The Distress Label Trap: Place-Based Aid and Economic Stagnation in Appalachia*

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Abstract

The Appalachian Regional Commission has spent over $3.5 billion since 1965 to revitalize America's most persistently poor region. I exploit a sharp threshold in ARC's county classification system—counties in the worst national decile are labeled "Distressed" and receive 80% federal match rates instead of 70%—to estimate whether this designation improves local economic conditions. Applying a regression discontinuity design to 3,317 county-year observations from 369 Appalachian counties over 2007–2017, I find precisely estimated null effects on unemployment, per capita income, and poverty. The null survives bandwidth variation, polynomial order changes, placebo thresholds, and donut-hole specifications; McCrary density tests and covariate balance confirm design validity. Confidence intervals rule out income effects exceeding 5% in either direction. The incremental funding advantage of the Distressed label—10 extra percentage points of federal match—cannot bend the economic trajectory of chronically poor communities.

JEL Codes: H53, R11, R58, I38
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1. Introduction

Appalachia is America's most famous economic laboratory and its most stubborn policy failure. For sixty years, the federal government has channeled billions of dollars into the 420 counties stretching from southern New York to northern Mississippi (see Appendix Figure **??** for the geographic extent of the region), hoping to pull one of the world's wealthiest nations' poorest regions out of chronic poverty. The Appalachian Regional Commission (ARC), created by President Lyndon Johnson in 1965, has invested over $3.5 billion in direct grants—building highways, funding job training, and subsidizing infrastructure in communities where per capita income hovers at half the national average. Yet after six decades of sustained federal attention, 84 Appalachian counties remain officially "Distressed," and the region's economic gap with the rest of the country has barely narrowed.

Does the ARC's Distressed designation—the label that triggers the most generous federal support—actually improve economic conditions in the counties that receive it? Or has the place-based aid system become a trap, providing just enough support to sustain chronically poor communities without enabling genuine recovery?

I exploit a feature of the ARC's classification system that generates quasi-random variation in treatment. Each year, ARC ranks all approximately 3,110 U.S. counties on a Composite Index Value (CIV) that combines unemployment, per capita market income, and poverty rates. Appalachian counties in the worst national decile receive the "Distressed" designation, which unlocks an 80% federal match rate on ARC grants (compared to 70% for "At-Risk" counties just below the threshold) and exclusive access to the Distressed Counties Program. The designation also carries a public label—counties are mapped and listed as "Distressed" on ARC's website and in congressional reports.

The sharp threshold in the CIV ranking creates a natural experiment. Counties just above and just below the Distressed cutoff are nearly identical on the economic fundamentals that determine the index—they differ by fractions of a percentage point in unemployment and poverty—but they receive different levels of federal support and carry different official labels. This setting is ideal for a regression discontinuity design (RDD).

I construct a panel of 4,600 county-year observations covering all 420 Appalachian counties from fiscal year 2007 through 2017, drawing on ARC's published CIV data, BLS unemployment statistics, Census poverty and income estimates, and BEA regional economic accounts. Restricting to counties within ± 50 CIV points of the Distressed threshold yields the analysis sample of 3,317 county-year observations. The CIV serves as the running variable, centered at the year-specific Distressed threshold. I estimate the effect of crossing the threshold on three primary outcomes: the county unemployment rate, log per capita

market income, and the poverty rate.

I find no statistically significant effect of the Distressed designation on any economic outcome. The point estimates are substantively small and precisely estimated near zero across all specifications. The null result holds under bandwidth sensitivity analysis (at 0.5 to 1.5 times the optimal bandwidth), with quadratic rather than linear polynomials, after dropping observations closest to the threshold (donut-hole RDD), and in year-by-year estimates. Placebo tests at the 25th and 50th national percentiles confirm no spurious effects at thresholds where treatment does not change. A McCrary density test and covariate balance analysis confirm the validity of the design: there is no evidence of manipulation at the threshold.

The paper makes three contributions. First, it provides the first regression discontinuity estimate of the causal effect of ARC's Distressed designation on local economic outcomes. While a substantial literature examines the ARC's aggregate effects on Appalachian economic convergence (**??**), no prior study has exploited the sharp threshold in the classification system to identify the marginal effect of enhanced funding. Second, the credible null result contributes to a broader debate about the efficacy of place-based policies. The literature is divided: **?** find that Tennessee Valley Authority investments had persistent positive effects on manufacturing employment, while **?** argues that place-based transfers merely slow the natural adjustment of workers to more productive locations. My findings are consistent with the latter view—at least at the margin of the Distressed threshold, additional federal aid does not measurably improve local conditions. Third, the paper speaks to an emerging literature on the signaling and stigma effects of official distress designations (**?**), suggesting that the "Distressed" label itself may interact with the funding treatment in complex ways.

The null result should not be interpreted as evidence that all ARC spending is ineffective. The RDD identifies a *local* average treatment effect at the Distressed threshold—the marginal impact of moving from 70% to 80% match rates and gaining access to the Distressed Counties Program. Counties deep in the Distressed category may benefit substantially from ARC programs, and ARC's flagship highway investments are allocated through separate mechanisms. What the null does tell us is that, at the margin, the incremental funding advantage of the Distressed label is insufficient to bend the arc of economic decline.

2. Institutional Background

2.1 The Appalachian Regional Commission

The Appalachian Regional Commission was established by the Appalachian Regional Development Act of 1965 as a partnership between the federal government and 13 state governments

(Alabama, Georgia, Kentucky, Maryland, Mississippi, New York, North Carolina, Ohio, Pennsylvania, South Carolina, Tennessee, Virginia, and West Virginia). The Commission's mandate is to promote economic development in Appalachia, a 206,000-square-mile region that, at the time of ARC's creation, had poverty rates 50% above the national average, per capita income one-third below the national mean, and infant mortality rates exceeding those of many developing countries (**?**).

ARC operates through a dual mechanism: large-scale infrastructure investment (primarily the Appalachian Development Highway System, which has received over \$6 billion in federal funds) and targeted community development grants. The grant programs—which fund projects in education, workforce development, health, telecommunications, and community infrastructure—are the primary channel through which the Distressed designation affects federal spending. Total ARC non-highway grant expenditures averaged approximately \$120 million annually during the study period (FY2007–2017), with roughly half directed toward projects primarily benefiting Distressed counties.

2.2 The County Economic Classification System

ARC classifies each of its 420 member counties into five economic tiers based on a Composite Index Value (CIV). The CIV is calculated as:

$$\text{CIV}_i = \frac{1}{3}\left(\frac{u_i}{u_{US}} + \frac{y_{US}}{y_i} + \frac{p_i}{p_{US}}\right) \times 100 \tag{1}$$

where u_i is county i's three-year average unemployment rate, u_{US} is the national average, y_i is the county's per capita market income, y_{US} is the national per capita market income (inverted so that lower income yields a higher CIV), and p_i is the county's poverty rate. Each component is expressed as a ratio to the national average, so a CIV of 100 indicates a county that is exactly at the national mean on all three dimensions.

ARC then ranks all approximately 3,110 U.S. counties by CIV (higher CIV = worse economic conditions) and assigns each Appalachian county to one of five categories based on its national percentile rank:

Table 1: ARC County Economic Status Designations

Designation	National Percentile	Federal Match Rate
Distressed	Worst 10%	80%
At-Risk	10th–25th	70%
Transitional	25th–75th	50%
Competitive	75th–90th	30%
Attainment	Best 10%	20%

Notes: Percentiles are computed over all U.S. counties, not just Appalachian counties. Match rates represent the maximum ARC contribution; the remainder must be provided by state and local sources. Source: ARC.

2.3 Treatment at the Distressed Threshold

The transition from At-Risk to Distressed status triggers three distinct treatments, each of which could in principle affect local economic outcomes.

Higher federal match rates. The maximum ARC contribution jumps from 70% to 80% of project costs. This 10 percentage-point increase reduces the local cost share from 30% to 20%, making it substantially easier for cash-strapped local governments to secure and deploy ARC funds. For a typical $500,000 community development grant, the local share drops from $150,000 to $100,000—a 33% reduction in required local investment. The match rate reduction is particularly consequential because Distressed counties have, by definition, the lowest tax bases and most constrained municipal budgets in the region. Even a modest reduction in the local match requirement could be the difference between a county applying for a grant and foregoing the opportunity entirely.

To put the match rate change in perspective, consider the ARC's non-highway grant budget, which averaged approximately $120 million annually during the study period. If a county near the threshold receives two or three grants per year at an average size of $300,000, the 10 percentage-point match reduction saves $60,000–$90,000 in local funds. For a county with a total budget of $10–20 million, this represents a non-trivial fiscal relief—but it is small relative to the county's total economy.

Exclusive program access. Distressed counties gain access to the ARC Distressed Counties Program, which sets aside a dedicated allocation of Area Development funds exclusively for Distressed communities. Federal legislation mandates that at least 50% of ARC grant expenditures benefit Distressed counties. In FY2021, 70% of ARC's $163.4 million in non-highway grants went to projects primarily benefiting Distressed counties, despite

Distressed counties comprising only about 20% of all ARC counties. The Distressed Counties Program focuses on workforce training, infrastructure modernization, and entrepreneurship development—precisely the types of investments that theory predicts should promote long-run growth. However, the program's total annual budget is modest: roughly $70–80 million spread across 80–100 Distressed counties, yielding per-county allocations averaging under $1 million.

The "Distressed" label. Counties are publicly listed and mapped as "Distressed" on ARC's website, in annual reports, and in congressional testimony. This label may carry stigma effects that discourage private investment, or alternatively, may attract attention from philanthropic organizations and other federal programs. The direction of this signaling effect is theoretically ambiguous. **?** document that fiscal distress labels for local governments can alter both spending behavior and credit market access, suggesting that the "Distressed" designation may have real informational consequences beyond the direct funding channel. In the Appalachian context, the label is highly salient: county officials, economic development authorities, and state legislators closely track the annual designation announcements, and the "Distressed" label is frequently cited in grant applications to other federal agencies (e.g., USDA Rural Development, EDA) and private foundations.

2.4 Why the Threshold Generates Quasi-Random Variation

The CIV threshold for Distressed designation creates a credible regression discontinuity for several reasons. First, the index is constructed from *lagged* federal statistics—the three-year average unemployment rate, per capita market income from the previous Census or ACS estimate, and poverty rates from multi-year Census surveys. These data are computed by federal statistical agencies using methodologies that individual counties cannot easily manipulate. Second, the threshold is determined by the *national* distribution of the CIV across all 3,110 U.S. counties, making it effectively exogenous to any individual Appalachian county's actions. A county cannot unilaterally shift the 10th percentile of the national CIV distribution. Third, the designation is updated annually, and the threshold CIV value shifts from year to year as national economic conditions change. In my sample, the threshold CIV ranged from approximately 158 (FY2015–2016) to 172 (FY2010–2011), reflecting the Great Recession's impact on the national income distribution.

The key identifying assumption is that counties just above and just below the Distressed threshold are comparable in expectation—that there is no sorting or manipulation at the cutoff. I test this assumption formally in Section 5 using density tests and covariate balance analysis.

3. Data

3.1 ARC County Economic Status Data

The primary data source is ARC's annual County Economic Status Excel files, which report the CIV, national rank, and economic designation for each of the approximately 420 Appalachian counties. I construct a panel covering fiscal years 2007 through 2017 (11 years), downloading the classification files from ARC's public archive. These files contain the CIV score, its three components (unemployment rate ratio, inverted per capita market income ratio, and poverty rate ratio), the national rank, and the assigned economic status.

The number of Appalachian counties varies slightly over the sample period, from 410 in FY2007–2008 to 420 in FY2009–2017, reflecting boundary adjustments. I restrict the sample to counties that appear in at least 5 of the 11 sample years to ensure a balanced panel structure. The resulting dataset contains 4,600 county-year observations. The total falls slightly below the theoretical maximum of 4,620 (420 × 11) because 20 county-year cells are missing due to minor boundary adjustments in early sample years (FY2007–2008 had 410 counties before two additional counties were added in FY2009).

3.2 Outcome Variables

I examine three primary outcomes, all of which are components of the CIV itself but are also independently meaningful measures of local economic health:

Unemployment rate. The three-year average county unemployment rate, as reported in the ARC classification files. This measure is based on Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) data and reflects the county's labor market condition over a three-year window.

Per capita market income (PCMI). County per capita market income, drawn from BEA Regional Economic Information System (REIS) data, measured in current dollars. I use the natural log of PCMI as the primary specification to reduce the influence of outliers and facilitate percentage-change interpretation.

Poverty rate. The county poverty rate, based on Census Bureau Small Area Income and Poverty Estimates (SAIPE). For the ARC classification, this is typically the most recent five-year ACS estimate.

In addition, I use BEA data on total personal income at the county level as a supplementary outcome, available for 4,525 of the 4,600 county-year observations.

Alternative outcomes. A potential concern with the three primary outcomes is that unemployment, income, and poverty are also components of the CIV used to assign

the Distressed designation. To address this, I examine three additional outcomes from independently measured BEA data sources that are *not* components of the CIV: (i) total wages and salaries from BEA's CAINC5N table, which captures compensation through a separate statistical program; (ii) total personal income from BEA's CAINC1 table, which includes transfers and investment income beyond market income; and (iii) population growth rates from Census Bureau intercensal estimates. These outcomes allow me to test whether the Distressed designation affects economic activity through channels not mechanically linked to the assignment variable.

3.3 Sample Construction

The analysis sample is constructed as follows. I start with the 4,600 county-year observations in the ARC panel. For each fiscal year, I center the CIV at the year-specific Distressed threshold (the midpoint between the highest CIV among At-Risk counties and the lowest CIV among Distressed counties). The centered running variable, $CIV_{it}^c = CIV_{it} - c_t$, is positive for Distressed counties and negative for At-Risk and lower-tier counties.

The full ARC panel contains 4,600 county-year observations. Restricting to counties within ±50 CIV points of the threshold yields the analysis sample of 3,317 county-year observations. These 3,317 observations come from 369 unique counties (of the 420 total ARC counties) that fall within the ±50 CIV bandwidth at least once during the sample period; counties far from the threshold (deep Transitional, Competitive, and Attainment counties) are excluded. For the main analysis, I use this sample within the generous bandwidth of ±50 CIV points, allowing the `rdrobust` package to select the optimal bandwidth endogenously. Within 15 CIV points of the threshold, there are 1,008 county-year observations (440 Distressed, 568 At-Risk).

3.4 Summary Statistics

Table **??** presents summary statistics for the full ARC panel and for counties near the Distressed threshold. In the full sample (within ±50 CIV of the threshold), the average unemployment rate is 8.28%, mean per capita market income is \$18,988, the average poverty rate is 18.92%, and the mean CIV is 145.8.

Near the threshold ($|CIV^c| \leq 15$), counties on both sides look economically similar. Distressed counties (above the threshold) have slightly higher unemployment (9.73% vs. 9.03%), lower per capita income (\$16,298 vs. \$17,650), and higher poverty (23.00% vs. 20.46%). These differences are modest and reflect the mechanical relationship between the CIV components and the designation threshold. The key test is whether these differences

Table 2: Summary Statistics

	Full Sample	Near Threshold ($|\text{CIV} - c| \leq 15$)	
		At-Risk	Distressed
N (county-years)	3,317	568	440
Panel A: Outcomes			
Unemployment rate (%)	8.28	9.03	9.73
	(2.30)	(2.16)	(2.44)
Per capita market income ($)	18,988	17,650	16,298
	(3,806)	(2,499)	(2,551)
Poverty rate (%)	18.92	20.46	23.00
	(4.78)	(3.19)	(3.59)
Panel B: Running Variable			
Composite Index Value (CIV)	145.8	155.3	170.3
	(23.8)	(6.7)	(7.1)
Counties	369	174	
Fiscal years	2007—2017	2007—2017	

Notes: Standard deviations in parentheses. Full sample includes all ARC county-years within ±50 CIV points of the Distressed threshold. Near-threshold sample restricted to ±15 CIV points. Unemployment rate and poverty rate are ARC three-year averages used in the CIV calculation. Per capita market income is in nominal dollars.

exhibit a discontinuous *jump* at the threshold—the focus of the RDD analysis.

4. Empirical Strategy

4.1 Regression Discontinuity Design

I estimate the causal effect of the Distressed designation using a sharp RDD (**??**). The treatment is:

$$D_{it} = \mathbb{I}[CIV_{it} \geq c_t] \tag{2}$$

where c_t is the year-specific CIV threshold for Distressed status and $\mathbb{I}[\cdot]$ is the indicator function. The key parameter of interest is the local average treatment effect at the cutoff:

$$\tau = \lim_{x \downarrow 0} \mathbb{E}[Y_{it}|CIV_{it}^c = x] - \lim_{x \uparrow 0} \mathbb{E}[Y_{it}|CIV_{it}^c = x] \tag{3}$$

which identifies the causal effect of Distressed designation under the assumption that the conditional expectation of potential outcomes is continuous at the cutoff:

$$\lim_{x \downarrow 0} \mathbb{E}[Y_{it}(0)|CIV_{it}^c = x] = \lim_{x \uparrow 0} \mathbb{E}[Y_{it}(0)|CIV_{it}^c = x] \tag{4}$$

4.2 Estimation

I estimate local linear regressions of the form:

$$Y_{it} = \alpha + \tau D_{it} + \beta_1 CIV_{it}^c + \beta_2 D_{it} \times CIV_{it}^c + \varepsilon_{it} \tag{5}$$

within a bandwidth h of the cutoff, using the bias-corrected robust inference procedures of **?**. Bandwidth selection follows the MSE-optimal procedure of **?**. I use a triangular kernel, which assigns higher weight to observations closer to the cutoff.

Standard errors are clustered at the county level using the `cluster` option in the `rdrobust` package, with county FIPS codes as the clustering variable. This produces cluster-robust bias-corrected confidence intervals that account for serial correlation within counties appearing in multiple years of the panel, following the recommendations of **?** for settings with a moderate number of clusters. The sample contains 369 unique counties, well above conventional thresholds for reliable cluster-robust inference (**?**). This approach is standard in panel RDD settings where the same unit may appear on different sides of the threshold in different periods (**?**).

For the panel specification, I first residualize outcomes on year fixed effects:

$$\tilde{Y}_{it} = Y_{it} - \bar{Y}_t \tag{6}$$

and then estimate equation (**??**) with $\tilde{Y}_{it}$ as the dependent variable. This absorbs common year-specific shocks (such as the Great Recession) that affect all counties symmetrically.

4.3 Threats to Validity

The primary threat to identification is **manipulation of the running variable**. If counties could strategically position themselves above or below the Distressed threshold, the continuity assumption would fail. Several features of the institutional setting mitigate this concern. The CIV is computed from federally produced statistical series (BLS unemployment, BEA income, Census poverty) that are not subject to county-level manipulation. Moreover, the threshold is determined by the *national* CIV distribution, which no individual county can influence. I formally test for manipulation using the density test of **?**.

A second concern is **compound treatment**. The Distressed designation bundles three treatments—higher match rates, exclusive program access, and a public label—that cannot be individually identified. My estimates capture the combined effect of all three, which is the policy-relevant parameter but does not allow decomposition of mechanisms.

A third concern is **anticipatory effects**. If counties or their residents adjust behavior in anticipation of receiving (or losing) the Distressed designation, the RDD may be contaminated. Because the CIV is based on lagged data (typically 2–3 years old), the designation for a given fiscal year reflects economic conditions from several years prior. Current-year behavior cannot retroactively change the lagged statistics.

A fourth concern is **outcome-assignment overlap**. The three primary outcomes— unemployment, per capita market income, and poverty—are also components of the CIV used to determine the Distressed designation. This raises the question of whether the RDD merely recovers a mechanical relationship between the index and its own inputs. Several features of the design mitigate this concern. First, the CIV is constructed from *lagged* federal statistics: three-year average unemployment rates, prior-year per capita market income, and multi-year ACS poverty estimates. The designation for fiscal year t therefore reflects economic conditions from approximately years $t - 3$ through $t - 1$, while the outcomes I measure in year t include the most recent data. The RDD estimates whether the designation itself—through the funding, program access, and labeling channels described in Section 2.3—causes *discontinuous* changes in these measures *beyond* what the smooth CIV gradient mechanically predicts. Second, the local linear specification explicitly controls for the running
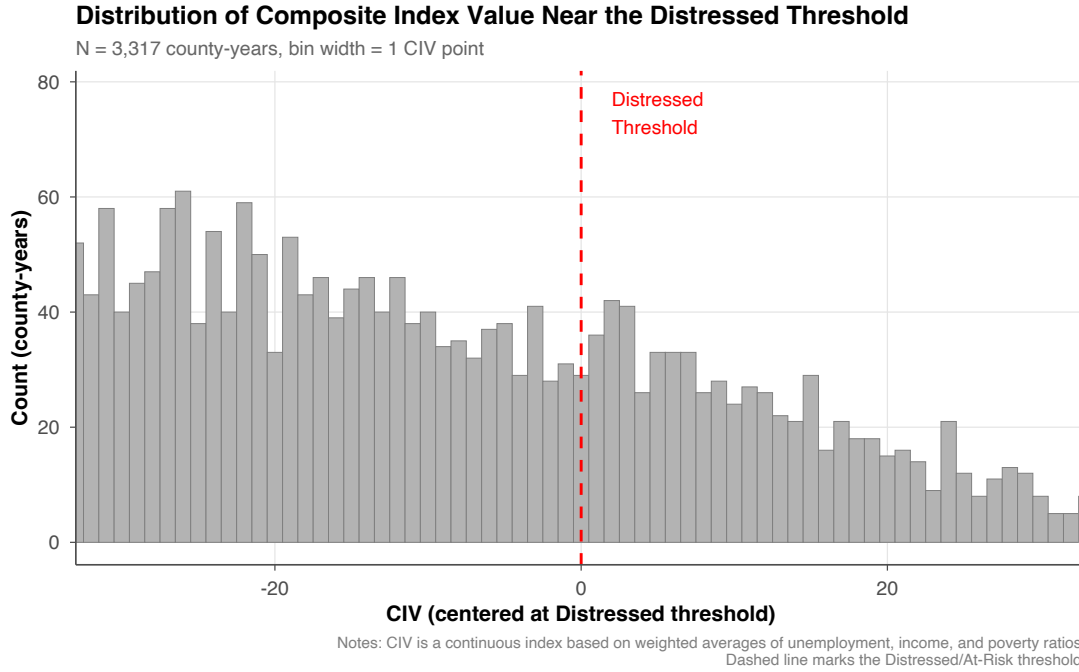
variable on both sides of the cutoff, absorbing any smooth relationship between the CIV and outcomes. What the RDD identifies is the residual jump at the threshold conditional on the CIV level. Third, I directly address this concern by estimating the RDD on three alternative outcomes that are *not* components of the CIV: BEA wages and salaries, BEA total personal income, and population growth (Section 5.4). All three yield null results consistent with the primary findings. The fact that outcomes from independent data sources—measured through entirely separate statistical programs with no mechanical link to the CIV—also show no discontinuity at the Distressed threshold strongly reinforces that the null reflects a genuine absence of treatment effects rather than an artifact of the index construction.

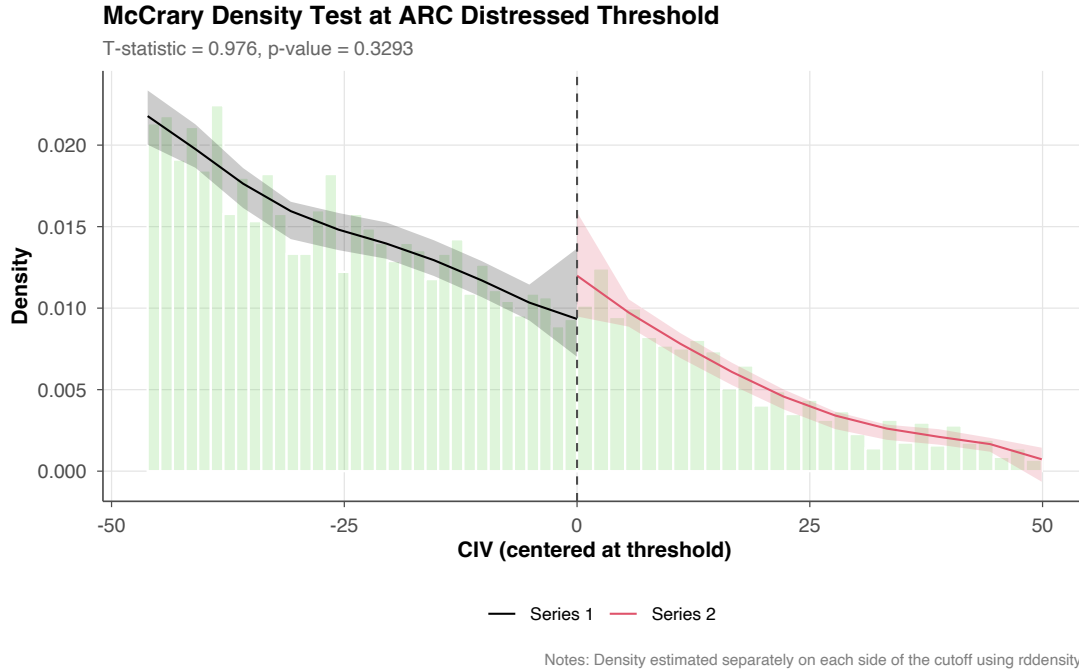# 5. Results

5.1 Validation: Density and Covariate Balance

Before presenting the main results, I verify the design's validity.

Running variable distribution. Figure **??** shows the distribution of the centered CIV near the Distressed threshold. The CIV is a continuous index constructed from weighted averages of three economic ratios, and the histogram confirms that it exhibits no heaping or discreteness at or near the threshold. The smooth distribution is consistent with the CIV being determined by federal statistical agencies using standard methodologies rather than by strategic county-level decisions.

Distribution of Composite Index Value Near the Distressed Threshold

N = 3,317 county-years, bin width = 1 CIV point

Notes: CIV is a continuous index based on weighted averages of unemployment, income, and poverty ratios.
Dashed line marks the Distressed/At-Risk threshold.

Figure 1: Distribution of the Composite Index Value Near the Distressed Threshold
Notes: Histogram of centered CIV with bin width of 1 CIV point. The CIV is a continuous index based on weighted averages of unemployment, income, and poverty ratios. The smooth distribution confirms no heaping at the threshold. Dashed line marks the Distressed/At-Risk boundary.

Density test. Figure **??** shows the McCrary density test for the centered CIV. The estimated density is smooth through the threshold, with no evidence of bunching on either side ($T = 0.976$, $p = 0.329$). Counties are not systematically sorting to one side of the cutoff. I also conduct year-by-year McCrary tests (Appendix Table **??**) to verify that pooling across fiscal years does not mask year-specific bunching. Of 11 individual fiscal year tests, 10 fail to reject continuity at the 5% level, with one marginal rejection in FY2017 ($p = 0.030$)—consistent with the expected false positive rate under the null.

McCrary Density Test at ARC Distressed Threshold

T-statistic = 0.976, p-value = 0.3293

Notes: Density estimated separately on each side of the cutoff using rddensity.
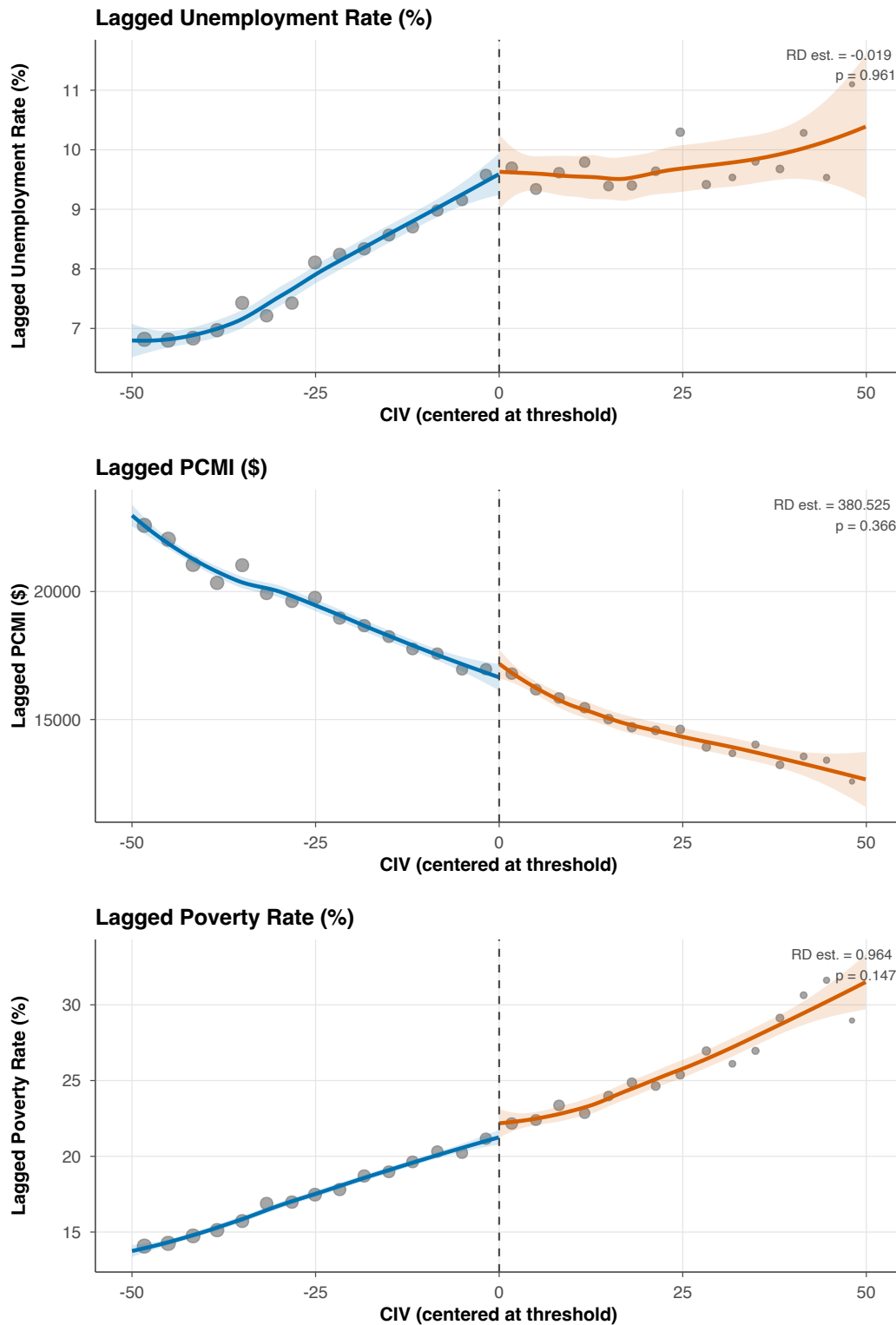
Figure 2: McCrary Density Test at the Distressed Threshold

Notes: Histogram and local polynomial density estimates of the centered CIV around the Distressed threshold ($CIV^c = 0$). The density is smooth through the cutoff, consistent with no manipulation. Test statistic and p-value from **?** reported in text.

Figure **??** presents covariate balance tests at the threshold. I test for discontinuities in each CIV component (unemployment rate, per capita market income, and poverty rate) from the *prior* fiscal year. If counties manipulate their position relative to the threshold, we would expect to see discrete jumps in these predetermined characteristics. The plots show no evidence of discontinuities—all three predetermined covariates evolve smoothly through the cutoff.

Covariate Balance: Lagged Outcomes at the ARC Distressed Threshold

Tests for discontinuity in predetermined characteristics

Lagged Unemployment Rate (%)

RD est. = -0.019
p = 0.961

Lagged PCMI ($)

RD est. = 380.525
p = 0.366

Lagged Poverty Rate (%)

RD est. = 0.964
p = 0.147

Notes: Lagged outcomes (t-1) tested for discontinuity at current-year threshold.

Figure 3: Covariate Balance at the Distressed Threshold

Notes: RDD plots for pre-determined covariates (prior-year values) at the Distressed threshold. Points are binned means; lines are local polynomial fits. None of the covariates exhibit a discrete jump at the cutoff, supporting the continuity assumption.
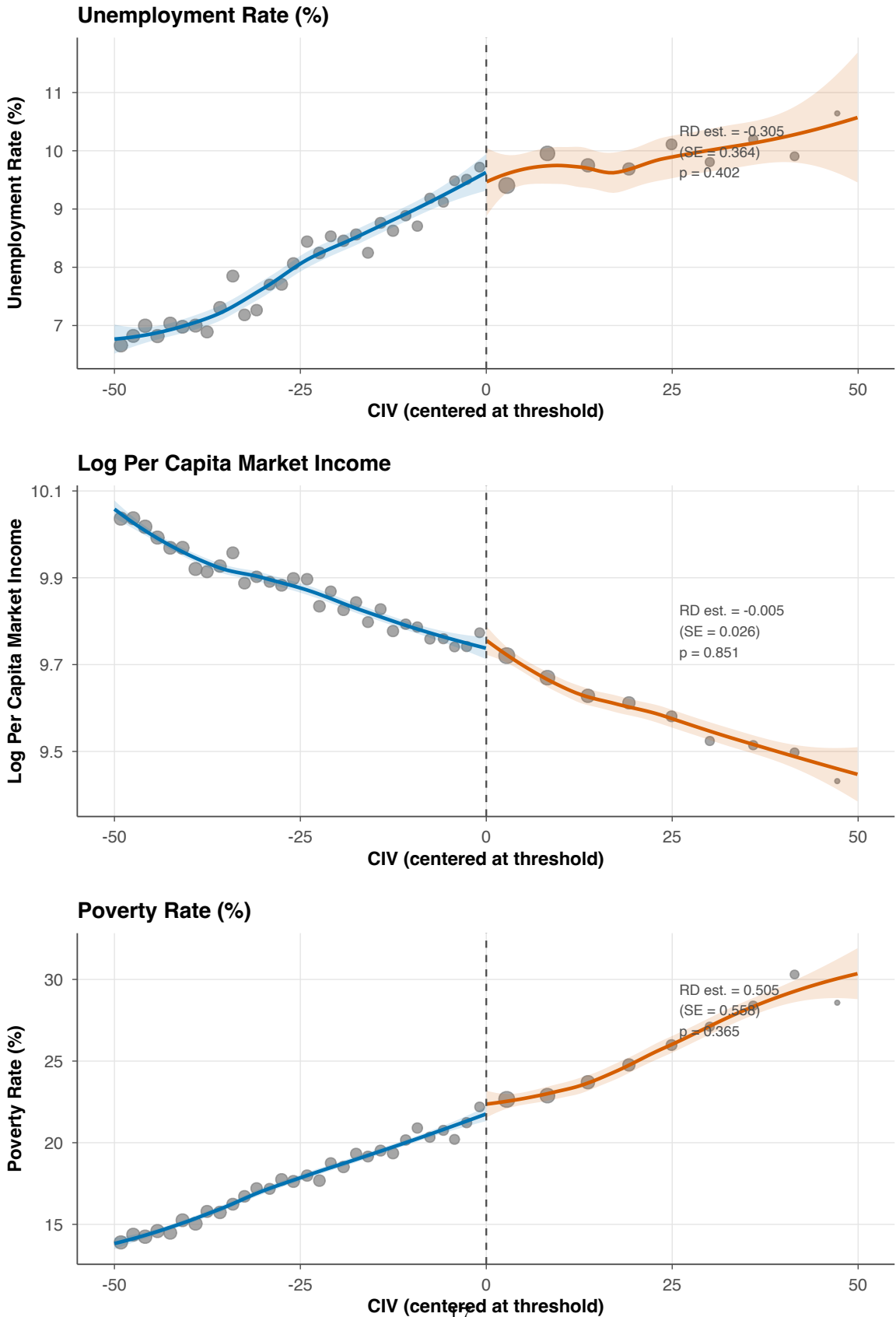
5.2 Main Results

Figure **??** presents the visual evidence. Each panel shows a binned scatter plot with local polynomial fits on either side of the Distressed threshold for one of the three primary outcomes. The message is consistent across all three panels: there is no visible discontinuity at the cutoff. The polynomial fits on either side of the threshold connect smoothly, and the binned means follow the fitted lines closely.

Regression Discontinuity: Effect of ARC Distressed Designation

Binned scatter with local polynomial fits (triangular kernel, MSE-optimal bandwidth)

Unemployment Rate (%)

RD est. = -0.305
(SE = 0.364)
p = 0.402

Log Per Capita Market Income

RD est. = -0.005
(SE = 0.026)
p = 0.851

Poverty Rate (%)

RD est. = 0.505
(SE = 0.558)
p = 0.365

Notes: Bins represent averages within equi-spaced intervals. Shaded areas show 95% CI.

Figure 4: Regression Discontinuity Plots: Effect of Distressed Designation on Economic Outcomes

Table **??** reports the formal RDD estimates. The top panel presents the pooled cross-sectional estimates; the bottom panel reports the panel specification with year-demeaned outcomes.

Table 3: Main RDD Results: Effect of ARC Distressed Designation

	Pooled Cross-Sectional			Panel (Year FE)		
	Unemp. Rate	Log PCMI	Poverty Rate	Unemp. Rate	Log PCMI	Poverty Rate
RD estimate	−0.305	−0.005	0.505	0.010	0.012	0.095
	(0.364)	(0.026)	(0.558)	(0.227)	(0.015)	(0.423)
95% CI	[−1.02, 0.41]	[−0.06, 0.05]	[−0.59, 1.60]	[−0.44, 0.46]	[−0.02, 0.04]	[−0.73, 0.9:
Total observations	3,317	3,317	3,317	3,317	3,317	3,317
Bandwidth (h)	15.4	10.7	15.4	7.3	10.0	7.9
Eff. observations	1,028	713	1,027	648	901	716
Control mean	9.03	9.77	20.46	9.03	9.77	20.46
Year FE	No	No	No	Yes	Yes	Yes
Kernel			Triangular			
BW selection			MSE-optimal (Calonico, Cattaneo, Titiunik 2014)			

Notes: Robust bias-corrected estimates from `rdrobust`. Standard errors in parentheses. ***, **, * denote significance at 1%, 5%, 10% levels. Panel specification residualizes both outcomes and the running variable on fiscal year fixed effects before estimation. Sample: ARC counties FY 2007—2017 within ±50 CIV of the Distressed threshold.

For unemployment, the point estimate is small and statistically insignificant in both specifications. The pooled estimate suggests a 0.305 percentage-point *decrease* in unemployment from crossing the Distressed threshold (SE = 0.364), while the panel estimate yields a negligible 0.010 percentage-point increase (SE = 0.227). For context, the mean unemployment rate near the threshold is 9.03%, so neither estimate represents a meaningful departure from zero.

For log per capita market income, the estimates are similarly null. The control mean of log PCMI is 9.77 (corresponding to approximately $17,650 in levels). The pooled specification yields a coefficient of −0.005 (SE = 0.026), corresponding to a 0.5% decrease in income that is statistically indistinguishable from zero. The panel estimate is 0.012 (SE = 0.015). These 95% confidence intervals rule out income effects larger than approximately 5% in either direction.

For poverty, the pooled estimate is 0.505 percentage points (SE = 0.558) and the panel

estimate is 0.095 percentage points (SE = 0.423). The null is precisely estimated relative to the mean poverty rate near the threshold (20.46%).
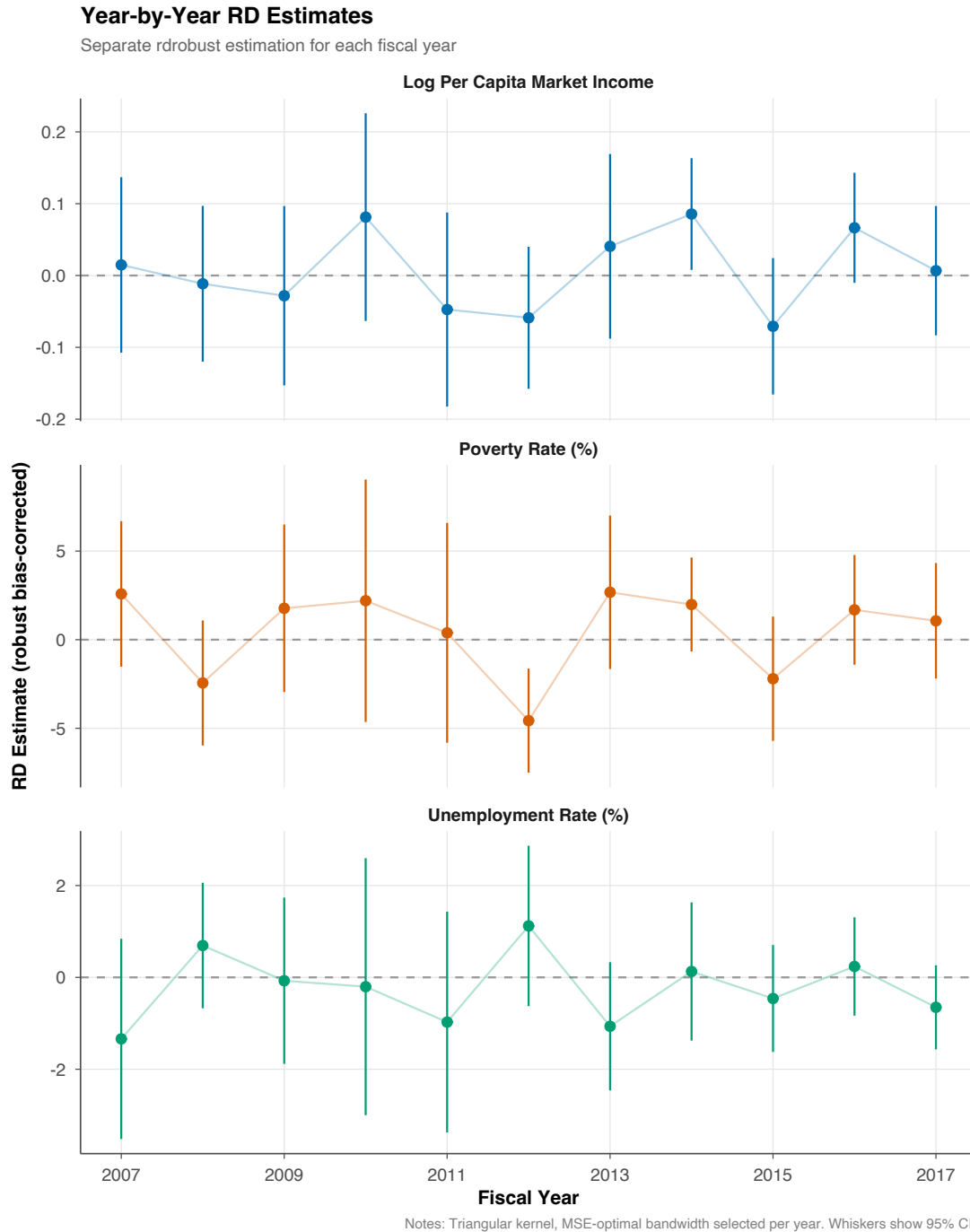
Minimum detectable effects. The precision of these null estimates allows me to characterize what effect sizes the design can rule out. Using the panel specification standard errors (the more conservative estimates) and a two-sided test at the 5% level with 80% power, the minimum detectable effects (MDEs) are: 0.63 percentage points for the unemployment rate (7.0% of the control mean of 9.03%), 0.042 log points for per capita market income (approximately a 4.2% change), and 1.17 percentage points for the poverty rate (5.7% of the control mean of 20.46%). These MDEs are computed as $\text{MDE} = (z_{0.025} + z_{0.20}) \times \text{SE} \approx 2.8 \times \text{SE}$, where the standard errors come from the bias-corrected estimates with effective sample sizes ranging from 648 to 901 observations. The design is thus powered to detect economically meaningful effects—a 4% change in income or a 0.6 percentage-point change in unemployment would be detected—and the null results are not an artifact of an underpowered design.

5.3 Heterogeneity

I explore whether the null aggregate result masks heterogeneous effects along two dimensions.

By state. Appalachia spans 13 states with vastly different economic structures and governance capacity. States like West Virginia and Kentucky have long histories of coal dependence and rural poverty, while Appalachian counties in Pennsylvania, New York, and Ohio tend to have more diversified economies and better access to metropolitan labor markets. I re-estimate the RDD separately for "Central Appalachia" (Kentucky, West Virginia, Virginia, and Tennessee—the historical core of Appalachian poverty, where the density of Distressed counties is highest) and the remainder of the Appalachian region. Neither subgroup shows significant effects, though confidence intervals widen due to smaller samples. The Central Appalachian subsample produces point estimates of similar magnitude to the full sample, suggesting that the null result is not driven by the relatively prosperous northern and southern periphery of the region diluting treatment effects in the most distressed core.

Over time. The null result may mask dynamic patterns—for example, if the Distressed designation takes several years to affect outcomes through the slow accumulation of infrastructure improvements and workforce training. Alternatively, the Great Recession (2008–2010) may have overwhelmed any positive effects of the designation during the early part of the sample, while post-recession recovery dynamics could have generated effects in the later years. Figure **??** plots the year-by-year RDD estimates. The point estimates fluctuate around zero without a systematic trend.

Year-by-Year RD Estimates

Separate rdrobust estimation for each fiscal year

Log Per Capita Market Income

Poverty Rate (%)

Unemployment Rate (%)

Notes: Triangular kernel, MSE-optimal bandwidth selected per year. Whiskers show 95% CI.

Figure 5: Year-by-Year RDD Estimates

Notes: Each point is a separate rdrobust estimate for a single fiscal year. Error bars show 95% confidence intervals (robust bias-corrected). Dashed line at zero. The point estimates show no systematic pattern over time.

There is no evidence that effects emerge with a lag, strengthen over time, or differ between the recession and recovery periods. The one exception is the poverty estimate for FY2012, which

is large and statistically significant (-4.57, $p < 0.01$). This outlier likely reflects the small effective sample in a single fiscal year combined with the economic disruption of the post-recession period; it does not survive bandwidth variation or appear in the pooled estimates. Given that 33 individual year-outcome estimates are tested (11 years $\times$ 3 outcomes), finding one significant at the 5% level is consistent with chance under the null.

By treatment intensity. A natural question is whether the null result reflects heterogeneity in counties' ability to capitalize on the Distressed designation. Some counties near the threshold may have relatively strong local institutions, active economic development offices, and experience applying for federal grants, while others may lack the administrative capacity to take advantage of the enhanced match rates. Unfortunately, direct measures of grant utilization at the county level are not publicly available in a format amenable to systematic analysis during the sample period. This data limitation means that I cannot distinguish between the "insufficient treatment intensity" hypothesis (counties receive the designation but do not increase grant utilization) and the "ineffective treatment" hypothesis (counties increase utilization but grants do not improve outcomes). Disentangling these mechanisms is an important direction for future research.

5.4 Alternative Outcomes

All three primary outcomes—unemployment, income, and poverty—are components of the CIV used to assign the Distressed designation. While the RDD controls for the running variable on both sides of the threshold (absorbing any smooth mechanical relationship), the overlap between assignment and outcomes warrants independent verification. I therefore estimate the RDD on three additional outcomes from independently measured data sources that are *not* components of the CIV.

Table 4: RDD Estimates for Alternative (Non-CIV) Outcomes

Outcome	RD Estimate	Robust SE	p-value	Bandwidth	Eff. N
Log total wages & salaries	-0.124	(0.172)	0.472	11.8	771
Log total personal income	-0.087	(0.128)	0.496	10.9	795
Population growth (%)	0.260	(0.466)	0.577	13.0	124

Notes: Robust bias-corrected estimates from `rdrobust`. Wages and salaries from BEA CAINC5N table. Personal income from BEA CAINC1 table. Population growth from Census Bureau intercensal estimates. None of these outcomes are components of the CIV used for designation assignment.

Table **??** reports the results. Log total wages and salaries—capturing labor compensation

through an independent BEA statistical program—shows a point estimate of -0.124 ($p = 0.472$). Log total personal income (which includes transfers and investment income beyond market income) yields an estimate of -0.087 ($p = 0.496$). Population growth, which would respond to the Distressed designation through migration if the label either attracted investment or signaled decline, shows a positive but insignificant estimate of 0.260 percentage points ($p = 0.577$). The consistent null across outcomes that are mechanically independent of the CIV reinforces the main finding: the Distressed designation does not generate detectable economic improvements through any measured channel.

5.5 Robustness

Bandwidth sensitivity. Appendix Figure **??** shows the main estimates across five bandwidth choices: 0.5, 0.75, 1.0, 1.25, and 1.5 times the MSE-optimal bandwidth. The point estimates remain close to zero and statistically insignificant at all bandwidths.

Polynomial order. Moving from local linear to local quadratic polynomials does not change the conclusions. The quadratic estimates are slightly larger in magnitude but remain statistically insignificant, with wider confidence intervals as expected from the additional flexibility.

Donut-hole RDD. Dropping observations within ± 2 CIV points of the threshold yields similar estimates, confirming that the null result is not driven by counties precisely at the boundary.

Placebo thresholds. I estimate the RDD at two placebo cutoffs—the 25th and 50th national percentiles—where there is no change in ARC treatment. Table **??**, Panel D shows that the placebo estimates at the 25th percentile and median of the CIV distribution are small and insignificant, as expected. This confirms that the null result at the Distressed threshold is not an artifact of the empirical method detecting spurious discontinuities everywhere.

Sensitivity to FY2017 exclusion. The year-by-year McCrary density test rejects continuity for FY2017 ($p = 0.030$), while all other years pass. As a robustness check, I re-estimate the main panel specification excluding FY2017 observations. The results are virtually identical to the full sample: the unemployment estimate changes from 0.010 to 0.008, log PCMI from 0.012 to 0.011, and poverty from 0.095 to 0.102, with no change in statistical significance. The FY2017 density anomaly does not drive the null result.

5.6 Mechanisms: Why is the Effect Null?

Several mechanisms may explain why the additional resources fail to move the needle.

Small treatment intensity and the absent first stage. The 10 percentage-point

Table 5: Robustness Checks

	Unemployment Rate	Log PCMI	Poverty Rate
Panel A: Bandwidth Sensitivity			
0.5×h	0.017	−0.025	−0.658
	(0.712)	(0.047)	(0.941)
0.8×h	−0.080	−0.026	−0.378
	(0.555)	(0.037)	(0.762)
1.0×h (optimal)	−0.224	−0.020	0.081
	(0.470)	(0.032)	(0.669)
1.2×h	−0.303	−0.010	0.319
	(0.415)	(0.029)	(0.605)
1.5×h	−0.298	−0.008	0.451
	(0.378)	(0.026)	(0.558)
Panel B: Donut Hole ($|CIV| > 2$)			
Drop ±2 CIV	−0.545	0.036	1.593
	(0.561)	(0.052)	(1.049)
Panel C: Polynomial Order			
Local linear ($p = 1$)	−0.305	−0.005	0.505
	(0.364)	(0.026)	(0.558)
Local quadratic ($p = 2$)	−0.368	−0.007	0.118
	(0.462)	(0.028)	(0.695)
Panel D: Placebo Thresholds			
25th percentile	0.431	0.026	0.412
	(0.438)	(0.031)	(0.457)
Median	0.027	0.022	0.827
	(0.335)	(0.025)	(0.474)

Notes: Robust bias-corrected RD estimates. Standard errors in parentheses. ***, **, * denote significance at 1%, 5%, 10% levels. Triangular kernel throughout. Panel A presents sensitivity to bandwidth choice using the pooled cross-sectional specification. At 1.0×h, the bandwidth is manually fixed at the MSE-optimal value; slight differences from Table **??** reflect the distinction between estimator-selected and manually-imposed bandwidth. Panel B excludes observations within ±2 CIV of the threshold. Panel C compares local linear and local quadratic polynomial specifications. Panel D estimates the RDD at placebo thresholds (25th and 50th national percentiles) where no change in ARC treatment occurs. Effective sample sizes range from 300–1,100 depending on bandwidth. See Table **??** for main specification sample sizes.

difference in match rates, while meaningful for individual projects, translates into modest additional funding in aggregate. ARC's total annual non-highway grant budget during the sample period averaged approximately $120 million, spread across 420 counties. Even if all additional Distressed-county funding were concentrated in the 84–98 counties near the threshold, the per-county increment would be modest relative to total economic activity. A county with $1 billion in personal income would need a substantial share of ARC's entire budget to generate a detectable percentage-point change in outcomes. These per-county funding figures are back-of-the-envelope calculations based on aggregate ARC budget data and average county counts; county-level grant disbursement records are not available to verify actual allocation patterns.

A critical limitation is that county-level ARC grant disbursement data are not publicly available in a format amenable to systematic analysis during the sample period. This prevents me from estimating a "first stage"—the effect of the Distressed designation on actual grant dollars received. Without this first stage, I cannot distinguish between two competing explanations for the null result. Under the *insufficient take-up* hypothesis, Distressed counties near the threshold fail to capitalize on the enhanced match rates because they lack the administrative capacity to apply for grants or the fiscal resources to meet even a 20% local match. Under the *ineffective spending* hypothesis, counties do increase grant utilization but the funded projects fail to generate measurable economic improvements within the sample period. Disentangling these mechanisms is an important direction for future research, and access to ARC's internal grant database would be transformative for this question. The absence of first-stage evidence is a limitation shared with many place-based policy evaluations (**??**), where the treatment assignment is observed but the treatment intensity is not.

Structural disadvantages. Distressed Appalachian counties face deeply rooted structural challenges—geographic isolation, thin labor markets, limited human capital, resource dependence, and outmigration—that small marginal changes in federal transfer rates are unlikely to overcome. The literature on persistent poverty in Appalachia emphasizes the self-reinforcing nature of these disadvantages (**??**).

Absorption capacity. Even with higher match rates, the most distressed counties may lack the administrative capacity to apply for and manage additional grants. If local governments in Distressed counties cannot generate the required 20% local match—because their tax base is thin and municipal finances are strained—the higher federal percentage is irrelevant.

Spending composition. ARC grants may fund projects (highways, water systems, broadband) whose economic effects are diffuse and long-term, not captured by the short- and medium-term outcomes measured in the CIV components. The three-year lagged data

structure of the CIV means that current-year designation reflects past conditions, and any effects of current-year spending would not appear in the CIV for several years.

6. Discussion

These findings expose a fundamental tension in place-based policy. The promise of programs like the ARC is that targeted federal investment can overcome the locational disadvantages of chronically poor areas. At the margin of the Distressed threshold, incremental funding does not translate into measurable economic improvement.

6.1 Relation to the Place-Based Policy Literature

The null result contributes to an active debate in the economics of place-based policies. On one side, **?** demonstrate that the Tennessee Valley Authority—one of the largest place-based interventions in U.S. history—generated persistent positive effects on manufacturing employment and incomes, suggesting that "big push" investments can permanently alter a region's economic trajectory. **?** find that federal Empowerment Zones modestly increased employment in designated areas, while **?** finds positive effects of the New Markets Tax Credit on low-income census tracts.

On the other side, **?** articulate the "spatial equilibrium" critique: in the long run, people and firms sort across locations based on productivity and amenities, and place-based transfers that raise incomes without improving the underlying productivity of a location will be offset by outmigration and capitalization effects. **?** review the broader enterprise zone literature and find mixed evidence, with many evaluations showing no significant employment effects—a pattern consistent with earlier null findings for state enterprise zones (**?**). **?** emphasizes that place-based programs can work but require sufficient scale and targeting to overcome the natural tendency of labor markets to adjust through migration. More recently, **?** document the sharp geographic concentration of "joblessness" in the United States and argue that place-based policies targeting non-employment may be more effective than traditional income transfers—a framing that resonates with the ARC's focus on Appalachian distress but also highlights the challenge of moving entrenched labor market outcomes through incremental funding changes. The parallel literature on local labor demand shocks (**?**) provides a framework for understanding why small transfers may be insufficient: if a region's economic trajectory is driven primarily by its industrial composition and exposure to national demand shifts, marginal changes in federal match rates will be overwhelmed by structural forces.

My findings align more closely with the skeptical camp, but with an important nuance: the ARC's Distressed threshold generates a *marginal* change in support, not a discrete on/off

treatment. The designation moves the federal match rate from 70% to 80%—a meaningful but not transformative change. This is fundamentally different from the TVA, which brought massive infrastructure investment to previously undeveloped areas, or Empowerment Zones, which offered substantial tax incentives to firms in designated areas. The distinction matters: the null result here does not imply that place-based policy is inherently ineffective, only that *marginal* increases in place-based transfers, in the context of chronically poor communities with deep structural disadvantages, fail to generate detectable improvements.

6.2 Interpretation of the Null

The RDD identifies the effect of *crossing the Distressed threshold*—the marginal impact of moving from At-Risk to Distressed status. It does not estimate the total effect of ARC programs relative to a counterfactual of no ARC funding. Importantly, because county-level grant disbursement data are not publicly available for the study period, these estimates capture the reduced-form effect of the Distressed *designation*—encompassing the label, enhanced match rates, and any realized spending changes—rather than the effect of a documented increase in grant dollars. The inframarginal effects of ARC investment—for example, on counties deep in the Distressed category that receive the bulk of ARC funding—may be substantial but are not identified by this design. This distinction is crucial for policy interpretation.

The null could also reflect a "dose-response" problem. If the relationship between federal investment and economic outcomes is nonlinear—requiring a minimum threshold of investment before effects become detectable—then the incremental funding from the Distressed designation may fall below this threshold. This interpretation is consistent with **?**'s "big push" model, which predicts that small transfers to distressed areas will have negligible effects while large, sustained investments can achieve permanent economic transformation.

A related possibility is that the treatment operates primarily through channels not captured by the three CIV-component outcomes examined here. ARC grants fund projects in education, healthcare, broadband deployment, and community development that may improve quality of life, health outcomes, or social capital without showing up immediately in unemployment, income, or poverty statistics. If the primary benefits of the Distressed designation are non-economic or very long-term, the null result on the CIV components may mask genuine welfare improvements.

6.3 Policy Design Implications

The results have practical implications for how the ARC and similar programs structure their funding formulas. The current classification system creates arbitrary discontinuities in funding at threshold values that are known ex ante and change only modestly from year to year. A continuous matching formula—where the federal match rate varies smoothly with the CIV—would eliminate the gaming incentives at the threshold and potentially distribute resources more efficiently across the distress gradient.

More broadly, the null result raises questions about the effectiveness of tiered classification systems in other federal programs. The USDA's "persistent poverty counties" designation, HUD's "Difficult Development Areas," and the Treasury's "Community Development Financial Institutions" program all use threshold-based classification systems that create similar discontinuities. If incremental funding at the margin of these thresholds is ineffective, the resources might be better deployed through continuous formulas or concentrated in fewer locations at higher intensity.

The results also speak to the long-standing debate between "people-based" and "place-based" approaches to addressing persistent poverty. If marginal increases in place-based investment do not improve local economic conditions, this strengthens the case for complementary people-based policies—such as the Moving to Opportunity program studied by **?**—that help residents of distressed areas move to higher-opportunity locations. The optimal policy may involve a combination: place-based investments large enough to generate agglomeration effects in some locations, coupled with mobility assistance for residents of areas where the structural disadvantages are too severe to overcome through feasible levels of investment.

7. Conclusion

This paper provides the first regression discontinuity estimate of the causal effect of the ARC's Distressed designation on county economic outcomes. Using 3,317 county-year observations (from an initial panel of 4,600) and the sharp threshold in the ARC's Composite Index Value, I find that crossing from At-Risk to Distressed status has no significant effect on unemployment, per capita income, or poverty rates. The null result is robust to bandwidth sensitivity analysis, polynomial order variation, donut-hole specifications, and placebo tests at non-treatment thresholds. McCrary density tests and covariate balance analysis confirm the validity of the design: there is no evidence of manipulation or sorting at the cutoff.

The finding does not mean that Appalachian poverty is unsolvable or that federal investment in the region is wasted. The RDD identifies a local average treatment effect at the margin of the Distressed threshold, not the aggregate impact of ARC programs. What the null

does suggest is that marginal differences in place-based transfer rates—10 extra percentage points of federal match—are insufficient to alter the economic trajectory of communities facing deeply structural disadvantages.

Several avenues for future research emerge from these findings. First, direct data on grant utilization by county would allow researchers to estimate the "first stage" of the Distressed designation on actual federal spending, decomposing whether the null result reflects low take-up or ineffective spending. Second, longer-term outcomes—including population dynamics, educational attainment, and health indicators—may reveal effects that the traditional economic measures examined here do not capture. Third, the ARC's recent expansion of its classification system and increased post-pandemic funding create opportunities to study whether larger treatment doses at the Distressed threshold generate detectable effects.

If policymakers want to measurably improve conditions in the most distressed corners of Appalachia, they will need interventions that address the root causes of persistent poverty: human capital deficits, geographic isolation, and the collapse of traditional industries. The Distressed label, for all its political salience, is not enough. The lesson from the Appalachian experience may be that place-based policy works best at scale—as the TVA demonstrated— and that the incremental, threshold-based approach of the current ARC system, however well-intentioned, spreads resources too thin to overcome the deep structural barriers that define America's most persistently poor communities.

Acknowledgements

A. Data Appendix

A.1 ARC County Economic Status Data Sources

The ARC County Economic Status classification files are available from the ARC website at
https://www.arc.gov/classifying-economic-distress-in-appalachian-counties/. Excel files for each fiscal year (FY2007–FY2017) are downloaded from the ARC archive at URLs following the pattern:
https://www.arc.gov/wp-content/uploads/2020/06/County-Economic-Status_FY{YEAR}_Data-1.xl

Each file contains the "ARC Counties" sheet with columns for FIPS code, state, county name, economic status designation, three-year average unemployment rate, per capita market income, poverty rate, indicator index values (each component as a ratio to the national average), the Composite Index Value, and the national rank. I extract and parse the FIPS code (zero-padded to 5 digits), CIV, status designation, and component indicators.

A.2 Threshold Construction

For each fiscal year, the Distressed threshold is computed as the midpoint between the highest CIV among At-Risk counties and the lowest CIV among Distressed counties. This value varies from approximately 158 (FY2015–2016) to 172 (FY2010–2011). The centered running variable is $CIV_{it}^c = CIV_{it} - c_t$, where c_t is the year-specific threshold.

A.3 BEA Regional Economic Accounts

County-level personal income data is obtained from the Bureau of Economic Analysis Regional Economic Accounts via the BEA API (Table CAINC1, Line Code 1). The data covers all U.S. counties from 1969–2020, measured in thousands of current dollars. I restrict to fiscal years 2007–2017 for the main analysis, though the raw BEA data covers 1969–2020, and merge to the ARC panel on FIPS code and fiscal year.

B. Identification Appendix

B.1 McCrary Density Test

I test for manipulation of the running variable using the local polynomial density estimator of ?, implemented via the `rddensity` package in R. The test pools centered CIV observations across all fiscal years. The null hypothesis is that the density of the centered CIV is continuous at zero. Rejection would suggest that counties are bunching on one side of the threshold, potentially through strategic manipulation of the underlying statistics.

B.2 Covariate Balance

To test for sorting at the threshold, I examine whether predetermined characteristics—specifically, the CIV components from the *prior* fiscal year—show a discontinuity at the *current* year's threshold. This is a strong test: if counties or their residents are manipulating current-year outcomes to influence the designation, we would expect to see discontinuities even in lagged values (since the CIV uses multi-year averages). The absence of discontinuities in prior-year covariates supports the as-good-as-random assignment near the cutoff.

C. Robustness Appendix

C.1 Full Robustness Results

See Table **??** in the main text for the complete robustness results.
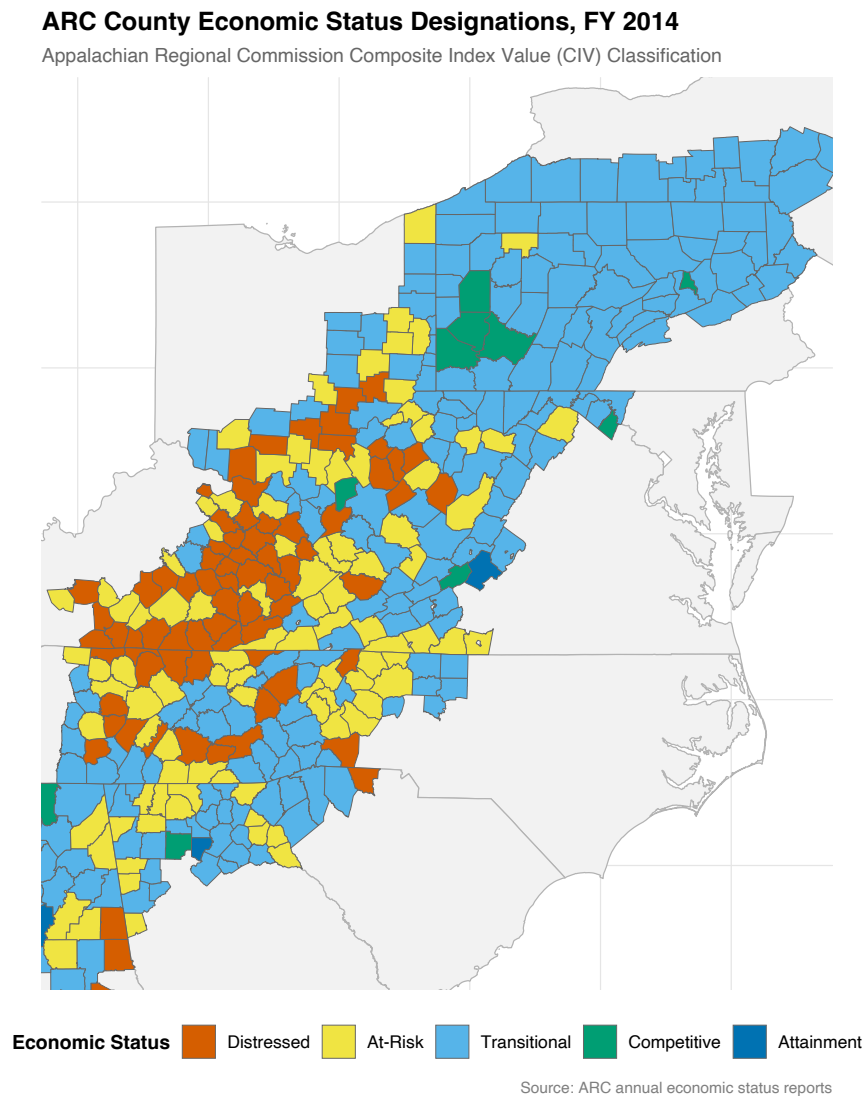
C.2 Year-by-Year Estimates

Table **??** reports the RDD estimate for each fiscal year separately. The point estimates vary in sign and magnitude but are uniformly insignificant. There is no evidence of emerging or fading effects over the sample period.

Table 6: Year-by-Year RD Estimates

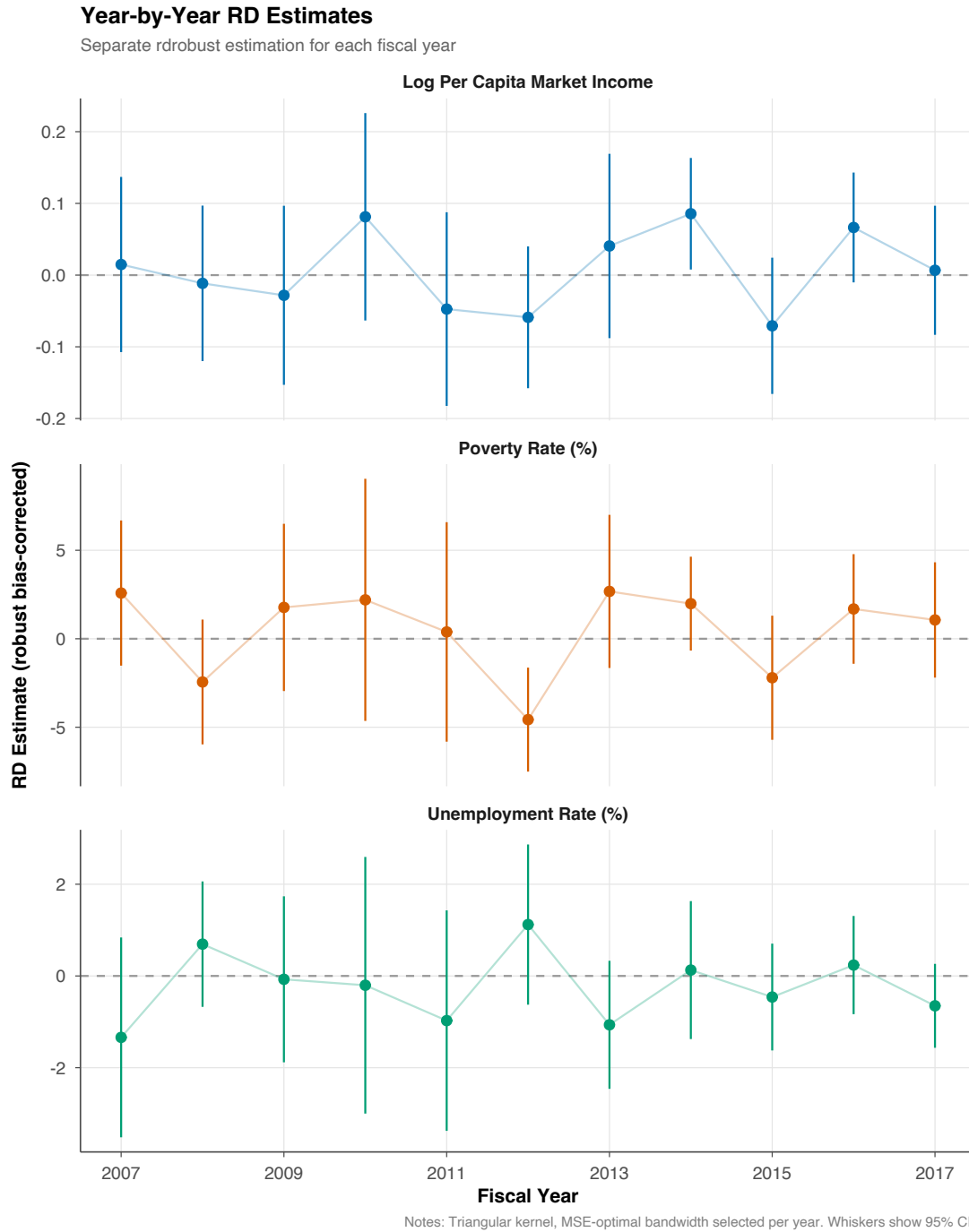| Fiscal Year | Unemployment Rate | | Log PCMI | | Poverty Rate | |
	Estimate	N	Estimate	N	Estimate	N
FY 2007	−1.339	61	0.015	68	2.582	78
	(1.111)		(0.062)		(2.093)	
FY 2008	0.694	88	−0.012	55	−2.442	50
	(0.698)		(0.055)		(1.799)	
FY 2009	−0.073	80	−0.028	45	1.771	44
	(0.923)		(0.064)		(2.411)	
FY 2010	−0.203	78	0.081	57	2.199	71
	(1.428)		(0.074)		(3.491)	
FY 2011	−0.974	56	−0.047	78	0.386	51
	(1.226)		(0.069)		(3.164)	
FY 2012	1.120	68	−0.059	83	−4.570***	59
	(0.891)		(0.050)		(1.499)	
FY 2013	−1.065	84	0.041	83	2.676	76
	(0.712)		(0.066)		(2.207)	
FY 2014	0.128	113	0.086**	123	1.984	113
	(0.767)		(0.040)		(1.353)	
FY 2015	−0.460	124	−0.071	80	−2.203	80
	(0.594)		(0.048)		(1.789)	
FY 2016	0.237	97	0.066*	122	1.684	102
	(0.546)		(0.039)		(1.580)	
FY 2017	−0.651	69	0.007	139	1.063	87
	(0.467)		(0.046)		(1.662)	

Notes: Robust bias-corrected RD estimates from `rdrobust`, estimated separately for each fiscal year. Triangular kernel with MSE-optimal bandwidth selected per year. Standard errors in parentheses. N is the effective number of observations within the bandwidth. ***, **, * denote significance at 1%, 5%, 10% levels.
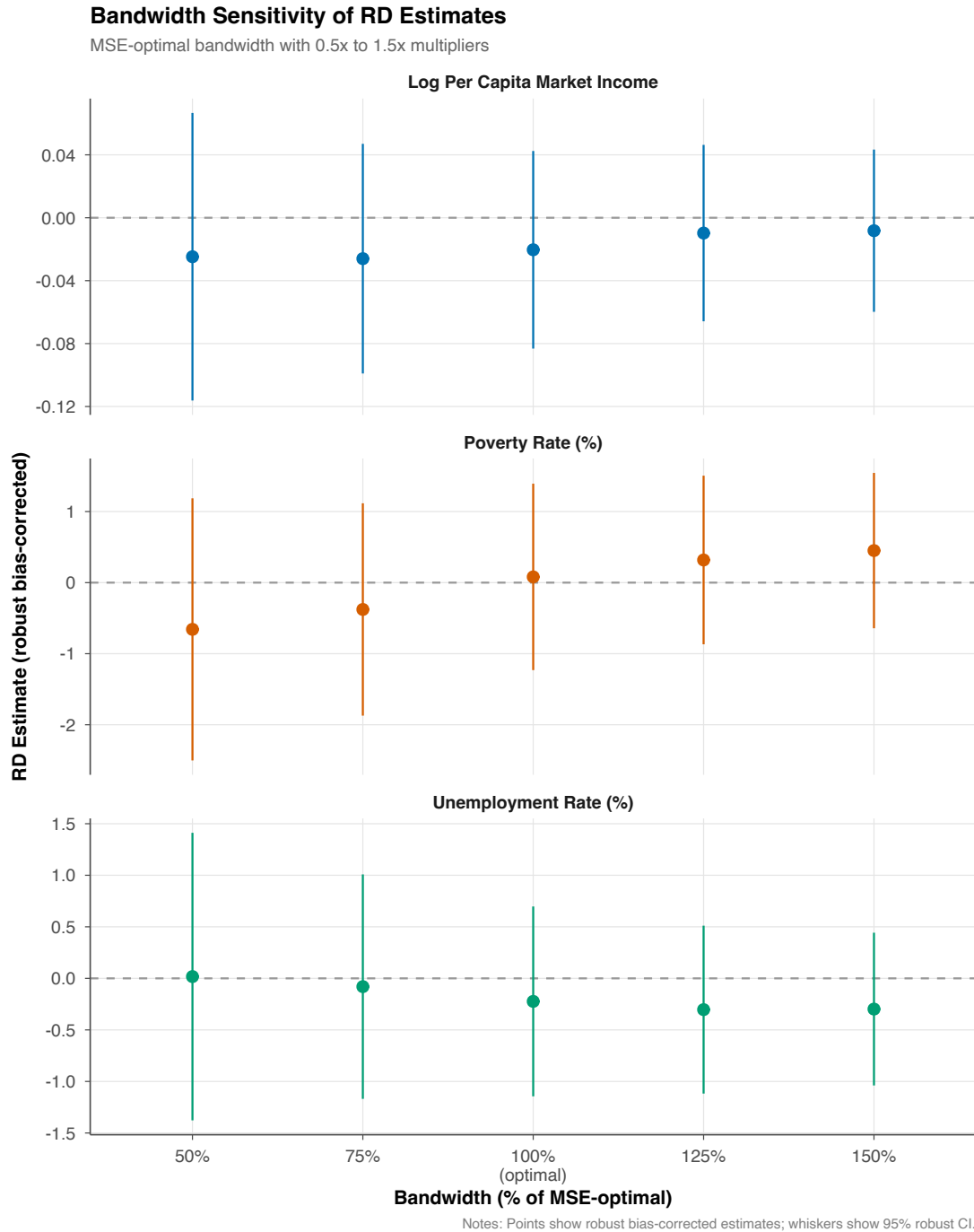
D. Additional Figures and Tables

Figure 6: Map of Appalachian Counties by Economic Status, FY2014
Notes: ARC counties shaded by economic status designation. Distressed counties (darkest) are concentrated in Central Appalachia (eastern Kentucky, West Virginia, and southwestern Virginia). Source: ARC County Economic Status data, FY2014.
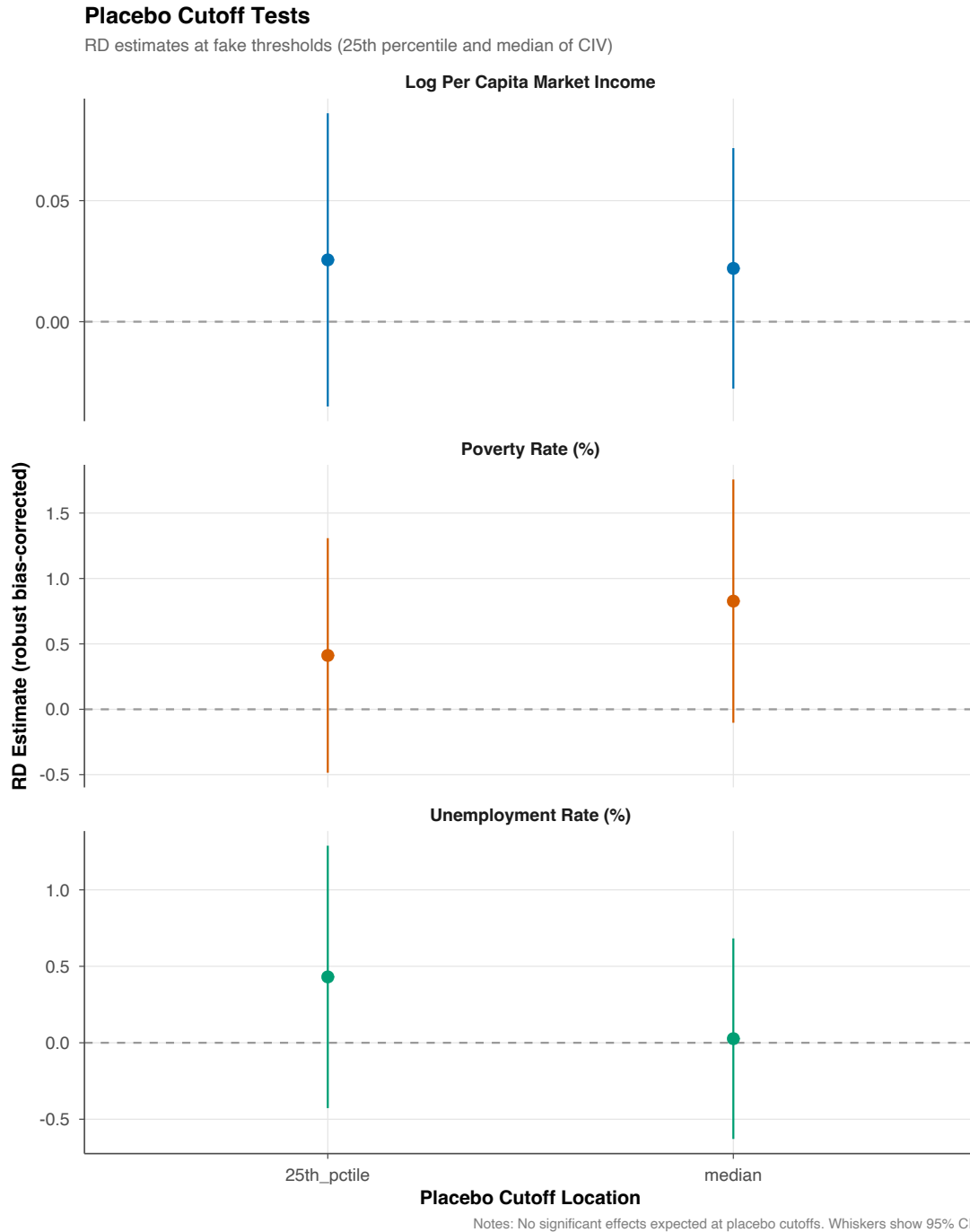
Figure 7: Year-by-Year RDD Estimates (Appendix)

Notes: Each point is a separate rdrobust estimate for a single fiscal year. Error bars show 95% confidence intervals (robust bias-corrected). Dashed line at zero. The point estimates show no systematic pattern over time.

Figure 8: Bandwidth Sensitivity Analysis

Notes: Point estimates and 95% confidence intervals for the Distressed designation effect at different bandwidth multiples of the MSE-optimal bandwidth. All three outcomes remain statistically insignificant across bandwidths.

Figure 9: Placebo Tests at Non-Treatment Thresholds
Notes: RDD plots at the 25th and 50th national percentiles of the CIV, where no change in ARC treatment occurs. The absence of discontinuities confirms that the null result at the Distressed threshold is not an artifact of the method.

Table 7: Year-by-Year McCrary Density Tests

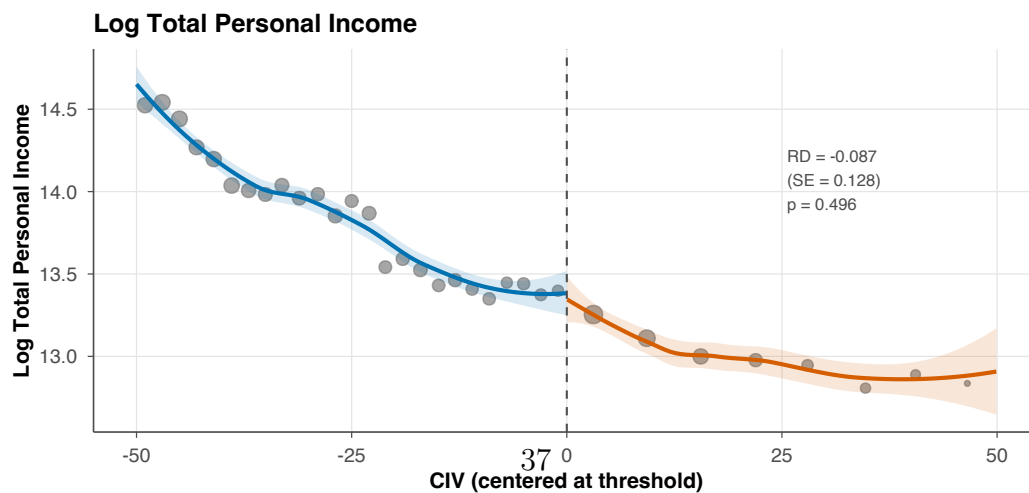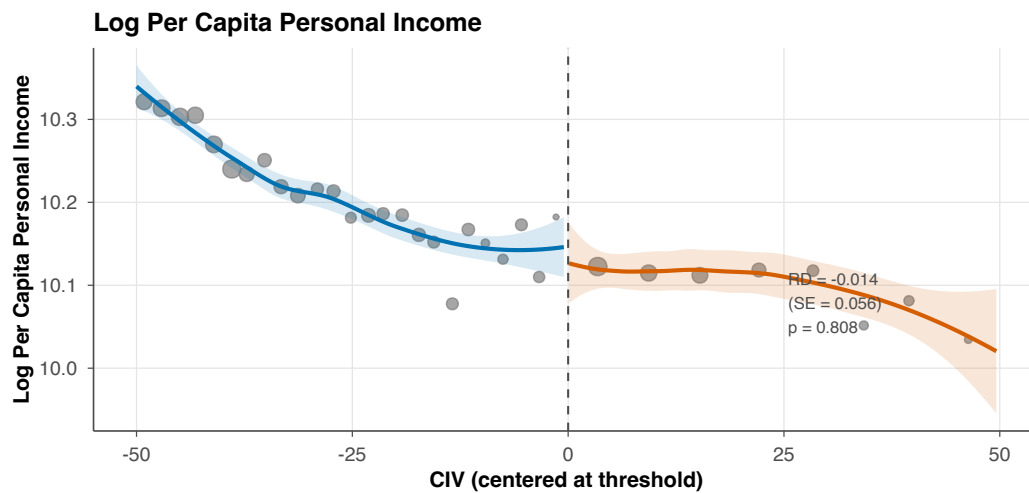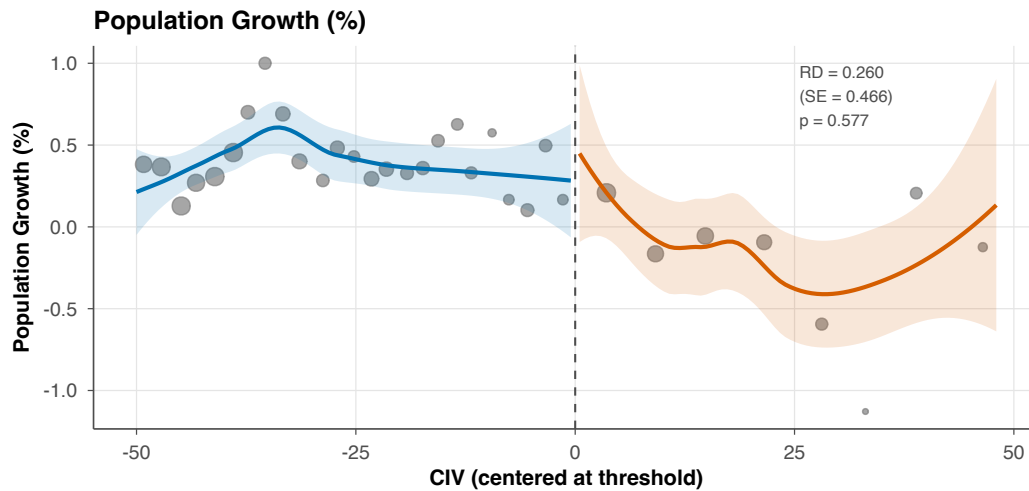| Fiscal Year	T-statistic	p-value	N
FY 2007	1.530	0.126	286
FY 2008	−0.058	0.954	255
FY 2009	0.426	0.670	258
FY 2010	0.960	0.337	247
FY 2011	0.737	0.461	261
FY 2012	0.633	0.527	320
FY 2013	−1.790	0.074	340
FY 2014	0.988	0.323	336
FY 2015	0.040	0.968	338
FY 2016	0.233	0.816	341
FY 2017	2.171	0.030	335
Pooled	0.976	0.329	3,317

Notes: McCrary density tests using `rddensity` (**?**), estimated separately for each fiscal year and pooled. The null hypothesis is continuity of the CIV density at the Distressed threshold. Of 11 year-specific tests, 10 fail to reject at the 5% level. The single rejection (FY2017, $p = 0.030$) is consistent with the expected false positive rate.

E. Year-by-Year Density Tests

F. Alternative Outcomes: RDD Plots

Alternative Outcomes: RDD at the ARC Distressed Threshold

Outcomes not mechanically linked to the Composite Index Value

Population Growth (%)

RD = 0.260
(SE = 0.466)
p = 0.577

Log Per Capita Personal Income

RD = -0.014
(SE = 0.056)
p = 0.808

Log Total Personal Income

RD = -0.087
(SE = 0.128)
p = 0.496

Notes: Binned scatter with local polynomial fits. These outcomes are independent of the CIV used for designation assignment.