Who Pays for the Cloud?

The Impact of Data Center Entry on Grocery Purchases

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November 3, 2026

Abstract

Data centers serve the economy broadly, but any potential externalities they cause might be localized. To document these potential externalities, our research focuses on a consumer perspective by asking: Does data center entry impact local grocery spending? With establishment records and household scanner data from 2004–2014, we show that annual grocery spending declines by 12.3% after data centers enter (i.e., a $419 decline per household-year). The decline is largely driven by fewer purchase occasions. This same pattern is not observed in areas that experienced the entry of warehouses, which have similar physical footprints but much lower electricity needs relative to data centers.

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This project benefited from comments and discussions from conferences/seminars at the OSU Fisher AI in Business Conference, and University of Illinois Urbana-Champaign, as well as Ying Bao, Norris Bruce, Feng Chi, George Deltas, Alejandro Dominguez-Garcia, Kate Donahue, Avi Goldfarb, Vineet Kumar, Ben Leyden, Jūra Liaukonytė, Adam Osman, Vithala Rao, Aric Rindfleisch, Mario Samano, Kosuke Uetake, Tianli Xia, Song Yao, and Daniel Yue. We would also like to thank the Kilts Center for Marketing at the University of Chicago Booth School of Business for providing the NielsenIQ Consumer Scanner Data. The results and conclusions are those of the authors and do not reflect the views of NielsenIQ or the Kilts Center. Researcher(s)' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

1 Introduction

In recent years, data center growth has accelerated, and construction reached $45.2 billion in the first eight months of 2026, up 41.7% from the same period in 2025 (U.S. Census Bureau 2026). At the same time, local communities have pushed back on this rapid growth (Volcovici and Baertlein 2026), which has been at odds with tax incentives aimed at enticing data center entry (National Conference of State Legislatures 2026). Much of the recent policy discussion has focused on electricity and water use, environmental effects, and the role of subsidies (Volcovici and Lange 2026). Recent research examines how data center entry affects employment, income, housing, and electricity prices (e.g., Alvarez et al. 2026, Kay et al. 2026, Meeks et al. 2026, Muller 2026, Senga et al. 2026, Schlauch and Greenhill 2026, Watten et al. 2026, Yue and Zeng 2026). Our paper adds a consumer perspective by examining how data center entry affects grocery purchasing in host communities.

Grocery purchases are informative because households can adjust them as economic conditions change, while energy and water bills are costly to change quickly (Chetty and Szeidl 2007, 2016). Households can adjust how often they buy groceries, how much they spend on each occasion, what they buy, how many retailers they use, and how far those retailers are from home. Past marketing research documents similar responses to gasoline prices, income and wealth, liquidity constraints, and broader economic conditions (Ma et al. 2011, Dubé et al. 2018, Orhun and Palazzolo 2019, Scholdra et al. 2022). Data center entry may support purchasing through employment and income opportunities, while higher local expenses may tighten household budgets. The net response depends on how these changes reach households and which purchasing decisions they adjust. For retailers and manufacturers, purchase occasions represent opportunities to sell more products and brands, while spending per purchase occasion represents transaction value. We therefore examine annual spending together with purchase occasions and spending per purchase occasion. We also consider product composition (e.g., food and nonfood spending) and retailer choice (e.g., number of retailers).

Our research design focuses on the years 2004 to 2014, before the recent rise in public attention to data center proposals. Google search interest was substantially lower during our study period. Greater awareness can mobilize local opposition and increase the risk that proposed facilities never open (Kennedy 2026).[1] Moreover, utility companies that have already committed to infrastructure may recover costs from existing customers when developer guarantees or exit payments fall short (Hiller 2026, Satchwell et al. 2025). These "phantom" projects can even expose counties without operating facilities to household budget pressure, which further complicates comparisons of communities with versus without data centers. For these reasons, our data period of lower public attention offers a clean setting for our analysis.

We combine annual establishment records from the National Establishment Time Series (NETS) with household purchase records from the NielsenIQ Consumer Panel (Kilts Center for Marketing 2026a). NETS covers all industries, allowing us to identify the entry of data centers and related facilities (e.g., warehouses) in our analysis from a common source (Walls & Associates 2014b). The establishment records identify a county's first data center entry. During this period, the data-processing and hosting industry supplied web and application hosting, data processing, storage, and related computing infrastructure (Bureau 2012, Shehabi et al. 2016). We compare post-entry changes in county-year purchasing outcomes with contemporaneous changes in never-entry counties using the Callaway–Sant'Anna difference-in-differences design (Callaway and Sant'Anna 2021).

Our results show that annual grocery spending declines by 12.3% after the entry of a data center in a county, with an estimated $419 decline per household-year. The decline comes primarily from a 16.5% reduction in the number of purchase occasions. We do not observe a statistically significant change in spending per purchase occasion. The estimated decline is larger for nonfood products. Households also purchase from fewer retailers.

[1] For example, the Port of Cascade Locks attributed the cancellation of a proposed data center to insufficient community support (Port of Cascade Locks 2023).

Why might data center entry reduce purchase occasions for households? It is possible that higher utility bills leave households with less to spend on groceries. While household utility bills are not recorded in our data, we explore whether electricity expenses contribute to the decline using a warehouse placebo. Warehouses have similar physical footprints and locations as data centers but use substantially less electricity. Repeating the same analysis, but for warehouse entry, we do not detect a significant effect of warehouse entry. The warehouse placebo provides suggestive evidence that the spending decline is uniquely apparent after data center entry, consistent with electricity costs contributing to the response.

To understand where the purchasing decline is concentrated, we compare county purchasing responses with subsequent exposure to AI. The 2019 AI Geographic Exposure (AIGE) index measures occupational exposure to advances in AI capabilities (Felten et al. 2018, 2021). Counties with higher subsequent AIGE also tend to have experienced larger declines in grocery spending.

Our findings connect data center development to household shopping responses to changing economic conditions. Household purchase records therefore add evidence on changes in retail demand to assessments of local investment, employment, and business activity.

2 Related Literature and Contribution

We contribute to the marketing literature by examining how households adjust their shopping behavior after the entry of a data center. Our evidence builds on research on household shopping responses to economic conditions and localized externalities, and complements emerging work on the local economic effects of data centers.

2.1 How Economic Conditions Shape Household Purchases

Research on business cycles documents changes in consumption, price sensitivity, and marketing effectiveness (Lamey et al. 2007, Dutt and Padmanabhan 2011, Lamey et al. 2012,

Gordon et al. 2013, Van Heerde et al. 2013). Several studies examine income and wealth effects on private-label demand, liquidity constraints on bulk buying and deals, and the role of broader economic conditions (Dubé et al. 2018, Orhun and Palazzolo 2019, Scholdra et al. 2022). Other work examines grocery responses to gasoline prices and minimum wages (Gicheva et al. 2010, Ma et al. 2011, Palazzolo and Pattabhiramaiah 2021). Households adjust how often they shop, how much they buy, and where they shop in response to changes like prices and promotions, shopping costs, store assortment, and the entry of new retailers (Bucklin and Lattin 1991, Chintagunta 1993, Bell et al. 1998, Briesch et al. 2009, Singh et al. 2006, Vroegrijk et al. 2013). Shriver and Bollinger (2022) show that opening stores closer to existing customers increases an apparel retailer's sales through more frequent purchases.

These studies examine either economy-wide shocks that move household budgets in a predictable direction (e.g., greater price sensitivity and a shift toward private labels during recessions, or a shift toward promotions and cheaper store formats when gasoline prices rise), or changes within the retail setting itself (e.g., promotions that change when and how much households buy, or the entry of a supercenter or discounter that shifts visits away from incumbent stores). In contrast, data center entry occurs outside the retail setting and can bring jobs and income to some residents while raising costs for others through utility bills, so whether household spending rises or falls after entry is an open question that existing work cannot answer.

2.2 How Localized Externalities Shape Household Purchases

Local environmental and safety conditions can change what households buy and how they shop. Zivin et al. (2011) show that drinking-water violations increase bottled-water purchases, illustrating how households spend to avoid an environmental hazard. Kim and Trusov (2025) find that poorer air quality raises consumer spending in South Korea, particularly in hedonic categories, while Lim et al. (2025) show that wildfire smoke increases demand for sustainable cleaning products. Shaik et al. (2025) also document fewer grocery

trips and lower spending after fatal school shootings. These studies link purchasing responses to environmental conditions and perceived safety.

Nearby industrial facilities can impose costs through housing markets and health. Davis (2011) finds that power-plant openings reduce nearby housing values and rents. Currie et al. (2015) connect lower housing values and adverse infant-health outcomes to proximity to toxic plants. These comparisons focus on residents close to a facility and show how local costs are reflected in health and housing values.

Data centers draw on shared local infrastructure while serving users beyond the host community. The facilities closest to our investigation are cryptomining operations, which are also electricity-intensive, employ few workers, and process transactions for users elsewhere. Benetton et al. (2023) find that cryptomining in Upstate New York raised electricity prices for households and small businesses. Our research contributes to this body of work by studying a local shock that can benefit some residents and burden others, and whose effects extend beyond the immediate neighbors of the facility.

2.3 Data Center Effects

Research on data center development examines the employment gains, infrastructure costs, and environmental effects that inform local approval decisions. Studies of local economies consider employment, wages, business establishments, income, and house prices (Alvarez et al. 2026, Yue and Zeng 2026). In electricity markets, Kay et al. (2026) model wholesale-price effects, and Mamkhezri et al. (2026) estimate changes in the congestion and loss components of wholesale prices. Procurement arrangements, retail tariffs, and infrastructure cost allocation determine how these changes reach households. The simulations in Reguant (2019) show how retail cost-recovery rules distribute energy-policy costs across customers. Regulatory review also determines which capital costs and returns utilities recover (Borenstein 2016, Gowrisankaran et al. 2026, Dunkle Werner and Jarvis 2026). Schlauch and Greenhill

(2026) find that data centers lower nearby housing values. Other studies quantify environmental damages and the effects of flexible electricity demand on system costs and emissions (Muller 2026, Senga et al. 2026).

The closest study is Yue and Zeng (2026), who use facility activation dates to document gains in county employment, wages, establishments, and median household income, together with higher electricity prices where utility boundaries support the comparison. Employment gains and higher local expenses can reach different residents, so gains in aggregate employment or income need not imply higher household demand. Our evidence on grocery purchases shows how households adjust routine spending after entry, adding a consumer outcome to the assessment of local benefits and costs.

3 Conceptual Framework

Data center entry has the potential to expand local employment and business activity while increasing the expenses households face. Figure 1 links these local changes to household grocery purchases. New jobs, higher wages, and demand for local suppliers can support purchasing. Higher electricity, housing, and water costs can leave less income available for groceries. The net response depends on which households receive the gains and pay the costs, and when these changes occur.

Electricity demand is especially important because new facilities can require additional generation, transmission, and distribution capacity. Developers may fund dedicated connections, while utilities recover approved shared infrastructure costs through residential tariffs and adjustments (National Association of Regulatory Utility Commissioners 1992, Lazar et al. 2020). Equinix's 2008 commitment to a substation for its planned Los Angeles facility illustrates these investment requirements (Equinix 2008). The household burden depends on the investment required, developer payments, and the allocation of remaining costs. These

obligations can persist when projects are delayed or abandoned (Greenstein 2026). Additional sales can also spread existing fixed costs when revenue exceeds the incremental cost of service (California Public Utilities Commission 1996). Higher recurring bills tighten grocery budgets, and cost recovery can reach households at a different time from employment gains.

Retail access, travel costs, household composition, and expectations form the third pathway in Figure 1. Changes in retail availability and travel costs can affect how often households shop and how many retailers they use. Expectations about future income and expenses may also shape current purchases. New residents may have different purchasing habits, changing county averages alongside any adjustment by existing households. These local changes can therefore affect both household purchasing decisions and the composition of observed spending.

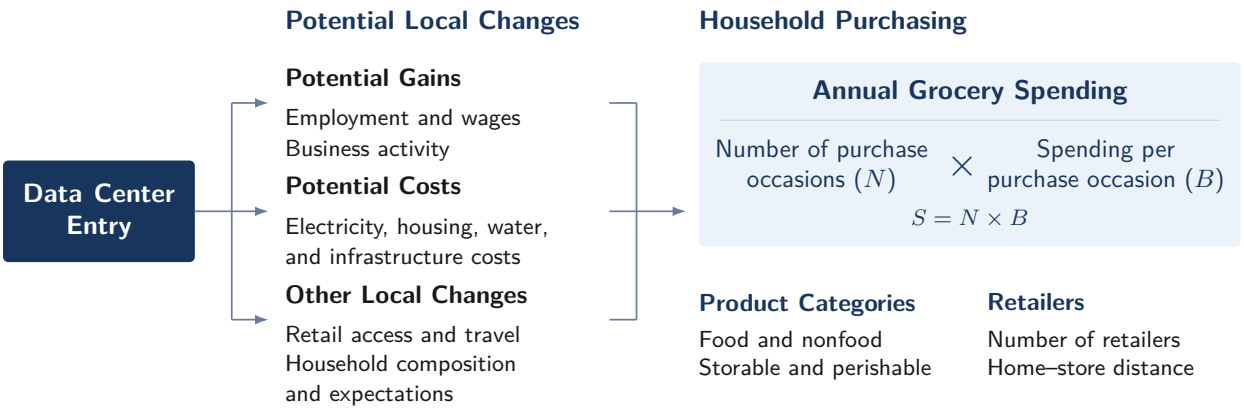


Figure 1: Conceptual Framework for Household Purchasing Responses

Notes: Arrows show potential relationships between data center entry, local conditions, and household purchasing. Annual grocery spending S equals the number of purchase occasions N multiplied by spending per purchase occasion B: $S = N \times B$. Product categories, number of retailers, and home–store distance describe additional purchasing responses.

Annual grocery spending can change through the number of purchase occasions, spending per purchase occasion, or both. For a household in a given year, let S denote annual grocery spending, N the number of purchase occasions, and B average spending per purchase occasion, all covering the same products. These measures satisfy the accounting identity $S = N \times B$. Households facing budget pressure may reduce the number of purchase occasions

or spend less on each occasion. They may also consolidate purchases into fewer, larger occasions. Fewer occasions reduce opportunities for retailers and brands to reach households, while spending per purchase occasion determines transaction value. Marketing research similarly distinguishes whether a purchase occurs from decisions conditional on purchase (Gupta 1988, Bucklin and Lattin 1991, Ma et al. 2011, Gabel et al. 2024).

The additional outcomes in Figure 1 help distinguish these purchasing responses. Food and nonfood spending show how adjustment differs across product categories, while storable purchases help assess whether households consolidate purchases into fewer occasions. The number of retailers and home–store distance describe changes in the breadth and geography of shopping. The warehouse comparison examines whether a similar purchasing response follows development with much lower electricity requirements (Section 6.4), providing evidence on the role of electricity expenses among the pathways in the figure.

4 Data and Empirical Design

Annual NETS establishment records identify county entry, while the NielsenIQ Consumer Panel records household purchases. Differences in entry years allow us to compare purchasing changes in counties that experienced their first data center entry (i.e., entry counties) and those that did not experience an entry (i.e., never-entry counties) in our data period.

4.1 Data Center Entry

Data centers house servers and networking equipment used to store information, process transactions, and run applications. Commercial operators offer colocation, in which clients place their own equipment in a facility that provides power, cooling, security, and connections to telecommunications networks (Equinix 2009a). Clients can also purchase managed

hosting and cloud services, with the provider supplying computing infrastructure and technical support for their applications.[2]

NETS covers all industries and provides a common historical source for data center and warehouse entry (Section 6.4). This dataset includes a broad set of establishments affiliated with a publicly listed firm at least once during 1990–2014 (Walls & Associates 2014a). Annual industry and location records follow establishments over time and retain their histories after closure, allowing us to identify prior county activity before the household panel begins (Walls & Associates 2014b). These records let us date entry without relying on reported capacity or assigning current facility size to earlier years.[3]

We map each establishment's annual industry codes to the 2012 North American Industry Classification System (NAICS) and identify NAICS 518210, *Data Processing, Hosting, and Related Services*.[4] This industry code covers establishments that provide infrastructure for hosting and data processing, including web and application hosting, computer storage, time-sharing mainframes, streaming support, and automated data-processing services (Bureau 2012). An establishment enters when it is classified in this industry throughout its observed history or switches into the industry and remains there. The earliest such year in a county defines its data center entry year.[5]

Our sample comprises 52 entry counties and 2,160 never-entry counties. Entry counties experience their first data center entry during 2006–2014 and have purchasing observations before entry and in the entry year or later; all 52 have at least two observed pre-entry county-years.[6] Never-entry counties have no active data center establishment in the 1990–2014

[2]Equinix describes financial firms using its facilities for electronic trading, while QTS described clients running email-archiving and healthcare applications on its infrastructure (Equinix 2009b, QTS Realty Trust 2014).

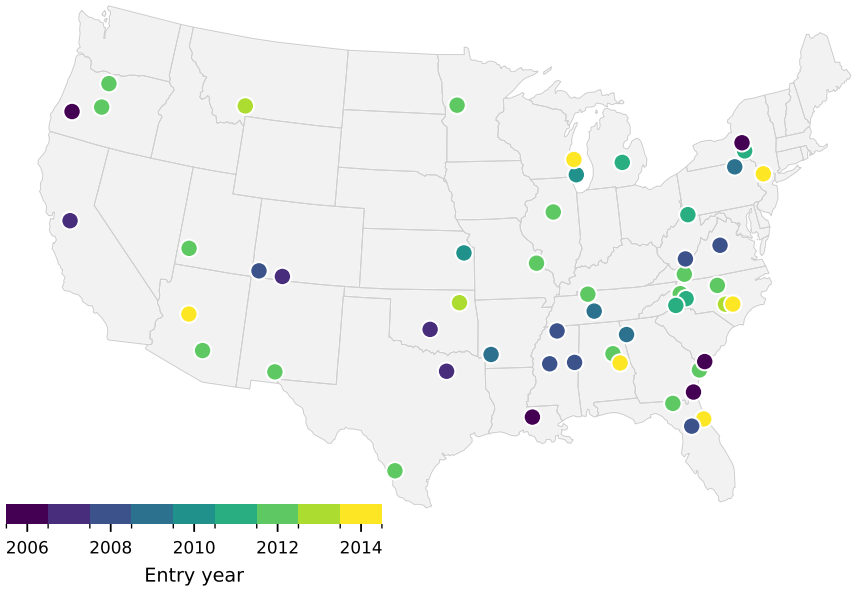
[3]Meeks et al. (2026) report imprecise pre-2005 activation years in the Aterio database. Watten et al. (2026) begin in 2015 because earlier capacity data are incomplete. Alvarez et al. (2026) assign currently reported facility attributes to historical build cohorts.

[4]The NETS Public 2014 files supply a crosswalk from Dun & Bradstreet eight-digit SIC codes to 2012 NAICS, providing a common classification for 1990–2014.

[5]Treatment begins with the first entry in a county and remains on thereafter.

[6]Using entry years from 2006 onward allows two pre-entry calendar years in the 2004–2014 Consumer Panel window.

Figure 2: Geography and Timing of Data Center Entry



Notes. The map shows the contiguous United States and the 52 counties with data center entry during 2006–2014. Colors indicate the county's first entry year.

history. Counties with earlier industry presence are excluded from both groups. Figure 2 shows the entry counties and years.

Figure 3 shows large physical data centers at NETS establishment addresses, verified through operator histories, facility specifications, and public records. Additional matches include Internap's Seattle facility, SoftLayer's Dallas Infomart operations, and CyrusOne and SunGard sites (Web Appendix Table A1). These examples confirm that the NETS dataset includes large facilities with server and colocation infrastructure.

4.2 Consumer Panel and Sample

Household purchases come from the 2004–2014 Nielsen Homescan panel, distributed by the Kilts Center for Marketing (Kilts Center for Marketing 2026a). Prior studies use these data to examine income, wealth, liquidity constraints, and shopping responses (Dubé et al. 2018,

Figure 3: Large Data Center Facilities at NETS Addresses

A. Google
Lenoir, North Carolina



Opening	**2008**
Size	**337,000 sq. ft. (est.)**
Capacity	**Not reported**

B. Facebook
Forest City, North Carolina



Opening	**2012**
Size	**2 × 370,000 sq. ft.**
Capacity	**Not reported**

C. CyrusOne
Houston West I, Texas



Opening	**2009**
Size	**110,190 sq. ft. (IT)**
Capacity	**15.6 MW (critical IT)**

D. CyrusOne
Carrollton, Texas



Opening	**2012**
Size	**Up to 670,000 sq. ft.**
Capacity	**Up to 60 MW (critical)**

E. AT&T
Hawthorne, California



Opening	**2001**
Size	**288,583 sq. ft. (gross)**
Capacity	**8 MW (standby)**

F. AT&T
Watertown, Massachusetts



Opening	**2002**
Size	**201,590 sq. ft. (gross)**
Capacity	**16 MW (standby)**

Notes. Opening is the documented facility opening or completion year. Sizes and capacities describe the cited configurations, including later expansions. Facebook's area covers two buildings; Houston's area is IT space. Critical capacity serves IT equipment; standby capacity includes backup generators. "Up to" denotes advertised maximum capacity; Lenoir's area is estimated. Facility details come from industry, operator, and builder documents (e.g., Panattoni 2010, AT&T 2011, Facebook 2012, CyrusOne and Business Wire 2013, Ghiasi and Baca 2014, Evoque Data Center Solutions 2019a,b, CyrusOne n.d., DataBank n.d., DPR Construction n.d., Google n.d.).

Orhun and Palazzolo 2019, Scholdra et al. 2022). Participating households scan purchases across grocery, drug, mass-merchandise, club, convenience, and other retail channels.

Annual panelist records identify households, residential counties and ZIP codes, and demographics.[7] Trip records provide purchase dates, trip identifiers, and retailer and store codes. Purchase records report each product's universal product code (UPC), quantity, price, and coupon value. The product dictionary provides departments, modules, brands, and other attributes. We assign households to counties using the FIPS code in each annual panelist record. The year of the annual NielsenIQ file defines the panel year; individual purchase dates can cross calendar-year boundaries.

The dataset contains over 150,000 household-years. We retain household-years with purchases in at least nine months and positive annual gross spending, exclude both annual records surrounding a county or ZIP-code change, and require households to be observed in at least two years. The final sample contains 133,130 household-years. Before screening, households in our counties have similar household sizes and shares with children to the national Homescan panel, but lower average income (Web Appendix Table A2).

4.3 Outcome Construction

Main Outcomes Our primary outcome is annual grocery spending, covering food, beverages, household and personal-care products, and selected general merchandise. We sum the price paid minus coupon value for each purchase in the NielsenIQ product records. The spending measure covers standard UPC-coded products, excluding fresh produce, fresh meat, and in-store bakery items without standard UPCs. Let S_{ict} denote this spending for household i in county c and panel year t.

[7] NielsenIQ supplies weights to align panel households with the demographic and geographic composition of the broader U.S. population (Kilts Center for Marketing 2026a). We use these weights when aggregating household purchases to county-year outcomes.

A *purchase occasion* is a NielsenIQ trip identifier containing at least one product included in S_{ict}. Let N_{ict} denote the household's annual number of these occasions. For household-years with positive annual spending and at least one recorded occasion, we define spending per purchase occasion as $B_{ict} = S_{ict}/N_{ict}$. The identity $\log S_{ict} = \log N_{ict} + \log B_{ict}$ links annual spending to the number of purchase occasions and spending per purchase occasion. Additional outcomes include spending, the number of purchase occasions, and packages per purchase occasion for food and beverages, nonfood products, storable products, and perishable products.[8] We also examine the number of small, medium, and large purchase occasions. We count the number of retailers using distinct retailer codes and measure home–store distance between household and store ZIP centroids. Table 1 reports variable definitions and descriptive statistics.

County Characteristics. County Business Patterns (US Census Bureau 2024) and the Bureau of Economic Analysis (U.S. Bureau of Economic Analysis 2026b) provide the county characteristics in Table 1. The baseline specification excludes contemporaneous controls.

County-Year Aggregation. Because entry varies at the county-year level, we aggregate household outcomes before estimation. For $q \in \{S, N, B\}$, let w_{it} denote household i's annual projection factor, and let $\mathcal{H}_{ct}$ denote the household-years in our sample for county c and year t. The county-year outcome is

$$Y_{ct}^{q} = \frac{\sum_{i \in \mathcal{H}_{ct}} w_{it} \log q_{ict}}{\sum_{i \in \mathcal{H}_{ct}} w_{it}}. \tag{1}$$

This outcome averages household logs using NielsenIQ projection factors. With the same households and weights for all three measures, $Y_{ct}^{S} = Y_{ct}^{N} + Y_{ct}^{B}$, and linear aggregation preserves the equality among their treatment effects. The estimates describe purchasing

[8] Food and nonfood are classified using NielsenIQ product-module descriptions, following the broad distinction in Dubé et al. (2018). The storable/perishable classification uses the product-module mapping of Baker et al. (2024).

Table 1: Descriptive Statistics and Variable Definitions

Variable	Definition	Mean	SD
Panel A. Household purchasing			
Annual grocery spending ($)	Total spending net of coupons	3,660.2	1,464.5
Number of purchase occasions	Unique shopping trips	129.9	53.9
Spending per purchase occasion ($)	Spending per unique shopping trip	33.3	15.4
Food and beverage spending ($)	Spending on food and beverages for human consumption, including baby food, alcohol, and food-use ice	2,302.8	917.8
Nonfood spending ($)	Spending on household and personal-care products, pet food and care, diapers, medicines, supplements, tobacco, and general merchandise	1,357.3	736.6
Number of purchase occasions with food	Occasions with at least one food product	106.0	44.1
Number of purchase occasions with nonfood products	Occasions with at least one nonfood product	76.3	31.6
Food packages per purchase occasion	Food packages divided by occasions with food	11.2	4.6
Nonfood packages per purchase occasion	Nonfood packages divided by occasions with nonfood products	4.3	1.9
Number of small purchase occasions	Occasions with positive spending below $10	49.8	36.6
Number of medium purchase occasions	Occasions with spending from $10 to below $40	55.0	24.2
Number of large purchase occasions	Occasions with spending of $40 or more	24.9	13.2
Panel B. Additional shopping measures			
Storable-product spending ($)	Spending on longer-lived products, such as dry, canned, and frozen foods and most nonfood products	3,117.7	1,283.2
Number of purchase occasions with storable products	Occasions with at least one storable product	124.4	51.1
Storable-product packages per purchase occasion	Storable packages divided by occasions with storable products	10.1	4.2
Perishable-product spending ($)	Spending on short-lived products, such as milk, yogurt, and fresh bread	541.9	248.3
Number of purchase occasions with perishable products	Occasions with at least one perishable product	63.4	23.9
Perishable-product packages per purchase occasion	Perishable packages divided by occasions with perishable products	4.0	1.4
Number of retailers	Distinct NielsenIQ retailer codes per household-year	12.5	4.5
Home–store distance (miles)	Great-circle distance between household and store zip centroids	33.3	43.2
Panel C. County characteristics			
Population	Resident population	36,568.2	41,017.4
Personal income per capita ($)	Total personal income divided by population	32,177.0	7,980.0
Total personal income ($000)	Personal income of county residents	1,194,062.0	1,479,132.1
Private employment	Private-sector employees	9,989.6	12,522.6
Private establishments	Private-sector establishments	794.1	901.5
AI geographic exposure	County AIGE index of Felten et al. (2021), 2019 values	-0.284	1.103

Notes. The sample covers 52 entry and 2,160 never-entry counties in 2004–2014, with $N = 21{,}030$ county-years. Spending and counts are annual unless stated per purchase occasion. Purchase-size thresholds are in 2005 dollars. Package counts sum positive quantities, and package sizes differ across products. Data sources are the NielsenIQ Consumer Panel (Kilts Center for Marketing 2026b), the Bureau of Economic Analysis for population and income (U.S. Bureau of Economic Analysis 2026a), County Business Patterns for employment and establishments (U.S. Census Bureau 2024), and the AI Geographic Exposure index (Felten et al. 2021).

among panel households observed in each county-year. Household composition can change as participants enter or leave the panel.

A log estimate $\widehat{\beta}$ corresponds to a percentage change of $100\{\exp(\widehat{\beta}) - 1\}$. To estimate the dollar effect, we construct the weighted mean of annual household spending,

$$\overline{S}_{ct} = \frac{\sum_{i \in \mathcal{H}_{ct}} w_{it} S_{ict}}{\sum_{i \in \mathcal{H}_{ct}} w_{it}}, \tag{2}$$

and repeat the analysis with spending in dollars.[9]

4.4 Empirical Design

Purchasing responses may develop gradually, and earlier entry cohorts can be followed for longer. A conventional two-way fixed-effects regression can use already-treated counties as controls when comparing cohorts. We estimate separate cohort- and year-specific effects using the Callaway–Sant'Anna difference-in-differences design (Callaway and Sant'Anna 2021).

Let $G_c = g$ indicate that county c first records entry in year g, and let $G_c = \infty$ denote a county with no entry in the 1990–2014 NETS history. For outcome k, let $Y_{ct}^k(g)$ denote the potential county-year outcome if entry occurs in year g, and let $Y_{ct}^k(\infty)$ denote the potential outcome without entry. For cohort g and year $t \geq g$,

$$ATT_k(g,t) = \mathbb{E}\left[Y_{ct}^k(g) - Y_{ct}^k(\infty) \mid G_c = g\right]. \tag{3}$$

Here, $ATT_k(g,t)$ is the average effect of entry on purchasing in year t for counties entering in year g.

For a cohort entering in g, we compare the change in purchasing from $g-1$ to t with the change over the same calendar years among never-entry counties. Under no anticipation

[9] Web Appendix Table A3 reports estimates in original units.

and unconditional parallel trends,

$$\mathbb{E}\left[Y_{ct}^{k}(\infty) - Y_{c,g-1}^{k}(\infty) \mid G_c = g\right] = \mathbb{E}\left[Y_{ct}^{k}(\infty) - Y_{c,g-1}^{k}(\infty) \mid G_c = \infty\right]. \tag{4}$$

Parallel trends permits different purchasing levels while requiring equal average changes without entry. Comparing the same calendar years removes national shocks, including the Great Recession and recovery, to the extent that they affect entry and comparison counties similarly.

The baseline uses never-entry counties as controls, allows an unbalanced panel, and clusters inference by county. We exclude contemporaneous county characteristics because employment, population, income, housing, and retail activity may themselves respond to entry. Let event time be $e = t - g$. We average the cohort effects observed at each event time:

$$ATT_k^{\text{dyn}}(e) = \sum_{g:g+e\leq T} \omega_{g,e} ATT_k(g, g+e), \qquad \sum_g \omega_{g,e} = 1. \tag{5}$$

The weight $\omega_{g,e}$ is the contribution of cohort g to the estimate at event time e. The main event study uses the year before entry, $e = -1$, as the common reference period.

Only early entry cohorts contribute to the longest horizons. The average post-entry estimate combines all observed post-entry cohort–year effects, giving earlier cohorts more total weight because they contribute more years.

4.5 Identification Assumptions

The purchasing panel begins in 2004 and includes entry cohorts from 2006–2014, so all counties are initially untreated. Using the 1990–2014 NETS history, we verify that each entry county had no active data center establishment before its assigned entry year and that never-entry counties had none throughout this period. Treatment begins with the first entry and remains on regardless of subsequent establishment exits or additional entries. The comparisons follow the identification conditions in Callaway and Sant'Anna (2021).

We assume that entry does not affect purchasing before the entry year. Our study period predates the recent rise in public attention to data centers. In a continuous U.S. Google Trends Web Search series for "data center" across all categories, mean monthly search interest was 5.9 during 2004–2014 and 50.1 during August 2025–July 2026. The comparison uses 132 study-period months and 12 complete recent months from a series retrieved on August 17, 2026; the partial month of August 2026 is excluded. Google Trends normalizes search interest within the requested geography and period to a 0–100 scale, so the values describe relative search interest rather than search counts.[10]

Without entry, the average change in purchasing for each entry cohort would equal that in never-entry counties over the same calendar years, as stated in equation (4). Identification also requires a positive probability of belonging to each entry cohort and of remaining untreated. We estimate cohort–year effects where both groups are observed. Section 6 examines pretreatment paths alongside the cohort-specific comparisons and spending components. Callaway and Sant'Anna also permit later-entry counties to serve as controls before their own entry. We retain never-entry counties in the baseline and report this alternative as a sensitivity check in Section 6.2.

We apply the same entry definition across counties and assume limited exposure of comparison counties to entry elsewhere. Electricity networks, labor and housing markets, and retail catchment areas can transmit effects across county boundaries. Excluding neighboring comparison counties leaves the spending estimate nearly unchanged, although utility networks can extend farther. Excluding comparison counties that share an electricity provider also leaves the estimate close to the baseline (Web Appendix Table A4). PJM's regulatory filing distinguishes upgrades allocated within a transmission-owner zone from regional upgrades associated with aggregate load growth (PJM Interconnection, L.L.C. 2025).[11]

[10] Data source and methodology: `https://trends.google.com/trends/` and `https://support.google.com/trends/answer/4365533`.

[11] Recent local opposition has also prevented proposed facilities from opening (Port of Cascade Locks 2023). When utilities recover the costs of prior infrastructure commitments from existing customers, these "phantom" projects can raise household expenses in counties without operating facilities (Hiller 2026, Satchwell et al. 2025, Greenstein 2026).

Inference assumes independence across counties and allows unrestricted dependence over time within each county. We construct annual county outcomes from household records and use 1,000 county-clustered multiplier-bootstrap draws for inference. Each draw assigns the same multiplier to all observations from a county, accommodating serial dependence and repeated observations of the same household. Event-study panels report 95% simultaneous confidence intervals for each primary outcome. Summary estimates report bootstrap standard errors and conventional 95% confidence intervals. Purchase-size comparisons include the multiple-testing adjustments reported in Section 5.1.

5 Results for Household Purchases

Annual grocery spending declines by 12.3% after the entry of a data center in a county. Table 2 reports an average log effect of -0.131 (SE $= 0.052$). Figure 4 shows annual spending and its components relative to the year before entry. The point estimates become more negative at longer horizons, which contain the earlier entry cohorts. Section 6.1 examines pretreatment paths.

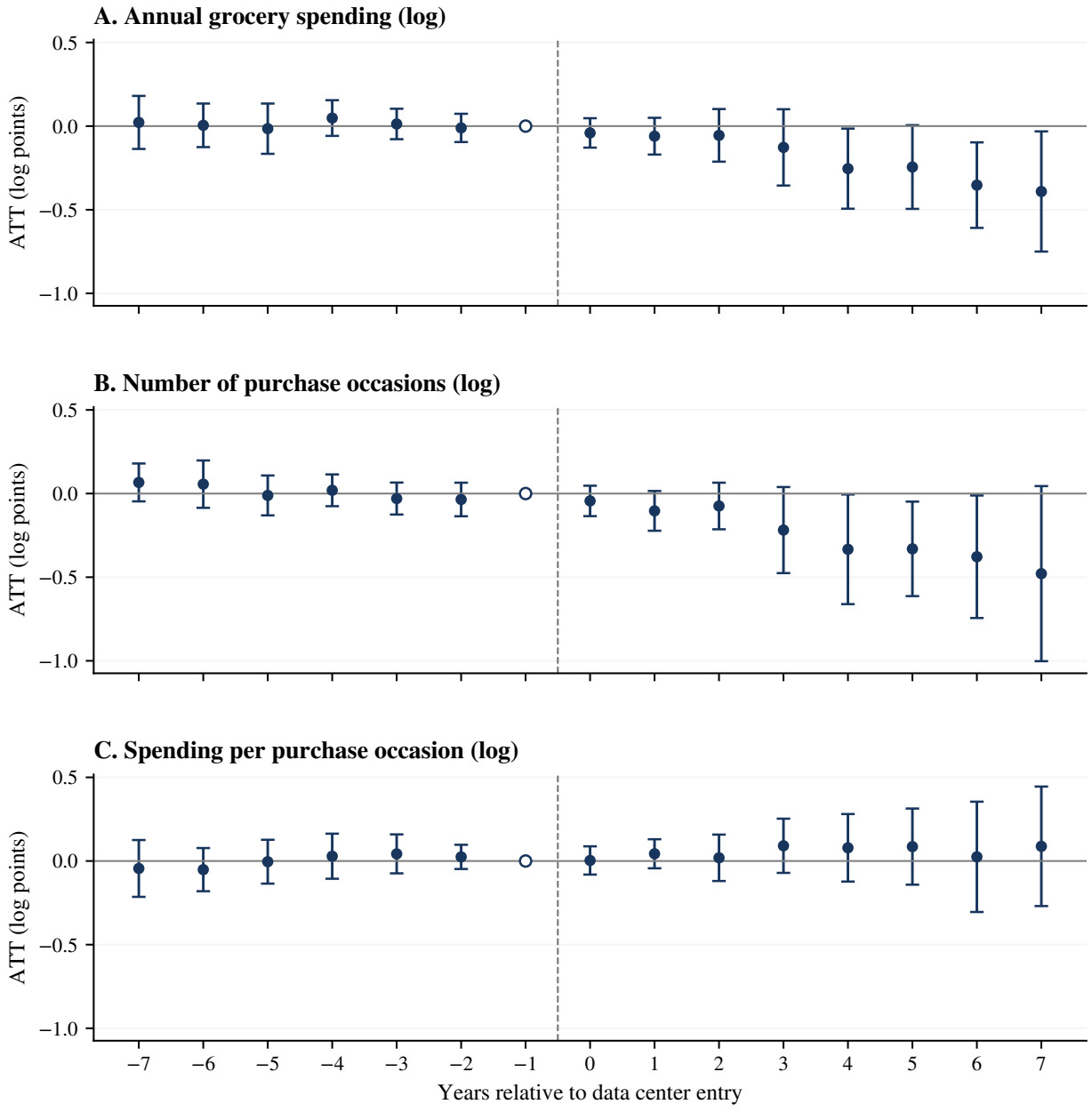
Table 2: Data Center Entry and Household Purchasing

Dependent variable	ATT	SE	p-value
Annual grocery spending (log)	-0.131^{**}	0.052	.012
Number of purchase occasions (log)	-0.180^{***}	0.069	.009
Spending per purchase occasion (log)	0.049	0.047	.297

Notes. Callaway–Sant'Anna average post-entry effects for 52 entry and 2,160 never-entry counties (21,030 county-years, 2004–2014). Standard errors use 1,000 county-clustered multiplier-bootstrap draws; p-values are two-sided and unadjusted. Outcomes are county-year means of household logs, weighted by NielsenIQ projection factors. Dollar estimates are reported in Web Appendix Table A3. $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

If these costs reduce purchasing in comparison counties, the difference between entry and comparison counties can understate the purchasing decline.

Figure 4: Effects of Data Center Entry on Annual Grocery Spending, Purchase Occasions, and Spending per Purchase Occasion over Time



Notes. Points are Callaway–Sant'Anna event-study estimates for county-year means of household log outcomes, weighted within county-year by NielsenIQ projection factors. The reference year is −1 (open marker); the dashed line separates pre-entry and post-entry years. Bars are 95% simultaneous confidence intervals from 1,000 county-clustered multiplier-bootstrap draws, calculated separately by outcome. Joint Wald-test p-values for years −7 through −2 are .723, .220, and .729 for annual grocery spending, number of purchase occasions, and spending per purchase occasion, respectively. The sample contains 52 entry and 2,160 never-entry counties.

The decline in annual grocery spending comes primarily from fewer purchase occasions. Purchase occasions fall by 16.5% after entry (Table 2), or approximately 23 per household-year in the count specification. We do not observe a statistically significant change in spending per purchase occasion; the point estimate is a 5.0% increase ($p = .297$). Figure 4 shows how the two components vary with years since entry.

5.1 Product Categories, Purchase Size, and Other Adjustments

We use NielsenIQ product-module descriptions to group products into food and beverages or nonfood products. The proportional specifications use $\log(1+x)$, where x is the county-year outcome, retaining county-years with zero spending or purchase occasions. Dollar estimates use nominal spending. Spending declines by approximately 6.9% for food and beverages and 14.2% for nonfood products (Table 3). The dollar estimates are approximately \$194 and \$225 per household-year, respectively (Web Appendix Table A3). The estimated proportional spending decline is larger for nonfood, although we do not observe a statistically significant difference between the groups ($p = .227$).

The number of purchase occasions with food declines by approximately 14.2%, compared with 12.8% for nonfood products. We do not observe statistically significant changes in packages per purchase occasion for either group.

To examine whether purchase occasions of different sizes decline, we group positive-spending occasions into purchases below \$10, from \$10 to below \$40, and at least \$40, using spending net of coupons in 2005 dollars. In 2004–2005, the smallest group contains an average of 2.3 product lines per occasion, compared with 21 in the largest group.

Small purchase occasions decline by 13.8 per household-year (Table 4). This is the largest estimated decline in both absolute and proportional terms. Differences across groups are not statistically significant at the 5% level. The table reports unadjusted p-values and Holm adjustments for the three purchase-size tests (Holm 1979).

Table 3: Food and Nonfood Purchases

Dependent variable	ATT	SE	p-value
Panel A. Annual spending			
Food and beverage spending ($\log(1+x)$)	-0.071	0.048	.135
Nonfood spending ($\log(1+x)$)	-0.153^{**}	0.067	.023
Panel B. Number of purchase occasions			
Number of purchase occasions with food ($\log(1+x)$)	-0.153^{***}	0.054	.004
Number of purchase occasions with nonfood products ($\log(1+x)$)	-0.136^{**}	0.058	.018
Panel C. Packages per purchase occasion			
Food packages per purchase occasion ($\log(1+x)$)	0.063	0.040	.117
Nonfood packages per purchase occasion ($\log(1+x)$)	-0.027	0.049	.589

Notes. Callaway–Sant'Anna average post-entry effects. The study sample contains 52 entry and 2,160 never-entry counties (21,030 county-years, 2004–2014). Standard errors use 1,000 county-clustered multiplier-bootstrap draws; p-values are two-sided and unadjusted. An occasion may contain both food and nonfood products. The food-minus-nonfood spending difference has $p = .227$. $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

Table 4: Number of Purchase Occasions by Purchase Size

Dependent variable	ATT	SE	p-value
Number of small purchase occasions	-13.82^{**}	6.00	.021
Number of medium purchase occasions	-6.66	4.80	.166
Number of large purchase occasions	-2.55	1.86	.170

Notes. Callaway–Sant'Anna average post-entry effects for 52 entry and 2,160 never-entry counties (21,030 county-years, 2004–2014). Standard errors use 1,000 county-clustered multiplier-bootstrap draws; p-values are two-sided and unadjusted. Outcomes are annual numbers of purchase occasions. Positive-spending occasions are small (below \$10), medium (\$10 to below \$40), or large (\$40 or more), in real 2005 dollars. Holm-adjusted p-values for small, medium, and large occasions are .064, .331, and .331. The small–medium difference, standardized by each group's pre-entry mean, has Holm-adjusted $p = .066$. Stars use unadjusted p-values: $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

Households could respond to tighter budgets by consolidating purchases, using fewer retailers, or shopping closer to home. We examine storable products, for which consolidation would mean fewer occasions and more packages per occasion, and the number of retailers and home–store distance (Table 5).

Storable products can ordinarily be kept for several weeks, including frozen and nonfood products. Short-lived fresh or refrigerated products are perishable. Purchase occasions with storable products decline by approximately 13.9%, compared with 12.7% for perishable products. Spending declines by approximately 10.0% and 9.2%, respectively. We do not observe a statistically significant change in packages per purchase occasion for either group. The results provide little evidence that larger purchases offset the decline in purchase occasions.

The number of retailers declines by 10.3% during the year. The estimated change in home–store distance is close to zero. We measure great-circle distance between the household zip centroid and the store zip centroid, describing the locations of observed purchases.

Table 5: Product Storage, Number of Retailers, and Home–Store Distance

Dependent variable	ATT	SE	p-value
Panel A. Storable-product purchases			
Storable-product spending ($\log(1+x)$)	-0.105^{**}	0.048	.030
Number of purchase occasions with storable products ($\log(1+x)$)	-0.150^{**}	0.060	.013
Storable-product packages per purchase occasion ($\log(1+x)$)	0.041	0.038	.283
Panel B. Perishable-product purchases			
Perishable-product spending ($\log(1+x)$)	-0.097	0.064	.132
Number of purchase occasions with perishable products ($\log(1+x)$)	-0.135^{***}	0.048	.005
Perishable-product packages per purchase occasion ($\log(1+x)$)	0.009	0.036	.801
Panel C. Number of retailers and home–store distance			
Number of retailers (log)	-0.108^{**}	0.050	.029
Home–store distance ($\log(1+x)$)	0.000	0.023	.998

Notes. Callaway–Sant'Anna average post-entry effects. The study sample contains 52 entry and 2,160 never-entry counties (21,030 county-years, 2004–2014). Standard errors use 1,000 county-clustered multiplier-bootstrap draws; p-values are two-sided and unadjusted. Number of retailers is based on distinct retailer codes. Distance is computed as great-circle distance between the home and store ZIP centroids for each household. $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

6 Validity and Sensitivity Analyses

6.1 Pretreatment Purchasing

Our research design requires that, without entry, purchasing in entry counties would have changed like purchasing in never-entry counties, so we first examine whether the two groups followed similar paths before entry. Figure 4 expresses annual grocery spending and its two components relative to the year before entry, $e = -1$. The joint pretreatment test for annual spending yields $p = .723$; the corresponding values are .220 for purchase occasions and .729 for spending per purchase occasion.

6.2 Comparison Counties

Our main specification uses counties with no entry in the 1990–2014 NETS history as the comparison group. Counties that eventually host a data center may differ from those that never do. These later entrants provide a comparison more similar in size and economic activity. Allowing future-entry counties to serve as comparisons before their own entry leaves the spending decline unchanged at 12.3%. Web Appendix Figure A1 shows that simultaneous confidence intervals include zero for the aggregated pretreatment estimates under both comparison groups. The joint test of all cohort–year pretreatment effects is unavailable in the baseline because its covariance matrix is singular. It rejects after not-yet-treated counties are added ($p < .001$). Thus, the average spending estimate is stable across comparison groups, while the more detailed pretreatment tests give mixed evidence.

Restricting comparisons to the 1,581 never-entry counties in entry states yields a 12.1% decline. Excluding the 185 comparison counties bordering an entry county yields a 12.4% decline. Across these four comparison groups, spending declines range from 12.1% to 12.4% (Web Appendix Table A5, Panel A). Adding fixed 2004–2005 county characteristics also preserves the negative result when specifications use the same complete-case sample (Web Appendix Table A6).

6.3 Entry Definition and Timing

Restricting the analysis to establishments that switch into and remain in the industry yields a 14.4% spending decline in 17 entry counties. This restriction changes both the entry definition and the counties included. Moving entry one year earlier produces a 9.4% decline; moving it one year later produces an 11.6% decline (Web Appendix Table A5, Panel D).

To examine anticipation, we assign placebo entry two years early and use only observations before actual entry. The estimate is -0.020 (SE $= 0.034$, $p = .560$) for the 43 counties in the 2008–2014 cohorts (Web Appendix Table A7).[12]

6.4 Warehouse Placebo

Higher electricity bills can leave households with less to spend on groceries. Connecting a local increase in electricity demand to household expenses requires knowing how utilities allocate and recover costs, the tariffs households face, and their electricity use (Martin and Peskoe 2025, pp. 1–2). Some operating and reliability costs are recovered through charges outside wholesale energy prices, and their allocation determines which customers pay them (FERC 2014, Hirst and Kirby 2003). Fixed charges, usage rates, and credits can distribute the same infrastructure costs differently across households (Borenstein 2012, Lazar et al. 2020). Households also adjust their electricity use as prices change (Ito 2014, Deryugina et al. 2020). Average utility revenue per MWh combines these responses and billing arrangements. It can remain unchanged even as bills rise for some households.[13]

Warehouse entry provides a comparison with buildings that resemble data centers but use much less electricity (Virginia Joint Legislative Audit and Review Commission 2024). Measurements from four data center buildings imply annual electricity use per square foot about 4–17 times the 2012 benchmark for nonrefrigerated warehouses (Figure 5) (Mitchell-Jackson et al. 2003, U.S. Energy Information Administration 2016). Data centers' continuous

[12] Web Appendix Table A8 summarizes the identification conditions and supporting checks.

[13] Web Appendix Section D.1 explains these relationships and provides historical examples.

operation sustains demand for power and cooling throughout the day (Greenberg et al. 2006). Warehouses nevertheless involve substantial investment and energy use: Dollar Tree described a roughly $100 million distribution center, while Lowe's discussed reducing diesel costs through shorter delivery routes (Dollar Tree 2012b, Lowe's 2008).[14]

NETS identifies general warehousing and storage establishments using NAICS 493110. Warehouse-entry counties have no earlier warehouse activity and no data center activity in the 1990–2014 NETS history; comparison counties have neither industry during that period. These restrictions leave 41 warehouse-entry and 1,526 comparison counties. Repeating the same analysis, but for warehouse entry, we estimate a statistically insignificant 0.5% spending decline (SE = 0.036 in log points, $p = .887$), compared with 12.3% after data center entry. Table 6 reports annual spending, purchase occasions, and spending per purchase occasion. The two industry estimates use different county samples. The smaller and insignificant warehouse estimate is consistent with electricity expenses contributing to the data center response and provides further evidence that the decline in grocery spending is more apparent after data center entry.

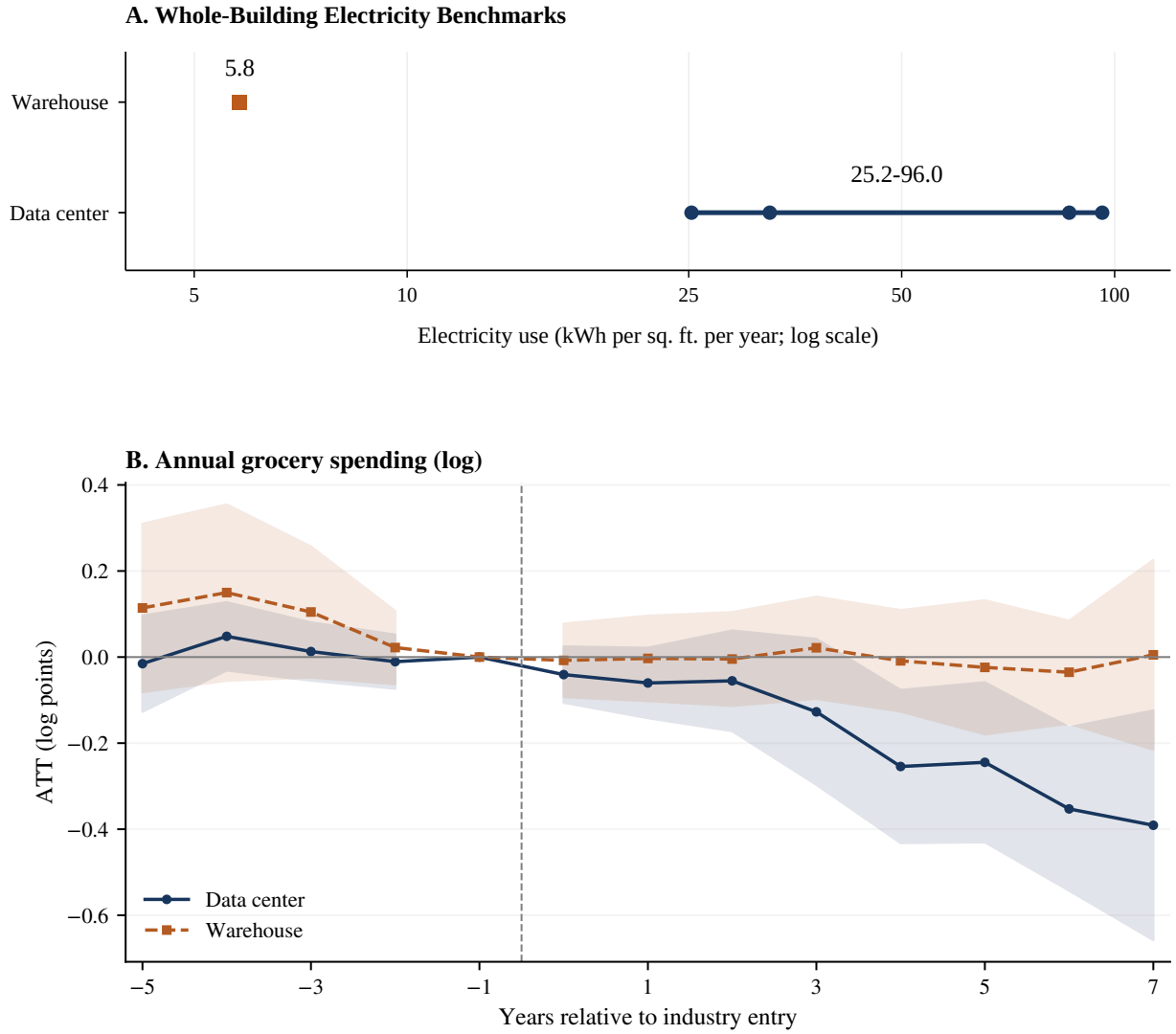
Table 6: Warehouse Entry and Household Purchasing

Dependent variable	ATT	SE	p-value
Annual grocery spending (log)	−0.005	0.036	.887
Number of purchase occasions (log)	−0.066	0.059	.267
Spending per purchase occasion (log)	0.061	0.053	.255

Notes. Callaway–Sant'Anna average post-entry effects for 41 warehouse-entry and 1,526 comparison counties (14,353 county-years, 2004–2014). Warehouse entry is the first county appearance of NAICS 493110, subject to the industry-history restrictions used for data center entry. Entry counties have no data center history; comparison counties have neither industry in NETS during 1990–2014. Outcomes are county-year means of household logs. Standard errors use 1,000 county-clustered multiplier-bootstrap draws; p-values are two-sided and unadjusted. Joint Wald tests of event-time coefficients for years −7 through −2, relative to year −1, yield $p = .319$ for spending, $p = .991$ for purchase occasions, and $p = .072$ for spending per purchase occasion.

[14] Web Appendix Table A9 compares these development accounts with data center investments in substations and transmission.

Figure 5: Electricity Intensity and Household Spending After Facility Entry



Notes. Panel A uses total building floor area. Data center points annualize the four reported building power densities (31–118 W/m^2) at 8,760 hours; the line spans the observations from the end of 2000 (Mitchell-Jackson et al. 2003, Table 5). These building totals include support space and other uses. The warehouse point is the 2012 CBECS nonrefrigerated intensity of 5.8 kWh per square foot (U.S. Energy Information Administration 2016). Panel B shows separate Callaway–Sant'Anna event studies of household log grocery spending: data centers, 52 entry and 2,160 comparison counties; warehouses, 41 entry and 1,526 comparison counties. Year −1 is the reference. Shading denotes pointwise 95% confidence intervals with county-clustered inference. The two industry estimates use different county samples.

7 Discussion

7.1 Contextualizing the Purchasing Response

Our results show that annual grocery spending declines by 12.3% after data center entry. How does this estimate translate to economic impact? The estimated annual decline is approximately $419 per household (Web Appendix Table A3). Annual grocery spending declines primarily through fewer purchase occasions, while the change in spending per purchase occasion is statistically insignificant. The largest estimated decline in the number of purchase occasions occurs among small purchases, and the estimated spending decline is larger for nonfood products. These patterns show how purchase occasions and spending per purchase occasion can respond differently to local development. Households facing higher expenses may preserve food consumption through closer meal planning, greater use of leftovers, and reduced spoilage. Yu and Jaenicke (2020) estimate household food waste using a model of household food production. At the retail level, Sanders (2024) finds that dynamic pricing can reduce waste and increase consumer surplus in an estimated model, while Hansen et al. (2024) show how consumers' attention to expiration dates affects product choices and retailer waste.

7.2 Retailer and Manufacturer Implications

Retailers planning inventory need to track purchase occasions alongside spending per purchase occasion. The annual spending decline in our estimates comes primarily from fewer purchase occasions, so monitoring spending per purchase occasion alone could miss a reduction in demand. Households also use fewer retailers, with little evidence of larger purchases of storable products per occasion. Firms adjust product lines and assortments in response to prices and differences in demand (Draganska and Jain 2006, Draganska et al. 2009, Hwang et al. 2010). Combining these household measures with inventory and assortment data would help identify which retail adjustments improve performance.

7.3 AI Exposure and Local Welfare

We compare the geography of the spending decline with subsequent occupational exposure to AI. The index combines occupational AI exposure with the county's employment mix, building on Felten et al. (2018). Splitting entry counties at the median of the published 2019 AI Geographic Exposure (AIGE) index yields spending declines of 22.9% in the high-exposure group and 1.3% in the low-exposure group.

Funding and Competing Interests. All authors certify that they have no affiliations with or involvement in any organization or entity with any financial interest or non-financial interest in the subject matter or materials discussed in this manuscript. The authors have no funding to report.

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A Documented Data Center Facilities in NETS

Table A1 extends Figure 3 with company and address matches to operator specifications and public records. We select documented facilities from NETS records first observed during 2000–2015.

Table A1: Additional Physical Data Centers Matched to NETS Records

NETS Company and Facility	Matched Address	Documentary Evidence
Internap Corporation Fisher Plaza, Seattle	140 4th Ave. N, Suite 200, Seattle, WA	January 2014 operator specification identifies 26,000 square feet of colocation space and names Seattle City Light.
SoftLayer Technologies Inc. Infomart, Dallas	1950 N Stemmons Fwy., Dallas, TX	December 2007 city council record reports 12,690 square feet of operations and more than 8,000 servers at this address.
Cyrus Networks LLC Galleria, Houston	4211 Southwest Fwy., Suite B, Houston, TX	The 2012 property schedule lists the Galleria data center building at 4211 Southwest Freeway.
Cyrus Networks LLC Greenspoint, Houston	12085 North Fwy., Houston, TX	The 2012 property schedule lists a leased data center at this address.
CyrusOne Inc. Bryan Street, Dallas	2323 Bryan St., Dallas, TX	The 2012 property schedule lists a leased data center at this address.
Cyrus Networks LLC Met Center I, Austin	7401 E Ben White Blvd., Austin, TX	The 2012 property schedule lists a leased data center at this address.
SunGard Availability Services LP San Ramon	2481 Deerwood Dr., San Ramon, CA	A 2021 site assessment documents cloud-storage and backup-server operations and SunGard directory entries in 2005, 2010, and 2014.

Notes. The examples come from the NETS establishment source file before the county-treatment and household-panel restrictions. Company and street abbreviations are expanded for readability. Cyrus Networks and CyrusOne records are matched to CyrusOne's property schedule. The Galleria match identifies the building; the schedule does not identify Suite B separately. Document dates describe the evidence used for each match. The SunGard assessment provides retrospective historical occupancy evidence.
Sources. Internap: Internap Network Services Corporation (2014), technical specification. SoftLayer: City of Dallas (2007), addendum item 13. CyrusOne properties: CyrusOne LP and Data Centers South Inc. (2012), Exhibit A. SunGard: ENGEO Incorporated (2021), pp. 6–7.

B Homescan Sample and Measures

B.1 Mobility and Panel Composition

We exclude both annual records surrounding a county or ZIP-code change, including transitions separated by gaps in observation. Households can still enter or leave the panel after this screen. Table A2 compares households in our counties before screening with the national

Homescan panel over the same years. The samples have similar household sizes and shares with children. Households in the study counties have lower average income.

Table A2: Household Characteristics in our Counties and National Homescan Panel

Household characteristic	Our counties	National panel	Difference	SE	p-value
Household income ($)	46,348	53,863	$-7,515$	458.48	$< .001$
Household size	2.554	2.556	-0.002	0.018	.911
Households with children (%)	33.8	33.2	$+0.7$	0.47	.152

Notes. Means use the pre-screening household sample and NielsenIQ projection factors, pooling 2004–2014 panelist records. Our counties include 152,289 household-years in 52 entry and 2,160 never-entry counties; the national panel includes 607,464 household-years. Income uses bracket midpoints, top-coded at $100,000 in all years; children are under age 18. Differences are our counties minus the national panel, computed before rounding. The difference and SE for households with children are in percentage points. Standard errors cluster by county and household and account for the study sample being part of the national panel; p-values are two-sided.

Observed Residential Mobility. Before household screening, a confirmed cross-county move requires both county and ZIP code to change between consecutive annual records; a ZIP change within the same county is a within-county move. Same-ZIP county-code changes are classified separately as administrative recodes. To count each household once, we group it by the first study county in which it appears and follow its subsequent records through 2014, including records outside the study counties. Among households with at least one consecutive-year pair with complete addresses, 184 of 2,094 entry-county households (8.79%) and 1,651 of 24,815 never-entry-county households (6.65%) have a confirmed cross-county move. The corresponding shares with a within-county ZIP change are 8.74% and 3.04%; households can experience both types of move. The shares with any ZIP change are 16.14% and 9.22%.

These household shares differ from annual move rates. Assigning each observed transition to its origin county, we observe cross-county moves in 200 of 8,636 entry-county transitions (2.32%) and 1,683 of 99,897 never-entry-county transitions (1.68%) over 2004–2013, with destinations observed through 2014. These descriptive comparisons pool pre-entry and post-entry years.

Entry and Annual Mobility. We aggregate the consecutive-year move indicators to county-year shares and estimate Callaway–Sant'Anna effects using the main entry dates and never-entry comparison counties. Cross-county and within-county move rates use only households observed in both years with complete addresses. The cross-county effect is -2.11 percentage points (SE = 2.62, $p = .419$). Within-county ZIP changes increase by 0.76 percentage points (SE = 0.28, $p = .007$), while the effect on any ZIP change is -1.36 percentage points (SE = 2.73, $p = .620$). Cross-county mobility shows no statistically significant change, while within-county mobility increases. Using all origin-year households, the estimated change in next-year panel observation is $+0.02$ percentage points (SE = 4.95, $p = .997$). Standard errors use 1,000 county-clustered multiplier-bootstrap draws, and p-values are two-sided.[15]

[15]The move-rate models use 18,403 county-years, 46 entry counties, and 2,155 never-entry counties. The panel-observation model uses 19,007 county-years, 46 entry counties, and 2,159 never-entry counties. The six counties

B.2 Estimation in Original Units

Panels A and B of Table A3 report purchasing effects in nominal dollars. The sample, product coverage, coupon deductions, entry dates, cohort–year comparisons, and aggregation follow the main analysis. Annual grocery spending is the weighted county-year mean defined in equation (2). For spending per purchase occasion, we divide each household's annual spending by its number of occasions and average these ratios within county-year using projection factors. Annual dollar spending is also separated into food and beverages, nonfood products, and unassigned products. Effects are measured in dollars per household-year or per purchase occasion. The estimated annual spending decline is \$419.22 per household-year.

Panel C reports the storable-product and retailer outcomes in Table 5 without taking logs. Storable-product spending, purchase occasions with storable products, and the number of retailers are annual household measures averaged within county-year. Home–store distance averages each household's mean distance across purchases with available locations, measured in miles between home and store zip centroids. These county-year means use the same household weights and observations as the corresponding transformed outcomes.

For storable-product packages per purchase occasion, let U^s_{ict} denote the number of packages purchased by household i and N^s_{ict} the number of its purchase occasions containing a storable product. We construct the county-year outcome as

$$Q^s_{ct} = \frac{\sum_{i\in(c,t)} w_{it} U^s_{ict}}{\sum_{i\in(c,t)} w_{it} N^s_{ict}}. \tag{6}$$

The ratio uses county-year package and occasion totals with the same household weights. Package counts sum units whose sizes vary across products.

The Callaway–Sant'Anna estimates in original units yield declines of \$359.37 in annual storable-product spending (SE = 158.31, $p = .023$), 21.65 purchase occasions with storable products (SE = 10.47, $p = .039$), and 1.55 retailers per household-year (SE = 0.77, $p = .043$). Storable-product packages per purchase occasion increase by an estimated 0.320 (SE = 0.375, $p = .392$). Home–store distance increases by an estimated 1.97 miles (SE = 1.07, $p = .065$). The distance estimate in original units is positive, whereas the $\log(1+x)$ estimate in Table 5 is close to zero; neither is statistically significant at the 5% level. The two specifications estimate effects on different outcome scales. For all estimates in Table A3, standard errors use 1,000 county-clustered multiplier-bootstrap draws, and p-values are two-sided.

entering in 2014 are excluded from these models because post-entry transitions require 2015 records; they are not used as controls. Missing next-year records enter the panel-observation outcome but not the move-rate denominators.

Table A3: Household Purchasing Effects in Original Units

Dependent variable	ATT	SE	p-value
Panel A. Overall purchasing			
Annual grocery spending	−419.22**	182.96	.022
Spending per purchase occasion	1.02	1.49	.495
Panel B. Annual spending by product group			
Food and beverage spending	−193.80	120.57	.108
Nonfood spending	−225.45**	94.75	.017
Panel C. Storable-product purchases, number of retailers, and home–store distance			
Storable-product spending ($)	−359.37**	158.31	.023
Number of purchase occasions with storable products	−21.65**	10.47	.039
Storable-product packages per purchase occasion	0.320	0.375	.392
Number of retailers	−1.55**	0.77	.043
Home–store distance (miles)	1.97*	1.07	.065

Notes. All outcomes are in original units; dollar amounts are nominal. Unassigned products remain in total spending but are not assigned to either product group. County-year means use NielsenIQ projection factors. Spending per purchase occasion averages each household's annual spending divided by its number of purchase occasions. Panel C uses the outcomes in Table 5 without log transformations. Storable-product packages per purchase occasion use only occasions containing storable products, as in equation (6). Number of retailers is based on distinct retailer codes within the year; home–store distance is in miles between home and store zip centroids. Callaway–Sant'Anna estimates average post-entry effects across 52 entry and 2,160 never-entry counties (21,030 county-years, 2004–2014); distance uses 20,525 county-years and 2,150 never-entry counties. Standard errors use 1,000 county-clustered multiplier-bootstrap draws; p-values are two-sided and unadjusted. Panel B separates food and beverages from nonfood products; unassigned products complete the total. $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

C Additional Validity and Sensitivity

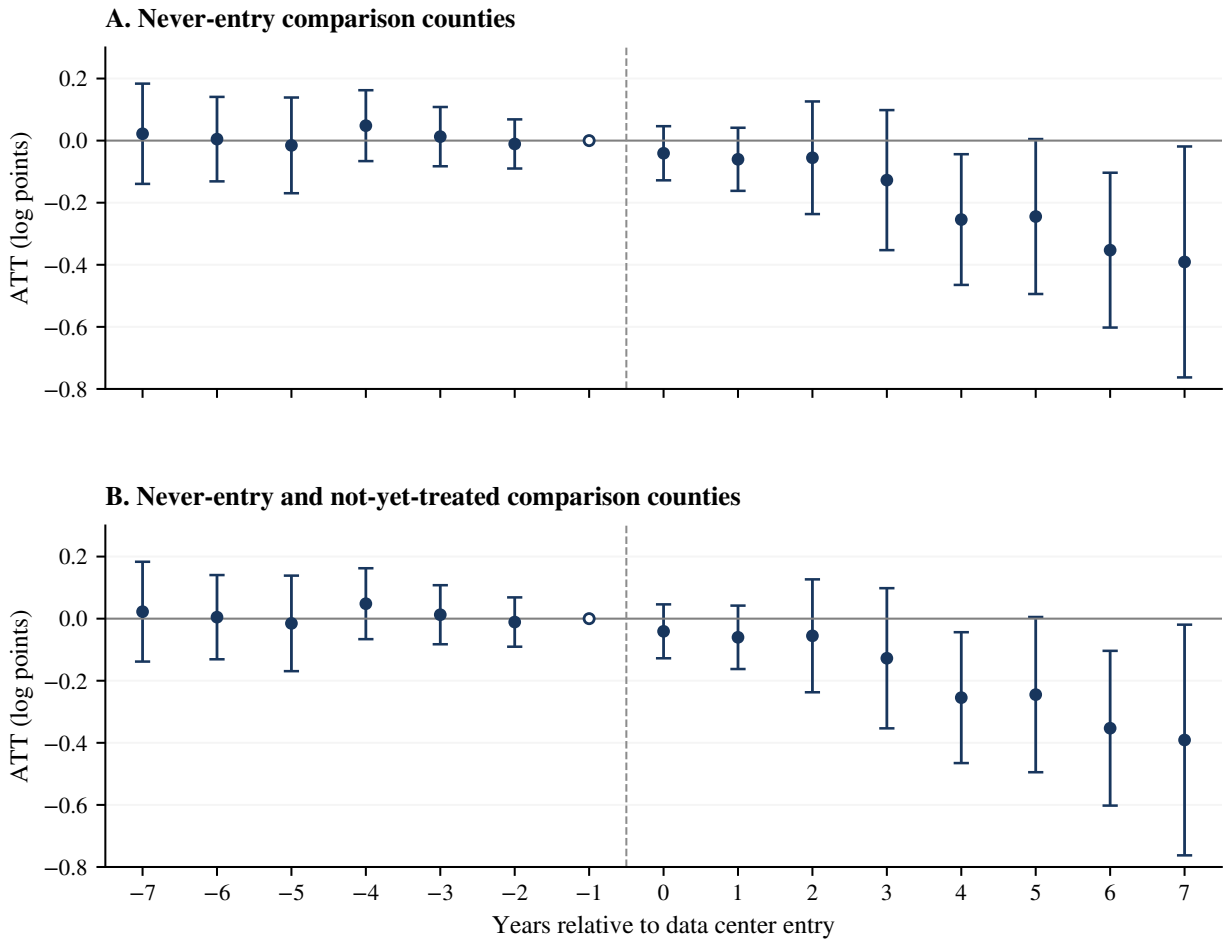
C.1 Pretreatment Diagnostics

Joint tests use the influence functions of the aggregated event-time estimates. The common-base test covers six coefficients, $e = -7, \ldots, -2$, and yields $\chi^2(6) = 3.65$ ($p = .723$). The covariance matrix has full rank. For purchase occasions, the common-base test yields $p = .220$; for spending per purchase occasion, it yields $p = .729$. The `did` package also tests all pretreatment cohort–year effects jointly. That larger covariance matrix is singular in the baseline, so this test is unavailable.

C.2 Alternative Comparison Counties

Allowing later-entry counties to serve as controls before their own entry yields the same 12.3% spending decline as the baseline comparison with never-entry counties. Restricting comparisons to the 1,581 never-entry counties in entry states yields a 12.1% decline. Excluding 185 counties that border an entry county, using the 2010 Census County Adjacency File (Bureau 2010), leaves 1,975 comparison counties and yields a 12.4% decline.

Figure A1: Grocery Spending Under Alternative Comparison Groups



Notes. Points are Callaway–Sant'Anna estimates for mean household log grocery spending relative to year −1. Bars are 95% simultaneous confidence intervals across the displayed nonreference years, obtained from county influence functions using 1,000 multiplier-bootstrap draws separately for each panel. Panel A uses never-entry counties; Panel B also uses future-entry counties while they remain untreated. The joint pretreatment test for years −7 through −2 yields $p = .723$ in each panel.

C.3 County and Utility Geography

We examine whether the spending estimate depends on comparison counties sharing electricity providers with entry counties. Annual EIA-861 records identify providers with distribution equipment in each county during 2004–2014 (U.S. Energy Information Administration 2014). We include residential bundled or delivery service, short-form customer records, and distribution activity without exclusively energy-only sales. Each entry county's providers are the utility-state pairs listed in its entry year. Beginning that year, a comparison county shares a listed provider if its annual records contain one of those pairs.

Both checks require provider records in every year from 2006–2014. The first excludes comparison counties sharing a listed provider with an entry county during that period. The

second excludes comparison counties sharing a provider with an entry county anywhere in their 2004–2014 histories. Table A4 shows that both exclusions leave the spending estimate close to the baseline. The entry counties and outcome remain fixed while the comparison group changes.

Table A4: Spending Estimates under Alternative Utility Comparisons

Comparison group	ATT	SE	p-value
Annual grocery spending (log)			
All never-entry counties	-0.131^{**}	0.052	.012
No shared listed provider during 2006–2014	-0.129^{**}	0.053	.015
No shared provider across 2004–2014 records	-0.128^{**}	0.054	.018

Notes. Callaway–Sant'Anna estimates of average post-entry effects retain 52 entry counties. The three comparison groups contain 2,160, 991, and 975 never-entry counties, respectively. The outcome is the county-year mean of household log annual grocery spending, weighted by NielsenIQ projection factors. Both provider screens require observed mapping in each year from 2006 through 2014. A shared provider is a shared utility-state pair in the EIA-861 provider listings. Estimates use the unbalanced county panel and the year before entry, $g-1$, as the reference. Standard errors use 1,000 county-clustered multiplier-bootstrap draws; p-values are two-sided and unadjusted. $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

C.4 Specification Sensitivity

Table A5 reports estimates using alternative comparison counties (Panel A), estimators (Panel B), aggregation of effects (Panel C), and entry definitions or dates (Panel D).

Table A5: Sensitivity of the Grocery-Spending Estimate

Dependent variable	Specification	ATT	SE	p-value
Panel A. Comparison counties				
Annual grocery spending (log)	All never-entry counties	-0.131^{**}	0.052	.012
—	Never-entry and not-yet-treated counties	-0.131^{**}	0.052	.011
—	Never-entry counties in entry states	-0.129^{**}	0.056	.021
—	Nonadjacent never-entry counties	-0.133^{**}	0.053	.012
Panel B. Estimator				
Annual grocery spending (log)	Callaway–Sant'Anna	-0.131^{**}	0.052	.012
—	Sun–Abraham	-0.132^{***}	0.046	.004
—	Borusyak–Jaravel–Spiess	-0.140^{***}	0.040	$< .001$
—	Gardner	-0.140^{***}	0.046	.002
Panel C. Aggregation				
Annual grocery spending (log)	Cohort-weighted	-0.087^{**}	0.037	.019
—	Calendar-year-weighted	-0.118^{*}	0.062	.057
—	Event years 0–5	-0.130^{**}	0.056	.020
—	Common follow-up through year 5	-0.175^{**}	0.076	.020
Panel D. Entry definition and timing				
Annual grocery spending (log)	Switch into classification and remain	-0.156^{**}	0.071	.028
—	Entry date one year earlier	-0.099^{**}	0.039	.012
—	Entry date one year later	-0.123^{**}	0.063	.049

Notes. A dash repeats the preceding outcome within a panel. The baseline averages household log spending within county-year using NielsenIQ projection factors; controls are never-entry counties. Panel C averages effects by cohort, calendar year, or event time (years 0–5). The common-follow-up summary uses cohorts observed through year 5. Standard errors are county-clustered using each estimator's inference procedure. Most rows use 52 entry counties and 21,030 county-years. The state and nonadjacent comparisons use 15,541 and 19,197 county-years. The common-follow-up summary uses 20 entry counties. Panel D's switch-into-classification row uses 17 entry counties and 20,651 county-years; the later-date row uses 46 and 20,966. Earlier/later dates are timing-sensitivity checks, retaining actual post-entry observations. $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$. All p-values are two-sided.

Across four staggered difference-in-differences estimators, estimated spending declines are 12.3%–13.1% (H. C. and Zhao 2020, Callaway and Sant'Anna 2021, Gardner 2022, Borusyak et al. 2024). Cohort aggregation averages post-entry effects within each cohort and then weights by cohort size. Calendar-year aggregation instead averages across years. The two event-time summaries cover $e = 0, \ldots, 5$. Using all available cohorts at each event year yields a 12.2% decline; restricting the analysis to the 20 counties observed through $e = 5$ yields a 16.1% decline. These summaries differ in both weights and contributing counties.

C.5 Entry Histories and Timing

Restricting entry to establishments that switch into and remain in the industry retains 17 entry counties and yields a 14.4% spending decline. Moving entry one year earlier yields a 9.4% decline; moving it one year later yields an 11.6% decline (Table A5, Panel D).

Adding fixed pretreatment county characteristics leaves the spending estimate negative on the same complete-case sample (Table A6). The timing placebo uses only observations before actual entry and yields a statistically insignificant estimate (Table A7). Table A8 connects each identification condition to its empirical check.

Table A6: Grocery Spending with Pretreatment County Controls

Dependent variable	Controls	ATT	SE	p-value
Annual grocery spending (log)	None	-0.120^{**}	0.055	.029
Annual grocery spending (log)	Population, income, employment, establishments	-0.112^{**}	0.055	.040

Notes. Controls are fixed at county averages for 2004–2005 and enter the Callaway–Sant'Anna outcome-regression specification. Both specifications use the same complete-case sample of 51 entry counties and 20,698 county-years. The outcome averages household log spending within each county-year using NielsenIQ projection factors. Standard errors are clustered by county. $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$. All p-values are two-sided.

Table A7: Pretreatment Timing Placebo

Dependent variable	Assigned entry	ATT	SE	p-value
Annual grocery spending (log)	$G - 2$	-0.020	0.034	.560

Notes. Actual entry is G. The placebo compares $G - 2$ and $G - 1$ with $G - 3$, excluding all observations at or after actual entry. The 2008–2014 cohorts include 43 entry and 2,160 never-entry counties (20,772 county-years). Estimates use Callaway–Sant'Anna with county-clustered inference. $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$. All p-values are two-sided.

Table A8: Identification Conditions and Empirical Checks

Condition or concern	Design or check	Reference
Identification conditions and comparison design		
Staggered entry	First entry starts treatment, which remains on.	Section 4.5
Limited anticipation	Compare true pre-entry years; separately assess entry-date sensitivity.	Table A7; Table A5, Panel D
Parallel untreated trends	Examine common-base pre-entry changes.	Figures 4 and A1
Comparison counties	Add baseline controls and vary comparison counties.	Table A6; Table A5, Panel A
Treatment definition and spillovers	Vary entry definitions and exclude neighboring comparisons.	Table A5, Panels A and D
Inference and measurement checks		
Within-county dependence	Cluster inference at the county level of treatment assignment.	Section 4.5

Notes. The checks assess different conditions. Pre-entry diagnostics and alternative specifications provide complementary evidence; county clustering addresses inference.

D Household Bills and Aggregate Electricity Prices

D.1 Household Bills and Cost Recovery

Electricity tariffs determine how utility costs reach individual households. Bills combine fixed charges, usage charges, adjustments, and credits, while average residential revenue per MWh divides total revenue by electricity sales. When revenue and sales cover the same accounts, period, and service components, this ratio multiplied by average use recovers the average bill. EIA revenue includes service charges and current-period billing adjustments (U.S. Energy Information Administration 2007, 2026). The aggregate measure therefore describes charges per unit of electricity across accounts; household expenses also depend on usage and the distribution of those charges.

Rate design can raise some households' bills while leaving average revenue per MWh unchanged. Under a common two-part tariff and unchanged use, a revenue-neutral increase in the fixed charge paired with a lower usage rate raises bills for households consuming less than the average and lowers bills for those consuming more (Borenstein 2016, Lazar et al. 2020). Wisconsin Public Service's January 2014 single-phase urban residential schedule raised the equivalent monthly customer charge from \$5.70 to \$10.40 and lowered the energy rate from 12.061 to 11.143 cents/kWh (Public Service Commission of Wisconsin 2013). These components increased monthly bills by \$2.41 at 250 kWh and reduced them by \$4.48 at 1,000 kWh, excluding other adjustments and taxes. Households can also conserve electricity after a tariff increase, limiting spending while reducing electricity services (Deryugina et al. 2020).

Infrastructure costs enter household bills through approved recovery schedules and allocations across customer classes. For example, Oncor's advanced-metering rider charged covered residential accounts \$2.21 per month beginning in January 2009 (Public Utility Commission of Texas 2008). Construction accounting also affects timing. Georgia Power's 2011 annual report explains that capitalized construction financing costs raise later revenue requirements through the rate base and depreciation; recording those costs does not itself collect revenue. Its nuclear construction recovery tariff instead began collecting financing costs in January 2011 (Georgia Power Company 2012). These examples show how investment costs can appear in current charges or later bills, depending on the approved arrangement.

Utilities' pricing decisions further determine the extent of pass-through. Forest City's municipal system raised electric rates by 4.5% in January 2014 following a Duke wholesale-rate increase, then absorbed the 2015 wholesale increase, projected at less than 1%, without raising retail rates (Town of Forest City 2015, p. 3). These tariff examples document cost recovery without attributing the charges to data centers.

Utilities can recover infrastructure costs from customers across a service territory or wider region. PJM rules approved in 2013 generally allocated half the cost of eligible transmission projects at 345 kV and above across the region, with the remainder allocated by benefits (Virginia State Corporation Commission 2013). Dominion recovered its transmission revenue requirement through base rates and Rider T1 (Virginia State Corporation Commission 2014). These allocations can raise bills in both entry and comparison counties, narrowing differences in household expenses between the groups. Delayed recovery can place charges beyond the observation period. Connecting local demand to household expenses therefore requires the recovery schedule, the customers charged, and their tariffs and electricity use.

D.2 Facility Development and Infrastructure

Data center and warehouse projects both require substantial investment to serve customers beyond their immediate locations. The projects in Table A9 illustrate the difference in infrastructure: substations and transmission for data centers, and regional distribution capacity for warehouses. Lowe's discussion of delivery routes also documents diesel use in warehouse logistics. Dollar Tree linked its Northeast investment to store expansion and the tradeoff between facility size and delivery distance, showing how regional retail growth can shape warehouse siting (Dollar Tree 2012a).

Table A9: Data Center and Warehouse Development

Company and Location	Disclosure	Development and Infrastructure
Panel A. Data Centers		
Equinix, LA4 El Segundo, California	Feb. 2008	Committed to an on-site substation before construction was scheduled to begin (Equinix 2008).
DuPont Fabros Ashburn, Virginia	May 2009	Regulators approved a 230 kV transmission line and 230/34.5 kV substation to serve campus expansion (Virginia State Corporation Commission 2009, pp. 319–321).
CyrusOne Chandler, Arizona	May 2012	Announced an on-site substation before groundbreaking for a planned 110 MW campus (CyrusOne 2012).
Panel B. Warehouses and Distribution		
Target Distribution network	Feb. 2007	Described typical distribution-center investment above $100 million; internal store supply and new-store growth involved separate decisions (Target 2007).
Dollar Tree Windsor, Connecticut	Aug. 2012	Described a one-million-square-foot facility under construction for Northeast stores, with anticipated investment of approximately $100 million (Dollar Tree 2012b).
Dollar General Bessemer, Alabama; network expansion	June 2012; Dec. 2013	Reported that Bessemer supplied about 700 stores; later explained that new locations reduce transportation costs after initial operating inefficiencies (Dollar General 2012, 2013).
Lowe's Distribution network	Aug. 2008	Described shorter delivery routes, fuller truck loads, and return-trip collections as ways to offset higher diesel costs (Lowe's 2008).

Notes. Selected historical disclosures; dates include the Ashburn regulatory order and otherwise refer to company announcements or earnings calls. Warehouse companies appear in the project's parent-company records; named projects are not individually matched placebo events. Investment amounts are nominal. The 110 MW figure is announced campus capacity; MW denotes megawatts and kV denotes kilovolts. Dollar General's 2013 statement concerns new locations generally. Residential cost allocation depends on tariffs and regulatory decisions.