# Valuing Power Outages: Evidence from U.S. Housing Transactions

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**Abstract**

Utility regulators lack credible behavioral estimates of household willingness to pay for power reliability to benchmark grid investment decisions. I compare individual home sale prices on either side of electric utility service boundaries within the same county, combining transaction data from 2015 to 2022 with automated outage records compiled from utility monitoring systems. Each additional hour of chronic outage exposure reduces home sale prices by 1.3 to 2.3 percent. Applying the Bishop and Murphy (2019) dynamic hedonic correction implies a marginal willingness to pay of $539 to $939 per household per year for an avoided hour of chronic outage. Holding the set of utilities fixed and changing only the outage measure, the self-reported data that utilities file with regulators cannot reproduce this result, and the two measures barely agree on the cross-boundary reliability gap. The choice of outage data, not the research design alone, determines what one finds. The estimate is a marginal willingness to pay for homes on the higher-outage side of the boundaries I study, in counties served by more than one utility. Within that scope the results provide a housing-market benchmark for reliability that regulatory reviews and conventional estimates do not currently capture.

**JEL codes:** L94; Q41; Q48; R31; D12

**Key Words:** power reliability; hedonic valuation; regression discontinuity; housing markets; willingness to pay

# 1 Introduction

Power outages impose large and measurable costs on households and the broader economy. Households face direct costs from the disutility of lacking access to basic household needs during outages as well as indirect costs from infrastructure and network services being out of service (Vennemo et al., 2022). Unreliable power supply has also been shown to reduce industrial productivity, employment, and human capital formation (Dinkelman, 2011; Lipscomb et al., 2013; Burlando, 2014; Fisher-Vanden et al., 2015; Allcott et al., 2016; Lee et al., 2020). It follows that households may price this amenity into home values. With growing future reliability concerns (North American Electric Reliability Corporation, 2023) from aging infrastructure and growing demand, the 2021 Infrastructure Investment and Jobs Act targets $13 billion toward modernizing the electricity grid in an effort to mitigate the costs imposed on households and the greater economy from outages (Borenstein et al., 2023). This motivates the need for understanding the value households place on reliability in order to justify or reject additional spending.

Determining how much households value reliable electricity is essential for evaluating the social return to grid investment. Utility regulators and the Federal Energy Regulatory Commission (FERC) use measures of outage duration and frequency in rate cases and reliability planning but have few reliable behavioral estimates of household marginal willingness to pay (MWTP) to benchmark against. Without a credible MWTP estimate, it is impossible to determine whether a dollar of grid hardening investment is welfare improving or not.

I compare houses within the same county on opposite sides of electric utility service boundaries to recover estimates of household MWTP to avoid power outages. Adjacent homes on opposite sides of a utility service boundary share the same taxes, schools, local services, weather, terrain, and neighborhood. They are served by utilities with different power reliability. Using individual home transactions instead of an aggregate price index allows me to control for property characteristics and neighborhood characteristics. I provide evidence that observable characteristics change smoothly at utility boundaries, which emphasizes the need for transaction-level observations and sub-county treatment variation to identify the relationship between home prices and power reliability.[1] This paper makes several contributions. The central one is a measurement result. Holding the set of utilities fixed and changing only the outage measure, I show that the self-reported reliability data utilities file with regulators cannot reproduce the capitalization that automated detected

[1] Bishop et al. (2020) indicate best practices for hedonic valuation studies include utilizing transaction level price data and ideally Census Tract-level or more granular controls.

outage data recover, and that the two measures barely agree on the reliability gap the boundary design uses. This reconciles my estimates with prior work built on the self-reported series and shows that the choice of outage data, not the research design alone, drives what one finds. Three further contributions enable and extend this result. I bring detected power outages together with transaction-level housing data for the first time in this literature. I use a within-county utility service boundary so that reliability, rather than the bundle of attributes that changes at a county line, is what varies across the comparison. Finally, I recover a forward-looking willingness to pay whose magnitude is in line with estimates from studies of battery and backup power adoption.

I find that chronic power outages are negatively capitalized into home prices at the utility boundary. I estimate this effect by combining data on individual home transactions with data on automated system reported power outages for the contiguous United States. The nationwide data span 2015 to 2022, a period in which the country sees growing electricity demand from increasing electrification of transportation and heating along with aging power infrastructure. I use coarsened exact matching (CEM) to estimate spatial discontinuity models of home sale prices across utility borders within the same county. My results imply that a one-hour increase in chronic outage exposure is associated with a 1.3 to 2.3 percent decrease in home sale price on average, and this association is stable across boundary bandwidths. Next, I apply the Bishop and Murphy (2019) forward-looking correction and find a MWTP of $539 to $939 per household per year for an avoided hour of chronic outage evaluated at the mean house price in my sample. This annual household expenditure on grid reliability brackets the $776 per household per year found by Brehm et al. (2024) looking at backup battery adoption in California after severe outages. Ewing et al. (2026) finds a static capitalization effect of chronic power outages that is substantially lower than my results.

Motivated by these findings, I consider how to reconcile the difference estimated in my main results and those in Ewing et al. (2026). I initially adopt the same empirical strategy detailed above but swap the outage data for the same data source used by Ewing et al. (2026). I find a small negative effect of outages on home prices driven by severe outage events. This provides evidence that self-reported outage data differs from system detected outage data used in my main specification and shows that structural differences in the utilities that report reliability contribute to this result. I then utilize a county border comparison as an analog to the county-level analysis done in Ewing et al. (2026). Again, I find small but statistically significant effects of severe outages on home prices across all bandwidths. I show that CEM on property characteristics is not able to overcome the bundling of changes in the many priced amenities that comes with county border differences.

I also analyze the potential for a price discount for homes on the higher outage side of the utility border. I find a discrete price decrease of 1.1 to 1.3 percent for homes that fall in the higher outage utility service area. With utility self-reported outage data there is no statistically significant relationship between home prices in high versus low outage utilities. I interpret this to mean that the self-reported outage data is too noisy to produce a clear signal. With county outage data, I recover a statistically significant 1.5 to 2.4 percent price discount for homes on the higher outage side of county borders, a confounded contrast with the continuous outage effect I unpack in the results.

The contrast in results for self-reported and detected outage data is due to the difference in samples produced. Among overlapping utilities in the sample, the outage rates reported are similar. However, using the detected data allows me to include nearly double the amount of utilities covered in the main sample. These utilities are substantially smaller in size and number of customers served compared to the reporting firms.

I retain the utility level analysis with detected outage data as the preferred specification through the remainder of the paper. I find the estimates are robust to a battery of geographic restrictions and alternative matching procedures. I find that homes in the highest decile of price distribution explain much of the price responsiveness. I also show that much of the pricing result is driven by border pairs in the upper quartiles of the utility pair outage gap. One channel that may contribute to higher valuations is homebuyers who work from home, who are more sensitive to short and frequent outages. I provide suggestive evidence that census tracts with higher shares of people who work from home more severely discount homes with higher outage rates.

Three strands of prior work speak to the value of electricity reliability but none produces a credible nationwide estimate of household willingness to pay in the United States. There are a number of studies that have utilized stated preference methods to elucidate a WTP to avoid outages in both developing (Meles et al., 2021; Aweke and Navrud, 2022) and developed (Carlsson and Martinsson, 2008; Praktiknjo et al., 2011; Ozbafli and Jenkins, 2016) economies. Recent revealed preference studies have looked at how households invest in adaptation technologies like generators, solar, or batteries (Brehm et al., 2024; Brown and Muehlenbachs, 2024; Harris, 2024). However, these studies are restricted by scope of geography, low-frequency high-severity weather shocks, or both.[2] All

[2] Brehm et al. (2024) and Brown and Muehlenbachs (2024) look at household investment in batteries in California. Harris (2024) utilizes generator purchase data in the Southeast U.S. in the few days leading up to and after hurricanes.

of the WTP estimates from the aforementioned studies provide evidence that households value reliability, but estimates are sensitive to context and timing. More recently, Ewing et al. (2026) provides the first evidence of power reliability being capitalized into home values.

The Ewing et al. (2026) study is the closest related to this paper and finds that chronic outage exposure reduces county-level housing prices but those estimates face three methodological limitations this paper addresses. Using the Federal Housing Finance Agency (FHFA) county-level House Price Index (HPI), they find that a one-standard deviation[3] increase in power interruption is associated with a 0.25% decrease in home values driven by minor outage events.[4] Using the FHFA county-level housing price data does not allow the researchers to control for neighborhood characteristics, conflating the capitalization of correlated local amenities which may overstate the value of reliability. The outage data come from Energy Information Administration (EIA) Form 861, in which utilities report the system average interruption duration index (SAIDI) directly to regulators. Calculating this measure is left to the utility without standardization[5] and reporting is voluntary, so the reported measure covers a self-selected and systematically larger set of firms that is not measured on a common basis across utilities. Using a dynamic panel with county and year fixed effects (FE), the authors compare reliability within counties across time, capturing the effect of contemporaneous changes to reliability. The capitalization of an amenity into home prices is inherently a long term purchase where buyers are likely considering the future expectation of that amenity, not the instantaneous value. This potentially understates households' true valuation of power outages. Ewing et al. (2026) does not apply the Bishop-Murphy forward-looking dynamic correction. If power outage differences across locations are persistent, the static estimate understates the full capitalized value that forward-looking buyers internalize at the time of purchase. Taken together, this study provides some evidence that homebuyers value power reliability, but current estimates should be taken with caution and motivates the need for additional research.

The remainder of this paper is organized as follows. Section 2 describes the data sources, sample construction, and summary statistics. Section 3 presents the empirical strategy. Section 4 reports the main results and robustness. Section 5 explores heterogeneity. Section 6 concludes.

[3] About 87 minutes in their study.

[4] Minor outage events are outages on days that are non-Major Event Days (MEDs) and reflect routine operation failures rather than large storm events.

[5] According to https://www.eia.gov/electricity/data/eia861/ utilities are allowed to use Institute of Electrical and Electronics Engineers (IEEE) standards or any other method.

## 2 Data

I construct the analysis dataset by merging three main sources: power outage records from the US Department of Energy (2015-2022), residential transaction records from ATTOM Data Solutions (2019-2022), and utility service territory boundaries. The full sample consists of 5.3 million housing transactions within 760 counties that have at least two electric utilities. There are 1,430 utility service area boundaries in the sample.

### 2.1 Power Outage Data

I collect county-level 15-minute interval data on the count of customers without power. The Environment for the Analysis of Geo Located Energy Information (EAGLE-I) data have recently been compiled by researchers at Oak Ridge National Laboratory (ORNL) with data beginning November 2014 through November 2022. An estimated 92% of customers in the U.S., Washington D.C., and Puerto Rico are represented by the final year of the sample (Brelsford et al., 2024).

EAGLE-I is a geographic information system developed at ORNL that monitors electricity outages by scraping the publicly available outage maps of electric utilities across the US at fifteen minute intervals (Brelsford et al., 2024). At each collection interval, automated web parsers visit several hundred utility outage websites and record the number of customers currently reported without power. Most utility outage maps update automatically from each utility's internal outage management system, so the scraped counts reflect near real time conditions rather than figures compiled after the fact for regulatory filings. ORNL attributes the raw customer counts to counties using a population-weighted allocation rule based on the utility's ratio of customers to population.[6]

I measure outage exposure as the System Average Interruption Duration Index (SAIDI), which equals total customer outage hours divided by total customers served, expressed in hours per customer per year. The EAGLE-I database records the number of customers without power in each county at intervals of fifteen minutes. I multiply each record by 0.25 hours to convert it to customer outage hours, sum within each county day to obtain daily customer outage hours, divide by total reported customers to obtain daily SAIDI, and aggregate to the annual level by summing across all days in the calendar year.

[6]Within each utility service territory, the estimated number of customers in county $i$ is $c_i = p_i \times \frac{C}{P}$, where $C$ is the utility's total customer count from EIA Form 861, $P$ is the total population of the service territory from LandScan, and $p_i$ is the LandScan population of county $i$ within the territory (Brelsford et al., 2024). This approach distributes customers across counties in proportion to where people live within each service area rather than in proportion to land area.

I decompose annual SAIDI into chronic and storm components following the Major Event Day classification of the Institute of Electrical and Electronics Engineers (IEEE) Standard 1366-2012. For each county I compute the threshold

$$T_{\mathrm{MED},c} = \exp\left(\hat{\alpha}_c + 2.5\,\hat{\sigma}_c\right),$$

where $\hat{\alpha}_c$ and $\hat{\sigma}_c$ are the mean and standard deviation of the log of daily SAIDI computed over all nonzero outage days in the 2015 to 2022 sample. County days with daily SAIDI exceeding $T_{\mathrm{MED},c}$ are classified as Major Event Days. Minor SAIDI sums daily SAIDI on days below this threshold and reflects routine distribution system failures. Major SAIDI sums daily SAIDI on Major Event Days and captures large storm events. The two components sum to total annual SAIDI by construction.

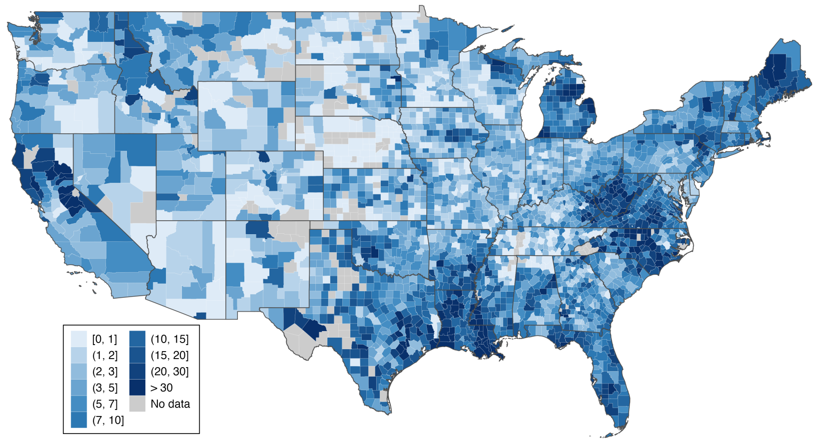
I apply three restrictions to the county year panel. First, I drop Puerto Rico, which falls outside the contiguous U.S. electric grid and the housing market sample. Second, I drop county years with fewer than 1,000 reported customers, where a small denominator inflates SAIDI to implausible levels.[7] Third, I winsorize SAIDI at the 0.1th and 99.9th percentiles of the pooled distribution to limit the influence of remaining extreme values. In the resulting panel, mean annual SAIDI is 4.26 hours per customer, decomposed into 1.88 hours of minor SAIDI and 2.38 hours of major SAIDI.

Figure 1 maps mean annual SAIDI averaged over 2015 to 2022 and shows that total outage exposure varies across the contiguous US. Northern California, Appalachia, the Southeast, the mid-Atlantic, Maine, and the Gulf Coast exhibit higher than average outage duration. Much of these regions are exposed to storm-driven outages due to tropical cyclones in the coastal Southeast and Gulf, severe cold and snow in the Northeast, or exposure to wildfires in California. Appalachia is much less at risk for severe weather-related power outages but is one of the poorest regions in the country, which may contribute to a lack of infrastructure upkeep and exposure to frequent system failures.

Figure A1 maps mean annual minor SAIDI and the pattern shifts slightly, reflecting how much of the coastal Southeast is exposed to outages from severe weather events. Dark blue-shaded counties remain through much of central Appalachia, northern California, inland counties near the Gulf of Mexico, and parts of the Deep South. Many of California's power outages are triggered by util-

[7] The upper winsorization bound is 265 hours per customer per year. The lower bound is effectively zero and trims counties where EAGLE-I records very sparse outage activity in a given year. New Hanover County, NC (FIPS 37129) exhibits a persistently implausible SAIDI across all years due to a likely error in the customer count denominator and appears as a recurring outlier even after winsorization.

Figure 1: Annual Hours of Outages per Customer




**Notes:** Mean annual SAIDI by county in the contiguous United States, averaged over 2015 to 2022. SAIDI is total customer hours without power divided by customers served, in hours per customer per year, from the EAGLE-I database compiled by Oak Ridge National Laboratory from utility control system readings. Counties with fewer than 1,000 reported customers or without EAGLE-I coverage appear in gray.

ities briefly shutting off power in parts of their network during increased wildfire risk to avoid widespread outages, contributing to short duration, high-frequency interruptions computed by minor SAIDI. Figure A2 maps the complementary major SAIDI, which concentrates in the storm-exposed coastal Southeast, the Gulf, and the Northeast. Central Appalachia as well as Arkansas and Louisiana stand out as being on the upper-end of the minor SAIDI distribution. One possible explanation for this is that these regions historically have had persistently depressed economic conditions and therefore lack the tax base necessary to spend on public amenities (Oates, 1969).

### 2.1.1 Mapping Outages to Utility Service Areas

I obtain service territory boundaries for all regulated electric utilities in the contiguous United States from the Homeland Infrastructure Foundation-Level Data (HIFLD) Electric Retail Service Territories shapefile. Each polygon carries an EIA utility identifier that links directly to the EAGLE-I outage records and the EIA Form 861 data used in the robustness specification.

Because EAGLE-I reports outages at the county level and a utility may span multiple counties, I construct utility-level SAIDI by computing a population-weighted average of county SAIDI across each utility's service territory. For each utility $u$ and year $t$ I compute

$$\text{SAIDI}_{u,t} = \sum_{c \in \mathcal{C}(u)} \text{SAIDI}_{c,t} \cdot \frac{\text{pop}_c}{\sum\limits_{c \in \mathcal{C}(u)} \text{pop}_c},$$

where $\mathcal{C}(u)$ is the set of counties in utility $u$'s service territory and $\text{pop}_c$ is population of county $c$. The fraction multiplying a county's SAIDI is the population of that county divided by the total population of all the utility's service area. Population weights are computed from each county's average population across all observed years, and held fixed across the annual SAIDI calculations.[8] I apply these weights separately to minor and major SAIDI so that the components sum to total utility SAIDI by construction.

Allocating outages to utilities in the manner described above attenuates the contrast between utilities within the same county. EAGLE-I county SAIDI is itself a population-weighted blend of the utilities serving a county. Assigning county SAIDI back to each utility gives both sides of a within-county border a measure that partly reflects the same shared-county outages. The resulting measurement error is a systematic shrinkage of the high versus low gap rather than classical noise which attenuates the first stage and biases the estimated capitalization toward zero.

I gauge the size of this compression by attempting to invert the blend. County SAIDI is a share-weighted average of the true utility SAIDI values, so recovering the utility values is an ecological inference problem (Goodman, 1953) that I solve by non-negative least squares over the connected components of the utility county graph. Even after trimming the recovered values at a physical ceiling to remove unstable estimates, the inverted utility SAIDI has about 1.7 times the interquartile spread of the forward measure, which confirms that the forward aggregation discards much of the true cross-utility variation. The inversion is not stable enough to use as a corrected regressor, because utilities that serve overlapping sets of counties are nearly collinear in the share matrix and the recovery pins a large share of utilities at zero.[9] These estimates are therefore best read as a lower bound on the capitalization of chronic reliability.

[8] County population is from the annual American Community Survey for 2015 to 2022.

[9] Across the utilities recovered in identified components, the inverted minor SAIDI has an interquartile range of 2.4 hours against 1.4 hours for the forward measure, but roughly 40 percent of utilities are pinned at zero and a small tail is implausibly large, so I use the inversion only to gauge the compression and treat the reported estimates as a conservative lower bound rather than applying the correction.

## 2.2 Housing Transaction Data

I obtain residential property transaction records from ATTOM Data Solutions, a national data aggregator that compiles sales from county deed records. The estimating sample covers 2019 to 2022 sales.

ATTOM Data Solutions compiles residential property transaction records from county deed and assessor offices across the contiguous United States. For each transaction I observe the sale price, sale date, and census tract FIPS code. Structural characteristics from the assessor file include building square footage, lot size, year built, bedroom count, bathroom count, story count, and indicators for pool, fireplace, and garage. Location coordinates from the assessor record are used to assign each transaction to a utility service territory. The records do not include buyer characteristics such as household income or mortgage details, so I cannot directly test for differential sorting of buyer types across the boundary and rely instead on the covariate balance and donut-hole evidence in Section 4.

I assign each transaction to a utility service territory by spatially joining its latitude and longitude from the ATTOM assessor records to the HIFLD service territory polygons. The join assigns each property an EIA utility identifier, which I use to merge the fixed utility SAIDI and the EIA residential rate control. I then compute each property's distance to the nearest boundary segment identified in the previous step. Properties on the side with lower SAIDI receive a negative signed distance and properties on the side with higher SAIDI receive a positive signed distance.

I apply eight restrictions to arrive at the estimating sample. First, I retain only single family residential properties using ATTOM property use codes. Second, I restrict to open market transactions recorded by warranty or grant deed, excluding transfers between related parties, foreclosure sales, and properties disposed of by lenders. Third, I drop observations missing a census tract FIPS code or property coordinates, as these cannot be assigned to a utility territory or boundary segment. Fourth, I drop observations missing square footage or year built. Fifth, I winsorize sale prices within cells defined by county and year at the 1st and 99th percentiles. Finally, I cap bedroom and bathroom counts at 20, which removes 0.03 percent of transactions attributable to data entry errors.

## 2.3 Census Tract Characteristics

I merge census tract-level demographic and economic characteristics from the 2020 American Community Survey (ACS) five-year estimates to each transaction using the census tract FIPS code. I include variables on median household income, share of adults with a bachelor's degree or higher, unemployment rate, poverty rate, and share identifying as white (non-Hispanic).

## 2.4 Summary Statistics

Table 1 reports the mean and standard deviation of all variables separately for the full sample of home transactions in counties with multiple utilities. The table displays the full sample and for each successively narrower distance from utility border band. As the bandwidth narrows the sample becomes more geographically concentrated but structurally similar. The means and standard deviations of property characteristics and neighborhood demographics are largely stable across columns, providing evidence that restricting to homes near a utility boundary does not select an unusual portion of the housing stock.

The mean price of a home sold in the full sample is about $382K and is slightly larger among homes sold closer to the utility border, rising to about $395K within 500 meters. Tracts have slightly higher shares of white population closer to utility borders than in the full sample, but the remainder of tract-level characteristics are stable. The standard deviation of SAIDI increases close to utility boundaries, because homes near borders by construction span pairs of utilities with different reliability levels.

Because the design requires a within-county utility boundary, the estimating sample is drawn from the counties that more than one electric utility serves, and it is worth asking how those counties compare to the nation. The 516 counties in the boundary sample contain about 95 million residents, or roughly 29 percent of the population covered by the American Community Survey. They are more populous than the average county, with a median county population near 42,000 against 24,000 nationally, so the sample tilts toward more urban areas. On the characteristics that bear on home prices the counties are close to the national profile. Population-weighted median household income is $75,766 against $73,049 nationally, the share of adults with a bachelor's degree is 33.0 against 32.3 percent, and the poverty rate is 12.8 against 13.2 percent. The estimating counties are slightly more diverse than the nation. The estimate therefore describes reliability capitalization in a broadly representative set of more urban counties rather than an unusual slice of

Table 1: Summary Statistics: EAGLE-I Utility Boundary Sample

| | Full sample | | 1,500 m | | 1,000 m | | 500 m | |
|---|---|---|---|---|---|---|---|---|
| | Mean | S.D. | Mean | S.D. | Mean | S.D. | Mean | S.D. |
| **A. House price and characteristics** | | | | | | | | |
| House price ($000s) | 382.3 | 286.9 | 391.8 | 311.4 | 392.0 | 310.6 | 394.9 | 308.4 |
| Square feet | 1,884 | 839 | 1,826 | 796 | 1,828 | 788 | 1,835 | 782 |
| Lot size (sq. ft.) | 49,246 | 2,424,822 | 47,744 | 1,194,647 | 46,489 | 1,340,794 | 38,754 | 246,183 |
| House age (years) | 34.4 | 26.9 | 38.9 | 30.0 | 38.1 | 29.4 | 36.6 | 28.5 |
| Bedrooms | 3.20 | 0.97 | 3.18 | 0.95 | 3.19 | 0.94 | 3.20 | 0.93 |
| Bathrooms | 2.52 | 1.00 | 2.37 | 0.95 | 2.38 | 0.94 | 2.40 | 0.94 |
| Stories | 1.35 | 0.69 | 1.39 | 0.55 | 1.39 | 0.55 | 1.39 | 0.54 |
| Has pool | 0.163 | 0.370 | 0.092 | 0.288 | 0.090 | 0.286 | 0.088 | 0.284 |
| Has fireplace | 0.349 | 0.477 | 0.547 | 0.498 | 0.553 | 0.497 | 0.562 | 0.496 |
| Has garage | 0.833 | 0.373 | 0.791 | 0.406 | 0.799 | 0.401 | 0.808 | 0.394 |
| **B. Reliability (2015–2018 average)** | | | | | | | | |
| SAIDI, total (hrs) | 4.26 | 5.62 | 5.46 | 8.94 | 5.30 | 8.60 | 5.21 | 8.66 |
| SAIDI, minor (hrs) | 1.88 | 2.33 | 2.34 | 4.46 | 2.27 | 4.28 | 2.25 | 4.31 |
| SAIDI, major (hrs) | 2.38 | 3.85 | 3.12 | 4.98 | 3.03 | 4.84 | 2.96 | 4.86 |
| **C. Neighborhood characteristics** | | | | | | | | |
| Tract median income ($000s) | 80.5 | 32.3 | 83.7 | 31.7 | 84.4 | 31.8 | 84.7 | 31.9 |
| Tract percent bachelor's | 35.0 | 17.9 | 35.8 | 16.6 | 36.0 | 16.5 | 36.1 | 16.5 |
| Tract percent poverty | 10.2 | 8.2 | 9.2 | 7.3 | 9.0 | 7.1 | 8.8 | 7.0 |
| Tract percent unemployed | 5.1 | 3.6 | 4.7 | 3.2 | 4.6 | 3.1 | 4.5 | 2.9 |
| Tract percent white | 64.5 | 24.0 | 74.8 | 19.1 | 75.5 | 18.4 | 75.9 | 17.8 |
| Observations | 5,363,300 | | 208,435 | | 148,326 | | 78,999 | |
| Boundary segments | 1,430 | | 857 | | 796 | | 653 | |
| Counties | 760 | | 471 | | 444 | | 368 | |
| Utilities | 634 | | 471 | | 447 | | 383 | |

**Notes:** Residential transactions in 2019–2022 within counties containing at least one within-county utility service territory boundary. Each column restricts to transactions within the stated distance of the nearest boundary; the full-sample column has no distance restriction. The distance-band columns use the same regressor-complete sample as the estimator, so their $N$ matches Table 3. Panel A reports house price and structural characteristics. Panel B reports the fixed 2015–2018 population-weighted EAGLE-I SAIDI for each transaction's utility, split into minor and major components at the IEEE major-event-day threshold. Panel C reports 2020 ACS tract characteristics. House price and tract median income are in thousands of dollars; lot size is in square feet.

the housing market, though it does not speak to counties served by a single utility.

## 3 Empirical Strategy

I recover the capitalization of power outage exposure into home prices from a boundary discontinuity at electric utility service territory borders that lie inside counties. This section develops the theoretical framework, the identification strategy, and the dynamic MWTP correction.

### 3.1 Basic Relationship and Identification Problem

A hedonic price function describes the equilibrium, where the sales price of a good is described by its characteristics, and the price associated with each characteristic represents that of the marginal buyer (Rosen, 1974). In order for this to hold, the analysis must be done in a single integrated market (Palmquist, 1984; Bishop et al., 2020). To satisfy this requirement, I compare homes within the same county, across utility service area boundaries. The basic relationship of interest in measuring the home price effect of outages is as follows:

$$\ln(p_{iuc}) = \alpha + X'_{iuc}\theta + Z'_c\delta + \beta \text{Outages}_{uc} + \varepsilon_{iuc} \quad (1)$$

where $p_{iuc}$ is the price of house $i$ served by utility $u$ in county $c$. The vector $X_{iuc}$ includes property characteristics of house $i$ such as its age, size, number of bedrooms, and bathrooms. $Z_c$ is a vector of county characteristics. The variable of interest, $\text{Outages}_{uc}$, is the average hours of power outages experienced by a customer served by utility $u$ in county $c$ and $\varepsilon_{iuc}$ is the error term.

The coefficient of interest $\beta$ is difficult to estimate causally. Electricity outages are correlated with terrain, tree cover, and storm exposure which are directly priced into homes through risk, not only through outages. Power reliability is likely correlated with income and local economic conditions that move home prices independently. Households also sort across neighborhoods on unobserved preferences, which biases hedonic estimates that do not hold location fixed (Bayer et al., 2007, 2009). Similar to other spending on public goods, I expect utility investment to respond to changes in the tax base and property values (Oates, 1969).

Recent research on the relationship between house prices and power reliability estimates versions of equation (1). Comparing one county to another loads every one of these differences onto the estimated coefficient on outages. A county is an administrative unit whose borders coincide with discontinuities in property tax rates, school districts, public services, zoning, and demographics. Crossing a county line changes many priced attributes at once, a compound-treatment problem that confounds a discontinuity drawn at the county line (Keele and Titiunik, 2015), so price differences across counties cannot be attributed to outages alone.

Employing a discontinuity at the county border still conflates taxes, schools, and services with the reliability channel. Section 4 shows that a matched county-border design recovers a wrong-signed continuous estimate alongside a binary discount, the signature of a confounded contrast in which these bundled attributes, not reliability, drive the price difference. This motivates the use of a boundary discontinuity where only reliability changes.

### 3.2 The Utility Service Territory Boundary

I compare homes sold on opposite sides of an electric utility service boundary that lie within a single county. Two homes just across this line share the same county property tax rate, school district, public services, labor market, weather, terrain, and neighborhood. The only systematic difference is which utility company serves them and how reliable that utility's service is. The estimation strategy explored here borrows from Black (1999) and is used in the environmental hedonic literature to value localized disamenities at a boundary (Muehlenbachs et al., 2015). It replaces the vector of county characteristics $Z_c$ with tract-level characteristics $Z_j$ along with quarter-by-year and boundary fixed effects (FE) that indicate houses that share a utility service area boundary:

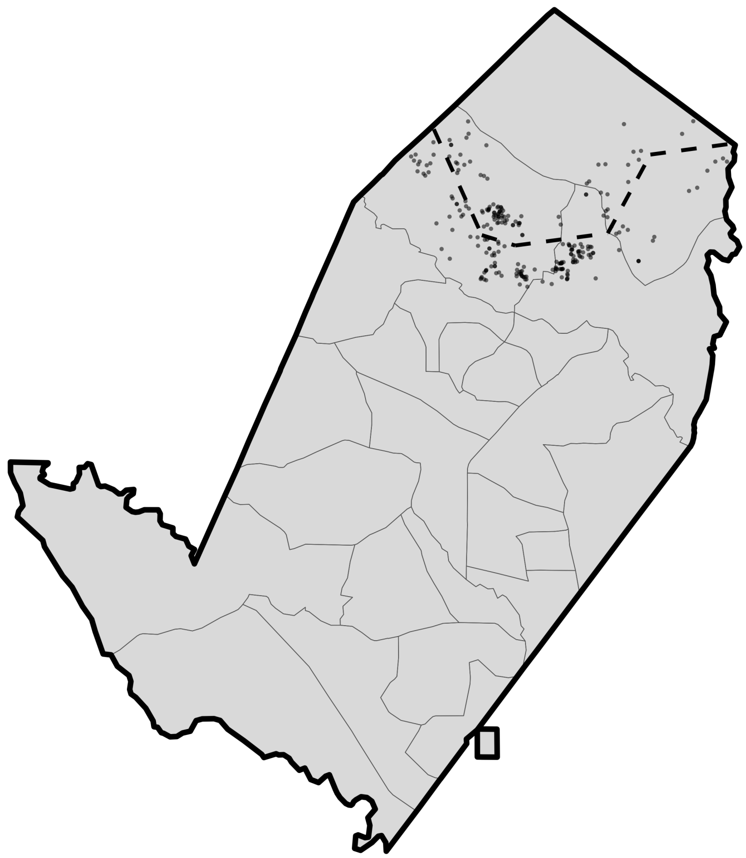
$$\ln(p_{iubj}) = X'_{iubj}\theta + Z'_j\delta + \beta \text{Outages}_u + \phi_{qy} + \eta_b + \varepsilon_{iubj} \tag{2}$$

where $p_{iubj}$ is the price of house $i$ served by utility $u$ near service boundary pair $b$ in tract $j$. The vector $X_{iubj}$ includes property characteristics and $Z_j$ is a vector of tract characteristics. The variable of interest, $\text{Outages}_u$, is the average hours of power outages experienced by a customer served by utility $u$. I include year-by-quarter ($\phi_{qy}$) FE to average out housing market conditions common to the entire sample during a period that included historically low interest rates and overlapped with the global COVID-19 pandemic. I also include boundary pair FE, $\eta_b$, to account for any unobserved characteristics of houses on either side of the utility boundary, and $\varepsilon_{iubj}$ is the error term.

This methodology addresses both the omitted variable problem and concern of endogeneity. By looking at home sales very close to utility service area boundaries with discrete changes in outage rates, I can effectively rule out differences in omitted neighborhood characteristics that are directly priced into homes. Power reliability is also likely set at the county level, where public spending and property tax rates differ, both of which are correlated with home prices and outages. Comparing houses within the same county eliminates this concern.

Figure 2 presents an example of a county in my sample. The example displayed is of Rockdale County, Georgia which is in the southeast of the Atlanta metro area. The thick black line is the county border, the dashed black line represents the utility service area boundary, census tracts are represented with thin gray lines, and the black dots are home sales that take place in 2019-2022 within 1,500m of the boundary. Note that census tracts are not coincident with utility service

Figure 2: The Within-County Utility Boundary Design



**Notes:** Rockdale County, Georgia. The dashed black line is the within-county utility franchise boundary used in estimation, and thin lines are census tract boundaries. Black dots are 2019–2022 home sales within 1,500 meters of the boundary.

boundaries. Electric utility territories were established by state public utility commission regulatory orders, primarily in the 1930s-60s, for reasons unrelated to current housing market conditions (Jacobsen and Kotchen, 2013). These historic fixed borders sever the reverse-causality channel: the gap in outages across boundaries reflects long-standing infrastructure differences, not current prices.

I use utility-level average outage rates over 2015–2018 as the treatment assigned to every 2019–2022 sale in that utility territory. This is the utility's pre-period reliability level, observable to buyers before purchase. Buyers capitalize the expected persistent reliability, not idiosyncratic outage rates realized in the sale year which are subject to variation from large storm-driven outage events. The multi-year pre-period average filters out the noise and aligns with the boundary hedonic literature evaluating the effects of school quality on home prices.[10] Because buyers capitalize the expectation of future outages rather than random fluctuations due to severe weather, I also decompose the outage measure into duration of outages from severe days (major SAIDI) and duration of outages from regular system operation days (minor SAIDI). Minor SAIDI reflects the structural quality of distribution infrastructure, major SAIDI reflects storm exposure. Theory predicts and early empirical evidence from Ewing et al. (2026) shows only minor SAIDI will capitalize into home values. Buyers can observe and anticipate chronic reliability by finding the utility a home is served by through public information and easily accessing the reliability history through the utility's website. Storm-event outages are largely unpredictable at purchase and are correlated with storm risk already priced through insurance and local amenities. To decompose the outage types, I replace total SAIDI with the two SAIDI components (minor, major) jointly entering the regression.

### 3.3 The Matched Sample

The boundary regression in equation 2 recovers $\beta$ from the within-pair contrast, but it does so by extrapolating a linear price function across whatever composition differences remain between the two sides. My preferred estimator instead constructs a covariate balanced sample of homes on opposite sides of the same utility boundary and reads the price gap off comparable homes. Matching is a preprocessing step that makes the estimate robust to the functional form of the hedonic (Ho et al., 2007) and is common among the voter turnout and political analysis literature (Iacus et al., 2012; Keele et al., 2015) and applied in the energy (Rinne, 2024) and environmental economics (Guignet et al., 2018; Fernandez et al., 2018).

I use coarsened exact matching (CEM) to create a sample where observations are matched on utility boundary pair $b$ and select housing characteristics. Each home is matched exactly on the boundary segment, so every comparison is between a higher- and lower-SAIDI home on the same boundary line in the same county. Homes are additionally matched on coarsened bins of number of bedrooms, bathrooms, log square footage, and house-age. Only strata that contain homes on both sides of the boundary are retained. Controls are reweighted so each stratum's control mass equals its treated mass, therefore the weighted within-stratum comparison recovers the average treatment effect on the treated (ATT). Treatment for the matching step is the binary higher-SAIDI side, defined from the fixed 2015–2018 ordering. The outcome regression on the matched sample uses the continuous SAIDI measure, so $\beta$ remains a per-hour MWTP.

[10] Black (1999) assigns a single test score averaged over three years to each home across all outcome years.

Bishop et al. (2020) argue that best practices for hedonic property value models include a covariate-balanced research design and flexible spatial controls. I estimate equation 2 on the matched, reweighted sample, retaining boundary FE $\eta_b$ and quarter-by-year FE $\phi_{qy}$. Matching removes composition differences across the service area boundary and $\eta_b$ removes the common location price level, satisfying both Bishop et al. (2020) points.

### 3.4 Forward-Looking MWTP: The Time-Varying Correction

Annual power reliability is a time-varying attribute, so the static price gradient does not equal the willingness to pay for a permanent change in reliability. The static estimate understates the true willingness to pay when outage differences are mean reverting and overstates it when they are mean diverging. Bishop and Murphy (2019) show that the forward-looking MWTP equals the static estimate divided by $\gamma$, the fraction of a current SAIDI difference that persists on a present-discounted-value basis over the buyer's tenure. A mean reverting process implies $\gamma < 1$, so a current gap in outages shrinks over the buyer's tenure and the observed price gap reflects only the persistent fraction of that gap, which scales the static estimate up.

$$\hat{\beta}^f = \frac{\hat{\beta}^s}{\gamma}, \qquad \gamma = \frac{\sum_{s=1}^{T} \psi^{s-1} \rho_1^{s-1}}{\sum_{s=1}^{T} \psi^{s-1}}, \qquad \psi = 0.95, \; T = 7 \tag{3}$$

where $\hat{\beta}^f$ is the forward-looking MWTP that adjusts the static MWTP $\hat{\beta}^s$ from estimation equation 2 by the $\gamma$ capitalization adjustment factor. The adjustment factor is determined by the AR(1) persistence of utility-level annual SAIDI, $\rho_1$; the discount factor $\psi$, set to 0.95 in this study; and the household tenure $T$, set to the median US household tenure of 7 years (Bishop and Murphy, 2019).

The persistence parameter $\rho_1$ is estimated on the utility-year SAIDI panel from EAGLE-I over 2016–2022, using 2015 as the initial lag. I assume that households have rational expectations and specify the transition of outages as following an AR(1) process. The AR(1) captures how a one-unit difference in the pre-period SAIDI signal translates into expected future SAIDI flows. I estimate the following equation,

$$\text{SAIDI}_{u,t} = \rho_0 + \rho_1 \, \text{SAIDI}_{u,t-1} + \rho_2 \, t + \varepsilon_{u,t} \tag{4}$$

where $u$ denotes utility and $t$ denotes year. This allows me to express $\overline{SAIDI}$ as $\overline{SAIDI}_{u,t} =$

$\mu_t + \gamma\,\text{SAIDI}_{u,t}$, where $\mu_t$ collects the intercept and time trend and $\gamma$ is given by equation (3). I follow Bishop and Murphy (2019) to estimate the pooled $\rho_1$ capturing the persistence of the cross-utility difference. This is the relevant object for buyers comparing two adjacent utilities at a boundary. I estimate this for total, minor, and major SAIDI separately, with results shown in Table A1. My estimate of minor SAIDI $\rho_1$ is 0.875 with standard error equal to 0.005. This amounts to a minor SAIDI adjustment factor $\gamma$ equal to 0.713 with bootstrap standard errors indicating a 95% confidence interval of 0.593 to 0.823.

To express these estimates in dollars, I evaluate the semi-elasticity at the mean sale price $\bar{P}$. A one-hour reduction in annual outages implies a one-time capitalized MWTP of $|\hat{\beta}| \times \bar{P}$ and an annualized MWTP of $|\hat{\beta}| \times r \times \bar{P}$, where $r$ is the annual user cost of housing, set to 0.075. The forward-looking dollar MWTP applies the same conversion to $\hat{\beta}^f$.

# 4 Results

I begin by presenting results from estimating equation 2 on the unmatched sample across narrowing bandwidths. Then, I detail the CEM process and how the unmatched dataset compares to the matched sample. Next, I present the main results for the matched utility boundary estimate. Finally, I provide evidence that estimates are robust to alternative matching algorithms and removing observations right at the boundary, yet self-reported data and county border designs yield misleading estimates.

## 4.1 Unmatched Boundary Estimate

Table 2 presents results from estimating equation 2 for the sample of home sales from 2019–2022 within counties served by $\geq 2$ electric utilities without matching observations on covariates. Columns 1 and 2 include all transactions regardless of distance to the utility service area border, and drop border FE. A one-hour increase in all outages is associated with a 1.15 percent decrease in home prices. However, the decomposition in column 2 indicates this result is driven entirely by days with major outage disruptions. Restricting the sample to observations 500 to 1,500m from utility boundaries flips the signs of the decomposition results and reduces the precision of the estimates. Columns 4, 6, and 8 all agree on the sign and magnitude of the relationship between home prices and chronic outages, and the minor SAIDI estimates within 1,000m and 500m of the boundary are marginally significant. The statistical imprecision of these results motivates exploring the differences between the treatment and control groups.

Table 2: Boundary Discontinuity, Unmatched Benchmark: SAIDI and Home Prices

| | Full | | 1,500m | | 1,000m | | 500m | |
|---|---|---|---|---|---|---|---|---|
| | Total (1) | Decomp. (2) | Total (3) | Decomp. (4) | Total (5) | Decomp. (6) | Total (7) | Decomp. (8) |
| Total SAIDI | $-0.0115^{***}$ | | $-0.0010$ | | 0.0005 | | 0.0021 | |
| | (0.0033) | | (0.0017) | | (0.0015) | | (0.0019) | |
| Minor SAIDI | | $0.0203^{**}$ | | $-0.0114$ | | $-0.0151^{*}$ | | $-0.0132^{*}$ |
| | | (0.0095) | | (0.0089) | | (0.0082) | | (0.0073) |
| Major SAIDI | | $-0.0287^{***}$ | | 0.0003 | | 0.0024 | | 0.0040 |
| | | (0.0081) | | (0.0024) | | (0.0021) | | (0.0026) |
| $N$ | 5,363,300 | | 208,435 | | 148,326 | | 78,999 | |
| Borders | | | 857 | | 796 | | 653 | |
| Border FE | No | | Yes | | Yes | | Yes | |
| Within $R^2$ | | | 0.483 | 0.483 | 0.477 | 0.477 | 0.478 | 0.478 |

**Notes:** Benchmark for the matched estimator in Table 5, estimated on the identical sample with no matching or weighting. Each column is a hedonic regression of log sale price on the fixed 2015–2018 utility SAIDI (hours per year) over 2019–2022 residential sales on a within-county utility border. Total columns report the coefficient on total SAIDI; Decomp. columns report minor and major SAIDI entered jointly. The Full columns pool the entire boundary sample at all distances with no border fixed effects, recovering the confounded cross-section. The 1,500m, 1,000m, and 500m columns take the same population, add within-county border fixed effects, and narrow the bandwidth. All specifications include property and census-tract controls and year-by-quarter fixed effects. Standard errors clustered by utility in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A4 displays means of each covariate by signed distance in meters from utility service area boundary, the higher-SAIDI side is represented with a positive number. The first stage of differences in outages experienced across boundaries is satisfied with large discrete jumps in total, minor, and major SAIDI. Another reassuring visual is that the house price outcome evolves continuously across the utility border. However, there are subtle, yet observable differences in home size, age, number of bedrooms, and bathrooms close to service area borders. These facts motivate employing a matched sample to create better comparisons between transactions across utility boundaries.

## 4.2 Coarsened Exact Matching Process

The boundary fixed effect holds the utility pair fixed, but homes on the two sides of the service area line still differ, so I match homes that are alike on observables before estimating equation 2. CEM splits values of matching covariates into discretized bins, forming strata from the full cross of those bins interacted with the boundary segment (Iacus et al., 2012). The matched sample retains only strata that contain homes on both sides. Imposing this common support restriction prunes roughly one-third of the boundary sample at the widest bandwidth. Table A4 compares the retained and pruned homes. The matched sample skews toward higher-priced homes in higher-income and more-educated tracts, and toward borders with a smaller chronic reliability gap, though structural characteristics are similar and the normalized differences are modest. Figure A3 displays the geographic coverage of the matched sample at the 1,500m bandwidth. Because CEM has only been derived for binary treatments, I assign treatment to the higher fixed 2015-2018 SAIDI side of each pair. Within each stratum CEM weights the lower-SAIDI homes to match the higher-SAIDI distribution, so the estimate recovers the effect for households on the higher-outage side of the line.

I match exactly on the border segment and on bins of four property characteristics that drive price. In the main CEM sample, bedrooms are split into integer bins from 1 to 6, bathrooms split into integer bins of 1 to 5, log square feet in 0.25 log-unit bins, and house age into twelve ten-year bins. The matched sample is a strict subset of the unmatched boundary sample. At 1,500m, matching retains 129,178 of 208,435 homes across 9,596 non-empty strata and 287 of the 857 borders (Table 2 and Table 5). Table 3 shows that across bandwidths, roughly 60 percent of higher-SAIDI homes find support.

Table 3: Matching Balance and Sample Diagnostics

| | 1,500m | 1,000m | 500m |
|---|---|---|---|
| Mean \|SMD\|, before | 0.048 | 0.039 | 0.045 |
| Mean \|SMD\|, after | 0.018 | 0.018 | 0.024 |
| Max \|SMD\|, before | 0.141 | 0.147 | 0.161 |
| Max \|SMD\|, after | 0.089 | 0.100 | 0.106 |
| $L_1$ imbalance, before | 0.149 | 0.157 | 0.189 |
| $L_1$ imbalance, after | 0.000 | 0.000 | 0.000 |
| Homes in band | 208,435 | 148,326 | 78,999 |
| Matched homes | 129,178 | 90,183 | 44,762 |
| Pruned homes | 79,257 | 58,143 | 34,237 |
| Non-empty strata | 9,596 | 7,459 | 4,489 |
| Effective $N$ (control) | 17,652 | 12,804 | 7,086 |
| Share of treated matched | 63.4% | 62.5% | 59.7% |

**Notes:** Balance and sample diagnostics for the coarsened exact matching within-county utility boundary estimator, at each distance bandwidth. Standardized mean differences are averaged (mean) and maximized (max) in absolute value across the fourteen covariates in Table 4. $L_1$ is the Iacus–King–Porro multivariate imbalance over the coarsened structural cells; it is zero after matching because CEM equalizes those cells exactly. Non-empty strata are border-by-structure cells containing homes on both sides. Effective $N$ on the control side is $(\sum w)^2 / \sum w^2$ under the CEM weights. Share of treated matched is the fraction of higher-SAIDI homes that fall in a non-empty stratum (common support).

Table 4 displays Imbens-Rubin standardized mean differences across matched and non-matched covariates before and after the CEM procedure. Matching drives the four structural covariates it targets to near-zero imbalance (Panel A). Because I do not match on tract demographics, their balance in Panel C of Table 4 is a check on the design rather than a mechanical outcome. In the matched sample within 1,500m of a utility boundary, the higher-SAIDI and lower-SAIDI sides differ by no more than 0.06 of a pooled standard deviation in median income, percent in poverty, percent of adults with a bachelor's degree, and percent unemployed. Tract percent white imbalance rises modestly from .024 to .089 at 1,500m and reaches .106 at 500m. Unmatched property characteristics are highly correlated with matched ones and their standardized mean differences also collapse to near-zero. Matching drives the mean absolute standardized difference from .048 to .018 and the multivariate $L_1$ imbalance from .149 to zero. Figure A5 shows the standardized differences collapsing inside the $\pm 0.10$ rule of thumb for every covariate across all three bandwidths. The residential electricity rate, which I do not match on, is also balanced across the boundary. Its standardized difference between the two sides falls from 0.11 to essentially zero after matching, and the rate is nearly uncorrelated with SAIDI. A rate discontinuity at the franchise line, which would capitalize directly, is therefore not driving the estimated discount. Utility ownership does differ across the line, with the higher-SAIDI side more likely to be investor owned and the lower-SAIDI side more likely to be a municipal or cooperative provider. This difference is inherent to a franchise boundary, which by construction separates two distinct utilities, and ownership is therefore part of the reliability treatment rather than a separable confounder. Restricting to same-ownership border pairs is a natural further check.

### 4.3 Matched Utility Boundary Results

Estimating equation 2 on the matched sample tells a similar story to the unmatched sample but with more statistical precision. Table 5 shows the core relationship demonstrated in the unmatched sample that chronic outages are negatively associated with home prices. All columns include property and tract controls, year-by-quarter and border FE, with standard errors clustered by utility. I find that an additional hour of chronic outages (minor SAIDI) is associated with a 1.3 to 2.3 percent decrease in home price. The estimate is negative and stable across all bandwidths. It is significant at the 5 percent level under standard errors clustered by utility, but the design identifies from roughly 200 to 240 utility clusters, so I also report a wild cluster bootstrap, which is more reliable with a moderate number of clusters (Cameron et al., 2008). Under the bootstrap the effect

Table 4: Covariate Balance in the Matched Sample

| | Std. mean diff. | | Variance ratio | |
|---|---|---|---|---|
| | Before | After | Before | After |
| *A. Matched on (structural)* | | | | |
| Bedrooms | 0.027 | 0.000 | 1.12 | 1.00 |
| Bathrooms | 0.042 | -0.002 | 1.08 | 0.98 |
| Log square feet | 0.018 | 0.002 | 1.05 | 1.00 |
| House age | 0.063 | -0.000 | 1.04 | 1.01 |
| *B. Not matched on (structural)* | | | | |
| Log lot size | 0.141 | -0.019 | 1.16 | 0.97 |
| Stories | 0.005 | 0.003 | 1.02 | 1.01 |
| Has pool | -0.038 | -0.013 | 0.90 | 0.96 |
| Has fireplace | 0.050 | 0.001 | 0.99 | 1.00 |
| Has garage | -0.047 | 0.004 | 1.07 | 0.99 |
| *C. Not matched on (neighborhood)* | | | | |
| Tract median income | 0.053 | 0.021 | 1.05 | 1.01 |
| Tract pct bachelor's | 0.047 | -0.020 | 1.06 | 1.01 |
| Tract pct poverty | -0.024 | -0.017 | 1.07 | 0.98 |
| Tract pct unemployed | -0.074 | -0.059 | 1.06 | 0.95 |
| Tract pct white | -0.024 | -0.089 | 1.14 | 1.09 |
| *D. Not matched on (utility)* | | | | |
| Retail rate (cents/kWh) | 0.113 | -0.014 | 1.00 | 1.00 |
| Investor-owned utility | 0.744 | 0.720 | 1.78 | 1.48 |
| Mean \|SMD\| | 0.047 | 0.018 | | |
| Max \|SMD\| | 0.141 | 0.089 | | |
| $L_1$ imbalance | 0.149 | 0.000 | | |

**Notes:** Standardized mean differences (Imbens–Rubin, pooled standard deviation) and variance ratios between the higher- and lower-SAIDI side of each within-county utility border, before matching (all 2019–2022 sales within 1,500m of a border) and after coarsened exact matching with CEM weights. Panel A covariates are matched on (coarsened bedrooms, bathrooms, log square footage, and house-age decade); Panels B, C, and D are not. The neighborhood covariates in Panel C balance even though they are untargeted, and the outcome regression additionally absorbs tract controls and border fixed effects. Panel D reports the residential electricity rate, which balances at the boundary and would capitalize directly if it did not, and the investor-owned indicator, which differs because a franchise boundary separates two distinct utilities and ownership is part of the reliability treatment. A variance ratio near one and |SMD| below 0.10 indicate balance. Figures are for the 1,500m bandwidth; Table 3 reports summary balance across all bandwidths.

is significant at the 10 percent level at the 1,000m and 500m bandwidths and short of conventional levels at 1,500m.[11] I therefore read the evidence as a negative price effect that is robust in sign across every bandwidth, matcher, and geographic subsample rather than one that rests on the

[11] Wild cluster restricted bootstrap $p$-values (Rademacher weights, null imposed, 999 replications, clustered by utility) are 0.14, 0.06, and 0.09 at the 1,500m, 1,000m, and 500m bandwidths, against clustered-standard-error $p$-values of 0.05, 0.01, and 0.02.

conventional significance threshold. All three coefficients are statistically indistinguishable from one another even as the number of observations falls to less than 45,000. Magnitude rises slightly at the tighter bands, consistent with the comparison having less contamination at the exact boundary. The matched sample confirms the same pattern as earlier: the effect of total and major SAIDI is near zero or slightly positive. The pattern is consistent with buyers capitalizing chronic reliability and not pricing outages driven by major outage events. Appendix table A2 displays coefficients for the full suite of covariates, showing that house price varies with characteristics that are consistent with theory.

Table 5: Boundary Discontinuity, Matched Sample: SAIDI and Home Prices

| | 1,500m | | 1,000m | | 500m | |
|---|---|---|---|---|---|---|
| | Total (1) | Decomp. (2) | Total (3) | Decomp. (4) | Total (5) | Decomp. (6) |
| Total SAIDI | $-0.0007$ | | 0.0007 | | 0.0006 | |
| | (0.0016) | | (0.0013) | | (0.0016) | |
| Minor SAIDI | | $-0.0134^{**}$ | | $-0.0233^{***}$ | | $-0.0204^{**}$ |
| | | (0.0067) | | (0.0088) | | (0.0088) |
| Major SAIDI | | 0.0006 | | $0.0032^{**}$ | | 0.0027 |
| | | (0.0018) | | (0.0016) | | (0.0020) |
| $N$ (matched) | 129,178 | | 90,183 | | 44,762 | |
| Borders | 287 | | 254 | | 195 | |
| Within $R^2$ | 0.503 | 0.503 | 0.504 | 0.505 | 0.516 | 0.516 |

**Notes:** Coarsened exact matching pairs 2019–2022 home sales on opposite sides of the same within-county utility border, exact on the border segment and on coarsened bedrooms, bathrooms, log square footage, and house-age decade. Each column is a hedonic regression of log sale price on the fixed 2015–2018 utility SAIDI (hours per year) on the matched sample with CEM weights. Total columns report the coefficient on total SAIDI; Decomp. columns report minor and major SAIDI entered jointly. All include property and census-tract controls, border, and year-by-quarter fixed effects. Standard errors clustered by utility in parentheses, with 244, 222, and 191 utility clusters at the 1,500m, 1,000m, and 500m bandwidths. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

I also estimate the discrete price effect of homes on the high-SAIDI side of a utility boundary by replacing $Outages_u$ in equation 2 with a binary indicator equal to 1 if the observation falls in the higher outage utility service area. Table A3 displays these results looking at high-total SAIDI side and high-minor SAIDI side. I find no evidence of a pricing effect for homes on the higher total SAIDI side of the boundary after controlling for property and tract characteristics as well as border and year-by-quarter FE. Columns 2, 4, and 6 show that there is a 1.1–1.3% discount for homes on the high-minor SAIDI side of the boundary. This effect is statistically significant at 5% for homes within 500m of the border, but only marginally significant for home 500–1,500m from the border.

Why do buyers capitalize chronic reliability but not storm-driven outage days? Major Event

Day outages are common regional shocks. Both sides of a within-county border are likely to experience the same exposure, therefore major SAIDI creates no cross-boundary price gap. Chronic minor SAIDI differs at the utility boundary and is a clear signal of network quality. My results corroborate the qualitative finding in Ewing et al. (2026) recovered from a county panel on self-reported outage data. My results are roughly five to nine times larger than Ewing et al. (2026), which aligns with theory that buyers value long-term expectations of amenities more than transitory shocks. The differences between detected outage EAGLE-I data and self-reported EIA data are analyzed below and contribute to the gap in estimates between my study and Ewing et al. (2026).

I apply the Bishop and Murphy (2019) forward-looking adjustment and recover the annual household MWTP to avoid an hour of chronic power outages. Because reliability is persistent and time-varying, I follow the literature and estimate an AR(1) transition process for total, minor, and major SAIDI and report the estimates in Table A1. Focusing on minor SAIDI, I find $\hat{\gamma} = 0.713$ which is $< 1$, meaning annual chronic outage rates are mean reverting and the static MWTP understates the true capitalization. At the full estimation sample mean sale price of \$382,320, an additional hour of annual chronic outages capitalizes into a one-time home value reduction of \$5,125 to \$8,922 (Table 6). Applying an annualized user cost of 7.5 percent of purchase price gives a static annual MWTP of \$384 to \$669, and dividing by $\hat{\gamma}$ yields a forward-looking annual MWTP of \$539 to \$939 to avoid an additional hour of annual chronic outages. Brehm et al. (2024) finds an annual household expenditure on backup power in California of \$776 which falls squarely in the range I estimate.

### 4.4 Robustness

I provide several robustness tests to show that the price effect of chronic outages holds up to a battery of alternative specifications and geographic restrictions. I begin by showing that the same result holds across alternative matching algorithms and bin constraints. Next, I show that the results remain unchanged after eliminating observations right at the boundary. I provide evidence that the price effect reflects reliability and is not driven by residential electricity rates. Finally, I perform a leave-one-out state jackknife analysis that confirms the effect is not driven by over-represented states or by a single region.

I use coarsened exact matching as the baseline because it fits the boundary design. It pairs homes exactly within the same border segment and bounds the imbalance on every structural covariate in advance, a guarantee that nearest-neighbor and propensity score matching do not

Table 6: Magnitude of Results: Dollar Willingness to Pay for Chronic Reliability

| | Boundary bandwidth | | |
|---|---|---|---|
| | 1,500m | 1,000m | 500m |
| Minor SAIDI coefficient | $-0.0134^{**}$ | $-0.0233^{***}$ | $-0.0204^{**}$ |
| | (0.0067) | (0.0088) | (0.0088) |
| Price change per outage-hour | -1.3% | -2.3% | -2.0% |
| Capitalized MWTP per home | \$5,125 | \$8,922 | \$7,802 |
| Annual MWTP (static) | \$384 | \$669 | \$585 |
| Annual MWTP (forward-looking) | \$539 | \$939 | \$821 |
| $N$ (matched) | 129,178 | 90,183 | 44,762 |

**Notes:** Dollar magnitudes implied by the matched within-county utility boundary estimate (border and year-by-quarter fixed effects, property and tract controls, CEM weights). The coefficient is the semi-elasticity of sale price with respect to fixed 2015–2018 minor SAIDI, in hours per year; standard errors clustered by utility in parentheses. The capitalized MWTP is $|\beta| \times \bar{P}$, the one-time home-value effect of a one-hour reduction in annual chronic outages. The static annual MWTP is $|\beta| \times r \times \bar{P}$ with user cost $r = 0.075$. The forward-looking MWTP divides the static estimate by the Bishop-Murphy (2019) persistence factor $\gamma = 0.713$ (from the utility-year AR(1), Table A1), a 40% amplification. All dollar figures apply a single mean sale price of $\bar{P} = \$382,320$, the average over the full estimation sample of 2019–2022 sales with complete controls (the Full sample column of Table 1). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

provide (Iacus et al., 2012). Propensity score matching also requires a correctly specified model of which side of the line carries higher outages, and it can widen covariate imbalance rather than narrow it (King and Nielsen, 2019). I therefore read the nearest-neighbor and propensity score rows as checks on the baseline rather than as competing estimates.

Table 7: Matched Estimate Across Alternative Matchers

| | Minor SAIDI | | Major SAIDI | | | |
|---|---|---|---|---|---|---|
| Matcher | Coef. | (s.e.) | Coef. | (s.e.) | $N$ | Borders |
| CEM (baseline) | $-0.0134^{**}$ | (0.0067) | 0.0006 | (0.0018) | 129,178 | 287 |
| CEM (finer) | $-0.0129^{*}$ | (0.0072) | 0.0010 | (0.0023) | 122,107 | 279 |
| CEM (coarser) | $-0.0187^{***}$ | (0.0060) | 0.0016 | (0.0019) | 157,748 | 339 |
| NN Mahalanobis (1:1) | $-0.0108$ | (0.0077) | 0.0009 | (0.0024) | 128,550 | 487 |
| Propensity score (1:1) | $-0.0143^{*}$ | (0.0077) | $-0.0000$ | (0.0022) | 119,570 | 398 |

**Notes:** Each row re-estimates the matched hedonic under a different matcher, all followed by the same regression of log sale price on minor and major SAIDI with property and tract controls, border and year-by-quarter fixed effects. The treatment matched across the border is the higher total-SAIDI side. CEM finer and coarser shrink and widen the coarsening bins on bedrooms, bathrooms, log square footage, and house age; the nearest-neighbor and propensity-score rows match one-to-one within the border segment. Standard errors clustered by utility in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The sign and magnitude of chronic outage rates' effect does not depend on the matching algorithm or on the coarsening of the bins. The minor SAIDI coefficient is stable across all five matching procedures explored. Table 7 displays the estimation results for minor and major SAIDI across baseline CEM, finer and coarser CEM bins, nearest-neighbor, and propensity score matching at the 1,500m bandwidth. Bin constraints trace the bias-variance tradeoff. Coarser bins retain more homes and tighten precision, finer bins keep fewer. Across the alternative CEM bins, point estimates for minor SAIDI are between 0.013 and 0.019. Under nearest-neighbor Mahalanobis the minor SAIDI coefficient loses statistical significance at conventional levels but retains the same sign and ballpark magnitude. Propensity score matching slightly inflates the coefficient magnitude but reduces the precision slightly. The major SAIDI coefficient is not statistically significant from zero across all matching procedures.

I perform a donut-hole test by dropping observations closest to the utility boundary to evaluate the possibility of sorting right at the boundary or that the estimate is an artifact of measurement error. Table 8 reports estimates from eliminating homes within 50 and 100m of the boundary. The minor SAIDI coefficient is stable across donut widths and boundary bandwidths even as the matched sample shrinks.

Table 8: Donut-Hole Robustness: Matched Minor SAIDI

| | 1,500m | 1,000m | 500m |
|---|---|---|---|
| No donut | $-0.0134^{**}$ | $-0.0233^{***}$ | $-0.0204^{**}$ |
| | (0.0067) | (0.0088) | (0.0088) |
| 50 m donut | $-0.0128^{*}$ | $-0.0240^{**}$ | $-0.0183^{*}$ |
| | (0.0072) | (0.0094) | (0.0098) |
| 100 m donut | $-0.0126^{*}$ | $-0.0250^{***}$ | $-0.0233^{***}$ |
| | (0.0073) | (0.0093) | (0.0085) |
| $N$ (no donut) | 129,178 | 90,183 | 44,762 |

**Notes:** Each cell is a separate hedonic regression of log sale price on minor and major SAIDI (hours per year), estimated on the CEM-matched within-county utility boundary sample with border and year-by-quarter fixed effects, property and tract controls, and standard errors clustered by utility. Rows drop homes within the stated distance of the border before matching. Only the minor SAIDI coefficient is shown. The matched sample shrinks as the donut widens; the coefficient is stable, so the discount is not driven by the homes nearest the franchise line. Standard errors clustered by utility in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

I also confirm that the discount reflects reliability rather than the price of power. Interacting each SAIDI component with the residential electricity rate, the chronic effect remains negative at the average rate and the interaction is small, so the estimate does not simply track the rate. If anything the discount is larger where electricity is more expensive, consistent with buyers valuing reliability more where the power at stake costs more (Table A7).

Motivated by the fact that some states have many small utilities which in turn means more within-county utility borders, I test whether the results are driven by a single state or small set of over-represented states using a leave-one-out jackknife. I re-estimate the 1,500m matched equation 2 with decomposed SAIDI while dropping each state in turn. Figure 3 displays the minor SAIDI coefficient and 95% confidence interval for each of these regressions. The minor coefficient is negative in every case and clusters around the $-0.0134$ baseline, ranging from $-0.0079$ (Oregon) to $-0.0177$ (California, Indiana). All but six drops remain significant at the 5 percent level. I also drop the three states accounting for the most borders[12] and the three states that account for the most transactions.[13] Table A5 displays border and transaction counts by state across bandwidths. Among all leave-one-out scenarios, the sign and magnitude of the main estimating sample is preserved.

### 4.5 Alternative Data and Spatial Boundary

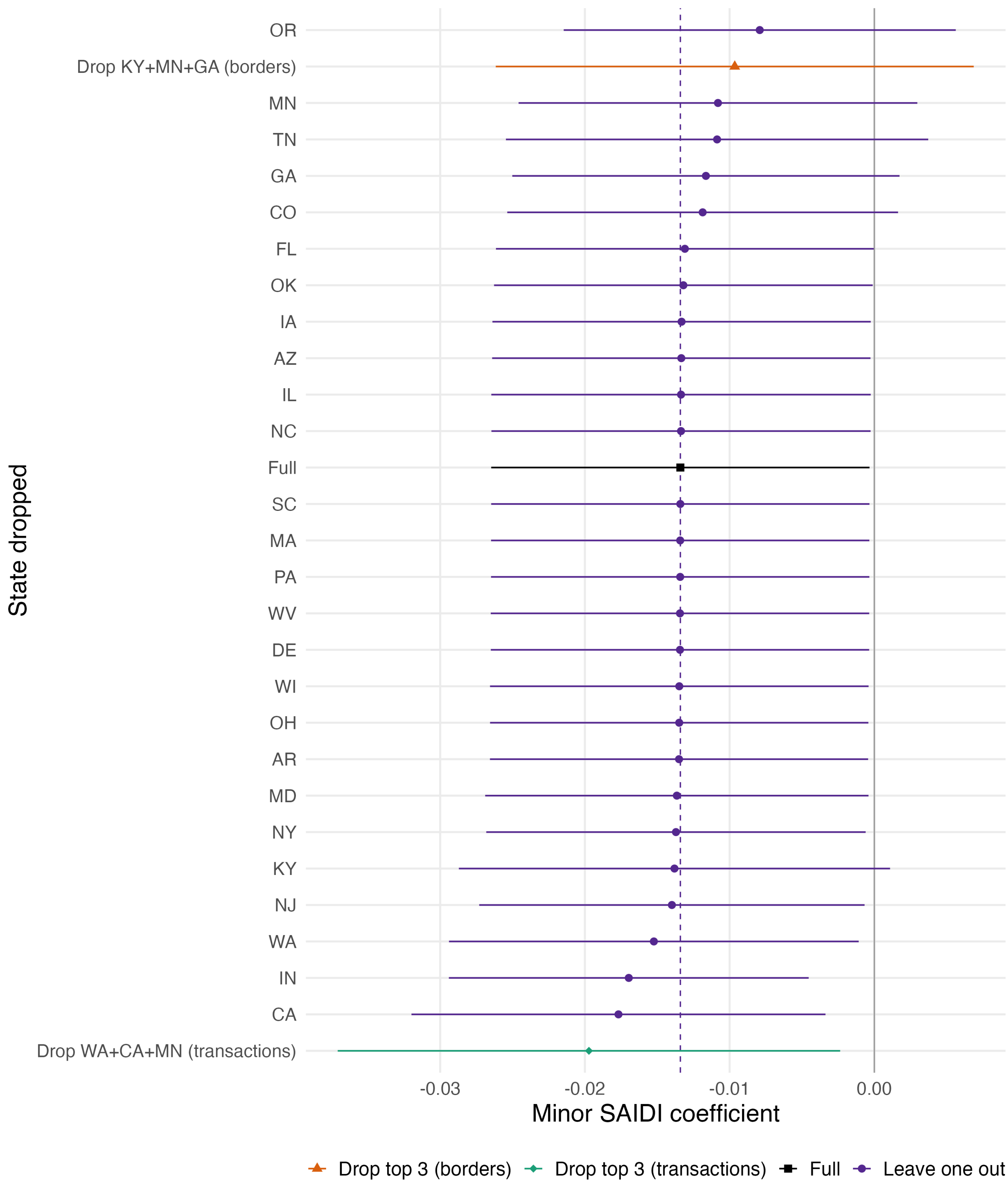
My preferred estimate makes two joint choices, an objective measure of outages and a boundary where reliability changing is unlikely to be confounded with other unobservables changing. Swapping the outage data for self-reported EIA Form 861, or the utility border for a county border, overturns the finding for reasons that are themselves informative. Table 9 displays results from estimating equation 2 substituting the EAGLE-I data for EIA self-reported data and for a county-level specification of the estimating equation with EAGLE-I data. Both alternatives attribute the price effect to major SAIDI and either flip the sign or lose the minor outage effect.

The finding reversal with EIA Form 861 SAIDI is a coverage artifact, not evidence that chronic reliability is not capitalized in home prices. The total effect is negative and significant across all bandwidths, but the decomposition shows this relationship is entirely driven by major SAIDI. Table 10 compares the data coverage for EIA and EAGLE-I samples. Only 653 of the roughly 2,079 utilities in the EAGLE-I universe report reliability to EIA. Using EIA outage data drops the boundary sample from 487 to 247 utilities and from 9.8 to 7.5 million population coverage. Utilities that do report outages are systematically larger utilities, serving 7.8 counties on average versus 2.5 counties for non-reporting utilities. Small municipal and rural electricity providers, which often have worse reliability, are absent. Responding utilities choose their own reporting method and whether

[12] Kentucky, Minnesota, and Georgia.

[13] Washington, California, and Minnesota.

Figure 3: Leave-One-State-Out Jackknife of the Matched Minor SAIDI Estimate




**Notes:** Each point is the matched minor SAIDI coefficient (EAGLE-I, 1,500 meter bandwidth) re-estimated after dropping the labeled state, with 95 percent confidence intervals from standard errors clustered by utility. The dashed line marks the full-sample estimate. The orange triangle drops the three states contributing the most matched borders (Kentucky, Minnesota, Georgia) jointly, and the green diamond drops the three contributing the most transactions (Washington, California, Minnesota). All twenty-six states in the matched sample are shown.

Table 9: Matched Boundary Estimates: Alternative SAIDI Sources and Boundaries

| | 1,500m | | 1,000m | | 500m | |
|---|---|---|---|---|---|---|
| | Total (1) | Decomp. (2) | Total (3) | Decomp. (4) | Total (5) | Decomp. (6) |
| *Panel A. EIA Form 861 SAIDI (utility border)* | | | | | | |
| Total SAIDI | $-0.0012$ | | $-0.0023^{*}$ | | $-0.0041^{**}$ | |
| | (0.0009) | | (0.0013) | | (0.0016) | |
| Minor SAIDI | | 0.0052 | | 0.0063 | | 0.0068 |
| | | (0.0065) | | (0.0075) | | (0.0072) |
| Major SAIDI | | $-0.0021^{***}$ | | $-0.0037^{***}$ | | $-0.0068^{***}$ |
| | | (0.0004) | | (0.0006) | | (0.0013) |
| $N$ (matched) | 67,522 | | 46,847 | | 22,837 | |
| Borders | 151 | | 133 | | 102 | |
| Within $R^2$ | 0.535 | 0.536 | 0.550 | 0.550 | 0.537 | 0.537 |
| *Panel B. County EAGLE-I SAIDI (county border)* | | | | | | |
| Total SAIDI | $-0.0003$ | | $-0.0002$ | | $-0.0003$ | |
| | (0.0003) | | (0.0004) | | (0.0004) | |
| Minor SAIDI | | $0.0016^{*}$ | | 0.0014 | | 0.0012 |
| | | (0.0009) | | (0.0009) | | (0.0011) |
| Major SAIDI | | $-0.0017^{**}$ | | $-0.0015^{**}$ | | $-0.0014^{*}$ |
| | | (0.0007) | | (0.0007) | | (0.0008) |
| $N$ (matched) | 1,013,722 | | 604,463 | | 238,479 | |
| County pairs | 1,748 | | 1,481 | | 1,076 | |
| Within $R^2$ | 0.468 | 0.469 | 0.464 | 0.464 | 0.461 | 0.461 |

**Notes:** Coarsened exact matching pairs 2019–2022 home sales across the same boundary and estimates the hedonic on the matched sample with CEM weights. Panel A uses EIA Form 861 self-reported SAIDI on within-county utility borders, restricted to utilities reporting to EIA (utility border fixed effects, clustered by utility). Panel B uses county EAGLE-I SAIDI on shared county borders (county border fixed effects, clustered by county). Total columns report the coefficient on total SAIDI (hours per year); Decomp. columns report minor and major SAIDI jointly. All include property and census-tract controls and year-by-quarter fixed effects. Standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

to include major-event days. The major and minor outage designation is therefore set by the respondent and is exactly the margin the decomposition depends on. Relying on EIA Form 861 data substantially reduces coverage and comparability across utilities utilizing different methods.

The EIA comparison in Table 9 changes two things at once, the outage measure and the sample of utilities. To separate them, I restrict to the within-county borders where both utilities report to EIA and estimate the identical matched hedonic twice, changing only the measure. Table 11 reports the result at the 1,500m bandwidth. On these same 151 borders, the detected EAGLE-I minor SAIDI coefficient is negative while the self-reported EIA minor coefficient is positive, and the EIA effect instead loads onto major SAIDI. The two sources agree poorly on the very object the design uses, the cross-border SAIDI gap, which correlates only 0.14 across borders (Figure A6). Holding the sample fixed, the self-reported measure cannot reproduce the chronic reliability contrast that the

Table 10: Reliability Data Coverage: EAGLE-I versus EIA Form 861

| *A. Utility size by EIA reporting status (EAGLE-I universe, 2015–2018)* | | |
|---|---|---|
| | Reporters | Non-reporters |
| Number of utilities | 653 | 1,426 |
| Mean counties served | 7.8 | 2.5 |
| Median counties served | 3.8 | 1.0 |
| *B. Within-county boundary sample (2019–2022, within 1,500m)* | | |
| | EAGLE-I | EIA Form 861 |
| Transactions | 228,574 | 151,768 |
| Utilities | 487 | 247 |
| Counties | 488 | 379 |
| Population covered (millions) | 9.8 | 7.5 |

**Notes:** EIA Form 861 has collected distribution reliability metrics only since 2013, and only from utilities that elect to calculate SAIDI and SAIFI, so the reliability sample is a self-selected subset of the utility universe. Panel A compares utilities in the EAGLE-I panel that do versus do not report reliability to EIA over 2015–2018; reporters serve far larger territories (difference in mean counties served significant at $p < 0.001$) and a median of 26,221 customers. Panel B compares the within-county utility boundary sample built on EAGLE-I with the subset served by EIA-reporting utilities. Population covered is the 2020 ACS population of the census tracts in sample. Across the 653 utilities in both panels, self-reported EIA and observed EAGLE-I total SAIDI correlate only 0.22, and EIA reporters further self-select the IEEE 1366 methodology and whether to include major-event days, so their SAIDI values are not on a common basis.

detected data capture. The EAGLE-I coefficient on this overlap is also smaller and less precise than in the full sample, consistent with the largest chronic effects occurring among the small utilities that do not report to EIA.

I also estimate a version of equation 2 and create a matched sample for a county border design. Panel B of table 9 reports a positive and statistically significant effect for minor SAIDI as well as a negative and statistically significant result for this estimation. The positive and significant result at 1,000 and 1,500m bandwidths for minor SAIDI contradicts the expected sign for a disamenity. Table A6 shows that across all bandwidths, homes on the higher SAIDI side sell for a discount. This goes for both total SAIDI and minor. Yet, the continuous minor SAIDI coefficient has the opposite sign. A binary comparison that finds a discount alongside a wrong-signed continuous decomposition is the signature of a confounded contrast. The county line picks up a level difference in prices between higher and lower SAIDI counties that has little to do with reliability.

Two features of the county border design explain the contamination. First, county lines bundle reliability with everything else set at the county level. Property tax rates, school attendance zones, zoning regimes, and public service provision all change at a county line, and any of them can move

Table 11: Same-Sample Measure Swap: Detected (EAGLE-I) versus Self-Reported (EIA) SAIDI

| | EAGLE-I (detected) | | EIA Form 861 (self-reported) | |
|---|---|---|---|---|
| | Total<br>(1) | Decomp.<br>(2) | Total<br>(3) | Decomp.<br>(4) |
| Total SAIDI | $-0.0010$ | | $-0.0011$ | |
| | (0.0021) | | (0.0009) | |
| Minor SAIDI | | $-0.0012$ | | 0.0058 |
| | | (0.0095) | | (0.0068) |
| Major SAIDI | | $-0.0009$ | | $-0.0021^{***}$ |
| | | (0.0027) | | (0.0004) |
| $N$ (matched) | 67,522 | | 67,522 | |
| Borders | 151 | | 151 | |
| Utilities | 112 | | 112 | |
| Within $R^2$ | 0.535 | 0.535 | 0.535 | 0.535 |

**Notes:** Both column groups estimate the identical matched hedonic on the same sample of 2019–2022 home sales at the 1,500m bandwidth, restricted to within-county utility borders where both utilities report a 2015–2018 EIA Form 861 SAIDI. Coarsened exact matching, controls, border and year-by-quarter fixed effects, and utility clustering are held fixed; only the outage measure changes across the two groups. EAGLE-I is the detected (control-system) SAIDI; EIA Form 861 is the utility self-reported SAIDI. Across the 151 matched borders the two sources' cross-border total-SAIDI gaps correlate r = 0.14 (Figure A6). Standard errors clustered by utility in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

prices. Second, the utility is the unit at which reliability is actually delivered. Outage exposure at a given address is determined by the utility that serves it, not by a county average, so the utility boundary is the correct unit of treatment regardless of how buyers process the information. The empirical contrast between the two designs is what carries this point: the utility boundary recovers a chronic discount and the county boundary does not, even though both use the same detected outage data. One interpretation, which I take as a maintained assumption rather than a tested claim, is that reliability is more salient at the utility level, since a buyer can recover the serving utility for an address and its published reliability record, whereas a county average is neither how service is delivered nor how reliability is encountered at purchase. The design does not require buyers to consult these records. It requires only that the priced reliability difference occur at the franchise line, which is where the objective measure says it does.

Only the EAGLE-I utility-boundary specification pairs an objective, comparable, high-coverage outage measure with a boundary at which reliability is the attribute that changes and is salient to buyers. Neither alternative data or spatial discontinuity explored here recovers estimates for the outage disamenity that are consistent with theory. These estimates are the result of a mismeasured outage variable and a confounded boundary and they generate supporting evidence that the EAGLE-I utility border is the credible empirical strategy.

# 5 Heterogeneity

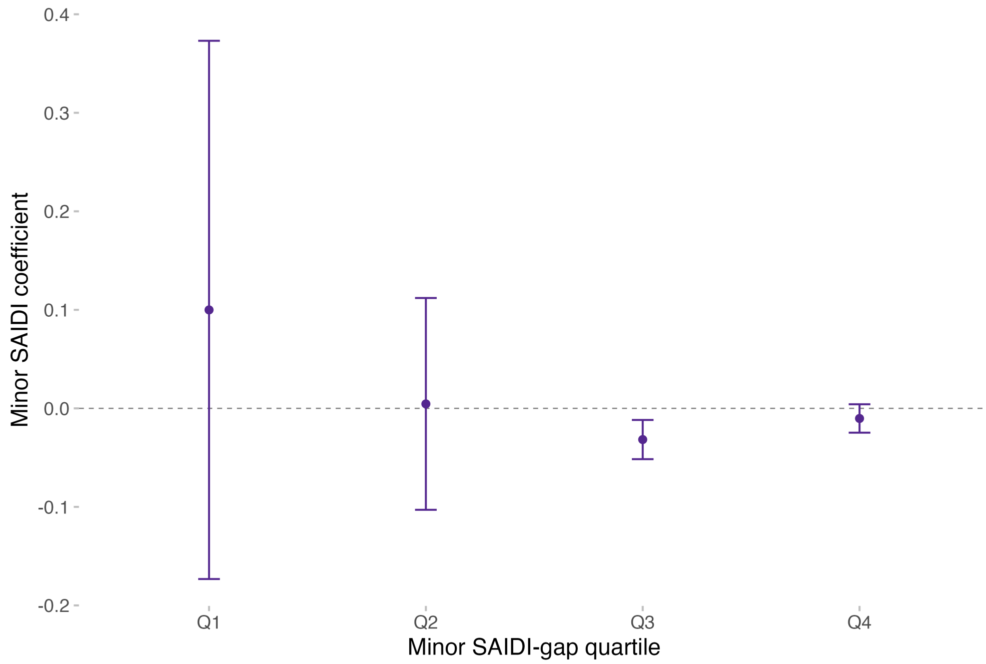
I explore three different channels of potential heterogeneity of the price effect of power outages. First, I analyze whether the effect exhibits a dose-response across border pairs of different gaps in outage rates. Next, I look at whether the price discount differs across the house-price distribution. Finally, I ask whether the price effect is larger among people who may value power reliability more and evaluate capitalization across quartiles of working from home rates.

## 5.1 Dose-Response in the Chronic SAIDI Gap

Each within-county utility border pairs two utilities whose chronic reliability records differ by a fixed amount, the gap $|\Delta\text{minor SAIDI}|$ between the two sides. If the discount reflects capitalization of chronic reliability, it should be absent where this gap is negligible and present where it is large. I sort the boundary sample into quartiles of the gap and re-run the matched minor and major decomposition inside each quartile, at the 1,500 meter bandwidth. This is a dose-response test that imposes no linearity in the gap, and the lowest quartile doubles as a placebo.

Figure 4 shows the pattern. In the bottom two quartiles, where the two utilities differ in chronic SAIDI by less than about a third of an hour per year, the matched minor SAIDI coefficient is statistically zero. The first quartile in particular has almost no reliability contrast to exploit, so its confidence interval is an order of magnitude wider than the others and the point estimate is uninformative. The discount emerges in the upper half of the gap distribution. In the third quartile, where the gap averages about half an hour, the minor SAIDI coefficient is $-.032$ and significant at the one percent level, and it remains negative in the top quartile. Pooling the entire upper half of the gap distribution, the borders where neighboring utilities differ in chronic reliability by at least half an hour per year, the minor SAIDI coefficient is $-.0127$ and significant at the ten percent level. The effect is therefore concentrated where neighboring utilities actually differ in chronic reliability, which is what capitalization predicts and what a spurious correlation would not produce. The point estimate does not increase all the way through the top quartile, consistent with a per-unit price response that flattens at the largest gaps, a range that also contains a few extreme border pairs whose gap exceeds ten hours per year.

Figure 4: Matched Minor SAIDI Coefficient by Chronic SAIDI-Gap Quartile



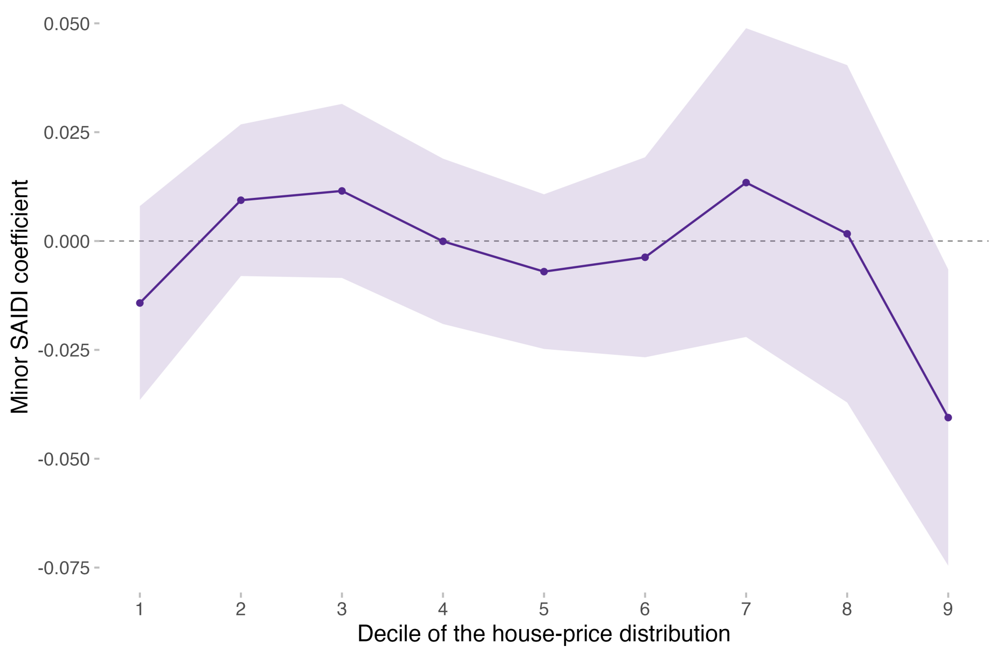

**Notes:** Matched minor SAIDI coefficient (EAGLE-I, 1,500 meter bandwidth) estimated separately within quartiles of the border-pair chronic gap $|\Delta \text{minor SAIDI}|$, with 95 percent confidence intervals from standard errors clustered by utility. Mean gaps run .06, .33, .54, and 1.24 hours per year from Q1 to Q4, so the bottom quartile carries almost no reliability contrast and serves as a placebo.

### 5.2 Incidence Across the House-Price Distribution

I next ask where in the house-price distribution the chronic discount falls. Splitting the sample on price would condition on the outcome, so I instead estimate an unconditional quantile regression using the recentered influence function of Firpo et al. (2009). For each quantile of the matched log-price distribution I regress its recentered influence function on minor and major SAIDI, keeping the controls, border and year-by-quarter fixed effects, and CEM weights of the main specification. The term unconditional refers to the target quantile, which is a quantile of the overall price distribution rather than of price conditional on the covariates. The specification is otherwise identical to the main matched hedonic, with the recentered influence function of log price replacing log price as the outcome, so at the mean it reduces to that hedonic. The coefficient at a given quantile is the effect of chronic reliability on that quantile of the unconditional price distribution. Figure 5 traces the minor SAIDI coefficient across the distribution.

The discount is concentrated at the top of the market. Across the bottom and middle of the price distribution the minor SAIDI coefficient is statistically indistinguishable from zero. At the ninetieth percentile, around home values of $800,000, it is $-.041$ and significant at the five percent level, roughly three times the average matched estimate. The welfare incidence of poor reliability, as it registers in the housing market, falls disproportionately on high-value homes. One potential reason the effect appears only at the top end of the price distribution is that buyers of higher value homes may require more reliable power. Higher income homebuyers are more likely to own electric vehicles (Borenstein and Davis, 2016) and work remote jobs (Dingel and Neiman, 2020), both of which require stable electric utility service.

Figure 5: Minor SAIDI Coefficient Across the House-Price Distribution

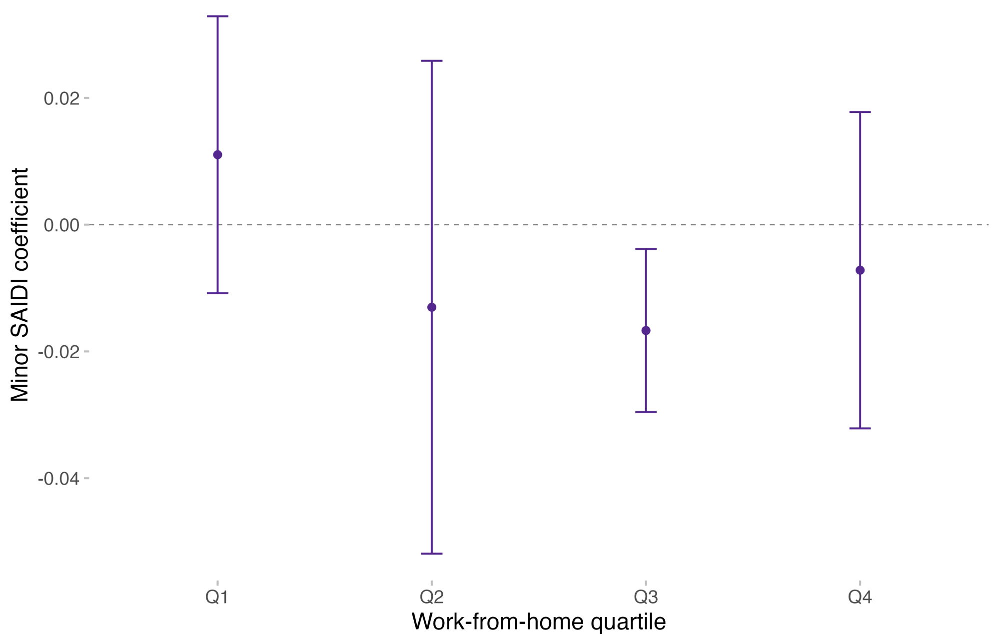



**Notes:** Unconditional quantile (recentered influence function) regression of log sale price on minor and major SAIDI, estimated on the EAGLE-I matched sample (1,500 meter bandwidth) with property and tract controls, border and year-by-quarter fixed effects, and CEM weights. The line is the minor SAIDI coefficient at each decile of the price distribution; the band is a 95 percent confidence interval from standard errors clustered by utility. The tenth decile is omitted because the recentered influence function divides by a density that vanishes at the sample maximum.

### 5.3 Electricity Dependence: Working From Home

A final cut asks whether the discount is larger where households depend more on grid power during the day. I measure work from home as the tract share of workers who worked from home in the 2018 to 2022 American Community Survey (table B08301), sort borders into quartiles of this share, and re-run the matched decomposition in each. Figure 6 shows the minor SAIDI coefficient by quartile.

Figure 6: Matched Minor SAIDI Coefficient by Work-from-Home Quartile




**Notes:** Matched minor SAIDI coefficient (EAGLE-I, 1,500 meter bandwidth) estimated separately within quartiles of the tract work-from-home share (ACS 2018–2022, table B08301), with property and tract controls, border and year-by-quarter fixed effects, CEM weights, and 95 percent confidence intervals from standard errors clustered by utility. Mean work-from-home shares run about 4, 7, 11, and 18 percent from Q1 to Q4.

Households that engage in remote work spend more hours at home during the day, draw more power and stand to lose more from an outage. It follows that the price effect of chronic outages should be larger where working from home is more common. The lowest work-from-home quartile shows no discount. Across the moderate and high work-from-home quartiles, the coefficient is negative and about in line with the main matched estimate, but it is precisely estimated only in the third quartile, where it is $-.017$ and significant at the five percent level. The point estimate does not keep rising into the top quartile, the same non-monotonicity that appears in the chronic-gap quartiles and across the price distribution.

All of these extensions indicate the same thing. The price effect of power reliability is absent in the placebo borders and concentrates where reliability matters most: at borders with observable reliability gaps, among expensive homes, and in neighborhoods that depend more on grid power.

# 6 Conclusion

Power outages impose real costs on households, yet how much people will pay to avoid them is hard to observe directly. I recover that value from the housing market. I compare home sales on opposite sides of the boundary between two electric utilities that serve the same county. I measure reliability with EAGLE-I data, which record outages from utility control systems every fifteen minutes rather than from what utilities choose to report. Coarsened exact matching then pairs homes that are similar in structure across a shared utility service boundary.

Chronic outages capitalize into home prices while storm outages do not. An additional hour in the annual duration of chronic outages corresponds to a 1.3 to 2.3 percent lower home value. At the average sale price near $382,000 this is a capitalized difference between $5,125 and $8,922 per home, or an annual willingness to pay between $384 and $669 per household for each hour of chronic outage avoided. Because reliability is time-varying and persistent, I apply the Bishop and Murphy (2019) adjustment and find a forward-looking annual willingness to pay between $539 and $939 per household for each hour of chronic outage avoided. Storm outages show no cross-boundary price effect, which the design cannot detect because both sides of a within-county boundary share the same regional storm exposure.

The chronic outage price effect is stable across a battery of alternative matching algorithms and robustness tests. Designs that appear similar but rest on weaker foundations give contradictory answers. Utility reported reliability data cover a smaller and systematically larger set of utilities. Using a county border design confounds reliability with a bundle of other changes. Both alternatives attribute the price effect to major outage event days and miss the chronic effect, which supports the utility boundary design over these alternatives.

These findings speak to the current debate over where to direct grid investment. The housing market reveals that homeowners capitalize chronic everyday reliability. Two features of the design bound what the estimate describes. The matched comparison recovers a willingness to pay for homes on the higher-outage side of the boundaries I study, which sit in counties served by more than one utility and skew toward higher-value homes, so the estimate is not a national average. The design also differences out storm exposure that is common across a within-county boundary, so it cannot speak to how households value protection from rare storms. Read within that scope, the results suggest that improving chronic reliability delivers benefits that homeowners capitalize into home values.

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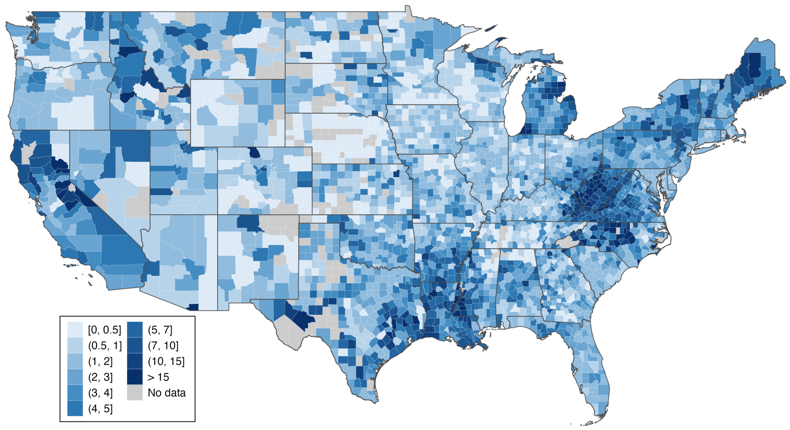
# Appendix

Figure A1: Annual Hours of Outages per Customer - Minor Event Days



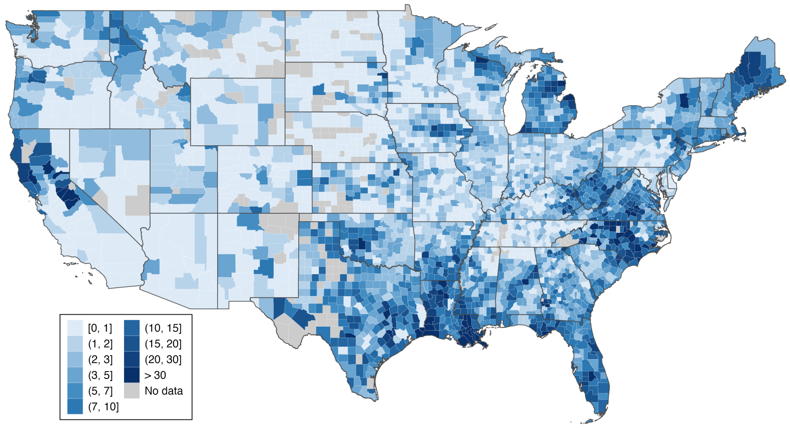

**Notes:** Mean annual minor SAIDI by county in the contiguous United States, averaged over 2015 to 2022. Minor SAIDI sums customer hours without power on days that do not qualify as Major Event Days under the IEEE classification, capturing chronic reliability failures unrelated to large storms.

Table A1: AR(1) Outage Persistence and Bishop-Murphy Adjustment Factor

| | $\hat{\rho}_1$ | $\hat{\gamma}$ | 95% CI for $\hat{\gamma}$ | Obs. |
|---|---|---|---|---|
| Total SAIDI | 0.246*** | 0.216 | [0.204, 0.228] | 13,033 |
| | (0.009) | | | |
| Minor SAIDI | 0.875*** | 0.713 | [0.593, 0.823] | 13,033 |
| | (0.005) | | | |
| Major SAIDI | 0.058*** | 0.175 | [0.172, 0.178] | 13,033 |
| | (0.009) | | | |

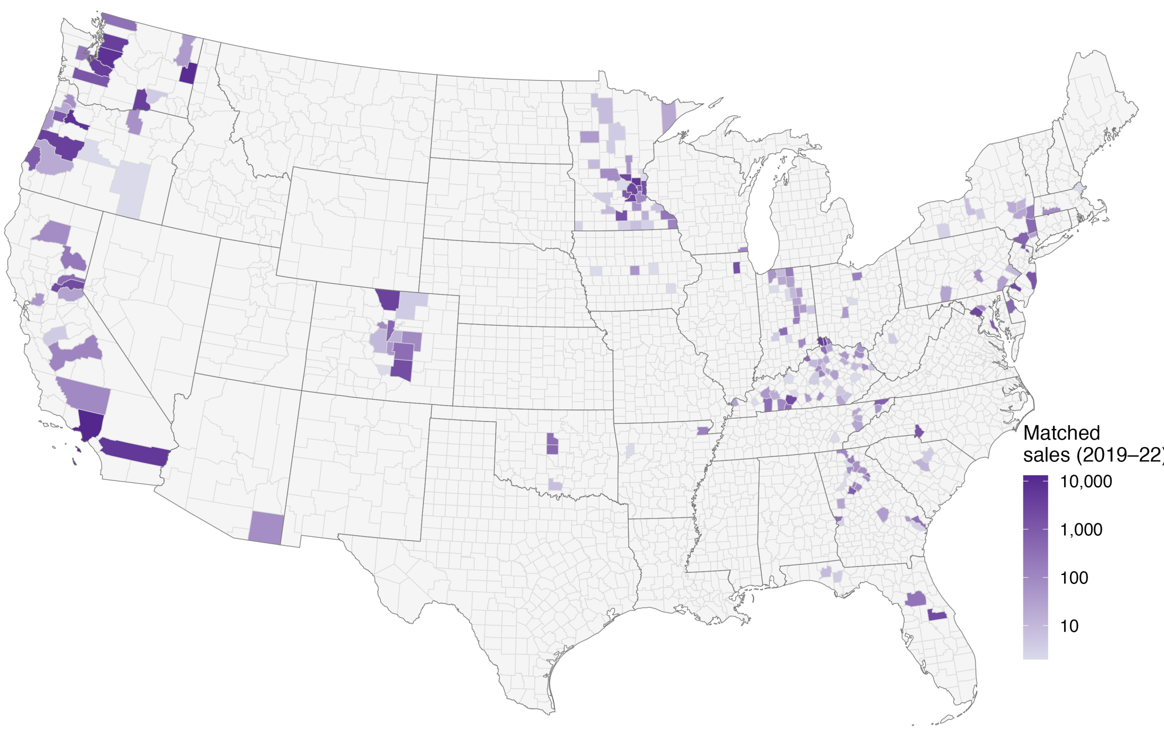
**Notes:** Pooled OLS estimates of $\text{SAIDI}_{u,t} = \rho_0 + \rho_1 \, \text{SAIDI}_{u,t-1} + \rho_2 \, t + \varepsilon_{u,t}$ on the 2016–2022 utility-year panel. $\hat{\rho}_1$ is the AR(1) persistence coefficient; standard errors in parentheses. $\hat{\gamma}$ is computed from $\hat{\rho}_1$ using the Bishop-Murphy (2019) formula with discount factor $\psi = 0.95$ and tenure horizon $T = 7$ years. The 95% confidence interval for $\hat{\gamma}$ is from a utility cluster bootstrap with 1,000 draws. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Figure A2: Annual Hours of Outages per Customer - Major Event Days



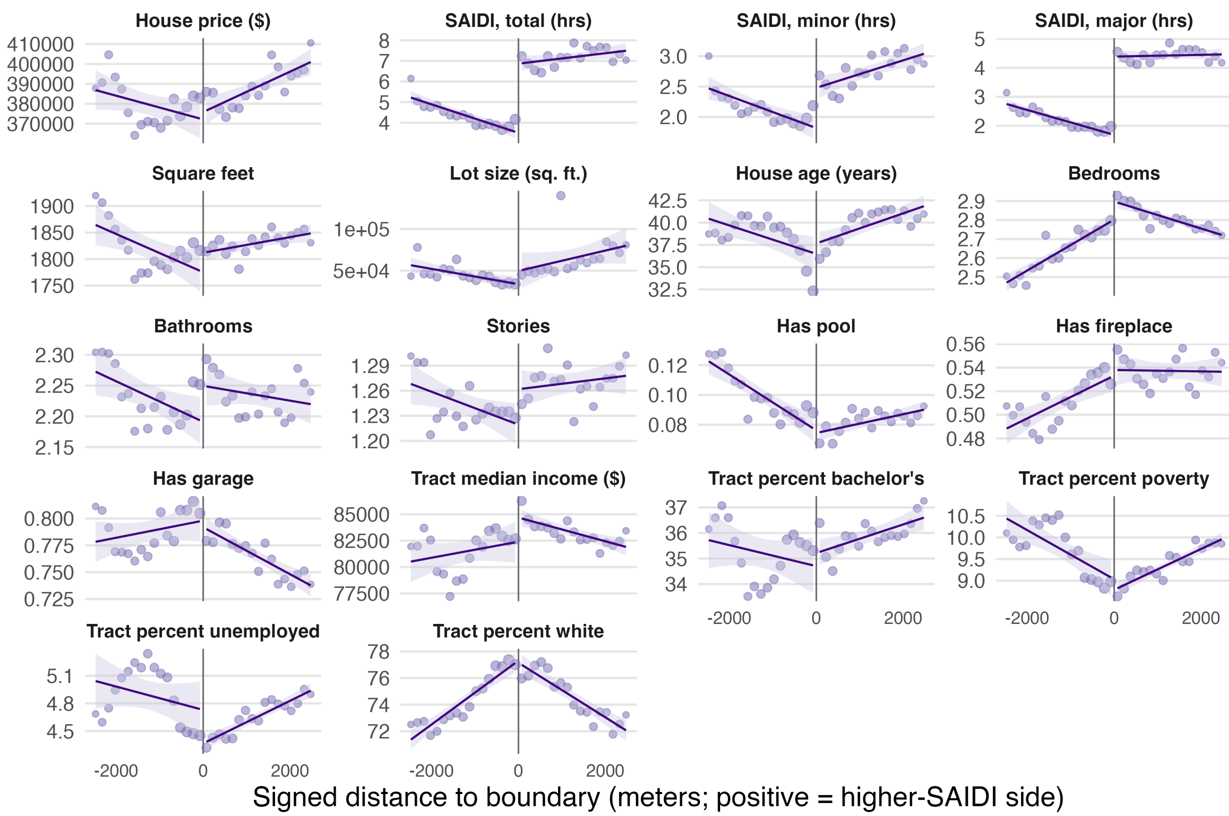

**Notes:** Mean annual major SAIDI by county in the contiguous United States, averaged over 2015 to 2022. Major SAIDI sums customer hours without power on Major Event Days, days whose outage duration exceeds a county-specific threshold under the IEEE standard, capturing outages driven by large storms.

Figure A3: Matched Sample Geographic Coverage



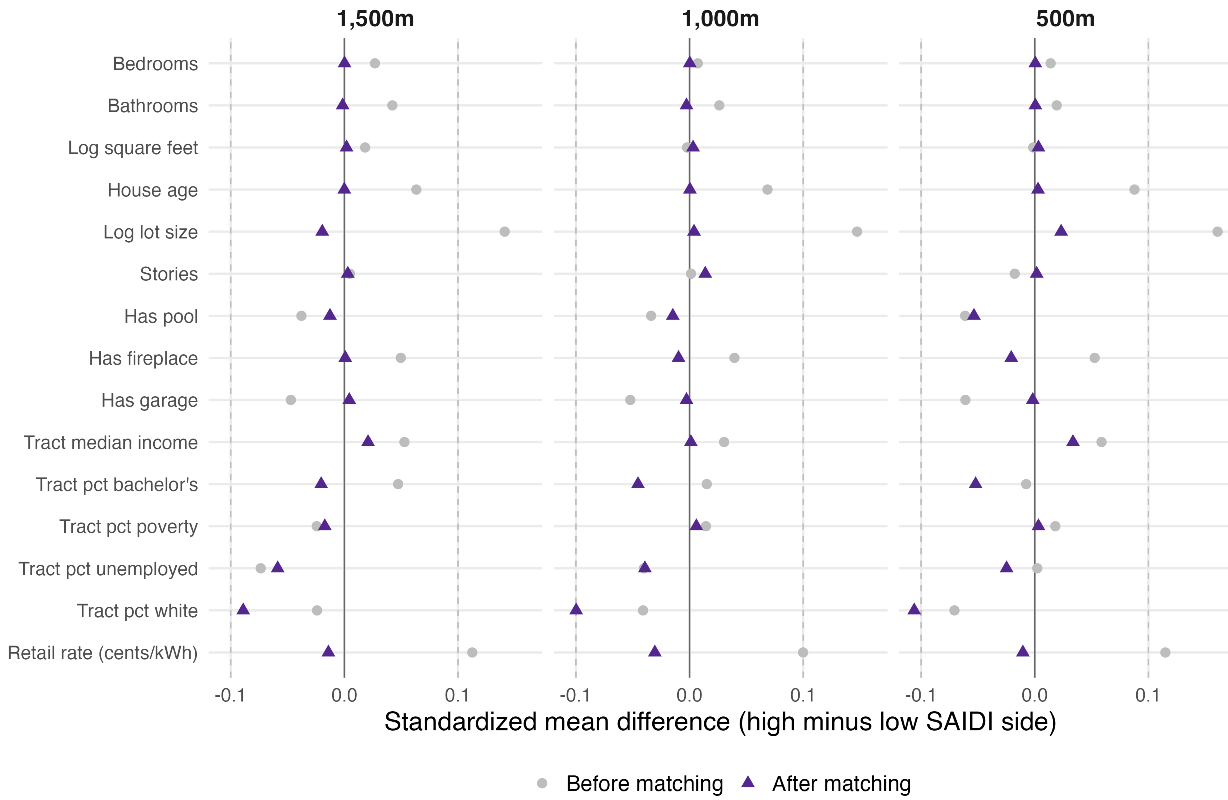

**Notes:** Each shaded county is colored by the number of 2019–2022 residential sales retained in the coarsened-exact-matched within-county utility boundary sample at the 1,500m bandwidth, on a log scale. Counties with no matched sale are unshaded. The matched sample contains 129,178 sales across 287 border segments in 201 counties. Home sales are from ATTOM Data Solutions and outage records from EAGLE-I.

Figure A4: Covariate continuity at Utility Borders




**Notes:** Binned means of each covariate by signed distance in meters to the nearest within-county utility service area boundary, with the higher-SAIDI side positive, over 2019–2022 residential sales within 1,500m of a boundary. The price panel plots log sale price. Home sales are from ATTOM Data Solutions and the 2015–2018 fixed SAIDI from EAGLE-I.

Figure A5: Covariate Balance Before and After Matching




**Notes:** Imbens-Rubin standardized mean differences between the higher- and lower-SAIDI sides for each covariate, before and after coarsened exact matching, by bandwidth. Dashed lines mark ±0.10 standard deviations.

Table A2: Boundary Discontinuity, Matched Sample: Total SAIDI and Home Prices

| | 1,500m (1) | 1,000m (2) | 500m (3) |
|---|---|---|---|
| Total SAIDI (hrs/yr) | $-0.0007$ | 0.0007 | 0.0006 |
| | (0.0016) | (0.0013) | (0.0016) |
| Bedrooms | 0.0280*** | 0.0345*** | 0.0311*** |
| | (0.0078) | (0.0080) | (0.0074) |
| Bathrooms | 0.0415*** | 0.0389*** | 0.0350*** |
| | (0.0085) | (0.0079) | (0.0077) |
| Log square feet | 0.5369*** | 0.5249*** | 0.5268*** |
| | (0.0343) | (0.0350) | (0.0331) |
| House age | $-0.0044$*** | $-0.0052$*** | $-0.0056$*** |
| | (0.0007) | (0.0006) | (0.0007) |
| Log lot size | 0.0485*** | 0.0493*** | 0.0526*** |
| | (0.0105) | (0.0110) | (0.0112) |
| Stories | $-0.0801$*** | $-0.0823$*** | $-0.0791$*** |
| | (0.0089) | (0.0097) | (0.0099) |
| Has pool | 0.0469 | 0.0437 | 0.0265 |
| | (0.0345) | (0.0329) | (0.0276) |
| Has fireplace | 0.0449*** | 0.0424*** | 0.0323*** |
| | (0.0108) | (0.0096) | (0.0082) |
| Has garage | 0.0957*** | 0.0970*** | 0.1006*** |
| | (0.0132) | (0.0137) | (0.0136) |
| Tract median income | 0.0000*** | 0.0000** | 0.0000* |
| | (0.0000) | (0.0000) | (0.0000) |
| Tract % bachelor's | 0.0045*** | 0.0048*** | 0.0050*** |
| | (0.0008) | (0.0009) | (0.0011) |
| Tract % poverty | 0.0013 | 0.0010 | 0.0016 |
| | (0.0010) | (0.0010) | (0.0010) |
| Tract % unemployed | $-0.0014$ | $-0.0003$ | $-0.0005$ |
| | (0.0030) | (0.0029) | (0.0017) |
| Tract % white | 0.0016** | 0.0020** | 0.0017* |
| | (0.0007) | (0.0010) | (0.0010) |
| Border FE | Yes | Yes | Yes |
| Year×Quarter FE | Yes | Yes | Yes |
| $N$ (matched) | 129,178 | 90,183 | 44,762 |
| Borders | 287 | 254 | 195 |
| Within $R^2$ | 0.503 | 0.504 | 0.516 |

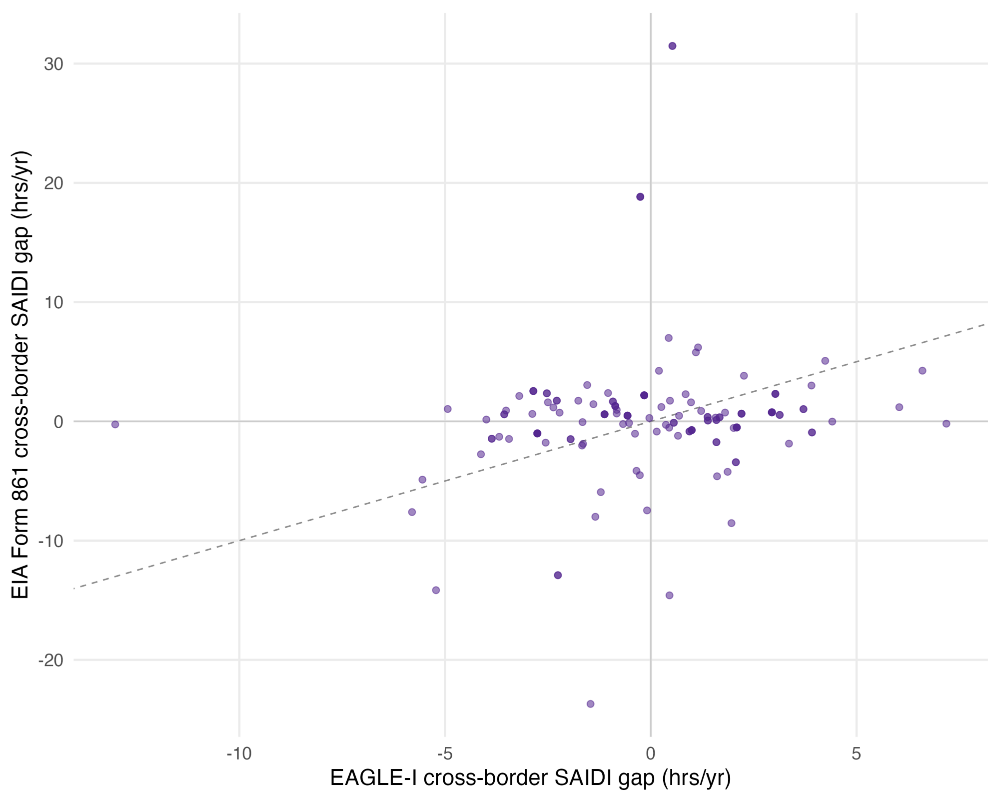
**Notes:** Coefficients for the Total SAIDI specification on the matched within-county utility boundary sample, at each bandwidth. Coarsened exact matching pairs 2019–2022 home sales on opposite sides of the same border segment, exact on the segment and on coarsened bedrooms, bathrooms, log square footage, and house-age decade; each regression uses the CEM weights. The dependent variable is log sale price and the reliability regressor is the fixed 2015–2018 utility total SAIDI in hours per year. House age also enters quadratically. All specifications include border and year-by-quarter fixed effects. Standard errors clustered by utility in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Higher-SAIDI Side and Home Prices, Matched Sample (Binary)

| | 1,500m | | 1,000m | | 500m | |
|---|---|---|---|---|---|---|
| | Total (1) | Minor (2) | Total (3) | Minor (4) | Total (5) | Minor (6) |
| Higher-SAIDI side (= 1) | 0.0079 | $-0.0112^{*}$ | 0.0111 | $-0.0132^{*}$ | 0.0075 | $-0.0121^{**}$ |
| | (0.0063) | (0.0058) | (0.0082) | (0.0070) | (0.0054) | (0.0060) |
| $N$ (matched) | 129,178 | | 90,183 | | 44,762 | |
| Borders | 287 | | 254 | | 195 | |
| Within $R^2$ | 0.503 | 0.503 | 0.505 | 0.505 | 0.516 | 0.516 |

**Notes:** Coarsened exact matching pairs 2019–2022 home sales on opposite sides of the same within-county utility border. Each column regresses log sale price on one binary indicator on the matched sample with CEM weights. Total columns set the indicator to one on the higher total-SAIDI side of the border; Minor columns set it on the higher minor-SAIDI side, using the fixed 2015–2018 ordering. A negative coefficient means the worse-reliability side sells at a discount. All include property and census-tract controls, border, and year-by-quarter fixed effects. Standard errors clustered by utility in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A6: Cross-Border SAIDI Gap: Detected (EAGLE-I) versus Self-Reported (EIA)




**Notes:** Each point is a within-county utility border in the same-sample overlap (both utilities report to EIA), at the 1,500m matched bandwidth. The axes are the cross-border difference in fixed 2015–2018 total SAIDI between the two utilities, measured with EAGLE-I detected data (horizontal) and EIA Form 861 self-reported data (vertical). The dashed line is the 45-degree line; the two measures of the border gap correlate $r = 0.14$.

Table A4: Retained versus Pruned Homes in the Matched Sample (1,500m)

| | Retained | Pruned | Norm. diff. |
|---|---|---|---|
| Chronic SAIDI gap $\|\Delta\|$ (hrs) | 0.47 | 0.62 | -0.249 |
| Sale price ($) | 412,441 | 358,152 | 0.175 |
| Square feet | 1,794 | 1,880 | -0.105 |
| House age (years) | 37.3 | 41.7 | -0.144 |
| Bedrooms | 3.14 | 3.19 | -0.050 |
| Bathrooms | 2.36 | 2.40 | -0.046 |
| Tract median income ($) | 86,992 | 78,412 | 0.274 |
| Tract % bachelor's | 37.7 | 32.6 | 0.313 |
| Tract % poverty | 8.8 | 9.9 | -0.152 |
| Tract % white | 72.9 | 77.8 | -0.259 |

**Notes:** Comparison of the 2019–2022 home sales that coarsened exact matching retains versus prunes at the 1,500m bandwidth. Matching keeps only strata with homes on both the higher- and lower-SAIDI side of a border, so a home is pruned when its border-by-structure cell has no counterpart across the line. Columns report the mean of each variable among retained (129,178) and pruned (79,257) homes, and the normalized difference (difference in means over the pooled standard deviation). The chronic SAIDI gap is the absolute difference in fixed 2015–2018 minor SAIDI between the two utilities at a home's border.

Table A5: State Composition of the Matched Utility Boundary Sample

| | Transactions | | | Border segments | | |
|---|---|---|---|---|---|---|
| State | 1,500m | 1,000m | 500m | 1,500m | 1,000m | 500m |
| KY | 12,844 | 9,826 | 5,290 | 63 | 54 | 40 |
| MN | 20,950 | 15,513 | 7,960 | 46 | 41 | 31 |
| GA | 2,689 | 1,593 | 612 | 24 | 21 | 15 |
| WA | 31,917 | 21,455 | 10,932 | 23 | 22 | 19 |
| OR | 13,656 | 10,881 | 6,412 | 22 | 20 | 15 |
| IN | 1,170 | 677 | 260 | 20 | 18 | 14 |
| CA | 21,728 | 14,428 | 6,571 | 19 | 19 | 15 |
| CO | 6,391 | 4,836 | 2,508 | 12 | 10 | 9 |
| NY | 1,334 | 765 | 201 | 12 | 8 | 5 |
| TN | 898 | 460 | 119 | 7 | 6 | 6 |
| FL | 2,396 | 1,536 | 665 | 5 | 5 | 3 |
| PA | 135 | 78 | 14 | 4 | 4 | 1 |
| IA | 60 | 6 | 4 | 4 | 3 | 2 |
| NJ | 4,311 | 2,775 | 1,188 | 3 | 3 | 3 |
| MD | 3,671 | 2,296 | 935 | 3 | 3 | 3 |
| OK | 844 | 502 | 114 | 3 | 3 | 2 |
| OH | 194 | 73 | 22 | 3 | 2 | 1 |
| MA | 80 | 29 | 2 | 3 | 1 | 1 |
| IL | 1,707 | 1,083 | 353 | 2 | 2 | 2 |
| AR | 150 | 105 | 39 | 2 | 2 | 2 |
| SC | 17 | 12 | 7 | 2 | 2 | 2 |
| NC | 1,356 | 834 | 416 | 1 | 1 | 1 |
| DE | 491 | 309 | 102 | 1 | 1 | 1 |
| WI | 109 | 63 | 11 | 1 | 1 | 1 |
| AZ | 76 | 46 | 25 | 1 | 1 | 1 |
| WV | 4 | 2 | 0 | 1 | 1 | 0 |
| *Total* | 129,178 | 90,183 | 44,762 | 287 | 254 | 195 |

**Notes:** Composition of the coarsened-exact-matched within-county utility boundary sample by state, at each distance bandwidth. Columns report the number of 2019–2022 residential transactions and the number of distinct utility border segments retained after matching within the stated distance of a within-county utility service boundary. Matching follows the baseline specification in Table 3, so the transaction and border totals equal the matched-sample counts reported there. States are ordered by their number of matched border segments at 1,500m. A border segment lies within a single state, so border counts sum to the total.

Table A6: Higher-SAIDI Side and Home Prices: Alternative Sources and Boundaries (Binary)

| | 1,500m | | 1,000m | | 500m | |
|---|---|---|---|---|---|---|
| | Total (1) | Minor (2) | Total (3) | Minor (4) | Total (5) | Minor (6) |
| *Panel A. EIA Form 861 SAIDI (utility border)* | | | | | | |
| Higher-SAIDI side (= 1) | 0.0095 | 0.0109 | 0.0162 | 0.0180 | 0.0129 | $0.0170^{*}$ |
| | (0.0093) | (0.0093) | (0.0120) | (0.0121) | (0.0088) | (0.0088) |
| $N$ (matched) | 67,522 | | 46,847 | | 22,837 | |
| Borders | 151 | | 133 | | 102 | |
| Within $R^2$ | 0.536 | 0.536 | 0.550 | 0.550 | 0.537 | 0.537 |
| *Panel B. County EAGLE-I SAIDI (county border)* | | | | | | |
| Higher-SAIDI side (= 1) | $-0.0219^{***}$ | $-0.0246^{***}$ | $-0.0178^{***}$ | $-0.0215^{***}$ | $-0.0162^{**}$ | $-0.0182^{**}$ |
| | (0.0072) | (0.0071) | (0.0069) | (0.0068) | (0.0081) | (0.0078) |
| $N$ (matched) | 1,013,722 | | 604,463 | | 238,479 | |
| County pairs | 1,748 | | 1,481 | | 1,076 | |
| Within $R^2$ | 0.469 | 0.469 | 0.465 | 0.465 | 0.461 | 0.461 |

**Notes:** Coarsened exact matching pairs 2019–2022 home sales across the same boundary and estimates the hedonic on the matched sample with CEM weights. Each column regresses log sale price on one binary indicator: Total columns set it on the higher total-SAIDI side, Minor columns on the higher minor-SAIDI side, using the fixed 2015–2018 ordering. A negative coefficient means the worse-reliability side sells at a discount. Panel A uses EIA Form 861 SAIDI on utility borders (clustered by utility); Panel B uses county EAGLE-I SAIDI on county borders (clustered by county). All include property and tract controls and year-by-quarter fixed effects. Standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: Matched Estimate with SAIDI Interacted with the Electricity Rate

| | 1,500m (1) | 1,000m (2) | 500m (3) |
|---|---|---|---|
| Minor SAIDI | $-0.0133^{*}$ | $-0.0240^{***}$ | $-0.0263^{***}$ |
| | (0.0073) | (0.0090) | (0.0085) |
| Major SAIDI | $-0.0005$ | 0.0033 | $0.0041^{*}$ |
| | (0.0017) | (0.0020) | (0.0022) |
| Minor SAIDI × rate | $-0.0023^{**}$ | $-0.0021$ | $-0.0022^{*}$ |
| | (0.0012) | (0.0015) | (0.0012) |
| Major SAIDI × rate | $-0.0004$ | $-0.0003$ | $-0.0012^{**}$ |
| | (0.0004) | (0.0005) | (0.0005) |
| Retail rate | $0.0092^{**}$ | $0.0124^{**}$ | $0.0153^{***}$ |
| | (0.0046) | (0.0060) | (0.0041) |
| $N$ (matched) | 122,847 | 85,885 | 42,287 |
| Borders | 275 | 243 | 186 |

**Notes:** Matched within-county utility boundary hedonic with the residential electricity rate interacted with each SAIDI component, at each bandwidth. The rate is centered at its CEM-weighted mean, so the Minor SAIDI and Major SAIDI rows are the effects at the average rate and the interaction rows are the change per additional cent per kWh. Each column includes the rate main effect, property and census-tract controls, border and year-by-quarter fixed effects, and CEM weights, with standard errors clustered by utility. The rate ranges roughly 4 to 22 cents per kWh across the sample. Small and insignificant interactions indicate the outage discount does not simply track the price of power. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.