreased food access in block groups that received at least one post-2005 dollar store.

D.2 Impact of Gross Dollar Store Densities

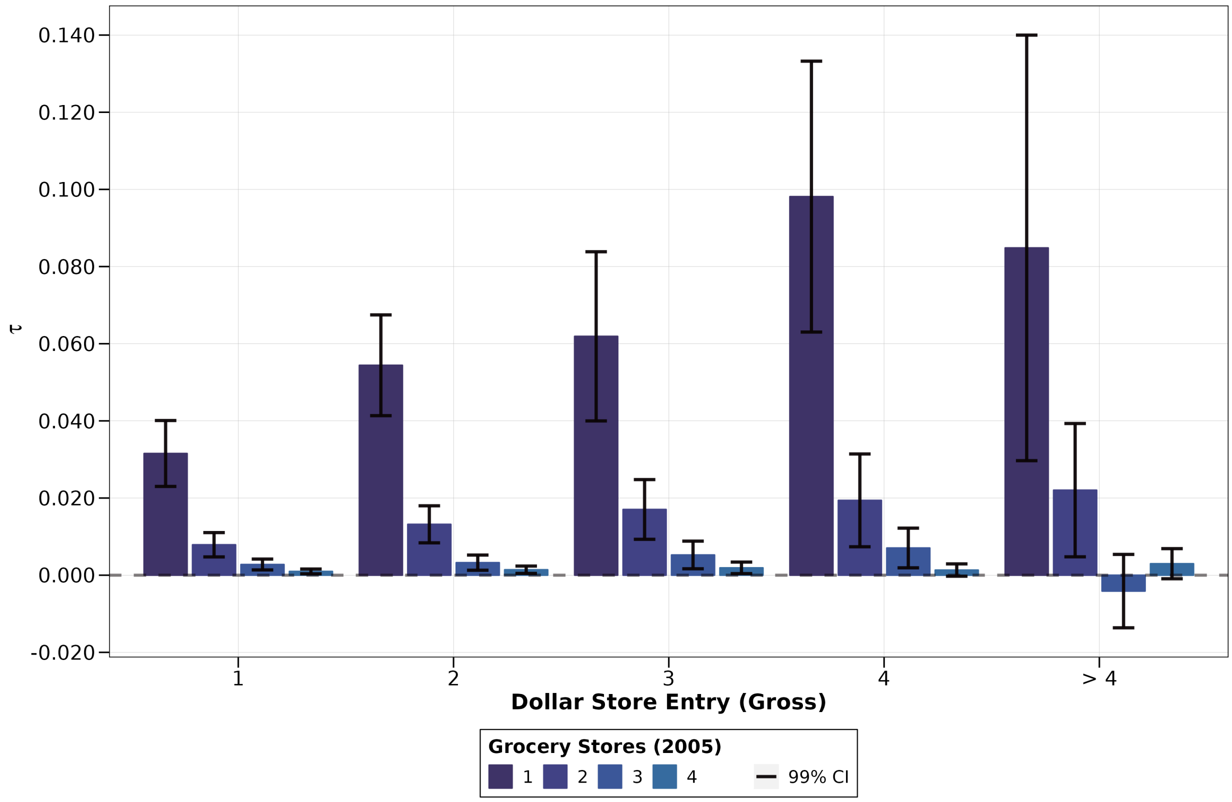


Figure 4.D: Impact of Gross Dollar Store Entries by Pre-Entry Grocery Stores in 2005 (Urban Areas). Each bar represents the point estimate from the regression in Equation 5. In the regression, we substitute dollar store entry events for gross dollar store entries. The 99% confidence intervals corresponding to each estimated coefficient are derived from the bootstrapped estimation procedure described in Section 4.2. Similar to Figure 6, the impact of gross dollar store entries on block-group low-access status is conditional on the number of initial, year 2005 grocery store levels, whereby increasing dollar store densities only affects food access in markets with one or two grocery stores.

D.3 Supplementary Rural Area Figures

The ATT in rural areas is approximately zero. We provide the rural ML model results to compare with the treatment effect heterogeneity uncovered in urban areas.

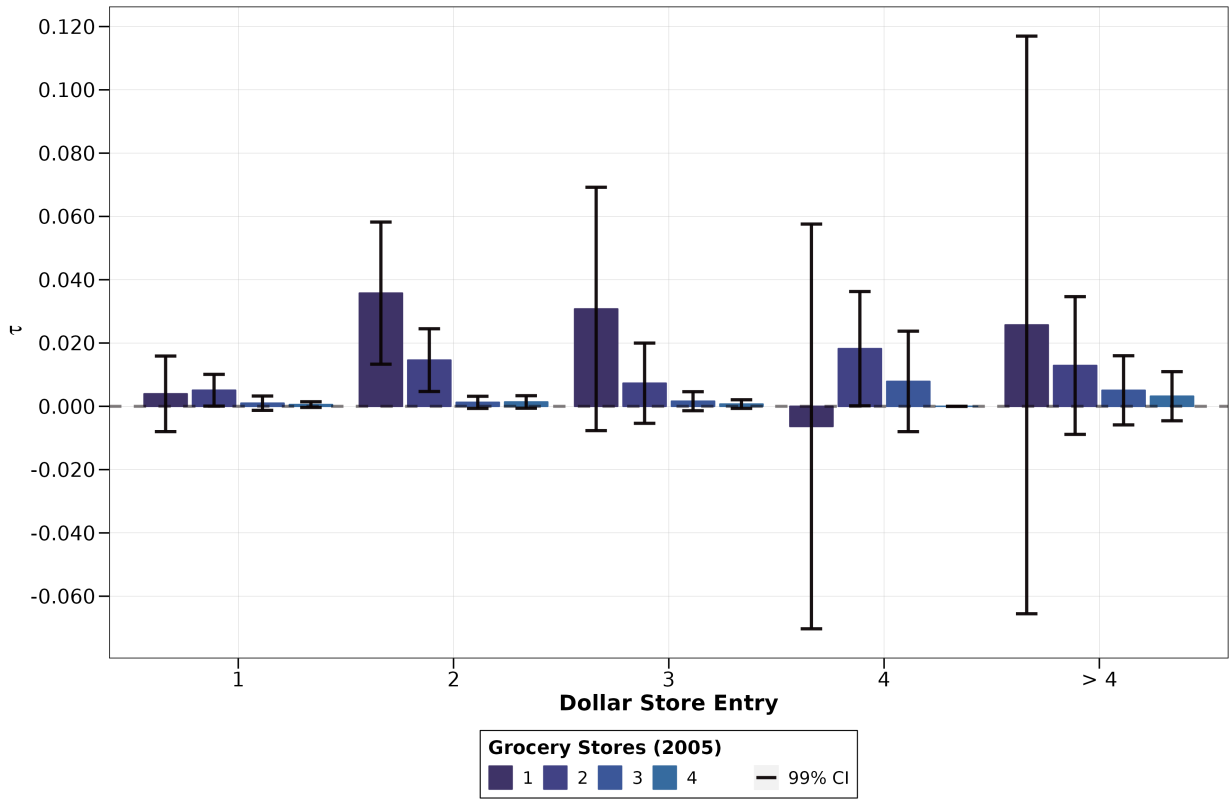


Figure 5.D: Impact of Multiple Dollar Store Entries by Pre-Entry Grocery Stores in 2005 (Rural Areas). Each bar represents the point estimate from the regression in Equation 5 using the estimated treatment effects and dollar store entries from the rural-area model. The 99% confidence intervals corresponding to each estimated coefficient are derived from the bootstrapped estimation procedure described in Section 4.2. Figure 5.D indicates that, conditional on the number of initial year 2005 supermarkets and grocery stores, additional dollar store entries have minimal impact on block-group food access within a ten-mile drive. The point estimates corresponding to the second dollar store entry, conditioned on one and two year 2005 grocery stores, are marginally statistically significant. However, all other 99% confidence intervals contain zero. In summary, within a ten-mile drive from population-weighted block-group centroids, the effects of dollar store entry on block-group low-access status are mostly statistically insignificant.

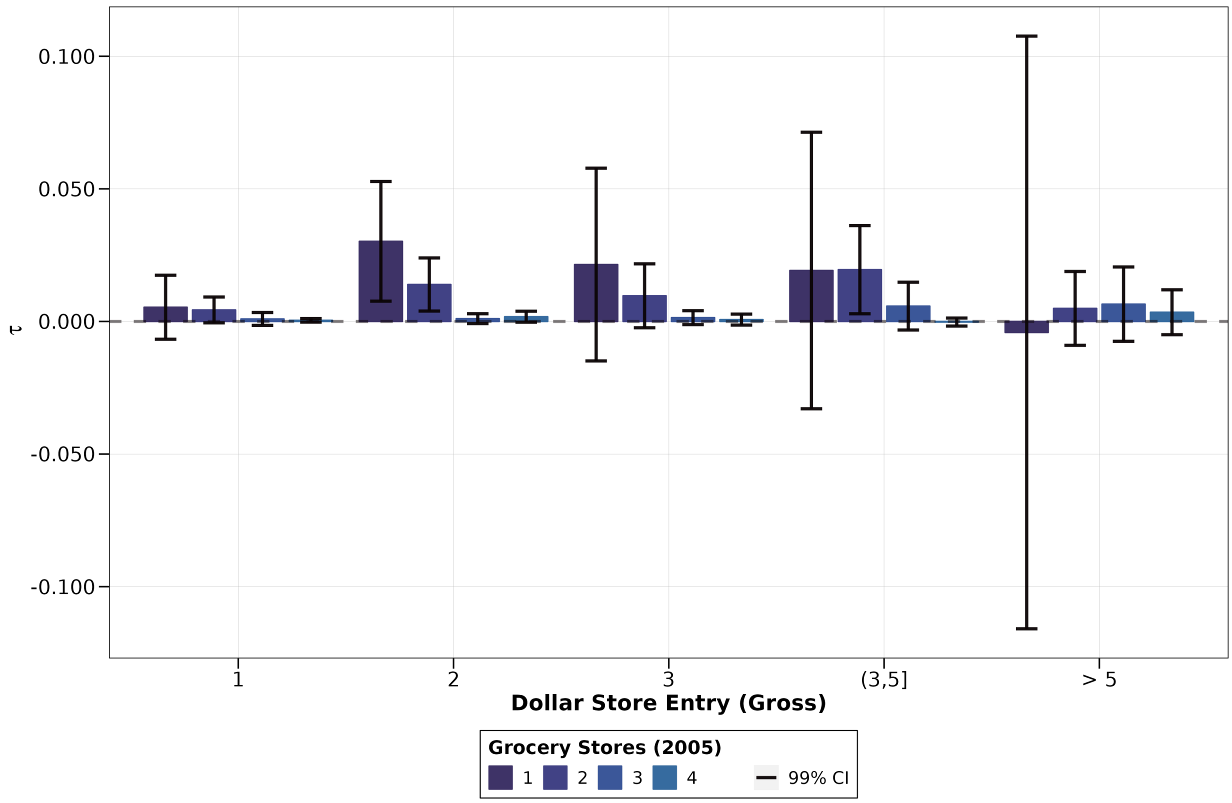


Figure 6.D: Impact of Gross Dollar Store Entries by Pre-Entry Grocery Stores in 2005 (Rural Areas). Each bar represents the point estimate from the regression in Equation 5 using the estimated treatment effects from the rural-area model. In the regression, we substitute dollar store entry events for gross dollar store entries. The 99% confidence intervals corresponding to each estimated coefficient are derived from the bootstrapped estimation procedure described in Section 4.2. Within a ten-mile drive of block-group population-weighted centroids in rural areas, we again find little evidence that dollar store densities impact food access, conditional on pre-entry grocery store counts.

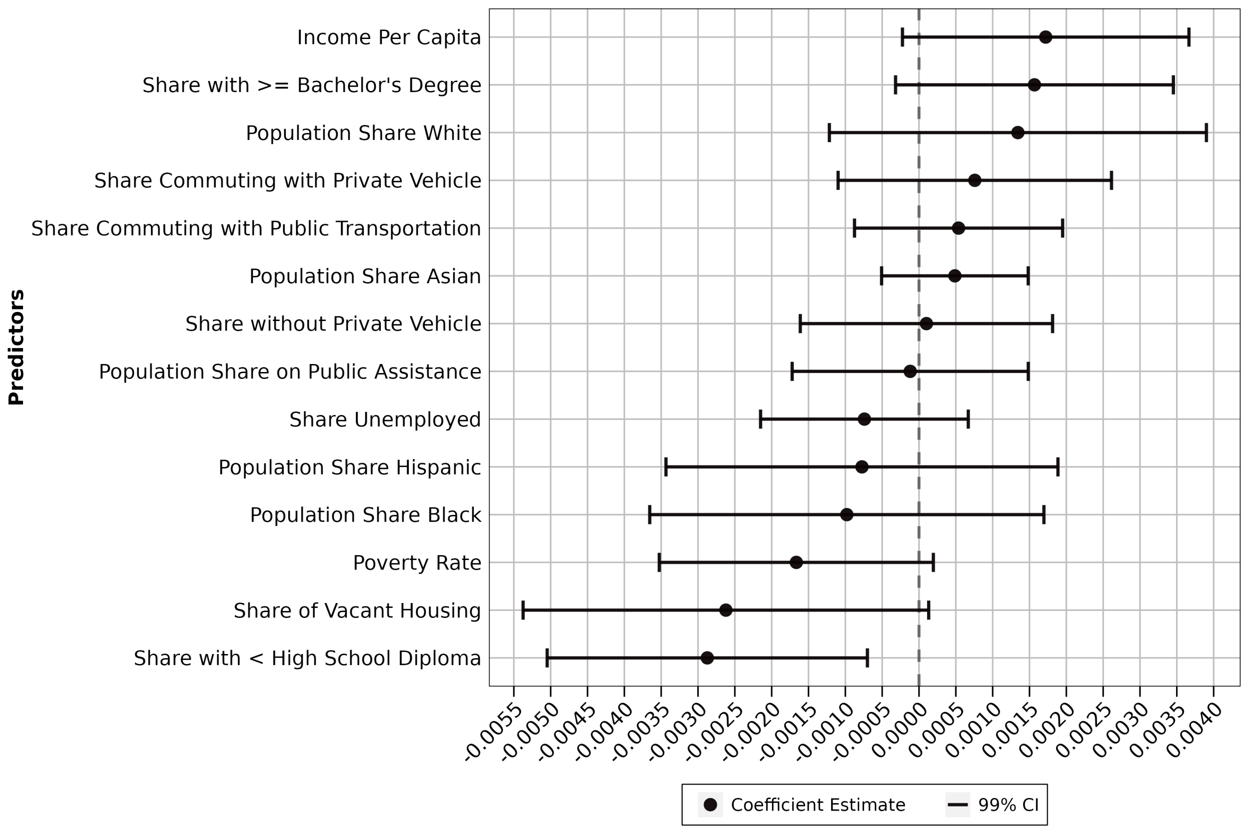


Figure 7.D: Treatment-Effect Heterogeneity by Socio-Demographic Characteristics (Rural Areas). Figure 7.D contains each standardized predictor coefficient from estimating Equation 6 and the confidence intervals constructed from bootstrapped standard errors for rural areas. With the exception of one covariate, none of the estimated coefficients from each of the bivariate regressions are statistically significantly correlated with the estimated treatment effects in rural areas. The magnitude and rank of the estimated coefficients in rural areas is mostly opposite of the magnitude and rank of predictor estimates in urban areas. The 99% confidence intervals are derived from bootstrapped standard errors.

E Cross-Validated Prediction Errors

Figures 8.E and 9.E contain the estimated average CV errors plotted against the relative time from treatment for urban (top panel) and rural areas (bottom panel).[25] In both urban and rural-area models, the average CV errors do not display a strong pattern in any direction. The average CV error point estimates and confidence intervals get smaller as yet-to-be treated block groups approach the year of dollar store entry. In the urban-area model, the average

[25]The point estimates are obtained from the regression: $\hat{\varepsilon}_{it}^{cv} = \sum_{r \leq -1} \beta_r \mathbb{1}\left[r = t - e_i\right] + u_{i,t},$ for all $t < e_i$. We compute 99% confidence intervals based on bootstrapped standard errors described in Section 4.2.

CV error point estimates are mostly small and not statistically different from zero. Figures 8.E and 9.E further alleviate concerns that violations of the anticipatory effects or conditional parallel trends assumptions bias the estimated treatment effects.

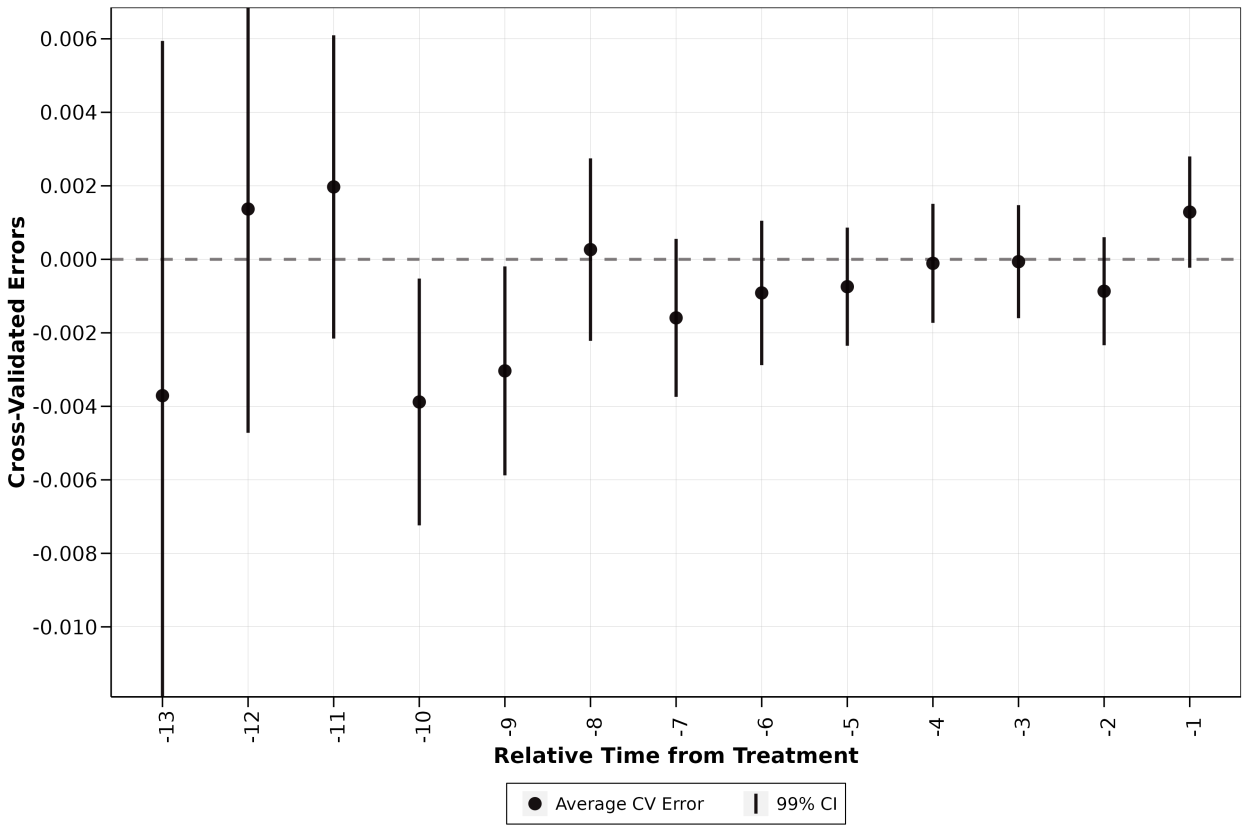


Figure 8.E: Average CV Errors by Time from Treatment in Urban Areas. Figure 8.E shows the cross-validated prediction errors against the relative time from treatment in urban areas. Figure 8.E indicates that the CV errors are small and not statistically different from zero. The line-range associated with each point estimate represents 99% confidence intervals based on bootstrapped standard errors.

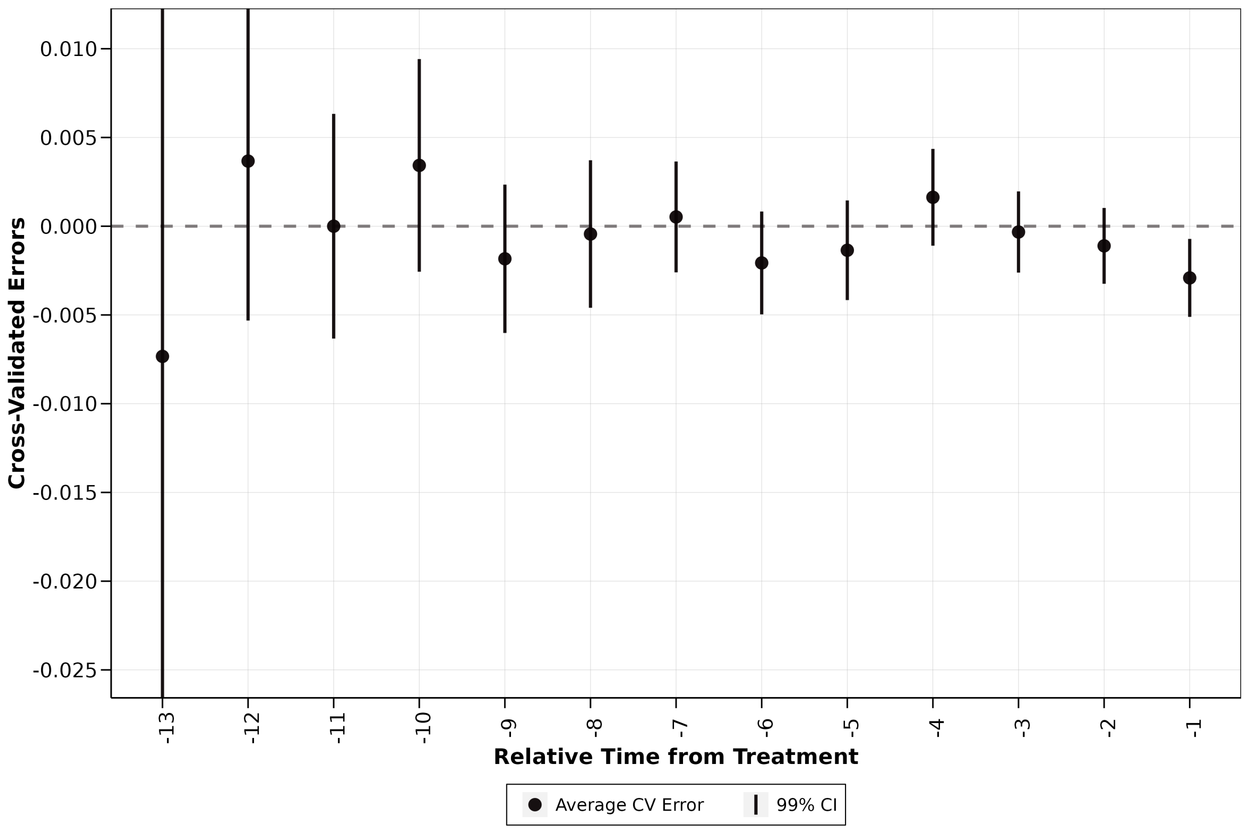
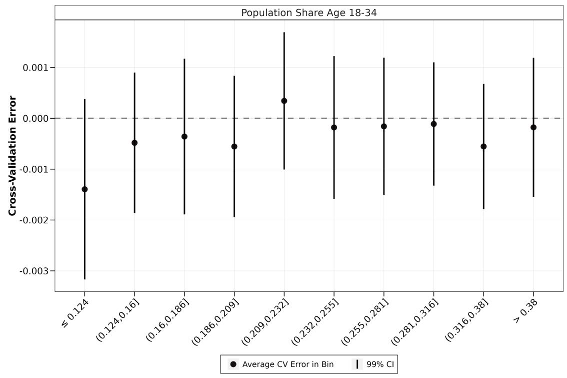
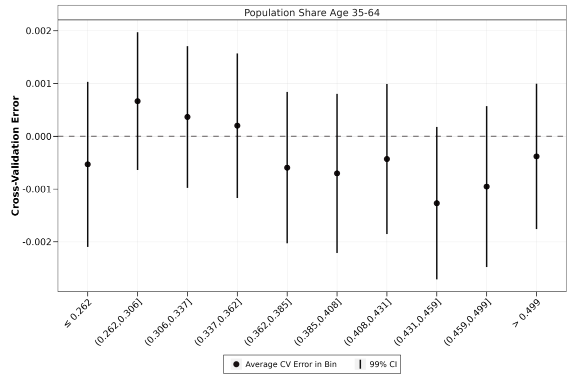
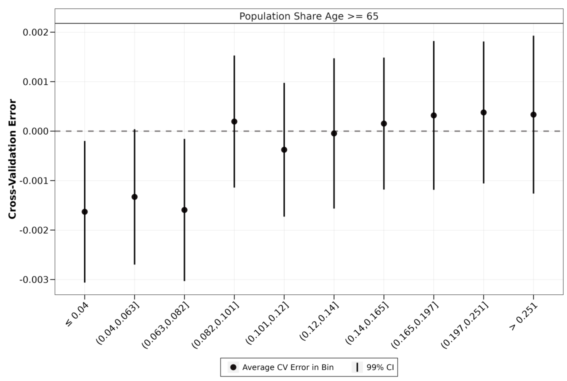


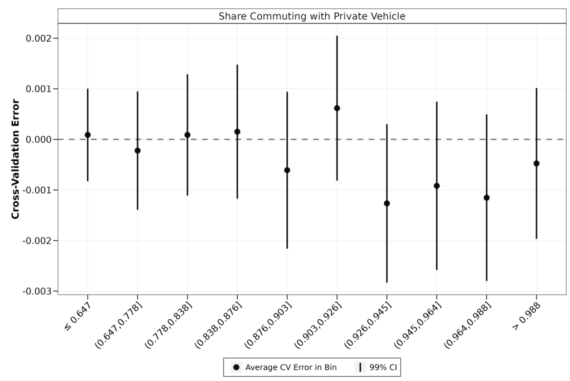
Figure 9.E: Average CV Errors by Time from Treatment in Rural Areas. Figure 9.E plots the cross-validated prediction errors against the relative time from treatment in urban areas. Figure 9.E indicates that the CV errors are small and not statistically different from zero. The line-range associated with each point estimate represents 99% confidence intervals based on bootstrapped standard errors.

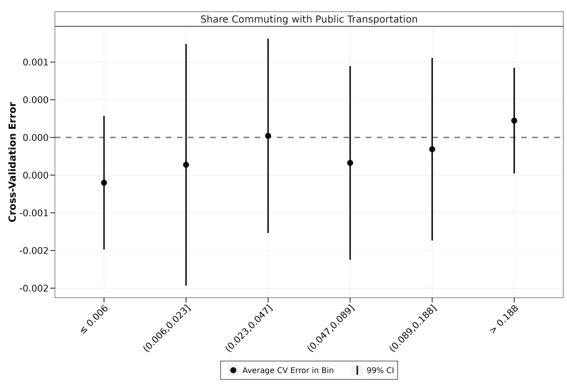
E.1 Urban Areas

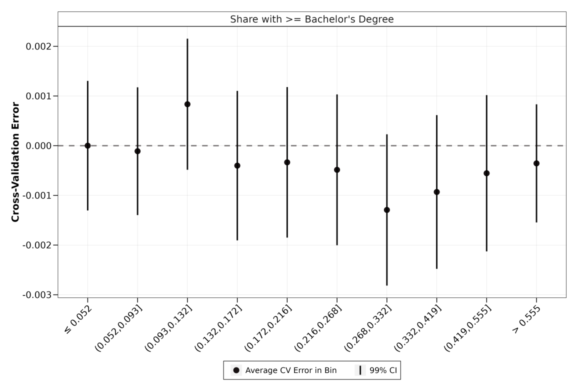


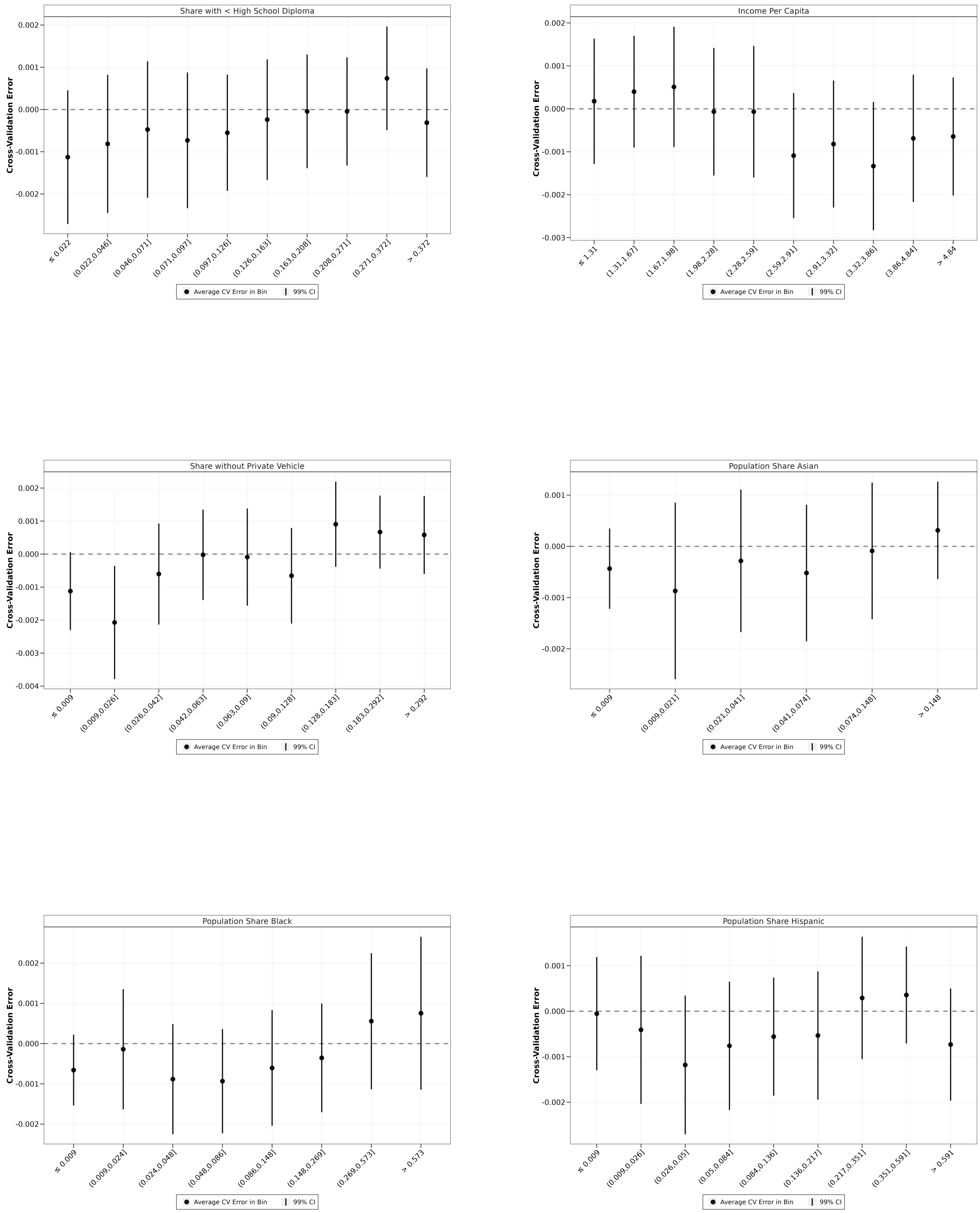











Share with < High School Diploma
Cross-Validation Error
≤ 0.022
(0.022,0.046]
(0.046,0.071]
(0.071,0.097]
(0.097,0.126]
(0.126,0.163]
(0.163,0.208]
(0.208,0.271]
(0.271,0.372]
> 0.372
Average CV Error in Bin
99% CI
Income Per Capita
Cross-Validation Error
≤ 1.31
(1.31,1.67]
(1.67,1.98]
(1.98,2.28]
(2.28,2.59]
(2.59,2.91]
(2.91,3.32]
(3.32,3.86]
(3.86,4.84]
> 4.84
Average CV Error in Bin
99% CI
Share without Private Vehicle
Cross-Validation Error
≤ 0.009
(0.009,0.026]
(0.026,0.042]
(0.042,0.063]
(0.063,0.09]
(0.09,0.128]
(0.128,0.183]
(0.183,0.292]
> 0.292
Average CV Error in Bin
99% CI
Population Share Asian
Cross-Validation Error
≤ 0.009
(0.009,0.021]
(0.021,0.041]
(0.041,0.074]
(0.074,0.148]
> 0.148
Average CV Error in Bin
99% CI
Population Share Black
Cross-Validation Error
≤ 0.009
(0.009,0.024]
(0.024,0.048]
(0.048,0.086]
(0.086,0.148]
(0.148,0.269]
(0.269,0.573]
> 0.573
Average CV Error in Bin
99% CI
Population Share Hispanic
Cross-Validation Error
≤ 0.009
(0.009,0.026]
(0.026,0.05]
(0.05,0.084]
(0.084,0.136]
(0.136,0.217]
(0.217,0.351]
(0.351,0.591]
> 0.591
Average CV Error in Bin
99% CI

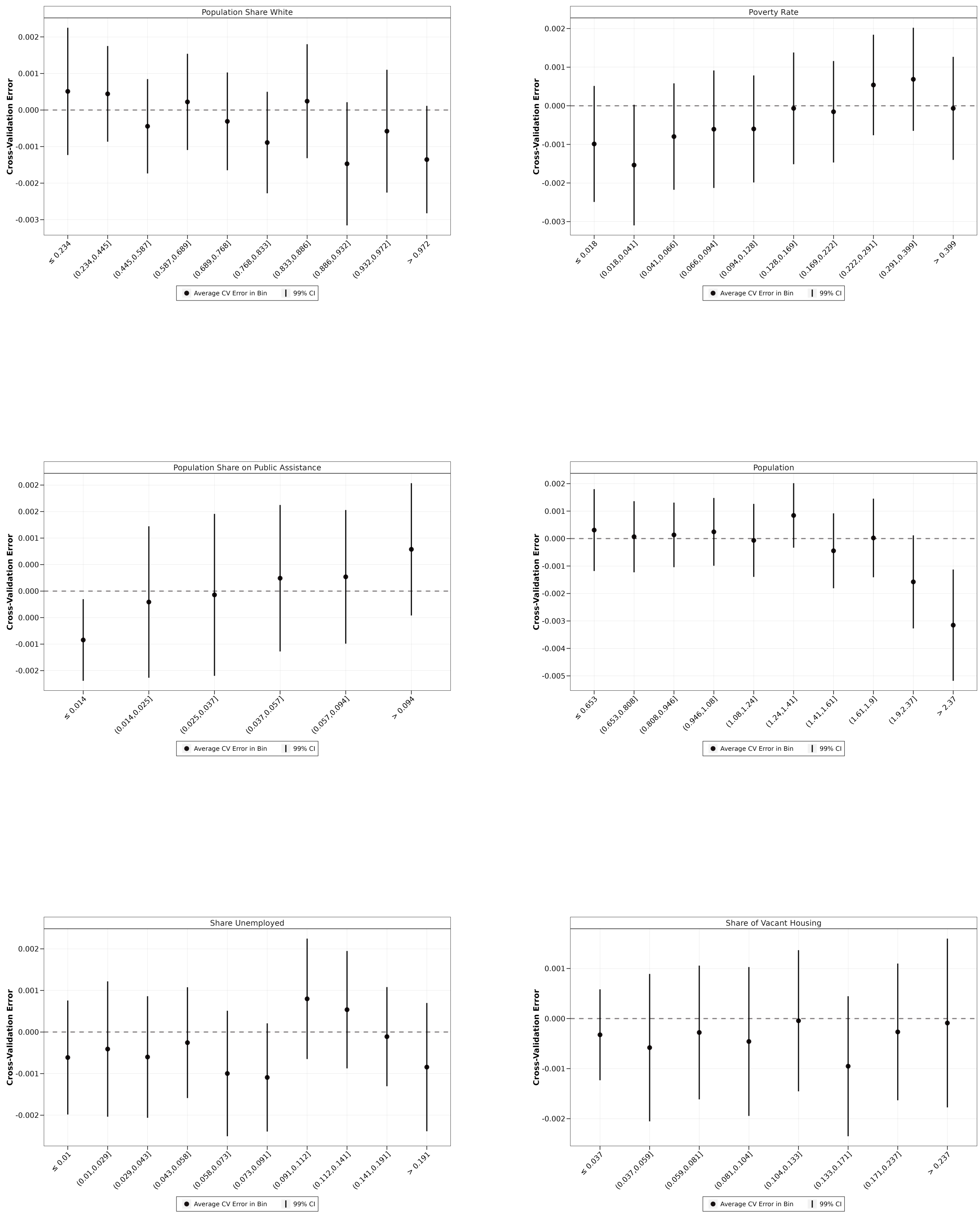
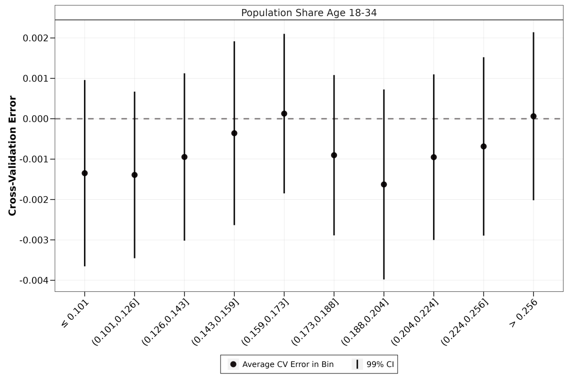
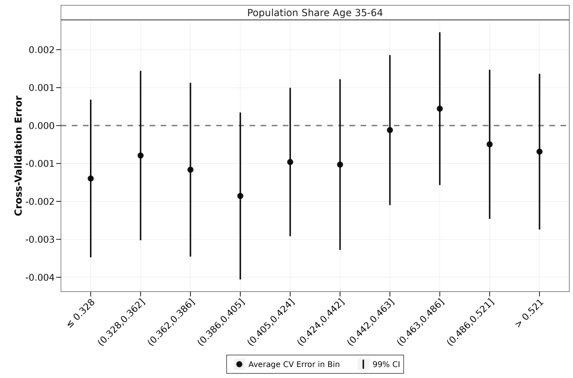
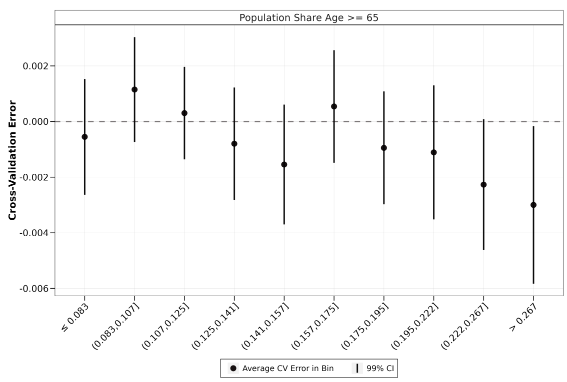


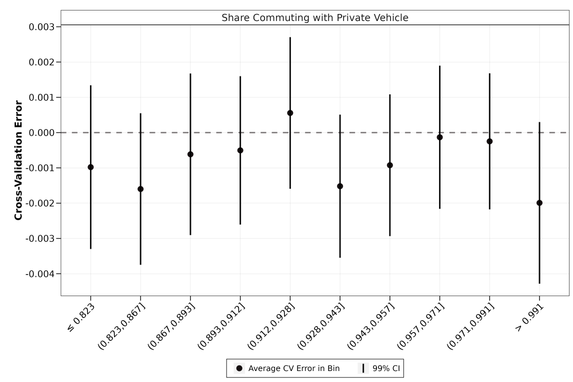
Figure 10.E: Urban-Area Cross-Validation Errors by Binned Model Covariate. Each panel contains the estimated average CV errors with respect to the binned covariate. The line-range associated with each point estimate represents 99% confidence intervals based on bootstrapped standard errors.

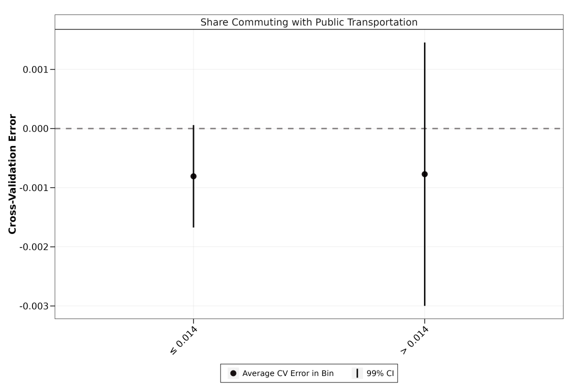
E.2 Rural Areas

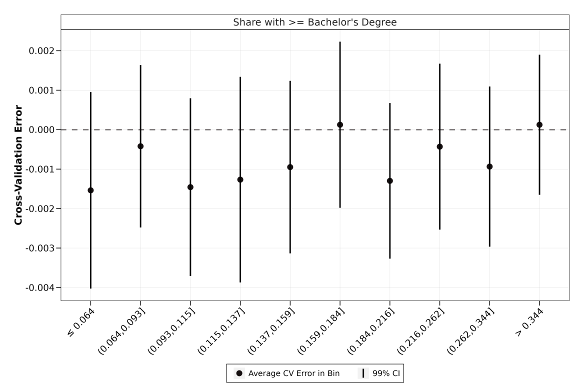


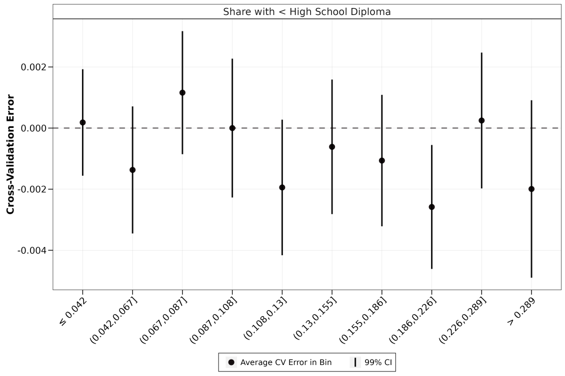


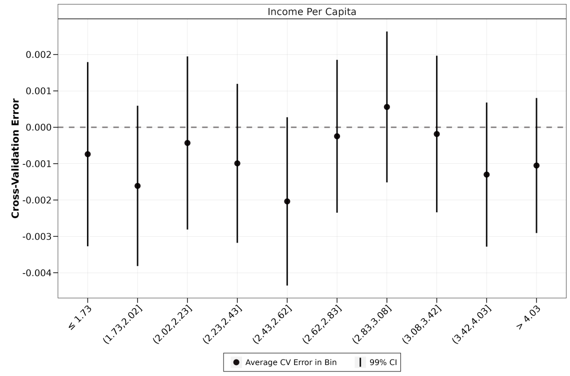


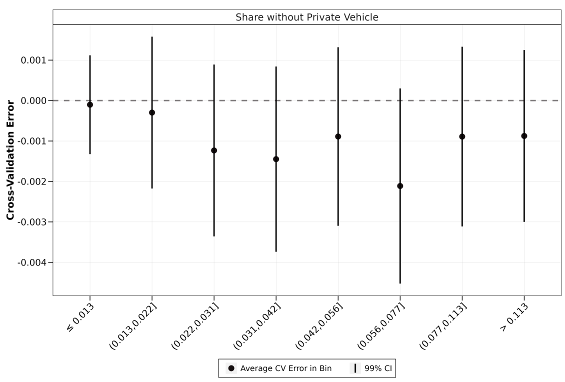


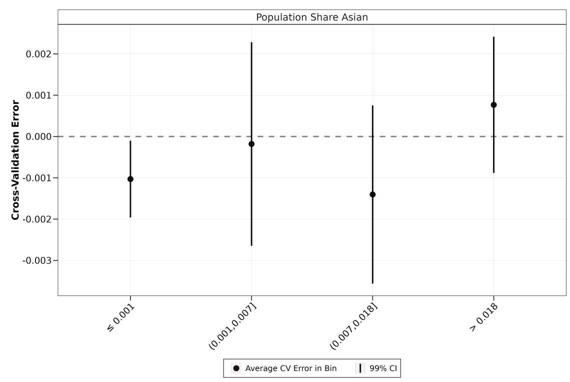


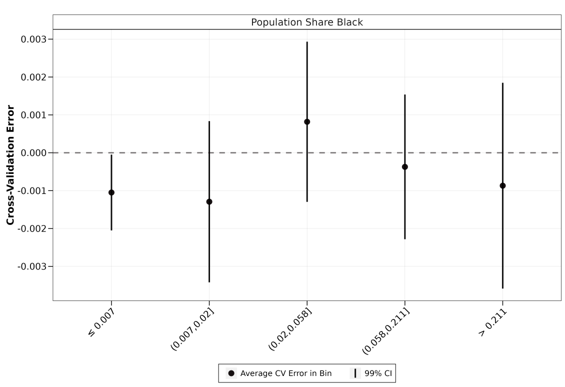


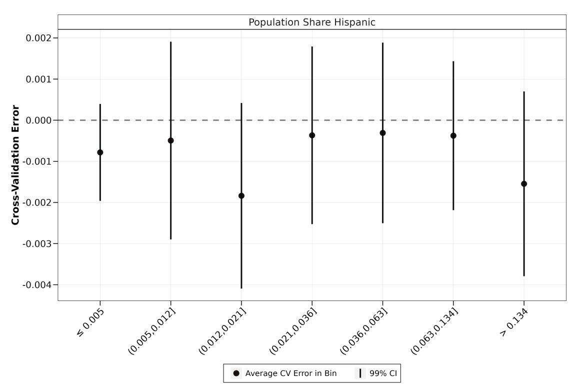

Share with < High School Diploma
Cross-Validation Error
0.002
0.000
-0.002
-0.004
≤ 0.042
(0.042,0.067]
(0.067,0.087]
(0.087,0.108]
(0.108,0.13]
(0.13,0.155]
(0.155,0.186]
(0.186,0.226]
(0.226,0.289]
> 0.289
Average CV Error in Bin
99% CI


Income Per Capita
Cross-Validation Error
0.002
0.001
0.000
-0.001
-0.002
-0.003
-0.004
≤ 1.73
(1.73,2.02]
(2.02,2.23]
(2.23,2.43]
(2.43,2.62]
(2.62,2.83]
(2.83,3.08]
(3.08,3.42]
(3.42,4.03]
> 4.03
Average CV Error in Bin
99% CI


Share without Private Vehicle
Cross-Validation Error
0.001
0.000
-0.001
-0.002
-0.003
-0.004
≤ 0.013
(0.013,0.022]
(0.022,0.031]
(0.031,0.042]
(0.042,0.056]
(0.056,0.077]
(0.077,0.113]
> 0.113
Average CV Error in Bin
99% CI


Population Share Asian
Cross-Validation Error
0.002
0.001
0.000
-0.001
-0.002
-0.003
≤ 0.001
(0.001,0.007]
(0.007,0.018]
> 0.018
Average CV Error in Bin
99% CI


Population Share Black
Cross-Validation Error
0.003
0.002
0.001
0.000
-0.001
-0.002
-0.003
≤ 0.007
(0.007,0.02]
(0.02,0.058]
(0.058,0.211]
> 0.211
Average CV Error in Bin
99% CI


Population Share Hispanic
Cross-Validation Error
0.002
0.001
0.000
-0.001
-0.002
-0.003
-0.004
≤ 0.005
(0.005,0.012]
(0.012,0.021]
(0.021,0.036]
(0.036,0.063]
(0.063,0.134]
> 0.134
Average CV Error in Bin
99% CI

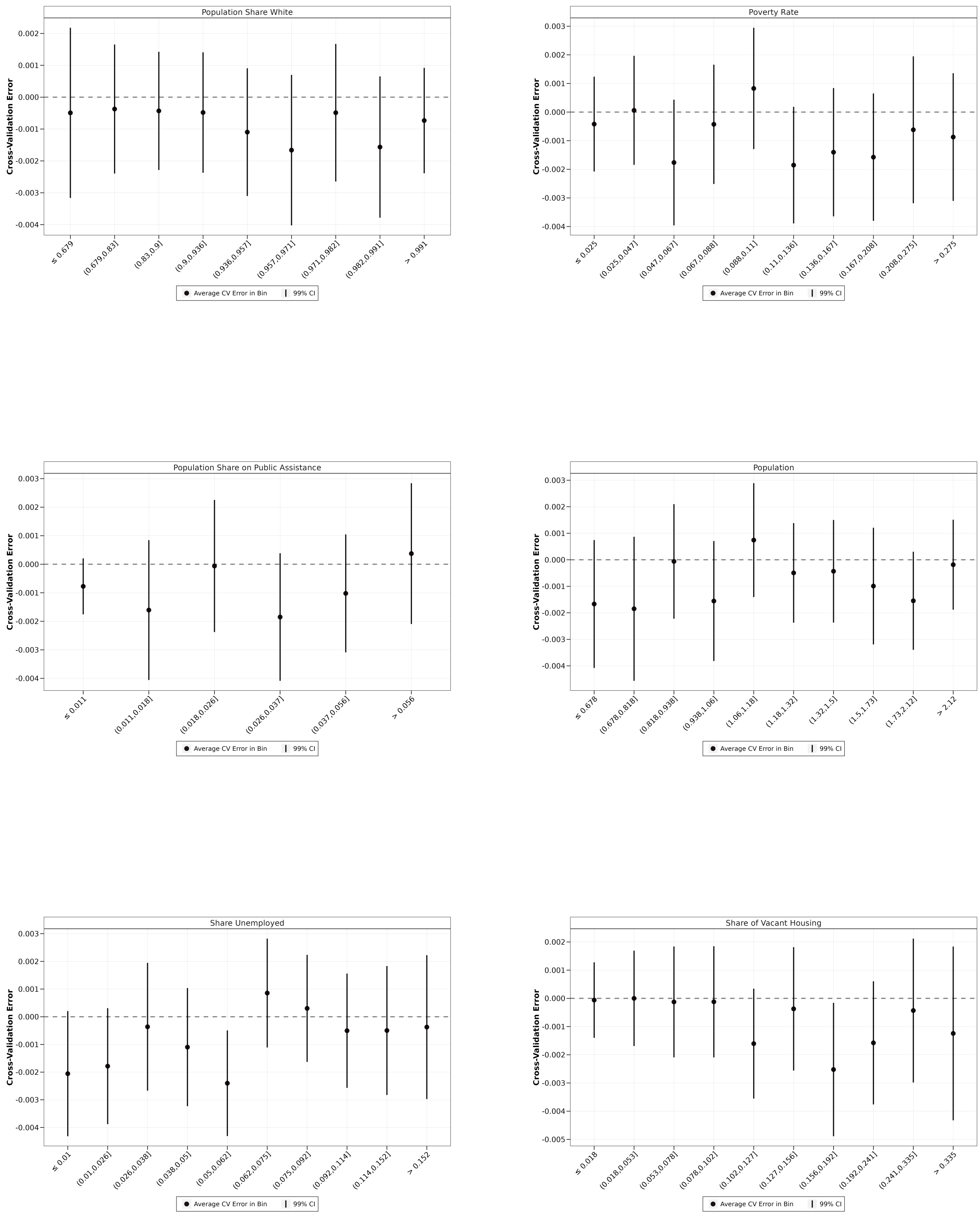


Figure 11.E: Rural-Area Cross-Validation Errors by Binned Model Covariate. Each panel contains the estimated average CV errors with respect to the binned covariate. The line-range associated with each point estimate represents 99% confidence intervals based on bootstrapped standard errors.

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