Economic Distress and Electoral Consequences: Evidence from Appalachia

Daniel Firoozi*

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Abstract

Information about inequality can change political attitudes in lab and survey experiments. I use data from the Appalachian Regional Commission and a regression discontinuity design to test whether salient information about local poverty can impact voter behavior in a field setting. I find that when the poorest decile of counties is labeled "economically distressed," the Democratic share of the Presidential and House popular vote rises in subsequent elections. I present suggestive evidence linking this result to local news coverage, rather than spending or other outcomes.

Keywords: Inequity, Elections, Information

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*Daniel Firoozi: University of California, Irvine, 3151 Social Science Plaza Building A, Irvine CA 92697-5100, dfiroozi@uci.edu. I thank Damon Clark, David Neumark, Yingying Dong, Matthew Freedman, Daniel Ladd, and Forouzandeh Firoozi, as well as seminar participants at UC Irvine, UC Santa Barbara, and ITAM for helpful advice, support, and feedback on this paper. Special thanks to Kye Lippold for providing data on SNAP work requirement waivers. No financial support was provided for this project. IRB approval was not required for this paper.

1 Introduction

Beliefs about inequality influence attitudes toward redistribution, but there is little evidence on how redistributive policies shape voter perceptions of inequality (Alesina et al., 2004; Alesina and Angeletos, 2005; Benabou and Tirole, 2006; Alesina et al., 2018). Imperfect information can disrupt the theoretical prediction that poorer voters will be more supportive of causes and parties associated with redistribution (Romer, 1975; Meltzer and Richard, 1981; Bublitz, 2016; Gimpelson and Treisman, 2018). If voters care about relative economic deprivation, then correcting misperceptions about inequality could change their political views (Black, 1948; Piketty, 1995; Benabou and Ok, 2001; Alesina et al., 2018). This has important ramifications for policy interventions that label people or places as poor relative to others.

Lab and survey experiments show that updating voter information about inequality can impact support for redistribution and political party preferences. In Argentina and Spain, notifying poor people about their position relative to others in the income distribution increased support for redistribution (Cruces et al., 2013; Fernandez-Albertos and Kuo, 2018). Similarly, a Swedish experiment correcting high income voters who believed themselves to be middle-income increased hostility toward redistribution and support for the center-right (Karadja et al., 2016). Americans' beliefs about inequality also change in response to new information, but their policy preferences are more stable (Kuziemko et al., 2015; Bublitz, 2016; Hoy and Mager, 2021). While these experiments have high internal validity, it is not clear whether they generalize to political outcomes in field settings[1].

To test whether updated information about poverty and relative deprivation can change votes, I use data from America's premier regional development program, the Appalachian Regional Commission (ARC). The ARC is a compelling test case because of a rigorous identification strategy and the generalizability of this field setting's results to other redistributive

[1] A related natural experiment in Norway found that publicizing information about citizens' income exacerbated the socioeconomic status gradient in happiness and life satisfaction across the income distribution (Perez-Truglia, 2020).

programs[2]. An administrative reform in the late 2000s led the ARC to issue county economic status labels according to a strict formula that designated the decile of counties with the poorest residents as "economically distressed". Local news coverage focuses attention on said counties and disseminates information about poverty rates, income, and unemployment to the broader public.

Using a regression discontinuity design, I find that classifying a marginal county as economically "distressed" instead of "at-risk" causes significant ($p < 0.01$) increases in support for Democratic Party candidates. My preferred estimates suggest that the distress label leads to an average 2 to 6 percentage point increase in the Democratic candidates' two-party vote share in Presidential and House of Representative elections. The evidence suggests that effects accrue to noncompetitive states with little campaign activity. My results are robust to covariates, alternative bandwidth selection, and a higher order polynomial control for the running variable.

I investigate several plausible mechanisms and present suggestive evidence consistent with a media coverage mechanism, rather than government spending or other outcomes. While they don't lead to discontinuities in economic conditions, the labels generate media coverage of "economically distressed" counties from the poorest decile of the distribution. Stories about the ARC regularly discuss county labels, emphasize the economic distress designation, and publish county status maps. Using supply side constraints on access to media coverage, I find that the political effects of the distress label are larger in counties with greater media penetration.

My findings contribute two central points to the political economy literature. First, my results are consistent with the idea that updating voter information about inequality matters in a field setting. The impacts show some degree of persistence and assist multiple left-of-center candidates. Second, my findings suggest that singling out poor geographic regions

[2]The ARC model of federal-state partnership was replicated by members of congress during the creation and proposal of the Delta Regional Authority, the Northern Border Regional Commission, the Southwest Regional Border Commission, the Southeast Crescent Regional Commission, the Northern Great Plains Regional Authority, and the Denali Commission (Wood, 2005; Cecire, 2019).

has the potential to change votes, which could have important ramifications for policies that tag people or places as poor relative to others.

2 The ARC and County Classification System

The Appalachian Regional Commission is a federal-state partnership established by the Appalachian Regional Development Act of 1965 during the "War on Poverty" to bring the region into economic parity with the rest of the United States (Bollinger et al., 2011). Like many redistributive programs championed by President Lyndon Johnson, support became polarized between Democrats who favored its expansion and Republicans who sought its elimination (Isserman and Rephann, 1995; Wood, 2005; Burns, 2017). As Appalachia transitioned into a Republican bastion (See Appendix A Figure 6), the ARC retained bipartisan support among local elected officials but continued to draw the ire of fiscal conservative groups that viewed it as wasteful redistributive spending (Bandow, 2000; Ditch, 2019).

The ARC's economic footprint evolved alongside the region's politics, expanding to cover 22 million people in 428 counties and 13 states. Congress appropriated 34 billion dollars to the ARC since its creation, raising per capita income, earnings, and population growth (Isserman and Rephann, 1995; Jaworski and Kitchens, 2018). Despite the convergence, ARC counties still rank among the highest on poverty and lowest on median income and educational attainment (Bollinger et al., 2011). With most of the Apalachian Development Highway System now complete, a growing share of the budget is allocated to Area Development Program grants that match non-profit, state, and local spending, often in construction and infrastructure[3] (Jaworski and Kitchens, 2018). Grant proposals by these entities that meet the ARC's eligibility criteria are sent to state governments, who in turn submit a slate of prioritized proposals to the federal commission for approval.

[3]The overwhelming share of spending is allocated toward the Appalachian Development Highway System (ADHS) and federal grant funding in the five categories of "creating economic opportunities, developing a ready workforce, investing in critical infrastructure, leveraging natural and cultural assets, and bolstering leadership and community capacity".

To improve equity and raise awareness of local poverty, the ARC releases "county economic status labels" on an annual basis. Prior to fiscal year 2006, all counties were assigned to one of four labels using a complicated combination of criteria and data from the 2000 census. Each category afforded a different maximum level of grant generosity. Poorer county categories could request relatively higher dollar to dollar federal match rates on approved projects. Beginning in fiscal year 2007, the availability of new county-level data from the ACS led to an overhaul of the ARC's system. By calendar year 2008, a fifth category was introduced and the myriad of assignment criteria were simplified into a single composite index with different cutoffs for county economic status (Cecire, 2019).

The new system uses a three step procedure to assign economic status labels to ARC counties each June for the upcoming fiscal year. First, all counties in the US are given a raw disadvantage score which is an average of its rescaled unemployment rate, per capita market income, and poverty rate[4]. Second, all counties are ranked by the raw score from least to most disadvantaged. Finally, ARC counties are categorized as "Attainment" if they fall below the 10th percentile of disadvantage among all US counties, "Competitive" for the 10th to 25th, "Transitional" for the 25th to 75th, "At-Risk" for the 75th to 90th, and "Distressed" if they are above the 90th percentile [5]. I note that all observed county economic status labels comply with the assignment process and 93 percent of county observations are rated as "transitional" or worse.

This reform to the county classification system provides a unique setting to study the effects of economic status labels, because the labels do not generate attendant discontinuities in federal or state policy. By law, the requested federal "matching rate" on ARC projects can rise at the threshold for each label, but in practice it does not[6]. A mix of budget

[4]Unemployment and poverty rates are divided by the national average. Per capita market income is divided by the national average and then inverted. All three values are then multiplied by 100 and averaged together into the raw disadvantage score.

[5]In ranking counties ordinally by economic distress, the ARC transforms the raw disadvantage scores into percentile rank index values that are set relative to the total distribution of American counties. I offer a detailed accounting of the differences between the raw disadvantage score and percentile rank index in Appendix A.1.

[6]The matching rate refers to the maximum percentage of total spending on an ARC-approved project that

pressure and exceptions to ARC rules[7] render the maximum matching rates effectively non-binding (See Appendix A Figure 7). As I explore further in Section 6, the absence of discontinuities in matching rates, per capita project spending, and county employment rates allows identification of the economic status labels' political impacts.

3 Data

Most of the administrative records used in this paper are from arc.gov, the ARC's public website (Appalachian Regional Commission, 2020). County-level demographic, political, and economic variables are combined with novel datasets I construct using ARC media coverage and public documents on ARC grant spending. The data are matched on FIPS code to generate a county-level panel with one observation in each election cycle following the administrative reform to county classification[8]. Consistent with the retrospective voting literature, I use the county labels assigned in the first post-reform election cycle to test for impacts on partisan vote share in subsequent Presidential and House of Representative races (Healy and Lenz, 2014). More recent labels are included alongside initial classifications to disentangle whether effects in later years are driven by persistence or serial correlation in the post-reform classification system.

County economic status data come directly from the ARC's public files and I replicate the commission's procedure for generating economic status labels. The ARC includes information from the 2000 Census for county-level poverty rates and educational attainment,

federal grants may cover. The matching rate also varies across the category of grant, with local development districts (LDD) and rural access roads differing from the general expenditures that compose most of the grant budget. Appalachian Development Highway System projects always qualify for an 80 percent matching rate, irrespective of county economic status.

[7]Any project in a high poverty census-tract qualifies for the same matching rate as a distressed county, regardless of the classification of the county in which the census-tract is located. In practice, this means that most of the projects in marginally at-risk counties are already entitled to the same maximum level of generosity as projects in distressed counties. Local development districts and multi-county projects have a different set of rules for matching rates and distribute money to member counties at rates orthogonal to their classification.

[8]This represents a total of 1,284 observations over three Presidential races and 2,568 over six House of Representative cycles.

from the Bureau of Labor Statistics' Local Area Unemployment Statistics for lagged county unemployment rates, and from the Bureau of Economic Analysis' Regional Economic Information System on county-level per capita market income[9]. I merge these records with US presidential election results from the MIT Election Lab (MIT Election Data and Science Lab, 2018), House of Representatives and supplemental presidential election data from Dave Leip's Election Atlas (Leip, 2020), data from Nate Silver's 538 election forecast model, population totals from the US Census Bureau (US Census Bureau, 2020), county-level net migration data from the US Census Bureau's American Community Survey (US Census Bureau, 2021), employment data from the BLS Quarterly Census on Employment and Wages (Bureau of Labor Statistics, 2020), county-level federal grant spending data from the US Department of Agriculture's Economic Research Service (USDA Economic Research Service, 2021), and data on county exemptions from SNAP work requirements courtesy of Lippold and Levin (2021). To calculate an employment to adult population ratio, I divide the number of persons employed in a given county by the total population of residents aged 20 to 65. There are no missing or incomplete observations for any of the aforementioned variables.

To illustrate the gaps between Appalachia and the rest of the United States, I provide descriptive statistics in Table 1. Consistent with the existing literature, panel A demonstrates that Appalachia and its most distressed counties remain less educated than the country as a whole. This pattern is reflected in the region's economic and political outcomes. Panel B shows that ARC counties have higher unemployment rates and poverty rates, as well as lower levels of per capita market income. Panel C makes evident the political realignment of Appalachia's rural and non-college educated electorate toward the Republican Party. In every election since 2000, the region has become less Democratic relative to the country as a whole, trending from 6.4 to 17.0 percentage points less Democratic by 2016. In all cases, the differences between ARC counties and the United States are magnified in counties labeled

[9]Lagged unemployment rates refer to a three year average rate ending in the second most recent calendar year. For example, the 2004-2006 average unemployment rate was used for the FY2009 ARC calculations. Lagged per capita market income is from three years prior to the ARC county designation. For example, the 2005 per capita market income level was used for the FY2009 ARC calculations.

"economically distressed".

Demographic, political, and economic variables are combined with data I scrape from the ARC's list of approved projects. The project files include the title of the program, the spending category, the federal matching rate, and the total project expenditure. The records capture the full universe of ARC grant spending, which I then manually match to specific counties. I identify more than 764 projects which sum to roughly 213 million dollars in total spending leading up to the 2008 General Election. This includes all non-highway projects that received ARC funding between October 2007 and October 2008. Roughly 87 percent of project spending could be linked to a specific county and 98 percent of ARC counties received funding for a project[10]. Of the 186 million dollars that could be traced to a specific county approximately 57 percent was for water and sewer infrastructure, 11 percent for education and workforce development, 9 percent for tourism, housing, or community facilities, 8 percent for administrative expenses, 8 percent for business or industrial sites, and the remaining 7 percent for other categories.

To assess public awareness and the characteristics of ARC media coverage, I rely on information from Google and NewsBank Inc.. I pull relative search interest rates for the phrase "Appalachian Regional Commission" at the Nielsen Media Market-level from GoogleTrends to proxy for public awareness. ARC counties are then matched to the 43 media markets in which they are nested. Likewise, I quantitatively characterize ARC media coverage by assembling a dataset of articles from NewsBank Inc.'s "Access World News Research Collection: 2021" (NewsBank Inc., 2021). I begin with a sample of roughly 20,000 newspaper articles that include the precise phrase "Appalachian Regional Commission" between the years 1995 and 2020 to characterize the quantity and locations of news media sources that focus on the topic. I then draw on the top 300 Google News stories with the precise phrase "Appalachian Regional Commission" to identify characteristics of news articles with the greatest reader-

[10]The 13 percent of project spending that was not traced to specific counties mostly took the form of intergovernmental grants that were statewide, or ARC-wide. Multi-county project budgets were divided equally per capita among member counties. ARC spending per capita is 9 dollars on average.

ship and public exposure. I manually tabulate the location of the media service, the number of times each economic status label is used, and binary indicators for mentioning politicians by name, discussing grant funding, and including county economic status maps. For articles that mention county classification labels, I track the number of times a county was singled out by name or highlighted on a map. Because media coverage invokes the distress label more than all other classifications combined, I test for discontinuities at the corresponding 90th percentile threshold.

Finally, to quantify the impact of media coverage as a mechanism for the economic distress label, I draw on data from the Federal Communications Commission (FCC) and peakbagger.com. The FCC estimates signal strength for every geographic point in the US based on their own internal model of DTV signal propagation[11] across the landscape "assum[ing] an outdoor antenna 30 feet above ground level" (Federal Communications Commission, 2021a). For each county, I record the number of DTV channels with a moderate or strong signal strength in the county seat. Similarly, I use FCC Form 477 data from 2008 on county-level internet service providers as a determinant of internet access (Federal Communications Commission, 2021b). To capture variation in radio wave propagation due to terrain ruggedness, I incorporate maximum mountain peak elevation data from peakbagger.com. Rather than use the measures separately, I use the predicted first principal component for each county across all three measures and employ it as an aggregate measure of "media penetration". The values of the component are standardized such that the mean is set to zero and a one unit change is normalized to one standard deviation. The resulting "media penetration" index is continuous, accounts for half the variation across individual measures, varies negatively with mountain peak elevation, and varies positively with county-level ISPs, DTV signal strength, and channel availability.

[11]Note that 2008 was the end of the mandated transition period from analog to digital television (DTV) under the Digital Transition and Public Safety Act of 2005.

4 Empirical Approach

Since the ARC county "economic distress" label uses a rank index and an arbitrarily selected cutoff, the program is an ideal setting for a sharp regression discontinuity design with the satisfaction of two key assumptions. First, we must assume the exclusion restriction holds. Given that the rank index formula is composed of multiple scaled variables and was determined by the ARC independently, we can expect it is unlikely that other federal or state agencies' policies are discontinuous at the threshold. I discuss this assumption at further length and test it empirically in Section 6 along with other plausible mechanisms. Second, we must assume imperfect control of the rank index around the cutoffs. The assumption is credible because counties would not be able to manipulate the rank index unless they anticipated the reform 8 years prior, knew the precise formula before it was drafted, and adversely harmed their own economies to become eligible for grant funding. The formula likewise binds the ARC to economic status labels, preventing manipulation by policymakers.

I evaluate the assumptions empirically by testing for discontinuities in predetermined covariates and the density of observed rank index values. There is no visual evidence of discontinuous jumps in the density of county observations (see Appendix B Figure 8) and I fail to reject the null hypothesis of a smooth density of observations around the threshold [12]. As I show in Appendix B Figures 9 through 11, background characteristics also trend smoothly around the cutoff. I find that for 14 covariates[13], none reject the null hypothesis of a smooth trend in density at a 90 percent confidence interval, which is consistent with non-manipulation of the rank index (see Appendix B Table 9). This result holds across alternative bandwidth choices, with the sole exception of one covariate at very large bandwidths (see Appendix B Figures 12 through 14). The results are consistent with the expectation that counties and policymakers are not capable of systematic sorting around the cutoffs for

[12] A second order polynomial does not reject the null hypothesis of a smooth density of observations using the rddensity command (McCrary, 2008; Cattaneo et al., 2018, 2019).

[13] This includes all variables tracked by the ARC to measure progress toward socioeconomic parity, the lagged versions of my electoral outcomes of interest, and lagged county employment rates.

economic status labels.

Given continuity of the conditional distribution function, the general form of the RD equation for assignment to distressed county status is:

$$VoteShare_{i,t} = \alpha + \beta Distressed_{i,2008} + f(R_{i,2008}) + \mathbf{X}'_{i,t}\Omega + \varepsilon_{i,t}, \tag{1}$$

where $VoteShare_{i,t}$ is the Democratic share of the two party vote in county i during election year t, $Distressed_{i,2008} = I[R_{i,2008} \geq 0]$ is an indicator for a county being assigned to distressed status in June of 2008, $R_{i,2008}$ is the raw composite rank index with the 90th percentile cutoff normalized to zero, $f(\cdot)$ is a continuous function, $\mathbf{X}_{i,t}$ is a vector of covariates which always includes lagged vote shares, and $\varepsilon_{i,t}$ is an idiosyncratic error term with standard errors clustered on county[14]. Given the RD assumptions, the parameter β identifies an average effect of the "economic distress" label among counties local to the threshold. I vary the order of a polynomial control for the rank index, add a full set of covariates, and check my estimates across a range of bandwidths to demonstrate robustness[15].

5 Results

5.1 Presidential Elections

In Figure 1, I plot the normalized rank index against the Democratic share of a county's two-party presidential vote and the 2004 to 2008 swing. The left-hand panels display the two pre-treatment election cycles, the upper right panel displays the first post-treatment election cycle, and the bottom right panel shows the vote swing between 2004 and 2008. Consistent

[14]To distinguish between persistent effects and serial correlation in post-reform rank index values, I include the terms $Distressed_{i,Recent}$ and $R_{i,Recent}$ in later specifications.

[15]The full set of covariates includes 2004 two-party US Presidential vote share, 2004 two-party House of Representatives vote share, lagged county-level poverty rates, per capita market income, unemployment rates, high school completion rates, bachelor's degree attainment rates, and fixed effects for Nielsen media markets, states, and election years. This includes the exact set of continuous covariates recorded by the ARC to track progress toward socioeconomic parity.

with the balance checks presented in Appendix B, the economic distress labels that resulted from the reform to county classification are not related to election results in either 2000 or 2004. By contrast the right-hand panels suggest that labeling a county as economically distressed results in a roughly four percentage point increase in Democratic vote share. This visual evidence is robust when using a pooled specification with all post-treatment election cycles (See Appendix C Figure 15). In Table 2, I test these outcomes formally, varying bandwidth selection, the order of a polynomial control for the rank index, and the inclusion of demographic covariates and fixed effects for state, media market, and election year.

Each column of the table represents a different specification of the RD model and each panel reflects a different way to divide the sample. Panel A begins with the full sample of ARC counties across every election year. Panel B splits the sample by presidential election cycle. Panels C divides the counties by their presence in a contested presidential swing state[16]. These successive cuts allow me to track the persistence of effects over time and test for plausible heterogeneous treatment effects which could be relevant to identifying causal mechanisms.

Beginning with the pooled data in Panel A, I find consistent evidence that labeling a county as "economically distressed" during the 2008 election cycle caused a roughly 3 to 6 percentage point increase in Democratic vote share in subsequent US presidential elections. My preferred specifications are the local linear models with demographic controls and fixed effects in Columns 2 and 6, because they are the best fit to the raw data shown in Appendix C Figure 15 and substantially reduce residual variance. My estimates strongly suggest ($p < 0.01$) that the distressed county label induces around four percentage points of the Appalachian electorate in marginal counties to vote for Barack Obama and Hillary Clinton. Raising the order of the polynomial rank index control, shortening the preferred bandwidth, and varying the inclusion of covariates do not substantially change my results. Moreover, running the same specifications on placebo data from pre-treatment election cycles yields no

[16] I define Ohio, Pennsylvania, North Carolina, Virginia, and Georgia as swing states, because they voted for candidates of both parties since 2004.

significant impacts on electoral outcomes (See Appendix C Table 10).

In Panel B, I split the sample by election cycle to identify whether this increase in Democratic presidential vote share lasts beyond a single year. I find significant results in later election cycles with less precision, which is suggestive of some degree of persistence. However, I caution against over-interpreting this result for two key reasons. First, I cannot reject the null hypothesis that the impact is constant or smaller in later years. Second, there is serial correlation in the post-reform county classification, meaning that post-2008 results may capture multiple years of assignment to the distress label. That possibility is examined in further depth in Section 5.3.

While it is plausible that the distress label could encourage presidential campaigns to target resources to distressed counties in swing states, I present evidence contradicting this hypothesis in Panel C. I find that the overall results are driven by significant effects in uncontested "safe states", rather than swing states. The estimates are robust to an alternative definition of swing states from Nate Silver's 538 forecast (See Appendix C Table 11).

Although I use bandwidths at roughly half and one quarter of the full range observed in the data, results are robust to alternative bandwidth selection. Figure 2 shows the coefficient on the economic distress label and confidence intervals across a range of potential bandwidths [17]. At nearly every bandwidth and for every election year we would reject the null hypothesis that the coefficient was zero. Likewise, the point estimates are fairly consistent irrespective of bandwidth choice. I demonstrate that the specifications from Table 2 are quite robust to alternative bandwidth selection and show null effects on the 2000 and 2004 Democratic vote shares across a set of bandwidths and specifications (See Appendix C Figures 16 through 21).

To ensure that the results I observe at the true cutoff are not due to chance, I perform a falsification test by evaluating my preferred specification with a synthetic "cutoff" at every raw rank index score for which I have a sufficient number of observations. The results

[17] This includes the optimal values which span from 20 to 40 under different polynomials, kernels, and methods in the first post-treatment election (Calonico et al., 2020).

are displayed in Figure 3. For the pooled dataset and for every election year, the only significant t-statistics appear right around the threshold for the "economically distressed" county label, which I take as evidence that the estimates reflect a genuine causal effect, rather than one of many discrete jumps in vote share. I repeat this procedure using the narrower bandwidth in Appendix C Figures 22 and 23, finding that the t-statistic at the true threshold exceeds the 95th percentile of all synthetic t-statistics for each election year[18]. Performing the same falsification tests with the 2000 Al Gore and 2004 John Kerry vote shares in Appendix C Figures 24 and 25 shows no comparable discontinuity at the threshold across either bandwidth or year.

5.2 House of Representative Elections

If updating information about poverty buoys support for the Democratic Party as a whole, then designating a county as economically distressed could boost Democratic candidates in congressional races. Hence, I supplement my findings on US presidential elections with data from campaigns for the US House of Representatives. From a theoretic standpoint, down-ballot impacts should be weaker, because support for redistributive programs like the ARC is much less polarized among Appalachian legislators than between presidential candidates (Burns, 2017). From a methodological standpoint, down-ballot impacts should be less precisely identified because of uncontested races and the large number of heterogeneous candidates sorted into districts that are rarely co-terminus with counties.

In Appendix C Figure 26, I plot the normalized rank index against the Democratic share of a county's two-party House of Representatives vote, excluding counties with no contested seat and pooling across the six election cycles between 2008 and 2018. Consistent with expectations and my findings from presidential election data, the graph suggests that the economic distress label induces a small segment of the electorate to vote for Democratic

[18] A Bonferroni correction for multiple hypothesis testing renders the occasional positive placebo results null. The estimated coefficients at the true threshold for the economic distress label remains significant at a 95 percent confidence interval for each election year.

congressional candidates. In Table 3, I replicate my specifications from the preceding subsection, dividing the sample this time by ancestral Democratic support in the 2000 election because of Appalachia's high rates of split-ticket voting.

Starting with the pooled sample in Panel A, I once again find evidence that the economic distress label improves Democratic vote share. Under each of the specifications, I estimate that an additional two to five percentage points of the electorate vote for Democratic candidates in the House of Representatives because of the distress label. This finding is notable for three key reasons. First, the estimate is fairly robust to raising the order of a polynomial control for the running variable, shortening the bandwidth, and varying the inclusion of demographic covariates and fixed effects. Second, the estimated impact on Democratic vote share in the House of Representatives is similar, if slightly weaker, to the point estimate in presidential races. Third, evidence on heterogeneous treatment effects identifies one group of voters who may be responsive to the county labels.

Panel B divides counties at the median by the fraction of "ancestral Democrats", defined as the share of the two-party vote that went to Al Gore in the 2000 presidential election[19]. Most of the effect of the economic distress label in the House of Representatives appears to accrue to counties with relatively more ancestral Democratic voters. However, this heterogeneity does not hold under some alternative specifications and does not fully translate to presidential outcomes (See Appendix C Tables 12 and 13). To the extent that it may exist, such a pattern is consistent with experimental evidence that suggests information about inequality has heterogeneous effects depending on ideology (Karadja et al., 2016; Alesina et al., 2018).

Parallel to the presidential election data, I test the estimated effect in House of Representative races for robustness to alternative bandwidth selection. My preferred specification in Column 2 of Panel A uses a local linear estimate with covariates at a 50 point composite

[19] As a former Senator from Tennessee, Al Gore was the last Democratic nominee from an Appalachian state. He won the highest vote share for a Democrat in the 21st Century both region-wide and in 62 percent of ARC counties, making his performance a maximum benchmark for subsequent Democratic Party candidates.

index bandwidth, but narrower and longer bandwidths like those shown in Figure 4 yield similar point estimates at the same confidence interval. In Appendix C Figure 27 I show the corresponding estimates across bandwidth for all four specifications that were tested in Table 3. Point estimates are similar across specification and bandwidth, but due to lower precision when estimating House of Representative treatment effects, there are a range of bandwidths at which I fail to reject the null hypothesis of no effect for specifications that exclude demographic covariates and lack state, year, and media market fixed effects. I also replicate my falsification test from the preceding subsection, evaluating results with a synthetic "cutoff" across the composite score rank index. As Figure 5 illustrates, the t-statistic I estimate at the actual cutoff for the economic distress label is the largest among every potential cutoff. Likewise with the previous iteration of this falsification test, the only significant values are centered around the true cutoff value.

5.3 Persistence and Time-Variation in Labels

I run an alternative specification for races after the 2008 election cycle to disentangle whether the estimated impacts in later years reflect persistence or serial correlation in the post-reform county classification system (See Appendix C Figure 28). The specification includes a local linear RD for both a county's most recent label and its 2008 cycle label. Bandwidths are set to include any county observation for which either label was within 50 rank index composite points of either threshold. Results are shown in Table 4.

Across the six columns, I test for effects in the 2012 presidential election, the 2016 presidential election, and the 2010 through 2018 House of Representative elections, varying the inclusion of fixed effects and covariates. The first row of estimates reflect the coefficient on crossing the threshold for the 2008 distress index and the second row of estimates reflect the coefficient on crossing the threshold for the most recent distress index to the observed election. In general, I find that the 2008 distress label has smaller point estimates than the corresponding values in Tables 2 and 3, but I can still reject the null hypothesis that the

estimated effects of the 2008 labels in later years are zero under most specifications. This suggests that there is some degree of persistence to the impact of 2008 county classifications, but serial correlation may inflate estimated effects in later years.

I offer some important notes of caution against over-interpretation of Table 4 because field settings are limited in their ability to directly examine voter decision-making. First, it is important to note that persistence of the economic distress labels' political impacts into later years does not necessarily imply that voters remember the intervention. Voters need not recall why they did or did not cast a ballot for habit formation or social spillovers to influence their future voting patterns (Fujiwara et al., 2016). Likewise, the fact that the coefficient on the 2008 labels are larger does not imply that more recent labels are inconsequential. Under most specifications I cannot reject the null hypothesis that the estimated effects of the 2008 and most recent label are the same. To the extent that a simple comparison of point estimates can be taken at face value, then there may be diminishing returns to repeatedly applying the same information intervention to marginal counties that shift back and forth across the threshold. Moreover, 2008 classifications may also have been particularly potent (See Section 6.2) because they were issued under a new system at the onset of the Great Recession.

6 Discussion

Existing research suggests three plausible channels through which ARC county economic status labels may influence the electorate: political multiplier effects, signage effects, and media coverage. Under the first, voters observe and react to higher per capita spending, more generous federal policies, or better employment rates in distressed counties (Bagues and Esteve-Volart, 2016; Huet-Vaughn, 2019). Under the second, projects may amplify public awareness of the ARC in distressed counties and sway the electorate (Voigtlaender and Voth, 2014; Huet-Vaughn, 2019). Under the third, news coverage of the "economic

distress" label may influence voters by acting as salient information about poverty and relative deprivation (Cruces et al., 2013; Kuziemko et al., 2015; Karadja et al., 2016; Alesina et al., 2018; Fernandez-Albertos and Kuo, 2018). I evaluate each pathway, finding suggestive evidence in favor of a media coverage mechanism.

6.1 Political Multipliers and Signage Mechanisms

For either the political multiplier or signage mechanisms, the economic distress label applied by the ARC must generate meaningful project spending differences between counties. The former hypothesis would suggest that increased project spending in distressed counties leads to an improved local economy. The latter implies that the dedication of additional funding for projects is noticed by the electorate directly, raising public awareness. Either of these channels could be complicated further if the distress label induces more generous federal grant spending overall or larger transfer payments to county residents from redistributive programs.

Both channels would raise incumbent party vote share at the threshold, which is contradicted by the main findings in Section 5. The Democratic Party benefits from the distress label despite being the opposition party in 2008, suggesting political multipliers and signage are not at play. Moreover, the label does not generate attendant discontinuities in the employment rate preceding each election, ARC project spending per capita, ARC grant matching rates, an ARC public awareness index, total federal grant spending per capita, and a proxy of county-level generosity of SNAP[20].

In Table 5, I display the results for discontinuities in these intermediate outcomes formally, using a local linear specification and starting with a 50 point composite index bandwidth. For each variable, the estimated impacts of distressed county status are small in magnitude and fail to reject the null hypothesis of no effect. In the first column, I demonstrate that the election year employment rates of counties do not differ substantially across

[20]Counties may be exempted from work requirements for able-bodied adults without dependents to be eligible for SNAP. These exemptions are tied to county unemployment rates (Lippold and Levin, 2021).

the threshold. Consistent with this finding, the measures of ARC project generosity in Columns 2 and 3 do not meaningfully change. Undercutting the signage hypothesis, Column 4 illustrates that marginally distressed counties are not more likely to be nested in media markets with higher ARC awareness and search interest. Columns 5 and 6 show that the ARC distress classification does not impact total federal grant receipts or the generosity of redistributive transfer payments like SNAP.

In Appendix D Figure 29, I present a bandwidth selection graph for each of the aforementioned intermediate outcomes. Across all bandwidths and variables, the estimated impacts of distressed county status are small and I cannot reject the null hypothesis that there is no effect[21]. In each case, point estimates fluctuate around or near zero and do not differ substantially from the initial bandwidth used in Table 5. Although it cannot have an impact in the 3 month window between label assignment and the 2008 election, I also test long-run county net migration in Appendix D Figure 31 and again find no effect across bandwidths. Null effects from each of these plausible mechanisms are robust across the specifications I use in Table 2 (See Appendix D Table 14). On balance, the evidence suggest political multipliers, signage effects, and incidental impacts from other federal grants and redistributive programs do not drive the estimates found in Section 5.

6.2 Media Coverage and Information Mechanisms

Given that the distress label does not induce differences in spending or economic outcomes, it is worth examining whether it had a direct impact on voters through media coverage. Previous lab and survey experiments demonstrate that updating voters' information about relative deprivation impacted support for political parties and redistributive policies (Cruces et al., 2013; Kuziemko et al., 2015; Karadja et al., 2016; Alesina et al., 2018; Fernandez-Albertos and Kuo, 2018). If ARC county status labels have such an effect, then there should be two clear patterns in media coverage. First, news stories should use ARC

[21] I show RD graphs at the full bandwidth for each intermediate outcome in Appendix D Figure 30.

county classifications to place an emphasis on distressed counties. Second, counties with greater media penetration should have stronger reactions to the distress label than their more insular counterparts. I test these questions empirically, using a sample of approximately 20,000 articles about the Appalachian Regional Commission from NewsBank Inc., a narrower sample of 300 articles with greater exposure from Google News, and an index of media penetration derived from FCC data and terrain ruggedness.

6.2.1 A Quantitative Characterization of ARC News Coverage

I use data from NewsBank Inc. to tabulate descriptive statistics on roughly 20,000 American newspaper articles that include the phrase "Appalachian Regional Commission" between the years 1995 and 2020. Appendix D Figure 32 demonstrates that there was a sudden and sustained increase in the volume of coverage of the ARC around the time period of the reform to county classification. There was no comparable increase in news coverage of the second largest regional commission, the Delta Regional Authority, which did not change its approach to distressed county labels.

The NewsBank Inc. data provide clear evidence that local media are the primary means through which news about the ARC is disseminated. Roughly 93 percent of domestic newspaper articles about the commission come from publishers headquartered in one of the region's 13 states. The articles come from a diverse array of cities, with the top 10 towns comprising only 22.6 percent of the total and no other location accounting for more than 1.1 percent of articles. The leading papers that focus on the ARC are The Lexington Herald-Leader, The Herald-Dispatch, and The Cumberland Times-News. This strong response from local media is consistent with a direct information mechanism and enables the use of media penetration as a tool for identifying the role of information (DellaVigna and Kaplan, 2007; Snyder and Stromberg, 2010; Enikolopov et al., 2011; Durante et al., 2019).

To examine stories in greater depth, I leverage Google News' relevance ranking to identify 300 high impact articles. Beginning with a basic characterization of media coverage, there

are 186 uniquely identified news organizations in the high visibility Google News sample. 83 percent of the coverage is published by local media, consistent with the NewsBank Inc. data. A total of 66 percent of stories explicitly describe programs funded by the ARC, often offering examples to explain the commission's purpose. Roughly half mention an elected official by name and politicians frequently hail new infrastructure or tout their role in securing funding.

Discussions of the economic distress label are common and far outpace every other label. County economic status is discussed explicitly in 36 percent of stories and 99 percent of articles that invoke county classification discuss the economic distress label. Most of the stories that discuss county economic status exclusively mention distressed status, and 97 percent mention distressed status more than any other label. In total, distressed county status is mentioned on average more than once per article, which is more than all other labels combined[22].

Appendix D Figure 33 illustrates the disproportionate emphasis placed on distressed counties. Using the set of articles that discuss any county status, I manually record the number of times a county is explicitly mentioned by name or highlighted on a figure, table, or map. I include all mentions of a county except sentences in which positive references are made toward a county's economic conditions or trajectory. Mentions and highlights are tabulated at the county-announcement period level and include mentions in figures, captions, and headlines. Even for this limited sample of ARC news coverage, there is a clear discontinuity in neutral or negative attention given to economically distressed counties[23].

I note that county economic status is the primary or sole focus of many stories, which is in line with a media and information mechanism. If the ARC impacts voters by correcting, updating, or reinforcing their perceptions about the dire economic condition of their county, then some of the media coverage should directly disseminate these labels. I find that one

[22]Distressed county status was mentioned on average 1.06 times per article, including articles with zero mentions. The corresponding numbers for the at-risk, transitional, nesse, and attainment categories are 0.32, 0.21, 0.14, and 0.13. These numbers exclude uses of the words comprising the label that are not related to a county classification.

[23]Appendix D Figure 34 confirms that this pattern is robust to bandwidth selection.

in every eight ARC stories spotlights county economic status in this way. That pattern is reflected in headlines like "In New ARC Ranking, Athens County downgraded to distressed", "Rowan County named distressed for the first time in five years", and "Graham County Listed as Economically Distressed, Leaders See Opportunity". In each of the articles, there is a pointed effort to highlight how local poverty, income, and unemployment rates fare worse in a given county than the remainder of the country. Some 9 percent of news stories go further and publish some version of the ARC's county status maps, presented in Appendix D Figure 35, which highlight distressed counties in bright red to emphasize the severity of regional poverty and relative deprivation.

6.2.2 Media Penetration and the Distress Label

I test whether media penetration is related to county responses to the distress label, combining information from several sources of supply-side variation in media access in the existing literature: terrain ruggedness, internet service, signal reception, and channel availability (DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Yanagizawa-Drott, 2014; Adena et al., 2015; Amorim et al., 2018; Durante et al., 2019)[24]. Using the procedure I describe in Section 3, I take the first principal component across mountain peak elevation, internet service providers, and DTV channels with a strong or moderate signal as my aggregate measure of media penetration[25].

To help identify whether media penetration is the mechanism behind the impact of distressed county status, I include both a control for the index of media penetration and interact the index with the indicator for distressed county status. I then use my specifications from Tables 2 and 3, restrict to narrow bandwidths around the threshold and vary covariate con-

[24]Rugged terrain can impede both radio and television broadcasts. Lack of internet service can impede access to online news, social media, cable news, and digital copies of newspapers. Poor DTV signal reception and channel availability impede access to local television networks.

[25]As Appendix D Table 15 highlights, the first principal component accounts for approximately half the variation across the inputs and places roughly equal weight on each. Mirroring the pattern observed in Figure 32, this index grows more strongly related to the Democratic share of the two-party vote in the post-treatment period (See Appendix D Tables 16).

trols. A positive coefficient on the interaction term can be interpreted in two congruent ways. First, a positive coefficient implies that the distressed label has a pro-Democratic effect that scales up with greater media penetration. Second, it would suggest that media penetration has a uniquely pro-Democratic relationship in marginally distressed counties when compared to at-risk counties within a tight bandwidth of the threshold.

In Appendix D Table 17, I test the results formally in presidential races and find that the coefficient on the interaction term is positive and significant[26]. A one standard deviation increase in media penetration translates to an increase in the distress label effect size of more than half the baseline at the average media penetration level. Likewise, a one standard deviation decrease relative to the sample average wipes out most of the effect of the distress label on electoral outcomes. I test this across six specifications and find robust results, even after varying bandwidth selection and accounting for fixed effects for Nielsen DMA media markets, states, and election years, as well as a set of county-level covariate controls. I follow the same approach in Appendix D Table 19 using House of Representatives data, finding similar but weaker results in line with the prediction in Section 5.2.

Because the specifications include a control for the media penetration index and because I am interpreting the coefficient on an interaction term, it is worth evaluating whether these results are picking up another interaction. Specifically, identification is threatened if the relationship between Democratic vote share and the media penetration index is stronger for marginally distressed than marginally at-risk counties for some reason other than media penetration. In Appendix D Tables 20 and 21 I show formally that the interaction term is unrelated to counties' pre-treatment characteristics and that my findings in Appendix D Table 17 are robust to the inclusion of seven competing interaction terms across four specifications.

The data on ARC media coverage suggest an interpretation consistent with a media and information mechanism, but because the primary outcome variables come from actual

[26]This result is robust to an alternative construction of the media penetration index as the normalized sum of the three input variables (See Appendix D Table 18).

elections, my results are limited in an important way. Outside of a lab or survey, it is not possible to identify the precise thought process that leads voters in distressed counties to vote for Democratic Party candidates. To the extent that the choice is understandable, existing research points to three pathways. First, residents exposed to the distress label may expect that their community benefits from greater redistribution, and cast a ballot for the Democratic Party because it supports geographic and income-based transfers like the ARC (Hoy and Mager, 2021). Second, Appalachia's socially conservative, fiscally liberal voters may interpret the distress label as bad news about relative deprivation during the Great Recession and punish President Bush's Republican Party (Roemer, 1998). Third, voters in the at-risk counties may become more hostile to redistributive transfers like the ARC and the Democratic Party that supports them due to last-place aversion (Kuziemko et al., 2014).

7 Conclusion

Using a discontinuity introduced by an unanticipated policy reform, I find strong evidence that the Appalachian Regional Commission improves the vote share of Democratic Party candidates in counties marginally labeled "economically distressed". The impacts show some degree of persistence and are robust to bandwidth selection, covariate inclusion, and increasing the order of a polynomial control for the running variable. This change in vote share is meaningful compared to typical campaign interventions and slowed the rightward drift of Appalachian counties which were less than 10 percent college educated and largely rural, even as this group became the most reliable Republican voting bloc (Cohn, 2016).

I present evidence consistent with the interpretation that the ARC's political ramifications stem from information and media coverage generated by county economic status labels themselves. Across the threshold for the distressed county label, there are not significant discontinuities in expenditures per capita, the employment to prime age adult ratio, the federal grant matching rate, or an ARC public awareness index. News stories mention county

economic status frequently, place disproportionate emphasis on distressed status and counties, include maps adapted from the ARC, and emphasize announcements of and changes to county economic status. My results are consistent with an important role for local media in disseminating information about regional economic conditions.

There are two literatures that dovetail with the ARC's political effects. First, my findings build on lab and survey experiments by highlighting a case where updating voter information about relative deprivation may affect actions in a field setting (Cruces et al., 2013; Kuziemko et al., 2015; Karadja et al., 2016; Fernandez-Albertos and Kuo, 2018). Second, this paper ties into a growing body of evidence about the potent influence news coverage may have over voter opinions (DellaVigna and Kaplan, 2007; Snyder and Stromberg, 2010; Enikolopov et al., 2011; Voigtlaender and Voth, 2014; Durante et al., 2019; Huet-Vaughn, 2019; Garz and Martin, 2020). My observation that the distress label decelerated some counties' drift toward the Republican Party is consistent with the idea that information about poverty and relative deprivation can be a determinant of voters' choices.

The impact of the ARC has ramifications for a wider set of place-based policies. Half a dozen regional development commissions were set up to mirror the ARC and some states have replicated its county economic status approach to improve local labor market conditions (Cecire, 2019; Perez and Suher, 2019). Several other place-based policies work by tagging relatively poor locations for program eligibility and may inadvertently confer information about local poverty and relative deprivation in the process. Moreover, interventions akin to the "economic distress" label may exist outside of public spending programs. My findings show that there may not need to be fiscal consequences attached to an economic label for it to affect party support or sway the electorate.

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Figures

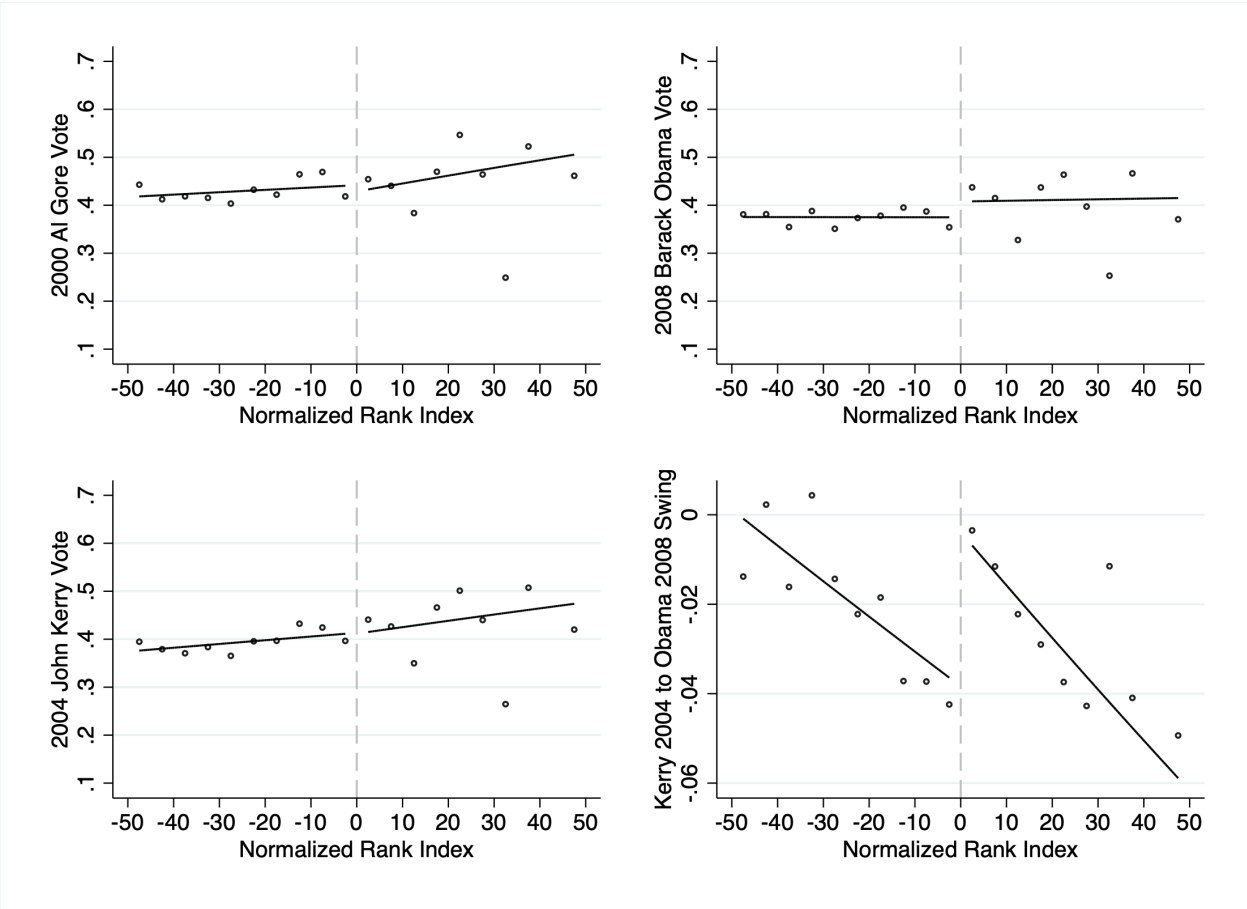

Figure 1: Discontinuity in Democratic Presidential Vote Share in 2008

Note: The left side panels display Democratic two-party vote shares in pre-reform election cycles. The top right panel displays average Democratic two-party presidential vote share in the post-reform 2008 election cycle. The bottom right panel displays the shift between the 2004 Democratic two-party vote share and the 2008 post-reform Democratic two-party vote share. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

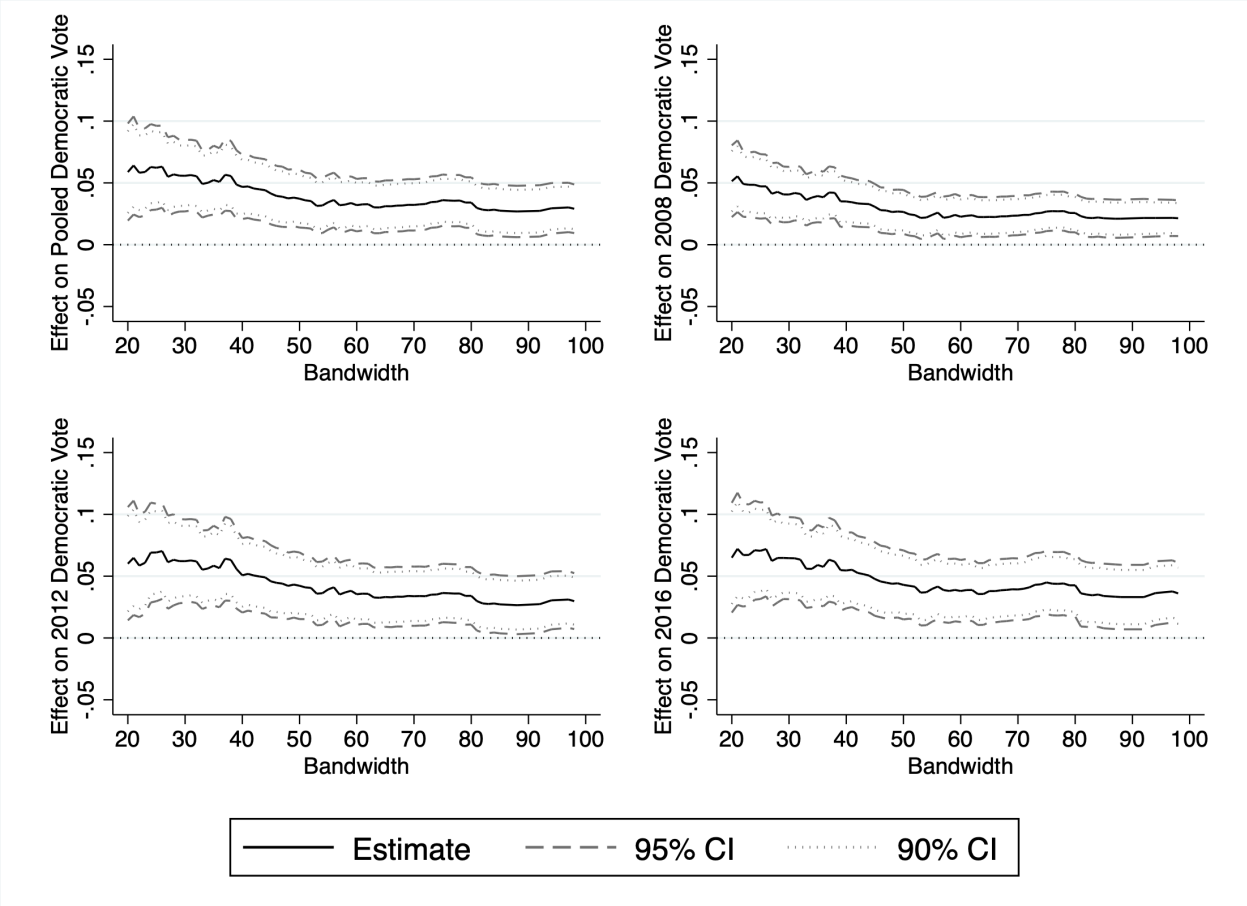

Figure 2: Robustness of Presidential Results to Bandwidth Selection

Note: Each graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the effect of the county economic distress label on respective electoral outcomes. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

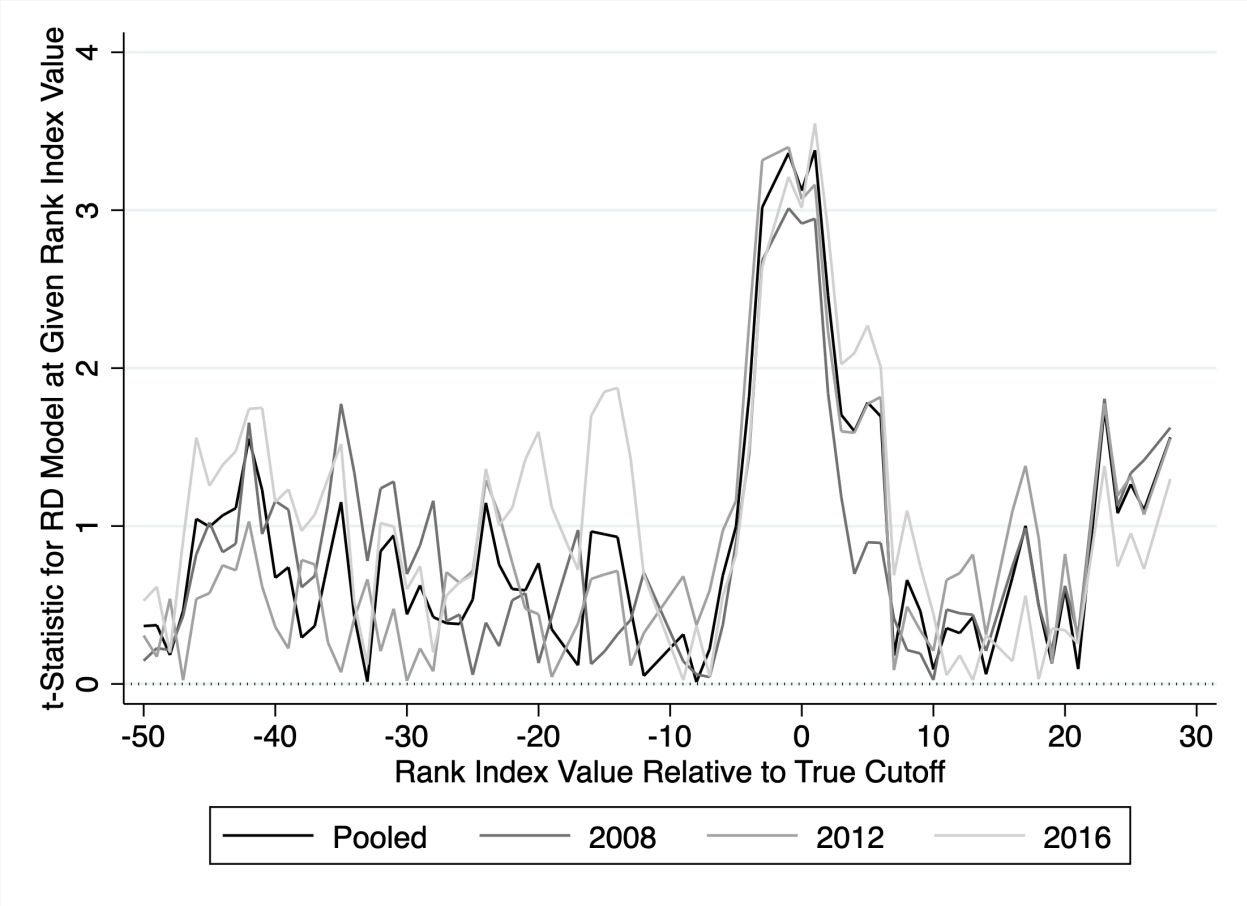

Figure 3: Presidential Falsification Test with Alternative Cutoffs

Note: Each line reflects the estimated t-statistic of the impact of the economic distress label on a particular electoral outcome if the threshold were reassigned to a particular score along the rank index. Alternative cutoffs are indexed on the x-axis relative to the true cutoff value. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

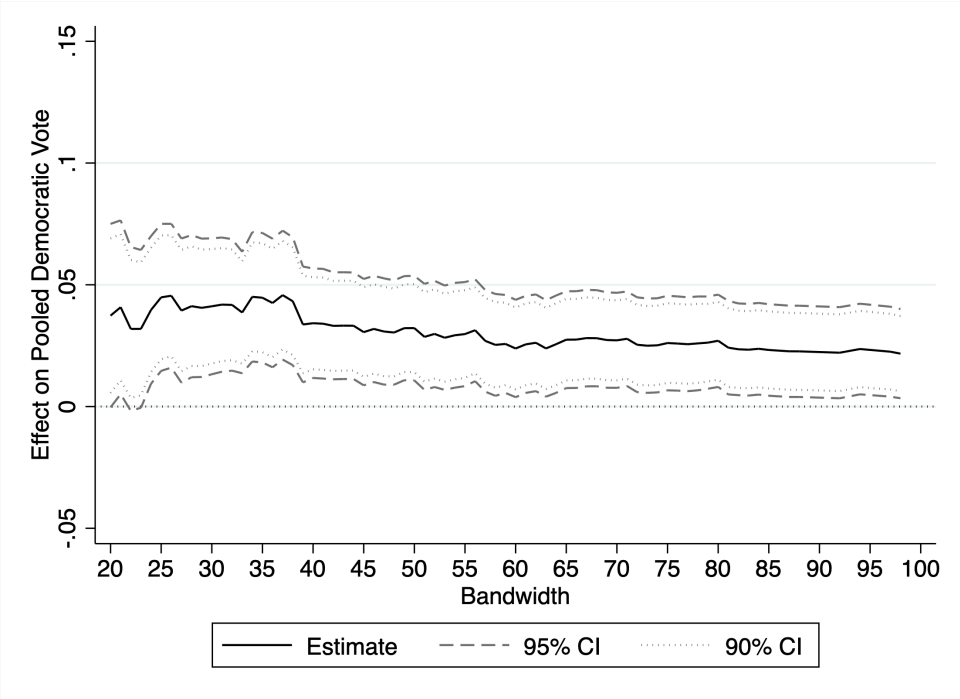

Figure 4: Robustness of House Results to Bandwidth Selection

Note: The graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the effect of the county economic distress label on the Democratic share of the two-party House of Representatives vote in counties with races contested by both major parties. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

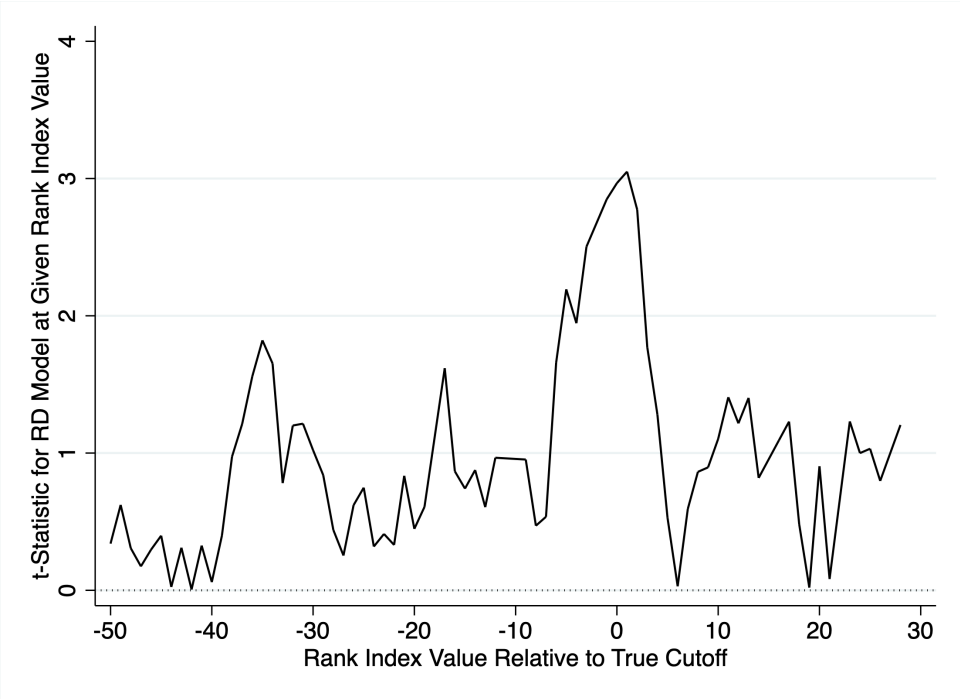

Figure 5: House Falsification Test with Alternative Cutoffs

Note: The graph reflects the estimated t-statistic of the impact of the economic distress label on the Democratic share of the two-party House of Representatives vote in counties with races contested by both major parties if the threshold were reassigned to a particular score along the rank index. Alternative cutoffs are indexed on the x-axis relative to the true cutoff value. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Tables

Table 1: Appalachian Regional Commission Descriptive Statistics

Variable	Mean	Min	Max	Distress	USA
A. Educational Background					
2000 High School Graduate	71.1%	49.2%	91.4%	61.2%	80.4%
2000 Bachelor's Degree Holder	13.0%	4.9%	47.5%	9.0%	24.3%
B. Economic Conditions					
2004-6 Unempoyment Rate	5.9%	2.7%	12.6%	7.8%	5.1%
2000 Census Poverty Rate	16.3%	5.2%	45.4%	26.1%	12.4%
Per Cap. Market Income ($1000s)	18.4	8.2	36.3	12.9	29.3
C. Presidential Two-Party Vote Share					
2000 Al Gore Vote	43.9%	14.6%	87.5%	46.9%	50.3%
2004 John Kerry Vote	40.9%	14.9%	83.2%	44.8%	48.7%
2008 Barack Obama Vote	42.0%	14.4%	87.1%	42.4%	53.6%
2012 Barack Obama Vote	39.2%	8.8%	87.1%	37.1%	51.9%
2016 Hillary Clinton Vote	34.1%	8.9%	84.0%	28.3%	51.1%

Note: Mean values are for the full set ARC counties. "Distress" refers to the subsample of ARC counties labeled distressed for the 2009 Fiscal Year. "USA" refers to the respective national values. Vote shares are weighted by the respective number of ballots cast. Data are from the MIT Election Lab and ARC Administrative Documents.

Table 2: Effects on Democratic Presidential Vote Share

Outcome Variable	(1)	(2)	(3)	(4)	(5)	(6)
A. Pooled Data with All Elections						
Pooled Dem. Vote	0.044*	0.037**	0.061*	0.061**	0.052*	0.062**
	(0.017)	(0.012)	(0.026)	(0.018)	(0.025)	(0.017)
	[786]	[786]	[786]	[786]	[351]	[351]
B. Effects Decomposed by Election Year						
Obama 2008 Vote	0.034**	0.027**	0.057**	0.049**	0.047**	0.047**
	(0.013)	(0.009)	(0.019)	(0.014)	(0.017)	(0.013)
	[262]	[262]	[262]	[262]	[117]	[117]
Obama 2012 Vote	0.052**	0.042**	0.068*	0.066**	0.060*	0.069**
	(0.020)	(0.014)	(0.030)	(0.021)	(0.029)	(0.020)
	[262]	[262]	[262]	[262]	[117]	[117]
Clinton 2016 Vote	0.047*	0.043**	0.058+	0.067**	0.050	0.071**
	(0.022)	(0.014)	(0.034)	(0.022)	(0.033)	(0.020)
	[262]	[262]	[262]	[262]	[117]	[117]
C. Effects Decomposed by State Competitiveness						
Swing State	-0.006	0.005	0.082	0.023	0.044	0.018
	(0.051)	(0.026)	(0.096)	(0.047)	(0.064)	(0.016)
	[258]	[258]	[258]	[258]	[84]	[84]
Safe State	0.046*	0.037**	0.047+	0.071**	0.047+	0.068**
	(0.019)	(0.014)	(0.027)	(0.022)	(0.026)	(0.020)
	[528]	[528]	[528]	[528]	[267]	[267]
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial Order	1	1	2	2	1	1
Bandwidth Size	50	50	50	50	25	25
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-12,5]	[-12,5]

Note: Standard errors clustered on county in parentheses. Sample size in brackets. Symbols denote $p < 0.1$ (+), $p < 0.05$ (∗), and $p < 0.01$ (∗∗). Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 3: Effects on Democratic House of Representatives Vote Share

Outcome Variable	(1)	(2)	(3)	(4)	(5)	(6)
A. Pooled Data with All Elections						
Pooled Dem. Vote	0.023	0.032**	0.045+	0.041*	0.051*	0.045**
	(0.016)	(0.011)	(0.023)	(0.017)	(0.021)	(0.015)
	[1,399]	[1,399]	[1,399]	[1,399]	[631]	[631]
B. Effects Decomposed by County-Level 2000 Presidential Vote						
More Democratic	0.048*	0.038**	0.065*	0.043*	0.068*	0.040*
	(0.022)	(0.013)	(0.031)	(0.020)	(0.028)	(0.019)
	[808]	[808]	[808]	[808]	[391]	[391]
More Republican	-0.009	0.007	0.009	0.021	0.036	0.060*
	(0.023)	(0.020)	(0.034)	(0.032)	(0.028)	(0.026)
	[591]	[591]	[591]	[591]	[240]	[240]
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial Order	1	1	2	2	1	1
Bandwidth Size	50	50	50	50	25	25
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-12,5]	[-12,5]

Note: Standard errors clustered on county in parentheses. Sample size in brackets. Symbols denote $p < 0.1$ (+), $p < 0.05$ (∗), and $p < 0.01$ (∗∗). Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 4: Effects on Vote Share including Time-Variation in Label

Outcome	(1) Obama 2012	(2) Obama 2012	(3) Clinton 2016	(4) Clinton 2016	(5) House 10-18	(6) House 10-18
2008 Distress	0.039^{*}	0.040^{**}	0.033	0.023	0.027^{*}	0.035^{**}
	(0.017)	(0.014)	(0.022)	(0.015)	(0.014)	(0.010)
Reecent Distress	0.029^{+}	0.004	0.034^{+}	0.015	0.011	0.002
	(0.017)	(0.013)	(0.019)	(0.012)	(0.011)	(0.009)
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial	1	1	1	1	1	1
Bandwidth	50	50	50	50	50	50
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-35,7]
Sample Size	352	352	349	349	1,483	1,483

Note: Standard errors clustered on county in parentheses. Bandwidth is Symbols denote $p < 0.1$ (+), $p < 0.05$ ($*$), and $p < 0.01$ ($**$). Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. 2008 Distress refers to the coefficient on crossing the cutoff value for the economic distress index announced in the 2008 calendar year. Recent distress refers to the coefficient on crossing the cutoff value for the economic distress index announced in the current election year. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 5: Effect of Distressed Label on Six Potential Mechanisms

	(1) Employed	(2) ARC $	(3) Match	(4) Awareness	(5) All Grants	(6) SNAP
Distressed	-0.006	9.346	-0.035	-0.889	-120.658	-0.022
	(0.032)	(13.401)	(0.056)	(9.466)	(215.273)	(0.042)
Polynomial	1	1	1	1	1	1
Bandwidth	50	50	50	50	50	50
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-35,7]
Sample Size	1,572	262	262	262	262	262

Note: Standard errors clustered on county in parentheses. Symbols denote $p < 0.1$ (+), $p < 0.05$ ($*$), and $p < 0.01$ ($**$). Employed refers to election year employment rates. ARC $ refers to total ARC project spending. Match refers to the dollar to dollar generosity on ARC project spending. Awareness refers to the normalized Google search interest index for the ARC. All grants refers to total grant spending by the US Federal Government in a particular county. SNAP refers to an indicator for county-level waivers from work requirements for able-bodied adults without dependents. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Online Appendices

Economic Distress and Electoral Consequences: Evidence from Appalachia

Daniel Firoozi

Appendix A: Background Information

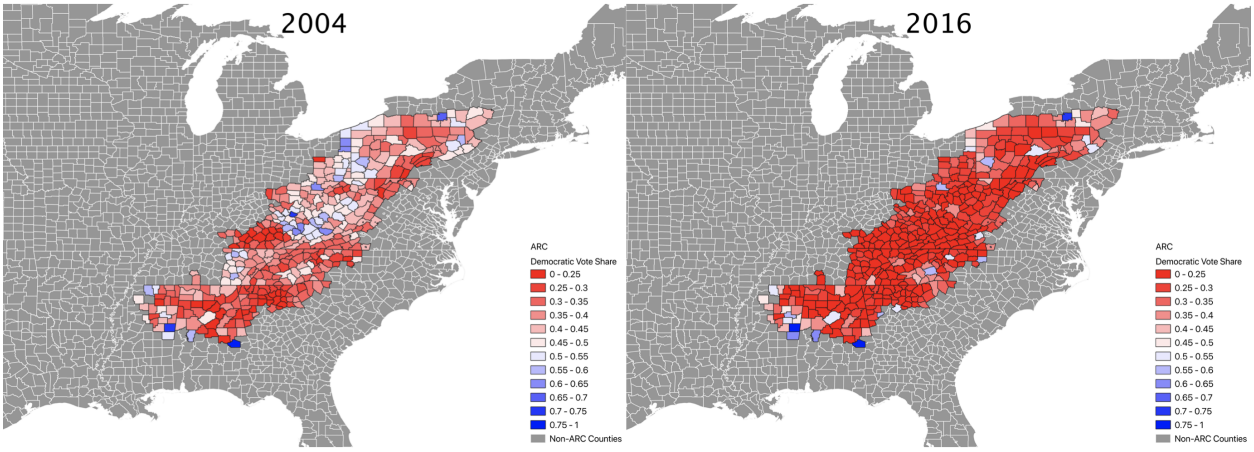

Figure 6: Presidential Election Results in Appalachia from 2004 and 2016

Note: Data from MIT Election Lab.

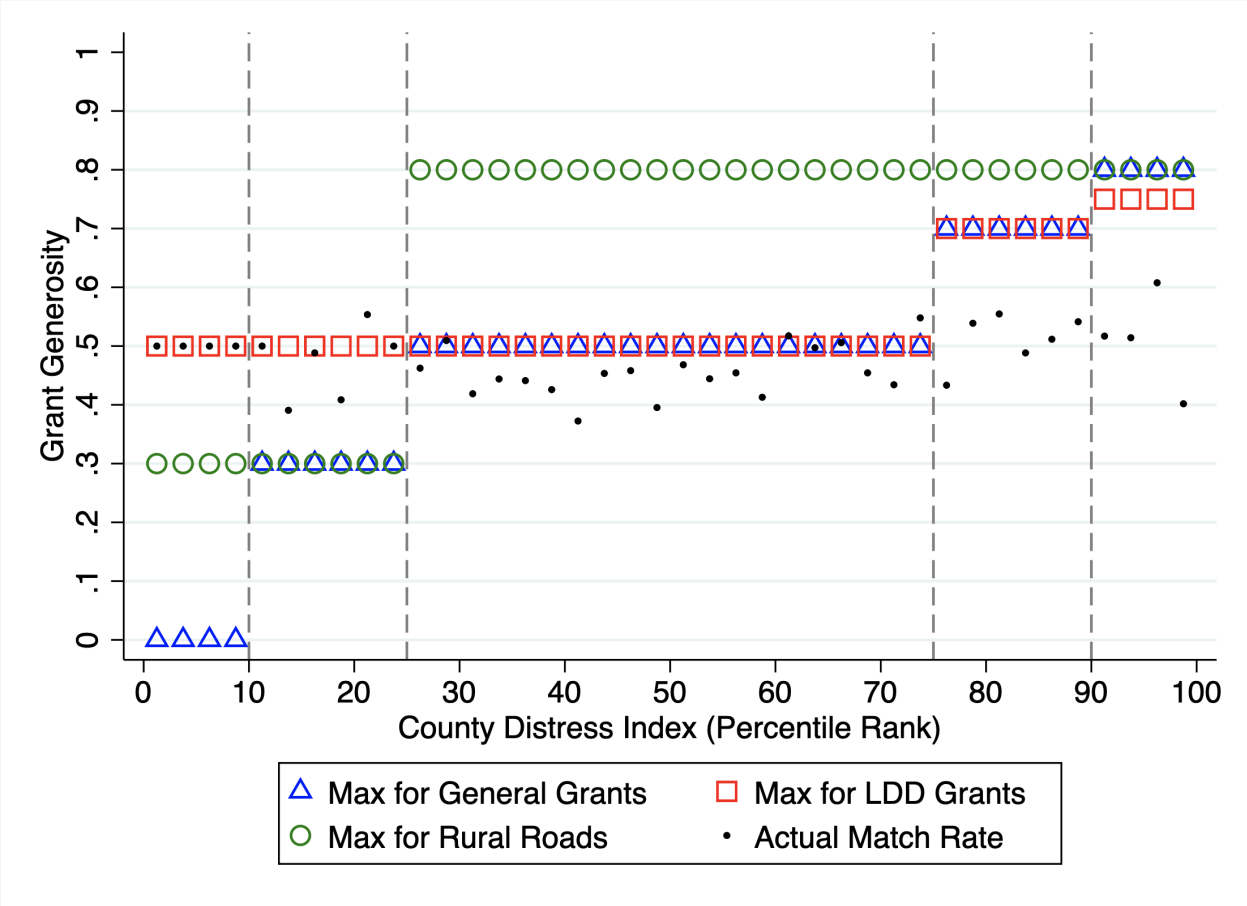

Figure 7: ARC Grant Generosity by Category

Note: Open shapes represent the maximum level of federal grant generosity allowed for a county at a particular percentile of the composite distress index distribution. These maximum levels vary across grant category. Dots reflect the actual generosity across all grant types disbursed to counties prior to the first post-reform election.

Appendix A.1: Raw Disadvantage Scores and Ordinal Mapping

In this Appendix subsection, I explain the use of a raw rank disadvantage score over the ordinal percentile rank index. The primary justifications for this choice are the potential loss of information and the credibility of the functional form assumptions, but in this setting the choice is immaterial for estimation. In the text below, I (1) provide an account of the small differences between these approaches, (2) explain my rationale for choosing the raw rank index, and (3) offer a transparent mapping of bandwidths between indexes. As a short summary, the approaches yield essentially identical estimates, but there is a small interpretability vs precision tradeoff. All tables make the percentile index bandwidths clear and more transparent.

1. Different Approaches with Nearly Identical Results

   The percentile index maps raw index values to the ordinal rank the county would have among all US counties based on its raw index score. In effect, this compresses variation in economic conditions at the highest levels of economic distress such that, with some abuse of notation $\frac{\partial(\text{Ordinal Rank})}{\partial(\text{Raw Index})} > 0$ and $\frac{\partial^2(\text{Ordinal Rank})}{\partial(\text{Raw Index})^2} < 0$. I provide Table 6 below, which shows sample counties equally spaced apart on the raw index to demonstrate this phenomenon and provide context on economic conditions across the distribution of index values. This compression at high levels of distress does not affect the size of the discontinuity at the threshold because the ordinal ranking of counties remains the same.

Table 6: Evenly-Spaced Sample Counties

County	Forsyth	Surry	Patrick	Mason	Choctaw	Lawrence	McDowell
State	GA	NC	VA	WV	MS	KY	WV
Unemployment	3.4%	5.7%	6.2%	7.5%	8.0%	8.2%	8.5%
Poverty	5.5%	12.4%	13.4%	19.9%	24.7%	30.7%	37.7%
Market Income	$32,295	$19,944	$14,324	$14,454	$12,899	$11,706	$8,829
Raw Index	-101.65	-50.05	-24.95	0.85	25.05	50.15	98.35
Percentile Rank	2.8th	54.3th	77.8th	90.1th	95.7th	97.8th	99.5th

2. Reasons to Prefer a "Raw" Rank Index

   (a) Loss of Meaningful Variation

   The transformation from the raw index to the percentile index eliminates meaningful variation in economic outcomes at the tails of the economic distress distribution. A 1 percentage point increase in unemployment and poverty and a $1,000 decrease in per capita market income translate precisely into a 7.25, 3.27, and 2.27 point increase in the raw index, respectively. By contrast, the relationship between these economic measures and the percentile index is nonlinear. An increase in poverty, an increase in unemployment, or a decline in per capita market income only affect the percentile index when a given county passes another county in the ordinal ranking. Because the distribution of counties is thin in the upper tail of distress, relatively large differences in economic conditions are reduced to minimal or no differences in county rank. This does not impact the size of the discontinuity but does slightly reduce precision because the percentile index is a slightly noisier predictor of electoral outcomes than the raw index.

   (b) Functional Form Assumptions

   The raw index makes a less burdensome functional form assumption. For the raw index I impose the functional form assumption that the relationship between outcome variables and poverty, income, and unemployment is linear or quadratic. For the percentile index I have to make the less credible assumption that the relationship between the outcome variables and the rank index is linear or quadratic in an ordinal mapping of the scaled index of poverty, income, and unemployment among all American counties.

3. A Transparent Mapping between Indexes

In Table 7, I map bandwidths from the percentile index to the raw index. The number of counties within a given bandwidth of the threshold is included in the row titled

"unique counties".

Table 7: Percentile Index to Raw Index Mapping

Percentile Index Bandwidth	0	10	20	30	40	50	60	Max
Raw Index Upper Bound	0	104.1	104.1	104.1	104.1	104.1	104.1	104.1
Raw Index Lower Bound	0	-21.4	-34.0	-45.0	-53.5	-62.0	-69.7	-108.1
Unique Counties	0	134	186	254	303	352	381	428
Unique Counties Above	0	81	81	81	81	81	81	81
Unique Counties Below	0	53	105	173	222	271	300	347

In Table 8, I do the reverse and map bandwidths from the raw index to the percentile index. The number of counties within a given bandwidth of the threshold is once again included in the row titled "unique counties". All tables in the paper include this bandwidth mapping information for transparency.

Table 8: Raw Index to Percentile Index Mapping

Raw Index Bandwidth	0	10	20	25	30	40	50	Max
Percentile Index Upper Bound	0	2.7	4.9	5.7	6.3	7.1	7.7	9.6
Percentile Index Lower Bound	0	-3.2	-8.6	-12.1	-15.3	-24.6	-35.5	-88.5
Unique Counties	0	44	88	117	136	191	262	428
Unique Counties Above	0	24	41	49	53	60	62	81
Unique Counties Below	0	20	47	68	83	131	200	347

Appendix B: Balance Checks

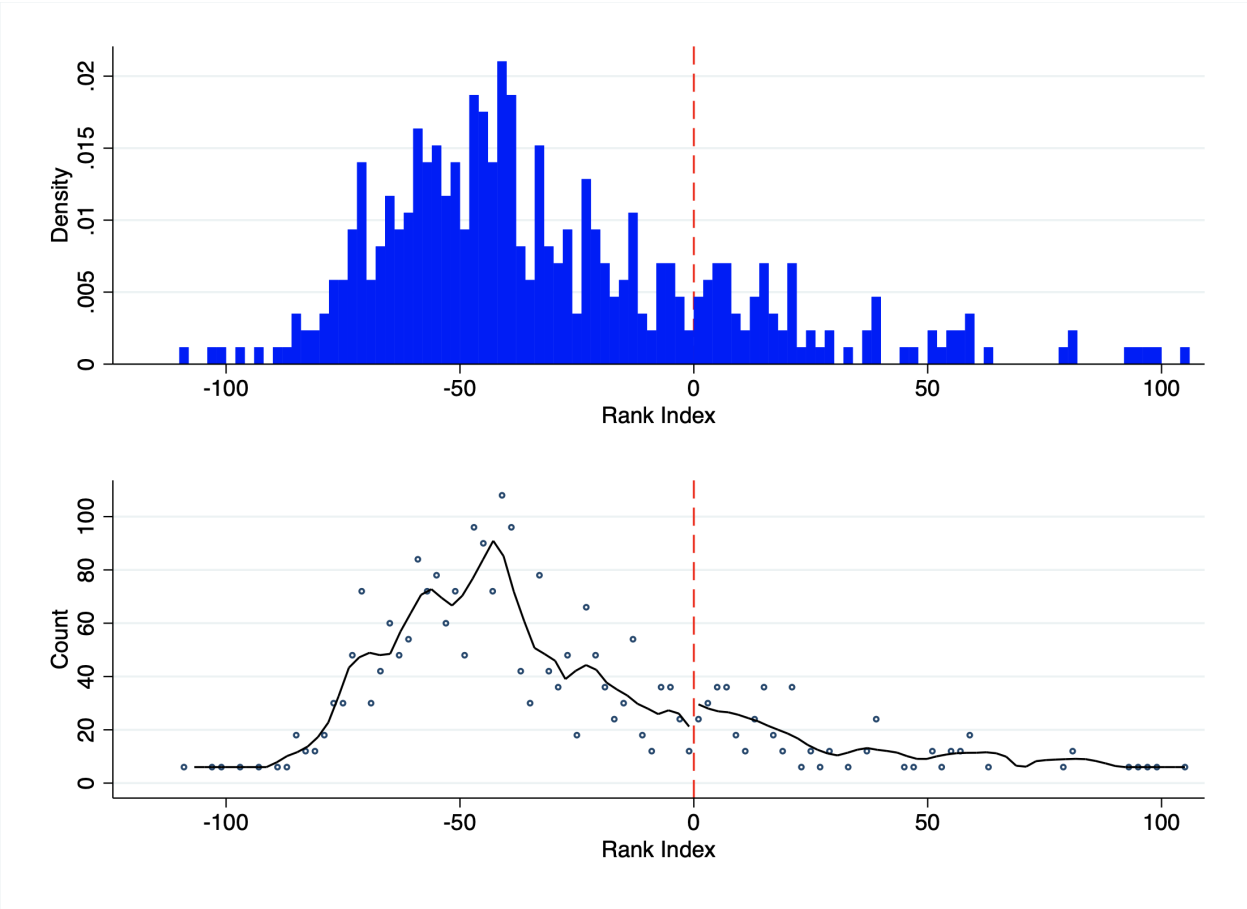

Figure 8: McCrary Density Test

Note: Figure displays density of observations across the raw composite index normalized to the cutoff of distressed county status. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

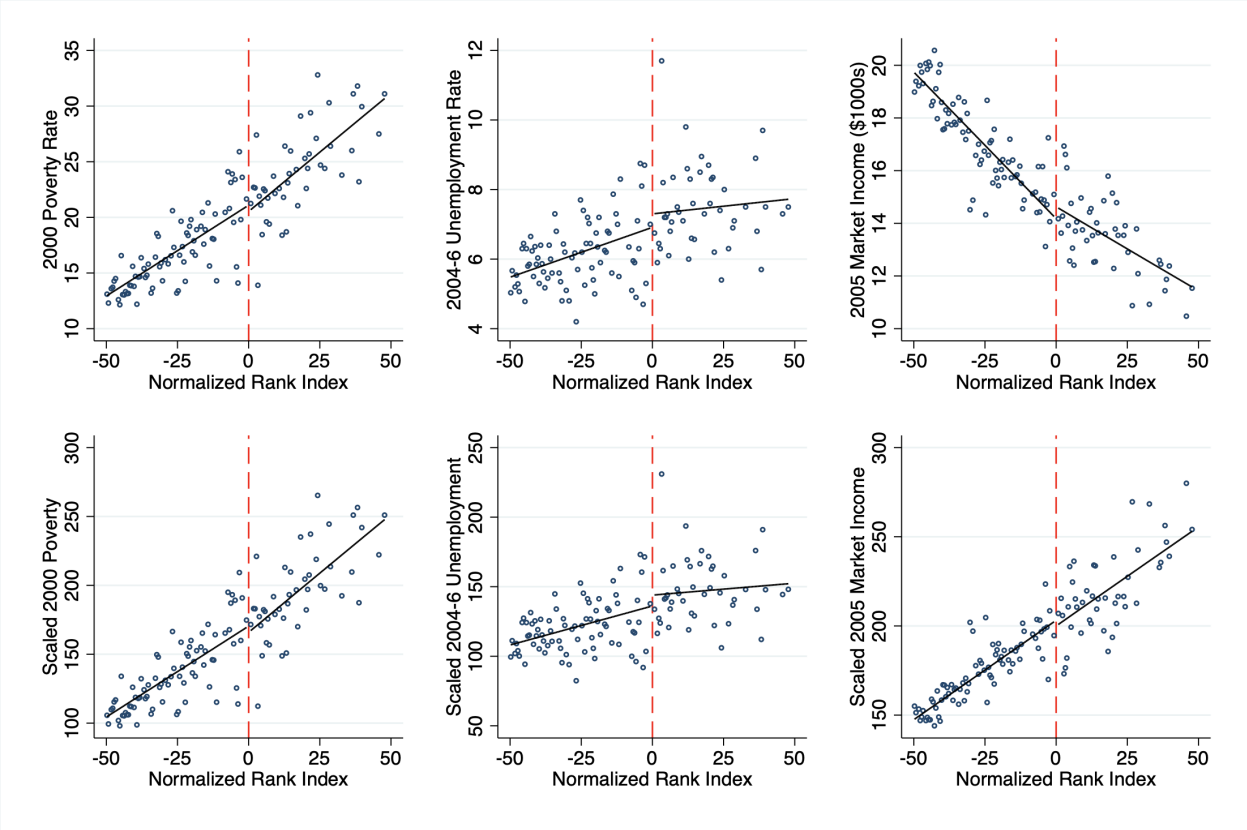

Figure 9: Covariate Balance Checks

Note: Rank index values are normalized from the raw composite index to the 90th percentile cutoff for distressed county status. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

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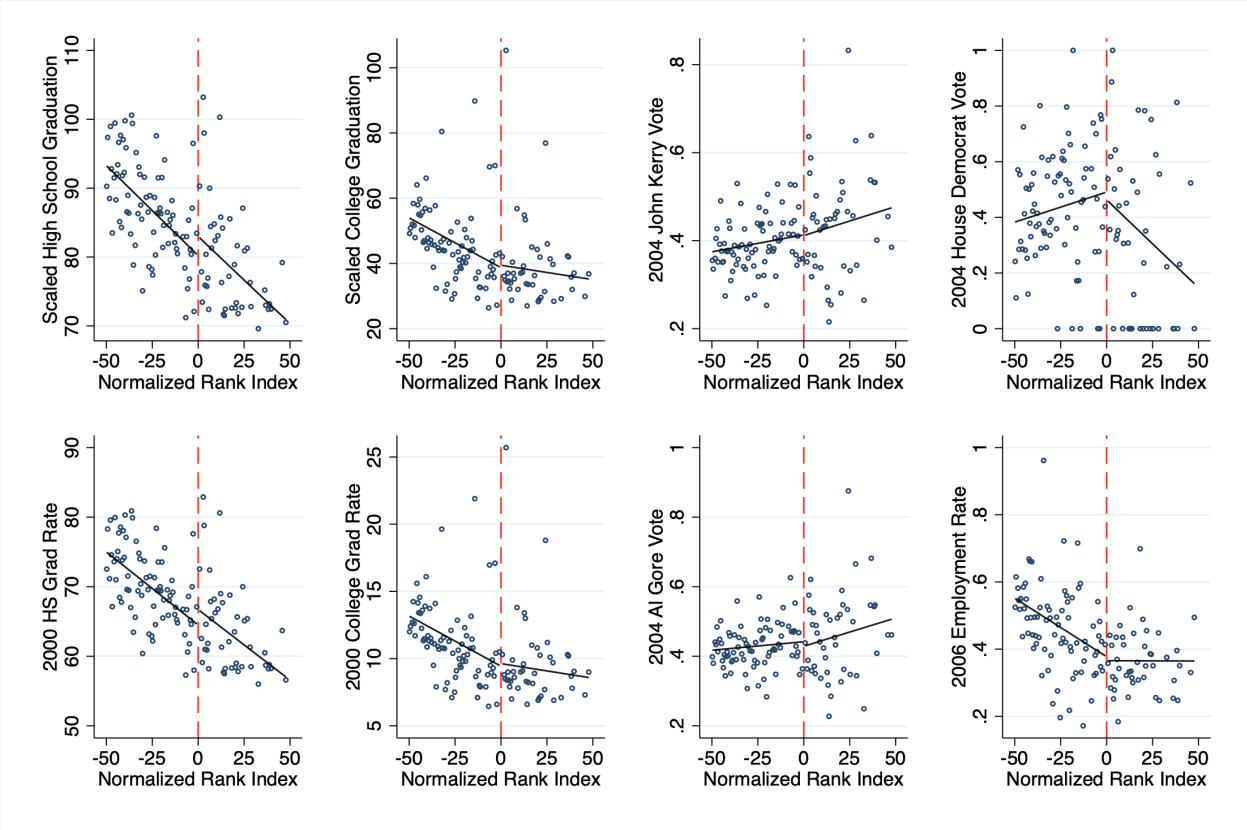

Figure 10: Covariate Balance Checks

Note: Rank index values are normalized from the raw composite index to the 90th percentile cutoff for distressed county status. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

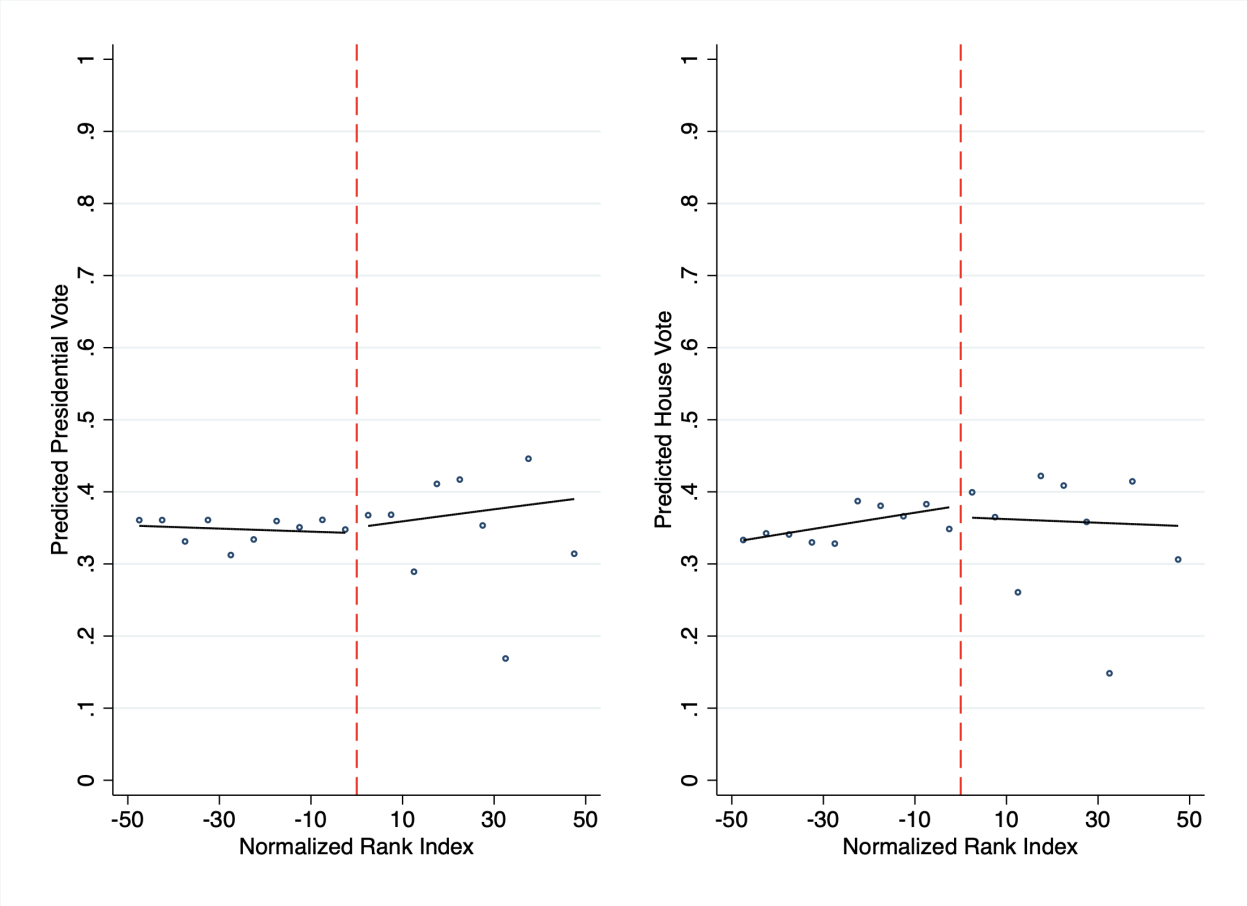

Figure 11: Predicted Democratic Vote using All Covariates and Fixed Effects

Note: The left-hand panel displays the predicted Democratic share of a county's two-party presidential vote using the full set of covariates included in the paper. The right-hand panel displays the predicted Democratic share of a county's two-party House of Representatives vote in counties with contested races using the full set of covariates included in the paper. Rank index values are normalized from the raw composite index to the 90th percentile cutoff for distressed county status. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 9: Covariate Balance Checks

Covariate Tested	Effect
Employment Rate [0-1]	-0.012
	(0.033)
Al Gore 2000 Vote [0-1]	-0.013
	(0.028)
John Kerry 2004 Vote [0-1]	-0.002
	(0.028)
2004 House Dem. Vote [0-1]	-0.028
	(0.078)
Unemployment [0-100]	0.387
	(0.370)
Poverty Rate [0-100]	-0.565
	(0.890)
Market Income [Dollars]	454.163
	(355.554)
High School [0-100]	2.239
	(1.837)
College [0-100]	0.197
	(1.112)
Scaled Unemployment	7.798
	(7.293)
Scaled Poverty	-4.569
	(7.185)
Scaled Market Income	-3.221
	(4.691)
Scaled High School	2.801
	(2.289)
Scaled College	0.759
	(4.558)

Note: Standard errors clustered on county in parentheses. Symbols denote $p < 0.1$ (+), $p < 0.05$ ($*$), and $p < 0.01$ ($**$). Each variable is tested using a local linear specification at a 50 point composite index bandwidth. The sample size is 262 counties and the equivalent percentile bandwidths are 35 points on the left hand side and 7 points on the right hand side.

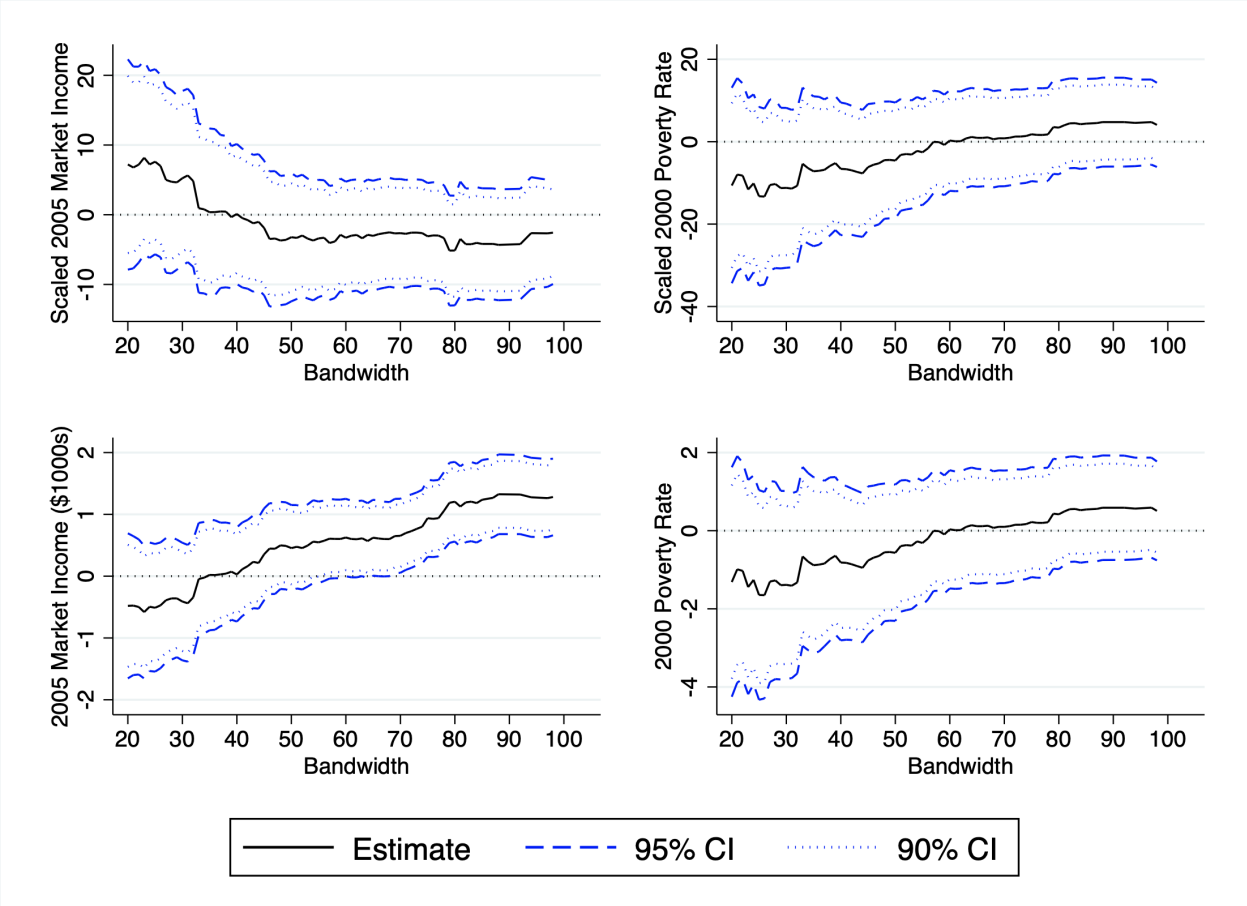

Figure 12: Covariate Balance Check Robustness to Bandwidth

Note: Each graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the discontinuity at the threshold in a given covariate using a local linear specification at a respective bandwidth. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

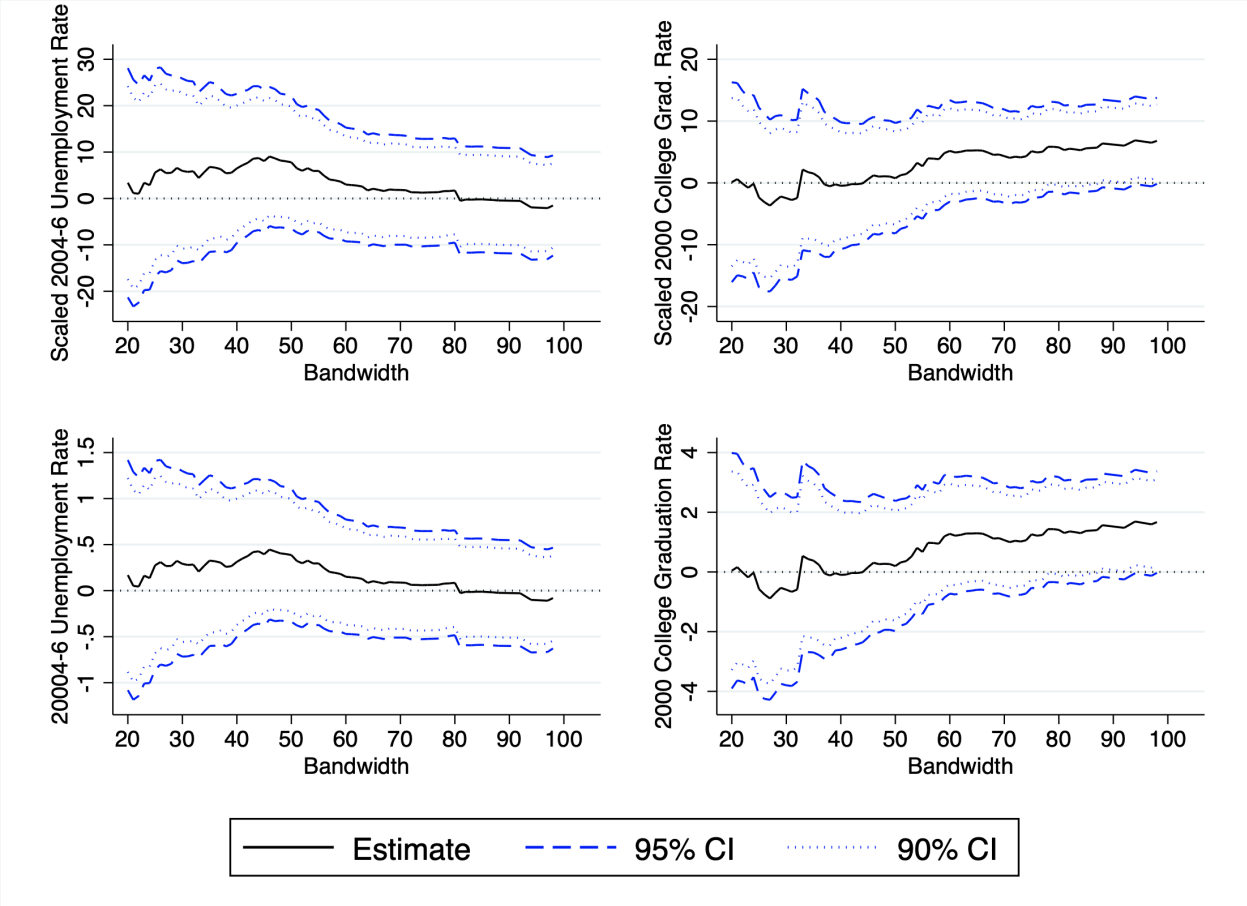

Figure 13: Covariate Balance Check Robustness to Bandwidth

Note: Each graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the discontinuity at the threshold in a given covariate using a local linear specification at a respective bandwidth. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

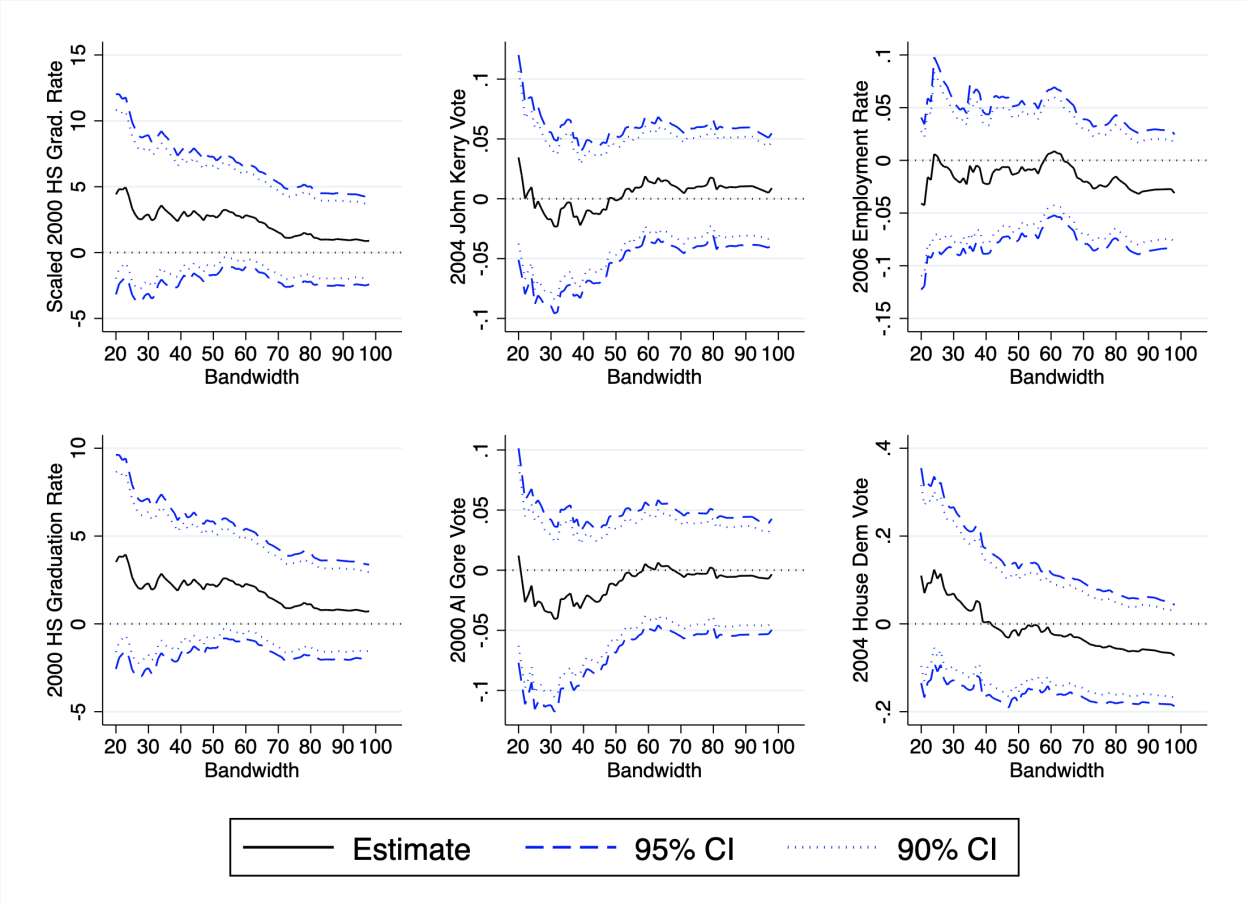

Figure 14: Covariate Balance Check Robustness to Bandwidth

Note: Each graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the discontinuity at the threshold in a given covariate using a local linear specification at a respective bandwidth. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Appendix C: Robustness Checks and Falsification Tests

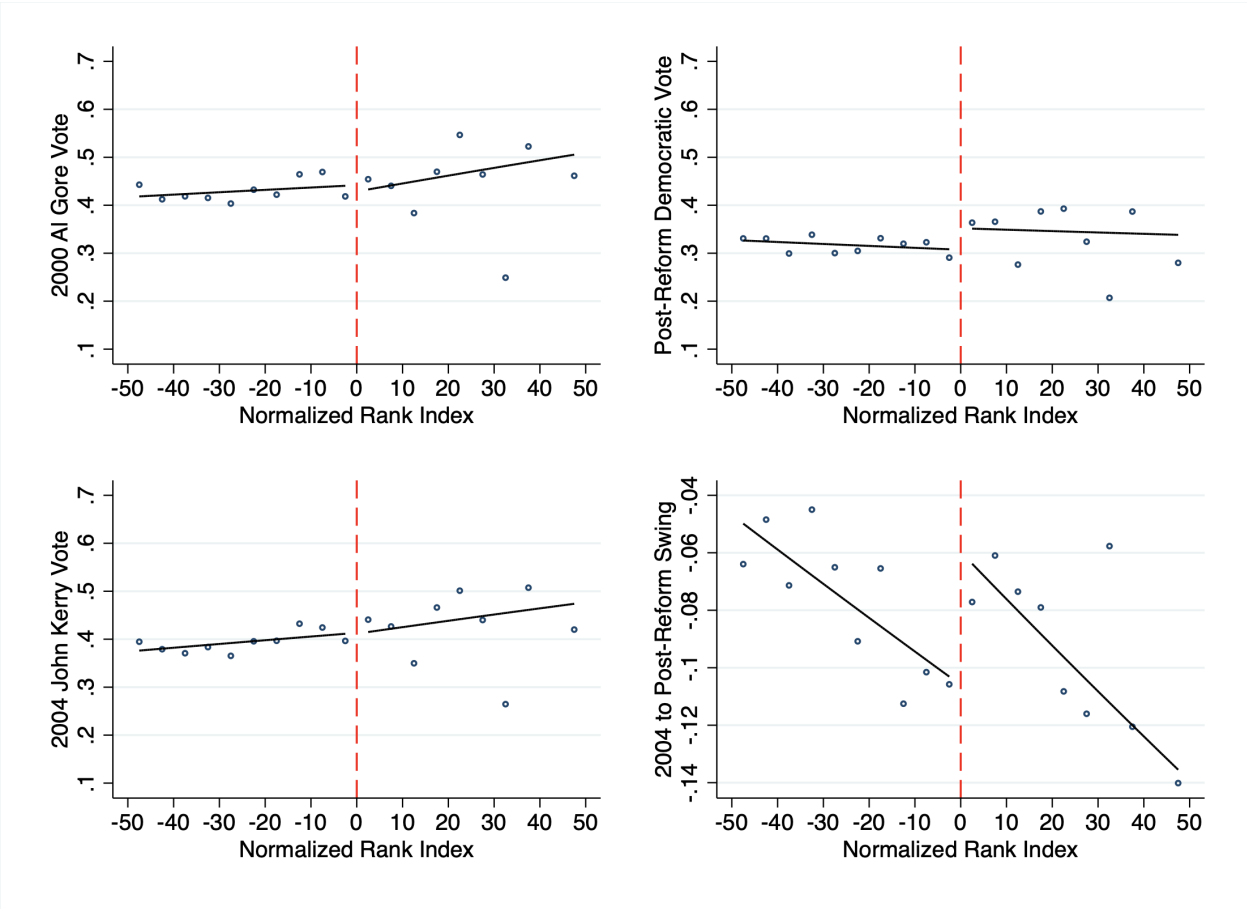

Figure 15: Discontinuity in Democratic Presidential Vote Share

Note: The two left side panels display Democratic two-party vote shares in pre-reform election cycles. The top righ panel displays average Democratic two-party presidential vote share in post-reform election cycles. The bottom right panel displays the shift between the 2004 Democratic two-party vote share and the average post-reform Democratic two-party vote share. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 10: Placebo Tests on Pre-Treatment Democratic Presidential Vote Share

Outcome Variable	(1)	(2)	(3)	(4)	(5)	(6)
Gore 2000 Vote	-0.013	0.008	-0.028	-0.001	-0.030	0.004
	(0.028)	(0.007)	(0.045)	(0.011)	(0.043)	(0.009)
	[256]	[256]	[256]	[256]	[84]	[84]
Kerry 2004 Vote	-0.002	0.011	-0.007	0.008	-0.008	0.019
	(0.028)	(0.010)	(0.044)	(0.015)	(0.041)	(0.013)
	[256]	[256]	[256]	[256]	[84]	[84]
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial Order	1	1	2	2	1	1
Bandwidth Size	50	50	50	50	25	25
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-12,5]	[-12,5]

Note: Standard errors clustered on county in parentheses. Sample size in bracket. Symbols denote $p < 0.1$ (+), $p < 0.05$ (∗), and $p < 0.01$ (∗∗). Lag Vote Controls refer to 1996 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 11: Effects on Democratic Presidential Vote Share

Outcome Variable	(1)	(2)	(3)	(4)	(5)	(6)
A. Effects Decomposed by Nate Silver's 538 Forecast Tipping Point Index						
538 Swing State	-0.001	0.007	0.075	0.011	0.048	0.016
	(0.051)	(0.026)	(0.096)	(0.046)	(0.064)	(0.016)
	[233]	[233]	[233]	[233]	[80]	[80]
538 Safe State	0.043^{*}	0.036^{**}	0.050^{+}	0.073^{**}	0.047^{+}	0.068^{**}
	(0.019)	(0.014)	(0.027)	(0.022)	(0.026)	(0.020)
	[553]	[553]	[553]	[553]	[271]	[271]
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial Order	1	1	2	2	1	1
Bandwidth Size	50	50	50	50	25	25
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-12,5]	[-12,5]

Note: Standard errors clustered on county in parentheses. Sample size in brackets. Symbols denote $p < 0.1$ $(+)$, $p < 0.05$ $(*)$, and $p < 0.01$ $(**)$. Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

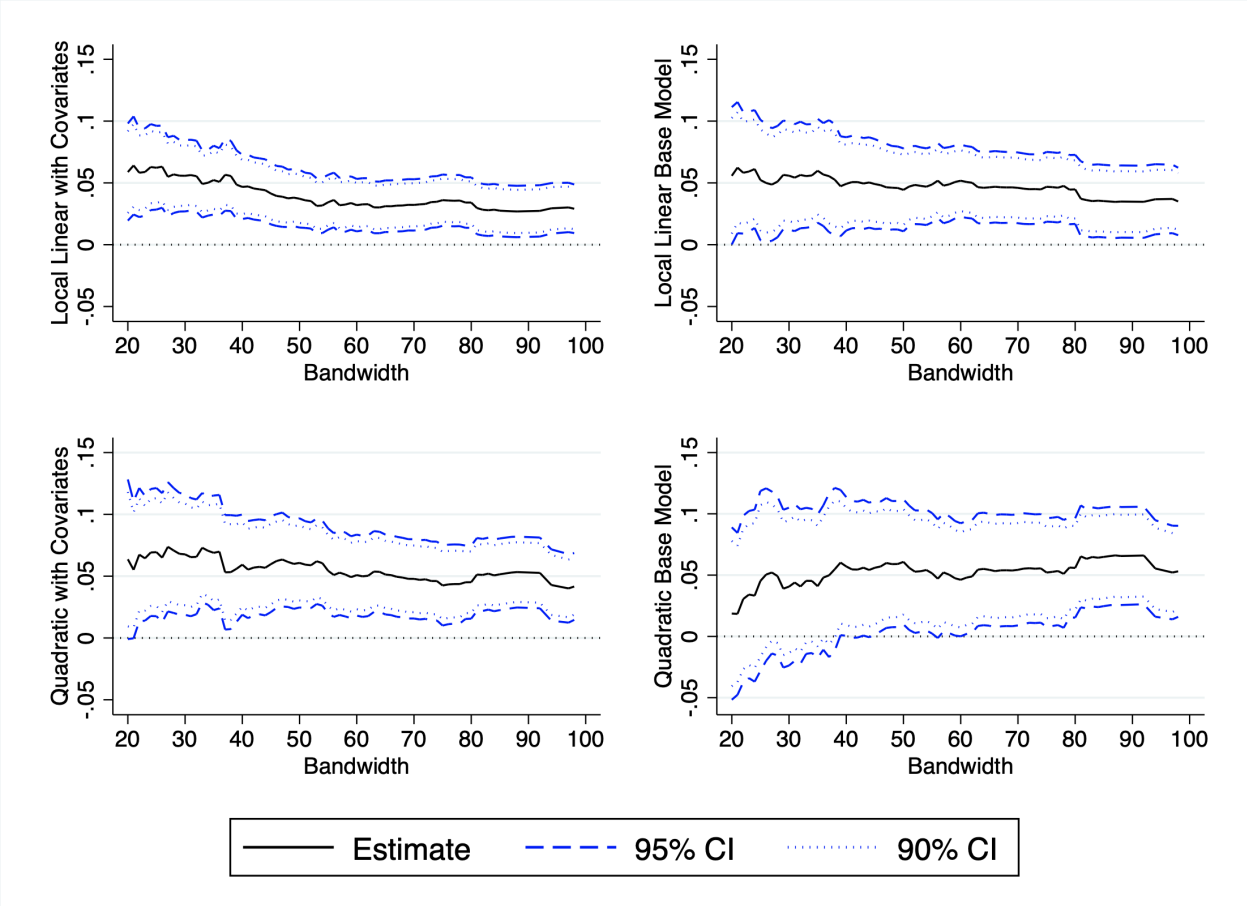

Figure 16: Presidential Robustness to Bandwidth Selection Across Specifications

Note: The graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the effect of the county economic distress label on the post-treatment Democratic share of the two-party presidential vote. Each panel represents a different specification. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

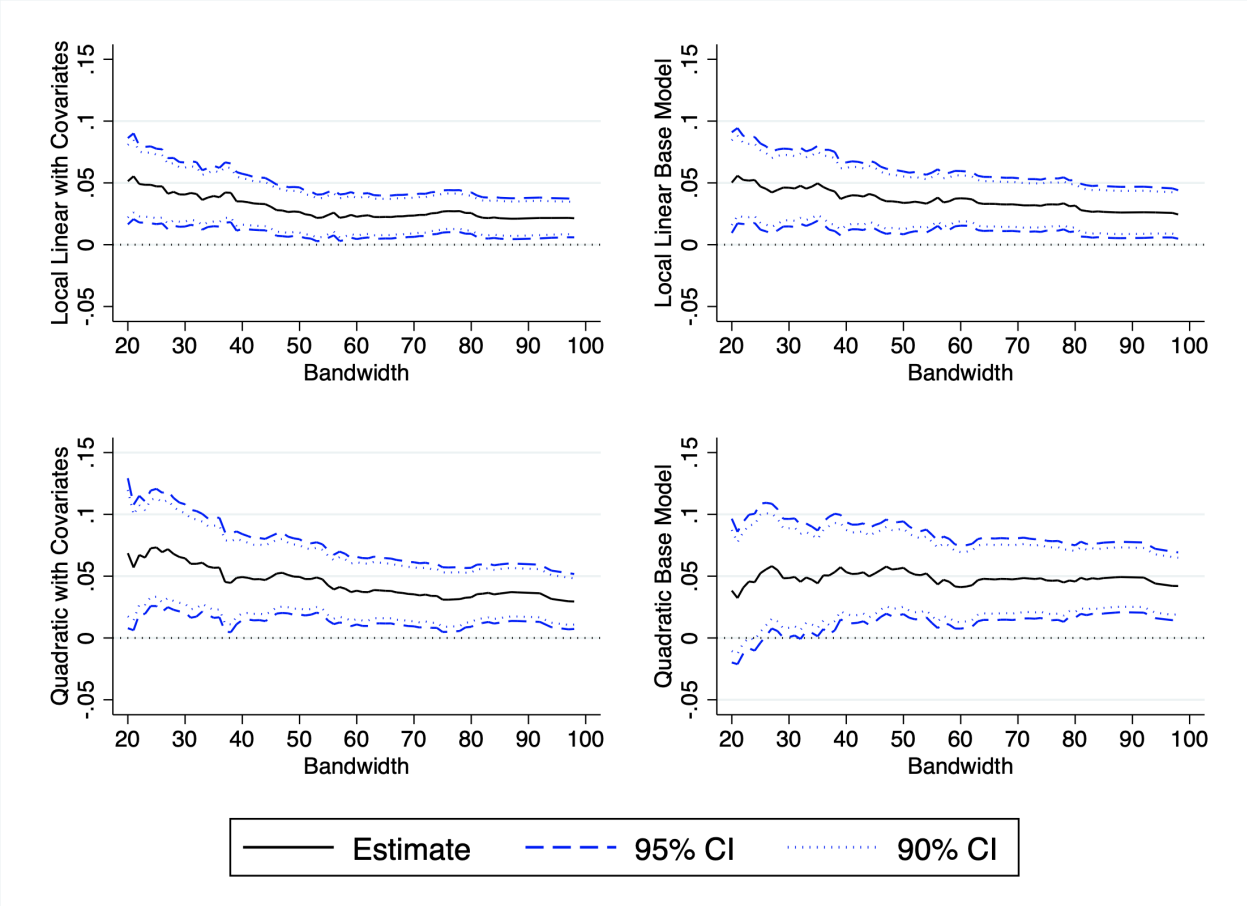

Figure 17: 2008 Presidential Robustness to Bandwidth Selection Across Specifications

Note: The graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the effect of the county economic distress label on the 2008 Democratic share of the two-party presidential vote. Each panel represents a different specification. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

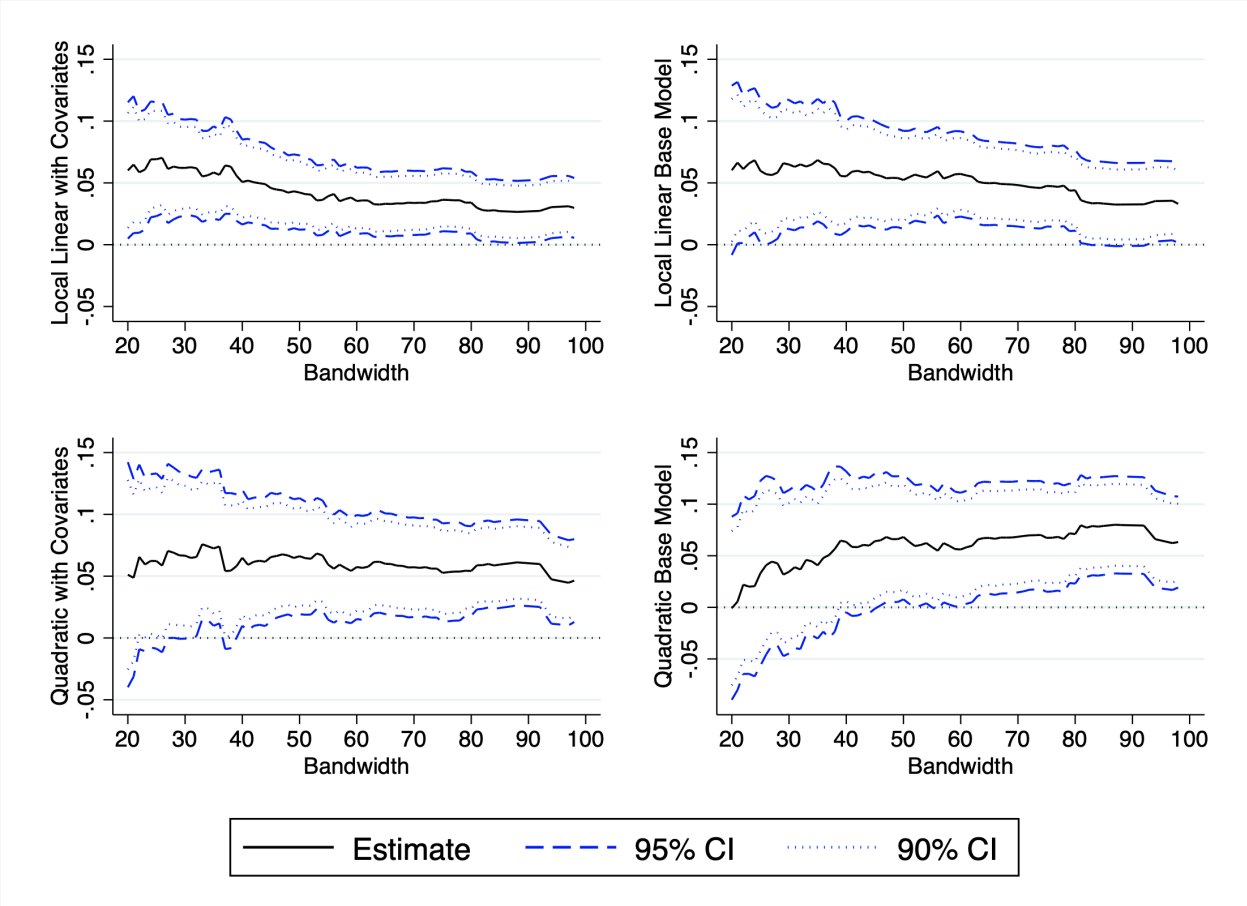

Figure 18: 2012 Presidential Robustness to Bandwidth Selection Across Specifications

Note: The graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the effect of the county economic distress label on the 2012 Democratic share of the two-party presidential vote. Each panel represents a different specification. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

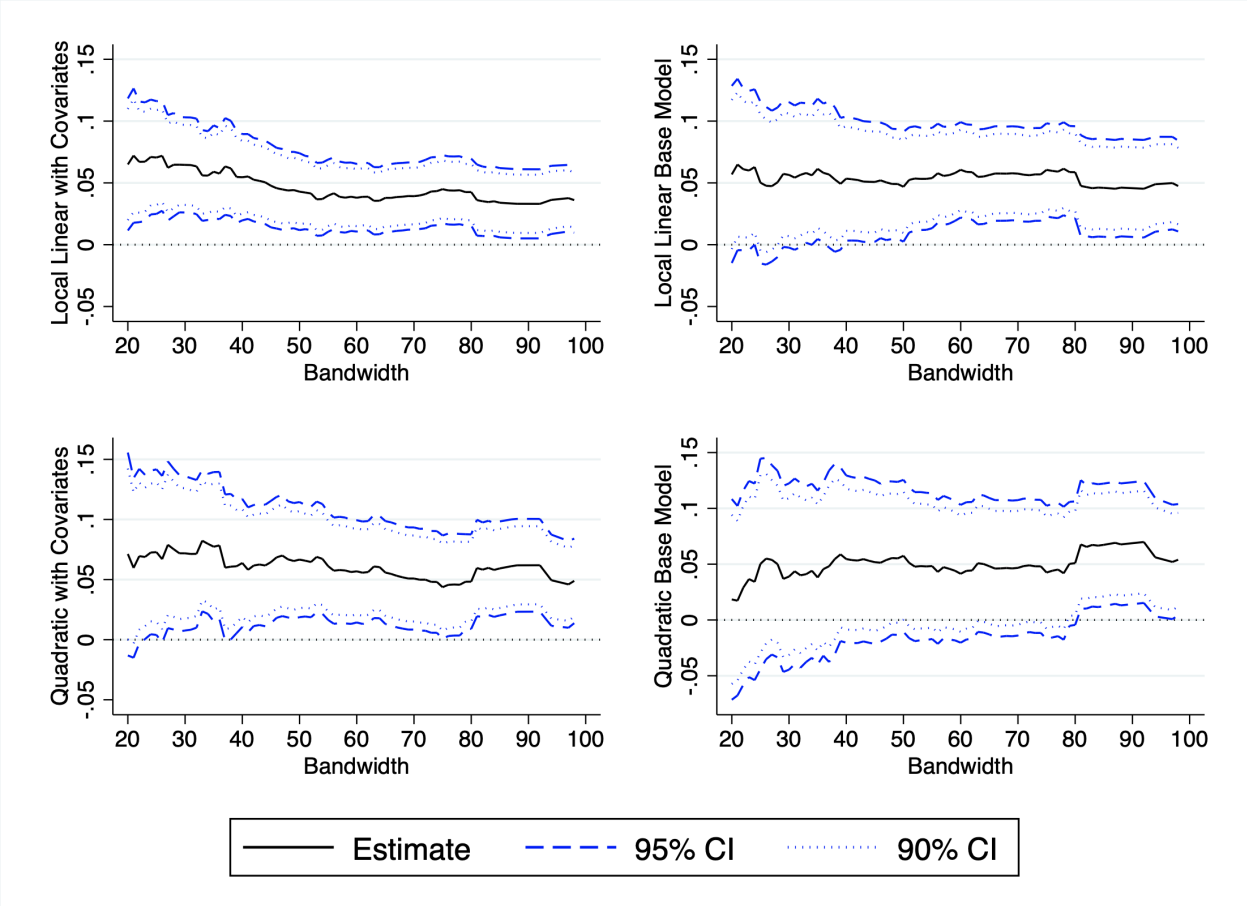

Figure 19: 2016 Presidential Robustness to Bandwidth Selection Across Specifications

Note: The graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the effect of the county economic distress label on the 2016 Democratic share of the two-party presidential vote. Each panel represents a different specification. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

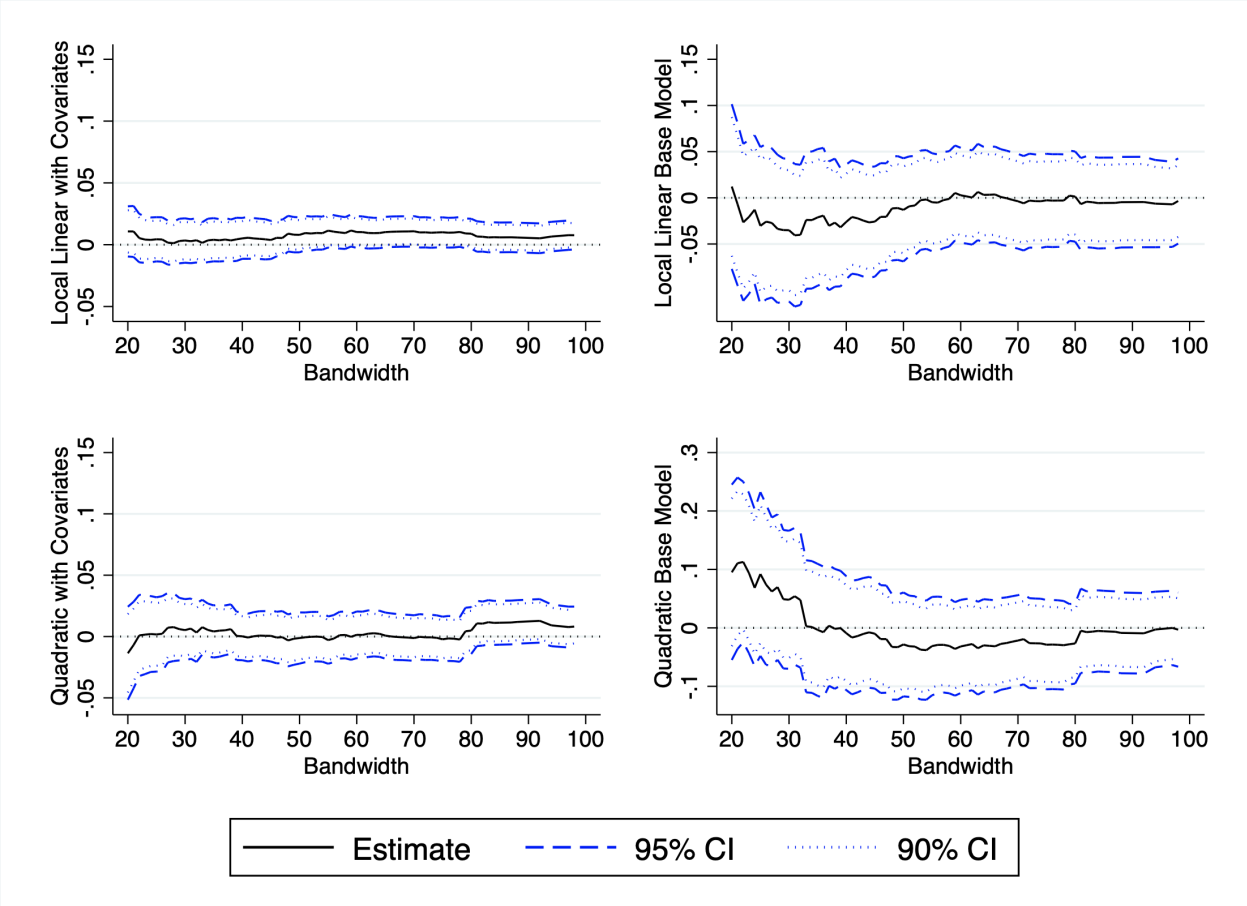

Figure 20: 2000 Presidential Placebo Test by Bandwidth Selection Across Specifications

Note: The graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the placebo test of the county economic distress label on the 2000 pre-treatment Democratic share of the two-party presidential vote. Each panel represents a different specification. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

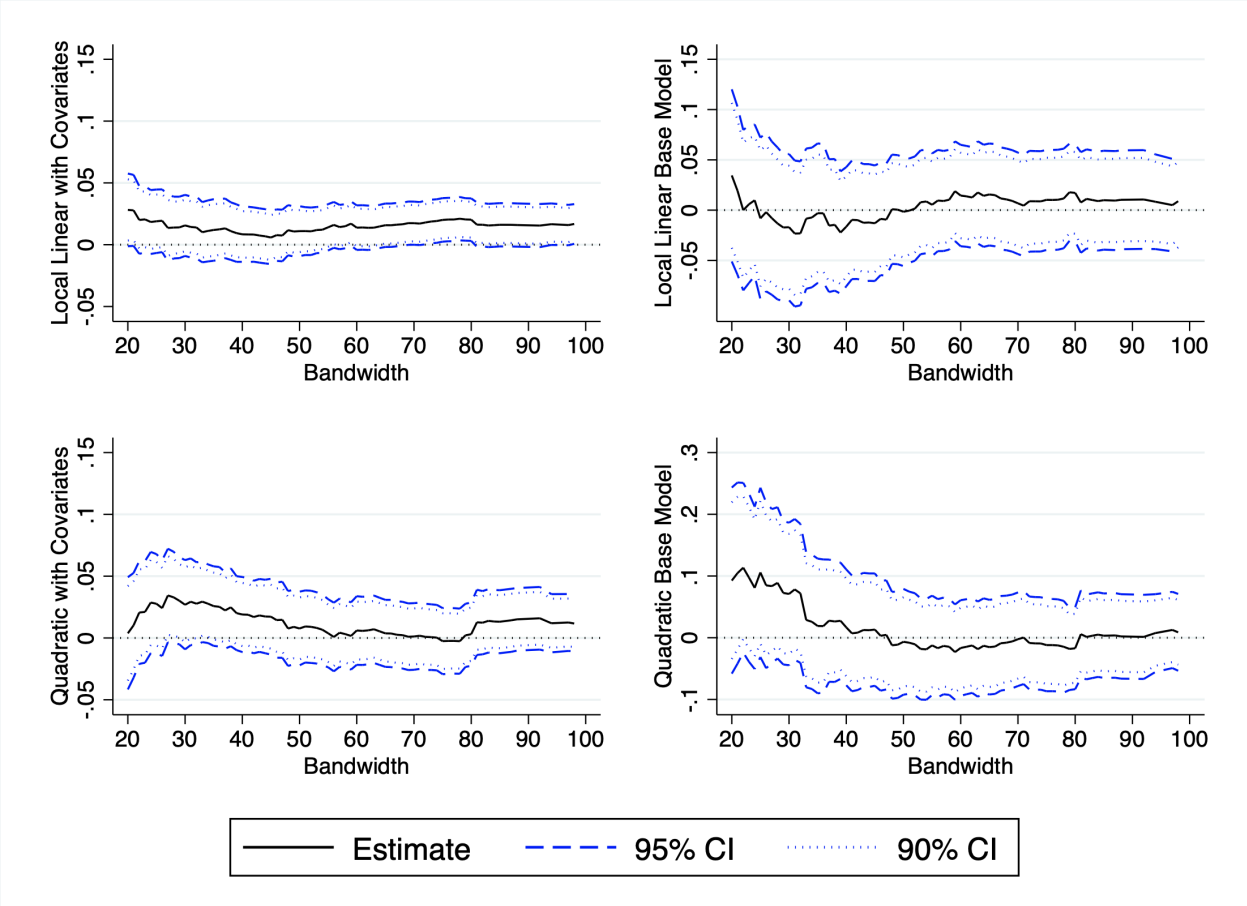

Figure 21: 2004 Presidential Placebo Test by Bandwidth Selection Across Specifications

Note: The graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the placebo test of the county economic distress label on the 2004 pre-treatment Democratic share of the two-party presidential vote. Each panel represents a different specification. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

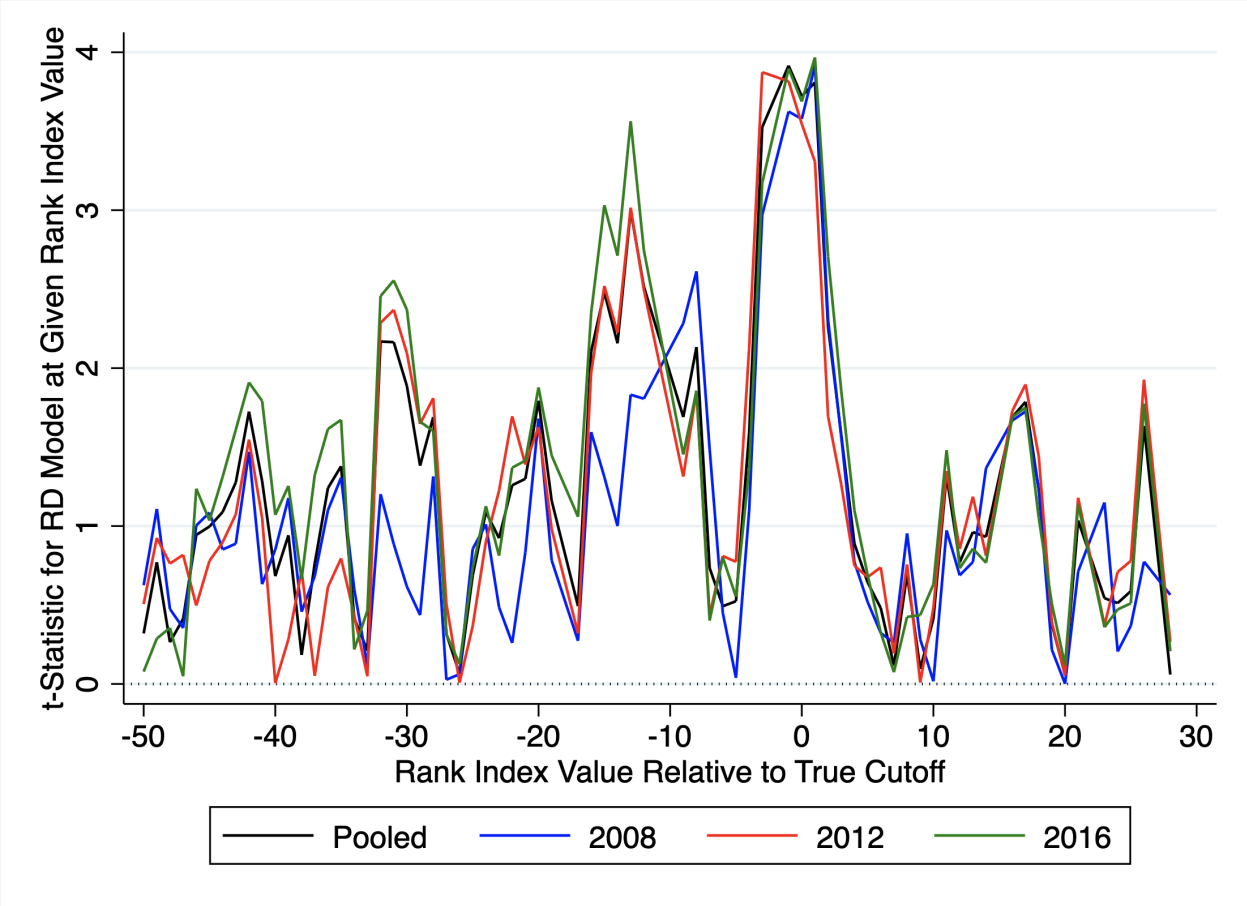

Figure 22: Presidential Falsification Test with Alternative Cutoffs (Narrow Bandwidth)

Note: Each line reflects the estimated t-statistic of the impact of the economic distress label on a particular electoral outcome if the threshold were reassigned to a particular score along the rank index at a 25 point rank index bandwidth. Alternative cutoffs are indexed on the x-axis relative to the true cutoff value. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

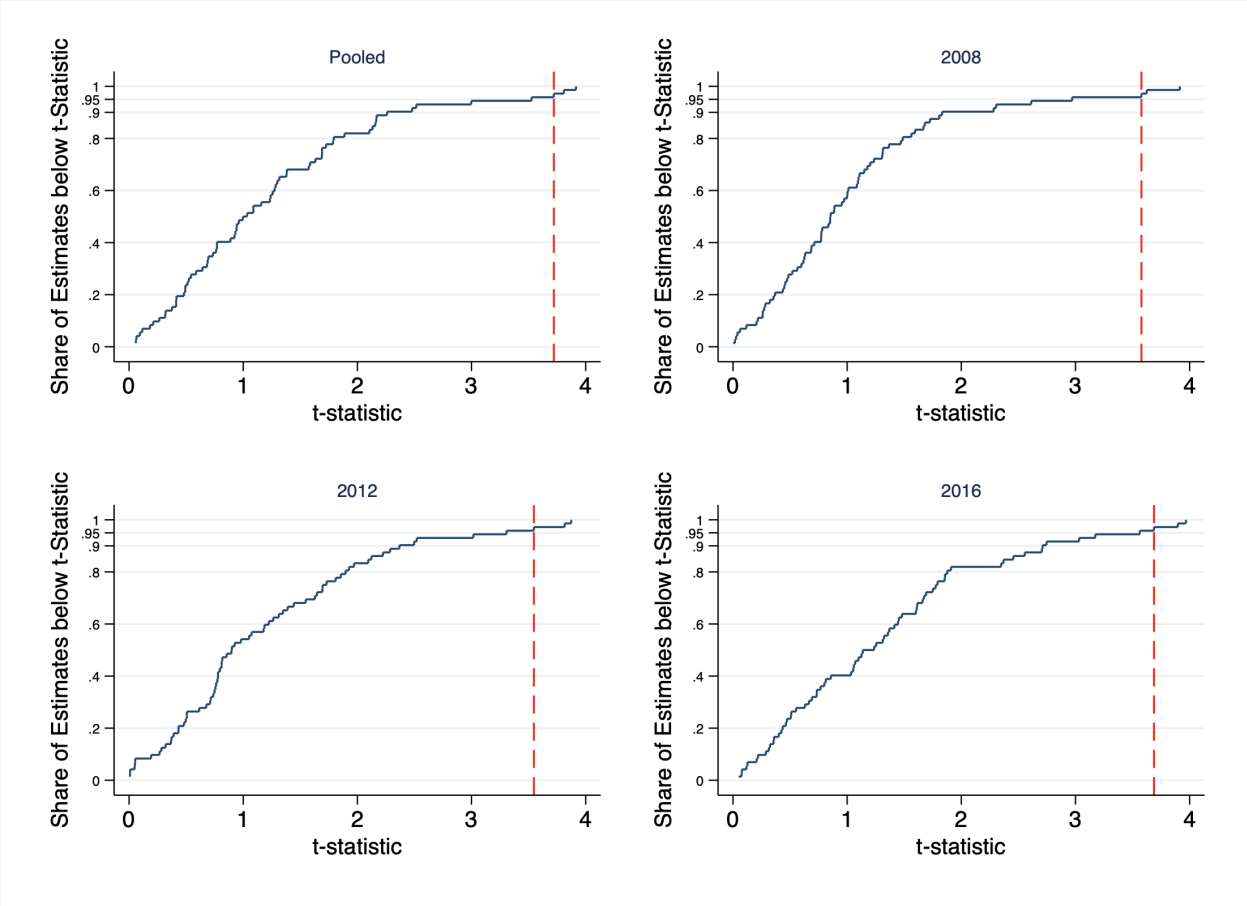

Figure 23: Presidential Falsification Test with Alternative Cutoffs (Cumulative Distribution)

Note: Each graph reflects the cumulative distribution of estimated t-statistics using the falsifcation test in Figure 22 for a particular election year. Red dashed lines denote the estimated t-statistic at the true threshold for the economic distress label.

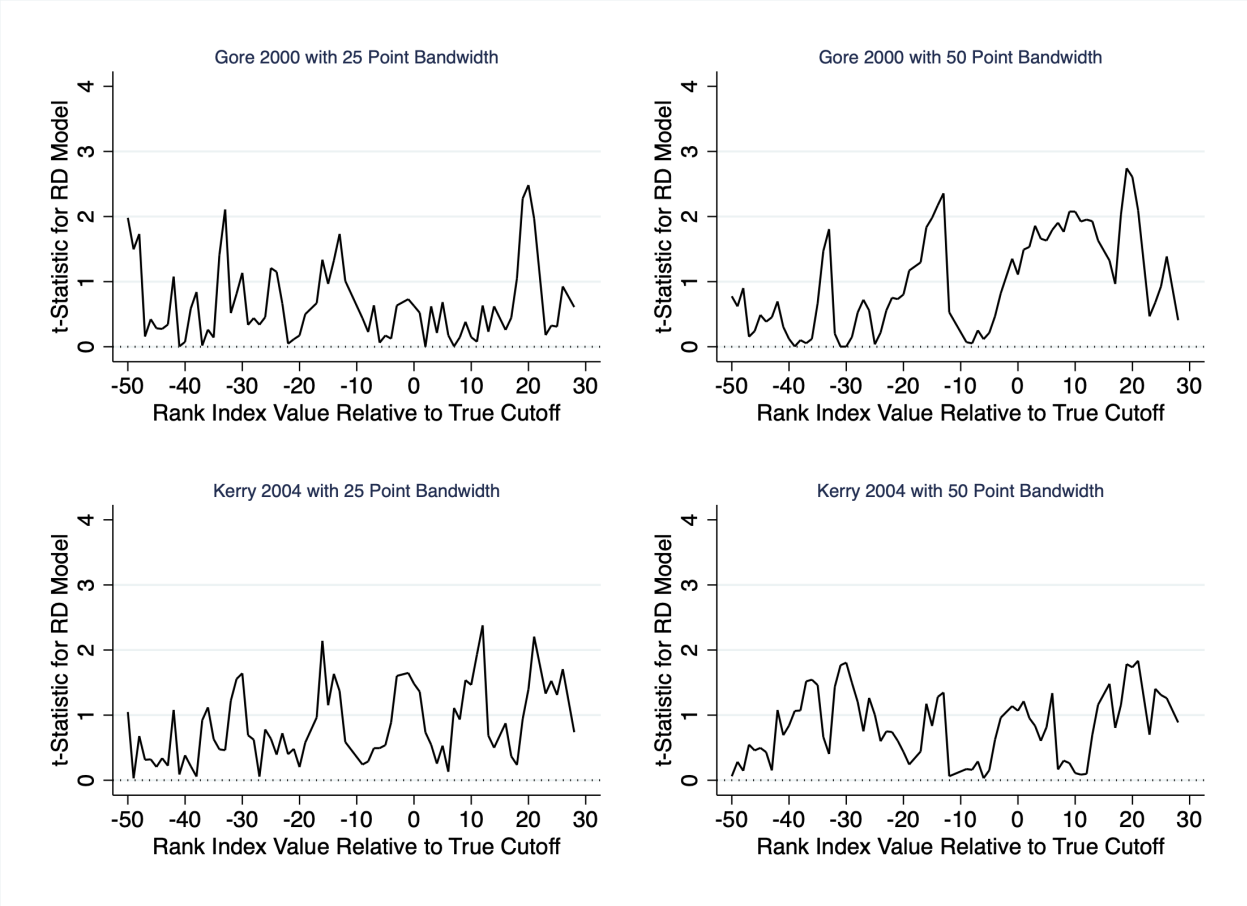

Figure 24: Placebo Presidential Falsification Tests with Alternative Cutoffs

Note: Each line reflects the estimated t-statistic of the impact of the economic distress label on a particular electoral outcome if the threshold were reassigned to a particular score along the rank index at a given bandwidth for a given pre-tretament year. Alternative cutoffs are indexed on the x-axis relative to the true cutoff value. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

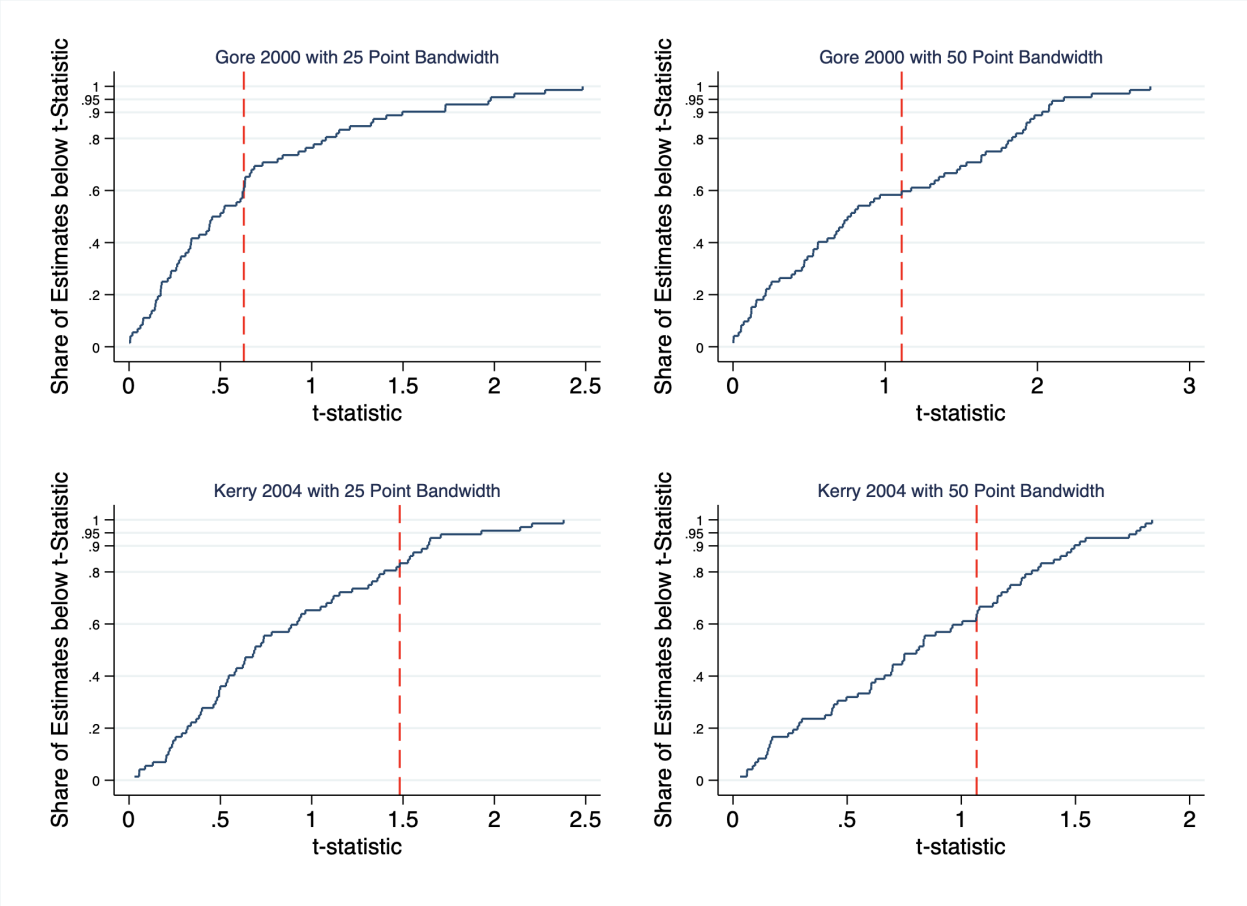

Figure 25: Placebo Presidential Falsification Tests with Alternative Cutoffs (Cumulative)

Note: Each graph reflects the cumulative distribution of estimated t-statistics using the falsifcation tests in Figure 24. Red dashed lines denote the estimated t-statistic at the true threshold for the economic distress label.

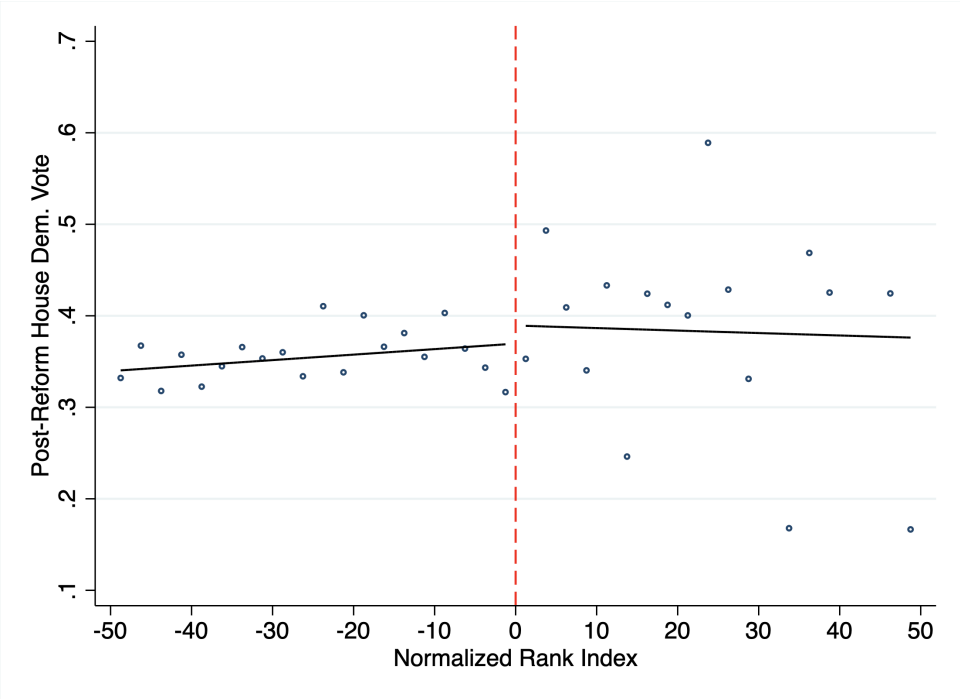

Figure 26: Discontinuity in Democratic House of Representatives Vote Share

Note: The figure displays the Democratic share of the two-party US House of Representatives vote in counties with at least one congressional race contested by members of both major parties. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 12: Distress Label Effects Interacted with Ancestral Democratic Vote

Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Distress	-0.064^{+}	0.044	-0.048	0.053	-0.088^{+}	0.022
	(0.039)	(0.032)	(0.044)	(0.037)	(0.048)	(0.043)
Distress × Ancestral	0.189*	-0.029	0.185*	-0.033	0.312**	0.046
	(0.086)	(0.065)	(0.085)	(0.066)	(0.110)	(0.084)
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial Order	1	1	2	2	1	1
Bandwidth Size	50	50	50	50	25	25
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-12,5]	[-12,5]
Sample Size	1,399	1,399	1,399	1,399	631	631

Note: Standard errors clustered on county in parentheses. Symbols denote $p < 0.1$ (+), $p < 0.05$ ($*$), and $p < 0.01$ ($**$). Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. Distress refers to the coefficient on the indicator for a county being assigned to distressed status. Distress × Ancestral denotes the coefficient on the interaction between the distress label and the Al Gore 2000 vote. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 13: Effects on Democratic Presidential Vote Share

Outcome Variable	(1)	(2)	(3)	(4)	(5)	(6)
A. Decomposed by County-Level 2000 Presidential Vote						
More Democratic	0.055*	0.029*	0.081*	0.046*	0.055	0.027+
	(0.024)	(0.013)	(0.037)	(0.019)	(0.034)	(0.014)
	[456]	[456]	[456]	[456]	[219]	[219]
More Republican	0.025+	0.026	0.035+	0.053*	0.044*	0.062*
	(0.015)	(0.016)	(0.021)	(0.024)	(0.018)	(0.024)
	[330]	[330]	[330]	[330]	[132]	[132]
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial Order	1	1	2	2	1	1
Bandwidth Size	50	50	50	50	25	25
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-12,5]	[-12,5]

Note: Standard errors clustered on county in parentheses. Sample size in brackets. Symbols denote $p < 0.1$ (+), $p < 0.05$ (∗), and $p < 0.01$ (∗∗). Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

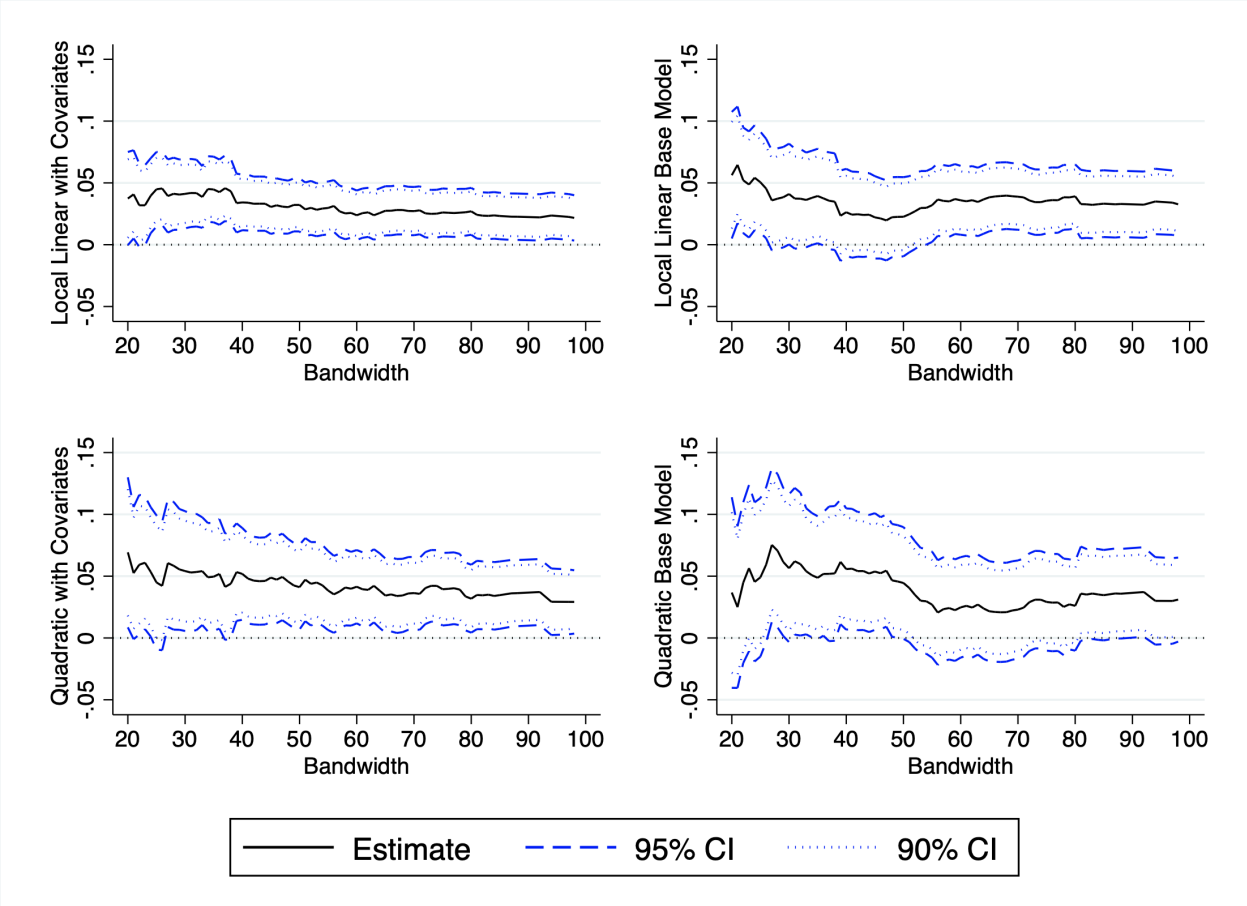

Figure 27: House Robustness to Bandwidth Selection Across Specifications

Note: Each panel reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the effect of the county economic distress label on the post-treatment Democratic share of the two-party House of Representatives vote in counties with contested races. Each panel represents a different specification. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

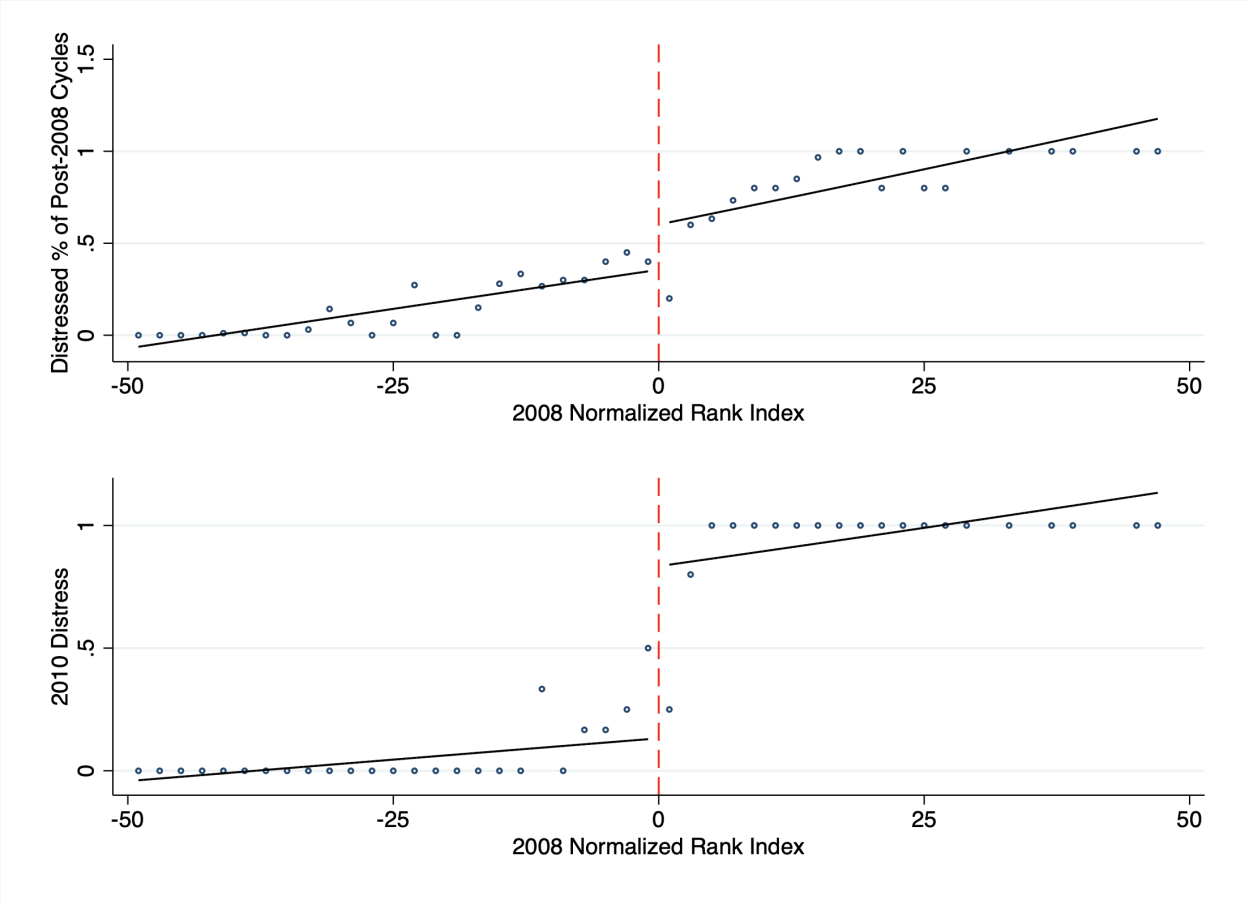

Figure 28: Persistence of Economic Distress Label

Note: The top panel highlights the share of post-2008 election cycles in which a county was labeled economically distressed sorted on the x-axis by their 2008 economic distress index value. The bottom panel highlights the fraction of counties which were labeled distressed in the 2010 election cycle sorted on the x-axis by their 2008 economic distress index value. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Appendix D: Mechanisms Supplement

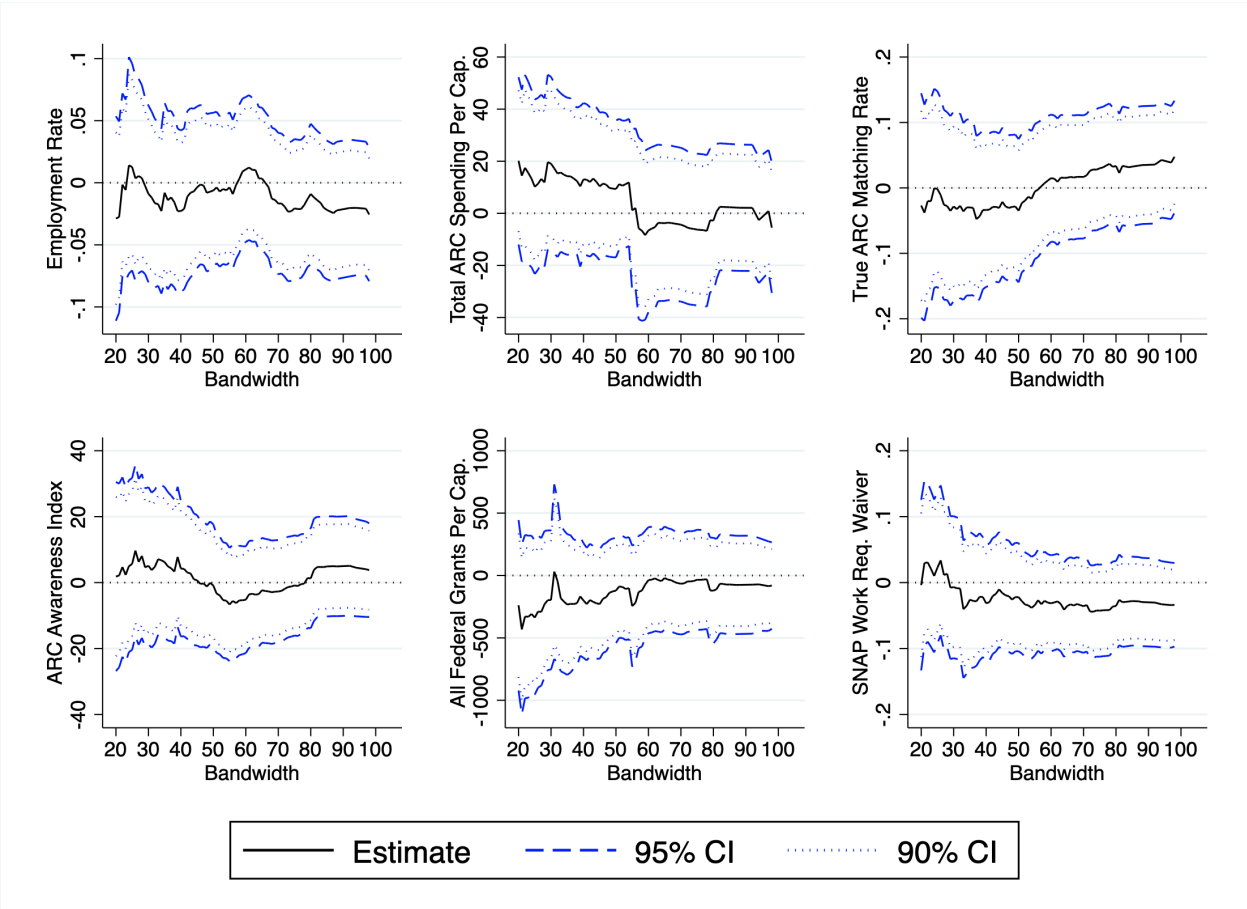

Figure 29: Robustness of Null Effects on Six Plausible Mechanisms across Bandwidth

Note: Each graph reflects the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the effect of the county economic distress label on respective intermediate outcomes. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

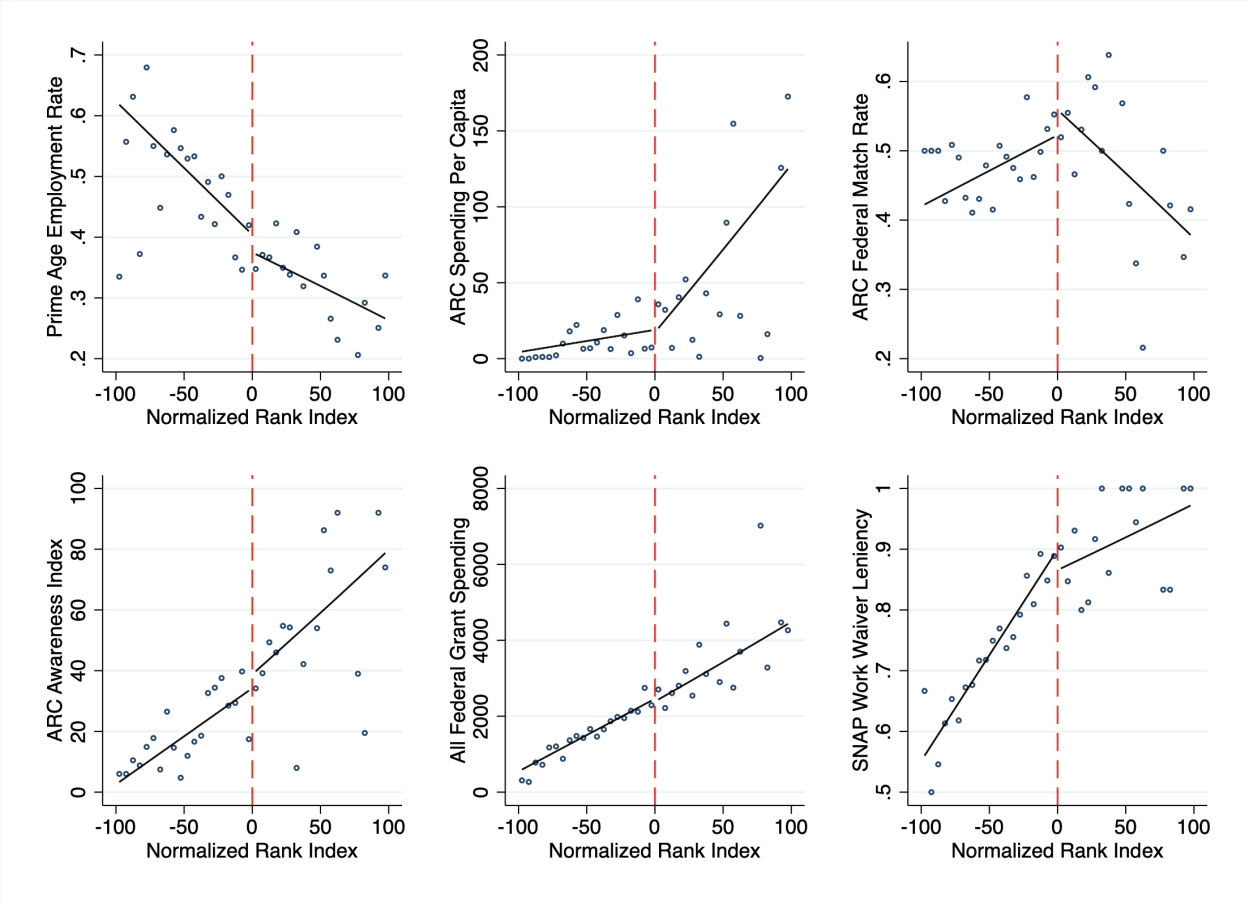

Figure 30: Regression Discontinuity Graphs of Six Plausible Mechanisms

Note: Rank index values are normalized from the raw composite index to the 90th percentile cutoff for distressed county status. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

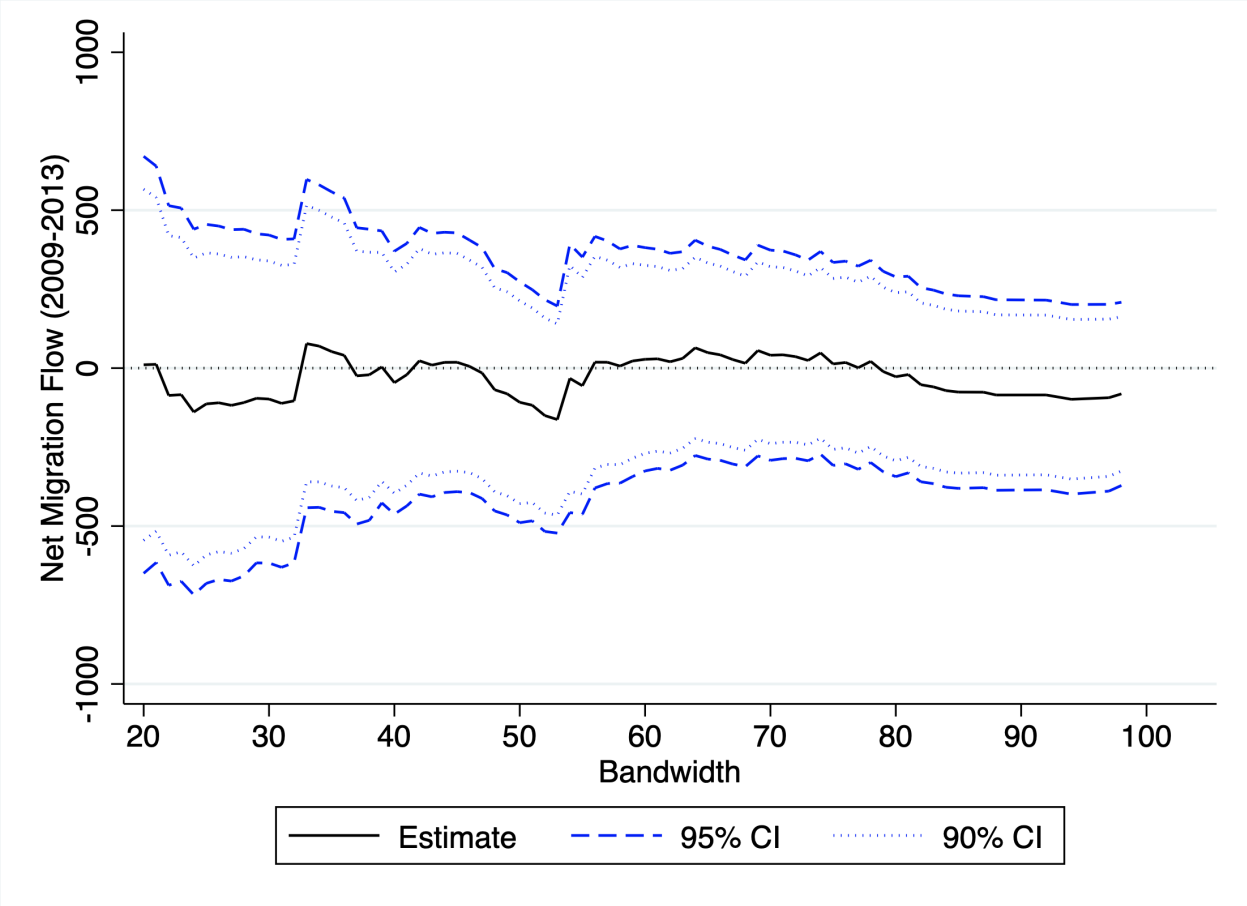

Figure 31: Robustness of Null Net Migration Flow to Bandwidth Selection

Note: The graph displays the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the effect of the county economic distress label on the 2009 to 2013 net migration flow among marginally distressed counties. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 14: Distress Label Effects on Plausible Alternative Mechanisms

Outcome Variable	(1)	(2)	(3)	(4)	(5)	(6)
A. Employment Rates						
Employment	-0.008	0.004	-0.013	-0.004	0.027	0.029
	(0.032)	(0.029)	(0.041)	(0.041)	(0.042)	(0.048)
B. ARC Total Funding Per Capita						
Total Funding	9.904	16.550	17.600	24.621	8.948	2.519
	(13.339)	(13.723)	(18.354)	(18.528)	(17.564)	(19.944)
C. ARC Funding Match Rate						
Match Rate	-0.034	-0.008	-0.008	0.003	-0.004	0.033
	(0.055)	(0.044)	(0.086)	(0.059)	(0.077)	(0.054)
D. ARC Awareness Index [a]						
Media Awareness	-0.455	3.812	4.543	6.809	3.432	4.620
	(9.428)	(8.143)	(14.004)	(13.097)	(13.247)	(11.021)
E. Total Federal Grants						
Total Grants	-108.742	25.542	-398.870	-250.594	-395.969	-229.102
	(210.185)	(169.789)	(333.228)	(247.773)	(309.531)	(235.789)
F. SNAP Work Requirement Waivers						
SNAP Waivers	-0.020	-0.027	-0.008	-0.027	0.008	-0.007
	(0.040)	(0.031)	(0.062)	(0.047)	(0.057)	(0.041)
G. Net County Migration 2009 to 2013						
Net Migration	-110.300	-153.028	213.875	69.283	-80.271	-178.282
	(192.867)	(138.539)	(325.236)	(227.224)	(284.268)	(203.520)
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial Order	1	1	2	2	1	1
Bandwidth Size	50	50	50	50	25	25
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-12,5]	[-12,5]
Unique Counties	262	262	262	262	117	117

Note: Standard errors clustered on county in parentheses. Symbols denote $p < 0.1$ (+), $p < 0.05$ (∗), and $p < 0.01$ (∗∗). Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. The superscript (a) denotes that the variable is measured at the media market level and, therefore, fixed effects for media market are excluded. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

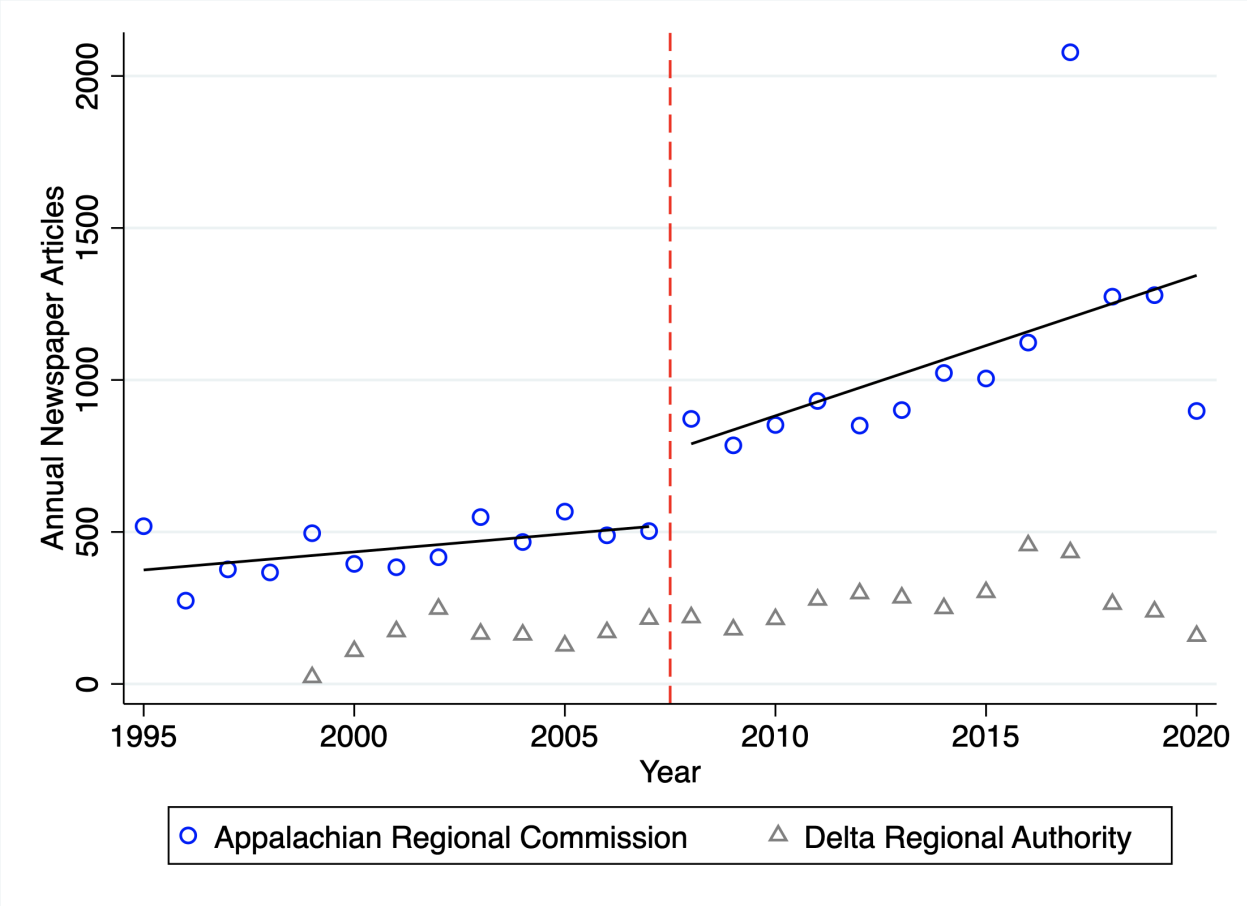

Figure 32: Newspaper Articles about the Appalachian Regional Commission over Time

Note: These data are from NewsBank Inc.s Access World News Research Collection: 2021 repository of American newspapers. Hollow circles and hollow triangles denote the raw number of articles in the newspaper database that include the precise phrase Appalachian Regional Commission (ARC) and Delta Regional Authority (DRA), respectively. The dashed vertical line denotes the timeframe of the ARCs reform to county classification. The DRA was created in 1999 and did not substantially reform its approach to county classification. News articles about the ARC spiked in 2017 when former President Trump proposed abolishing the commission in his first federal budget. Article counts for both the ARC and DRA in 2020 may be incomplete and impacted by the global Coronavirus Pandemic.

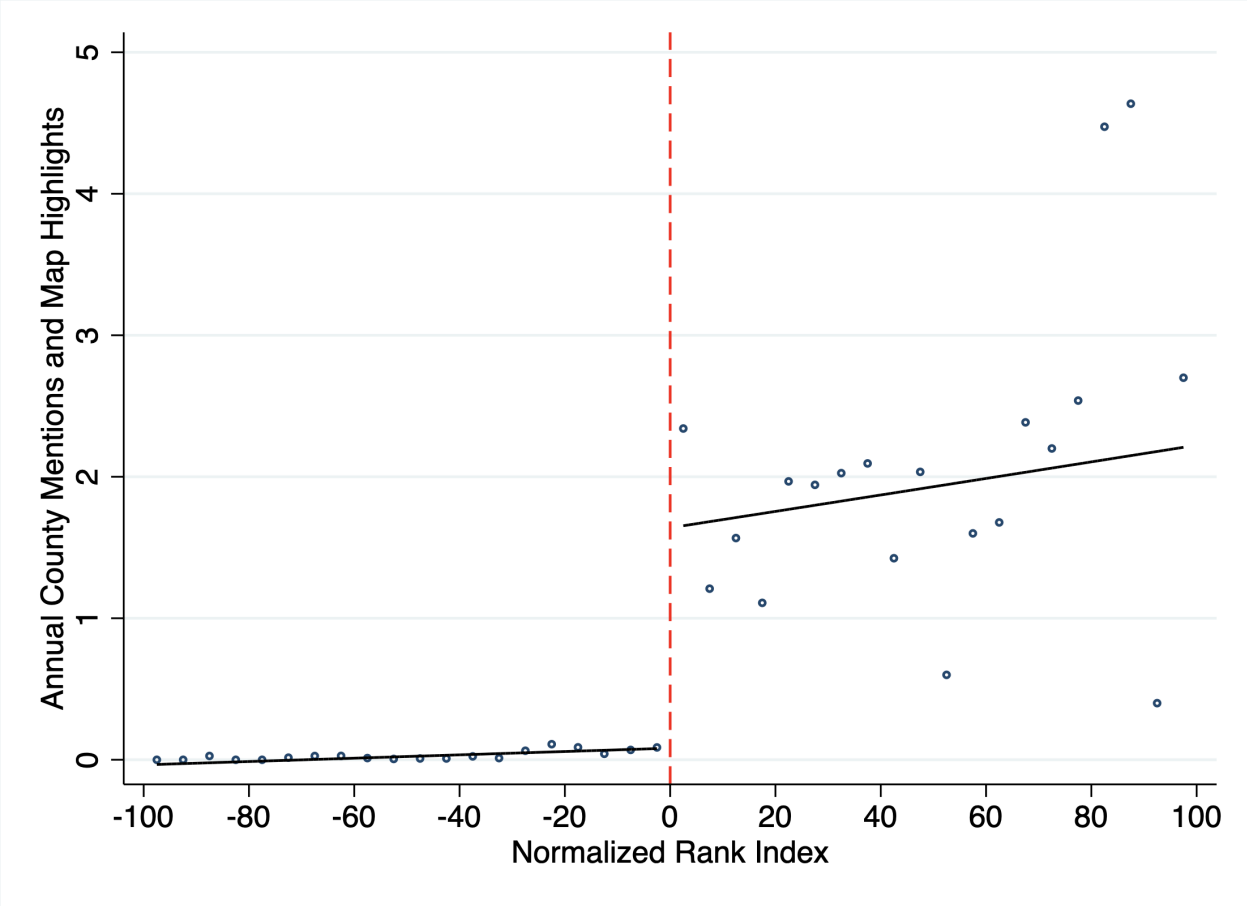

Figure 33: Frequency of County Mentions and Map Highlights in Google News Sample

Note: County mentions and highlights are tabulated in the time period of the most recently issued county classifications. Counties are recorded each time they are mentioned by name in a sentence that does not praise their economic conditions or trajectory. Counties are also recorded each time they are highlighted in a figure, table, or on a map in a way that emphasizes their economic deprivation relative to other counties. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

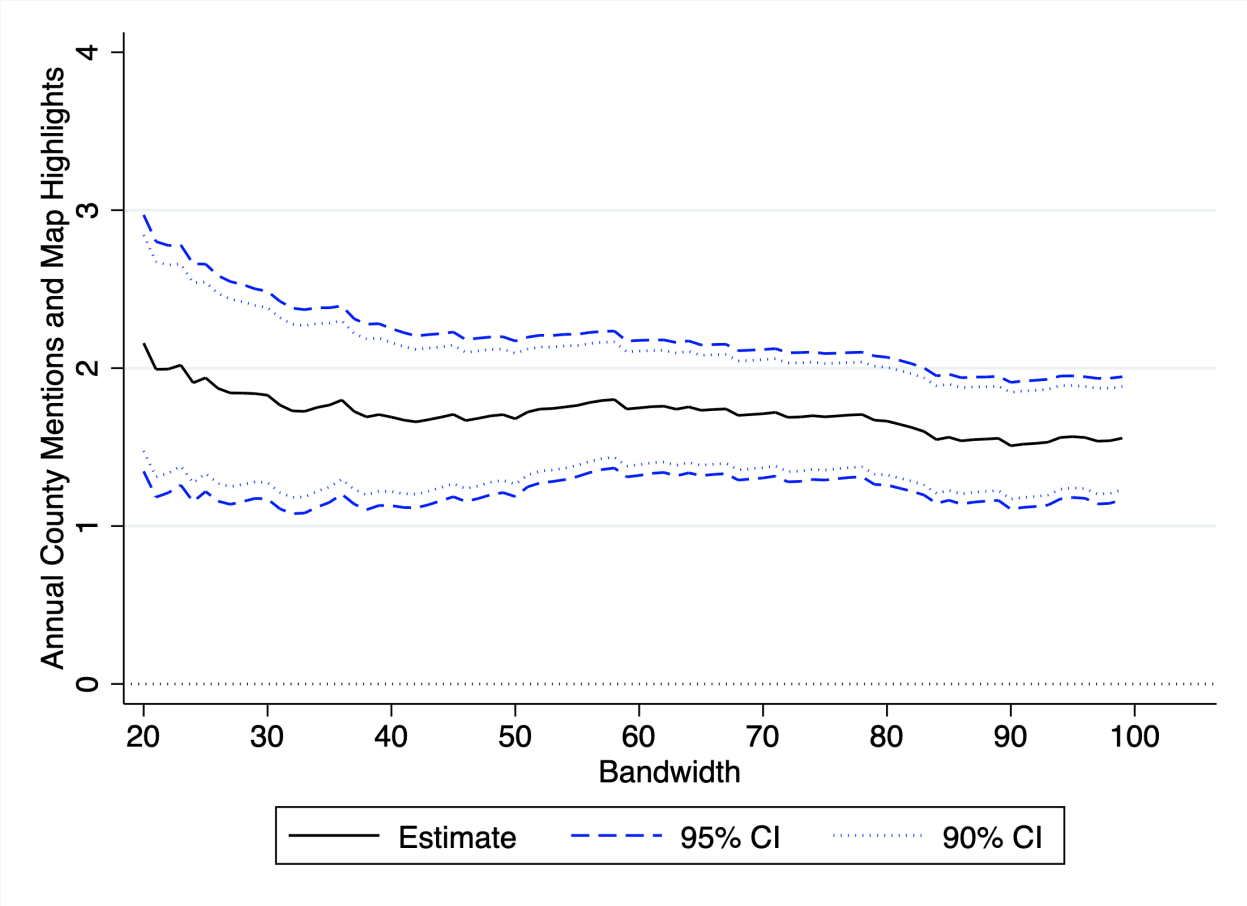

Figure 34: Robustness of Couny Mentions and Map Highlights to Bandwidth Selection

Note: The graph displays the point estimate, 95 percent confidence interval, and 90 percent confidence interval of the effect of the county economic distress label on the annual number of neutral or negative county mentions and map highlights at a respective bandwidth. Data are from my sample of Google News articles about county economic status. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

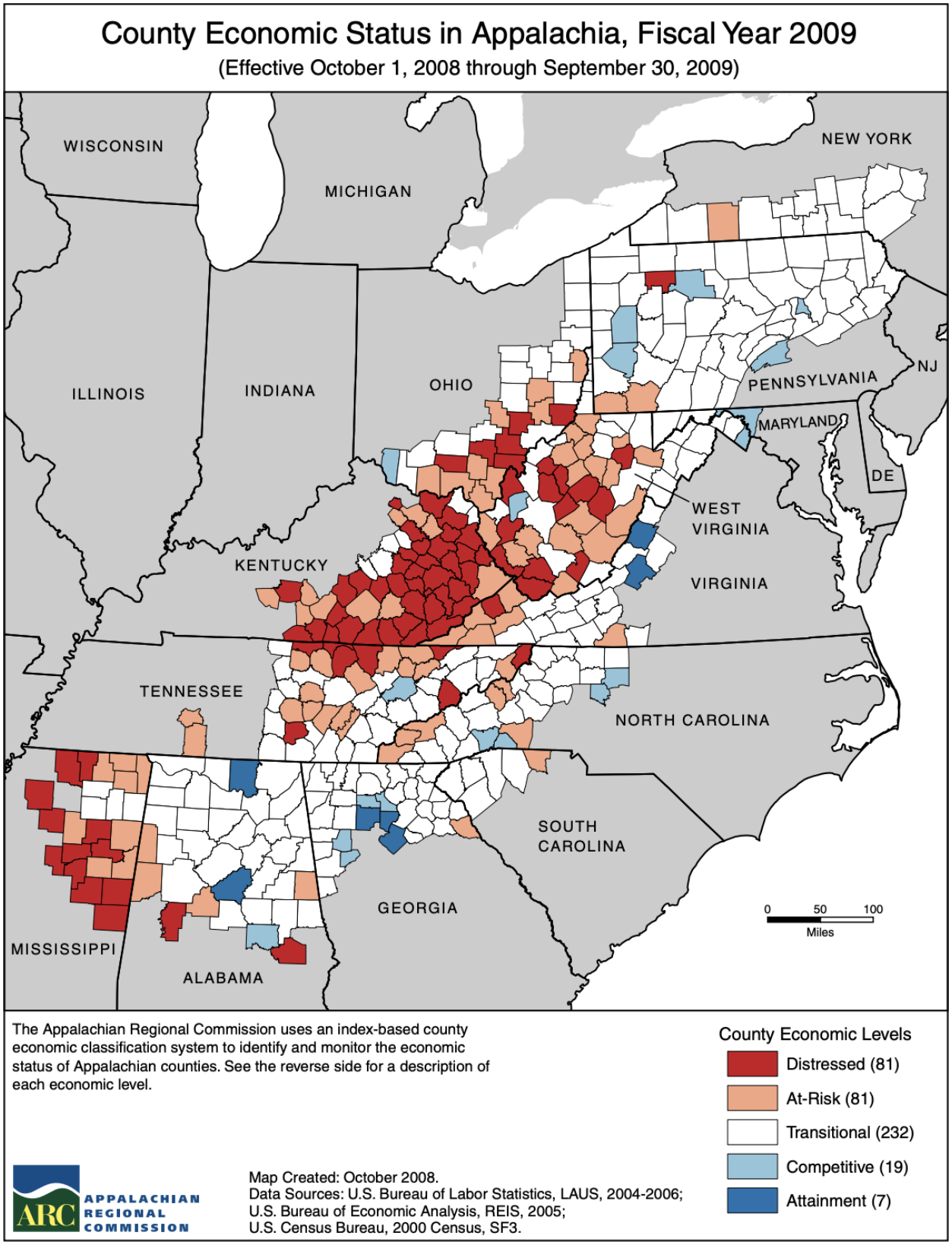

Figure 35: Sample County Economic Status Map

Note: Map retrieved from ARC.gov.

Table 15: Principal Component Analysis of Media Determinants

	Component 1	Component 2	Component 3
Peak Elevation	-0.4122	0.8894	0.1978
DTV Signal	0.6669	0.1466	0.7306
ISP Count	0.6208	0.4330	-0.6535
Proportion	0.4976	0.3048	0.1977

Note: Each column lists an eigenvector from the principal component analysis and the proportion of total variation explained by the component. Peak Elevation refers to the maximum mountain peak elevation within the county measured in feet above sea-level. DTV Signal refers to the total number of digital television channels available in the county seat with a moderate or strong signal as recorded by the FCC. ISP Count refers to the total number of internet service providers within the county as recorded by the FCC.

Table 16: Regression of Democratic Vote Share on Media Penetration Index

	(1) 2000	(2) 2004	(3) 2008	(4) 2012	(5) 2016	(6) Post-08
Media	0.021	0.023	0.041*	0.072**	0.082**	0.065**
	(0.018)	(0.017)	(0.018)	(0.018)	(0.020)	(0.018)
Polynomial Order	1	1	1	1	1	1
Bandwidth Size	25	25	25	25	25	25
Percentile Range	[-12,5]	[-12,5]	[-12,5]	[-12,5]	[-12,5]	[-12,5]
Unique Counties	117	117	117	117	117	117

Note: Standard errors clustered on county in parentheses. Symbols denote $p < 0.1$ (+), $p < 0.05$ (∗), and $p < 0.01$ (∗∗). Media refers to the coefficient on the media penetration index. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 17: Distress Label Effects on Presidential Vote with Media Penetration

Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Distress	0.051**	0.043**	0.072**	0.062**	0.068**	0.075**
	(0.016)	(0.012)	(0.025)	(0.019)	(0.024)	(0.017)
Distress × Media	0.050**	0.034**	0.050**	0.033**	0.058**	0.049**
	(0.012)	(0.011)	(0.013)	(0.011)	(0.017)	(0.015)
Media	0.015**	0.014*	0.015**	0.013*	0.022*	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.010)	(0.007)
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial Order	1	1	2	2	1	1
Bandwidth Size	50	50	50	50	25	25
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-12,5]	[-12,5]
Sample Size	786	786	786	786	351	351

Note: Standard errors clustered on county in parentheses. Symbols denote $p < 0.1$ (+), $p < 0.05$ ($*$), and $p < 0.01$ ($**$). Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. Distress refers to the coefficient on the indicator for a county being assigned to distressed status. Media refers to the coefficient on the media penetration index. Distress × Media denotes the coefficient on the interaction between the two aforementioned variables. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 18: Distress Label Effects on Presidential Vote with Media Penetration

Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Distress	0.045**	0.040**	0.068**	0.058**	0.061**	0.070**
	(0.015)	(0.012)	(0.024)	(0.018)	(0.022)	(0.016)
Distress × Media	0.054**	0.034**	0.053**	0.034**	0.060**	0.049**
	(0.012)	(0.011)	(0.013)	(0.011)	(0.016)	(0.015)
Media	0.013**	0.015**	0.014**	0.014*	0.020*	0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.010)	(0.007)
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial Order	1	1	2	2	1	1
Bandwidth Size	50	50	50	50	25	25
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-12,5]	[-12,5]
Sample Size	786	786	786	786	351	351

Note: Standard errors clustered on county in parentheses. Symbols denote $p < 0.1$ (+), $p < 0.05$ (∗), and $p < 0.01$ (∗∗). Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. Distress refers to the coefficient on the indicator for a county being assigned to distressed status. Media refers to the coefficient on the media penetration index. Distress × Media denotes the coefficient on the interaction between the two aforementioned variables. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 19: Distress Label Effects on House Vote with Media Penetration

Outcome Variable	(1)	(2)	(3)	(4)	(5)	(6)
Distress	0.033^{+}	0.034^{**}	0.058^{*}	0.043^{*}	0.072^{**}	0.053^{**}
	(0.017)	(0.012)	(0.024)	(0.018)	(0.023)	(0.017)
Distress × Media	0.042^{**}	0.008	0.042^{**}	0.008	0.065^{**}	0.017
	(0.013)	(0.010)	(0.013)	(0.010)	(0.019)	(0.015)
Media	-0.002	-0.001	-0.001	-0.001	-0.010	-0.015
	(0.006)	(0.005)	(0.006)	(0.005)	(0.013)	(0.009)
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	–	Yes	–	Yes	–	Yes
Fixed Effects	–	Yes	–	Yes	–	Yes
Polynomial Order	1	1	2	2	1	1
Bandwidth Size	50	50	50	50	25	25
Percentile Range	[-35,7]	[-35,7]	[-35,7]	[-35,7]	[-12,5]	[-12,5]
Sample Size	1,399	1,399	1,399	1,399	631	631

Note: Standard errors clustered on county in parentheses. Symbols denote $p < 0.1$ (+), $p < 0.05$ ($*$), and $p < 0.01$ ($**$). Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. Fixed effects refer to election year, media markets, and states. Distress refers to the coefficient on the indicator for a county being assigned to distressed status. Media refers to the coefficient on the media penetration index. Distress × Media denotes the coefficient on the interaction between the two aforementioned variables. Percentile range refers to the equivalent percentile rank bandwidths on either side of the discontinuity. Tables 7 and 8 in Appendix A.1 provide a clear mapping between the raw disadvantage score and the percentile rank of counties.

Table 20: Media Penetration Index Interaction Term Balance Tests

	(1) HS Grad	(2) College	(3) Unemp.	(4) Poverty	(5) Income	(6) Density	(7) Over 65
Distress	3.823	-0.813	3.776	-9.692	5.869	-1.727	0.011
	(3.402)	(8.098)	(11.077)	(11.250)	(6.285)	(14.055)	(0.010)
Distress × Media	1.156	3.977	-5.954	10.106	-4.235	-11.325	0.005
	(1.924)	(4.736)	(6.897)	(6.148)	(3.680)	(8.780)	(0.006)
Media	1.919	4.294	10.765**	-3.766	-6.958**	10.739	-0.012**
	(1.201)	(3.656)	(2.897)	(3.704)	(2.513)	(8.351)	(0.004)
Polynomial Order	1	1	1	1	1	1	1
Bandwidth Size	25	25	25	25	25	25	25
Percentile Range	[-12,5]	[-12,5]	[-12,5]	[-12,5]	[-12,5]	[-12,5]	[-12,5]
Sample Size	351	351	351	351	351	351	351

Note: Standard errors clustered on county in parentheses. Symbols denote $p < 0.1$ (+), $p < 0.05$ (∗), and $p < 0.01$ (∗∗). Distress refers to the coefficient on the indicator for a county being assigned to distressed status. Media refers to the coefficient on the media penetration index. Distress × Media denotes the coefficient on the interaction between the two aforementioned variables. The labels under each column number refer to the variables against which I test the interaction term for balance. Sequentially, these variables are high school graduation rate, college graduation rate, unemployment rate, poverty rate, per capita market income, population density per square mile, and the share of the population aged 65 or older.

Table 21: Robustness of Table 17 Results to Competing Interaction Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. 25 Point Bandwidth without Fixed Effects or Demographics (N=351)							
Distress × Media	0.061**	0.055**	0.058**	0.059**	0.066**	0.058**	0.055**
	(0.014)	(0.014)	(0.019)	(0.017)	(0.018)	(0.016)	(0.016)
B. 25 Point Bandwidth with Fixed Effects and Demographics (N=351)							
Distress × Media	0.056**	0.052**	0.045**	0.048**	0.053**	0.051**	0.051**
	(0.015)	(0.015)	(0.015)	(0.014)	(0.018)	(0.015)	(0.015)
C. 50 Point Bandwidth without Fixed Effects or Demographics (N=786)							
Distress × Media	0.052**	0.048**	0.049**	0.051**	0.064**	0.054**	0.056**
	(0.011)	(0.011)	(0.013)	(0.013)	(0.014)	(0.012)	(0.012)
D. 50 Point Bandwidth with Fixed Effects and Demographics (N=786)							
Distress × Media	0.036**	0.034**	0.035**	0.034**	0.043**	0.037**	0.036**
	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)	(0.010)	(0.011)
Polynomial Order	1	1	1	1	1	1	1
Lag Vote Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for Media	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for Distress	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2nd Interaction	HS Grad	College	Unemp.	Poverty	Income	Density	Over 65

Note: Standard errors clustered on county in parentheses. Symbols denote $p < 0.1$ (+), $p < 0.05$ (∗), and $p < 0.01$ (∗∗). Lag Vote Controls refer to 2004 election results. Demographics refer to previous unemployment rates, poverty rates, and per capita market income. Fixed effects refer to election year, media markets, and states. Distress refers to the coefficient on the indicator for a county being assigned to distressed status. Media refers to the coefficient on the media penetration index. Distress × Media denotes the coefficient on the interaction between the two aforementioned variables. All specifications include an indicator for distress and a control for the media penetration index. The 2nd Interaction row refers to the competing interaction effect included in each specification, which are high school graduation rate, college graduation rate, unemployment rate, poverty rate, per capita market income, population density per square mile, and the share of the population aged 65 or older. Controls for each of these variables are also included in the respective specifications.