Reaching the Unreached: Radio, Voting, and Representation in Interwar America

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July 4, 2026

Abstract

A new medium's political effect can turn not on its content but on its audience: on how much of the electorate the prior dominant medium excluded, and on the political misalignment that exclusion preserved. Interwar radio is a clean test: its audio format bypassed the literacy barrier that kept print political news from millions. Using geographic determinants of AM reception as instruments for county radio ownership, I find that a 10-percentage-point rise in ownership raised within-state Democratic presidential vote share by 2.5 points in the non-South, most where illiteracy was highest, by eroding inherited Republican support in the average county; the effect vanishes in the one-party South. Where the electorate moved, representation moved with it: high-illiteracy districts' Democratic seats passed to more liberal Democrats through within-party replacement. Information poverty preserves misalignment between inherited loyalty and material interest; a medium that breaks the access barrier releases what exclusion accumulated.

Keywords: media and politics, information access, electoral accountability, political representation, radio, interwar America

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1 Introduction

Before radio, political news in the United States traveled almost exclusively through print. For the millions of adults who could not read, that meant little independent political news beyond what rallies, canvassers, and neighbors carried by word of mouth. Illiteracy exceeded 10% in many states in the 1920s and 1930s, and rural communities often lacked daily newspapers. For these voters the cost of sustained, unmediated political information — which candidates were running, how their representatives had voted — was effectively prohibitive. The information environment was structured around print, and print required literacy.

Commercial radio broadcasting, emerging in 1920, changed this. Radio offered political news as spoken word, accessible regardless of literacy, and within two decades ownership rose from essentially zero to nearly 70% of American households. For voters excluded from print media, radio was not merely a new channel but the first.

Democratic accountability depends on voters' ability to monitor their representatives (Arnold, 2004; Ashworth, 2012), and monitoring presupposes access: whether political news reaches a voter at all is prior to what that news says. Yet most accounts locate a medium's political effect in its *content* — the slant, framing, or tone of what audiences receive (DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Chong and Druckman, 2007; Chiang and Knight, 2011) — or in the arrival of a new channel among publics the older media already reached (Gentzkow, 2006; Adena et al., 2015). The effect can instead be governed by the medium's *audience*: by whom it newly reaches, and by what the missing information does when it finally arrives. Interwar radio (1920–1942) is the rare setting in which this determinant operates in nearly pure form: its audio format carried the same national politics into every county, but its marginal contribution was largest where literacy had barred print.

Radio's political effect, I argue, operated by reducing *information poverty* — the exclusion of voters from political news by literacy barriers — so its informational gain was largest in low-literacy communities. When these previously unreached voters gained access, their electoral behavior changed — and where it changed most, the representatives their districts sent to Congress changed with them.

The argument turns on what information poverty does while it lasts: it *preserves misalignment*. Where voters cannot cheaply follow platforms and legislative records, partisanship inherited from family and community persists on habit even after the party stops serving the material interests beneath it — the information that would unsettle the loyalty cannot arrive. Exclusion therefore accumulates a stock of misaligned partisanship, and a new medium that bypasses the barrier releases the stock. In the rural non-South the deficit and the misalignment overlapped: Republican loyalties inherited from the Civil War persisted by habit even as the party's 1920s economic policy abandoned its farm base. Democratic voters sat in that overlap far less often, so the framework predicts a pro-Democratic disruption — not because radio favored Democrats, but because the misaligned, uninformed pool was concentrated among habitual Republicans.

The evidence bears this out. Using geographic determinants of AM reception — ground conductivity and woodland share, after Stromberg (2004) — as instruments for radio adoption, I find a 10-percentage-point increase in radio ownership raised non-South Democratic presidential vote share by 2.5 points, while in the South the effect collapses to near zero. Radio also depressed within-state turnout in the average county, reversing the positive association in prior county-level estimates. And the vote shift carries the central clue to the mechanism: Democratic gains came from declining Republican votes, not growing Democratic ones.

Representation registered the shift as well, in the bounded form the scope condition lets me test: in the non-South's high-illiteracy districts, Democratic seats passed to more liberal Democrats — electoral selection through within-party replacement, with no evidence of partisan seat capture or of incumbent conversion. These estimates rest on the assumption that, within state and year, reception geography moves electoral change only through radio adoption; I probe it directly with pre-radio placebos, single-instrument estimates, and New Deal and farm-distress controls, reporting where the probes fall short.

Two further tests separate the account from its rivals. First, radio's effect on Democratic vote share scales with county illiteracy in the non-South, yet within newspaper-tracked counties it does not vary with local newspaper density (Gentzkow et al., 2011): a content-based mechanism would not generate the illiteracy gradient, and a print-substitution mechanism would not leave the effect indifferent to newspaper supply. Second, the effect concentrates in the FDR era, inviting a simpler reading: FDR's own broadcasts, not information access, moved voters; but his broadcasts reached literate and illiterate listeners alike, so that channel predicts no literacy moderation, precisely the pattern it cannot explain. The Southern null, by contrast, tests the account against its own scope condition: information poverty was if anything more severe there, but one-party Democratic dominance left too few Republican voters for an information-driven disruption to register — the deficit without the pool.

Two studies of the same medium and era anchor the comparison. Wang (2021) finds Father Coughlin's broadcasts cost Roosevelt votes in 1936, a same-era effect opposite to mine, but the two estimate different things: Wang isolates one broadcaster's content, while my design recovers the average effect of radio *access*. And the founding panel study of radio-era campaigning, Lazarsfeld et al.'s (1944) Erie County survey, found radio mostly reinforced existing preferences. The literacy gradient reconciles my larger effects with that canon: Lazarsfeld studied a literate, media-saturated county, exactly where the framework predicts the minimal effects he found.

Three contributions follow. First, a new medium's political effect can turn on its audience and the misalignment its arrival exposes, not on its content alone: radio carried the same national politics everywhere, yet its effect concentrates where print had not reached. Whether the newly reached learned that their inherited party no longer served them or were persuaded by the broadcasts, both channels run through the same gate —

prior exclusion and the misalignment it preserved — which separates this account from content-based mechanisms operating on publics the media already reached (DellaVigna and Kaplan, 2007; Enikolopov et al., 2011). Second, I trace the electoral shift into the legislature and bound its channel: high-illiteracy non-South districts came to be represented by more liberal Democrats through within-party candidate replacement — electoral selection, with no evidence of seat capture or incumbent conversion. Third, I revise the received empirical picture: the turnout gains attributed to radio in the founding county-level study (Stromberg, 2004) reflect between-state differences, not radio; within states, in the average county, radio demobilized more voters than it mobilized — person-weighted, the same shock reads as urban Democratic mobilization, two faces of one realignment.

The case is a century old, but the question it isolates is current. Every information technology reaches some citizens before others, and which dimension of access is uneven follows from the technology's distinctive affordance: radio's audio format made *who could reach* political news the binding margin, much as the digital divide makes broadband connectivity one today (Norris, 2001).

2 Theory: Information Poverty and Electoral Politics

2.1 Information Poverty and Democratic Accountability

Effective representation depends on informed citizens: voters need political information to identify their representatives, connect policy to their interests, and decide whether incumbents merit retention (Arnold, 2004; Ashworth, 2012). When they lack it, the electoral connection weakens unequally: uninformed voters can neither select well nor sanction poor performance, so responsiveness tilts toward the informed — information determines not only whether representation works but whose preferences it tracks.

Both halves of this relationship appear across diverse settings: U.S. legislators whose districts have fuller local newspaper coverage vote more consistently with constituents and secure more federal spending (Snyder and Stromberg, 2010; Myers, 2025), and in developing democracies newspaper circulation, press campaigns, and broadcast audits strengthen responsiveness and accountability (Besley and Burgess, 2002; Reinikka and Svensson, 2005; Ferraz and Finan, 2008). These study how variation in the information *environment* translates into outcomes; the interwar United States poses a question one step earlier: what happens when the dominant medium itself excludes millions through the literacy barrier, and a new medium then bypasses that exclusion?

That access is prior to content is not merely definitional; two established models explain why. Zaller's reception-and-acceptance framework (Zaller, 1992) makes reception the necessary first stage of any media effect: persuasion, framing, and slant operate only on citizens who take the message in. Zaller measured reception through political awareness; the interwar case pushes the constraint back one step, to whether the medium was usable at all — for a voter who could not read, a printed message was never received, however attentive the reader. The knowledge-gap tradition (Tichenor et al., 1970) traced unequal *learning* from mass media to the literacy-linked skills print rewards, so that rising print flow widened knowledge gaps by favoring those already equipped to use it; a medium that removes the literacy requirement rather than rewarding it should work in reverse. Both models predict radio's effect should be largest where prior access was lowest.

The exclusion was stark: by the 1930 Census, illiteracy exceeded 10% in Southern states and 16% among African Americans nationally — millions of voting-age adults who could not read a newspaper, a campaign flyer, or a ballot. Newspaper supply varied widely across the interwar United States (Gentzkow et al., 2011), but even abundant newspapers could not close this gap: additional circulation meant more information for literate citizens and none for illiterate ones. Literacy, not circulation intensity, was the binding constraint on the print-information environment, the constraint radio's audio format was positioned to relax.

By "information poverty" I mean systematic exclusion from print political news due to literacy, adapting Norris (2001)'s term for unequal access to the internet's civic resources, with literacy rather than connectivity as the barrier. So defined, it is a property of *voters*, a literacy-imposed barrier to one medium, not of the local information *environment* (the abundance of news outlets); this is the distinction the newspaper-density test in Section 5 turns on. The framework predicts newly informed voters will change their behavior and representatives will change theirs in response.

2.2 Radio and the 1920s Political Order

Radio's political significance lies in reducing information poverty: its audio format removed the literacy precondition for political news. For literate voters already well-served by print it was complementary; for illiterate voters it was transformative, reaching households that had never subscribed to a newspaper.

Commercial broadcasting began in 1920; by the late 1920s NBC and CBS carried campaign speeches, convention coverage, and policy debates nationwide, and from 1933 Roosevelt's Fireside Chats reached tens of millions at once. By the mid-1930s roughly 60% of U.S. households owned a radio, climbing toward 90% by the early 1940s (Craig, 2000); the county mean in my sample reaches 69% by 1940. This content was predominantly national, presidential politics and policy rather than local or congressional races.

Whether the poorest of the newly reachable audience could afford a set is a fair question; early receivers were costly and adoption skewed urban and affluent. But listening was communal in ways print was not — neighbors, stores, and churches shared sets, so household ownership understates exposure among the poor (Craig, 2000) — and the affordability constraint collapsed over the study window: cheap AC table sets sold for under $20 by the mid-1930s, and farm penetration converged toward urban rates in precisely the FDR-era years where the effects concentrate. Within-county selection into ownership remains unobservable in a county-level design; these features bound that concern rather than removing it.

The county-level implication is direct: radio's electoral effects should concentrate in high-illiteracy areas, where its marginal contribution to the information environment was largest.

The framework does not by itself predict the *direction* of the shift; that depends on a historical misalignment between informationally poor voters' partisan behavior and their economic interests, one that information poverty itself helped sustain.

By the 1920s the Republican Party's dominance in the rural North rested not on policy but on identity. The Civil War created deeply localized partisan cultures — communities that had sent men to fight for the Union became reliably Republican, a loyalty transmitted through family, veterans' organizations, and small-town life; county-level Union war deaths predicted Republican vote shares through at least 1912 (Kalmoe, 2020). The 1896 realignment reinforced rather than replaced this inheritance: Northern farmers, more commercial and less indebted than their Southern and Plains counterparts, rejected Bryan's free silver for the Republican promise of sound money (Burnham, 1970; Sundquist, 1983). By the 1920s that rationale had decayed while the habit persisted.

This inherited partisanship had become a profound misalignment between partisan behavior and economic interest. For farm communities the crisis began not with the 1929 crash but a decade earlier: agricultural income collapsed by roughly two-fifths between 1919 and 1921 and never recovered during the Republican ascendancy (Saloutos, 1982). Heavily mortgaged farmers were acutely vulnerable to the tight money Coolidge and Hoover favored, and Coolidge twice vetoed the McNary-Haugen farm relief bill (1927, 1928); Republican economic policy, in its particulars, transferred wealth from agriculture to industry.

The grievance was loudly voiced — through the Farm Bloc, the Nonpartisan League, and La Follette's 1924 Progressive candidacy — but largely *within* the Republican Party, whose own senators sponsored the farm relief bills (Saloutos and Hicks, 1951; Hansen, 1991). What information poverty sustained was therefore not ignorance of grievance but a failure of partisan translation: recognizing that the *opposing* party might better serve one's interests required following platforms and legislative records that traveled almost entirely in print. That cost fell least on the literate farm belt and hardest on the non-South's low-literacy populations, where habitual Republican voting persisted by default.

Radio lowered this translation cost: for the first time a mass medium carried political debate in spoken language requiring no literacy. Broadcasters were cautious about overtly partisan content outside campaign windows (Craig, 2000), so the payload traveled through campaign broadcasts, convention coverage, and presidential addresses; the *National Farm and Home Hour*, a USDA–NBC broadcast aired daily from 1928, carried farm-policy information into households print had never reached, and after 1933 explained the new programs to their intended beneficiaries (Craig, 2000). For voters whose Republican partisanship was inherited rather than chosen, radio made the gap between that inheritance and the national alternatives newly legible.

The voters most affected sat at the intersection of informational and economic precarity, and the two margins of my design map onto different parts of that population. The literacy *gradient* is identified off the non-South counties where illiteracy concentrated: the Border South and Appalachia, the Hispanic Southwest, and immigrant industrial counties. The misaligned farm belt of the rural North was, by contrast, among the most literate and print-saturated populations; its response ran through radio's immediacy rather than the literacy bypass, loading on the *level* effect rather than the interaction.

What the newly reached heard depended on when they listened — and the timing is itself a prediction. Through most of the 1920s no aligned alternative existed: the Democratic Party of John W. Davis and Al Smith offered Eastern, urban candidacies with no programmatic answer to farm distress. The aligned alternative arrived with Roosevelt's 1932 campaign and the New Deal — farm price supports, labor protections, banking regulation, relief spending (Leuchtenburg, 1963; Cohen, 1990) — and radio's role was to make it audible to voters print could not reach. Because the mechanism requires information *and* an aligned alternative, and the alternative arrived only in 1932, the framework predicts that radio's pro-Democratic effect should *concentrate* in the FDR era rather than appear uniformly across the 1920s, a prediction the era split in Table 7 bears out.

The same account identifies where radio's effect should *not* appear: observable electoral change requires a population whose voting behavior can shift in response to new information, a scope condition the interwar South fails on two distinct margins.

First, among white voters the partisan structure left no population susceptible to disruption. The few white Republicans clustered in Appalachian mountain counties where Unionist Civil War heritage sustained a minority tradition; even if radio informed them, the pool was too small to move aggregates. The white Democratic majority was bound by regional and racial identity forged in the Civil War and Reconstruction: Democratic presidential vote share already averaged 0.77 across Southern counties, and where economic policy entered, especially under the New Deal, it reinforced that allegiance. For these voters radio revealed no misalignment.

Second, for Black Southerners, a large share of the region's informationally poor, Jim Crow disenfranchisement severed the link between information and voting: poll taxes, discriminatory literacy tests, white-only primaries, and the threat of violence (Key, 1949; Katznelson, 2013) ensured that even informed listeners could not act at the ballot box.

Together, these conditions generate a graduated rather than binary prediction: radio's effect should scale with the size of the local misaligned pool. Counties with substantial Republican minorities should show the largest Democratic gains; those with near-zero Republican vote shares, near-zero effects regardless of information poverty. The Southern null should therefore appear as a floor effect across all outcomes, not a selective failure of the information channel.

2.3 Behavioral Predictions and Competing Mechanisms

An information shock can change an individual voter's behavior along three margins, each with distinct aggregate implications. *Switching*: existing voters change allegiance, learning that the opposing party's platform better aligns with their economic interests than the party they had supported out of habit or inherited identity. *Demobilization*: existing voters withdraw, when new information creates dissonance with their partisan attachment but the costs of switching are too high. *Mobilization*: previous non-voters, now informed enough to form a preference, enter the electorate.

In the interwar context the shock was asymmetric: radio made visible the misalignment between Republican economic policy and low-literacy voters' interests while largely confirming Democratic voters' existing alignment. Switching and demobilization should therefore fall disproportionately on Republicans, while Democratic-side inflows (mobilization, switching) could be partly offset by the departure of right-leaning habitual Democrats under similar pressure. The aggregate prediction follows: an unambiguous decline in Republican counts, an ambiguous net change in Democratic counts and in turnout, and a net pro-Democratic shift in vote shares — sharp for vote share, ambiguous for participation, because the misaligned were concentrated among habitual Republicans. I formalize the decomposition in the Appendix.

These predictions follow from a single channel, information-poverty reduction; but several alternatives could produce similar aggregate patterns. *Salience mobilization* holds that radio drew disengaged citizens to the polls by making politics entertaining and accessible, regardless of policy learning. *Rhetorical persuasion* emphasizes spoken content's power over voters who already had print information, distinct from persuasion of newly *reached* listeners, taken up below. *Entertainment crowd-out* holds that radio privatized leisure and displaced civic engagement, depressing turnout without transmitting information at all (Gentzkow, 2006; Olken, 2009; Kim and Patterson, 2025). *FDR-specific messaging* attributes the pro-Democratic effect to Roosevelt's content rather than a general in-

Table 1: Competing Mechanisms and Testable Predictions

	Literacy interaction	Newspaper-density independence	FDR-era uniqueness	Legislator response	Asymmetric Rep vote loss
Information poverty reduction	Yes	Yes	No	Via elections	Yes
Salience mobilization	Yes (symmetric)	No	No	Unclear	No
Persuasion (of print-informed)	No	No	Depends	Via elections	Weak
Entertainment crowd-out	Turnout only	No	No	None	No
FDR-specific messaging	No	No	Yes	Via elections	Unclear
Elite signaling	n/a	n/a	Yes	Via incumbents	No
Observed (this paper)	Yes (1.62*)	Yes ($\approx$0)	Ambiguous	Selection only	Yes (Rep -1.07^{***})

Note: Rows are candidate mechanisms; columns are testable dimensions, each evaluated in the indicated table (literacy interaction, Table 6; newspaper-density independence, Table 8; FDR-era uniqueness, Table 7; legislator response, Table 5; asymmetric Republican vote loss, Table 4). Entertainment crowd-out predicts a literacy gradient on turnout only; salience mobilization predicts symmetric positive moderation by both literacy and newspaper density. The legislator-response column separates voter-side from elite-side accounts: mechanisms operating through voters reach representation through elections (selection), while elite signaling acts on sitting incumbents (adaptation); the observed response is selection only, within non-South Democratic-held seats (Section 4.2). The FDR-era-uniqueness column asks whether the effect should appear *only* in the FDR era, as a content-driven account requires; the information account predicts concentration, not uniqueness, because the aligned partisan alternative arrived only in 1932. The observed cell is therefore ambiguous by design, not by data limitation: the concentration (Table 7) is predicted by both accounts, and the literacy gradient, not timing, separates the two. The newspaper-density cell ($\approx$0) refers to per-capita circulation, while count and total-circulation measures carry small positive interactions about one-eighth the literacy gradient (Appendix Table A29).

formation mechanism. *Elite signaling* posits that legislators responded to party-leadership signals via radio rather than to voters; because leadership cues act on sitting members, it predicts *within-legislator adaptation*, requiring no electoral turnover. Each generates distinct predictions; Table 1 summarizes them.

The literacy gradient is the leading discriminator but cannot stand alone: both information poverty and salience mobilization predict larger effects where illiteracy is higher. They diverge on newspaper circulation — literacy was an absolute barrier, circulation only a difference of degree among those who could already read — so a steep illiteracy gradient alongside a weak newspaper-density one is the separating test. The remaining rivals make distinct predictions (no literacy gradient for persuasion or FDR-specific messaging; a turnout-only effect for crowd-out; an incumbent-adaptation legislator response for elite signaling), taken up in turn.

3 Research Design and Data

3.1 Identification Strategy

A naive regression of political outcomes on radio ownership would confound radio's effect with the characteristics of early-adopting communities: wealthier, more urban, more politically engaged places may have adopted radio faster while experiencing different political trends for unrelated reasons. I address this selection bias with instrumental variables (IV) that exploit geographic determinants of radio reception quality.

Building on Stromberg's (2004) cross-sectional instruments, I interact two time-invariant geographic features, ground conductivity and woodland share, with time, generating within-county variation in predicted radio adoption. In the early 1920s, with few stations broadcasting, reception quality mattered little; by the late 1920s and 1930s, when national networks offered hours of daily programming, the same geographic differences translated into meaningful variation in ownership and listening. The time interaction captures these increasing returns to reception quality as the industry matured — within-county variation a purely cross-sectional design would lack.

My primary specification is county-level two-stage least squares with county and state-by-year fixed effects. For county c in state s and election year t:

First stage:

$$\text{Radio}_{ct} = \alpha_c + \gamma_{st} + \beta_1(\text{GroundConductivity}_c \times t) + \beta_2(\text{WoodlandShare}_c \times t) + \mathbf{X}'_{ct}\boldsymbol{\delta} + \epsilon_{ct} \quad (1)$$

Second stage:

$$Y_{ct} = \alpha_c + \gamma_{st} + \theta \cdot \widehat{\text{Radio}}_{ct} + \mathbf{X}'_{ct}\boldsymbol{\lambda} + u_{ct} \quad (2)$$

where Y_{ct} is the electoral outcome: presidential Democratic two-party vote share, presidential turnout, House Democratic two-party vote share, or log total House votes. County fixed effects α_c absorb all time-invariant county characteristics (geography, settlement patterns, persistent political culture, long-run economic structure); state-by-year fixed effects γ_{st} absorb all state-level time-varying confounders such as state economic trends, gubernatorial elections, and policy changes, on top of national trends.

The control vector $\mathbf{X}_{ct}$ includes urbanization, Black population share, and log county population. Standard errors cluster at the county level (district level for the district analysis), matching where radio exposure varies; for interactions with county illiteracy, whose identifying variation is largely between-state, I report the more conservative state-clustered errors. Because the instruments are spatially smooth fields interacted with time, county clustering may also understate level-specification errors, so Appendix Table A10 reports every headline level estimate under both clusterings (the non-South share, turnout, and Republican-count results survive state clustering; the supplementary full-sample share estimates do not). Weak-instrument and overidentification diagnostics — partial R^2, Kleibergen–Paap statistics, Hansen J tests, Sanderson–Windmeijer conditional F — appear in Appendix Table A3 and the table notes.

The key identifying assumption is the exclusion restriction: conditional on county and state-by-year fixed effects and controls, the interaction of geographic radio propagation features with time affects electoral outcomes only through its effect on radio adoption. This is a non-trivial assumption that warrants careful discussion. County fixed effects absorb the direct influence of geography on political outcomes.

The assumption would be violated if geographic reception advantages translated into political change over time through channels other than radio. The two instruments differ here. Ground conductivity reflects subsurface geology, integrated far below the topsoil that governs agricultural productivity, so the exclusion restriction is most plausible for it. Woodland share is a more direct concern: heavily forested counties had different economic structures, lower densities, and potentially different trajectories independent of radio, and the design cannot lean on the cleaner instrument alone, since the two identify jointly and woodland carries the larger within-state partial R^2 (0.15 versus 0.04 in the non-South). Section 6 confronts the concern directly, with one bottom line used throughout the paper: the turnout and Republican-vote-decline results that carry the realignment argument hold using ground conductivity alone, so they do not rest on woodland; the Democratic vote-share magnitude is corroborated but partly woodland-dependent, and is flagged as such.

State-by-year fixed effects provide a further safeguard, identifying the effect from within-state, within-year variation alone. This removes the between-state component of the instruments, absorbing much of their variance, yet first-stage F-statistics remain above 600 in the main samples — within-state variation alone identifies strongly. I treat it as primary because between-state exposure correlates with state economic development, urbanization, and competitiveness, all of which independently shaped electoral trends; county-and-year estimates are reported alongside.

3.2 Data Sources

County-level radio ownership comes from the 1930 and 1940 U.S. Censuses (the first to ask about radio sets), with ground conductivity and woodland share from Stromberg (2004); ownership in 1920 is set to zero, as commercial broadcasting began only in November 1920. Measured as the fraction of households owning a receiver, diffusion was rapid: across my 3,000-plus counties, mean ownership rose from zero in 1920 to 26% in 1930 and 69% in 1940, surpassing daily newspapers as the most widely used political medium by the mid-1930s. I linearly interpolate to non-Census election years, so identification rests on the cross-decade Census variation; because the interpolation error is a systematic function of each county's adoption path rather than classical noise, I do not use it to adjudicate IV-versus-OLS magnitudes.

County-level presidential returns come from Clubb et al. (2006) (Democratic two-party vote share and turnout, six elections 1920–1940); House returns from the United States Historical Election Returns collection (Inter-university Consortium for Political and Social Research, 2025) (Democratic two-party share and total votes, eleven elections 1920–1942). I compute two-party share as the Democratic share of the combined Democratic and Republican vote, excluding third parties for comparability across elections.

Turnout is total votes cast divided by estimated voting-age population per county-year. Because county population is decennial, I interpolate between Censuses, introducing denominator measurement error unlikely to correlate with radio penetration conditional on my controls.

County illiteracy, the Census measure for persons aged 10 and over unable to read or write in any language, comes from the 1920 and 1930 Censuses; persons literate only in a non-English language count as literate, so it understates exclusion from the English-language press. The distribution was sharply uneven: mean county illiteracy was 5.3% in 1920 and 4.1% in 1930, but Southern states exceeded 10% versus below 4% in the North, with substantial within-state variation (in Texas, near zero to 44%). I assign it as a two-step function of election year, breaking at 1925 (the 1920 rate for 1920–1924 elections, the 1930 rate thereafter).[1]

County-level daily newspaper circulation comes from the United States Newspaper Panel (Gentzkow et al., 2011), tracking individual local papers' daily circulation across counties and years. I construct three county-year measures: distinct-paper count, total daily circulation, and circulation per capita. I use circulation per capita, the access-intensity measure, as the primary moderator in Section 5 (Appendix Table A29 reports the others). Of roughly 3,100 sample counties, 1,168 (38%) had a tracked daily at some point during 1920–1940; the rest had none, so county fixed effects leave nothing to identify there. The newspaper analysis therefore uses the ever-tracked counties (1,067 enter after merging and dropping singletons; Ns in Table 8). Summary statistics: Appendix Table A1.

[1]Unlike radio ownership (linearly interpolated), illiteracy uses a step function: the 1940 Census replaced the read/write question with "highest grade of school completed," leaving no terminal value to interpolate toward, so I carry the 1930 rate forward to the 1940 election. The cross-Census county correlation is 0.94, so the geographic pattern of illiteracy was highly stable across decades.

Legislator ideology scores come from the Voteview project (Lewis et al., 2023). I use two complementary measures on the first (liberal-conservative) dimension. The Nokken-Poole score is re-estimated each Congress and so varies within a legislator's career, capturing both adaptation (within-legislator change in roll-call behavior) and selection (replacement of incumbents). The common-space DW-NOMINATE score fixes each legislator's position across their career, so within-district changes reflect only selection. Comparing the two separates the adaptation and selection channels; I also add legislator fixed effects to the Nokken-Poole score to isolate the within-legislator adaptation margin on a single scale (Appendix Table A38).

For the district-level analysis, I aggregate county radio data to congressional districts using population-weighted area crosswalks (Ferrara et al., 2024), which handle the county-to-district mapping that shifts with each decennial redistricting.

The instruments and key variables are sharply patterned geographically: ground conductivity is highest in the Great Plains and woodland share in the Southeast and Pacific Northwest, both with substantial within-state variation, while 1930 radio ownership mirrors conductivity and illiteracy concentrates in the South and Appalachia (Figure 1).

3.3 Sample

The merged dataset is a county-year panel in which nearly all counties appear in every election year, with some entry and exit from boundary changes, missing returns, or absent Census data; county boundaries were largely stable across 1920–1940.

The presidential panel includes approximately 18,300 county-year observations across more than 3,000 counties and six elections (1920–1940); the House panel is larger, approximately 34,300 county-year observations across eleven elections (1920–1942), reflecting the biennial cycle.

Conventional first-stage F-statistics far exceed the Stock and Yogo (2005) weak-instrument critical values under both fixed-effect structures, and cluster-robust Kleibergen–

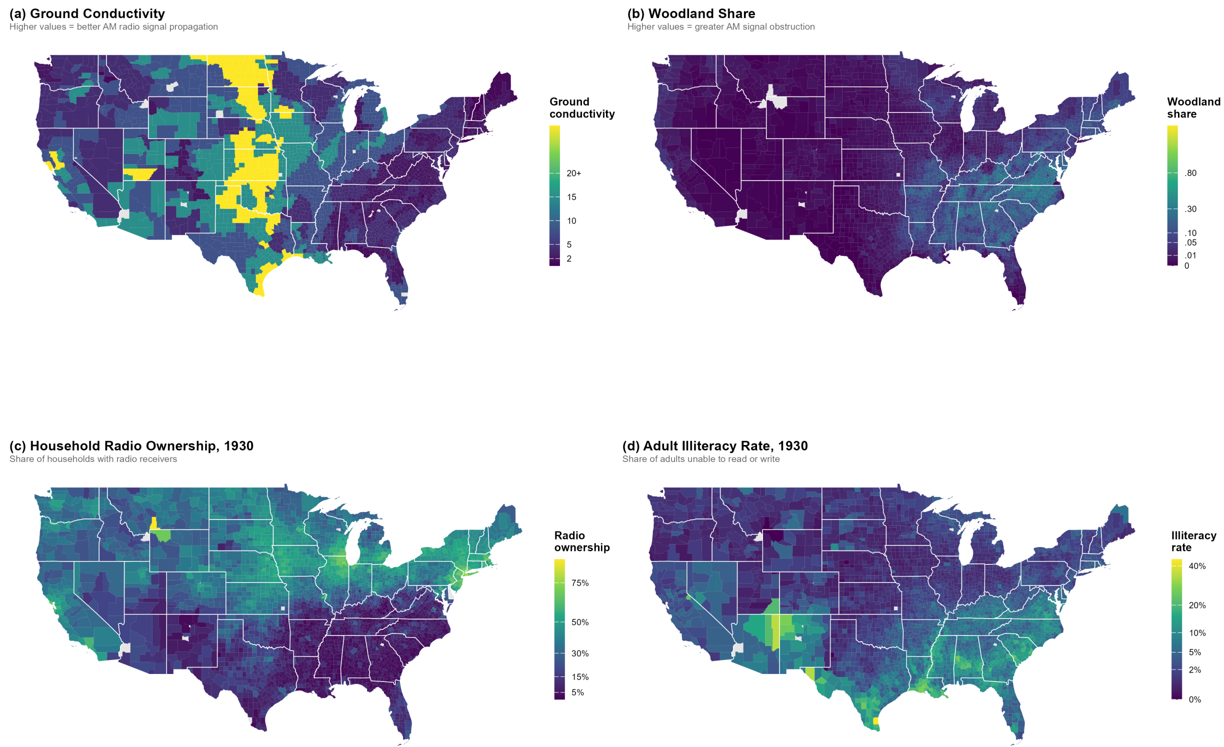

Figure 1: Geographic Variation in Instruments and Key Variables

Note: County-level maps of the two instrumental variables (ground conductivity and woodland share), radio ownership (1930 Census), and illiteracy rate (1930 Census). Ground conductivity measures the quality of AM radio signal propagation; higher values indicate better reception. Woodland share measures signal obstruction from tree cover. The county-level variation visible within states is the identifying variation that survives state-by-year fixed effects.

Paap statistics remain far above conventional thresholds in the main samples (Appendix Table A3). The district-level analysis uses 4,304 legislator-Congress observations from the 67th through 76th Congresses (1921–1941).

Table 2: Radio and Democratic Vote Share

	(1) Full	(2) Non-South	(3) South
Panel A: Presidential elections			
Radio	0.092^{+}	0.250^{***}	-0.109
	(0.051)	(0.040)	(0.132)
First-stage F	685	1,000	109
N	18,317	11,689	6,628
Panel B: House elections			
Radio	0.103^{+}	0.181^{**}	-0.020
	(0.061)	(0.056)	(0.121)
First-stage F	1,248	1,711	249
N	34,314	21,716	12,598
County FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes

Note: IV estimates with county and state-by-year fixed effects. Dependent variable is Democratic two-party vote share. Controls include urbanization, Black population share, and log population. Instruments are ground conductivity × time and woodland share × time. County-and-year fixed effects estimates are reported in Appendix Table A6. Significance based on county-clustered SEs: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

4 Results

4.1 Main Electoral Effects

Presidential Vote Share I begin with presidential elections, where radio's informational effect should be strongest given the national focus of interwar programming. The framework predicts Democratic gains concentrated where the misaligned-uninformed pool sat (primarily the rural non-South), strongest where information poverty was most severe. Table 2 presents IV estimates across regional subsamples.

Consistent with the theory, radio increased Democratic presidential vote share. Under state-by-year fixed effects, which restrict identification to within-state reception variation, the non-South effect is substantial and highly significant (Table 2, column 2): a 10-percentage-point increase in radio ownership corresponds to roughly 2.5 points of Democratic vote share. The county-and-year specification yields a much larger estimate (Appendix Table A6), but that gap reflects between-state Democratic vote growth more plausibly driven by state-level trends than by radio itself; adding state-specific linear time trends to the county-and-year specification, a far milder control, reproduces the within-state estimate almost exactly (Appendix Table A8). I therefore treat within-state estimates as primary.

The magnitude is large but consistent with the media-and-voting literature, where extending access to audiences that lacked an accessible source moves votes far more than shifting the slant of a source they already consume (Enikolopov et al., 2011; DellaVigna and Kaplan, 2007; Adena et al., 2015; Martin and Yurukoglu, 2017).

The regional pattern confirms the scope condition. In non-Southern counties[2], where a substantial Republican pool existed, the effect is more than double the full-sample estimate; in the South it collapses to statistical zero even though the first stage remains strong, because Southern Democratic vote share already averaged 0.77, leaving a negligible Republican pool, while Jim Crow disenfranchisement kept mobilization from the ballot box. Two tests bound this contrast. A pooled interaction puts the non-South–South difference at $+0.36$, significant under county clustering ($p = 0.009$) though not under the conservative state clustering ($p = 0.27$), where 48 clusters must carry a between-region comparison, so the scope condition rests on the point-estimate contrast and its institutional grounding (Appendix Table A31). And within the South itself, the counties that retained a substantial Republican pool show a positive though imprecise point estimate ($+0.42$), while near-zero-Republican counties show a precise zero — the gradient the floor-effect reading predicts (Appendix Table A32).

House Elections The House results (Panel B of Table 2) show a similar non-South pattern, significant and roughly three-fourths the presidential magnitude: smaller, as expected, given the presidential focus of interwar network programming, with individual House races remaining the province of local newspapers. The Southern House estimate

[2] I define the South as the eleven former Confederate states: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

Table 3: Radio and Voter Participation

	(1) Full	(2) Non-South	(3) South
Panel A: Presidential turnout (fraction of eligible population)			
Radio	-0.125^{**}	-0.322^{***}	0.192^{+}
	(0.042)	(0.037)	(0.109)
First-stage F	686	999	110
N	18,281	11,673	6,608
Panel B: House turnout (fraction of county population)			
Radio	-0.032	-0.173^{***}	0.138^{*}
	(0.029)	(0.034)	(0.060)
First-stage F	1,252	1,638	249
N	34,904	22,321	12,583
County FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes

Note: IV estimates; controls, instruments, and significance conventions as in Table 2. Panel A dependent variable: total presidential votes divided by ICPSR voting-age population, capped at 1.0 (binds in 39 county-years with undercounted denominators; without the cap the non-South coefficient is roughly 21% larger and remains significant at the 0.1% level); 6 election years. Panel B: total House votes divided by county population (no House-specific voting-age estimate exists); 11 election years, odd years excluded. County-and-year fixed-effects estimates in Appendix Table A6.

is again indistinguishable from zero.

Turnout and Vote Count Decomposition Table 3 presents turnout results. Under state-by-year fixed effects, radio *decreases* presidential turnout in the non-South, highly significantly.[3] In the South the presidential point estimate is marginally positive and its House counterpart (Panel B) reaches $p < 0.05$: in tension with, though not overturning, the scope-condition prediction of a clean Southern null — participation is the margin where the Southern estimates sit least comfortably with the floor-effect reading.

To understand the negative turnout effect, I decompose total votes into party components: Table 4 reports non-South IV estimates for log Democratic, log Republican, and log total votes under state-by-year fixed effects.

[3]Turnout (votes cast over voting-age population, VAP) exceeds an impossible 100% in 39 county-years (0.21% of obs) where ICPSR's VAP denominator is undercounted, mostly independent cities whose votes are coded to the city while VAP comes from another unit. I cap these at 1.0; without the cap the non-South coefficient remains negative and highly significant, the cap reducing its magnitude by roughly 18%.

Table 4: Vote Count Decomposition (Non-South, State $\times$ Year FE)

	Log Dem	Log Rep	Log Total	Dem Share	Turnout
Radio	0.039	-1.069^{***}	-0.438^{***}	0.250^{***}	-0.322^{***}
	(0.170)	(0.116)	(0.071)	(0.040)	(0.037)
County FE	Yes	Yes	Yes	Yes	Yes
State $\times$ Year FE	Yes	Yes	Yes	Yes	Yes

Note: IV estimates, non-South sample. Controls, instruments, and significance conventions as in Table 2. Standard errors clustered at county level in parentheses. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

The decomposition reveals a striking asymmetry at the non-South average (Table 4): within states, radio left Democratic vote counts statistically indistinguishable from their pre-radio level but sharply and significantly reduced Republican counts. The Democratic vote-share increase therefore came not from new Democratic voters but from Republican voters withdrawing, and the negative turnout effect follows mechanically — the disruption of habitual Republican voting the mechanism predicts. The near-zero change in Democratic votes can itself reflect cancellation between inflows and outflows (Appendix D.1). In the South, neither party's votes moved significantly (Appendix Table A19), consistent with the scope condition.[4]

These decomposition estimates are county averages: each county weighs equally, so they describe the modal county — rural and small-town America — rather than the average voter. Population weighting recovers the person-weighted aggregate, and it inverts the participation margins while preserving the partisan ones: the Republican decline survives (-1.44) and the share gain strengthens (0.65), but aggregate turnout becomes a precise null and Democratic counts turn positive (Appendix Table A22). Person-weighted, radio's signature adds the urban, mobilizing face of the realignment; county-averaged, it is the demobilization of habitual Republicans. Claims in this paper are stated in county-

[4]The county-and-year positive turnout finding (replicating Stromberg, 2004) reflects between-state Democratic vote growth that disappears within state, alongside a persistent Republican decline (Appendix Table A18). The turnout placebo supports state-by-year as primary: ground conductivity $\times$ time predicts pre-radio turnout (1908–1916) under county-and-year but not state-by-year; the Democratic-share placebo is at most marginal and negative-signed, against the headline (Appendix Table A4).

average terms unless marked otherwise.

The decomposition also speaks to a long-standing question about the New Deal realignment: whether the Democratic majority was built by converting Republicans or mobilizing new voters (Andersen, 1979; Erikson and Tedin, 1981). My within-state estimates add a channel neither pole anticipated — a Democratic-share gain with *no* growth in Democratic votes, produced by rural Republican demobilization — while cities show the mobilization Andersen described and dominate the person-weighted aggregate (Appendix D.3; Appendix Tables A21 and A22). Both are releases of the same misalignment: exit where leaving was the low-cost response, entry where joining was.

A caveat applies: because my data are aggregate county returns, "switching" and "demobilization" describe aggregate patterns rather than observed individual transitions — the decomposition establishes what happened to each party's vote totals, not the individual-level behavioral mechanism.

With that caveat, the competition split (Appendix Table A20) illuminates the relative contributions: where Republicans held safe 1920 majorities, radio produced a significant Democratic vote increase alongside the Republican decline (consistent with switching); in competitive counties, Republican votes declined with no Democratic increase (demobilization); and where Democrats held safe 1920 majorities — a thin cell of 66 counties whose misaligned pool was habitual Democrats rather than Republicans — the Democratic-share effect vanishes. The vote share effect declines monotonically across the three competition bins, matching the scope condition: radio's effect is larger where the misaligned pool is larger.

The competition split doubles as the direct test of entertainment crowd-out (Table 1): crowd-out predicts depressed participation wherever radio diffused with no directional vote movement, so it cannot generate the significant Democratic vote *increase* in safe-Republican counties, and it predicts a Southern turnout decline where the point estimate is instead positive (Table 3).

Two caveats bound these magnitudes. All estimates are within-state relative effects, not aggregate time-series predictions: a negative relative effect is fully compatible with the era's rising national turnout, which the fixed effects absorb. And the IV identifies a local average effect for the counties whose adoption responded to reception geography; extrapolating to the full 1920–1940 diffusion path would overstate what the design identifies.

4.2 Legislator Ideology

The behavior of elected representatives is the final link in the accountability chain, and the least guaranteed to follow from a shift in votes: incumbents can outlast a changing electorate, and replacement and adaptation may be gradual and noisy. Did a disruption that informed previously excluded voters also register in the representatives those districts sent to Congress?

Aggregating county radio data to congressional districts via the population-weighted crosswalks (Ferrara et al., 2024), I estimate IV regressions with district and Congress fixed effects, using the same geographic instruments.[5]

Table 5 presents the two complementary ideology measures: Panel A the Nokken-Poole score (adaptation plus selection); Panel B the DW-NOMINATE score (selection only). Each panel reports the national estimates and, below them, a non-South re-estimation; the contrast between the two rows is the analytical core of this section. Negative values indicate more liberal positions.

Radio produced a substantial leftward shift in national district-level legislator ideology (Table 5): the full-sample estimates are large, precise, and highly significant under both measures. The national rows cannot carry that reading, however. The panel includes

[5] State-by-Congress fixed effects are infeasible: they absorb 79–84% of district-level instrument variation, collapsing the first stage (county-level absorption shares in Table A2). I split the sample by district illiteracy via separate subsample regressions; a saturated interaction IV, equivalent to the split, is used only for the high-minus-low difference.

Table 5: Radio and Legislator Ideology (District Level)

	(1) Full	(2) High Illit	(3) Low Illit	(4) Dem, Hi Illit	(5) Rep, Hi Illit
Panel A: Nokken-Poole first dimension (adaptation + selection)					
National (incl. South)	-1.794^{***}	-1.550^{***}	-1.203	-1.033^{***}	-0.286
	(0.234)	(0.281)	(0.944)	(0.263)	(0.256)
Non-South	-0.330	-0.170	-0.557	-0.517	-0.325
	(0.539)	(0.717)	(0.925)	(0.341)	(0.333)
N (national; non-South)	4,304; 3,250	2,149; 1,150	2,149; 2,094	1,565; 603	545; 513
Panel B: DW-NOMINATE first dimension (selection only)					
National (incl. South)	-1.536^{***}	-1.412^{***}	-0.320	-0.726^{***}	-0.669^{**}
	(0.208)	(0.247)	(0.846)	(0.180)	(0.244)
Non-South	-0.154	-0.389	0.249	-0.705^{***}	-0.454
	(0.517)	(0.662)	(0.829)	(0.202)	(0.299)
N (national; non-South)	4,290; 3,240	2,141; 1,145	2,143; 2,089	1,560; 601	543; 511
District FE	Yes	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes	Yes

Note: IV estimates with district and Congress fixed effects; district-clustered standard errors in parentheses. Panel A uses the Nokken-Poole score; Panel B the DW-NOMINATE first-dimension score. Negative values indicate more liberal positions. Columns (4)–(5) restrict to Democratic and Republican legislators within high-illiteracy districts; the non-South rows apply the full-sample median cutoff, so each cell is directly comparable to its national counterpart. Conventional first-stage F: national 530/307/101/123/70 across columns; non-South 284/176/93/111/49 (114 for the Panel B Democratic cell). Saturated-interaction high-minus-low differences are insignificant in both samples (national $p = 0.72/0.22$, non-South $p = 0.39/0.60$ for Panels A/B). The non-South rows are the estimates the paper's scope condition licenses: 61.2% of the national column (4) observations are Southern districts (see text). $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

the Confederate 11, and its high-illiteracy cells prove structurally Southern: 61.2% of the observations behind the national Democratic high-illiteracy estimates come from Southern districts. By the paper's own scope condition those electorates could not respond to radio at the ballot box, and the Southern delegation's New-Deal-era leftward movement has ample explanations unconnected to radio. The non-South rows are therefore the estimates the theory licenses, and they discipline the finding sharply: the full-sample Nokken-Poole effect collapses to -0.33 (n.s.), with a confidence interval that excludes the national value — a compositional artifact exposed, not power lost — and the within-legislator adaptation margin and the party asymmetry it once supported dissolve with it (Appendix F sorts each full-sample estimate into refuted, no-evidence, or underpowered).

One caveat spans the section: state-by-Congress fixed effects are infeasible at the district level, so these estimates rest largely on between-state instrument variation of the kind the county analysis treats as potentially confounded.[6] A pre-radio placebo supports the design in the national panel — there the instruments do not predict 1913–1920 legislator ideology under its own fixed effects — while confirming that within-state identification is unavailable; in the non-South the woodland placebo is positive-signed (marginal to significant) but opposite the estimated effects, hence conservative, while conductivity is a clean null throughout (Appendix Tables A36 and A33).

One estimate survives, and survives everything the design can throw at it: in Democratic-held high-illiteracy districts, more radio produced the election of substantively more liberal Democrats (DW-NOMINATE -0.705, $p < 0.001$, first-stage F = 114). The South cut that destroys the rest leaves it unchanged (-0.726 national to -0.705 non-South); dropping the most-forested districts strengthens it (to -0.958 under the top-quartile woodland drop); and it holds when identified off ground conductivity alone (-0.916 at baseline, -0.725 under the quartile drop) — the same clean-instrument defense the county results pass (Appendix Tables A33 and A34).

The channel is unusually well identified: because DW-NOMINATE is fixed over a career, a within-district change in it is selection — different legislators, not changed minds — and the selection was within-party. Radio does not predict Democratic seat capture anywhere in the non-South (the national "capture" coefficient is a one-party-South artifact), while the surviving cell conditions on Democratic occupancy by construction (Appendix Table A35). The electorate shift thus expressed itself through candidate replacement inside the locally dominant party: Democratic seats came to be filled by more liberal Democrats, with no partisan turnover and no evidence that sitting incumbents converted — the within-legislator estimate falls to -0.11 (n.s.), though with only 137 identifying leg-

[6] Pushing further, census-division-by-Congress fixed effects, absorbing 56–57% of residual instrument variance beyond Congress fixed effects, preserve negative full-sample point estimates but render them imprecise, while the illiteracy and party subsample first stages weaken enough to be uninformative rather than contradictory (Appendix Table A37).

islators that is absence of evidence rather than refutation. The Republican-side selection estimate loses just 37 Southern observations to the cut but much of its power with them; the non-South data cannot decide it.

Read against the alternatives, the surviving pattern points to voters rather than leaders: an elite-signaling account operates on sitting legislators — leadership cues should move incumbents' roll-call behavior, requiring no electoral turnover — whereas the data show replacement without detectable adaptation. A recruitment variant, in which party gatekeepers steer more liberal candidates toward high-radio districts, cannot be excluded by design, though it supplies no mechanism for the literacy gradient. The compositional lesson is itself a finding: in a national district panel of this era, high-illiteracy cells are structurally South-dominated, so the Southern delegation's New-Deal-era movement will masquerade as an information-access effect unless the scope condition is imposed. The representational claim the paper carries forward is accordingly bounded: where the newly informed electorate moved, representation moved with it — through the candidates high-illiteracy districts elected, within the party those districts already held.

5 Mechanism: The Literacy Channel

The main results are consistent with information poverty, but several alternatives in Table 1 could produce similar aggregate effects. The decisive test is whether radio's effect scales with the severity of information exclusion: radio should matter most where voters were most cut off from print.

I test this three ways: the radio-by-illiteracy interaction, temporal heterogeneity across the pre-FDR and FDR periods, and a radio-by-newspaper-circulation interaction. My primary estimator is IV, instrumenting every radio term, interactions included, with the corresponding geographic-instrument interactions.[7] Because the scope condition restricts

[7] Using OLS for the mechanism while instrumenting the level would let a confound enter through the interaction. Interaction first-stage F-statistics are well above weak-instrument thresholds throughout (Table

Table 6: Radio × County Illiteracy Interaction: IV and OLS

	Non-South (scope-relevant)			Full sample (supplementary)		
	Pres Dem Share	Pres Turnout	House Dem Share	Pres Dem Share	Pres Turnout	House Dem Share
Panel A: IV interaction (primary)						
Radio	0.291**	−0.327***	0.222*	0.095	−0.102	0.125
	(0.107)	(0.060)	(0.091)	(0.079)	(0.099)	(0.079)
Radio × Illiteracy	1.620*	−1.421**	2.176***	0.208	−0.142	0.965
	(0.635)	(0.542)	(0.585)	(0.929)	(0.522)	(0.679)
Panel B: OLS interaction (supplementary)						
Radio	0.162***	−0.079+	0.150**	0.177***	−0.043	0.103***
	(0.039)	(0.041)	(0.046)	(0.038)	(0.030)	(0.029)
Radio × Illiteracy	1.074**	−0.261	1.835***	0.634*	0.002	0.946*
	(0.396)	(0.290)	(0.490)	(0.261)	(0.166)	(0.368)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	11,689	11,673	21,716	18,317	18,281	34,314

Note: Panel A: IV with two endogenous regressors (radio, radio × illiteracy) and four instruments (ground conductivity × time, woodland × time, each also interacted with county illiteracy). Non-South first-stage F: 717 (radio), 1,737 (interaction); Sanderson–Windmeijer conditional F: 162 and 73 (county clustering), 106 and 46 (state). The weak-instrument-robust subset Anderson–Rubin 95% confidence set for the non-South interaction is $[0.02, 3.58]$, and the Anderson–Rubin joint test rejects at $p = 0.003$ (state-clustered). Panel B: OLS analogue. Both panels include county illiteracy at level. County and state-by-year fixed effects; illiteracy time-varying by nearest Census (1920 rates through 1924, 1930 thereafter). Non-South excludes the Confederate 11 (see Table 2 note). State-clustered SEs. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

the mechanism to the non-South, I treat the non-South IV interaction as primary, with full-sample IV and OLS supplementary.

County Illiteracy Interaction The first test instruments radio and radio × illiteracy with the two geographic instruments and their illiteracy interactions.

The non-South IV interaction matches the framework's central prediction across all three primary outcomes: radio's effect on presidential Democratic vote share scales positively and significantly with county illiteracy; the House interaction is larger and more precise; and the turnout interaction is negative and significant — radio's turnout depres-

6 note; Table 8, exceeding 740 per interaction in the non-South and 800 in the full sample). Era-by-era IV is infeasible because the FDR-era first stage weakens ($F = 11.7$ full, $F = 37$ non-South) once radio saturates; I instead instrument radio and radio × *FDR era* jointly, with four instruments. The interaction also faces a more demanding identification requirement than the level: standard adoption confounders (income, urbanization, industrial composition) operate at the level, not differentially by the moderator.

sion concentrates in high-illiteracy counties. The three do not carry equal evidentiary weight. As at the level, the gradient's load-bearing members are the demobilization outcomes: the turnout and Republican-count interactions survive when identified off ground conductivity alone (-1.63 and -7.38, both $p < 0.05$), while the share interaction does not (1.62 to 0.81, n.s.; Appendix Table A17). I therefore rest the mechanism on the demobilization gradient — participation falling, and Republican votes falling fastest, where illiteracy was high — and read the share gradient as corroborating. The OLS interactions (Panel B) are same-signed but smaller, mirroring the ordering at the level; the full-sample IV interactions are same-signed but individually insignificant, the expected power cost of pooling Southern counties where the scope condition fails. In magnitude, radio's share effect is more than a quarter larger at the estimation sample's 90th percentile of illiteracy (5.1 percent) than in fully literate counties, and remains positive and significant across the observed range.

The gradient is plausible because county illiteracy proxies a broader community gradient: for every adult who could not read at all, several more read haltingly, so the interaction reflects community-level information poverty, not merely a direct effect on the completely illiterate. One measurement caveat: the 1940 Census dropped the literacy question, so the 1930 rate carries forward to the 1936 and 1940 elections; the 0.94 cross-Census county correlation bounds the resulting attenuation.

Decomposing the IV illiteracy interaction by outcome (Appendix Table A23), the gradient on log Democratic votes is too noisy to identify under IV, while the gradient on log Republican votes is negative and significant: the share gradient is driven primarily by differential Republican decline at high illiteracy, the cleanest count-level evidence for the mechanism.

A median-split alternative yields an apparent reversal: the share effect is larger in the low-illiteracy half. But this reflects sample composition (border-state counties, especially Kentucky, dominate the high-illiteracy cells), not a contradicted gradient; I treat the continuous interaction as primary and document the diagnosis in Appendix E.1.

Two further checks discipline the gradient's shape and location. A binned specification frees it from linearity: the marginal effect rises monotonically across illiteracy terciles (0.26, 0.28, 0.32), and the top-tercile increment is significant ($p = 0.009$; Appendix Table A25). And the gradient sits where the theory puts the movable pool: within 1920 safe-Republican counties the interaction is 2.06 ($p < 0.001$), against 1.10 (n.s.) in competitive counties, with the thin safe-Democratic cell at 1.06 ($p < 0.05$) (Appendix Table A26).

This literacy-graded pattern is inconsistent with persuasion of print-informed audiences and with strict FDR-specific messaging (Table 1), neither of which predicts a literacy-conditional response. It does not by itself adjudicate the rest: salience mobilization also predicts positive literacy moderation (separated by the newspaper-density asymmetry below); the persuasion-of-newly-reached complement (Zaller, 1992) shares the gradient — the framework treats it as a variant of the same release, not a rival to eliminate; elite signaling is addressed by the selection-only legislator response (Section 4.2); and a milder, Roosevelt-era-only version of FDR-messaging survives until the temporal evidence below.

Temporal Heterogeneity A natural concern is that radio's pro-Democratic effect is an artifact of its period — something peculiar to the 1920s or the Great Depression — rather than a general information mechanism. Instrumenting radio and radio $\times$ FDR-era jointly within a single specification separates the pre-FDR and New Deal–era effects, with the analogous OLS era interaction reported alongside.

Table 7 presents this joint-interaction era split (non-South primary; full-sample IV and OLS supplementary). Radio's pro-Democratic, turnout-depressing effect is concentrated in the New Deal era: in the non-South the radio $\times$ FDR-era coefficient on Democratic vote share is $+0.308$ ($p < 0.01$) and the turnout coefficient is negative and significant, every column sign-consistent, while the pre-FDR base is null or mildly negative (-0.134,

Table 7: Radio and Presidential Outcomes by Era

	Non-South IV (primary)		Full-sample IV (supp.)		Non-South OLS (supp.)	
	Pres Dem Share	Pres Turnout	Pres Dem Share	Pres Turnout	Pres Dem Share	Pres Turnout
Radio (pre-FDR base)	−0.134	−0.178	−0.221	0.035	0.017	0.079*
	(0.186)	(0.108)	(0.190)	(0.121)	(0.053)	(0.039)
Radio × FDR era	0.308**	−0.115*	0.249*	−0.127*	0.131**	−0.177***
	(0.102)	(0.058)	(0.113)	(0.053)	(0.044)	(0.033)
Implied FDR-era effect	0.174	−0.292	0.029	−0.092	0.148	−0.098
First-stage F (radio)	526	525	358	358	—	—
First-stage F (radio × FDR)	674	671	486	487	—	—
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	11,689	11,673	18,317	18,281	11,689	11,673

Note: IV columns estimate a single regression with radio and radio × FDR-era endogenous and four instruments (ground conductivity × time, woodland × time, each also interacted with the FDR-era indicator); per-column conventional first-stage F-statistics are reported. The non-South is the scope-relevant primary sample; the full sample is supplementary. Pre-FDR = elections of 1920, 1924, 1928; FDR era = 1932, 1936, 1940 (the November 1932 campaign already referenced the New Deal program). Era-specific subsample IVs are not reported because the FDR-era first stage weakens substantially (F = 11.7 full; F = 37 non-South); the joint-interaction era estimates need not average to the pooled Table 2 estimates (different instrument set, different complier weights). OLS columns report the analogous specification without instrumenting, on the IV sample. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

n.s.) — the pooled effect is not a pre-1932 phenomenon. The differential is estimated with power (joint first-stage F = 526 for radio, 674 for the interaction); only the separate era-subsample instruments weaken (F = 37 in the non-South), so the joint interaction is the informative estimator, and it places the effect firmly in the New Deal years. The concentration also survives the Depression-geography battery: the radio × FDR coefficient stays between $+0.32$ and $+0.39$ ($p \leq 0.003$) under drought, Dust Bowl, farm-share × year, retail-collapse, and New Deal spending controls (Appendix Table A13). Descriptive year-by-year estimates are available only under county-and-year fixed effects (Appendix Table A9), where the cross-state pattern peaks at 1928.

The 1928 peak under the cross-state specification has a ready historical reading unconnected to information poverty: Al Smith's Catholicism and the Prohibition cleavage moved Catholic, immigrant, and wet counties sharply Democratic (Lichtman, 1979). But state-by-year fixed effects absorb that statewide wave, the spike appears only under the non-primary specification, and the decomposition runs the other way: the Smith surge raised Democratic counts, whereas my within-state estimates show flat Democratic and declining Republican counts. In a direct horse race (Appendix Table A28), the foreign-born interaction is robust on vote share ($+0.054$ per SD, $p < 0.01$) while the illiteracy interaction attenuates ($+0.019$, $p \approx 0.13$), but the turnout gradients diverge in sign (illiteracy -0.049, $p < 0.001$; foreign-born $+0.041$, $p < 0.05$): radio *mobilized* immigrant electorates (Andersen, 1979; Zonszein, 2025) while *demobilizing* habitual Republicans where illiteracy was high — and because the Census counts foreign-language literates as literate, the foreign-born margin partly proxies the same English-print exclusion.

Temporal concentration is consistent with an information mechanism but does not by itself single one out: a mechanism operating only through Roosevelt-era broadcasts would predict the same split (Table 1). The conjunction with the literacy gradient breaks the tie: FDR's audio reached literate and illiterate listeners alike, so an effect running through his content would have no reason to track illiteracy. The gradient therefore identifies information poverty, with the New Deal supplying the occasion rather than the mechanism — content set *when* there was a realigning message to receive; access set *where* its arrival could move votes. The Depression itself is ruled out on the same evidence: county New Deal expenditure per capita (Fishback et al., 2003) interacted with a post-1932 indicator changes the radio $\times$ illiteracy coefficient by at most about 6% (Appendix Table A11), and farm-distress controls — farmland share $\times$ year and drought and Dust Bowl exposure $\times$ FDR era — move it by at most 16% with significance retained (Appendix Table A12).

Newspaper Circulation Within newspaper-tracked counties, the framework predicts that *literacy*, not newspaper availability, binds: the illiteracy gradient should replicate, while the gradient by circulation per capita (Gentzkow et al., 2011) — a dimension voters already had the literacy to access — should be weaker. The test isolates the *intensive* margin, whether more circulation attenuates radio's effect among counties

Table 8: Newspaper Circulation, Illiteracy, and Radio's Electoral Effects

	Non-South IV (primary)		Full-sample IV (supp.)		Non-South OLS (supp.)	
	Pres Dem Share	Pres Turnout	Pres Dem Share	Pres Turnout	Pres Dem Share	Pres Turnout
Radio	0.648***	−0.350**	0.305*	−0.221+	0.289***	0.031
	(0.192)	(0.119)	(0.154)	(0.132)	(0.063)	(0.050)
Radio × Illit. (std.)	0.093*	−0.004	0.036	0.002	0.079**	0.026*
	(0.037)	(0.019)	(0.030)	(0.025)	(0.027)	(0.011)
Radio × Circ. p.c. (std.)	−0.001	0.022+	0.005	0.008	0.022	0.019*
	(0.012)	(0.012)	(0.006)	(0.007)	(0.016)	(0.009)
First-stage F (radio)	80	80	143	144	—	—
First-stage F (radio × illit)	748	747	822	820	—	—
First-stage F (radio × circ)	1,138	1,137	1,653	1,530	—	—
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	4,418	4,412	5,548	5,539	4,424	4,418

Note: IV columns instrument the three endogenous radio terms with the two geographic instruments and their interactions with standardized illiteracy and standardized per-capita circulation (six instruments). OLS columns report the analogous specification without instrumenting. County and state-by-year fixed effects; illiteracy and per-capita circulation standardized within the newspaper-tracked sample (counties with tracked coverage, (Gentzkow et al., 2011)). Non-South excludes the Confederate 11 (see Table 2 note). State-clustered SEs. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

already served by print; it is silent by construction on the extensive margin, a fixed county trait the design differences out. The primary estimator is non-South IV with three endogenous regressors (radio, radio × illiteracy, radio × circulation per capita), instrumented by the six corresponding geographic-instrument interactions.

Table 8 replicates Table 6's illiteracy-gradient test in the newspaper-tracked subsample and confirms the gradient: the radio level effects are sign-consistent with the main specification, and first-stage F-statistics are healthy throughout. Magnitudes are not directly comparable across the two tables — the tracked subsample is more urban and its instrument compliers differ — so the informative comparison is the within-table one between the two moderators.

The moderator asymmetry is sharp and consistent across estimators: in the non-South, the radio × illiteracy interaction is significant under both OLS and IV, while the radio × circulation interaction is indistinguishable from zero under both — and because both are measured in the same sample under the same specification, this is not a difference in power or identification. Extending the test to the other two newspaper measures, the illiteracy interaction is unchanged (0.08–0.09 per SD), while paper count and total circulation carry small positive interactions about one-eighth the illiteracy gradient (Appendix Table A29). Within the newspaper-tracked sample, the binding constraint radio relaxes is primarily *literacy*, not newspaper density.

The turnout interactions clarify a sign pattern OLS alone would misread: the OLS radio $\times$ illiteracy turnout interaction is positive and significant here, the IV counterpart essentially zero, and the full non-South IV turnout interaction significantly negative (Table 6); the preferred reading is that radio's turnout response along the illiteracy dimension is weak or negative, not positive.[8]

That literacy binds while newspaper density does not is the test that separates information poverty from salience mobilization, which predicts symmetric positive moderation by both (Table 1). Persuasion of print-informed audiences and FDR-specific messaging predict neither moderator to operate; elite signaling is adjudicated by the legislator evidence instead (Section 4.2). Among the voter-side mechanisms, the joint pattern — positive literacy moderation with a null newspaper-density moderator in the same sample — is predicted by information poverty alone.

The argument rests on conjunction. No single result is decisive; each is consistent with more than one mechanism in Table 1. But jointly only information-poverty reduction matches all of them: the illiteracy interaction rules out persuasion of print-informed audiences and strict FDR-specific messaging; the moderator asymmetry rules out salience mobilization; the conjunction of that literacy gradient with the FDR-era concentration, reinforced by the New Deal and farm-distress controls, closes off the milder Roosevelt-era-only version; the asymmetric Republican decline rules out entertainment crowd-out (Section 4.1); and the legislator response runs through elections rather than sitting incum-

[8]The non-South IV illiteracy interaction slightly exceeds its OLS counterpart, the same IV $>$ OLS ordering that appears at the level and at the Table 6 interaction.

bents, pointing away from elite signaling. Radio shifted electoral outcomes by bringing political information to voters the print environment had excluded.

6 Robustness

Each identification concern maps to a dedicated diagnostic: instrument strength (Appendix Table A3 and the results-table notes); the exclusion restriction at the level (pre-radio placebo, single-instrument estimates, economic-geography controls, forest drops, and plausibly-exogenous bounds; Appendix Tables A4, A7, and A14–A16) and at the gradient (Appendix Tables A5 and A17); policy and distress confounders (Appendix Tables A11, A12, A13); inference (both clusterings and weak-IV-robust sets, Appendix Table A10); and sample composition (Table A30; Appendix Tables A31–A32).

State-by-year fixed effects absorb 59–66% of instrument variation at the county level (Table A2) and 79–84% at the district level, so the attenuation between specifications reflects removal of between-state confounding, not power loss.

Results are stable across additional specifications (Appendix Tables A8–A6): OLS estimates are positive, significant, and smaller than IV; the balanced-panel subsample reproduces the coefficient almost exactly (0.251 versus 0.250); and farm-distress controls leave the headline essentially unchanged (Appendix Table A12). Among illiteracy, urbanization, and schooling as competing moderators of the national district-level ideology effect, only illiteracy retains significance (Appendix Table A27), a significance horse race whose sign instability parallels the county-level interaction-versus-split tension and locates in the linear-in-moderator functional form (Appendix E.1).

Two further diagnostics support the design. Hansen J tests of joint validity do not reject in any primary state-by-year specification (p between 0.14 and 0.85), rejecting only for the non-South county-and-year specification ($p = 0.002$), the same between-state variation the placebo implicates (Appendix Table A3). Non-rejection is weak evidence by itself

— the J test has power only against violations that differ between the two instruments — so the woodland-specific probes below, not the J statistic, carry the exclusion-restriction defense. Weak-instrument-robust Anderson–Rubin sets for the headline non-South share effect exclude zero under both clusterings (Appendix Table A10).

Pre-Radio Placebo I estimate reduced-form regressions of the instruments on 1908–1916 presidential outcomes, before commercial broadcasting (Appendix Table A4). Ground conductivity $\times$ time is at most weakly associated with pre-radio Democratic vote share: under the state clustering appropriate to these spatially smooth instruments it is insignificant under both fixed-effect structures (non-South $p = 0.23$ to 0.36), reaching marginal significance only under county clustering ($p \approx 0.03$), and then negative. Woodland $\times$ time predicts pre-radio Democratic share under county-and-year fixed effects ($p < 0.001$), but state-by-year fixed effects absorb it ($p = 0.69$), indicating between-state geography rather than a within-state pre-trend. The marginal pre-trend that remains works *against* the headline: the pre-radio coefficients are negative, opposite the positive treatment-era reduced form, so extrapolating would attenuate not inflate the estimate. The same pattern holds for the interaction instrument: under state-by-year fixed effects neither ground conductivity nor woodland $\times$ time $\times$ illiteracy predicts pre-radio Democratic share, turnout, or Republican votes in the non-South (state $p \geq 0.17$; Appendix Table A5), so the illiteracy gradient in Table 6 is not a pre-existing woodland-by-illiteracy trend; the only pre-radio interaction signals — conductivity on turnout and woodland on Republican votes — surface solely under county-and-year fixed effects, the same between-state variation the level placebo flags. One residual failure sits on the mechanism's own margin: woodland $\times$ time predicts pre-radio non-South turnout even under state-by-year fixed effects (state $p = 0.04$), same-signed with the treatment effect; the turnout results accordingly lean on conductivity, on which they are estimated alone (Appendix Table A7).

Woodland and the Exclusion Restriction Woodland share is the instrument whose exclusion restriction is most open to question: heavily forested counties had distinct extractive economies, lower densities, and rougher terrain, any of which might have shaped interwar political trajectories directly; and because woodland carries the larger within-state first stage (partial R^2 0.15 versus 0.04), the design cannot simply drop it.

Controlling directly for the economic geography woodland proxies leaves the estimates intact: farm, urban, manufacturing, density, and ruggedness measures each interacted with year fixed effects, together with Depression severity (the 1929–1933 collapse in county retail sales), drought, and Dust Bowl exposure interacted with the FDR era, move the headline share only from 0.250 to 0.239, turnout from -0.322 to -0.389, and log Republican votes from -1.07 to -1.01, each still significant at the 0.1% level; the illiteracy interaction attenuates under the saturated economic-structure controls but stays positive and significant in the full battery (Appendix Table A14). Dropping the most woodland-suspect counties strengthens the estimates rather than erasing them: excluding the top woodland quartile raises the share effect to 0.37 and the illiteracy interaction to 2.6, and excluding the Kentucky–West Virginia Appalachian-coalfield cluster leaves the share at 0.33 with every core result significant (Appendix Table A15). A spurious forest-economy artifact would move the estimates the other way.

I bound a residual violation with Conley–Hansen–Rossi plausibly-exogenous intervals, letting woodland $\times$ time carry a direct effect on Democratic share (Appendix Table A16). The share keeps significance only while woodland's hypothesized direct effect stays below about one-fifth of its own reduced-form coefficient, so the share magnitude does lean on woodland, exactly as the conductivity-only attenuation (0.250 to 0.154) already signals. The realignment argument does not: the turnout decline (-0.31) and the Republican-vote decline (-1.48) that carry the demobilization mechanism survive estimation on ground conductivity alone (Appendix Table A7), tolerating an arbitrary woodland violation because they never use the instrument. I therefore rest the mechanism on the conductivity-robust results and read the woodland-dependent Democratic-share magnitude as corroborating rather than load-bearing.

Leave-One-Out by State and Year Refitting the headline non-South specification (Table 2, Panel A, column 2) while dropping each of the 37 non-South states in turn and then each of the six presidential years, all 43 fits remain highly significant ($p < 0.001$); no exclusion flips the sign or removes significance. Dropping a state moves the coefficient only modestly (0.215 to 0.365 around the headline 0.250); larger swings come from dropping a year (0.165 to 0.623), which reweights the within-county time variation, and the shifts that strengthen the estimate are the expected ones (excluding Kentucky, the high-leverage Border South cluster, and the no-radio 1920 baseline).

Subsample Heterogeneity by Region The headline averages over three structurally distinct subsamples. Splitting by Jim Crow severity — Northern Core (no Jim Crow), Border South (slave states that did not secede), and Confederate 11 — and refitting the headline IV on each yields three distinct estimates (Table A30): the Northern Core effect is roughly 2.2 times the headline, the Border South positive and significant but about 45% of it, and the Confederate 11 statistically indistinguishable from zero. The operative scope condition remains the South/non-South distinction, an institutional cliff rather than a demographic gradient: the South was the most illiterate region, so a continuous illiteracy moderator would predict the largest effect there; instead it is null, because one-party Democratic rule left too few Republican voters to disrupt.

7 Conclusion

A new information technology rarely reaches all citizens equally, and its political consequences, for electoral outcomes and representation alike, depend on which population is newly reached and what information it encounters. I examined this through radio in interwar America, where audio bypassed the literacy barrier that had excluded millions from print-based political news.

Radio raised Democratic presidential vote share where the literacy barrier was highest, and in the average county it did so by eroding an inherited Republican allegiance rather than manufacturing new Democratic enthusiasm; weighted by population, the same shock shows the realignment's urban, mobilizing face. The disruption also reached representation, in the bounded form the evidence supports: Democratic seats in high-illiteracy non-South districts passed to more liberal Democrats, electoral selection running through within-party candidate replacement — the last link of the accountability chain moved by replacement, not conversion.

The condition for these effects is an uneven information environment: a new medium must reach a population the existing media had bypassed; in interwar America that dimension was literacy — information poverty. The resulting shock can disrupt habitual voting that the exclusion had sustained: radio disrupted a Republican partisanship inherited from the 1860s and re-economized by the 1896 realignment, producing asymmetric partisan effects whose direction depends on the content of the new information and the economic interests of the newly reached. The mechanism also requires a scope condition, a sufficiently large pool of misaligned voters whose behavior can shift; in the interwar South, one-party Democratic dominance left too few Republican voters for a detectable partisan disruption, even though radio reached Southern households and literacy barriers were severe.

The framework extends to contemporary media through one analytic step the interwar case makes visible. Every medium has access, response, and content components, and which of them carries its political consequences is fixed by the technology's affordance; radio isolates the access component almost alone. In the media that followed, the binding axis moved on — toward who *responds*, then toward what each audience *encounters* — and personalized large language models push it further still, toward information environments individualized to each user. What generalizes is the diagnostic: identify the dimension a technology leaves uneven, then the population it differentially reaches along it.

Several limitations bound the interpretation. My county-level design identifies aggregate effects, not individual vote choices: the decomposition is consistent with some Republicans switching and others withdrawing, but I cannot observe individual transitions. I observe set-ownership shares rather than listening, so selection into ownership among the poor, mitigated but not eliminated by communal listening and the 1930s collapse in set prices, remains unobservable. And the findings are specific to one country, era, and medium.

What remains constant is the core insight: the political consequences of a new information technology depend on whom it reaches and the information environment they already inhabit, not on its content alone. When a new medium breaks through the barriers that sustained an uneven information environment, it does not merely add a voice to the conversation — it disrupts the political equilibrium that unevenness itself helped maintain.

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Appendix

A Data and Summary Statistics

Table A1 reports means and standard deviations for every variable entering the analysis, separately for the South and non-South.

B Instrument Validity and the First Stage

Identification rests on the residual within-state variation in the two geographic instruments. The tables in this section show that this residual is a small share of the raw instrument variance yet still yields a strong first stage (Table A2), that the per-instrument first-stage coefficients and the overidentification tests behave as expected (Table A3), and that the within-state design removes the instruments' association with pre-radio (1908–1916) presidential Democratic vote share in the non-South under the state-clustered inference appropriate to these instruments, with the one residual exception being woodland share's association with non-South turnout (Table A4).

The mechanism claim in Table 6 rests on the radio$_\times$illiteracy *interaction*, so its identifying instruments are the two geographic instruments interacted with county illiteracy. Table A5 subjects those interaction instruments to the same pre-radio test: reduced-form regressions of 1908–1916 outcomes on all four instrument terms. Under the primary state-by-year fixed effects (Panel B), none of the interaction terms predicts pre-radio Democratic share, turnout, or Republican votes in the non-South, so the illiteracy gradient is not a pre-existing woodland-by-illiteracy trend. The only pre-radio interaction signals — conductivity$_\times$time$_\times$illiteracy on turnout (the sole term significant under state clustering) and woodland$_\times$time$_\times$illiteracy on Republican votes (county $p < 0.001$; marginal under state clustering) — appear solely under county-and-year fixed effects (Panel A), the same between-state variation the level placebo flags and the within-state design removes.

Table A1: Summary Statistics

Variable	N	Mean	SD	Min	Max
Full sample					
Pres. Dem vote share	18,322	0.565	0.235	0.033	1.000
Pres. turnout (fraction)	18,286	0.534	0.244	0.000	1.000
Radio ownership (fraction)	18,322	0.311	0.280	0.000	0.985
Illiteracy rate	18,322	0.045	0.049	0.000	0.441
Urbanization	18,322	0.210	0.250	0.000	1.000
Black population share	18,322	0.111	0.186	0.000	0.908
Log population	18,322	9.821	1.030	4.205	15.218
Ground conductivity	18,322	9.166	8.062	0.500	30.000
Woodland share	18,322	0.062	0.078	0.000	0.424
Non-South					
Pres. Dem vote share	11,693	0.451	0.171	0.033	1.000
Pres. turnout (fraction)	11,677	0.689	0.118	0.196	1.000
Radio ownership (fraction)	11,693	0.381	0.294	0.000	0.985
Illiteracy rate	11,693	0.023	0.026	0.000	0.397
Urbanization	11,693	0.242	0.259	0.000	1.000
Black population share	11,693	0.019	0.045	0.000	0.491
Log population	11,693	9.860	1.102	5.485	15.218
Ground conductivity	11,693	10.468	8.276	0.500	30.000
Woodland share	11,693	0.034	0.052	0.000	0.383
South					
Pres. Dem vote share	6,629	0.765	0.197	0.060	1.000
Pres. turnout (fraction)	6,609	0.260	0.150	0.000	1.000
Radio ownership (fraction)	6,629	0.188	0.201	0.000	0.965
Illiteracy rate	6,629	0.084	0.054	0.000	0.441
Urbanization	6,629	0.153	0.223	0.000	1.000
Black population share	6,629	0.275	0.223	0.000	0.908
Log population	6,629	9.752	0.885	4.205	13.179
Ground conductivity	6,629	6.869	7.111	1.000	30.000
Woodland share	6,629	0.110	0.091	0.000	0.424

Note: Canonical presidential estimation sample (1920–1940; county-years with non-missing outcome, radio, both instruments, and controls), closely matching the regression tables: N here is the raw count of county-years in the estimation sample, one to five observations larger per cell than the regression N (18,317 / 11,689 / 6,628 for vote share; 18,281 / 11,673 / 6,608 for turnout), which additionally drops fixed-effect singletons. Turnout is capped at 1.0 (39 county-years; see footnote 3 in the main text), so the values describe the variable as used in estimation. Radio ownership comes from the 1930 and 1940 Censuses (the first to ask about radio sets) and is linearly interpolated; the 1920 anchor is zero, as commercial broadcasting began only in November 1920. Illiteracy rate is time-varying (1920 Census through the 1924 election; 1930 Census from 1926 onward). South is the eleven former Confederate states. Ground conductivity and woodland share are time-invariant geographic instruments from Stromberg (2004); one county carried a source woodland-share value exceeding 1 (a coding artifact in the inherited replication data), which we set to missing, so it does not enter any woodland-instrumented specification.

Table A2: Instrument Variance Decomposition

Instrument	Var (Cty+Yr resid)	Var (St×Yr resid)	Marginal absorbed	Total R^2 (St×Yr)
Ground conductivity × time	2,891	1,197	59%	0.91
Woodland share × time	0.282	0.095	66%	0.92

Note: Variance of instrument residuals after partialling out fixed effects (House election sample). "Marginal absorbed" is the fraction of post-(county-and-year) residual variance additionally absorbed by state-by-year FE. "Total R^2 (St×Yr)" is the fraction of raw instrument variance absorbed by county and state-by-year FE jointly — only the residual 8–9% drives identification, yet first-stage F-statistics remain above 600.

Table A3: First-Stage Coefficients, Instrument Strength, and Overidentification

	Full		Non-South		South	
	S×Y	C+Y	S×Y	C+Y	S×Y	C+Y
Conductivity × time (×10^3)	0.174***	0.000	0.145***	−0.064***	0.206***	0.254***
	(0.017)	(0.018)	(0.017)	(0.016)	(0.040)	(0.037)
Woodland × time	−0.029***	−0.062***	−0.056***	−0.057***	−0.013***	−0.003
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Partial R^2: conductivity	0.033	0.042	0.040	0.006	0.025	0.060
Partial R^2: woodland	0.060	0.198	0.153	0.118	0.017	0.023
Conventional first-stage F	685	1,885	1,000	671	109	178
Kleibergen–Paap rk Wald F	147	565	274	138	25	44
Hansen J p-value	0.847	0.846	0.24	0.002	0.219	0.554
N	18,317	18,317	11,689	11,689	6,628	6,628

Note: First-stage regressions of interpolated radio ownership on the two excluded instruments, with controls and the indicated fixed effects, on the presidential vote-share sample. SE clustered at the county level; conductivity coefficients and SEs scaled by 10^3. Partial R^2 is each instrument's incremental within-FE R^2 over the controls-only first stage. "Conventional first-stage F" is the homoskedastic F-test of the excluded instruments (the statistic in the body tables); the Kleibergen–Paap row is its county-cluster-robust Wald equivalent. The first-stage regressions run on the pre-singleton-drop samples (N = 18,322/11,693/6,629); the N row reports the matching second-stage samples. The Hansen J row reports two-step GMM overidentification tests (county-clustered): joint validity of the two instruments is not rejected in any state-by-year specification, and is rejected only in the non-South county-and-year cell ($p = 0.002$) — the same between-state variation flagged by the pre-radio placebo (Table A4) — consistent with the within-state design as primary; for the other primary outcomes under state-by-year FE it does not reject (p between 0.14 and 0.85). $^+p < 0.10$, $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

C Robustness of the Headline Estimate

The non-South presidential vote-share effect is stable across the choices that most plausibly threaten it: using both within- and between-state variation rather than only within-state variation (Table A6), dropping the woodland instrument (Table A7), substituting milder state-specific time trends for saturated state-by-year effects (Table A8), decomposing the effect election-by-election (Table A9), clustering at the state rather than the county

Table A4: Pre-Radio Placebo: Reduced-Form Instrument Effects, 1908–1916

	Dem Share		Turnout	
	Full	Non-South	Full	Non-South
Panel A: County and Year FE				
Conductivity × time	−0.000065	−0.000061	−0.000184	−0.000259
	(0.000027)*	(0.000029)*	(0.000042)***	(0.000049)***
	[0.000067]	[0.000066]	[0.000137]	[0.000137]$^{+}$
Woodland × time	−0.001	−0.030	0.006	−0.012
	(0.004)	(0.004)***	(0.004)	(0.006)*
	[0.015]	[0.009]***	[0.013]	[0.012]
Panel B: County and State × Year FE				
Conductivity × time	−0.000093	−0.000086	0.000015	−0.000034
	(0.000035)**	(0.000040)*	(0.000054)	(0.000069)
	[0.000050]$^{+}$	[0.000071]	[0.000072]	[0.000104]
Woodland × time	0.007	−0.003	−0.010	−0.033
	(0.003)*	(0.003)	(0.007)	(0.017)$^{+}$
	[0.005]	[0.006]	[0.012]	[0.016]*
Controls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
N	8,744	5,579	8,566	5,437

Note: Reduced-form regressions of pre-radio outcomes (1908, 1912, 1916 presidential elections) on the instruments interacted with a linear time trend anchored at 1908; because the instruments are time-invariant, county fixed effects render the coefficients invariant to the anchor year. Controls as in Table 2, interacted with time. Each coefficient carries two SEs — county-clustered in parentheses, state-clustered in brackets, stars attached separately. Because the instruments are spatially smooth geographic fields × time, state clustering is the appropriate inference (see Table A10 note); county clustering is shown for comparison. Panel A uses county and year FE; Panel B replaces year with state-by-year FE. Under state clustering, conductivity × time passes the non-South placebos with a negative (headline-opposing) sign; woodland × time passes the non-South Democratic-share placebo under state-by-year FE but fails the non-South turnout placebo there (state $p = 0.04$), a limitation the conductivity-only estimates in Table A7 address. Democratic-share and turnout Ns differ because turnout is missing for some county-years. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

level (Table A10), and controlling for New Deal spending (Table A11) and farm distress (Table A12).

C.1 Confronting the Woodland Instrument

Woodland share carries the larger within-state first stage yet is the instrument whose exclusion restriction is most open to question (Section 6). Tables A14–A16 confront it directly. Table A14 adds the economic geography woodland proxies — farm, urban, manu-

Table A5: Interaction Pre-Radio Placebo: Reduced-Form Instrument-×-Illiteracy Effects, 1908–1916 (Non-South)

	Dem Share	Turnout	Log Rep
Panel A: county and year FE			
Conductivity × time × illit	0.0023	0.0095*	0.0239
	(0.0033)	(0.0038)	(0.0204)
Woodland × time × illit	0.165	−0.283	1.827^{+}
	(0.227)	(0.364)	(1.060)
Panel B: county and state-by-year FE			
Conductivity × time × illit	0.0028	0.0026	−0.0021
	(0.0027)	(0.0030)	(0.0073)
Woodland × time × illit	0.090	−0.451	0.386
	(0.127)	(0.325)	(0.569)
N	5,579	5,437	5,579

Note: Reduced-form regressions of pre-radio non-South presidential outcomes (1908, 1912, 1916) on the four interaction instruments (ground conductivity and woodland, each × time and × time × 1920 illiteracy), with controls × time and × time × illiteracy absorbed. State-clustered standard errors in parentheses (the appropriate inference for spatially smooth instruments × time; see Table A4 note); the level terms mirror Table A4 and are omitted for space. Under state clustering the sole term reaching significance is conductivity × time × illiteracy on pre-radio turnout under county-and-year FE, absorbed by state-by-year FE; woodland × time × illiteracy on Republican votes is significant only under county clustering (Panel A, $p < 0.001$), the between-state signal the primary design removes. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

facturing, population-density, and terrain-ruggedness measures × year fixed effects, plus Depression severity (the 1929–1933 retail-sales collapse), drought, and Dust Bowl exposure × FDR era — and the headline level effects barely move, while the illiteracy interaction attenuates under the saturated structural controls but stays significant. Table A15 drops the most woodland-suspect counties; the estimates strengthen. Table A16 reports Conley–Hansen–Rossi plausibly-exogenous bounds, which locate precisely where the design is fragile: the Democratic-share *magnitude* tolerates only a small woodland direct effect, whereas the turnout and Republican-count results survive on ground conductivity alone (Table A7) and so need no woodland assumption at all. Table A17 extends the same confrontation to the illiteracy interaction: the share gradient does not survive on conductivity alone, while the turnout and Republican-count gradients do.

Table A6: County-and-Year Fixed Effects: Vote Share and Turnout

	(1) Full	(2) Non-South	(3) South
Panel A: Presidential Dem vote share			
Radio	0.452***	0.995***	0.289**
	(0.024)	(0.078)	(0.088)
First-stage F	1,885	671	178
N	18,317	11,689	6,628
Panel B: House Dem vote share			
Radio	0.182***	0.647***	0.025
	(0.029)	(0.074)	(0.101)
First-stage F	3,211	1,204	367
N	34,315	21,717	12,598
Panel C: Presidential turnout (fraction)			
Radio	0.319***	0.293***	−0.536***
	(0.019)	(0.045)	(0.103)
First-stage F	1,883	671	178
N	18,281	11,673	6,608
Panel D: House turnout (fraction)			
Radio	0.311***	0.271***	0.330***
	(0.013)	(0.032)	(0.060)
First-stage F	3,261	1,136	366
N	34,904	22,321	12,583
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Note: IV estimates with county and year fixed effects, using both within- and between-state variation in radio exposure. Controls, instruments, and clustering as in Table 2, but with year (not state-by-year) FE. The substantially larger coefficients relative to the state-by-year specification (Tables 2 and 3) reflect between-state confounding: favorable-reception states were also more developed, urbanized, and competitive. The positive turnout estimates (Panels C and D) reverse under state-by-year FE, indicating the baseline turnout effect is between-state, not causal. Panel C turnout is capped at 1.0 as in all turnout regressions; Panel D is total House votes divided by county population. Ns differ by one or two observations across FE structures because singleton cells are dropped. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

D Vote-Count Decomposition and the Demobilization Mechanism

The share-level effect decomposes into a sharp decline in Republican vote counts with no offsetting Democratic gain. This section states the accounting identity behind that pattern and reports the decomposition across fixed-effect structures, baseline partisan competi-

Table A7: Single-Instrument IV Estimates: Ground Conductivity Only

	Combined IV		Conductivity Only		
	Coef (SE)	F-stat	Coef (SE)	F-stat	N
Presidential Dem Share					
Full, County+Year	0.452*** (0.024)	1,885	0.441*** (0.058)	672	18,317
Full, State×Year	0.092+ (0.051)	685	0.080 (0.081)	511	18,317
Non-South, County+Year	0.995*** (0.078)	671	1.834*** (0.358)	61	11,689
Non-South, State×Year	0.250*** (0.040)	1,000	0.154 (0.094)	392	11,689
Presidential Turnout					
Non-South, State×Year	−0.322*** (0.037)	999	−0.306*** (0.070)	392	11,673
Vote Decomposition (Non-South, State×Year)					
Log Dem	0.039 (0.170)	1,000	−0.377 (0.383)	392	11,689
Log Rep	−1.069*** (0.116)	1,000	−1.476*** (0.320)	392	11,689

Note: IV estimates comparing the combined-instrument specification (conductivity × time and woodland × time) with conductivity × time alone, both on the common canonical sample requiring both instruments non-missing, so Ns match the main tables. SE clustered at county level; conventional first-stage F on the excluded instrument(s). The core findings survive dropping woodland: the Republican-count and turnout declines are similar across columns. The presidential Dem share estimate under state-by-year FE attenuates from 0.250 to 0.154 and loses significance with the single instrument; the conductivity-only interval is consistent with the combined estimate but does not establish the headline share effect on its own. First-stage F exceeds conventional thresholds throughout (weakest 61, non-South county-and-year). $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A8: Robustness: State-Specific Linear Time Trends

	County + Year	State Trends	State × Year
Panel A: Full sample			
Pres Dem Share	0.452***	0.121*	0.092+
House Dem Share	0.182***	0.101+	0.103+
Panel B: Non-South			
Pres Dem Share	0.995***	0.256***	0.250***
House Dem Share	0.647***	0.196***	0.181**

Note: IV estimates under county clustering. The State Trends column adds state-specific linear time trends to the county-and-year specification; the rightmost column instead uses saturated state-by-year fixed effects. The milder state-trend control reproduces the state-by-year estimate almost exactly in the non-South (0.256 vs 0.250 for the presidential effect) and lands close to it in the full sample (0.121 vs 0.092), indicating that the attenuation from county-and-year to state-by-year fixed effects reflects the removal of trending between-state confounders rather than power loss. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

tion, and county urbanization.

Table A9: Year-by-Year OLS Estimates: Radio and Presidential Dem Share

Election Year	Cumulative Radio Coef	President
1928	0.449	Hoover (R)
1932	0.303	FDR (D)
1936	0.370	FDR (D)
1940	0.173	FDR (D)

Note: OLS with county and year fixed effects. Each row is the within-county radio effect on presidential two-party Democratic vote share in that election year (cumulative interpolated radio ownership × election-year indicators). The 1924 election is omitted because radio ownership was still near zero. Full sample ($N = 18{,}321$, estimated on the common instrument sample so N matches the IV tables). Under county and year FE (which retain between-state variation) the strongest single-year effect is in 1928; under state-by-year FE the within-state pattern shifts toward the FDR era (Table 7), illustrating the sensitivity of temporal decomposition to specification. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A10: Headline Level Estimates Under County- and State-Clustered Standard Errors

Outcome	Sample	Coef	SE (county)	SE (state)
Pres Dem share	Full	0.092	(0.051)$^{+}$	(0.112)
	Non-South	0.250	(0.040)***	(0.103)*
	South	−0.109	(0.132)	(0.317)
House Dem share	Full	0.103	(0.061)$^{+}$	(0.112)
	Non-South	0.181	(0.056)**	(0.100)$^{+}$
	South	−0.020	(0.121)	(0.169)
Pres turnout	Full	−0.125	(0.042)**	(0.105)
	Non-South	−0.322	(0.037)***	(0.062)***
	South	0.192	(0.109)$^{+}$	(0.149)
House turnout	Full	−0.032	(0.029)	(0.078)
	Non-South	−0.173	(0.034)***	(0.078)*
	South	0.138	(0.060)*	(0.086)
Log Dem votes	Non-South	0.039	(0.170)	(0.526)
Log Rep votes	Non-South	−1.069	(0.116)***	(0.138)***
Log Total votes	Non-South	−0.438	(0.071)***	(0.181)*

Note: Every cell re-estimates the corresponding state-by-year IV level specification from Tables 2–4 and reports both clustering levels; stars attach to each SE separately. Because the instruments are spatially smooth geographic fields × time, state clustering is the more conservative inference. The substantive core survives it: the non-South presidential share effect stays significant at 5%, and the turnout and Republican-count declines stay significant at 0.1%. The full-sample share estimates — supplementary throughout, since the scope condition makes the non-South operative — lose significance under state clustering. Weak-instrument-robust Anderson–Rubin 95% confidence sets for the non-South share effect are $[0.17, 0.34]$ (county) and $[0.01, 0.51]$ (state), both excluding zero. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A11: Robustness: Illiteracy Gradient Controlling for New Deal Spending

	(1) Baseline (full)	(2) Fishback subsample	(3) + Total NDEXP	(4) + Relief + AAA
Radio	0.291**	0.278*	0.272*	0.289**
	(0.107)	(0.104)	(0.103)	(0.101)
Radio × Illiteracy	1.620*	1.538*	1.566*	1.632*
	(0.635)	(0.624)	(0.623)	(0.606)
NDEXP per capita × FDR era			6.5×10^{-5}**	
			(2.3×10^{-5})	
County FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
N	11,689	11,653	11,653	11,653

Note: IV estimates, non-South sample, county and state-by-year fixed effects. The endogenous variables (radio and radio × illiteracy) are instrumented by ground conductivity × time, woodland × time, and their interactions with county illiteracy; SE clustered at the state level. Column (1) replicates the non-South specification of Table 6, Panel A, column (1) on the canonical sample. Column (2) restricts to the Fishback et al. (2003) New Deal expenditure subsample (1,950 of 1,956 non-South counties matched); columns (3)–(4) add total New Deal expenditure per capita × post-1932, and relief and AAA spending × post-1932 separately. The radio × illiteracy coefficient moves by at most about 6% across specifications, so the illiteracy gradient is not attributable to FDR-era policy targeting of high-illiteracy counties; the spending terms themselves predict Democratic vote share, so the design controls for rather than ignores the returns to New Deal spending. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

D.1 A Formal Accounting Framework

Each voting-age citizen has a type $\theta \in \{L, R\}$ (true policy alignment) and information $\sigma \in \{0, 1\}$ (whether they know their type). The informed ($\sigma = 1$) vote their type; the uninformed ($\sigma = 0$) vote by habit, independent of type: fractions v_D, v_R, and $1 - v_D - v_R$ vote Democratic, Republican, and abstain, respectively. Let P_U denote the uninformed population and α the fraction of type-L citizens within it. In the interwar non-South the uninformed were disproportionately type-L, so $\alpha > 0.5$, and habitual voting leaned Republican, so $v_R \geq v_D$.

With these primitives, the key misaligned groups are type-L citizens voting Republican by habit, $R_L = \alpha \cdot v_R \cdot P_U$, and type-$R$ citizens voting Democratic by habit, $D_R = (1 - \alpha) \cdot v_D \cdot P_U$. Because $\alpha > 0.5$ and $v_R \geq v_D$, it follows that $R_L > D_R$.

Radio reveals θ to uninformed citizens, inducing six flows: mobilization of left non-

Table A12: Robustness: Farm-Distress Controls

	(1) Baseline (full)	(2) Fishback subsample	(3) + Farm share × year FE	(4) + Drought, Dust Bowl × FDR	(5) + All
Panel A: Headline level effect (Pres Dem share)					
Radio	0.250	0.237	0.311	0.239	0.308
SE (county)	(0.040)***	(0.040)***	(0.042)***	(0.040)***	(0.042)***
SE (state)	(0.103)*	(0.100)*	(0.104)**	(0.102)*	(0.104)**
First-stage F	1,000	997	924	984	923
Panel B: Illiteracy interaction (state-clustered)					
Radio	0.291**	0.278**	0.335**	0.280**	0.334**
	(0.107)	(0.104)	(0.106)	(0.105)	(0.106)
Radio × Illiteracy	1.620*	1.538*	1.292*	1.603*	1.360*
	(0.635)	(0.624)	(0.542)	(0.640)	(0.550)
County FE	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes
N	11,689	11,653	11,653	11,653	11,653

Note: IV estimates, non-South sample (Confederate-11), county and state-by-year fixed effects. Farm-distress measures come from the Fishback et al. (2003) county files (farm-land share, 1930s drought months, a Dust Bowl indicator); 1,950 of 1,956 non-South counties match. Column (3) interacts standardized farm-land share with election-year FE; column (4) interacts drought and Dust Bowl with the FDR-era indicator; column (5) includes all. Panel A reports the single-endogenous headline specification with both clustering levels (stars attach to each SE separately); Panel B reports the two-endogenous illiteracy-interaction specification of Table 6 with state-clustered SE. The headline effect is essentially unchanged or slightly larger under these controls, and the illiteracy interaction moves by at most 16% relative to the matched baseline while remaining significant. These controls absorb farm *exposure* and 1930s drought shocks; a time-varying county farm-income measure would be needed to control distress directly and remains for future work. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

voters (a) and right non-voters (b); demobilization of misaligned Democrats (c) and misaligned Republicans (e); and switching by misaligned Democrats to Republican (d) and misaligned Republicans to Democratic (f).

Table A13: FDR-Era Concentration Under Depression-Geography Controls

	(1) Baseline (full)	(2) Fishback subsample	(3) + Drought, Dust Bowl × FDR	(4) + Farm share × year FE	(5) + Retail collapse × FDR	(6) + New Deal spending × FDR	(7) Full battery
Panel A: Presidential Dem share							
Radio (pre-FDR base)	−0.134	−0.159	−0.160	−0.165	−0.159	−0.157	−0.161
	(0.186)	(0.182)	(0.182)	(0.212)	(0.182)	(0.183)	(0.213)
Radio × FDR era	0.308**	0.320**	0.325**	0.387**	0.317**	0.338**	0.384**
	(0.102)	(0.102)	(0.100)	(0.118)	(0.104)	(0.103)	(0.122)
First-stage F (radio; interaction)	526; 674	523; 668	517; 654	480; 615	450; 532	505; 626	421; 500
Panel B: Presidential turnout							
Radio (pre-FDR base)	−0.178	−0.175	−0.176	−0.166	−0.175	−0.175	−0.163
	(0.108)	(0.110)	(0.110)	(0.127)	(0.109)	(0.110)	(0.127)
Radio × FDR era	−0.115*	−0.122*	−0.120*	−0.117+	−0.141*	−0.116+	−0.128+
	(0.058)	(0.059)	(0.057)	(0.067)	(0.060)	(0.059)	(0.069)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N (Panel A / B)	11,689 / 11,673			11,641 / 11,625			

Note: Joint-interaction era IV of Table 7 (radio and radio × FDR-era endogenous, four instruments), non-South sample, county and state-by-year fixed effects, state-clustered SEs. Column (2) restricts to the Fishback-matched subsample of Table A12; columns (3)–(7) add Depression-geography controls to that subsample: drought months and Dust Bowl exposure × FDR era, standardized farm-land share × election-year FE, the 1929–1933 county retail-sales collapse × FDR era, and county New Deal spending (total, relief, and AAA per capita) × FDR era. The FDR-era concentration on Democratic share is stable at +0.32 to +0.39 ($p \leq 0.003$) in every column, so the concentration is not an artifact of Depression-distress geography; the turnout analogue softens to $p = 0.05$–0.08 only where farm-share × year FE inflate the SE, with the point estimate stable. Per-endogenous conventional first-stage F reported. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A14: Economic-Geography Controls: The Woodland-Trajectory Confound

	(1) Baseline (full NS)	(2) Fishback matched	(3) + Econ-structure	(4) + Depression shocks	(5) + All
Panel A: headline level effects (county-clustered)					
Pres Dem share	0.250***	0.239***	0.256***	0.220***	0.239***
	(0.040)	(0.040)	(0.047)	(0.044)	(0.048)
Turnout	−0.322***	−0.328***	−0.376***	−0.333***	−0.389***
	(0.037)	(0.038)	(0.050)	(0.043)	(0.053)
Log Rep votes	−1.069***	−1.069***	−1.034***	−1.039***	−1.014***
	(0.116)	(0.117)	(0.147)	(0.131)	(0.156)
Panel B: illiteracy interaction (state-clustered)					
Radio	0.291**	0.281**	0.276*	0.250*	0.254*
	(0.107)	(0.105)	(0.107)	(0.109)	(0.103)
Radio × Illiteracy	1.620*	1.551*	0.949+	1.592*	1.027*
	(0.635)	(0.628)	(0.494)	(0.625)	(0.486)
County FE	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes
N	11,689	11,641	11,641	11,641	11,641

Note: IV estimates, non-South (Confederate-11), county and state-by-year fixed effects. Column (1) is the canonical baseline; (2) restricts to the Fishback et al. (2003)-matched sample (9 of 1,957 non-South counties unmatched). "Econ-structure" = standardized farm-land share, urban share, manufacturing employment, log population (density), and elevation range, each interacted with election-year fixed effects. "Depression shocks" = standardized 1929–1933 retail-sales change, drought months, and Dust Bowl exposure, each interacted with the FDR-era indicator. Panel A reports the single-endogenous level specifications (county-clustered SE, the primary inference for levels); Panel B the two-endogenous illiteracy interaction (state-clustered SE). The level effects are essentially unchanged; the interaction attenuates under the saturated economic-structure controls (column 3) but stays significant, and the full battery (column 5) leaves it at 1.03. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A15: High-Forest and Appalachian-Coalfield Drop Robustness

	Pres Dem Share	Turnout	Log Rep votes	Radio × Illit	N
Full non-South (reference)	0.250***	−0.322***	−1.069***	1.620*	11,689
Drop top woodland quartile	0.370***	−0.159*	−1.249***	2.629*	8,751
Drop top woodland tercile	0.575***	−0.199*	−2.032***	3.621*	7,779
Drop KY + WV (Appalachian)	0.327***	−0.278***	−1.045***	2.249**	10,639

Note: Each row refits the headline IV on the indicated subsample. The first three columns are single-endogenous level effects (Pres Dem share, turnout, log Republican votes) with county-clustered significance; the fourth is the radio × illiteracy interaction (state-clustered). Woodland cut points are the non-South analytic-sample tercile (0.028) and quartile (0.041) of woodland share; the Appalachian-coalfield row drops Kentucky and West Virginia. Dropping the most-forested counties moves every estimate *away* from zero. The trade-off is first-stage power: the conventional first stage falls from 1,000 (full) to 184 (drop quartile) and 105 (drop tercile), so state-clustered inference on the woodland-drop rows is underpowered (share state $p > 0.10$ there) even as county-clustered significance and the point estimates hold; the Appalachian-coalfield drop retains a strong first stage (F=520) and significance under both clusterings. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A16: Conley–Hansen–Rossi Plausibly-Exogenous Bounds: Woodland Direct Effect on Democratic Share

γ_W/ρ_W	γ_W	$\widehat{\theta}$	SE	95% CI	Sig.
0.00 (exclusion holds)	0.0000	0.250	0.103	$[0.047,\ 0.452]$	yes
0.25	−0.0039	0.192	0.104	$[-0.012,\ 0.396]$	no
0.50	−0.0078	0.134	0.105	$[-0.071,\ 0.339]$	no
0.75	−0.0117	0.076	0.106	$[-0.131,\ 0.283]$	no
1.00	−0.0156	0.019	0.107	$[-0.191,\ 0.228]$	no
1.50	−0.0234	−0.097	0.110	$[-0.313,\ 0.118]$	no

Note: Conley–Hansen–Rossi (2012) union-of-confidence-interval bounds for the non-South Democratic-share estimate θ, allowing woodland $\times$ time a direct effect γ_W on the outcome. For each fixed γ_W, the adjusted outcome (Dem share $-$ $\gamma_W \times$ woodland $\times$ time) is re-estimated by IV using both instruments, state-clustered. γ_W is expressed as a multiple of woodland's own reduced-form coefficient $\rho_W = -0.0156$. The breakdown value at which the 95% interval first admits zero is $\gamma_W^* = 0.20\,\rho_W$: the share magnitude tolerates only a small woodland direct effect, consistent with the conductivity-only attenuation (0.250 to 0.154, Table A7). By contrast the turnout (-0.306) and Republican-count (-1.476) results are estimated on conductivity alone in Table A7 and so hold for *any* γ_W. γ_W and the pre-radio placebo coefficients (Table A4) are not on a common numeric scale (pre-period and treatment-period time trends differ), so the placebo bears on the *sign/significance* of a woodland pre-trend, not on the numeric size of γ_W^*.

Table A17: Conductivity-Only IV: The Illiteracy Interaction on the Clean Instrument

	Pres Dem Share	Pres Turnout	Log Rep Votes
Panel A: two-instrument baseline (conductivity + woodland families)			
Radio	0.291^{**}	-0.327^{***}	-1.116^{***}
	(0.107)	(0.060)	(0.118)
Radio × Illiteracy	1.620^{*}	-1.421^{**}	-4.395^{***}
	(0.635)	(0.542)	(1.181)
Panel B: conductivity only (just-identified)			
Radio	0.210	-0.345^{*}	-1.732^{*}
	(0.152)	(0.156)	(0.814)
Radio × Illiteracy	0.810	-1.632^{*}	-7.380^{*}
	(0.736)	(0.825)	(2.991)
SW conditional F (radio; interaction)	204; 182	204; 182	204; 182
County FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
N	11,689	11,673	11,689

Note: Two-endogenous interaction IV (radio, radio × illiteracy) on the non-South sample with county and state-by-year fixed effects and state-clustered SEs, as in Table 6. Panel A instruments with both geographic families (four instruments); Panel B drops the woodland family entirely, identifying just-identified off ground conductivity × time and its illiteracy interaction. The share gradient does not survive on the clean instrument alone (1.62 → 0.81, n.s.), while the turnout and Republican-count gradients do — matching the level-effect pattern (Table A7): the demobilization gradient is woodland-independent, and the share gradient is woodland-dependent at the gradient margin as at the level. A Conley–Hansen–Rossi analysis of the interaction (the analogue of Table A16) points the same way with a twist: the reduced-form woodland × time × illiteracy coefficient is +0.306 (state SE 0.114), same-signed as the interaction, so granting woodland a same-signed direct interaction effect *strengthens* the estimate; the interaction's 95% CI includes zero only if woodland's direct effect is opposite-signed and at least 0.41 times its own reduced form. SW = Sanderson–Windmeijer conditional F, state-clustered, per endogenous regressor. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

The observable changes are:

$$\Delta D = a + f - c - d \tag{3}$$

$$\Delta R = b + d - e - f \tag{4}$$

$$\Delta T = a + b - c - e \tag{5}$$

The empirical finding $\Delta D \approx 0$ does not imply that no flows affected Democratic votes. Rather, it implies approximate cancellation: inflows (mobilization of left non-voters a and switching from misaligned Republicans f) roughly offset outflows (demobilization c and switching d of right-leaning habitual Democrats who discover their misalignment). The large $|\Delta R|$ reflects the structural asymmetry $R_L \gg D_R$: many more voters were misaligned on the Republican side.

Because the system has four degrees of freedom (six unknowns, two independent equations), individual flows cannot be identified from aggregate data. Three features do follow directly from the signs and magnitudes of the estimated aggregate changes: (1) total Republican exits (demobilization e plus switching f) exceed Republican-side inflows ($b + d$) by the magnitude of the Republican count decline, which is large; (2) net demobilization exceeds net mobilization ($c + e > a + b$), which is what the turnout decline expresses; and (3) Democratic-side inflows and outflows approximately cancel. The aggregate moments alone cannot apportion Republican exits between demobilization and switching. The auxiliary evidence on baseline competition (Appendix Table A20) constrains the solution further: detectable switching ($f > 0$) appears only in safe Republican counties where R_L is largest, while in competitive counties the Republican decline arrives without Democratic gains, pointing to demobilization as the dominant exit channel there.

Table A18: Vote Count Decomposition by Fixed Effects Specification (Non-South)

	Log Dem	Log Rep	Dem Share	Turnout
Panel A: County and Year FE				
Radio	3.577***	−2.229***	0.995***	0.293***
	(0.273)	(0.348)	(0.078)	(0.045)
First-stage F	671	671	671	671
N	11,689	11,689	11,689	11,673
Panel B: County and State × Year FE				
Radio	0.039	−1.069***	0.250***	−0.322***
	(0.170)	(0.116)	(0.040)	(0.037)
First-stage F	1,000	1,000	1,000	999
N	11,689	11,689	11,689	11,673
County FE	Yes	Yes	Yes	Yes

Note: IV estimates, non-South sample. Controls, instruments, and significance conventions as in Table 2. Standard errors clustered at county level in parentheses. Under county-and-year fixed effects, which use both between-state and within-state instrument variation, the positive turnout estimate is driven by large increases in Democratic votes across states (3.58) alongside a large Republican decline (−2.23). Under state-by-year fixed effects, which restrict identification to within-state variation, the pattern shifts: Democratic votes are unchanged while Republican votes decline sharply (−1.07), producing a net turnout decline. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

D.2 Decomposition Estimates

The asymmetry between an essentially unchanged Democratic count and a sharply lower Republican count holds across fixed-effect structures (Table A18, with the South/non-South contrast in Table A19) and is concentrated in safe-Republican (Table A20) and, geographically, rural counties (Table A21).

Vote count decomposition by urbanization. The demobilization channel is concentrated in rural and small-town counties. Splitting the non-South at the median of baseline (1920) county urbanization and re-estimating the main IV decomposition in each half (Appendix Table A21), the rural half — the Republican farm belt of the Plains and border North (Kentucky, Missouri, Nebraska, Kansas, the Dakotas) — carries the demobilization signature: radio cut Republican votes, total votes, and turnout, each significant at $p < 0.001$, with no offsetting Democratic gain (the log Democratic coefficient is negative and insignificant at the median, and significantly negative in the bottom urbanization

Table A19: Vote Count Decomposition: South vs. Non-South (State × Year FE)

	Log Dem	Log Rep	Dem Share	Turnout
Panel A: Non-South				
Radio	0.039	-1.069^{***}	0.250^{***}	-0.322^{***}
	(0.170)	(0.116)	(0.040)	(0.037)
First-stage F	1,000	1,000	1,000	999
N	11,689	11,689	11,689	11,673
Panel B: South				
Radio	0.351	0.030	-0.109	0.192^{+}
	(0.375)	(0.827)	(0.132)	(0.109)
First-stage F	109	109	109	110
N	6,628	6,628	6,628	6,608
County FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes

Note: IV estimates with county and state-by-year fixed effects. Controls, instruments, and significance conventions as in Table 2. Standard errors clustered at county level in parentheses. In the non-South, the asymmetry between log Democratic votes (near zero) and log Republican votes (-1.07) drives the vote share increase and turnout decline. In the South, neither party's log vote count moves significantly, consistent with the scope condition: too few Republican voters remained for the information shock to produce detectable aggregate shifts; total Southern votes rise with radio (log total $+1.17$, $p < 0.01$), the same marginally positive participation signal seen in the Southern turnout estimate. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

tercile). The urban half — the industrial states (Ohio, Illinois, New York, Pennsylvania, Michigan) — shows the opposite composition: a positive Democratic vote-count effect with no turnout decline, the mobilization pattern Andersen (1979) documents for the immigrant cities, though estimated imprecisely given the smaller number of urban counties and state clusters. The contrast is robust to tercile-extreme and majority-rural (urb < 0.5) splits. This within-state evidence locates the aggregate decomposition's two channels geographically: Republican demobilization on the rural margin, where habitual Republican loyalties were strongest, and a weaker Democratic mobilization in the cities.

D.3 The New Deal Realignment Debate

The vote-count decomposition speaks directly to one of the oldest questions about the New Deal realignment: whether the Democratic majority was built by converting Republicans or by mobilizing new voters (Key, 1955; Burnham, 1970; Andersen, 1979; Erik-

Table A20: Vote Count Decomposition by Baseline Competition (Non-South, State × Year FE)

	Safe Rep (<40%) $N_{\text{cty}} = 1{,}379$	Competitive (40–60%) $N_{\text{cty}} = 497$	Safe Dem (>60%) $N_{\text{cty}} = 66$
Log Dem	0.930*	−0.273	−0.429*
	(0.445)	(0.167)	(0.187)
Log Rep	−1.522***	−0.838***	−0.195
	(0.262)	(0.172)	(0.316)
Log Total	−0.172	−0.610***	−0.365*
	(0.144)	(0.117)	(0.178)
Dem Share	0.505***	0.136**	−0.050
	(0.106)	(0.047)	(0.060)
First-stage F	212	413	244
N	8,268	2,946	378
County FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes

Note: IV estimates, non-South sample (Confederate-11), split by 1920 baseline Democratic presidential vote share. Controls, instruments, and clustering as in Table 2; conventional first-stage F on the excluded instruments per column. Safe Republican counties show Democratic-vote gains alongside Republican declines, a pattern consistent with switching; competitive counties show Republican declines without Democratic gains, suggesting demobilization predominated. The Safe Dem column rests on only 66 counties and is reported for completeness; its modest negative log Democratic and log total coefficients are consistent with demobilization falling on habitual *Democratic* voters where Democrats dominated, the mirror image of the safe-Republican pattern. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

son and Tedin, 1981; Sundquist, 1983). Andersen (1979) locates it in the mobilization of previously non-voting urban ethnics; Erikson and Tedin (1981) counter that conversion of former Republicans did much of the work. Both explain the Democratic majority as growth in the Democratic vote. My within-state estimates isolate a mechanism neither anticipates: a Democratic-share gain with no growth in Democratic votes at all, produced through the demobilization of habitual Republicans into abstention. Splitting the non-South by baseline urbanization locates this channel (Table A21): in the rural Republican farm belt radio sharply reduced Republican votes and depressed turnout with no offsetting Democratic gain — the demobilization signature, precisely estimated — while in the cities the same Republican decline was offset by rising Democratic votes, the mobilization Andersen (1979) describes, though imprecisely estimated here. Because state-by-year fixed effects absorb the national mobilization wave, my within-county estimates recover

Table A21: Vote-Count Decomposition by County Urbanization (Non-South, IV)

	Rural (low-urb)	Urban (high-urb)	All Non-South
Pres. Dem. share	0.125	0.397^{+}	0.250^{*}
	(0.089)	(0.214)	(0.103)
Log Dem. votes	−0.622	0.989	0.039
	(0.400)	(0.952)	(0.526)
Log Rep. votes	-1.105^{***}	-0.970^{***}	-1.069^{***}
	(0.147)	(0.294)	(0.138)
Log total votes	-0.673^{***}	−0.087	-0.438^{*}
	(0.158)	(0.347)	(0.181)
Turnout	-0.389^{***}	−0.177	-0.322^{***}
	(0.062)	(0.124)	(0.062)
County FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
First-stage F	521	214	1,000
N	5,827	5,844	11,689

Note: Each cell is a separate single-endogenous IV estimate of the radio coefficient, with county and state-by-year fixed effects and controls, non-South sample (Confederate-11). SE clustered at the state level. The sample is split at the non-South median of baseline (1920) county urbanization (0.17); the "All Non-South" column reproduces Table 4. The rural half displays the demobilization signature — a sharp Republican and turnout decline with no Democratic gain; the urban half shows the reverse, a Democratic vote gain without a turnout loss, but less precisely (weaker first stage, fewer state clusters). Tercile-extreme and majority-rural (urb < 0.5) splits give the same contrast, with the rural log-Democratic coefficient turning significantly negative (-0.80, $p < 0.05$) in the bottom urbanization tercile. The turnout rows are estimated on a few county-years fewer than the column N shown, owing to the capped-denominator filter. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

its two within-state margins: a rural Republican demobilization the debate never considered, and an imprecise echo of the urban mobilization it did.

The canonical synthesis did examine the countryside. Sundquist (1983, pp. 199–211) analyzes the rural North through the conversion lens and finds it "experienced no such conversion" as the cities, with a conservative rural countermovement offsetting urban Democratic gains — but identifies no media or information channel. Under the demobilization mechanism my decomposition recovers, a null in net rural realignment is exactly what such a lens should report. Darmofal (2008) likewise maps the rural realignment's geography without its mechanism, leaving open the channel this paper isolates.

Table A22: Population-Weighted versus Unweighted: Headline and Decomposition (Non-South)

	Dem Share	Log Dem	Log Rep	Log Total	Turnout
Panel A: unweighted (county-average estimand; Table 4)					
Radio	0.250	0.039	-1.069	-0.438	-0.322
SE (county)	(0.040)***	(0.170)	(0.116)***	(0.071)***	(0.037)***
SE (state)	(0.103)*	(0.526)	(0.138)***	(0.181)*	(0.062)***
Panel B: population-weighted (person-average estimand)					
Radio	0.650	1.688	-1.436	0.433	-0.019
SE (county)	(0.122)***	(0.417)***	(0.325)***	(0.189)*	(0.070)
SE (state)	(0.310)*	(1.262)	(0.491)**	(0.590)	(0.175)
County FE	Yes	Yes	Yes	Yes	Yes
State $\times$ Year FE	Yes	Yes	Yes	Yes	Yes
N	11,689	11,689	11,689	11,689	11,673

Note: IV estimates, non-South sample, county and state-by-year fixed effects; Panel B weights by time-varying county population. Stars attach to each SE row separately. First-stage F: 1,000 unweighted, 603 weighted (999 and 602 for turnout). Person-weighted, the share gain strengthens (0.250 $\rightarrow$ 0.650) and the Republican decline survives under both clusterings (-1.44), but the participation margins invert: turnout is a precise null, total votes tip positive, and Democratic counts turn positive (the latter two significant under county clustering only). The two estimands answer different questions. Averaging over counties, radio's modal effect was the demobilization of habitual Republicans — rural counties dominate the count; averaging over voters, its aggregate signature adds urban Democratic mobilization — population concentrates in cities, consistent with the urbanization split in Table A21. The text states the claim unit explicitly wherever the distinction bites. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

E The County Illiteracy Gradient

Radio's pro-Democratic effect is steeper where county illiteracy is higher. This section reports the preferred continuous-interaction IV and its count-level decomposition, the less-informative subsample split and why it is less informative, the competing moderators that might rival illiteracy, and the regional pattern of the headline estimate.

E.1 Interaction IV versus Subsample Splits

My primary mechanism test for county illiteracy (Table 6) is a continuous interaction IV that treats radio and radio $\times$ illiteracy as endogenous, mirroring the main specification at both the level and the interaction. A less-efficient alternative is to split the non-South sample at the illiteracy median (or at the top-third cutoff) and estimate the main IV separately in each subsample.[9]

Sample composition: border-state counties disproportionately populate the high-illiteracy subsample. When I restrict the non-South sample (Confederate-11 definition) to counties above the top one-third illiteracy cutoff, Kentucky alone contributes 18% of the high-illiteracy observations. Kentucky is a border state whose post-Reconstruction politics combined Democratic dominance with Appalachian Republican enclaves, and its counties exhibit a ceiling-type vote count pattern. When Kentucky is excluded, the non-South top-third IV yields a Democratic vote share coefficient of $+0.39$ ($p < 0.01$, state-clustered), a log Democratic votes coefficient of $+1.11$ (positive but imprecise, $p > 0.10$), and a log Republican votes coefficient of -1.31 ($p < 0.05$), consistent with the mechanism's prediction. Further excluding Oklahoma and West Virginia — the other two non-South states with substantial Appalachian Republican pockets — leaves the Democratic vote share coefficient at $+0.35$ and the log Democratic votes coefficient at $+1.10$, qualitatively the same result. Because interaction IV pools across the full non-South and uses within-county over-time variation rather than cross-county subsample splits, it is not sensitive to these composition asymmetries in the same way.

Instrument heterogeneity: the two instruments identify partly different complier populations. When the IV is estimated using each instrument separately on subsample splits of the non-South, the two instruments give related but non-identical pictures of

[9] The median split (Non-South median illit$_{ct}$ ≈ 0.014) is used for the balanced IV subsample contrast in Appendix Table A24; the top-third cutoff (≈ 0.022) is used for the Kentucky-composition diagnostic below, where focusing on the marginal high-illiteracy cells makes the border-state ceiling pattern legible. Both definitions place the same counties on the same side of the cut for the bulk of the distribution. I document that alternative here for completeness and to show that the subsample-split results are broadly consistent with the interaction-IV evidence but exhibit sample-composition and instrument-heterogeneity features that make the subsample split a less informative test of the illiteracy gradient than the interaction IV is. As Appendix Table A24 shows, the Democratic vote share coefficient in the median split is larger in the low-illiteracy non-South subsample (0.707) than in the high-illiteracy subsample (0.217), an apparent reversal that dissolves once one accounts for the composition and instrument-heterogeneity issues documented below. Both subsample coefficients are nevertheless same-signed, so the split does not overturn the directional finding that radio increased Democratic vote share in the non-South. The interaction IV in Table 6 is the preferred test of the illiteracy gradient itself.

the illiteracy gradient: they generally agree in sign but place different weights on the complier populations, so any single subsample split reflects a weighted average of LATEs that need not coincide with the policy-relevant parameter. The continuous interaction IV in Table 6 is not fully immune to this issue, but it aggregates the gradient across the full non-South rather than from a narrow subsample contrast, reducing the sensitivity of the point estimate to instrument-specific weighting.

Vote count decomposition. Appendix Table A23 extends the IV illiteracy interaction to the vote count outcomes. The interaction with log Republican votes is negative and significant ($-4.40,\ p < 0.001$), indicating that Republican vote decline is steeper in high-illiteracy counties; the interaction with log Democratic votes is same-signed (1.85) but too noisy under IV to identify a gradient. This pattern attributes the share-level illiteracy gradient (Table 6) primarily to differential Republican decline at high illiteracy, with Democratic-side flows insufficiently identified at the count level. The near-zero change in average non-South Democratic vote counts documented in Table 4 is consistent with the formal decomposition in Appendix D.1, where Democratic-side inflows and outflows can cancel; the dominant identifiable count-level driver of the share gradient is the Republican channel.

I note one residual tension between estimators. The interaction-IV in Table A23 gives a log Republican coefficient of -4.40 ($p < 0.001$) on the radio $\times$ illiteracy term, implying steeper Republican decline at high illiteracy. The median-split IV in Table A24, however, shows a *larger* log Republican coefficient in absolute value in the *low*-illiteracy half (-2.73) than in the high-illiteracy half (-1.13). The share-level gradient is consistently positive across both estimators (high coefficient 0.217, low 0.707, both significant), but the count-level gradient on log Republican votes flips sign across the linear interaction and binary split, most likely reflecting a non-linear functional form that the two estimators summarize differently. I treat the interaction IV as the primary specification for the count-level

Table A23: IV Radio × Illiteracy Interaction: Vote Count Decomposition (Non-South)

	Dem Share	Log Dem	Log Rep	Turnout
Radio	0.291**	0.151	−1.116***	−0.327***
	(0.107)	(0.545)	(0.118)	(0.060)
Radio × Illiteracy	1.620*	1.853	−4.395***	−1.421**
	(0.635)	(3.376)	(1.181)	(0.542)
County FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
N	11,689	11,689	11,689	11,673

Note: IV estimates with county and state-by-year fixed effects, non-South sample (Confederate-11). Standard errors (state-clustered) in parentheses. Both radio and radio × illiteracy are endogenous, instrumented by ground conductivity and woodland share interacted with time and with illiteracy. The Democratic vote share column replicates Table 6, column (1) of Panel A. The illiteracy gradient operates through the log Republican count (−4.40, $p < 0.001$); the log Democratic count interaction is same-signed (1.85) but too imprecise to identify a gradient under IV. Per-equation first-stage F-statistics: radio ≈ 717, radio × illiteracy ≈ 1,737 (Pres Turnout marginally lower owing to a 16-observation drop from the capped-denominator filter); Sanderson–Windmeijer conditional F-statistics, the appropriate per-regressor diagnostic, are 162 (radio) and 73 (radio × illiteracy) under county clustering and 106 and 46 under state clustering — all well above weak-instrument thresholds. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

gradient and note this disagreement as an open issue rather than a resolved finding.

E.2 Competing Moderators

County illiteracy could proxy for other traits that radio plausibly activated. Three horse-race tests address this: Table A27 pits it against urbanization and schooling at the district level, Table A28 against county foreign-born share, and Table A29 against alternative newspaper-access measures.

E.3 Regional Heterogeneity

The headline non-South estimate is a weighted average across regions with very different baseline partisan structures. Table A30 refits the headline IV separately on the Northern Core, the Border South, and the Confederate-11 South.

The scope condition receives two further tests. Table A31 formalizes the South/non-South contrast as a single pooled interaction IV, and Table A32 asks whether a positive

Table A24: IV Vote Count Decomposition by County Illiteracy (Non-South, Median Split, State × Year FE)

	High Illiteracy	Low Illiteracy
Dem Share	0.217***	0.707**
	(0.045)	(0.273)
Log Dem Votes	−0.007	−0.260
	(0.200)	(1.041)
Log Rep Votes	−1.127***	−2.733***
	(0.164)	(0.572)
Turnout	−0.324***	−0.445**
	(0.046)	(0.157)
First-stage F	649	53
N	5,841	5,832
County FE	Yes	Yes
State × Year FE	Yes	Yes

Note: IV estimates, non-South sample (Confederate-11; see Table 2 note), split at the non-South median of time-varying county illiteracy. SE clustered at county level. The Democratic vote share coefficient is roughly three times larger in the low-illiteracy subsample than in the high-illiteracy subsample, the opposite of what the mechanism would predict if the split were a clean test of the gradient. Appendix E.1 documents why a subsample split is a less informative test of the literacy gradient than the interaction IV (sample composition, single-instrument heterogeneity). The continuous interaction IV in Table 6 — with the count decomposition in Table A23 — is the preferred test. The turnout row is estimated on a few county-years fewer than the column N shown, owing to the capped-denominator filter. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A25: Binned Illiteracy Gradient: Marginal Radio Effect by Tercile (Non-South)

	T1 (low illit)	T2 (middle)	T3 (high illit)
Marginal radio effect on Dem share	0.261*	0.275*	0.322*
	(0.117)	(0.117)	(0.131)
Increment over T1	—	0.015	0.061**
		(0.013)	(0.024)
N		11,689	

Note: IV with three endogenous regressors (radio, radio × T2, radio × T3; T1 the reference tercile of county illiteracy), instrumented by the two geographic instruments and their tercile interactions; county and state-by-year fixed effects, state-clustered SEs. Conventional first-stage F: 383 (radio), 4,270 (T2), 2,704 (T3); Cragg–Donald 394. The marginal effect rises monotonically ($\text{T3} - \text{T2} = +0.047$) and the top-tercile increment is significant ($p = 0.009$), so the continuous interaction in Table 6 is not a linear-functional-form artifact. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

effect appears *within* the South exactly where a movable Republican pool existed.

Table A26: Illiteracy Gradient by 1920 Competition: The Movable-Pool Test (Non-South)

	Safe Rep	Competitive	Safe Dem	Pooled
Radio	0.569***	0.183*	−0.047	0.291**
	(0.166)	(0.092)	(0.033)	(0.107)
Radio × Illiteracy	2.062***	1.101	1.059*	1.648**
	(0.532)	(0.863)	(0.430)	(0.638)
First-stage F (radio; interaction)	156; 1,454	369; 423	128; 188	712; 1,712
N	8,268	2,946	378	11,634
Counties	1,379	497	66	1,942

Note: Two-endogenous interaction IV (radio, radio × illiteracy) estimated within 1920-baseline competition groups (Safe Republican: 1920 Democratic share < 0.40; Competitive: 0.40–0.60; Safe Democratic: > 0.60, the Table A20 classification); county and state-by-year fixed effects, state-clustered SEs. The gradient is strongest exactly where the misaligned pool is largest — safe-Republican counties (2.06, $p < 0.001$) — and attenuated and insignificant in competitive counties; the thin safe-Democratic cell (66 counties, VCOV required a positive-semidefinite correction) is read qualitatively. The theory's graduated prediction thus holds *within* the non-South, not only at the South/non-South cliff. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A27: Competing Moderators (Horse Race)

	Nokken-Poole (1st dim.)
Radio × Illiteracy (std.)	0.261***
	(0.050)
Radio × Urbanization (std.)	0.021
	(0.036)
Radio × Schooling (std.)	−0.091+
	(0.050)
District FE	Yes
Congress FE	Yes

Note: OLS with all three interactions simultaneously; district and Congress FE; SE clustered at district level. The dependent variable is the Nokken-Poole first-dimension score (negative = more liberal), so the positive Radio × Illiteracy coefficient implies a *weaker* leftward shift at high illiteracy — the opposite direction from the saturated-split IV gradient in Table 5, a district-level analogue of the interaction-versus-split sign instability documented in Appendix E.1. The instability is a functional-form artifact of the linear-in-moderator specification: re-estimating each moderator in the saturated-binary design of Table 5 yields a high-minus-low illiteracy difference of -1.10 (SE 0.26, $p < 0.001$) on the moderator's own split — same-signed as Table 5's point-estimate gradient, though the saturated radio-effect difference there is itself insignificant (national $p = 0.72$) — while schooling runs in the expected opposite direction (+1.21, SE 0.27) and urbanization at +0.58 (SE 0.26). I therefore read this table as a significance horse race among competing moderators, not as directional evidence on the gradient; it runs on the national panel with the Nokken-Poole outcome, whose national gradient the paper shows to be South-composition-driven (Table A33), so it is documented as a run record rather than a claimed representation result. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A28: Horse Race: Illiteracy versus Foreign-Born Moderators

	Pres Dem Share			Pres Turnout		
	(1)	(2)	(3)	(4)	(5)	(6)
Radio	0.328**	0.176***	0.210***	−0.360***	−0.342***	−0.400***
	(0.119)	(0.051)	(0.060)	(0.069)	(0.049)	(0.052)
Radio × Illit. (std.)	0.043*		0.019	−0.037**		−0.049***
	(0.017)		(0.012)	(0.014)		(0.013)
Radio × For.-born (std.)		0.054**	0.054**		0.026	0.041*
		(0.020)	(0.020)		(0.017)	(0.019)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	11,689	11,689	11,689	11,673	11,673	11,673

Note: Non-South IV with county and state-by-year fixed effects; SE clustered at the state level. Moderators are standardized within the estimation sample (raw illiteracy mean 0.023, SD 0.026; foreign-born mean 0.094, SD 0.098; within-sample correlation -0.01, so the horse race is not a collinearity contest). Every radio term is endogenous and instrumented by the corresponding interactions of the two geographic instruments (columns 3 and 6 use three endogenous regressors and six instruments; per-equation first-stage F 514, 1,517, 889 in column 3 and 514, 1,517, 888 in column 6). Level terms of each moderator enter as exogenous controls. In raw units the column (1) illiteracy interaction replicates the Table 6 estimate (1.620, SE 0.635). On vote share the foreign-born moderator survives the horse race while illiteracy attenuates ($p \approx 0.13$); on turnout the two diverge in sign, the signature of mobilization in immigrant counties versus demobilization in high-illiteracy counties. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

F Legislator Ideology

The district-level analysis in Section 4.2 rests on the non-South panel; this appendix documents why, and reports the full battery behind the bounded claim. The national district panel includes the Confederate 11, and its high-illiteracy cells are structurally South-dominated: 61.2% of Democratic-seat high-illiteracy observations are Southern districts. By the paper's own scope condition, Southern electorates could not respond to radio — disenfranchisement and one-party rule severed the electoral connection — and the Southern delegation's New-Deal-era leftward movement has ready explanations unconnected to radio, so national estimates from these cells cannot be attributed to voter information. Table A33 therefore re-estimates every cell of Table 5 on the non-South panel, with the within-legislator adaptation margin in Panel C.

The re-estimation sorts the full-sample findings into three epistemic bins. *Refuted*: the

Table A29: Newspaper Moderator Asymmetry: Alternative Newspaper Measures

	Moderator: paper count		Moderator: total circulation	
	Pres Dem Share	Pres Turnout	Pres Dem Share	Pres Turnout
Radio	0.567**	−0.420***	0.650***	−0.373**
	(0.176)	(0.112)	(0.196)	(0.126)
Radio × Illit. (std.)	0.081*	−0.019	0.089*	−0.014
	(0.034)	(0.019)	(0.036)	(0.020)
Radio × Newspaper (std.)	0.010**	0.015***	0.013+	0.021***
	(0.003)	(0.003)	(0.007)	(0.006)
First-stage F (radio)	84	84	85	85
First-stage F (radio × illit)	737	736	738	737
First-stage F (radio × news)	1,616	1,613	982	981
County FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
N	4,418	4,412	4,418	4,412

Note: Non-South IV re-estimation of Table 8 replacing circulation per capita with the count of distinct tracked dailies and total daily circulation, each standardized within the tracked sample. Instruments and specification as in Table 8; SE clustered at the state level. The illiteracy interaction on Democratic vote share is stable across all three newspaper controls (0.081–0.093 per SD). The scale measures carry small positive vote-share interactions (0.010–0.013 per SD, roughly one-eighth the illiteracy gradient per SD), unlike the null per-capita measure; count and total circulation track market size and newspaper entry/exit rather than print-access intensity, so per-capita circulation remains the theoretically relevant access-intensity moderator. Within-county SDs of the standardized moderators are 0.13 (illiteracy), 0.15 (circulation per capita), 0.30 (count), and 0.18 (total circulation). OLS analogues show the same pattern. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A30: Subsample Headline IV by Region

Subsample	Coefficient (SE)	First-stage F	County-years
Northern Core (Non-South excl. KY/MD/MO/WV/DE/OK)	0.558^{***} (0.093)	261	9,355
Border South (KY/MD/MO/WV/DE)	0.112^{**} (0.038)	624	1,872
Confederate 11 South	-0.109 (0.132)	109	6,628
Memo: Non-South (Confederate 11), headline	0.250^{***} (0.040)	1,000	11,689

Note: IV estimates of *radio* on presidential Democratic vote share, identical specification to the headline (Table 2, Panel A, column 2): county and state-by-year fixed effects; controls and instruments as there. Standard errors clustered at county level in parentheses. Oklahoma (N=462 county-years; ICPSR state code 53; admitted 1907; literacy test until 1916; partial white primary) is excluded from the Border row because of its mixed Jim Crow history; estimated separately it yields 0.141 (SE 0.056), in line with the Border row. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

full-sample Nokken-Poole effect (-1.79) falls to -0.33 with a confidence interval that excludes the former value — a compositional artifact, not a power loss. *No evidence*: within-legislator Democratic adaptation falls from -0.61 to -0.11 with a confidence interval cov-

Table A31: Formal South/Non-South Difference Test (Pooled Full-Sample IV)

	Pres Dem Share
Radio (South base)	−0.117
SE (county)	(0.130)
SE (state)	(0.294)
Radio × Non-South (difference)	0.356
SE (county)	(0.137)**
SE (state)	(0.321)
Implied non-South effect	0.239
First-stage F (radio; interaction)	469; 755
N	18,317

Note: Pooled full-sample IV treating radio and radio × non-South as endogenous (four instruments), county and state-by-year fixed effects. The implied subsample effects (−0.117 South, +0.239 non-South) match the separately estimated columns of Table 2. The difference is significant under county clustering ($p = 0.009$) but not under state clustering ($p = 0.27$), where 48 clusters must carry a between-region contrast. The scope condition therefore rests on the point-estimate contrast and its institutional grounding rather than on a formally certified difference under the conservative clustering; the within-South split below supplies the directional complement. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A32: Headline IV Within the South, by 1920 Republican Strength

	Full South	Substantial Rep pool	Near-zero Rep pool
Radio	−0.120	0.423	0.006
SE (county)	(0.133)	(0.363)	(0.095)
SE (state)	(0.320)	(0.544)	(0.156)
First-stage F	110	17	121
N	6,506	1,889	4,617
Counties	1,094	317	777

Note: Headline level IV within the Confederate-11 sample, split by 1920 Republican strength (substantial pool: 1920 Democratic share < 0.60; near-zero: ≥ 0.60); county and state-by-year fixed effects. Where the South retained a substantial Republican minority the point estimate is positive (+0.42) though imprecise (first-stage F = 17); where the pool was near zero the effect is a precise zero. A finer three-way split points the same way (safe-Republican +1.01, but F = 4.4 on 67 counties — uninformative). Directionally consistent with the floor-effect reading: the Southern null reflects the missing movable pool, not a failing information channel. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

ering both zero and the old value (only 137 two-plus-term legislators identify it), and the pooled adaptation asymmetry (full-sample $p = 0.048$) dissolves to $p = 0.50$–0.76 with unstable sign. *Underpowered*: the Republican selection estimate (-0.669 full sample) becomes -0.454 (n.s.); the cut removes only 37 Southern observations, and the cell retains roughly 61% power to detect even the full-sample effect, so the non-South data cannot decide it.

Table A33: Legislator Ideology Re-Estimated on the Non-South Panel

	Full	High Illit	Low Illit	Dem, Hi Illit	Rep, Hi Illit
Panel A: Nokken-Poole first dimension (adaptation + selection)					
Radio	−0.330	−0.170	−0.557	−0.517	−0.325
	(0.539)	(0.717)	(0.925)	(0.341)	(0.333)
First-stage F	284	176	93	111	49
N	3,250	1,150	2,094	603	513
Panel B: DW-NOMINATE first dimension (selection only)					
Radio	−0.154	−0.389	0.249	−0.705***	−0.454
	(0.517)	(0.662)	(0.829)	(0.202)	(0.299)
First-stage F	285	177	93	114	49
N	3,240	1,145	2,089	601	511
Panel C: Nokken-Poole, legislator FE (adaptation only)					
Radio	−0.305	—	—	−0.114	0.571
	(0.414)			(0.457)	(0.573)
N (2+-term legislators)	2,878 (702)	—	—	564 (137)	459 (108)

Note: Non-South (Confederate-11 excluded) district panel; district (Panels A–B) or legislator (Panel C) and Congress fixed effects; district- (legislator-) clustered SEs; instruments as in Table 5. High-illiteracy cells use the full-sample median cutoff of Table 5, so each cell is directly comparable to its national counterpart. Saturated-interaction high-minus-low differences: $p = 0.39$ (Nokken-Poole), $p = 0.60$ (DW-NOMINATE). The surviving cell is robust to a non-South-internal median cutoff (−0.666, SE 0.211, $p < 0.01$, $N = 739$). Pooled adaptation-asymmetry tests (full-sample analogue $p = 0.048$): $p = 0.50$–0.76 with unstable sign across common and party-specific Congress-FE variants. Pre-radio placebo on this panel (1913–1920): conductivity × time predicts nothing; woodland × time is positive-signed (0.14–0.20, marginal to significant) — opposite-signed to the estimated negative effects, hence conservative (see Table A34 note). $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

The survivor is Democratic-seat selection: DW-NOMINATE -0.705 ($F = 114$), essentially unchanged from its full-sample value of -0.726 — the one cell whose full-sample estimate was not carried by the South.

Table A34 stress-tests the surviving cell against the woodland exclusion-restriction concern, mirroring the county-level confrontation: dropping the most-forested districts *strengthens* it, and it holds when identified off ground conductivity alone. Table A35 identifies the channel: radio does not predict Democratic seat capture anywhere in the non-South, so the electorate shift expressed itself through within-party candidate replacement — Democratic seats filled by more liberal Democrats — not through partisan turnover.

The remaining tables document the full-sample record. Table A36 reports the national

Table A34: The Surviving Cell Under Clean-Instrument Stress (Dem Seats, High-Illiteracy Non-South)

	DW-NOMINATE (selection)		Nokken-Poole	
	Two instruments	Conductivity only	Two instruments	Conductivity only
Baseline non-South	-0.705^{***}	-0.916^{*}	-0.517	0.338
	(0.202)	(0.428)	(0.341)	(0.668)
First-stage F	114	40	111	39
Drop top woodland quartile	-0.958^{***}	-0.725^{***}	-0.868^{*}	-0.322
	(0.260)	(0.205)	(0.379)	(0.345)
First-stage F	110	154	111	154
Drop top woodland tercile	-0.705^{**}	—	-0.297	—
	(0.254)		(0.382)	
First-stage F	68		68	
Drop Kentucky + West Virginia	-0.592^{**}	—	-0.175	—
	(0.215)		(0.352)	
First-stage F	77		77	

Note: The Dem-in-High cell of Table A33 under woodland stress tests; district and Congress fixed effects, district-clustered SEs. DW-NOMINATE $N = 601$ (baseline), 407 (quartile drop), 391 (tercile drop), 501 (KY+WV drop); Nokken-Poole cells 603/408/392/502. "Conductivity only" drops the woodland instrument entirely (just-identified). The selection estimate strengthens when the most-forested districts are dropped and holds on the clean instrument alone — with the South-cut immunity of Table A33, its triple defense. The district-level woodland × time pre-radio placebo is positive-signed in every sample and does *not* clean under the drops (DW: 0.18* baseline → 0.81** under the quartile drop) — opposite-signed to the negative treatment effects, so any woodland violation biases *against* the finding; the conductivity × time placebo is a clean null throughout ($p \geq 0.12$, one marginal $p = 0.056$ under the KY+WV drop). $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

pre-radio placebo that supports reading the district design as between-state identified; Table A37 shows census-division-by-Congress fixed effects preserve full-sample signs but collapse the subsample first stages, confirming that caveat from the estimation side; Tables A38 and A39 decompose adaptation from selection on the national panel and probe that decomposition's robustness. The adaptation findings in those two tables are full-sample results that do not survive the South cut; they are documented as run records — they define the estimates the non-South re-estimation bounds — not claimed. Their identification caveats (between-state variation, two-plus-term legislators) apply as stated in the table notes.

Table A35: Seat Partisanship: Radio Did Not Flip Seats (LPM, District IV)

	Full	High Illit	Low Illit
Panel A: non-South (analytic sample)			
Radio	−0.543	−0.480	−1.450
	(0.775)	(0.985)	(1.290)
DV mean	0.394	0.529	0.320
First-stage F	284	176	93
N	3,250	1,150	2,094
Panel B: national reference (South included)			
Radio	1.062***	0.751*	−0.871
	(0.259)	(0.317)	(1.212)
DV mean	0.534	0.730	0.337
First-stage F	530	307	101
N	4,304	2,149	2,149

Note: Linear probability model of Democratic seat occupancy on instrumented radio; district and Congress fixed effects, district-clustered SEs. In the non-South, radio does not predict Democratic seat capture in any cell. The national positive coefficient is a one-party-South artifact: the Southern DV mean is 0.965, so Southern districts contribute level but almost no within-district flip variation, and pooling them manufactures a spurious "capture" effect. Because the surviving selection cell of Table A33 conditions on Democratic occupancy by construction, this null completes the channel identification: the electorate shift ran through within-party candidate replacement, not partisan seat capture. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A36: Legislator Ideology Pre-Radio Placebo: Reduced-Form Instrument Effects, 1913–1920

	Nokken-Poole		DW-NOMINATE	
	Pre-radio (1913–20)	In-sample (1922–40)	Pre-radio (1913–20)	In-sample (1922–40)
Panel A: district and Congress fixed effects (as in Table 5)				
Conductivity $\times$ time	0.06	0.43	0.31	0.35
	(0.46)	(0.43)	(0.48)	(0.34)
Woodland $\times$ time	-0.023	0.143***	-0.032	0.122***
	(0.050)	(0.025)	(0.055)	(0.023)
Panel B: state $\times$ Congress fixed effects				
Conductivity $\times$ time	1.46	0.15	1.88*	0.05
	(0.96)	(0.34)	(0.92)	(0.31)
Woodland $\times$ time	0.313*	0.059	0.276*	0.046
	(0.137)	(0.036)	(0.133)	(0.033)
N	1,721	4,304	1,723	4,290

Note: Reduced-form regressions of legislator first-dimension ideology on the two geographic instruments interacted with a linear time trend, full district panel. Pre-radio columns use House Congresses 63–66 (1913–1920), when radio penetration was near zero; the instruments are anchored to each district's 67th-Congress (1922) composition, on the 1910 apportionment the pre-radio Congresses share with the estimation sample. In-sample columns use the 1922–1940 window on the same per-year time scaling, so the coefficients are directly comparable. Controls are urbanization, Black population share, and log population; conductivity coefficients and SEs are scaled by 10^3. State-clustered standard errors in parentheses (the appropriate inference for spatially smooth instruments $\times$ time). Negative values indicate more liberal positions. Under the Table 5 fixed effects (Panel A) no instrument predicts pre-radio ideology, while woodland $\times$ time carries the in-sample reduced-form signal — so the leftward shift is not a pre-existing geographic trend. Under state $\times$ Congress fixed effects (Panel B) the pre-radio woodland trend is instead significant and larger than its in-sample counterpart, confirming that the within-state variation those fixed effects isolate is confounded and that identification must be read as between-state. The illiteracy-interaction instrument terms are likewise insignificant pre-radio under Panel A (none reaches $p < 0.10$). $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A37: Legislator Ideology Under Census-Division × Congress Fixed Effects

	Full	High illit	Low illit	Dem, Hi illit	Rep, Hi illit
Panel A: Nokken-Poole first dimension					
Baseline (Congress FE)	-1.794^{***}	-1.550^{***}	-1.203	-1.033^{***}	-0.286
	(0.234)	(0.281)	(0.944)	(0.263)	(0.256)
Division × Congress FE	-0.470	-0.036	0.226	0.464	0.120
	(0.647)	(2.561)	(0.885)	(1.270)	(0.912)
First-stage F (div × Cong)	155	5	126	13	27
Panel B: Nominate first dimension					
Baseline (Congress FE)	-1.536^{***}	-1.412^{***}	-0.320	-0.726^{***}	-0.669^{**}
	(0.208)	(0.247)	(0.846)	(0.180)	(0.244)
Division × Congress FE	-0.881	-1.272	0.498	0.495	-0.153
	(0.607)	(2.394)	(0.810)	(1.118)	(0.594)
First-stage F (div × Cong)	155	5	125	13	27
N (div × Cong, Panel A)	4,304	2,140	2,140	1,559	532

Note: District-level IV estimates with district FE and either Congress FE (baseline rows, replicating Table 5) or census-division × Congress FE; SE clustered at the district level. Division × Congress FE absorb an additional 56–57% of the instruments' residual variance beyond Congress FE (state-by-Congress FE, which would absorb 79–84%, are infeasible because many states elect fewer than five representatives per Congress). In the full sample the first stage stays strong (F = 155) and the estimates stay negative but lose precision; in the high-illiteracy and party subsamples the first stage collapses (F between 5 and 27), so those cells are uninformative rather than contradictory. This confirms the caveat in the text: district-level identification rests largely on between-state variation, which is why the text reads Table 5 for its sign pattern rather than its magnitudes; the non-South re-estimation (Table A33) governs which cells the paper claims. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A38: Within-Legislator Adaptation: District versus Legislator Fixed Effects

	Full	High illit	Low illit	Dem, Hi illit	Rep, Hi illit
Panel A: Nokken-Poole first dimension (adaptation channel)					
District FE (adaptation + selection)	-1.794^{***}	-1.550^{***}	-1.203	-1.033^{***}	-0.286
	(0.234)	(0.281)	(0.944)	(0.263)	(0.256)
Legislator FE (adaptation only)	-0.450^{*}	-0.422^{*}	-1.651^{*}	-0.612^{*}	0.573
	(0.186)	(0.205)	(0.805)	(0.289)	(0.413)
First-stage F (legislator FE)	379	233	31	129	80
Panel B: DW-NOMINATE first dimension (selection channel)					
District FE (selection only)	-1.536^{***}	-1.412^{***}	-0.320	-0.726^{***}	-0.669^{**}
	(0.208)	(0.247)	(0.846)	(0.180)	(0.244)
Legislator FE (career-static; absorbed)	0.000	0.000	0.000	0.000	0.000
N (legislator FE, Panel A)	3,861	1,959	1,900	1,469	485

Note: District-level IV estimates; the endogenous radio term is instrumented by ground conductivity $\times$ time and woodland $\times$ time. District-FE rows use district and Congress fixed effects (reproducing Table 5); legislator-FE rows replace district with legislator FE, identifying the within-legislator adaptation margin. SE (in parentheses) clustered at the district level (district-FE rows) or legislator level (legislator-FE rows). Because common-space DW-NOMINATE is career-static, it is absorbed entirely by legislator FE (Panel B legislator-FE row $\equiv 0$), confirming the specification isolates adaptation rather than selection. Legislator FE are identified off the 67% of legislators serving two or more Congresses; the Low-illiteracy cell has a weak first stage ($F = 31$) and should not be read. *Formal asymmetry test:* a pooled within-legislator IV interacting radio with party (high-illiteracy sample) gives a Democratic adaptation of -0.59 ($p < 0.05$), a Republican $-$ Democratic gap of +0.85 ($p = 0.048$), and a Republican adaptation indistinguishable from zero (+0.26); the gap is same-signed but not significant in the full sample. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A39: Within-Legislator Adaptation: Robustness to Tenure, Clustering, and Roll-Call Measure

	Dem, Hi illit	Rep, Hi illit	Rep $-$ Dem gap
Panel A: Nokken-Poole adaptation by tenure restriction			
Two or more Congresses (baseline)	-0.612^{*}	0.573	0.848^{*}
	(0.289)	(0.413)	(0.428)
Three or more Congresses	-0.603^{*}	0.545	0.765^{+}
	(0.293)	(0.419)	(0.455)
Radio-spanning legislators	-0.627^{+}	0.512	1.023^{*}
	(0.339)	(0.534)	(0.495)
Panel B: alternative roll-call measure (behavior-based)			
Common Voting Probability	-0.576^{***}	-0.147	0.430^{+}
	(0.155)	(0.171)	(0.235)

Note: Within-legislator IV estimates (legislator and Congress fixed effects; radio instrumented by conductivity $\times$ time and woodland $\times$ time), isolating the adaptation margin as in Table A38. Columns report high-illiteracy Democrats, high-illiteracy Republicans, and the Republican $-$ Democratic adaptation gap from a pooled radio-by-party interaction with party-specific Congress fixed effects. Panel A varies the tenure restriction that identifies legislator fixed effects: "radio-spanning" legislators serve in both a low-radio (67th–69th) and a high-radio (73rd–76th) Congress, so radio's rise falls within their own careers — the cleanest adaptation identifiers, where the Democratic estimate and the party gap are, if anything, larger. Panel B replaces the Nokken-Poole score with the GLS-adjusted Common Voting Probability score (a behavior-based measure; correlation with Nokken-Poole 0.90), which independently reproduces the Democratic adaptation and the Republican null. Standard errors clustered at the legislator level in parentheses; the baseline Democratic estimate (-0.61) also holds under state clustering ($p < 0.10$) and two-way legislator-and-Congress clustering ($p < 0.10$), and the Table 5 full-sample estimate (-1.79) remains significant at $p < 0.001$ under both state and two-way clustering. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.