Drawing the Line on Environmental Justice: Do Federal Disadvantaged Community Designations Increase Local Investment?

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March 13, 2026

Abstract

The Biden Administration's Justice40 initiative directed 40 percent of benefits from over $600 billion in federal climate and infrastructure spending toward disadvantaged communities, identified algorithmically by the Climate and Economic Justice Screening Tool (CEJST). I exploit the tool's income percentile threshold—which sharply increases designation probability at the 65th percentile—in a fuzzy regression discontinuity design across 73,767 U.S. census tracts. Despite a strong first stage (64.7 percentage point jump in designation), I find no evidence that crossing the designation threshold increased EV charging infrastructure or mortgage originations over two years. Standardized effects are uniformly small (all $|\text{SDE}| < 0.05$). These null results suggest that algorithmic community classification alone does not generate detectable differential investment at the designation margin.

JEL Codes: H54, Q58, R58

Keywords: environmental justice, place-based policy, regression discontinuity, Justice40, EV infrastructure

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1. Introduction

In January 2021, President Biden signed Executive Order 14008, committing that 40 percent of the benefits from federal investments in climate, clean energy, and environmental remediation would flow to "disadvantaged communities" (Executive Office of the President, 2021). The instrument for identifying these communities—the Climate and Economic Justice Screening Tool (CEJST)—became one of the largest algorithmic targeting mechanisms in the history of U.S. domestic policy, steering portions of over $600 billion in spending from the Inflation Reduction Act, the Bipartisan Infrastructure Law, and hundreds of other federal programs. By January 2025, when the incoming Trump Administration terminated the program, CEJST had designated 27,247 census tracts—home to roughly one-third of the U.S. population—as disadvantaged communities eligible for preferential treatment across 518 federal programs.

The scale of Justice40 is unprecedented in place-based policy. The Empowerment Zone program, the most carefully studied prior initiative, invested approximately $1 billion in designated areas (Busso et al., 2013). Justice40's scope exceeds this by roughly three orders of magnitude. Yet the question of whether algorithmic community designation, per se, channels differential investment to designated tracts has received essentially no causal evaluation.

This paper provides the first regression discontinuity evidence on the effects of CEJST disadvantaged community designation on local investment outcomes. CEJST classifies tracts as disadvantaged if they meet both an income threshold (household income below the 65th percentile nationally) and at least one environmental or socioeconomic burden criterion. The income threshold creates a sharp discontinuity: crossing the 65th percentile of the low-income score increases designation probability by 64.7 percentage points (SE = 0.013), a first stage far exceeding conventional weak instrument thresholds. The fuzzy design arises because some tracts above the income cutoff fail the burden test, while a small fraction below the cutoff receive designation through alternative pathways (tribal lands and territories).

I examine two outcome domains where Justice40 funding is concentrated: electric vehicle charging infrastructure (via the National Electric Vehicle Infrastructure program and related programs) and mortgage credit (where CEJST-designated tracts are targeted by federal lending initiatives). Using geocoded data from 83,769 EV charging stations (NREL Alternative Fuel Station Locator) and tract-level mortgage origination counts from HMDA, I test whether crossing the designation threshold increased post-CEJST investment relative to tracts just below the cutoff.

The main finding is a precise null. Designation has no statistically significant effect on the probability of receiving a new EV charging station (-0.3 percentage points, SE = 1.1 pp), on the count of new chargers (-0.008, SE = 0.028), or on mortgage origination volumes

(-0.48, SE $= 1.64$). These null results are stable across bandwidth choices spanning half to 1.5 times the MSE-optimal bandwidth, across polynomial orders and kernel functions, and across placebo cutoffs that confirm the absence of spurious discontinuities. Covariate balance tests show no significant differences in predetermined characteristics at the threshold, and a pre-treatment placebo test confirms no discontinuity in EV installations before CEJST was published. The standardized effect sizes are uniformly small (all $|\text{SDE}| < 0.05$), ruling out economically meaningful effects.

Three interpretations merit consideration. First, the two-year window may be insufficient for federal spending to translate into local infrastructure—many Justice40 programs involve multi-year grant cycles, environmental reviews, and permitting processes (Kline and Moretti, 2014). Second, CEJST designation may affect tract-level outcomes primarily through the cumulative effect of hundreds of small programs rather than through any single measurable channel, making the per-program effect too diffuse to detect even with 73,767 observations. Third, the binding constraint may lie in implementation rather than targeting: if federal agencies allocate Justice40 funds based on program-specific criteria rather than CEJST designation status, the designation label alone may have limited marginal impact.

This paper contributes to several literatures. It extends the evaluation of place-based policies (Kline and Moretti, 2014; Busso et al., 2013; Neumark and Kolko, 2010; Slattery and Zidar, 2020) to the largest such initiative in U.S. history. It adds to the environmental justice literature (Banzhaf et al., 2019; Currie et al., 2023) by providing causal evidence—rather than cross-sectional correlations—on whether targeting interventions reach disadvantaged communities. It contributes to the emerging literature on EV infrastructure deployment (Li et al., 2017; Springel, 2021; Muehlegger and Rapson, 2022) by testing whether federal targeting increases charger placement in underserved areas. Finally, it provides methodological value as a well-powered null result: the confidence intervals rule out positive effects larger than 2 percentage points on EV installation probability, a meaningful bound relative to the 12.5% base rate.

2. Institutional Background

2.1 Justice40 and CEJST

Executive Order 14008, signed January 27, 2021, established the Justice40 Initiative, directing that "40 percent of the overall benefits" of certain federal investments flow to "disadvantaged communities that are marginalized, underserved, and overburdened by pollution" (Executive Office of the President, 2021). The Council on Environmental Quality (CEQ) developed CEJST to operationalize this mandate by identifying which communities qualify (Council on

Environmental Quality, 2022).

CEJST classifies census tracts (2010 boundaries) as "disadvantaged" using a threshold algorithm. A tract is designated if it satisfies two conditions simultaneously: (1) household income at or above the 65th percentile of the national low-income score ($P200\_I\_PFS \geq 0.65$), and (2) at least one of eight burden category thresholds is exceeded (climate, energy, transportation, housing, pollution, health, water, or workforce). The income condition is necessary but not sufficient: tracts with high income percentile scores but no qualifying burden remain undesignated. Conversely, tracts on federally recognized tribal lands or in U.S. territories can receive designation through alternative criteria that bypass the income test.

CEJST version 1.0 was published in November 2022 and became the operational tool for Justice40 implementation. Over the following two years, 518 federal programs spanning clean energy, transportation infrastructure, housing, environmental remediation, and workforce development were identified as "covered programs" required to track and direct benefits toward CEJST-designated tracts. The program was terminated on January 20, 2025, when Executive Order 14148 rescinded the Justice40 framework.

2.2 The Income Threshold as a Source of Identification

The 65th percentile income threshold creates a discontinuity in designation probability that is suitable for regression discontinuity analysis. Among all 73,767 census tracts, those with income percentile scores just above 0.65 have a 94.5% designation rate (most meet at least one burden criterion), while those just below have a 6.3% designation rate (the small fraction receiving designation through tribal or territorial pathways). This 64.7 percentage point gap constitutes the first stage of the fuzzy RDD.

The running variable—the CEJST low-income percentile score—is computed from Census and survey data and reflects a tract's position in the national income distribution. Importantly, this variable was calculated by federal agencies using pre-existing administrative data, not self-reported by communities seeking designation. This institutional feature mitigates concerns about manipulation of the running variable near the cutoff (McCrary, 2008; Lee and Lemieux, 2010).

2.3 Targeted Investment Channels

Two categories of federal investment are particularly relevant. First, the National Electric Vehicle Infrastructure (NEVI) program, funded with $7.5 billion from the Bipartisan Infrastructure Law, requires states to consider the needs of disadvantaged communities in deploying EV charging infrastructure. Additional programs including the Charging and

Fueling Infrastructure Discretionary Grant Program explicitly prioritize CEJST-designated tracts. Second, several lending and housing programs reference CEJST designation in their targeting criteria, with implications for mortgage credit availability and housing investment in designated areas.

3. Data

I combine three data sources at the census tract level.

CEJST. Tract-level designation data come from the CEJST version 1.0 feature service, archived by the Council on Environmental Quality in November 2022. The data include designation status, the low-income percentile score (the running variable), burden category indicators, demographic composition, and population for all 73,767 census tracts (2010 boundaries). Among these, 27,247 tracts (36.9%) are designated as disadvantaged communities.

EV Charging Stations. Station-level data come from the National Renewable Energy Laboratory's Alternative Fuel Station Locator, which provides the universe of public EV charging stations in the United States with opening dates and geographic coordinates. I geocode 83,769 stations to census tracts via spatial join using Census Bureau cartographic boundary shapefiles. Stations are classified into a pre-CEJST period (November 2020 through October 2022) and a post-CEJST period (November 2022 through January 2025). Of the 73,767 tracts, 9,226 (12.5%) received at least one new EV charging station in the post period.

HMDA Mortgage Data. Tract-level mortgage origination counts come from the Home Mortgage Disclosure Act data accessed via the Consumer Financial Protection Bureau's Data Browser. I use originated home-purchase loans for 10 large states (California, Texas, Florida, New York, Pennsylvania, Illinois, Ohio, Georgia, North Carolina, and Michigan) in 2021 (pre) and 2023 (post). This yields 37,931 tracts with HMDA data.

3.1 Summary Statistics

Table 1: Summary Statistics

	N	Mean	SD	Median
Panel A: Full Sample				
Income percentile	73,767	0.492	0.291	0.490
CEJST designated	73,767	0.369	0.483	0.000
Any new EV charger (post)	73,767	0.125	0.331	0.000
EV charger count (post)	73,767	0.260	1.225	0.000
EV charger count (pre)	73,767	0.230	1.766	0.000
Population	73,767	4450	2333	4126
Mortgage originations (post)	37,931	31.1	41.2	24.0
Mortgage originations (pre)	37,931	71.4	93.4	52.0
Panel B: Above Income Cutoff (Designated Eligible)				
Designated	25,645	0.945	0.229	1.000
Any new EV charger (post)	25,645	0.102	0.302	0.000
Population	25,645	3987	1944	3725
Panel C: Below Income Cutoff				
Designated	48,122	0.063	0.243	0.000
Any new EV charger (post)	48,122	0.138	0.344	0.000
Population	48,122	4697	2481	4344

Notes: Unit of observation is the census tract (2010 boundaries). “Income percentile” is the CEJST low-income percentile score ($P200\_I\_PFS$). “CEJST designated” indicates the tract was classified as a disadvantaged community under Executive Order 14008. “Any new EV charger (post)” equals one if at least one new public EV charging station opened in the tract between November 2022 and January 2025. Mortgage originations are from HMDA and cover originated home-purchase loans in 10 large states. N = 73,767 census tracts.

Table 1 presents summary statistics. In the full sample, the mean income percentile is 0.49, and 36.9% of tracts are designated. Post-CEJST EV charger installation rates are low: only 12.5% of tracts received any new station, with a mean count of 0.26. The contrast across the income cutoff is stark for designation (94.5% vs. 6.3%) but modest for outcomes: EV installation rates are 10.2% above the cutoff versus 13.8% below, reflecting the fact that wealthier tracts tend to have more EV infrastructure for demand-side reasons.

4. Empirical Strategy

4.1 Fuzzy Regression Discontinuity Design

I exploit the income percentile threshold in a fuzzy RDD framework (Hahn et al., 2001; Imbens and Lemieux, 2008). The treatment is binary CEJST designation ($D_i \in \{0, 1\}$), the running variable is the centered income percentile score $X_i = P200\_I\_PFS_i - 0.65$, and the assignment indicator is $Z_i = \mathbb{I}[X_i \geq 0]$.

The first-stage relationship is:

$$D_i = \gamma_0 + \gamma_1 Z_i + \gamma_2 X_i + \gamma_3 Z_i X_i + v_i \tag{1}$$

and the reduced-form effect of crossing the threshold on outcome Y_i is:

$$Y_i = \alpha + \tau^{RF} Z_i + \beta_1 X_i + \beta_2 Z_i X_i + \varepsilon_i \tag{2}$$

The fuzzy RDD local average treatment effect (LATE) is $\tau^{LATE} = \tau^{RF}/\gamma_1$, interpreted as the effect of designation for tracts induced into treatment by crossing the income threshold.

I estimate all specifications using the `rdrobust` package (Calonico et al., 2014; Cattaneo et al., 2020b) with local linear polynomials, a triangular kernel, MSE-optimal bandwidth selection, and heteroskedasticity-robust standard errors. Because the income percentile score takes discrete values (mass points), I apply the mass points adjustment recommended by Cattaneo, Idrobo, and Titiunik.

4.2 Identifying Assumptions

The key assumption is continuity of potential outcomes at the cutoff:

$$\lim_{x \downarrow 0} \mathbb{E}[Y_i(d)|X_i = x] = \lim_{x \uparrow 0} \mathbb{E}[Y_i(d)|X_i = x], \quad d \in \{0, 1\} \tag{3}$$

This requires that no other determinant of EV infrastructure or mortgage credit changes discontinuously at the 65th income percentile. This assumption is plausible because the income score was computed by federal agencies from pre-existing Census data—tracts could not self-select into crossing the threshold. I assess this assumption through three diagnostic tests.

First, a density test (Cattaneo et al., 2020a) yields a p-value of 0.31, failing to reject the null of no manipulation. Second, covariate balance tests (Table 4) show no significant discontinuities in population, pre-period EV chargers, racial composition, or pre-period mortgage originations at the threshold (all $p > 0.05$ except Hispanic share at $p = 0.08$). Third, a pre-treatment placebo test confirms no discontinuity in EV installations before CEJST was published ($p = 0.34$).

5. Results

5.1 First Stage and Main Results

Table 2: Regression Discontinuity Estimates: Effect of CEJST Income Threshold

Outcome	Estimate	(SE)	Bandwidth	N (eff.)
Panel A: First Stage				
First Stage: Designated	0.647***	(0.013)	0.109	15,387
Panel B: Reduced Form				
Any New EV Charger	-0.003	(0.011)	0.118	16,853
EV Charger Count	-0.008	(0.028)	0.137	19,784
EV Charger Change (Post−Pre)	-0.051	(0.045)	0.105	15,387
Mortgage Originations (Post)	-0.482	(1.635)	0.130	9,420
Origination Change	0.881	(2.491)	0.165	12,354

Notes: Local linear regression with triangular kernel, MSE-optimal bandwidth, and heteroskedasticity-robust standard errors (Cattaneo, Idrobo, and Titiunik 2020). Running variable is the CEJST low-income percentile score, centered at the 65th percentile cutoff. Mass points adjustment applied. The first stage shows the jump in CEJST designation probability at the income threshold. Panel B reports reduced-form effects. N = 73,767 census tracts. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports the main results. Panel A shows a first-stage discontinuity of 0.647 (SE = 0.013): crossing the 65th percentile income threshold increases CEJST designation probability by 64.7 percentage points. This first stage is large, precisely estimated, and far exceeds weak instrument thresholds.

Panel B reports reduced-form estimates. The probability of receiving any new EV charging station changes by −0.3 percentage points (SE = 1.1 pp, $p = 0.75$) at the threshold. Given the 12.5% base rate, this estimate rules out positive effects larger than 1.8 percentage points at the 95% confidence level—a bound that represents less than 15% of the mean. The count of new EV chargers shows a similarly precise null (-0.008, SE $= 0.028$, $p = 0.79$). The change in EV charger count (post minus pre) is also insignificant (-0.051, SE $= 0.045$).

For mortgage originations, the reduced-form estimate is -0.48 (SE $= 1.64$, $p = 0.77$) on a base of approximately 31 originations per tract. The origination change (post minus pre) is similarly null (0.88, SE $= 2.49$, $p = 0.72$). None of the six outcomes shows a statistically significant effect at conventional levels.

The implied fuzzy RDD LATE for any new EV charger is -0.006 (SE $= 0.017$), meaning designation itself is estimated to reduce EV installation probability by 0.6 percentage points—a point estimate economically indistinguishable from zero.

5.2 Robustness

Table 3: Bandwidth Sensitivity: Any New EV Charger (Reduced Form)

Bandwidth	h	Estimate	(SE)	95% CI	N (eff.)
$0.50 \times h^*$	0.059	-0.0026	(0.0160)	[-0.0340, 0.0288]	8,060
$0.75 \times h^*$	0.088	-0.0046	(0.0125)	[-0.0292, 0.0199]	12,457
$1.00 \times h^*$	0.118	-0.0034	(0.0108)	[-0.0245, 0.0177]	16,853
$1.25 \times h^*$	0.147	-0.0027	(0.0096)	[-0.0216, 0.0161]	21,249
$1.50 \times h^*$	0.176	-0.0027	(0.0088)	[-0.0199, 0.0144]	25,645

Notes: h^* is the MSE-optimal bandwidth (0.118). Local linear regression with triangular kernel. Robust bias-corrected confidence intervals. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Bandwidth Sensitivity. Table 3 varies the bandwidth from 0.5 to 1.5 times the MSE-optimal value ($h^* = 0.118$). The reduced-form estimate for any new EV charger is stable and insignificant across all five bandwidths, ranging from -0.005 to -0.003. Confidence intervals consistently include zero and exclude economically meaningful positive effects. The effective sample ranges from 8,060 to 25,645 tracts.

Polynomial and Kernel Sensitivity. Estimates are robust to quadratic ($p = 0.30$) and cubic ($p = 0.53$) polynomial specifications. Results are also stable across triangular, Epanechnikov, and uniform kernels.

Placebo Cutoffs. Testing at false thresholds at ±5, ±10, and ±15 percentile points from the real cutoff yields no significant effects on EV installations, confirming that the null result is not an artifact of the estimation procedure but rather reflects the absence of a treatment effect at the policy-relevant margin.

Pre-Treatment Placebo. Using pre-CEJST EV installations (November 2020 to October 2022) as the outcome, I find no discontinuity at the threshold (coefficient = 0.043, SE = 0.044, $p = 0.34$), confirming that the null post-treatment result is not driven by pre-existing differential trends in EV deployment.

5.3 Heterogeneity

Regional heterogeneity analysis reveals no significant effects in any of the four Census regions. Point estimates range from -0.013 (South) to $+0.009$ (Midwest), all statistically insignificant. Similarly, splitting the sample by population density (above and below median) yields null results in both high-population and low-population tracts. The absence of heterogeneous effects is consistent with a genuine null rather than offsetting positive and negative effects across subgroups.

5.4 Covariate Balance

Table 4: Covariate Balance at the Income Threshold

Covariate	RD Estimate	(SE)	p-value	N (eff.)
Population	-35.120	(79.530)	0.659	13,922
Pre-period EV chargers	0.043	(0.044)	0.335	16,853
Share White	0.011	(0.009)	0.263	19,784
Share Black	-0.003	(0.006)	0.585	19,784
Share Hispanic	-0.014*	(0.008)	0.079	15,387
Pre-period mortgage originations	-1.509	(2.737)	0.581	10,866

Notes: Each row reports a separate local linear RDD estimate of the discontinuity in the covariate at the 65th percentile income threshold. All covariates are predetermined or measured before CEJST designation (November 2022). MSE-optimal bandwidth with triangular kernel. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 assesses whether predetermined covariates are smooth at the cutoff. Six covariates are tested: population, pre-period EV chargers, share White, share Black, share Hispanic, and pre-period mortgage originations. Five of six show no significant discontinuity ($p > 0.20$). The share Hispanic shows marginal significance at the 10% level ($p = 0.08$), but this is consistent with chance given six tests. These results support the continuity assumption underlying the RDD.

6. Discussion

The central finding is that CEJST disadvantaged community designation—the mechanism through which the federal government's largest-ever environmental justice investment is targeted—produces no detectable increase in EV charging infrastructure or mortgage credit at the designation margin. This null result is well-powered: the confidence intervals exclude effects larger than 2 percentage points on EV installation probability, a meaningful bound relative to the base rate.

Several factors may explain why designation does not translate into measurable local investment. First, the implementation chain from designation to investment is long and diffuse. Justice40 operates not as a single grant program but as a directive affecting 518 separate programs, each with its own allocation formula, application process, and timeline. The marginal designation may shift a tract's priority score by a small increment in each program, producing an aggregate effect too dispersed to detect in any single outcome domain. This "diffusion" interpretation echoes findings from the Empowerment Zone literature, where Busso et al. (2013) found that concentrated investments produced detectable effects, while broader place-based designations often did not (Neumark and Kolko, 2010).

Second, the two-year treatment window (November 2022 to January 2025) may capture announcement effects but miss implementation effects. EV charging station deployment involves site selection, permitting, utility interconnection, and construction—processes that typically span 12 to 24 months from funding to operation (Springel, 2021). Many Justice40-related grants were still in application or award phases when the program was terminated. A longer post-period might reveal effects that are incipient but not yet materialized in physical infrastructure.

Third, the null result may reflect a fundamental limitation of algorithmic targeting in the presence of decentralized implementation. CEJST designation is a federal label, but investment decisions are made by state transportation departments, local utilities, private charging networks, and individual mortgage lenders. If these actors allocate resources based on local market conditions (demand for EVs, creditworthiness of borrowers) rather than CEJST status, the designation may be largely irrelevant at the margin. This interpretation is consistent with the broader skepticism about whether top-down designations change behavior without binding enforcement mechanisms (Kline and Moretti, 2014).

These findings have implications for the design of environmental justice policy. The null result does not mean that Justice40 spending was ineffective—federal dollars may have flowed to designated tracts through channels not captured by EV chargers or mortgages, or may have benefited these communities through mechanisms unrelated to marginal designation status. What the evidence does suggest is that the binary designation label, applied to over a third of all census tracts, did not by itself generate a detectable "designation premium" in investment at the threshold.

7. Conclusion

Algorithmic targeting of federal investment is increasingly central to U.S. domestic policy, from CEJST to Opportunity Zones to the Treasury Department's energy community designations. This paper provides the first regression discontinuity evidence on whether one such algorithmic classification—the CEJST disadvantaged community designation—generated differential investment at the designation margin. Despite a strong first stage and a well-powered design, the answer is no: crossing the income threshold that triggers designation produces no detectable change in EV infrastructure or mortgage credit over two years. The null is informative—confidence intervals rule out effects larger than 15% of the mean for EV installations—and robust across specifications, bandwidths, and subgroups.

The finding raises a broader question about the architecture of place-based policy: when does labeling a community as "disadvantaged" actually change what happens to it? The answer may depend less on the sophistication of the targeting algorithm than on whether downstream implementers—states, firms, lenders—respond to the label with real resources. As governments increasingly rely on data-driven tools to allocate social spending, understanding the gap between algorithmic targeting and realized investment is essential.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

Project Repository: https://github.com/SocialCatalystLab/ape-papers
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A. Data Appendix

CEJST Data. Tract-level data from the Climate and Economic Justice Screening Tool version 1.0 were retrieved from the Council on Environmental Quality's archived ArcGIS Feature Service (`usa_november_2022` layer) in March 2025. The query retrieved all 73,767 census tract records with fields including GEOID10 (11-digit 2010 FIPS code), designation status (SN_C), low-income percentile score (P200_I_PFS), population (TPF), eight burden category indicators, burden percentile scores, and demographic composition variables. Records with missing income percentile scores were excluded from the RDD sample (0 tracts dropped).

EV Charging Stations. Data on public electric vehicle charging stations were downloaded from the National Renewable Energy Laboratory's Alternative Fuel Station Locator API (`developer.nrel.gov/api/alt-fuel-stations`). The extract includes all electric stations with status "E" (existing) and U.S. location. Stations were geocoded to 2020 census tracts via point-in-polygon spatial join using the Census Bureau's cartographic boundary file (`cb_2020_us_tract_500k`). The pre-CEJST period covers November 1, 2020 through October 31, 2022; the post-CEJST period covers November 1, 2022 through January 20, 2025. CEJST uses 2010 tract boundaries while the shapefile uses 2020 boundaries; approximately 73.6% of tracts have identical boundaries across vintages, with the remainder experiencing minor boundary changes.

HMDA Data. Mortgage origination data come from the Home Mortgage Disclosure Act via the Consumer Financial Protection Bureau's Data Browser API. I retrieve originated (action_taken = 1) home-purchase (loan_purpose = 1) loans for 10 large states in 2021 (pre-treatment) and 2023 (post-treatment). Tract-level origination counts are merged to the CEJST data by 11-digit FIPS code.

B. Identification Appendix

McCrary Density Test. The Cattaneo et al. (2020a) density test at the 65th percentile income threshold yields a test statistic of 1.02 and a p-value of 0.31 (jackknife variance estimator), failing to reject the null hypothesis of no discontinuity in the density of the running variable. The income percentile score exhibits mass points (97 unique values), which is expected for a variable constructed from Census data. The density test accounts for this discreteness.

Donut RDD. Excluding observations within narrow windows around the cutoff (0.5, 1, 2, and 3 percentile points) produces results qualitatively similar to the baseline.

Placebo Cutoffs. Testing at six false cutoffs (±5, ±10, ±15 percentile points from the real threshold) yields no significant discontinuities in any new EV charger, confirming that the estimated null at the true cutoff is not an artifact of the estimation method.

C. Robustness Appendix

Results are robust to: (1) quadratic and cubic polynomial specifications; (2) Epanechnikov and uniform kernels; (3) bandwidths ranging from 0.5 to 1.5 times the MSE-optimal bandwidth; (4) donut RDD specifications excluding observations near the cutoff; and (5) regional subsamples (Northeast, South, Midwest, West). All specifications produce statistically insignificant point estimates for EV charger outcomes.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Any New EV Charger	-0.0034	0.0108	0.331	-0.0103	0.0325	Small negative
EV Charger Count	-0.0077	0.0283	1.225	-0.0063	0.0231	Small negative
EV Charger Change	-0.0514	0.0450	1.705	-0.0302	0.0264	Small negative
Mortgage Originations	-0.4815	1.6353	41.213	-0.0117	0.0397	Small negative
Origination Change	0.8814	2.4912	89.381	0.0099	0.0279	Small positive

Notes: Standardized effect sizes: SDE $= \hat{\beta}/\mathrm{SD}(Y)$. Treatment is binary CEJST disadvantaged designation (0/1) via fuzzy RDD at the 65th percentile income threshold. SD(Y) is unconditional.

Research question: Does CEJST disadvantaged community designation increase local EV infrastructure investment and mortgage credit? **Treatment:** Binary CEJST designation, instrumented by the income threshold. **Data:** CEJST (Nov 2022), NREL AFDC EV stations (2020–2025), HMDA (2021–2023), census-tract level. **Method:** Fuzzy RDD at 65th percentile income threshold, local linear, MSE-optimal bandwidth. **Sample:** 73,767 US census tracts.

Classification thresholds: large negative (< -0.15), moderate negative (-0.15 to -0.05), small negative (-0.05 to -0.005), null (-0.005 to 0.005), small positive (0.005 to 0.05), moderate positive (0.05 to 0.15), large positive (> 0.15). Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\mathrm{SDE}| < 0.005$), not a failure to reject a null hypothesis.