Wealth Transfers Erode the Coalition That Enacted Them: Evidence from the Taylor Grazing Act

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September 6, 2026

Abstract

Do wealth transfers buy lasting political loyalty? We study the Taylor Grazing Act of 1934, which formalized grazing rights on 142 million acres of western rangeland for nearby ranchers. By converting contested access into documented, pledgeable property rights, the Act raised farm values and strengthened balance sheets in treated counties. Event-study estimates show a brief Democratic vote-share boost from 1936 to 1940, peaking at 3.7 percentage points, followed by a sharp reversal that turned persistently negative by 1942. A triple-difference exploiting pre-treatment debt-to-value ratios shows that the Democratic reversal was strongest in counties where the TGA most improved net worth. Roll-call votes are inconsistent with gratitude: counties whose own representative championed the Act saw the largest anti-Democratic swing. Gallup surveys from 1936 to 1948 show none of the lasting turn against government that ideological conversion predicts. The reversal reflected two reinforcing shocks: the TGA created the asset, and wartime commodity demand capitalized it. Wealth transfers can undermine the coalition that enacted them by pushing beneficiaries above the threshold where redistribution costs them more than it pays.

Keywords: wealth transfers; redistribution; voting behavior; public lands; New Deal; American West

JEL codes: D72, H23, N42, Q15

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1 Introduction

Governments redistribute wealth to form stable coalitions. Voters who receive tangible benefits reward the party that delivered them. That electoral contract is well documented: federal spending shifts vote shares toward incumbents (Levitt and Snyder, 1997), conditional cash transfers boost the ruling party (De La O, 2013; Zucco, 2013; Manacorda et al., 2011), and land titling programs generate political loyalty (Galiani and Schargrodsky, 2010). But the Meltzer and Richard (1981) framework identifies a limit to this logic. When a transfer operates through asset values rather than consumption, it can lift beneficiaries above the mean of the distribution and lead them out of the redistributive coalition. The more fully the transfer succeeds, the sooner they leave.

The Taylor Grazing Act (TGA) of 1934 provides a direct test. Signed by President Roosevelt and enacted by a Democratic Congress, the TGA withdrew 142 million acres of western public domain from open access and granted nearby ranchers formal, renewable grazing permits tied to their private land. The Act converted contested rangeland access into documented property rights, raising farm values and strengthening balance sheets across treated counties (Bühler, 2023). The transfer came in kind: incumbent users received a below-market renewable claim on a public resource, and that claim raised the value of the private land to which it attached. The setting combines the features needed to distinguish gratitude from wealth-driven realignment: a single, identifiable policy enacted during a period of one-party dominance, a clearly defined beneficiary group, and a wealth channel that operated through balance sheets rather than consumption.

We trace the electoral consequences in a county-level panel of U.S. congressional elections from 1910 to 1972. If the wealth channel drives the political response, two patterns should appear: an initial pro-Democratic effect should reverse as the wealth gain materializes, and the reversal should be largest where a given gain matters most for balance sheets. We test the first prediction with event-study and difference-in-differences estimates, and the second with a triple-difference that interacts TGA treatment with pre-treatment leverage, measured as the county's 1930 debt-to-value ratio. To discriminate among competing mechanisms, we examine heterogeneity across House roll-call votes on the TGA and draw on Gallup surveys of policy attitudes from 1936 to 1948.

Democratic vote share rose in treated counties during the first elections after enactment, then reversed sharply and turned persistently negative within a decade. The average reversal is imprecisely estimated once errors are allowed to correlate across neighboring counties (Section 8), so the argument rests on where the reversal concentrated. A triple-difference locates it: high-leverage counties accounted for most of the anti-Democratic shift, while low-leverage counties continued to reward Democrats.

The shock behind that reversal was large: treated counties gained \$9,499 per farm by 1950 over their non-grazing neighbors, 85 percent of the median 1930 farm value in those counties. The gain also reached well past the permit holders themselves. Range recovery and local spending would have capitalized into land values county-wide, which is what our farm-value measure records.

Event-study pre-trends confirm that treated and control counties followed indistinguishable partisan trajectories before 1934. Party labels carried inconsistent ideological content in the interwar West, so those flat coefficients establish only that the two groups voted alike. High- and low-leverage TGA counties are likewise balanced on pre-treatment observables, including prior Democratic vote share. A placebo triple-difference on the pre-treatment window produces no effect. Instrumental-variable estimates using the Bühler (2023) rainfall instrument corroborate the sign of both the main effect and the leverage interaction. Livestock-share controls and placebo tests on non-TGA counties rule out WWII commodity demand alone as the driver of the 1940–42 reversal, though wartime demand helped activate the wealth channel by capitalizing the TGA's asset gains. A turnout event study shows no effect on total votes cast, so the reversal reflects vote choice rather than participation.

Did gratitude fade, did wealth convert ideology, or did self-interest narrow? Roll-call evidence is inconsistent with gratitude: counties whose own representative championed the TGA saw the largest anti-Democratic swing. Gallup surveys are inconsistent with ideological conversion: TGA-state respondents show no uniform turn against government in 1936–37, and farm respondents in treated states show none through 1948. The selective pattern self-interest predicts does appear in the 1936–37 polls, where TGA-state respondents opposed reviving the AAA while favoring higher farm prices. Only the AAA comparison survives multiple-testing correction. The survey evidence therefore corroborates the self-interest account without identifying it, and the argument rests on the electoral evidence.

Together these findings identify a boundary condition on distributive politics. A transfer that raises beneficiaries above the mean of the wealth distribution converts them from net recipients into net contributors, and the electoral return reverses with their position. In the TGA case, the Act alone had not generated a large enough gain by 1940. The reversal came when wartime commodity demand capitalized the asset the Act had created, carrying farm values past the point where redistribution cost beneficiaries more than it paid. Asset-based transfers (homeownership subsidies, land reforms, debt relief) may carry a political cost that consumption transfers do not. By permanently shifting beneficiaries' position in the wealth distribution, such transfers alter the net returns to redistribution and, with them, the equilibrium coalition.

This paper contributes to the literature on distributive politics, which establishes that government transfers generate electoral returns for incumbents (Levitt and Snyder, 1997; De La O, 2013; Manacorda et al., 2011). Within the New Deal specifically, Kantor et al. (2013) show that relief and public-works spending solidified the 1932 Democratic realignment, raising long-run Democratic vote share by 2 to 10 percentage points. These consumption-based transfers maintained beneficiaries' dependence on continued federal spending and, with it, their loyalty to the party that delivered the checks. The TGA marks the limit of that loyalty: when the transfer operates through asset values rather than consumption, the electoral return reverses. Bühler (2023) establishes the economic first stage, showing that the TGA raised farm values by 40 percent and cattle stocks by 90 percent in treated counties. Leonard and Smith (2025) document how prior-appropriation water rights shaped Western settlement patterns and created the propertied class that the TGA later enriched. Ramey (2021) finds that Dust Bowl migration produced a persistent Democratic tilt in receiving areas; the TGA's wealth shock produced the opposite.

Finally, the paper connects to research on property rights, wealth, and political preferences. Di Tella et al. (2007) show that land titling in Buenos Aires shifted squatters toward pro-market beliefs, and Castañeda Dower and Pfutze (2015) find that Mexico's PROCEDE land certification program increased votes for opposition parties. Ansell (2014) shows that rising house prices reduce support for redistribution and social insurance. Hall and Yoder (2022) document that homeownership increases political engagement, with the effect scaling by purchase price. That scaling parallels our triple-difference finding that the political shift concentrated where the gain most improved net worth. Where this literature relies on modern survey or administrative data, the historical setting allows us to trace property formalization through the wealth channel to electoral realignment over two decades.

The remainder of the paper proceeds as follows. Section 2 describes the historical background. Section 3 develops the conceptual framework and derives testable predictions. Section 4 describes the data and Section 5 the empirical strategy. Sections 6 and 7 present the main results, Section 8 reports robustness checks, Section 9 tests competing mechanisms, and Section 10 concludes.

2 Historical Background

2.1 The Open Range and the Problem of the Commons

For decades before 1934, the federal rangelands of the American West operated as a de facto open-access commons. Between 1862 and 1934, a series of homestead acts transferred 236 million acres of public land to private individuals (Bühler, 2023). Yet these acts proved ill-suited to arid western conditions. The original Homestead Act of 1862 offered 160-acre plots, enough for Midwestern farming but far too little for western ranching, where Powell (1878) estimated that 2,580 acres were needed to sustain a household. Congress responded by increasing allotments to 320 acres (Kincaid Act, 1904) and then 640 acres (Stock-Raising Homestead Act, 1916), but the fundamental mismatch persisted. Fixed-size rectangular homesteads could not support commercially viable cattle operations on semi-arid rangeland (Foss, 1960, p. 28).

Ranchers ran livestock on adjacent public domain land without formal authorization, competing for forage in a race to the bottom that degraded rangeland quality and generated chronic conflict over access. By the early 1930s, the Forest Service estimated that 67 percent of the public domain was depleted (Foss, 1960, p. 34). Although the dust storms of 1934 helped break the legislative deadlock, the TGA addressed rangeland degradation in the western states, not the Dust Bowl's cropland crisis on the southern Great Plains.[1] The dust storms exposed the ecological consequences of unregulated use, and the economic consequences were equally severe: without secure tenure, ranchers could not use rangeland access as collateral, depressing land values and constraining credit access for western farmers.

The need for regulation was widely recognized, yet politically intractable. Over fifty years, ten grazing bills failed to pass Congress, blocked by jurisdictional disputes between the Departments of Interior and Agriculture and by western Republicans still committed to the homesteading ideal (Foss, 1960, p. 39). The Hoover administration proposed transferring public lands to the states, but the states refused the administrative burden (Muhn et al., 1988). A trial grazing district at Mizpah-Pumpkin Creek in Montana, proposed by local stockmen's associations, demonstrated the benefits of regulated access: when a severe drought struck in 1930, the trial district entered the following season with 20 percent more vegetation than adjacent unregulated rangeland (Pfeffer, 1951). Congress took note, but broader legislation remained stalled.

2.2 The Taylor Grazing Act of 1934

Three factors converged to break the legislative deadlock. First, the new heads of the Departments of Interior and Agriculture, Harold Ickes and Henry A. Wallace, both believed firmly in federal regulation of the western range. Second, Ickes engaged in political arm-twisting: he withheld Civilian Conservation Corps camps from the public domain, arguing that conservation work on unregulated land would be "unsound, economically" (Foss, 1960, p. 56), and threatened to withdraw public lands and begin regulating grazing under his own authority. Third, dust storms in May 1934 gave

[1] The Dust Bowl centered on western Kansas and the Oklahoma and Texas panhandles; the TGA states lie west of it. The separation matters for identification: the 1934 rainfall instrument (Section 5.5) captures rangeland conditions in TGA states, not Dust Bowl severity, and drought-relief programs (FERA, AAA drought payments) targeted the Dust Bowl states rather than the counties in our sample.

western senators the political cover to pass the bill.[2] The Senate passed the bill on June 12, and President Roosevelt signed it on June 28, 1934.[3]

The TGA ended nearly a century of homesteading by withdrawing 142 million acres of unreserved public domain from further entry (Bühler, 2023). After an extensive soil reconnaissance survey in October 1934 and public hearings in early 1935, the newly created Division of Grazing established 37 grazing districts across nine western states. The Act's core mechanism was the *grazing permit*: a formal, documented right to graze a specified number of livestock on a designated allotment for a ten-year renewable term. The land itself remained federal property: a permit conveyed grazing capacity, not a claim to a particular parcel, and the district board could move an operator's allotment between seasons. Grazing fees were set at $0.05 per animal unit month (roughly $1.15 in 2023 dollars), far below private range rates of up to $20 per AUM. Revenue was reinvested within the grazing districts to build fences, waterholes, and stock driveways.

Three features of the TGA matter for political economy. First, the Act allocated permits to ranchers with "prior use" of the land, rewarding established operators and creating a barrier to entry. Advisory boards of local stockmen distributed the animal unit months, and the largest operators took the bulk of them, an allocation that reached the courts (Libecap, 1981; Klyza, 1994). Second, the Act tied permit allocation to ownership of nearby "base property," linking grazing rights to specific parcels of private land. Third, the ten-year term was nearly automatically renewed: revocation was restricted to cases of grazing violations, and permits pledged as collateral for a loan were protected even then, making them de facto property rights tied to farms (Bühler, 2023). When a ranch was sold, the grazing permit effectively transferred with it.

The wealth implications were direct. Formalized grazing rights eliminated the uncertainty of open-access competition and made ranch operations viable on a long-term planning horizon. Because permits attached to base properties, they raised the collateral value of those properties, the mechanism De Soto (2000) emphasizes for property formalization generally, though Bühler (2023) finds that the TGA's value gains operated primarily through investment and herd expansion rather than through credit access. The enhanced collateral nonetheless eased credit constraints that had plagued

[2]When the House passed H.R. 6462 on April 11, 1934, Senate passage remained uncertain. On May 11, dust storms carried sand from the western deserts to the steps of the Capitol. Senator Gore of Oklahoma called the storms "the most tragic, the most impressive lobbyists, that have ever come to this Capital" (Foss, 1960, p. 58).

[3]The Act's origins complicate a simple partisan narrative. Republican Congressman Don Colton first proposed the bill that would become the TGA; Democrat Edward Taylor of Colorado copied it and reintroduced it in the seventy-third Congress (Foss, 1960, p. 56). The legislation passed with Democratic majorities, but the underlying idea had bipartisan roots in western stockmen's demands for regulated access.

western agriculture throughout the 1920s and early 1930s. Bühler (2023) estimates that the average rancher inside a grazing district reported 90 percent more cattle and 40 percent higher farm values compared to the trend in unaffected counties.

2.3 The Political Context

The TGA passed during the peak of New Deal legislative activity, enacted by a Congress with overwhelming Democratic majorities. The House roll-call vote on H.R. 6462 split partly along partisan and regional lines: delegations from the nine core TGA states (Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, and Wyoming) voted heavily in favor, though support was not universal. Republican opponents framed the Act as federal overreach, with some comparing it to "dictatorship in Russia" (Foss, 1960, p. 54). The expansion of federal authority over western lands, not the grazing permits themselves, was the main dividing line between the parties.

The ten-year renewable leases created a direct electoral incentive. Because a change in government could credibly have led to revocation of the TGA, affected ranchers had reason to support Democratic candidates in 1936 and subsequent elections. Yet this incentive proved transient. By the late 1930s, the Republican Party platform evolved: as the policy became fully implemented and its benefits visible, Republicans in Congress stopped challenging ranchers' grazing rights (Muhn et al., 1988). Once both parties accepted the TGA as settled policy, the electoral incentive to support Democrats on these grounds disappeared.[4]

3 Conceptual Framework

The TGA closed an open-access commons without privatizing it (Hardin, 1968; Ostrom, 1990; Coggins and Lindeberg-Johnson, 1982; Klyza, 1994). Ranchers received documented rights to a fixed number of animal unit months. Title to the land stayed with the federal government, and the district board could reassign the plot itself between seasons (Bühler, 2023). Those limited rights still carried a rent: permits went out at five cents per animal unit month, against private range rates up to twenty dollars. The district boards that made the allocation favored the operators already established on the range (Libecap, 1981). The permits attached to base property and passed to the buyer when a ranch was sold. Regulated access also raised what the range produced: vegetation inside the districts still runs about 10 percent above land just outside the boundary, and ranchers inside reported 90 percent more cattle and 40 percent higher

[4]The U.S. Grazing Service, created to implement the Act, merged in 1946 with the General Land Office to form the Bureau of Land Management within the Department of the Interior. Federal authority over grazing remains contentious in the western states to this day.

farm values than the trend outside (Bühler, 2023). The Act resolved a commons problem and granted a durable rent to a group defined by prior use, and our outcomes measure the political consequences of the rent.

Whether a reform of this kind builds a coalition or dissolves one depends on what its beneficiaries then want from government. Three hypotheses yield distinct predictions about the direction, heterogeneity, and behavioral content of the electoral response (Table 1).

Hypothesis 1: Gratitude. Beneficiaries reward the party that delivered tangible benefits, a channel documented for federal spending in U.S. House elections (Levitt and Snyder, 1997) and for conditional cash transfers (De La O, 2013; Zucco, 2013; Manacorda et al., 2011). Applied to the TGA, gratitude predicts a durable and uniform pro-Democratic shift in treated counties: durable as long as voters remember who enacted the policy, and uniform because every treated county received the same policy. In survey data it predicts broadly pro-government attitudes among beneficiaries; at the polls, Democratic gains should be larger in states whose congressional delegations visibly championed the legislation (Table 1).

Hypothesis 2: Wealth and the demand for redistribution. A competing account reverses the sign: three channels link a wealth gain to reduced demand for redistribution, and because our data cannot separate them we refer to the composite as the "wealth channel." In the Meltzer and Richard (1981) framework, voters whose income exceeds the mean oppose redistribution because net transfers flow away from them. Ansell (2014) adds that rising asset values substitute for social insurance by providing a private buffer against shocks. The third channel is debt: a mortgaged rancher in 1934 depended on the federal government not only for his grazing rights but for the credit and foreclosure relief that kept him on his land, and a reform that raised the value of that land cut his reliance on the relief.[5]

Leverage determines how strongly a given gain reduces that demand, because equity is a residual. A farm worth V against debt D holds equity $V - D$. A rise in V accrues entirely to the owner, so the same proportional gain improves a thin equity position more than a thick one. We split treated counties at the 1930 median debt-to-value ratio into a high-debt and a low-debt group. Mean leverage is 42.5 percent in the high-debt group and 32.2 percent in the low-debt group, so a one percent rise in farm

[5]The 1936–37 Gallup data (Section 4) do not separate this third channel from the other two. TGA-state respondents were no less supportive of federal farm loans than respondents elsewhere (-0.6 percentage points, SE 2.1), and that gap differs from the gap on farm price and income support by $+1.4$ points (SE 2.7). The comparison is state-level, predates the 1942 reversal, and could only have detected a contrast larger than roughly 8 points. It does not rule the channel out, but it provides no support for singling it out.

values raises equity by 1.74 percent in high-debt counties and 1.48 percent in low-debt ones, a response about 18 percent larger. Pre-treatment leverage therefore ranks counties by how much a given gain improves their net position, not by the size of the gain itself. The predicted response is a threshold: a household changes its vote when the gain carries it past the point where the coalition stops being worth supporting. The most leveraged households sat closest to that point, so a common gain should carry them past it while leaving the rest in place.

Applied to the TGA, the wealth channel predicts a delayed reversal rather than an immediate shift: the initial pro-Democratic boost reflects short-run policy salience, and the timing of the reversal depends on when market conditions capitalize the underlying asset (Section 7.4).[6] The shift away from Democrats should concentrate among counties whose leverage left them most exposed to a given gain, a step at high leverage rather than a gradient across the distribution, and in survey data the opposition should be selective: voters reject the platform that costs them while continuing to support the policies that benefit them directly (Table 1). The delegation's vote should be irrelevant, since the material benefit accrued regardless of political credit.

Hypothesis 3: Ideological conversion. Experience of successful government intervention may produce a broader shift in political worldview: newly prosperous ranchers might adopt a smaller-government philosophy that extends beyond redistribution to all forms of state involvement (cf. Sears and Funk, 1991; Margalit, 2013). Property formalization is one documented route to such a shift, since Di Tella et al. (2007) find that land titling in Buenos Aires moved squatters toward pro-market beliefs. The TGA conferred access to range that stayed in federal ownership, so that route is weaker here than in a titling program, though it is not absent, and the tests below treat it as live. Applied to the TGA, conversion predicts the same shift away from Democrats as the wealth channel but uniform across treated counties regardless of leverage, and in survey data lower support for government across *all* policy domains, in regulation, public ownership, and spending as well as in redistribution (Table 1). The breadth distinguishes ideology from self-interest: the ideological convert opposes government on principle, while the self-interested voter opposes only the policies that cost him (Alesina and Giuliano, 2011).

[6] Grazing districts were established in stages between 1935 and 1938, and the associated infrastructure improvements (fences, waterholes, stock driveways) took several years to materialize fully. The collateral value of formalized permits accumulated gradually as banks recognized the new property rights and adjusted lending practices.

Table 1: Competing Hypotheses and Observable Implications

Hypothesis	Long-run direction	Heterogeneity by debt	Roll-call pattern	Gallup pattern
Gratitude	Pro-Dem (durable)	Uniform	Pro-TGA deleg. → larger Dem gain	Broadly pro-government
Wealth channel	Anti-Dem (after reversal)	Step at high leverage	Delegation vote irrelevant	Selective: pro farm benefits, anti-redistribution
Ideological conversion	Anti-Dem	Uniform	Delegation vote irrelevant	Uniformly anti-government

4 Data

Outcome: Electoral Data The primary dataset is a county-by-election-year panel of U.S. congressional elections from 1910 to 1972. The outcome variable is the Democratic share of the two-party vote (Dem), measured in percentage points.

Our baseline specifications use the full 1910–1972 sample. Several robustness checks restrict the window to 1918–1960, which balances pre- and post-treatment coverage (eight elections on each side of the 1934 act) and avoids two sources of compositional noise: the entry of women into the electorate after the Nineteenth Amendment (1920), which altered turnout patterns in western states, and the Civil Rights realignment of the 1960s, which reshuffled party coalitions along dimensions orthogonal to the TGA.

Treatment: Historic Grazing Land The treatment variable *Historical Grazing* is a county-level binary indicator equal to one for any county whose boundaries overlap with a grazing district established under the TGA (Bühler, 2023). These counties are located in nine western states: Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, and Wyoming. The treatment captures whether a county's agricultural economy benefited when the TGA formalized grazing rights and the associated property-value gains. Because the binary indicator measures geographic overlap rather than individual permit receipt, our estimates are best interpreted as intention-to-treat effects for rangeland counties.[7]

The main difference-in-differences estimand interacts *Historical Grazing* with a post-1934 indicator (*TGA*), producing our main treatment variable *Historical Grazing* $\times$ *TGA*. Although the Act was signed on June 28, 1934, no grazing districts, permits, or infrastructure existed before the soil reconnaissance survey of October 1934 and public hearings in early 1935. The 1934 congressional election therefore serves as the event-study reference year, and the post-treatment period begins with the 1936 election, the first federal election after implementation was under way.

Figure 1 maps the grazing districts in two panels. Panel A sets the treatment geog-

[7] We also employ a continuous grazing area share definition and find stronger results, but retain the binary treatment for simplicity of interpretation.

raphy against the congressional district boundaries of the 73rd Congress, and shows how grazing districts cut across those boundaries, generating within-district variation in treatment exposure that our county-level analysis exploits. Panel B gives county-level Democratic vote share in the 1932 congressional election, the last before the Act. Treated counties span the partisan spectrum, from solidly Republican areas in the northern Rockies to strongly Democratic counties in the Southwest, alleviating the concern that treatment correlates with pre-existing partisan alignment.

Controls We control for per-capita New Deal spending to absorb the possibility that TGA counties received differentially higher federal transfers through channels other than the grazing act itself. County-level expenditure data for major New Deal programs (FERA, WPA, AAA, CCC, PWA, and RFC) come from Fishback et al. (2003). We divide each program's spending by 1930 county population and aggregate the resulting per-capita expenditures into a single index using the inverse-covariance-weighted method of Anderson (2008).[8]

The extended specification adds three pre-treatment agricultural covariates from the 1930 Census of Agriculture, the last available wave before treatment: average land value per acre, average farm size, and the debt-to-value ratio (total farm mortgage debt divided by total farm property value). The first two control for underlying economic activity. The debt-to-value ratio plays a dual role: it enters the covariate vector and serves as a key moderator in the wealth-channel analysis. For the latter, we split counties at the 1930 median (36.5%) into high- and low-debt groups, testing whether the political response to the TGA varied with pre-treatment balance-sheet exposure.

Roll-Call Votes House roll-call votes on H.R. 6462 (the Taylor Grazing Act) come from the Congressional Record, 11 April 1934. Our primary measure is the vote of each county's own representative, which requires assigning counties to congressional districts. We map each county to its 73rd-Congress district by largest intersected area share, using digitized historical district boundaries.[9] Paired and announced positions are coded with the vote they express, and members not voting are coded as missing rather than as opponents. We also report a coarser state-delegation measure, which classifies a delegation as "pro-TGA" if more than half of its representatives voted Yea.[10]

[8]The method weights each program by the inverse of its covariance with the others, so programs carrying independent information count more. A single index avoids collinearity among six correlated regressors while capturing how heavily New Deal spending targeted each county.

[9]Only 4 of 748 counties are divided by a district line, so the assignment rule does little work. The constituent-county lists carried in the boundary file agree with the spatial assignment for all 386 counties they cover.

[10]That threshold is not innocuous: Idaho's two representatives split one to one, so the strict inequality classifies the state as opposed. Coding ties as pro-TGA instead moves the delegation interaction from -8.8 to -17.7. The own-representative measure is not exposed to this choice.

Gallup Survey Data Individual-level survey data come from the American Institute of Public Opinion (AIPO) polls conducted in 1936–37. We extract respondents from TGA and non-TGA states and compare state-level attitudes across 13 policy questions (see Table 6 for the full list), grouped into farm-specific questions (government loans for farmers, AAA revival, farm product price supports, farm benefit spending) and general government questions (government takeover of business, profit regulation, public ownership of utilities, Roosevelt spending policy, among others). These data allow direct observation of whether TGA beneficiaries adopted a general pro-government ideology or maintained narrowly self-interested policy preferences.

The 1936–37 polls precede the electoral reversal. To observe the same populations afterwards we add the pooled AIPO records assembled by Caughey et al. (2019), which run from 1936 to 1952 and carry both state identifiers and an occupational classification marking farm households.[11] Restricting to farm respondents raises beneficiary density from 2.0 to 7.7 percent of the group observed, and the analysis in Section 9.2 uses that restriction throughout. Three questions on government ownership recur with identical wording across the period; the questions on federal farm support do not. We match those by policy object, using the topic classification of the source archive.

Descriptive Statistics Table 2 summarizes the variables used in our analysis. The sample covers 742 counties in 13 states across 20,802 county-election observations. The nine TGA states (Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, and Wyoming) supply both treated and control counties; four additional states (California, North Dakota, South Dakota, and Texas) contribute control counties only. The mean debt-to-value ratio stood at 35.1%, so farm mortgage debt represented roughly a third of property values on the eve of the TGA, and that ratio defines the high- and low-debt split used in the triple-difference below.[12] Table A.1 reports formal balance tests comparing pre-treatment covariates across TGA and non-TGA counties. Conditional on state fixed effects, the key variables for identification (debt-to-value ratios, land values, and the aggregate New Deal spending index) are balanced; the main imbalance is in AAA spending, which our New Deal spending control directly addresses.

[11] That classification matters more than the added years. Permittee households are 2.0 percent of the electorate in the nine treated states, against 4.6 percent of the median treated county (Section 7.3), so a contrast between whole state populations averages over roughly fifty non-beneficiaries for every beneficiary.

[12] The average Democratic vote share is 54.5 percent (SD = 27.1). Average land values in 1930 were \$37.76 per acre, which masks wide dispersion between irrigated cropland and arid rangeland. New Deal spending varied across programs: AAA transfers averaged \$66.65 per capita against \$21.90 for FERA and \$39.21 for WPA, reflecting the AAA's focus on crop-producing counties.

Table 2: Summary Statistics

	Mean	SD	Counties	Obs.
Democratic vote share (%)	54.45	27.05	742	20,802
Land value per acre (1930)	37.76	137.90	739	20,768
Farm size in acres (1930)	924.24	2,453.37	740	20,800
Debt-to-value ratio (1930)	35.12	7.46	740	20,800
FERA spending per cap.	21.90	15.94	740	20,800
AAA spending per cap.	66.65	87.91	740	20,800
WPA spending per cap.	39.21	47.18	740	20,800

Notes: The county $\times$ election-year panel: 742 counties in 13 western and plains states, contested congressional elections 1910–1972. Democratic vote share is the county-level Democratic share of the two-party vote (%). Agricultural variables are from the 1930 Census of Agriculture; the debt-to-value ratio is total farm mortgage debt over total farm property value, in percentage points. New Deal spending is dollars per capita of 1930 population (Fishback et al., 2003).

5 Empirical Strategy

5.1 Difference-in-Differences

The baseline specification estimates the effect of TGA treatment on Democratic vote share using a standard two-way fixed effects model.[13]

$$\text{Dem}_{ct} = \beta \cdot (\text{HG} \times \text{TGA})_{ct} + \alpha_c + \gamma_{st} + \sum_k \delta_k \cdot z_c \times \mathbf{1}(t = k) + \varepsilon_{ct} \tag{1}$$

where Dem_{ct} is the Democratic vote share in county c at election t; $HG \times TGA_{ct}$ is the interaction of the Historical Grazing indicator (HG) with a post-1934 dummy (TGA); α_c are county fixed effects; γ_{st} are state-by-year fixed effects; and $z_c \times \mathbf{1}(t = k)$ denotes the per-capita New Deal spending index interacted with year indicators, allowing the relationship between New Deal spending and Democratic vote share to differ in each election year. The extended specification adds 1930 land value, farm size, and debt-to-value ratio as additional covariates, each interacted with year indicators. Standard errors are clustered at the county level throughout.

State-by-year fixed effects absorb all time-varying state-level confounds, including changes in state political leadership, ballot initiatives, and statewide economic conditions. The identifying assumption is that, conditional on the full set of fixed effects and the New Deal spending control, counties with and without historic grazing land would have followed parallel trends absent the Taylor Grazing Act.

[13] Recent work has shown that TWFE can be biased when treatment timing varies across units (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). This concern does not apply here: the TGA took effect simultaneously for all treated counties in 1934. There is no staggered rollout, and the treatment-timing decomposition collapses to a single two-by-two comparison.

Identification Assumption A natural concern is that grazing districts were not randomly assigned. The institutional record clarifies how selection operated. In late 1934, the Division of Grazing dispatched soil reconnaissance teams to classify western rangeland by degradation severity, forage capacity, and erosion risk. Congress set the acreage ceiling; the Division drew district boundaries along the Public Land Survey System grid based on the survey results (Foss, 1960, pp. 34–41). Advisory boards composed of local ranchers were established *after* districts were created to manage permit allocation *within* existing boundaries (Foss, 1960, pp. 62–67). The boards did not determine which land entered the system; they distributed permits among ranchers inside boundaries already drawn by federal surveyors. District formation proceeded as a rapid administrative rollout between 1935 and 1938, its pace dictated by the availability of survey teams rather than by county-level political lobbying (Muhn et al., 1988). There is no historical evidence that surveyors considered local political conditions, voter registration, or partisan leanings; the relevant selection variables were ecological (aridity and prior grazing intensity), characteristics that county fixed effects and the agricultural controls absorb.

Bühler (2023) corroborates this reading, showing that the 1934 rainfall instrument generates treatment variation orthogonal to these selection factors. Our balance tests (Table A.1) confirm that, conditional on state fixed effects, TGA and non-TGA counties are balanced on debt-to-value ratios, land values, and the aggregate New Deal spending index. The event-study pre-trends provide the strongest evidence (Section 6).

Importantly, any residual political selection would bias *against* our finding. If politically organized, pro-Democratic ranching communities were disproportionately selected into treatment, they would be predisposed to *maintain* Democratic loyalty. Observing a reversal despite this positive selection makes the wealth-channel interpretation conservative: the true anti-Democratic effect of the TGA's wealth shock is, if anything, larger than what we estimate.

5.2 Event Study

The central threat to identification is political selection: the same political conditions that helped bring the Act about could predict how treated counties voted afterwards. If grazing-land counties were already trending toward or away from the Democrats before 1934, any post-1934 shift could continue that trend rather than respond to the Act. An event study tests this concern directly: replacing the single interaction in Equation (1) with year-specific interactions, $\sum_{k\neq 1934} \beta_k \cdot \mathrm{HG}_c \times \mathbf{1}(t = k)$, yields a coefficient for each election relative to 1934, the last pre-treatment election.[14] The pre-

[14]The Taylor Grazing Act was signed in June 1934 and implemented beginning in early 1935. The November 1934 election is therefore post-decision but pre-implementation: voters knew of the Act, but

treatment coefficients test the parallel-trends assumption, and the post-treatment coefficients trace the dynamic treatment effect. The sample runs from 1920 to 1950, seven pre-treatment election cycles and eight post-treatment ones.

Flat pre-trends establish partisan comparability, which is all the identifying assumption requires; they establish nothing about ideology, since party labels mapped loosely onto redistribution in this period. Section 9.2 addresses ideology directly with individual policy attitudes. The post-treatment path in turn discriminates between mechanisms: durable partisan loyalty predicts a shift to a new level, while the wealth channel predicts an initial pro-Democratic effect followed by a reversal once the wealth gain materialized.

5.3 Triple-Difference: The Collateral Channel

The difference-in-differences estimate tells us whether TGA counties shifted their voting behavior, but not *why*. Pre-treatment leverage provides the sharpest cross-sectional test of the candidate channels (Table 1). A given property-value gain improved net worth most where debt stood highest against the land, so the wealth channel predicts a political response concentrated in high-debt counties. Gratitude and ideological conversion instead predict effects independent of leverage, since every treated county received the same policy. We test this prediction with a triple-difference specification:

$$\begin{aligned} \text{Dem}_{ct} = & \beta_1 \cdot \text{HG} \times \text{TGA}_{ct} + \beta_2 \cdot \text{HG} \times \text{TGA}_{ct} \times \text{HighDebt}_c \\ & + \beta_3 \cdot \text{TGA}_t \times \text{HighDebt}_c + \alpha_c + \gamma_{st} + \varepsilon_{ct} \end{aligned} \tag{2}$$

where HighDebt_c indicates counties with above-median debt-to-value ratios in 1930 (median = 36.5%). The coefficient β_3 absorbs differential post-1934 trends among indebted agricultural counties regardless of TGA exposure. β_1 captures the TGA effect in low-debt counties, and $\beta_1 + \beta_2$ the effect in high-debt counties. The key parameter is β_2: zero or positive under gratitude, negative under the wealth channel.

5.4 Mechanism Discrimination

Even if the political response concentrates in high-debt counties, as the wealth channel predicts, voters might also reward the representatives who delivered the policy. Separating this credit from the benefit itself requires the two to vary independently, and House roll-call votes on the Act provide that variation: representatives split on H.R. 6462, while the gains from formalized grazing rights accrued to treated counties regardless of how their representative voted. We interact HG × TGA with the vote of

grazing districts had not yet been established and no permits had been issued. We classify 1934 as the base year accordingly.

the county's own representative:

$$\text{Dem}_{ct} = \beta_1 \cdot \text{HG} \times \text{TGA}_{ct} + \beta_2 \cdot \text{HG} \times \text{TGA}_{ct} \times \text{Yea}_{d(c)} + \alpha_c + \gamma_{st} + \varepsilon_{ct} \tag{3}$$

where $\text{Yea}_{d(c)}$ equals one if the representative of county c's district d voted for H.R. 6462. Gratitude predicts $\beta_2 > 0$, since counties whose representative championed the Act should shift more toward Democrats; the material benefit alone predicts $\beta_2 = 0$. A negative estimate rejects gratitude and then requires an account of its own, which Section 9 gives. Because the credit regressor varies at the district level, we report district-clustered standard errors alongside county clustering. We also report the state-delegation version of the interaction, so the two measures can be compared directly. The test assumes that a representative's vote is orthogonal to county-level trends in Democratic voting, conditional on the fixed effects. County fixed effects absorb time-invariant differences between Yea and Nay districts, and state-by-year fixed effects remove state-level partisan shocks. A differential within-state trend remains conceivable but unlikely, given that western members of both parties supported the Act (Section 2).

5.5 Instrumental Variables

We follow Bühler (2023), who constructs a rainfall instrument for TGA treatment in the nine western states. In October 1934, federal land surveyors classified rangeland into grades of degradation; counties that received less rainfall during the survey month appeared more degraded, making them more likely to be placed under TGA regulation and ultimately to receive more grazing permits. Standardizing October 1934 rainfall by each county's historical mean and standard deviation isolates this transitory shock from persistent climate differences that might correlate with economic structure or political preferences. We instrument HG $\times$ TGA with the interaction of the standardized measure and a post-1934 indicator. Section 8 discusses the exclusion restriction, that survey-month rainfall affected Democratic vote share only through TGA treatment intensity, together with the results.

6 Results I: The TGA and Democratic Vote Share

6.1 Event Study

The design requires that, absent the Act, counties with and without historic grazing land would have followed the same Democratic vote-share trajectory. Figure 2 reports the event-study coefficients from Section 5 and provides a direct test.

The pre-treatment coefficients (1920–1932) are reassuring. Across more than a decade of elections before the TGA, grazing-land counties tracked their non-grazing counterparts closely. All seven estimates cluster near zero, and none reaches conventional significance levels. While point estimates range from -0.42 to $+3.40$, all fall well within the 95% confidence band. Bühler (2023) corroborates this finding, showing that average farm values and cattle stocks followed parallel trajectories in TGA and non-TGA counties within the same states before 1934. No pre-existing divergence in partisan trajectories threatens identification.

After 1934, voters in treated counties rewarded Democrats in the three election cycles immediately following the Act, with the effect peaking at $+3.73$ percentage points in 1940 (SE $= 1.68$). Yet the effect proved short-lived. By 1942, the coefficient drops to -1.37 (SE $= 1.16$), a swing of 5.1 percentage points in a single two-year cycle, and it remains negative through 1950.[15] The TGA produced a temporary Democratic gain, not a lasting realignment: the effect reversed once the wealth transfer materialized in farm values.

Why did the reversal occur in 1942, not earlier? A farm-value event study (Figure C.3) shows that the wealth gain was still modest and statistically insignificant in 1940, six years after enactment, and widened sharply only during the war, when commodity demand capitalized the investments that secure tenure had enabled.[16] Secure tenure was necessary but not sufficient; wartime demand provided the second shock that activated the wealth channel (Section 7.4).

6.2 Difference-in-Differences

The event study traces the treatment effect over time; the pooled difference-in-differences collapses it into a single estimate that captures the average effect across the full post-treatment period. Because the pooled coefficient averages over both the brief pro-Democratic window (1936–1940) and the longer anti-Democratic period that followed, a negative sign indicates that the reversal dominates the initial boost.

Table 3 reports these estimates from Equation (1), progressively adding controls across five columns. Column (1) presents the baseline specification with county and state-by-year fixed effects only. Columns (2)–(4) progressively add the per-capita New Deal spending index, 1930 land values and farm size, and the 1930 debt-to-value ratio, each interacted with year indicators. The coefficient is stable throughout, indicating that observable differences in federal spending, agricultural structure, and pre-treatment leverage do not drive the result.

Column (5) restricts the sample to the 306 counties in the nine TGA states. The

[15] The 1944 coefficient reaches -3.57 percentage points (SE $= 1.72$).

[16] Prices for the goods produced on TGA land rose sharply during the war. USDA prices received by farmers show beef cattle increasing roughly 65% between 1940 and 1943 (from \$6.80 to \$11.20 per cwt), and wool rising approximately 40% (from \$0.27 to \$0.38 per pound). Source: USDA National Agricultural Statistics Service, Historical Prices Received by Farmers.

coefficient sharpens to -3.76^{***} (SE $=$ 1.23), more than double the full-sample estimate, though that significance level does not survive spatial adjustment (Section 8). The larger magnitude suggests that the full-sample estimate is diluted by non-western control counties whose political dynamics have little bearing on the TGA's effects.

Column (6) replaces the binary indicator with continuous grazing area share, the fraction of each county's area within a grazing district. The coefficient of -5.01^{***} (SE $=$ 1.76) confirms that the effect operates at the intensive margin: counties with larger grazing-district coverage shifted more strongly against Democrats. We retain the binary indicator in the main specifications for simplicity of interpretation: it compares counties with any grazing-district presence to those without.

All six columns cluster on the county, which allows a county's errors to correlate over time but not with those of its neighbors. Allowing correlation across neighbors as well leaves the sign and magnitude unchanged and widens the interval: the column (5) estimate carries a p-value of 0.057 under a 300-kilometer Conley variance and 0.177 clustering on electoral districts (Section 8).

7 Results II: Property Rights and the Wealth Effect

Section 6 established that TGA counties shifted against Democrats after 1934. Several channels could produce this pattern: gratitude wearing off, ideological conversion, or, as we argue, wealth reducing demand for redistribution. Bühler (2023) demonstrates that the TGA raised property values, primarily through investment and herd expansion rather than through credit access. If this wealth shock drives the political shift, its effect should vary with leverage, since a common gain did most to change the position of the most indebted counties.

7.1 The TGA and Farm Leverage

Farm mortgage data in the Census of Agriculture exist for 1920, 1925, 1930 and 1940, which gives three pre-treatment observations and one after the Act. A series that short cannot separate a level shift from a continuing trend, so we read it as corroboration of the mechanism rather than as identification of it. Figure C.4 plots mean debt-to-value ratios for grazing-district and non-grazing counties across the nine states where districts formed. On the eve of the Act the two groups carried the same leverage, 36.8 percent in each. Leverage then rose in both over the following decade, as it did across American agriculture, but it rose by 4.8 points in grazing-district counties against 8.5 points in their neighbors.[17]

[17] The one pre-treatment departure comes in 1925, when grazing-district counties carried 43.7 percent leverage against 39.3 percent among their neighbors, a gap that had closed entirely by 1930. The spike

A ratio can move for two reasons, and only one of them is a wealth effect: the debt in its numerator can change, or the value of the assets securing it can. The slower rise in grazing-district counties came from the value side: their debt per farm *rose* while their neighbors' fell, and their leverage nonetheless rose more slowly, because farm values held up.[18]

Identification of the political effect does not run through this series. It rests on the vote-share pre-trends in Figure 2, flat across seven pre-treatment elections, and on the placebo triple-difference estimated on pre-treatment data (Table C.4).[19] The ratio does different work in what follows: measured in 1930, before the Act, it ranks counties by how much a given gain improved net worth.

7.2 Who Turned Against Democrats?

If the wealth channel drove the political reversal, the anti-Democratic shift should concentrate where a given gain did most to improve net worth: in the counties carrying the most debt against their land. We exploit this variation in a triple-difference design that interacts TGA treatment with the 1930 debt-to-value ratio. The design requires leverage to be uncorrelated with treatment, and in 1930 it is. Among the nine states where grazing districts formed, grazing-district and non-grazing counties enter with the same mean leverage, 36.8 percent in each group, and a Kolmogorov–Smirnov test cannot reject equality of the full distributions ($p = 0.78$).[20]

Table 4 presents the estimates from Equation (2). The interaction with pre-treatment debt is negative and significant across all six specifications. In the richest binary specification (column 4, 1918–1960 with covariates), the base TGA effect in low-debt counties is +5.67** (SE = 2.50) and the interaction is −5.72** (SE = 2.40). Against a

came from borrowing rather than from values: land values collapsed after 1920 in both groups, while mortgage debt per farm rose 9.5 percent in grazing-district counties and their neighbors retired 4.3 percent of theirs. Operators running stock on range they held by custom rather than by title may have been readier to borrow against collateral that was losing value, and they unwound the debt over the second half of the decade. This single deviation is what the coefficient event study in Figure C.5 converts into significant pre-treatment coefficients, because its 1930 base year sits at the bottom of the recovery from the spike.

[18] Debt per farm rose 7 percent in grazing-district counties between 1930 and 1940 and fell 2 percent among their neighbors, while farm values fell 9 percent against 20 percent elsewhere. Two limits accompany the exercise: the Census of Agriculture reports the ratio and the value, so debt per farm is recovered as their product rather than measured independently, and the 1940 components are individually insignificant (+\$324, $p = 0.50$ on debt; +\$1,001, $p = 0.28$ on value). The decomposition locates which side of the ratio moved rather than testing it.

[19] Figure C.5 reports the coefficient event study for the leverage series with its pre-trend diagnostics; neither the joint test of its pre-treatment coefficients nor a test of the 1940 estimate against an extrapolated pre-trend supports a causal reading. Section 7.3 reads the size of the wealth shock off farm values in dollars instead.

[20] Medians are 36.6 and 36.3 percent and the quartiles bracket one another. The median split classifies 58 percent of grazing-district counties and 56 percent of their neighbors as high-debt. Across all thirteen states the raw gap is +1.9 points, which state fixed effects absorb (+0.83, SE = 0.81; Table A.1), and the specifications below carry state-by-year fixed effects throughout.

pre-treatment Democratic share of 45.7 percent in treated counties, the low-debt gain amounts to 12 percent of that base, while the net effect in high-debt counties is near zero. Replacing the binary treatment with continuous grazing area share strengthens the interaction.[21]

Low-debt TGA counties *rewarded* Democrats (+5.67**), a response the pooled estimate had averaged away. The negative pooled effect from Section 6 comes largely from high-debt counties, where the net effect ($5.67 - 5.72 \approx 0$) shows the wealth channel offsetting the initial Democratic boost. Outside TGA areas, high-debt counties trended *toward* Democrats after 1934, the opposite of the pattern in treated counties. The triple-difference therefore isolates a wealth effect specific to TGA counties.

The two responses do not reflect different gains. High- and low-debt TGA counties gained statistically indistinguishable amounts, in dollars and in proportion (Table C.9 and Figure C.6), and the difference in how far a common gain moved their equity is about 18 percent (Section 3). Converting that difference into an expected vote swing would require an elasticity we do not estimate, so we rest the argument on the shape of the response rather than its size. The effect is indistinguishable from zero across the three lower quartiles and significantly negative only in the most leveraged quarter, at −6.4 points (Table C.7), which is the step a threshold would produce rather than the gradient a linear response would produce.[22] What separated the two groups may therefore be what the gain changed for them. Our evidence does not identify which part of the wealth channel did that work. The gain may have lifted a rancher past the point where redistribution paid him, turned his land into a private buffer against bad years, or removed his need for federal credit. Each would have left him less reason to support the Democratic coalition.

7.3 How Large Was the Wealth Shock, and Who Received It?

A wealth channel has to clear two bars: the gain must be large enough to change how a household votes, and it must reach enough households to move a county's vote share. Size is the easier of the two. Treated counties gained $4,075 per farm by 1945 and $9,499 by 1950 relative to their non-grazing neighbors (Figure C.3), against a median

[21]The interaction ranges from −4.15** to −7.55*** across the six columns. Columns (5)–(6) report −7.54*** (SE = 2.54) without controls and −7.55*** (SE = 2.88) with controls, testing whether the wealth channel operates at the intensive margin.

[22]The estimates run from +6.7 points in the least leveraged quartile (SE = 4.11, $p = 0.11$), defined by a debt-to-value ratio below 30.8, through −3.4 and −2.8 in the middle two, to −6.4*** (SE = 2.27) in the most leveraged. The reward among low-debt counties is estimated more precisely when the lower half is pooled, as in Table 4, than within the bottom quartile alone. Raising the cutoff that defines a high-debt county strengthens the interaction from −4.15** at the median to −7.52*** at the top quartile, after which it flattens.

1930 farm value of $11,140 in those counties.[23]

Incidence is harder, and the arithmetic disciplines what we can claim. Permits attached to base properties, so the narrowest reading of the beneficiary group is permittee households. The 1950 Census of Agriculture records 19,446 farms reporting grazing permits across treated counties, 15.8 percent of farms in the median treated county.[24] Measured against congressional votes cast and allowing two voting adults per farm household, those households make up 4.6 percent of the 1940 electorate. Delivering the 1940 peak of 3.7 percentage points from that group alone would require 81 percent of them to change parties, and the larger post-1942 estimates would require more than all of them (Table C.8).

That figure bounds the requirement from above rather than refuting the mechanism, because it counts the permit holder alone and leaves out the relatives sharing the household economy, the hired hands, and the neighbors whose land and trade moved with his. Widening the group brings the requirement down quickly. All farm households cast 39 percent of the vote in the median treated county, and the peak effect asks a 9 percent swing of them, an ordinary electoral movement.

Two features of this reform carry the gain past the permit holder, and both are properties of a commons rather than of a transfer. Ending the race to graze a shared range improved the range itself (Bühler, 2023), and recovering forage, soil and water do not stop at a fence line, so land adjoining the public domain gained whether or not its owner held a permit. Herd expansion also raised what ranches spent locally on labor, supplies and credit. Both channels capitalize into land values across the county rather than onto base properties alone, which is what our outcome measures: average farm value in the Census of Agriculture is a county-wide mean over all farms, not a permittee-specific one.[25]

[23] $p = 0.038$ and $p = 0.004$ respectively, and the gains are consistent with the 40 percent estimate in Bühler (2023). Gains and base are both nominal, and the comparison spans years in which the price level rose. Deflated to 1930 dollars by the consumer price index, the gains are $3,780 and $6,580, or 34 and 59 percent of the 1930 value. The estimates themselves are unaffected, since each compares treated and comparison counties within the same census year (Section 7.4).

[24] 1950 is the first census wave to report grazing permits at the county level, sixteen years after the Act. If permits grew over the intervening period, these counts overstate the number of 1930s permittees, which makes the calculation below conservative.

[25] Were the entire county-wide effect concentrated on permit-holding farms, the county average would scale one-for-one with permit intensity (farms reporting permits in 1950 as a share of the county's 1930 farms), and regressing the 1930–1945 farm-value gain on that share across treated counties would return a slope near $25,900. The observed slope is $8,465 (SE = 3,571), about a third of that benchmark, so most of the measured gain does not track permit intensity. Two limits keep the exercise from partitioning the gain cleanly: permit intensity proxies treatment dose as well as incidence, and the 1950 counts measure 1930s permit incidence with error, which pulls the slope toward zero and would leave the same pattern even if the gain were fully concentrated.

7.4 The Timing of the Reversal: Two-Shock Complementarity

The decline from the 1940 peak to the 1942 trough is concentrated in a single two-year cycle rather than spread across elections. The break coincides with U.S. entry into World War II, raising the concern that wartime agricultural demand drove the anti-Democratic shift on its own, with no contribution from the TGA's wealth channel. Section 8 addresses this confound with three tests, and none of them supports wartime demand as the driver.

Wartime demand was not the only thing arriving in 1942. Federal price controls reached farm households directly and were in force before the election: the Emergency Price Control Act was signed in January 1942, and the General Maximum Price Regulation followed that spring. Meat rationing came later, in March 1943. Regulation can therefore contribute to the depth of the 1942 drop, and we do not claim otherwise. What it explains poorly is the persistence and the cross-section. The negative coefficients run through 1950, four years after controls lapsed. Controls also applied nationwide, so state-by-year fixed effects absorb their common component, and reproducing the triple-difference would require them to bind high-debt treated counties harder than low-debt treated counties within the same state. The within-TGA balance table (Table C.5) shows the two groups do not differ on livestock share or farm structure, which rules out the most direct such route on observables.

The war also raised the general price level, not only the prices of cattle and wool. Between 1940 and 1945 the consumer price index rose about 29 percent, while farm mortgages, written in nominal terms, did not move with it, so inflation cut the real burden of farm debt.[26] That relief was national, so state-by-year fixed effects absorb its common component. It was not uniform across households, because relief scales with what a farm owed, and leverage is the dimension the triple-difference splits on. But high-debt counties inside and outside the grazing districts received it alike, so that differential loads on the post-by-HighDebt term rather than on the triple interaction. Each farm-value estimate compares treated and comparison counties within the same census year, so the price level differences out of it.

The evidence points to a two-shock complementarity rather than a single treatment effect. The TGA was necessary: it created the asset (formalized rights, pledgeable collateral, and the investment they enabled). But the TGA alone had not generated a

[26] Consumer price index annual averages (1982–84 = 100) stood at 14.0 in 1940 and 18.0 in 1945. Source: U.S. Bureau of Labor Statistics, Consumer Price Index for All Urban Consumers, historical annual averages. The preceding decade ran the other way, from 16.7 in 1930 to 14.0 in 1940, so fixed mortgages grew heavier in real terms over exactly the window the leverage series covers, which may be part of why leverage rose in both groups in Figure C.4 rather than falling. The arithmetic is the one William Jennings Bryan campaigned on in 1896, when deflation raised the real value of farm mortgages and made free silver a farm-belt cause. County-level farm mortgage data end in 1940, so the wartime erosion of farm debt cannot be measured directly here.

large enough wealth gain by 1940 to trigger the reversal: farm values in treated counties had diverged only modestly and insignificantly (Figure C.3). WWII commodity demand was also necessary: it capitalized the TGA investments, which on the framework's account carried farm values past the point at which redistribution became a net cost. Nor was WWII alone sufficient: non-TGA livestock counties experienced the same commodity boom but showed no political reversal.

7.5 Partisan Convergence

An alternative interpretation is that Republicans neutralized the grazing issue by accepting the TGA as settled policy, removing the electoral incentive to support Democrats (Muhn et al., 1988). Two findings cut against convergence as the primary explanation. First, the timing is wrong. Republican acceptance of the TGA unfolded gradually through the late 1930s, yet the political reversal was sharp, concentrated in a single two-year cycle (1940–42). Second, convergence predicts a uniform shift across all TGA counties, since both parties accepted the same policy; it cannot explain why the reversal concentrated in high-debt counties while low-debt counties continued to reward Democrats. The roll-call evidence (Section 9) and the triple-diff heterogeneity (Table 4) further undermine convergence. Convergence may have facilitated the wealth channel by removing a competing reason to stay loyal, but it cannot account for the timing, the heterogeneity, or the direction of the shift in low-debt counties.

8 Robustness

Instrumental Variables As a complement to the DiD and event-study results, we report instrumental variables estimates using the rainfall instrument of Bühler (2023), described in Section 5.5.[27] Table C.1 reports a strong first stage ($F = 34$). Because more rainfall during the October 1934 survey meant less perceived degradation and fewer grazing permits, the positive reduced-form coefficient implies that greater TGA exposure reduced Democratic support.[28] Interacting the instrument with HighDebt also yields a positive reduced-form coefficient (+3.6**), and the 2SLS triple-difference in-

[27] The exclusion restriction is harder to defend for political outcomes than for the economic outcomes Bühler (2023) studied, since drought could affect voting through federal relief or out-migration; we therefore treat the IV as corroboration and emphasize the reduced form, which does not require it to hold exactly.

[28] The 2SLS estimate with controls (-8.7*, SE $= 5.04$) is 2.3 times the western-subsample OLS estimate (-3.76), consistent with classical measurement error in the binary treatment indicator and a LATE among marginal rangeland counties that may exceed the ATE. We cannot fully rule out a third contribution: if 1934 rainfall correlates with broader drought exposure, the IV estimate would be biased away from zero.

teraction matches the sign of the OLS interaction at several times its size (Table C.2).[29]

Placebo, Permutation, and Leave-One-Out Tests Permutation tests randomly reassign treatment status within each state 1,000 times. For the baseline DiD, the real coefficient falls in the tail of the placebo distribution (Figure B.1), yielding a two-sided p-value of 0.086. For the triple-difference, a separate permutation reassigns HighDebt status: the real interaction falls entirely outside the placebo distribution (Figure B.3). A placebo triple-difference estimated on 1920–1932 data with a fake treatment date of 1926 returns interactions of -2.0 ($p = 0.10$) with 1920 debt and $+0.9$ ($p = 0.49$) with 1925 debt (Table C.4), about a third the size of the treatment-period estimate of -5.72. Dropping each of the thirteen sample states in turn leaves the triple-difference interaction negative in every case and the baseline DiD estimate negative in twelve of the thirteen (Figures B.4 and B.2).

Spatial Correlation Clustering on the county lets a county's errors correlate across every election in the panel, but treats counties as independent of one another. Independence is the assumption most likely to fail: grazing districts span blocks of contiguous counties, and neighboring counties shared weather, livestock prices and political organization. Table C.13 therefore re-estimates the main coefficient of each headline table clustering on the electoral district and using Conley spatial-HAC variances (Conley, 1999) at 100, 200 and 300 kilometers; the event-study figures retain county-clustered bands. Both heterogeneity results hold: five of the triple-difference's six columns stay below the five percent level under every scheme, and the own-representative interaction never rises above 0.003. The average effect is less robust: the nine-state estimate's p-value moves from 0.002 clustering on counties to 0.057 at 300 kilometers and 0.177 clustering on districts. We read it as sign-consistent but imprecise, which is what the permutation test also reports ($p = 0.086$), and rest the argument on the heterogeneity in Section 7 and the roll-call evidence in Section 9.

WWII Commodity Demand The 1940–42 reversal coincides with U.S. entry into World War II. Because wartime cattle prices rose sharply and TGA counties had higher livestock shares, the reversal could reflect wartime commodity demand rather than the TGA's wealth channel. First, we interact each county's 1930 livestock share (and, separately, crop value per farm) with year indicators, letting counties with different agricultural compositions follow different partisan trajectories; the triple-difference interaction stays at -6.443*** (Table C.3). Second, if wartime demand caused all high-

[29] With a single cross-sectional instrument split by HighDebt, the 2SLS interaction is identified from functional form alone; the reduced form asks only whether rainfall predicts Democratic vote share *differentially* in high-debt counties.

livestock counties to swing Republican regardless of TGA exposure, non-TGA counties with high livestock shares should show the same 1940–42 reversal; no statistically clear reversal appears (Figure C.1; DiD coefficient -1.5, $p = 0.20$). Third, within TGA counties, state-by-year fixed effects absorb wartime shocks common to a state, so commodity demand could reproduce the triple-difference only by binding high-debt counties harder than low-debt ones within the same state. The two groups are balanced across ten pre-treatment comparisons, including livestock share, farm size, land values, and New Deal spending (Table C.5), which closes the most direct observable route.

Turnout and Compositional Change A separate concern is that the TGA affected *who voted* rather than *how they voted*. A turnout event study shows no significant TGA effect on total votes cast (DiD -1.1, $p = 0.87$; triple-diff interaction -5.7, $p = 0.52$; Figure C.2). The turnout null rules out mobilization but not compositional change through migration (cf. Fishback et al., 2006), which we cannot test directly with available data. The within-TGA balance on population (Table C.5) makes migration an unlikely primary driver.

Alternative Debt Definitions and Sample Windows The triple-difference survives alternative definitions of high debt: replacing the median split with a continuous debt-to-value interaction, or with tercile and quartile splits, leaves the anti-Democratic shift concentrated in the top debt group regardless of where the cutoff is drawn (Table B.1). Excluding WWII elections (1942–1946) leaves both the base TGA effect and the high-debt interaction substantively unchanged (Table B.2). The interaction also holds with presidential rather than congressional vote share as the outcome, addressing concerns about candidate quality, incumbency advantage, and redistricting (Table C.10). Restricting the sample to 1918–1960, which avoids both pre-suffrage noise and the Civil Rights realignment, strengthens the triple-difference to -7.843^{***} ($p < 0.001$); the 1960s realignment likely reshuffled coalitions along dimensions unrelated to the TGA, adding noise that attenuates the wealth channel in the full sample.

9 Results III: Gratitude, Self-Interest, or Ideology?

The previous sections pin down the *channel*, wealth operating through farm balance sheets, but not the *behavioral mechanism*. The conceptual framework (Section 3) identifies three competing accounts (gratitude, ideological conversion, self-interest), each predicting a distinct observable pattern (Table 1).

9.1 Gratitude

Table 5 tests whether the TGA's electoral effect varied with how the county's own representative voted on the Act. If gratitude drove the vote shift, Democratic gains should have been larger where the representative championed the TGA. The data show the opposite. Where the county's representative voted against the Act, treated counties moved *toward* Democrats by 9.7 percentage points (SE = 4.4); where he voted for it, they moved *away* by 4.3 points (SE = 1.5). The interaction between the two is −13.7 (SE = 3.4 clustering on counties, 4.4 on districts), the opposite sign from the one gratitude predicts.[30]

The positive arm of the split needs an account of its own, because a nine-point movement toward the enacting party could be read as the wealth channel running backwards where representatives opposed the Act. The composition of the Nay districts cannot support that reading. The 47 Nay-district counties comprise every treated county in Arizona, Nevada, and Wyoming, whose single-member delegations cast Nay votes, ten counties in northern Idaho, and four in California. Each arm of the split therefore averages within-state contrasts over a different set of states, and the two levels can differ for any reason the Act's effect differed across states. What the comparison rules out is narrower, and it is all the design asks of it: if credit for the Act drove the response, the counties of its champions should have moved toward Democrats relative to the counties of its opponents, and they moved 13.7 points the other way.

Classifying counties instead by whether a majority of their state's House delegation voted Yea, the state-delegation version in column (4), gives −8.8 (SE = 4.3). The two estimates differ only through Idaho, whose two members split on the bill and whose delegation the majority rule therefore classified as opposed. Exactly 25 of 213 treated counties change coding between the two measures, and all 25 are in Idaho. Outside Idaho the two measures are the same variable and return identical estimates. Dropping any one state leaves the own-representative interaction between −16.9 and −10.3, significant at the five percent level in all thirteen cases (Table C.12, Panel A).

A voter who feels grateful may credit her own representative, her state's delegation, or the national party. The roll-call interactions test the first two. National credit does not vary across counties, so no roll-call interaction can test it. Two patterns that do vary constrain it instead: a reward directed at the national party should not depend on a county's pre-treatment leverage, and grateful beneficiaries should not have opposed the AAA while endorsing price supports. Both patterns run the other way

[30] A concern is that Yea-voting members represent counties with larger grazing economies, so the interaction may recapture treatment intensity rather than political credit. We address this by controlling for each county's grazing area share interacted with year, absorbing differential trends driven by grazing-economy intensity (Table C.6).

(Table 4 and Table 6).

The Taylor Grazing Act was not a partisan credit-claiming issue in the West: the thirteen states in our sample voted 45 to 16 for it (36 to 6 outside California), and nine of the thirteen delegations were unanimous. A bill that western members of both parties supported gave voters little partisan signal to reward, whichever level of credit they had in mind. That near-unanimity also bounds what the roll-call test can show, since Idaho is the only state whose treated counties fall on both sides of the split.[31]

Turnover offers a further test of dyadic credit, since personal gratitude should track the individual member. We match the members who voted on H.R. 6462 to their subsequent service. Voting Yea predicted a 13.7 percentage point higher chance of returning in the 74th Congress (SE $=$ 5.5), but not among members representing grazing districts, where the interaction reverses and is indistinguishable from zero by the 76th Congress. Only 20 members represented grazing districts, so we read the turnover pattern as consistent with the county-level result rather than as independent evidence (Table C.12, Panel B).

9.2 Ideology or Self-Interest?

Survey evidence. We distinguish between ideology and self-interest using individual-level Gallup survey data from 1936–37, comparing respondents in TGA states to those in non-TGA states. This comparison exploits state-level variation with only nine treated states and cannot deliver the same causal precision as the county-level analysis; we interpret these results as suggestive evidence on the behavioral mechanism. The key test is whether TGA beneficiaries shifted against government *uniformly* (ideology) or *selectively* (self-interest).

Table 6 disaggregates Gallup responses by individual question and reports both raw p-values and Benjamini–Hochberg false discovery rate corrections across the 13 comparisons. The pattern supports narrow self-interest, though the statistical evidence thins after multiple-testing correction. On *farm-specific* questions, TGA-state respondents were 10.4 percentage points less likely to support reviving the AAA ($p_{\text{raw}} < 0.001$, $p_{\text{BH}} = 0.001$), the only farm-specific result that survives BH correction at the 1% level. They were 11.1 percentage points more likely to support higher farm prices ($p_{\text{raw}} = 0.02$, $p_{\text{BH}} = 0.10$). Opposition to the AAA is telling: TGA beneficiaries did not support farm policy generically, only the specific policies that raised *their* land values. They opposed the AAA's production controls, which constrained ranchers, while supporting the price supports that benefited them.

On *general government* questions, TGA-state respondents were 7.5 percentage points

[31]Table C.11 reports the distribution of treated counties across Yea and Nay districts, so the support of the interaction is visible alongside its coefficient.

less supportive of Roosevelt's spending policy ($p_{\mathrm{BH}} = 0.11$). They were also 9.6 percentage points *more* supportive of government takeover of business ($p_{\mathrm{BH}} = 0.07$), the opposite of what ideological conversion predicts.[32]

This pattern rules out pure ideological conversion, which predicts uniformly anti-government attitudes across all domains. After BH correction, only one of 13 comparisons survives at the 5% level, so the evidence is suggestive rather than definitive.

The 1936–37 polls also predate the reversal, which begins in 1942, so they establish a baseline rather than a change. Extending the comparison forward addresses that limit, and conditioning on farm respondents sharpens the contrast. Using the pooled AIPO records described in Section 4, we follow farm respondents in treated states against farm respondents in California, Texas and the Dakotas. Each group is benchmarked against its own non-farm neighbors, so any state-level shock common to both differences out. Three questions on government ownership recur with identical wording from 1936 to 1948, and the questions on federal farm support run through 1943.

Support for government ownership fell in every group over the period, the general retreat that followed the war (Figure C.8). Farm respondents in treated states sat below the comparison in the late 1930s, moved above it during the war, and by 1948 returned to a gap indistinguishable from where it stood in 1936 and 1937. That movement is already underway by 1940, two years before the electoral reversal, so it does not track the political break. Each of the three questions traces it separately, so it is not an artifact of which questions Gallup asked. Ideological conversion predicts a lasting shift against government among beneficiaries; across twelve years the series returns to its starting level (Figure C.9).

What the extension cannot do is establish the positive claim. The median comparison rests on roughly fifty farm respondents in the treated states, and permittee households remain 7.7 percent of farm households even after conditioning on farming, so a movement in beneficiary opinion would have to be implausibly large to register. The selective preference pattern that distinguishes self-interest from ideology therefore rests on the 1936–37 cross-section. We read the survey evidence as inconsistent with ideological conversion; it corroborates the self-interest account without identifying it.

The Anderson (2008) indices summarizing these questions into a farm-specific and a general-government index (Table 6, bottom panel) are both insignificant. This reflects a substantive feature of the data, not just a mechanical problem: the farm-specific index pools questions that pull in opposite directions (respondents favored higher farm prices but opposed reviving the AAA), which cancel when aggregated. The null

[32] Raw p-values are 0.03 and 0.01 respectively. The remaining differences, on profit regulation, public ownership, and government power, do not survive BH correction.

index result is itself informative: it indicates selective rather than blanket support for farm policy.

The representatives they elected. Survey responses are one way to observe ideology; whom a district sends to Congress is another, and one that does not depend on what party labels meant in a given decade. We extend the district crosswalk of Section 4 to every Congress from the 66th to the 87th and attach the Nokken–Poole first-dimension score (Nokken and Poole, 2004) of each district's member, a position re-estimated in each Congress against cutting lines held fixed at their common-space values.[33]

The grazing states were Republican country before the New Deal and returned to it afterwards. Democrats held 24 percent of their seats in 1930 against 51 percent nationally, 87 percent in 1934 against 74, and 25 percent again by 1946, when the national figure was still 43 (Figure C.7). The delegation these states sent in 1934 sat at -0.28 on the first dimension, to the left of the national Democratic median. These voters were not outside the coalition that built the New Deal. They entered it further than the country did, and they left it sooner. Most of the swing came from replacing members rather than from members changing their minds: holding the 1930 party composition fixed removes three-quarters of the movement in the delegation's position between 1930 and 1934.[34]

These are means rather than estimates, and deliberately so. A congressional district is too coarse an instrument for what the county results measure: the median exposed district drew 64 percent of its farm population from treated counties, so the 5.7-point swing of Section 7.2 implies a shift of under four points in a district whose median margin was eighteen points, and nine races in ten were not that close. What the series does establish is the premise the county estimates rest on. These were Republican districts that became, for one decade, among the most Democratic in the country, and then were Republican again.

[33]The career-constant DW-NOMINATE score cannot serve here: every member of these delegations who served four or more Congresses has exactly zero within-member variation in it, so any within-district test returns a null by construction. The Nokken–Poole score moves, with a median within-member standard deviation of 0.073 over the same window. Because the bill parameters are fixed at their common-space values, positions in different Congresses are read against the same cutting lines.

[34]The delegation's farm-weighted position moves from 0.260 in 1930 to -0.282 in 1934. Holding the 1930 Democratic seat share fixed and letting only within-party positions vary gives 0.024; holding within-party positions at their 1930 values and letting composition vary gives -0.140. The two counterfactual movements, -0.24 and -0.40, overlap and need not sum to the total of -0.54; the residual is the interaction of composition and position changes.

10 Conclusion

The Taylor Grazing Act made western ranchers wealthier, and wealthier ranchers turned against the party that enacted the policy. In 1934 a Democratic Congress ended open access on 142 million acres of rangeland; the renewable permits it granted incumbent users were a claim that raised the value of the private land they attached to. Counties covered by grazing districts rewarded Democrats for three elections, with a peak of 3.7 percentage points in 1940, then swung against them in 1942 and stayed negative through 1950. The reversal concentrated in the counties that carried the most debt against their land in 1930, where the same gain did most to improve net worth; counties with little debt kept rewarding Democrats.

Gratitude, ideological conversion, and self-interest each imply a different response to the Act. Counties whose own representative championed the Act saw the largest anti-Democratic swing, the opposite of what gratitude predicts. Beneficiaries show no general movement against government in the 1936–37 polls or across the 1940s, which is inconsistent with ideological conversion. Self-interest survives by elimination rather than by direct test; the surveys are too imprecise to establish the selective preference pattern that would identify it positively.

The reversal required two shocks. Formalizing grazing rights gave ranchers a pledgeable asset, but farm values had not diverged enough by 1940 to shift political preferences. Wartime demand activated the latent wealth gain. The coalition erosion was unintended, the product of a conservation policy and an exogenous demand shock that together crossed the Meltzer–Richard threshold.

The distinction between asset-based and consumption-based transfers is the key takeaway. Consumption transfers (food stamps, unemployment insurance, cash assistance) do not permanently shift beneficiaries' position in the wealth distribution. Asset-based transfers (homeownership subsidies, land reforms (Galiani and Schargrodsky, 2010), student loan forgiveness) do, and the TGA evidence suggests they carry a political cost that consumption transfers do not. Titling and privatization programs belong in the same class, with one qualification: the TGA granted secure access while the land stayed federal, so the wealth gain arrived without a change in legal status, and it is the gain that our estimates track.[35] The electoral return may reverse when market conditions capitalize the asset, a risk whose timing policy designers cannot control.

[35] The asset is one route to the same end rather than the only one. Hacker (2002) shows how publicly subsidized private benefits gave organized workers a stake in alternatives to public provision, which weakened the constituency for national health insurance. Any policy that durably moves its beneficiaries up the wealth or income distribution risks eroding the coalition that passed it, whether it works through an asset, as here, or through wages and benefits.

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Tables and Figures

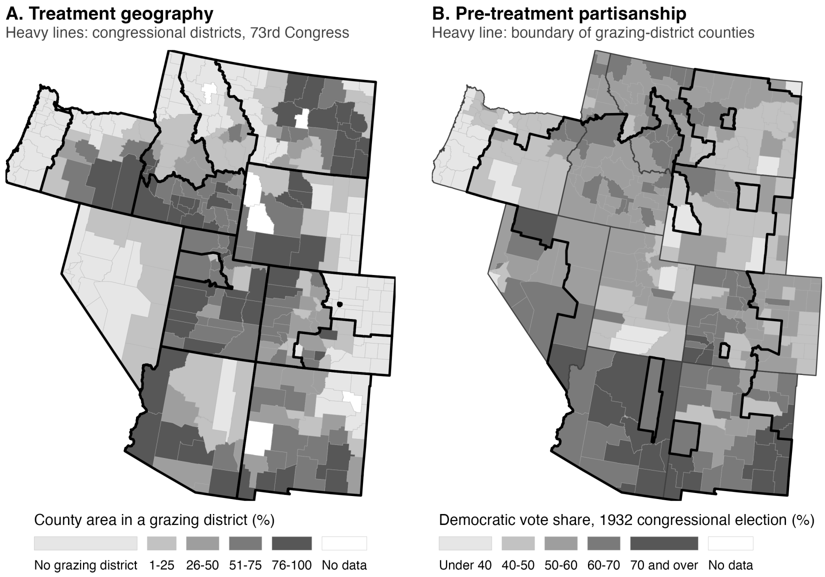

Figure 1: Grazing Districts, Congressional Districts, and 1932 Democratic Vote Share

Notes: The nine states in which grazing districts were established under the Act (AZ, CO, ID, MT, NV, NM, OR, UT, WY), 314 counties in all. Panel A shades each county by the share of its area lying inside a grazing district; the treatment indicator *Historical Grazing* equals one wherever that share is positive (199 counties). Heavy lines are congressional district boundaries for the 73rd Congress, the districts in place when the Act passed. Panel B shades each county by the Democratic share of the 1932 congressional vote, the last election before the Act, and outlines the counties overlapping a grazing district.

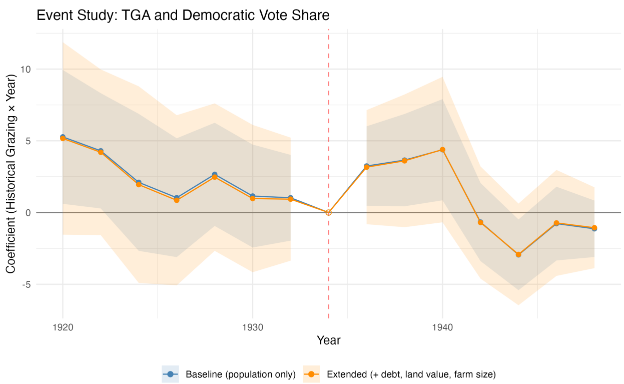

Figure 2: Event Study: TGA and Democratic Vote Share

Notes: Each point estimates the difference in Democratic vote share between TGA and non-TGA counties in a given election year, relative to 1934 (the last pre-treatment election). A joint test that all seven pre-treatment coefficients are zero does not reject ($p = 0.63$). The baseline specification includes county and state $\times$ year fixed effects; the extended specification adds 1930 agricultural covariates interacted with year indicators. Shaded bands show 95% confidence intervals from county-clustered standard errors, with no interval drawn at the omitted base year. Sample: 742 counties, 1920–1950.

Table 3: Difference-in-Differences: TGA and Democratic Vote Share

Dependent Variable:	Democratic vote share					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
HG × TGA	-1.531^{*}	-1.935	-1.862	-1.862	-3.757^{***}	
	(0.902)	(1.439)	(1.567)	(1.635)	(1.229)	
GrazingShare × TGA						-5.005^{***}
						(1.756)
Fixed-effects						
County	Yes	Yes	Yes	Yes	Yes	Yes
State × Year	Yes	Yes	Yes	Yes	Yes	Yes
Covariates						
New Deal Spending		Yes	Yes	Yes	Yes	Yes
Land Value (1930)			Yes	Yes	Yes	Yes
Farm Size (1930)			Yes	Yes	Yes	Yes
Debt-to-Value (1930)				Yes	Yes	Yes
Counties	740	740	739	739	306	739
Observations	20,800	20,800	20,768	20,768	9,551	20,768
R^2	0.860	0.885	0.885	0.885	0.806	0.885

Notes: The dependent variable is county-level Democratic vote share (%). HG × TGA equals one for counties overlapping a grazing district, in elections after 1934. Column (1) includes county and state × year fixed effects only; columns (2)–(4) progressively add the per-capita New Deal spending index, 1930 land value per acre and farm size, and the 1930 debt-to-value ratio, each interacted with year indicators. Column (5) restricts the sample to the nine TGA states. Column (6) replaces the binary indicator with the continuous grazing area share (fraction of county area within grazing districts, 0 to 1). All covariates are frozen at 1930 values. Standard errors clustered at the county level; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 4: Triple-Difference: TGA $\times$ Pre-Treatment Debt and Democratic Vote Share

Dependent Variable:	Dem vote share					
	Binary				*Continuous*	
	(1)	(2)	(3)	(4)	(5)	(6)
HG $\times$ TGA	0.710	0.551	-0.027	5.665^{**}		
	(1.096)	(2.233)	(1.170)	(2.498)		
HG $\times$ TGA $\times$ HD	-4.194^{***}	-4.146^{**}	-4.284^{***}	-5.717^{**}		
	(1.304)	(2.032)	(1.290)	(2.397)		
GS $\times$ TGA					-1.973	-1.965
					(1.701)	(1.930)
GS $\times$ TGA $\times$ HD					-7.536^{***}	-7.554^{***}
					(2.538)	(2.883)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates		Yes		Yes		Yes
Full sample	Yes	Yes			Yes	Yes
1918–1960			Yes	Yes		
Counties	740	739	740	739	740	739
Observations	20,800	20,768	13,582	13,561	20,800	20,768
R^2	0.860	0.885	0.896	0.914	0.885	0.885

Notes: The dependent variable is county-level Democratic vote share (%). HighDebt (HD) equals one for counties whose 1930 debt-to-value ratio exceeded the sample median of 36.5%. The coefficient on HG $\times$ TGA captures the TGA effect in low-debt counties; adding the interaction gives the effect in high-debt counties. Columns (1)–(2) use the full 1910–1972 sample; columns (3)–(4) restrict to 1918–1960; columns (5)–(6) replace the binary treatment indicator with the continuous grazing area share (GS). Covariates, where included, are 1930 land value, farm size, and the per-capita New Deal spending index, each interacted with year indicators. All specifications include county and state $\times$ year fixed effects. Standard errors clustered at the county level; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 5: Mechanism Discrimination: Roll-Call Heterogeneity

Dep. Variable:	Dem vote share			
	Own rep.	Rep. Nay	Rep. Yea	Delegation
Model:	(1)	(2)	(3)	(4)
HG × TGA	8.784**	9.710**	−4.327***	4.306
	(3.584)	(4.360)	(1.511)	(4.061)
HG × TGA × Yea	−13.674***			
	(3.420)			
HG × TGA × ProTGA				−8.806**
				(4.306)
County FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
Observations	20,768	3,917	16,851	20,768
R^2	0.885	0.806	0.904	0.885

Notes: Yea $=$ 1 if the representative of the county's 73rd-Congress district voted Yea on H.R. 6462 (the Taylor Grazing Act); counties are assigned to districts by largest intersected area share. Column (1) interacts TGA treatment with that vote, columns (2) and (3) split the sample by it, and column (4) reports the state-delegation version, where ProTGA $=$ 1 if more than 50% of the state's House delegation voted Yea. Columns (1) and (4) differ only through Idaho, whose delegation split evenly; 25 of 213 treated counties change coding between the two, all in Idaho (Table C.11). Clustering on districts rather than counties leaves the column (1) interaction unchanged with a standard error of 4.383. All specifications include county and state $\times$ year fixed effects plus 1930 agricultural covariates interacted with year indicators. Standard errors clustered at the county level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 6: Gallup Poll Responses: TGA vs Non-TGA States

Farm-specific						*General government*					
Question	TGA	Non-TGA	Diff	p (raw)	p (BH)	Question	TGA	Non-TGA	Diff	p (raw)	p (BH)
Govt loans for farmers	0.823	0.829	−0.006	0.769	0.769	Govt takeover business	0.602	0.506	+0.096	0.011**	0.073*
Revive AAA	0.333	0.437	−0.104	0.000***	0.001***	Regulate profits	0.765	0.707	+0.058	0.064*	0.166
Farm benefits	0.590	0.553	+0.037	0.361	0.544	Public electricity	0.714	0.664	+0.050	0.112	0.242
Fix farm prices	0.479	0.456	+0.023	0.511	0.604	Govt own railroads	0.371	0.342	+0.029	0.451	0.586
Higher farm prices	0.734	0.623	+0.111	0.022**	0.096*	Govt own banks	0.500	0.485	+0.015	0.595	0.645
						Roosevelt spending	0.543	0.618	−0.075	0.033**	0.107
						Congress power	0.542	0.596	−0.053	0.146	0.272
						President power	0.490	0.527	−0.037	0.377	0.544
Anderson (2008) summary indices											
Farm index			+0.001	0.794	—	Government index			+0.001	0.625	—
			(0.003)						(0.003)		

Notes: Individual-level Gallup survey responses (1936–37). Each question row reports the share of respondents supporting the named policy, separately for TGA and non-TGA states, with the difference, raw p-value, and Benjamini–Hochberg FDR-corrected p-value across all 13 comparisons; significance from two-sample t-tests. The bottom panel reports the Anderson (2008) inverse-covariance-weighted indices aggregating the five farm-specific and the eight general-government questions; each index row reports the coefficient on the TGA-state indicator from a regression with demographic fixed effects (sex, race, age group, occupation; $N = 63{,}050$), the state-clustered standard error in parentheses, and the regression p-value in the raw column. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Online Appendix

Not intended for publication

Wealth Transfers Erode the Coalition That Enacted Them:
Evidence from the Taylor Grazing Act

Mathias Bühler   Jérôme Schäfer

Contents

A Balance Tests

Table A.1 compares pre-treatment covariates across TGA and non-TGA counties, conditioning on state fixed effects and clustering standard errors at the county level. Two variables show statistically significant differences. First, TGA counties received substantially less AAA spending per capita (\$39.16 vs. \$77.77, $p = 0.002$), reflecting the AAA's design as a crop-support program that primarily targeted grain-producing counties rather than rangeland. Second, TGA counties had larger farms ($p = 0.056$), consistent with the extensive land use characteristic of ranching operations. Crucially, the variables most relevant to identification are balanced: debt-to-value ratios, land values, FERA and WPA spending, and the per-capita New Deal spending index all show no significant differences conditional on state. The AAA imbalance motivates our inclusion of the New Deal spending index as a control throughout, and the event-study pre-trends in Figure 2 confirm that these baseline differences do not translate into differential partisan trajectories before 1934.

Table A.1: Balance Tests: Pre-Treatment Covariates by TGA Status

	Mean				
	Non-TGA	TGA	Difference	SE	p-value
Land value per acre (\$)	43.04	24.63	-8.517	(6.998)	0.224
Farm size (acres)	814.60	1,195.51	1,279.992*	(668.795)	0.056
Debt-to-value ratio (%)	34.56	36.49	0.826	(0.849)	0.331
FERA spending per cap. (\$)	20.68	24.90	-1.496	(1.783)	0.402
AAA spending per cap. (\$)	77.77	39.16	-22.740***	(7.397)	0.002
WPA spending per cap. (\$)	35.90	47.39	-4.133	(4.940)	0.403
New Deal spending index (pop.-weighted)	0.09	0.12	-0.057	(0.037)	0.119

Notes: Each row reports a separate OLS regression of the named covariate on the TGA treatment indicator with state fixed effects; "Difference" is the coefficient on treatment, and the "Non-TGA" and "TGA" columns report unconditional group means. Covariates are from the 1930 Census of Agriculture and Fishback et al. (2003); the New Deal spending index is the Anderson (2008) inverse-covariance-weighted aggregate of per-capita FERA, WPA, AAA, CCC, PWA, and RFC spending. Standard errors clustered at the county level; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

B Robustness

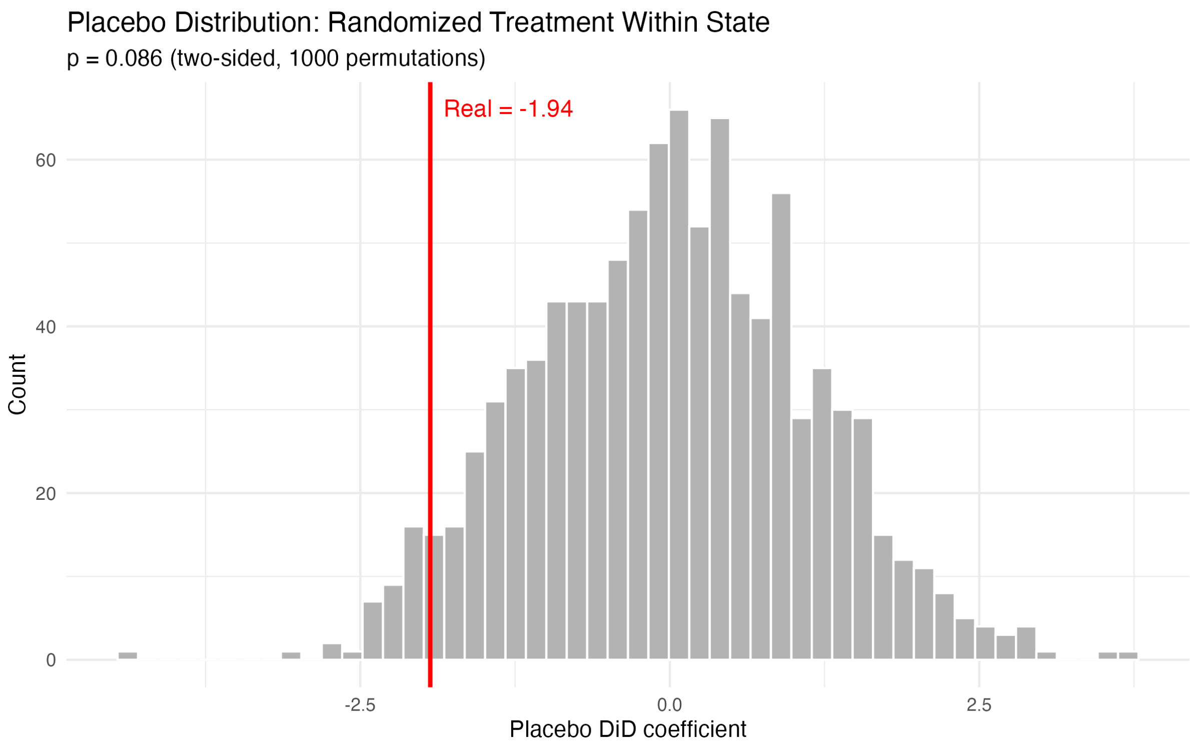

Figure B.1: Placebo Distribution: Randomized Treatment Within State

Notes: Distribution of 1,000 placebo DiD coefficients. Each iteration randomly reassigns treatment across counties within the same state, preserving the state-level share of treated counties, and re-estimates the specification of Table 3, column (1) plus the New Deal spending index interacted with year indicators. The vertical red line marks the real estimate; the permutation p-value is the share of placebo coefficients at least as extreme in absolute value.

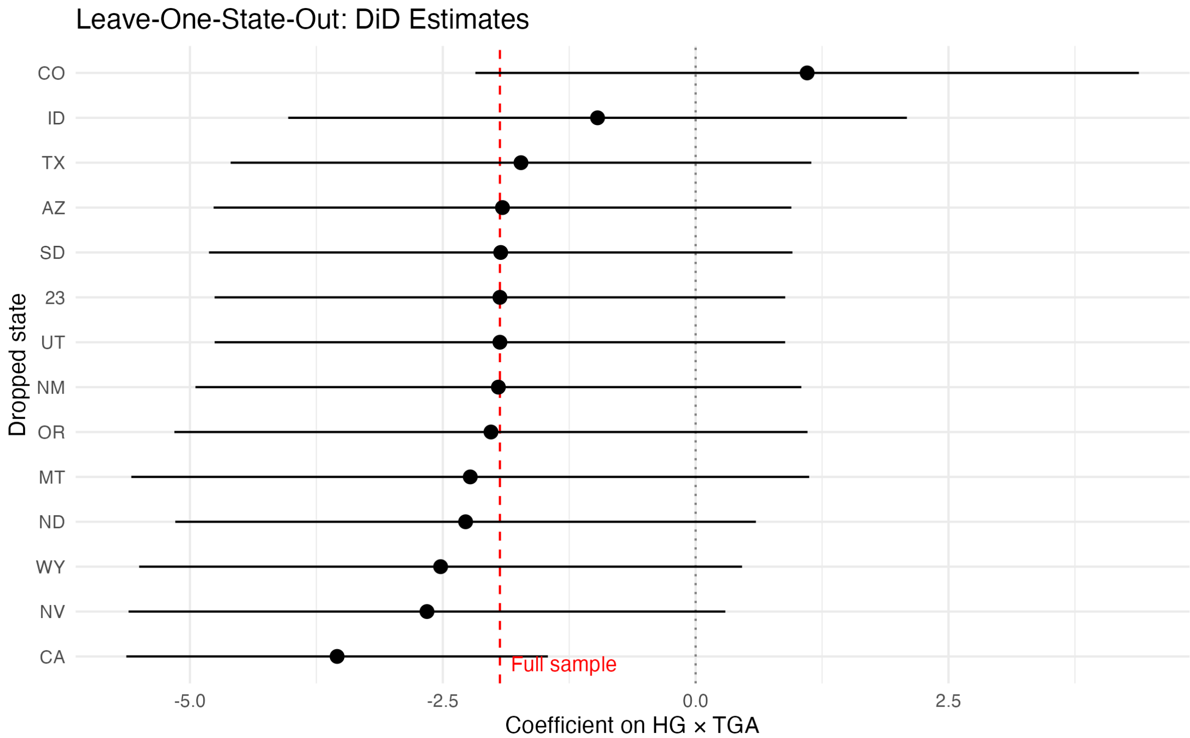

Figure B.2: Leave-One-State-Out: DiD Estimates

Notes: The coefficient on HG $\times$ TGA from the baseline DiD specification (Table 3, column 1, plus the New Deal spending index interacted with year indicators) after dropping the indicated state. Horizontal bars show 95% confidence intervals from county-clustered standard errors; the dashed red line marks the full-sample estimate.

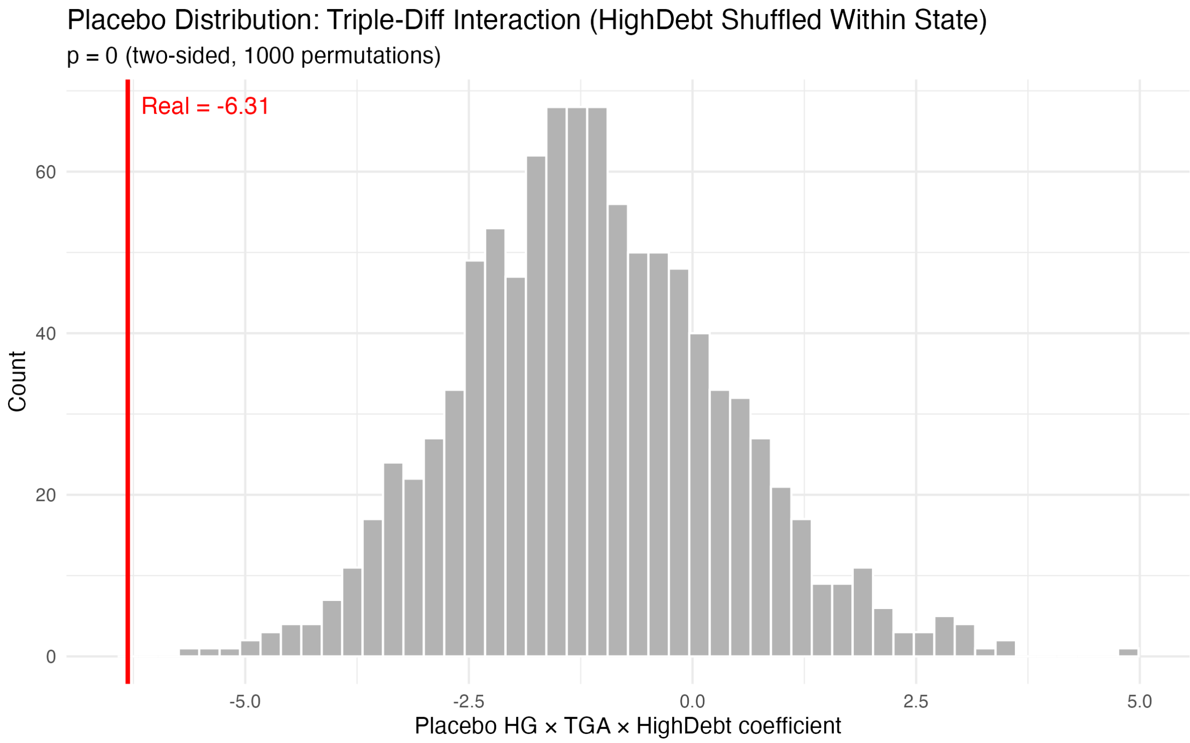

Figure B.3: Placebo Distribution: Triple-Difference Interaction

Notes: Distribution of 1,000 placebo triple-difference interaction coefficients. Each iteration randomly reassigns HighDebt status across counties within the same state, keeping treatment fixed, and re-estimates the triple-difference specification of Table 4. The vertical red line marks the real HG × TGA × HighDebt estimate; the permutation p-value is the share of placebo coefficients at least as extreme in absolute value.

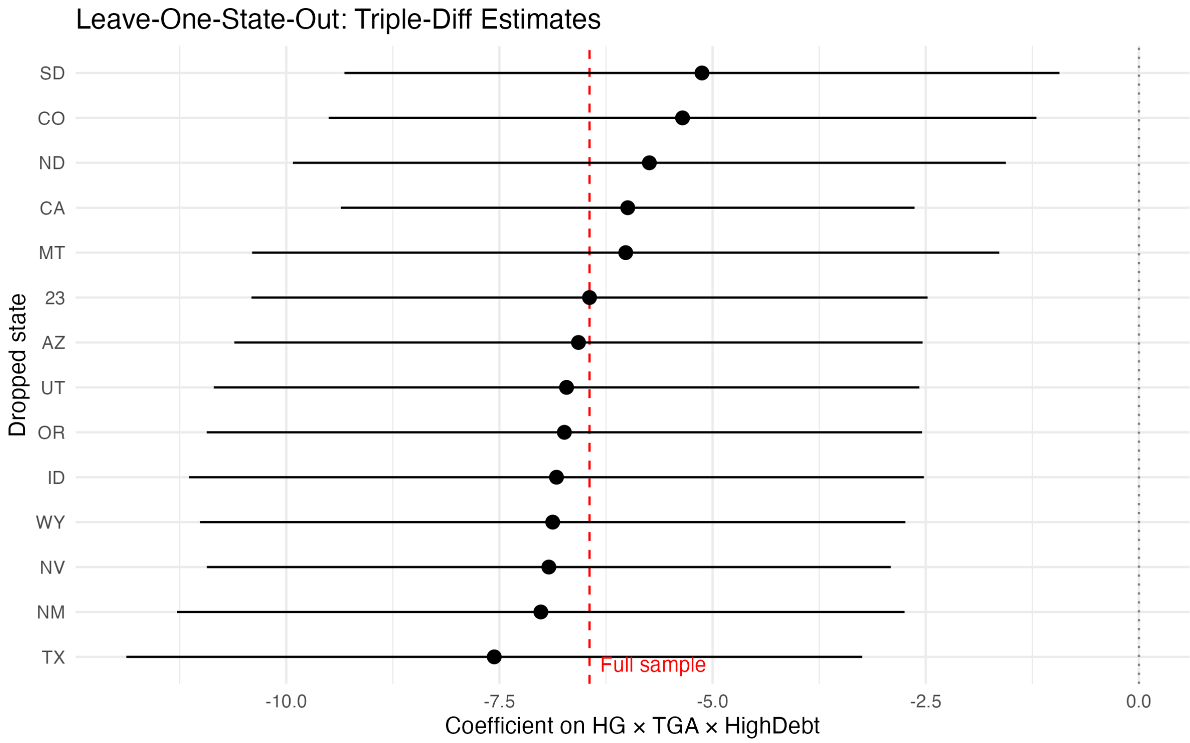

Figure B.4: Leave-One-State-Out: Triple-Difference Estimates

Notes: The triple-difference interaction HG $\times$ TGA $\times$ HighDebt after dropping the indicated state; the dashed red line marks the full-sample estimate (-6.4***). Dropping Colorado attenuates the estimate to -5.4 (95% CI $[-9.5, -1.2]$), still significant at the 5% level. Specifications include county and state $\times$ year FE plus 1930 agricultural covariates.

Table B.1: Triple-Difference Robustness: Alternative Debt Specifications

Dependent Variable:	Dem			
	Continuous	Continuous + Cov	Tercile	Quartile
Model:	(1)	(2)	(3)	(4)
Variables				
HG × TGA	10.22*	13.97**	1.024	0.5468
	(6.114)	(6.300)	(1.907)	(1.816)
HG × TGA × DtV30	-0.3333**	-0.4308***		
	(0.1503)	(0.1528)		
TGA × DtV30	0.0787	0.0761		
	(0.0743)	(0.0803)		
HG × TGA × HighDebt (tercile)			-7.046***	
			(1.914)	
TGA × HighDebt (tercile)			3.262***	
			(1.138)	
HG × TGA × HighDebt (quartile)				-7.522***
				(2.053)
TGA × HighDebt (quartile)				2.605**
				(1.230)
Fixed-effects				
County	Yes	Yes	Yes	Yes
State-Year	Yes	Yes	Yes	Yes
New Deal Spending	Yes	Yes	Yes	Yes
Land Value (1930)		Yes	Yes	Yes
Farm Size (1930)		Yes	Yes	Yes
Varying Slopes				
Year (New Deal Spending)	Yes	Yes	Yes	Yes
Year (Land Value (1930))		Yes	Yes	Yes
Year (Farm Size (1930))		Yes	Yes	Yes
Fit statistics				
Observations	20,800	20,768	20,768	20,768
R^2	0.88465	0.88475	0.88481	0.88480
Within R^2	0.00115	0.00157	0.00206	0.00196

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Variants of the triple-difference in Table 4. Column (1) replaces the binary HighDebt indicator with a continuous interaction in the 1930 debt-to-value ratio; column (2) adds 1930 agricultural covariates interacted with year indicators; columns (3)–(4) use tercile and quartile splits (top third or top quartile against the rest). All specifications include county and state-by-year fixed effects. Standard errors clustered at the county level; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table B.2: Excluding WWII Elections (1942–1946)

Dependent Variable:		Dem		
	DiD base	DiD cov	Triple base	Triple cov
Model:	(1)	(2)	(3)	(4)
Variables				
HG × TGA	-0.8109	-0.7510	2.255	2.379
	(1.514)	(1.744)	(1.905)	(2.096)
HG × TGA × HighDebt			-6.996***	-7.106***
			(1.834)	(2.017)
TGA × HighDebt			3.292***	3.285***
			(1.138)	(1.248)
Fixed-effects				
County	Yes	Yes	Yes	Yes
State-Year	Yes	Yes	Yes	Yes
New Deal Spending	Yes	Yes	Yes	Yes
Land Value (1930)		Yes		Yes
Farm Size (1930)		Yes		Yes
Debt-to-Value (1930)		Yes		
Varying Slopes				
Year (New Deal Spending)	Yes	Yes	Yes	Yes
Year (Land Value (1930))		Yes		Yes
Year (Farm Size (1930))		Yes		Yes
Year (Debt-to-Value (1930))		Yes		
Fit statistics				
Observations	19,106	19,077	19,106	19,077
R^2	0.88600	0.88605	0.88619	0.88625
Within R^2	4.7×10^{-5}	4.02×10^{-5}	0.00173	0.00176

Clustered (County) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The baseline DiD (columns 1–2) and triple-difference (columns 3–4) of Tables 3 and 4, excluding the wartime elections of 1942–1946, without and with 1930 agricultural covariates interacted with year indicators. All specifications include county and state-by-year fixed effects and the New Deal spending index interacted with year indicators. Standard errors clustered at the county level; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

C Additional Robustness Tests

Table C.1: Instrumental Variables: Rainfall and Democratic Vote Share

Dependent Variable:	HG × TGA		Democratic vote share			
	First Stage		Reduced Form		IV	
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Rainfall × TGA	-0.186^{***}	-0.186^{***}	4.186^{***}	1.619^{*}		
	(0.028)	(0.032)	(0.582)	(0.900)		
HG × TGA					-22.53^{***}	-8.68^{*}
					(4.39)	(5.04)
Fixed effects						
County	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Covariates						
Population		Yes		Yes		Yes
Land Value (1930)		Yes		Yes		Yes
Farm Size (1930)		Yes		Yes		Yes
Debt-to-Value (1930)		Yes		Yes		Yes
Counties	307	306	307	306	307	306
Observations	9,583	9,551	9,583	9,551	9,583	9,551
First-stage F	44.8	34.0			44.8	34.0

Clustered (county) standard errors. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: The instrument is standardized October 1934 rainfall interacted with a post-1934 indicator: drier counties during the federal land survey appeared more degraded and received more TGA regulation (Bühler, 2023). Columns (1)–(2): first stage; (3)–(4): reduced form; (5)–(6): 2SLS. The TGA states lie west of the Dust Bowl region; the instrument captures rangeland conditions, not Dust Bowl severity. County and year FE (not state × year, which would absorb the cross-sectional variation). Sample: nine TGA states. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.2: Instrumental Variables: Triple-Difference

	First Stage		Reduced Form		IV
Dep. Var.:	HG × TGA	HG × TGA × HighDebt	Dem vote share		Dem vote share
	(1)	(2)	(3)	(4)	(5)
Variables					
Rainfall × TGA	-0.240^{***}	0.000	1.688^{*}	-0.069	
	(0.040)	(0.000)	(0.894)	(1.253)	
Rainfall × TGA × HighDebt	0.107^{*}	-0.133^{***}		3.554^{**}	
	(0.063)	(0.048)		(1.790)	
HG × TGA					0.286
					(5.215)
HG × TGA × HighDebt					-26.49^{*}
					(13.81)
Fixed effects					
County	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes
Counties	306	306	306	306	306
Observations	9,551	9,551	9,551	9,551	9,551
First-stage F	562.5	241.5			562.5 / 241.5

Clustered (county) standard errors. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: The two endogenous variables (HG × TGA and HG × TGA × HighDebt) are instrumented with standardized October 1934 rainfall and its interaction with HighDebt. Columns (1)–(2): first stage for each endogenous variable; (3)–(4): reduced forms with the rainfall instrument only and with both instruments; (5): 2SLS. Covariates are 1930 population, land value, farm size, and debt-to-value, interacted with year indicators. County and year FE (not state × year, which would absorb the cross-sectional identifying variation). Sample: nine TGA states. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.3: WWII Robustness: Controlling for Differential Commodity Exposure

Dependent Variable:	Democratic vote share							
	DiD	DiD+LP	DiD+AS	DiD+AS+Crop	TD	TD+LP	TD+AS	TD+AS+Crop
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
HG × TGA	-1.862	-1.727	-1.862	-1.862	1.007	1.084	1.007	1.007
	(1.635)	(1.617)	(1.723)	(1.828)	(2.034)	(2.013)	(2.144)	(2.274)
Livestock Exposure		0.765***				0.759***		
		(0.160)				(0.160)		
HG × TGA × HighDebt					-6.443***	-6.321***	-6.443***	-6.443***
					(2.023)	(2.007)	(2.132)	(2.262)
Fixed effects								
County	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
New Deal Spending	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Land Value (1930)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Farm Size (1930)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Debt-to-Value (1930)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Livestock Share (1930)			Yes	Yes			Yes	Yes
Crop Value/Farm (1930)				Yes				Yes
Fit								
Observations	20,768	20,768	20,768	20,768	20,768	20,768	20,768	20,768
R^2	0.884	0.885	0.884	0.884	0.884	0.885	0.884	0.884

Notes: Columns (1)–(4): DiD; columns (5)–(8): triple-difference. "LP" adds 1930 livestock share × national cattle price as a time-varying control; "AS" adds 1930 livestock share interacted with year indicators; "AS+Crop" adds 1930 crop value per farm as well. The interaction is numerically identical across columns (5), (7), and (8) because the varying slopes are orthogonal to it after conditioning on county and state-by-year fixed effects; standard errors widen from 2.02 to 2.26. All specifications include county and state × year fixed effects plus baseline covariates. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

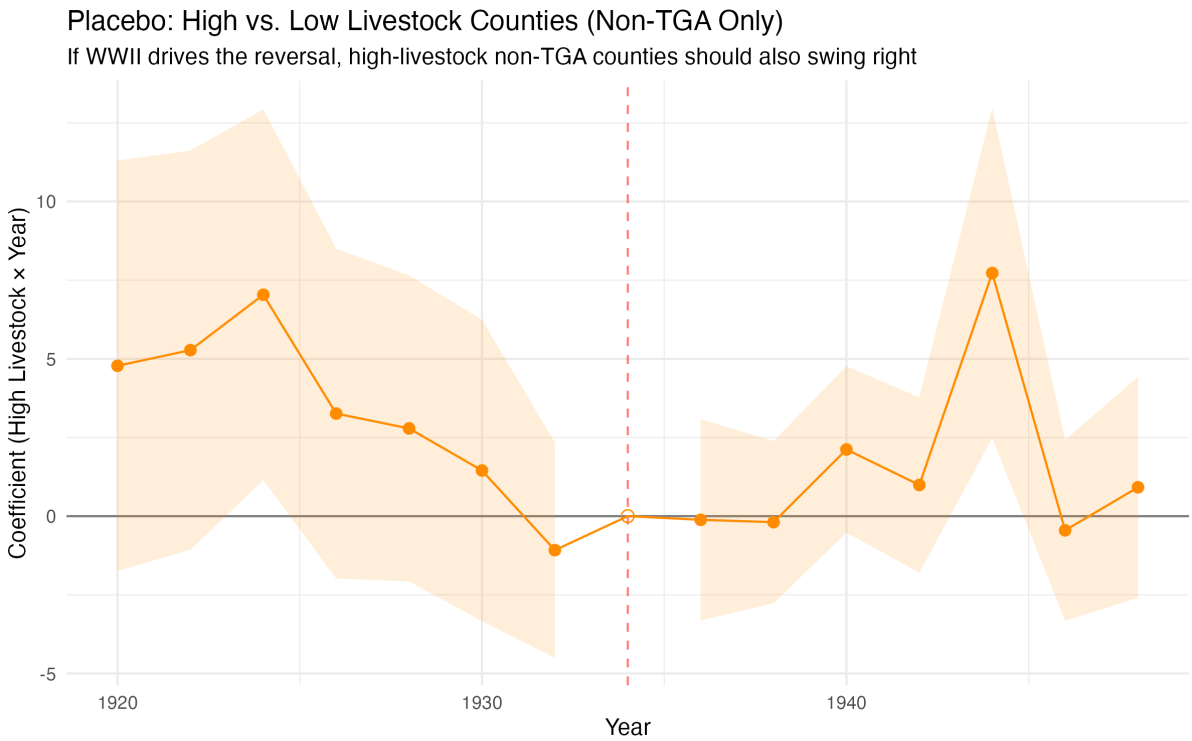

Figure C.1: Placebo: High vs. Low Livestock Counties (Non-TGA Only)

Notes: Event study comparing high-livestock to low-livestock counties (above- vs. below-median 1930 livestock share) *among non-TGA counties only*, with 1934 as the base year. County and state $\times$ year FE, New Deal spending control. $N = 527$ non-TGA counties. Shaded bands show 95% confidence intervals from county-clustered standard errors, with no interval drawn at the omitted base year (open marker).

Table C.4: Placebo Triple-Difference: Pre-Treatment Window (1920–1932)

Dependent Variable:	Democratic vote share	
	DtV 1920	DtV 1925
Model:	(1)	(2)
Variables		
HG × Post(1926)	-0.060	-2.165*
	(1.094)	(1.126)
HG × Post(1926) × HighDebt	-2.023	0.948
	(1.246)	(1.372)
Post(1926) × HighDebt	0.993	0.238
	(0.899)	(0.991)
Fixed effects		
County	Yes	Yes
State × Year	Yes	Yes
Fit		
# County	736	739
Observations	4,622	4,638
R^2	0.929	0.929

Notes: Placebo triple-difference estimated on the pre-treatment window (1920–1932) with a placebo treatment date of 1926, using 1920 (column 1) and 1925 (column 2) debt-to-value ratios. County and state × year FE. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.5: Balance Tests: High-Debt vs. Low-Debt Counties Within TGA Areas

Variable	High-debt	Low-debt	Difference	*p*-value
Land value/acre (1930)	21.00	27.20	−6.20	0.241
Farm size (1930)	797	1,476	−678	0.103
Animal share (1930)	0.555	0.569	−0.014	0.673
Grazing area share	0.477	0.479	−0.002	0.967
Total population	10,516	14,687	−4,171	0.104
New Deal spending index	0.103	0.130	−0.027	0.486
Pre-TGA Dem vote (1930–32)	51.82	52.05	−0.23	0.911
FERA per capita	24.48	25.20	−0.72	0.696
AAA per capita	43.25	36.28	+6.97	0.358
WPA per capita	46.64	47.91	−1.27	0.838

Notes: High-debt against low-debt counties *within TGA areas only* (213 TGA counties: 88 high-debt, 125 low-debt); HighDebt is a 1930 debt-to-value ratio above the sample median. *p*-values from two-sample *t*-tests.

Table C.6: Roll-Call Heterogeneity: Controlling for County Grazing Intensity

Dependent Variable:	Democratic vote share	
	Original	+Grazing Share
Model:	(1)	(2)
Variables		
HG $\times$ TGA	4.306	4.306
	(4.061)	(4.122)
HG $\times$ TGA $\times$ ProTGA	-8.806**	-8.806**
	(4.306)	(4.370)
Fixed effects		
County	Yes	Yes
State $\times$ Year	Yes	Yes
New Deal Spending	Yes	Yes
Land Value (1930)	Yes	Yes
Farm Size (1930)	Yes	Yes
Debt-to-Value (1930)	Yes	Yes
Grazing Area Share		Yes
Fit		
Observations	20,768	20,768
R^2	0.884	0.884

Notes: The delegation specification of Table 5, column (4), adding the county's grazing area share interacted with year indicators as a varying-slope control, so that ProTGA status cannot proxy for the state's economic dependence on grazing. All specifications include county and state $\times$ year FE plus 1930 agricultural covariates. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

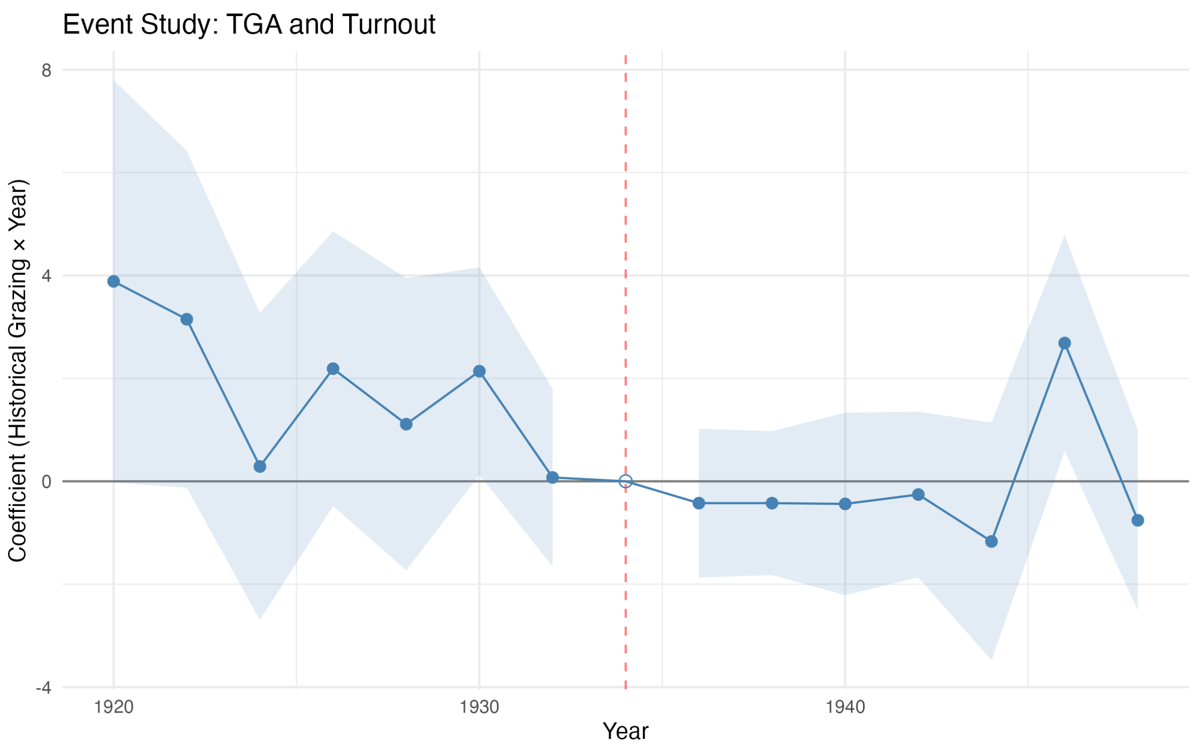

Figure C.2: Event Study: TGA and Turnout

Notes: Dynamic treatment effects of the TGA on county-level turnout (%), 1920–1950. Observations with turnout exceeding 100% excluded as coding errors. County and state × year FE, New Deal spending control. Shaded bands show 95% confidence intervals from county-clustered standard errors, with no interval drawn at the omitted base year (open marker).

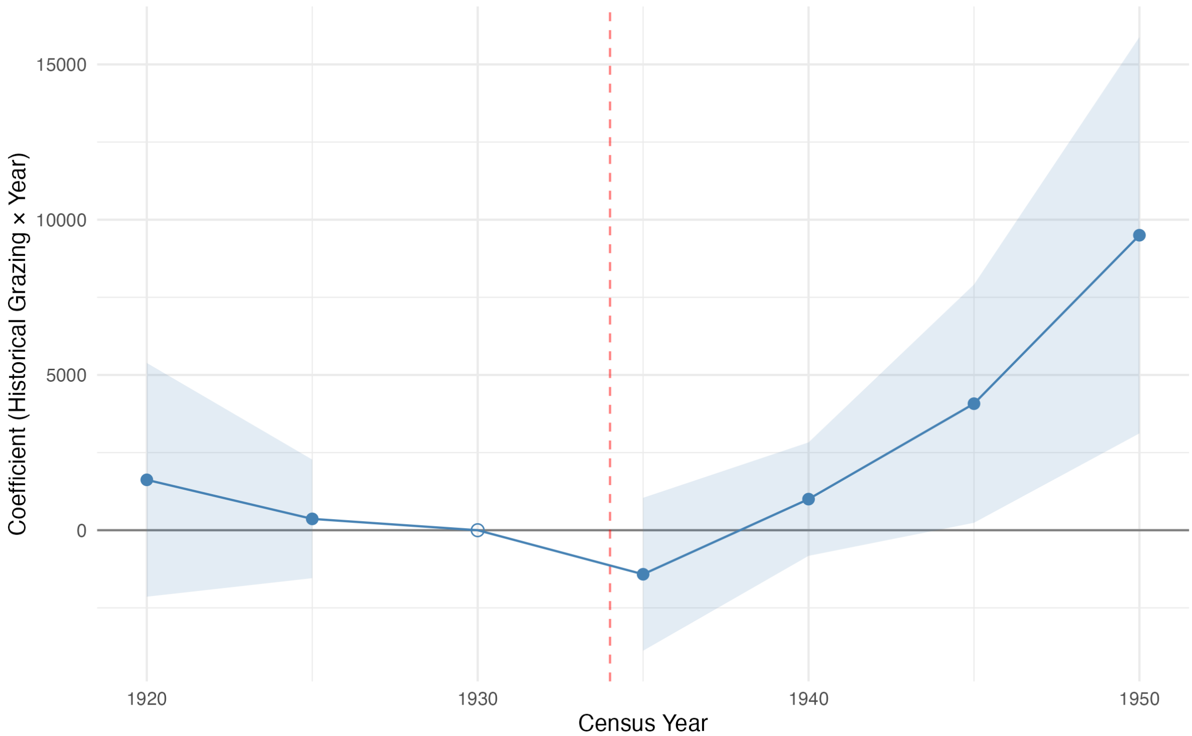

Figure C.3: Event Study: TGA and Average Farm Value

Notes: Each point estimates the difference in average total farm value per farm (dollars, Census of Agriculture) between TGA and non-TGA counties relative to 1930. Pre-treatment coefficients (1920, 1925) are small and insignificant, and a joint test that both are zero does not reject ($F = 0.36$, $p = 0.70$). County and state $\times$ year fixed effects. Shaded band shows the 95% confidence interval from county-clustered standard errors, with no interval drawn at the omitted base year (open marker). Census years 1920–1950; 739 counties.

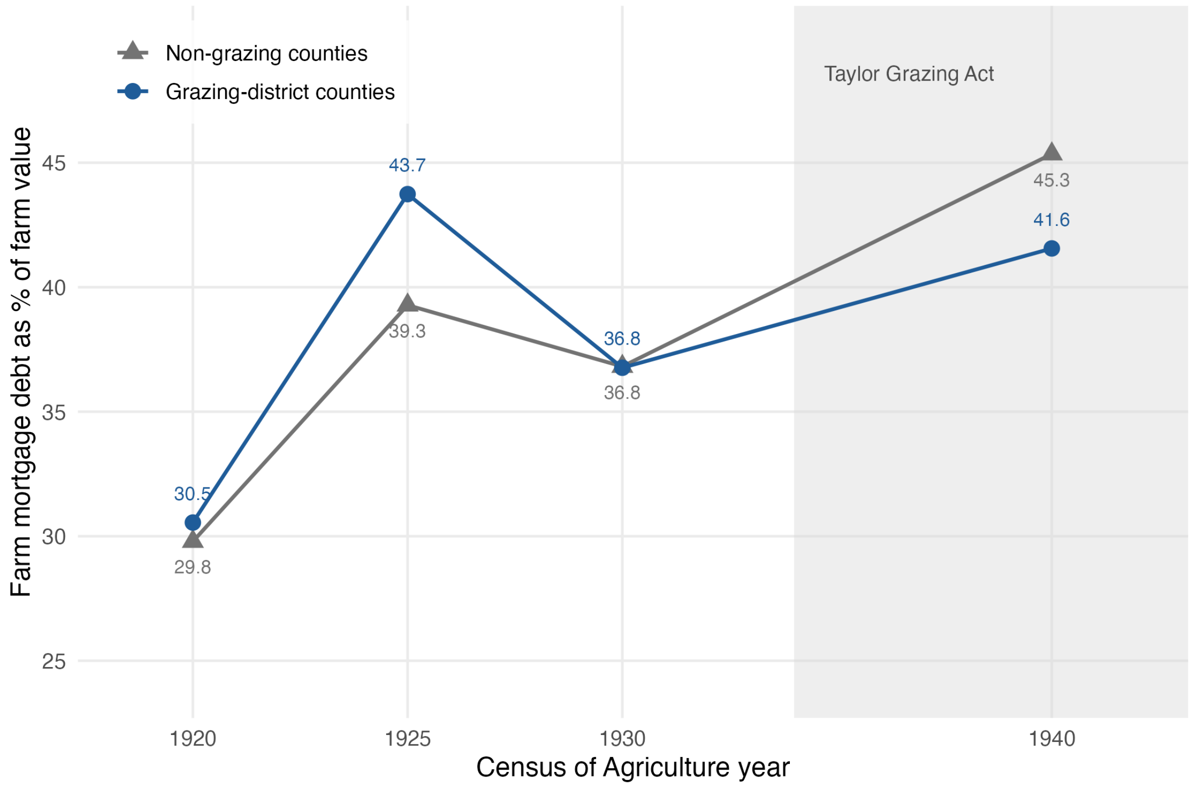

Figure C.4: Farm Leverage in Grazing-District and Non-Grazing Counties

Notes: Unweighted county means of the debt-to-value ratio from the Census of Agriculture, total farm mortgage debt as a percentage of total farm property value, so the vertical axis is in percentage points against a sample mean of 35.1. The sample is the nine states where grazing districts formed (the sample of Table 3, column 5). Census waves reporting county-level farm mortgage debt are 1920, 1925, 1930 and 1940; the lines are not interpolated through the intervening years. Adding the four states that contribute comparison counties only (CA, ND, SD, TX) reclassifies no county and leaves the 1930–1940 difference unchanged, at 5.4 points against 8.6. Figure C.5 reports the corresponding coefficient event study and its pre-trend tests.

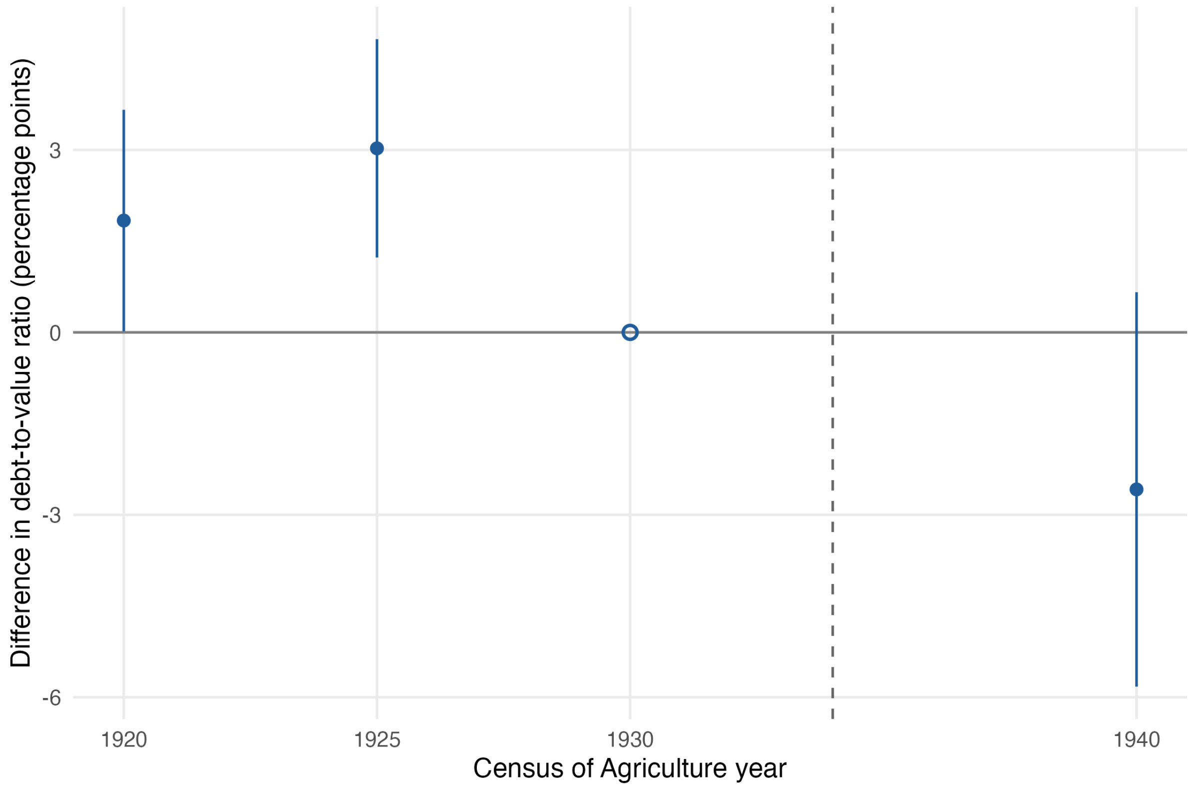

Figure C.5: Event Study: TGA and County Debt-to-Value Ratio

Notes: Coefficient version of Figure C.4, reported here because the series cannot bear a causal reading and Section 7.1 uses it descriptively. Each point estimates the difference in debt-to-value ratios (percentage points; sample mean 35.1) between TGA and non-TGA counties relative to 1930. Across all thirteen states the estimates are $+1.84$ (SE $= 0.93$) in 1920, $+3.03$ (SE $= 0.92$) in 1925 and -2.58 (SE $= 1.65$) in 1940; on the nine states in which grazing districts formed, the sample of Figure C.4, they are $+0.68$ (SE $= 1.04$), $+2.70$ (SE $= 1.04$) and -4.40 (SE $= 1.90$). A joint test of the pre-treatment coefficients rejects on both samples ($p = 0.003$ and $p = 0.032$), in each case through the 1925 coefficient alone. A differential linear trend over 1920–1930 gives -0.197 points per year on thirteen states ($p = 0.033$) but -0.069 on nine ($p = 0.51$); against its extrapolation, the 1940 estimate lies 1.44 points below on thirteen states ($p = 0.49$) and 4.49 below on nine ($p = 0.05$). With three pre-treatment waves and one after the Act, the design cannot distinguish a level shift from a continuing trend. County and state $\times$ year fixed effects; 95% intervals from county-clustered standard errors, none at the base year (open marker). $N = 2{,}957$ county-years, 740 counties.

Table C.7: The Political Response Across the Leverage Distribution

Panel A: TGA effect within each pre-treatment leverage quartile				
Quartile	Debt-to-value range	Counties	Treated	TGA effect
Q1	0.0–30.8	195	40	6.68
				(4.11)
Q2	30.9–35.4	184	54	−3.35
				(2.42)
Q3	35.4–39.6	185	57	−2.80
				(2.62)
Q4	39.6–84.2	176	62	−6.39***
				(2.27)

Panel B: triple-difference interaction as the HighDebt cutoff rises		
Cutoff	Debt-to-value	Interaction
Median	35.4	−4.15**
		(1.95)
Top third	38.2	−7.05***
		(1.91)
Top quartile	39.6	−7.52***
		(2.05)
Top decile	43.2	−6.54**
		(2.61)

Outcome is Democratic vote share. County and state-by-year fixed effects, population trend, and 1930 land value and farm size interacted with year; standard errors clustered by county in parentheses. Panel A estimates the TGA effect separately within each quartile of the 1930 debt-to-value distribution, so each row is a difference-in-differences on its own subsample. Panel B raises the threshold defining HighDebt and reports the triple-difference interaction at each cutoff. A response that scaled with the size of the net-worth gain would strengthen steadily as the cutoff rises; the interaction instead jumps and then plateaus. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.8: Incidence of the TGA Wealth Gain and Implied Vote Switching

	If each farm household holds		
How widely the gain is assumed to reach	1 voter	2 voters	3 voters
Panel A: the group's share of the treated-county electorate (%)			
Permit-holding farms	2.3	4.6	6.9
All owner-operators	11.3	22.6	33.9
All farm households	19.7	39.3	59.0
Panel B: share of the group that must change parties to deliver +3.7 pp (%)			
Permit-holding farms	161.6	80.8	53.9
All owner-operators	32.7	16.4	10.9
All farm households	18.8	9.4	6.3

Median treated county. Rows widen the assumed set of beneficiaries, from permit holders alone to every farm household in the county; columns vary the assumed number of voting adults per farm household. Permit-holding farms are farms reporting grazing permits in the 1950 Census of Agriculture; owner-operator and farm counts are from the 1930 Census. The electorate denominator is congressional votes cast in 1940, extracted from ICPSR 08611. Panel B divides the 1940 peak effect of 3.7 percentage points by each group's share of the electorate, so a value above 100 means the group is too small to generate the estimate even if every member changed parties.

Table C.9: Farm-Value Gains by Pre-Treatment Debt Group

Year	High debt	Low debt	Difference (p)
1940	0.115	0.160	-0.022 (0.57)
1945	0.145	0.177	0.030 (0.60)
1950	0.185	0.184	0.072 (0.29)

Log farm value per farm. Event-study coefficients on TGA treatment, 1930 omitted, county and state-by-year fixed effects, standard errors clustered by county. The High and Low debt columns are estimated on the corresponding subsample, each retaining its own treated and control counties. The difference column reports the Year $\times$ TGA $\times$ HighDebt coefficient from a single fully interacted model, which is the correct test: differencing the two subsample estimates by hand is not. Each subsample carries its own fixed effects and covariate coefficients, so the interacted difference can exceed, and even differ in sign from, the gap between the two subsample columns. Gains are reported in logs because the level estimates are sensitive to the right tail of the farm-value distribution, while the proportional gains are what the leverage argument turns on.

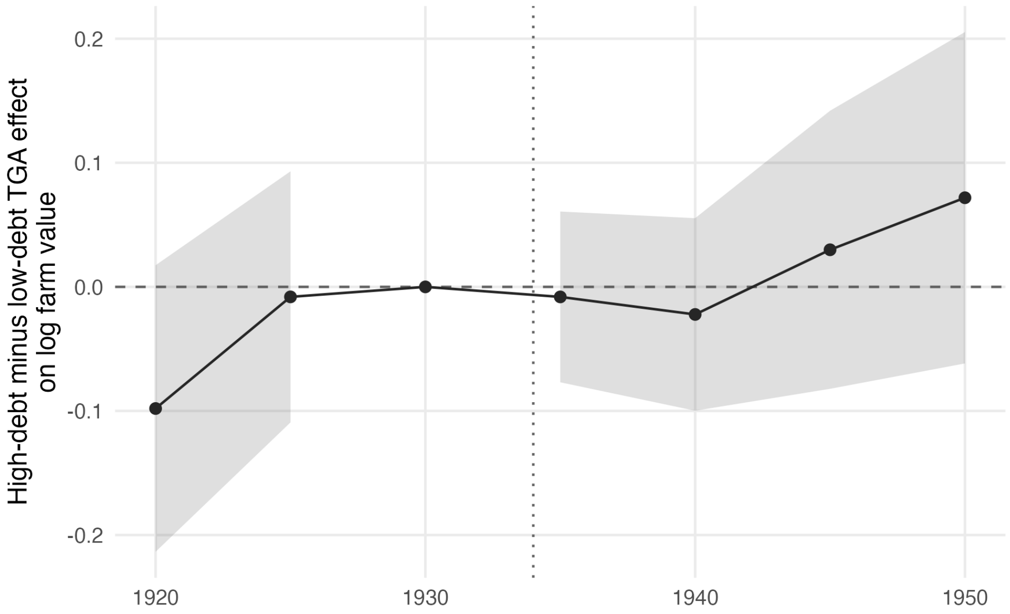

Figure C.6: Difference in the TGA Farm-Value Effect Between High- and Low-Debt Counties

Notes: Year × TGA × HighDebt coefficients on log farm value per farm, from a single fully interacted event study with 1930 omitted; estimating the event study separately on each debt group is not a test of the difference, since each subsample carries its own fixed effects and control counties. Gains are in logs because level estimates are sensitive to the right tail and the leverage argument turns on the proportional gain. County and state × year fixed effects, standard errors clustered by county; no interval at the base year. Census years 1920–1950.

Table C.10: Triple-Difference: Presidential Vote Share

Dependent Variable:	Dem presidential vote share (1)	(2)
HG × Post TGA	−1.776*	3.226***
	(0.913)	(1.228)
HG × Post TGA × HighDebt	−1.592	−2.922*
	(1.149)	(1.737)
County FE	Yes	Yes
State × Year FE	Yes	Yes
Covariates		Yes
Counties	740	740
Observations	11,622	11,606
R^2	0.876	0.915

Notes: Triple-difference estimates with county-level Democratic presidential vote share (%) as the outcome. Post TGA equals one for elections after 1936; the 1936 election is classified as pre-treatment because most grazing districts were not yet established by November 1936. Covariates (column 2) are 1930 land value, farm size, and debt-to-value ratio, each interacted with year, plus the New Deal spending index. Sample: 741 counties, presidential elections 1912–1972; uncontested elections excluded. Standard errors clustered at the county level; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

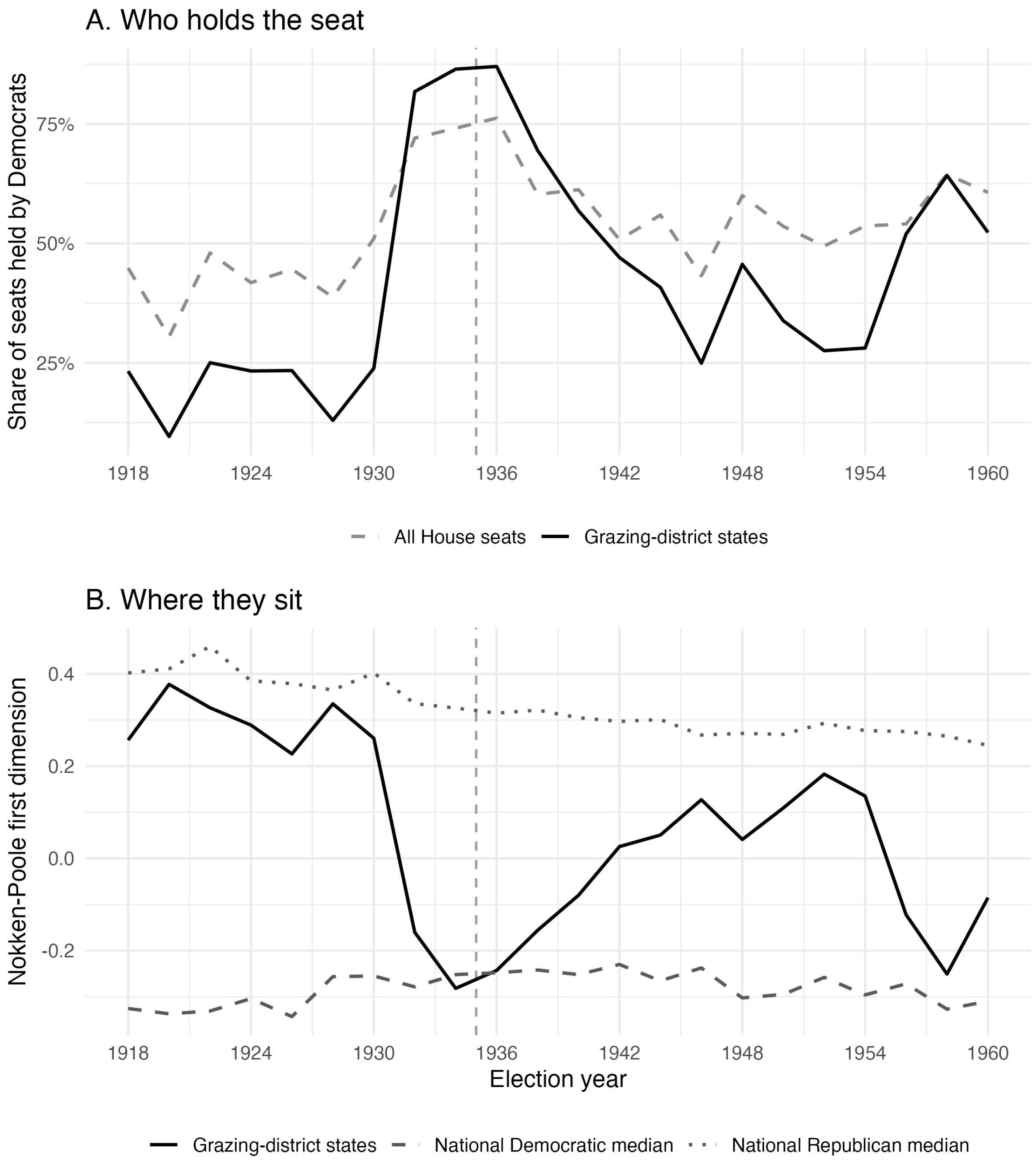

Figure C.7: The Grazing States Entered the New Deal Coalition and Left It

Notes: The nine states containing grazing districts, farm-weighted across the 17 fixed district units they contain, against national benchmarks. Panel A plots the share of seats held by Democrats in those states and in the House as a whole. Panel B plots the delegation's mean Nokken–Poole first-dimension position against the median House Democrat and Republican (higher is more conservative); because the procedure fixes bill parameters at their common-space values, levels are comparable across Congresses. The vertical line marks the Act. A differential event study on this panel, estimated on the interior West excluding Texas and California, finds no break at the Act in member position (0.058, SE = 0.105) or in the party of the seat (0.049, SE = 0.152); the design detects only movements of about 0.60 of an outcome standard deviation, far above what the county-level estimates imply, so we read these as uninformative rather than as evidence of no effect. Sources: Lewis et al. (2013), Lewis et al. (2026).

Table C.11: Delegation Votes on H.R. 6462 and the Support of the Roll-Call Interaction

	Delegation		Treated counties			Within-state
State	Yea	Nay	Total	Yea distr.	Nay distr.	split
AZ	0	1	13	0	13	No
CA	9	10	4	0	4	No
CO	4	0	34	34	0	No
ID	1	1	35	25	10	Yes
MT	2	0	34	34	0	No
ND	2	0	2	2	0	No
NM	1	0	23	23	0	No
NV	0	1	5	0	5	No
OR	2	1	11	11	0	No
SD	2	0	2	2	0	No
TX	20	1	6	6	0	No
UT	2	0	29	29	0	No
WY	0	1	15	0	15	No
Total	45	16	213	166	47	

Notes: House votes on H.R. 6462, 11 April 1934. Treated counties (counties with historic grazing land) are assigned to 73rd-Congress districts by largest intersected area share; 4 of 748 counties are divided by a district line. One California member did not vote, and three Texas members elected at large have no district. Nine of the thirteen delegations voted unanimously; Idaho is the only state whose treated counties fall on both sides of the Yea/Nay split.

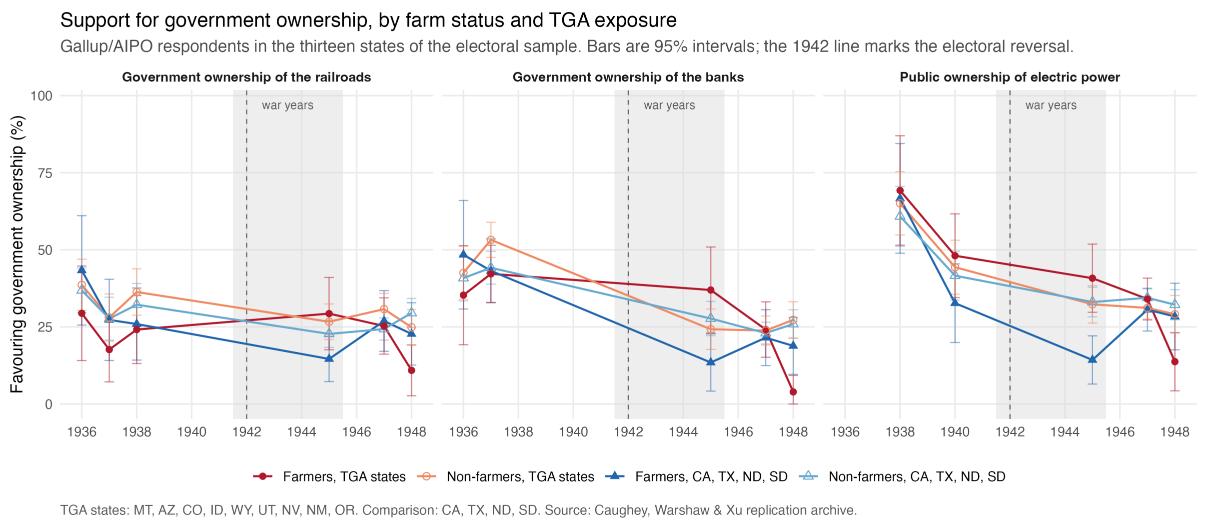

Figure C.8: Support for Government Ownership, by Farm Status and TGA Exposure
Notes: Share of AIPO respondents favoring government ownership, on the three questions whose wording is identical throughout the period (railroads, banks, electric power), restricted to the thirteen states of the electoral sample; treated states are the nine with grazing districts. Bars are 95% intervals. Source: the pooled AIPO records of Caughey et al. (2019).

Table C.12: Own-Representative Interaction: Leave-One-Out and Member Turnover

Panel A: Own-representative interaction, leave-one-state-out				
State dropped	Coefficient	SE	p	N
ID	−16.94	(5.37)	0.002	19,419
MT	−14.57	(3.44)	0.000	19,129
WY	−14.26	(4.17)	0.001	20,103
AZ	−14.21	(3.50)	0.000	20,334
OR	−14.09	(3.44)	0.000	19,626
ND	−14.04	(3.43)	0.000	19,085
NM	−13.89	(3.42)	0.000	19,854
SD	−13.74	(3.43)	0.000	18,641
UT	−13.67	(3.42)	0.000	19,876
TX	−13.55	(3.37)	0.000	15,214
NV	−13.29	(4.07)	0.001	20,232
CO	−11.84	(3.33)	0.000	18,788
CA	−10.29	(1.91)	0.000	18,915
Panel B: Share of members returning to the House				
Group	N	74th	75th	76th
Other district, voted Nay	92	0.674	0.533	0.467
Other district, voted Yea	259	0.811	0.637	0.429
Grazing district, voted Nay	7	0.857	0.429	0.429
Grazing district, voted Yea	13	0.769	0.538	0.538

Notes: Panel A re-estimates the own-representative interaction (Table 5, column 1) dropping one state at a time; the specification is otherwise unchanged. Panel B reports the share of members voting on H.R. 6462 who returned in each subsequent Congress (Voteview roster); only 20 members represented grazing districts, so Panel B is descriptive. A district-level version of the electoral outcome, aggregating county returns to the member's own district, is not estimable: districts drawn inside a single county cannot be assigned county returns, which excludes California, and only four of the 37 remaining districts had a Nay-voting representative, at which point the cluster-robust variance matrix is not positive semi-definite.

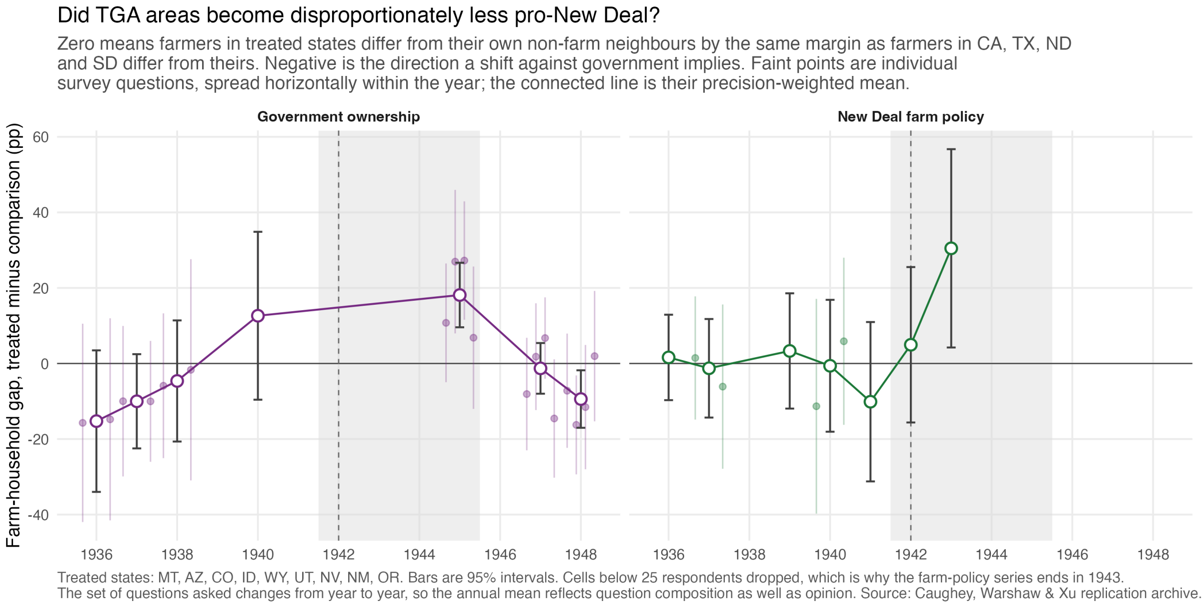

Figure C.9: Farm-Household Attitude Gaps Across Two Issue Domains, 1936–1948
Notes: Each faint point is one survey question in one year; the connected line is the precision-weighted mean of the questions asked that year. The vertical axis is the farm-minus-non-farm gap in treated states, net of the same gap in the comparison states; negative is the direction a shift against government implies. Farm-support questions changed wording between waves and are matched by policy object; the government-ownership questions recur verbatim. Cells below 25 respondents are dropped, which is why the farm-policy series ends in 1943, and the question set changes across years, so the annual mean reflects composition as well as opinion. Table C.14 reports every estimate. Source: the pooled AIPO records of Caughey et al. (2019).

Table C.13: Inference Under Alternative Clustering and Spatial-Correlation Schemes

		Clustered on		Conley, cutoff			
	Coefficient	County	District	100 km	200 km	300 km	Obs.
Panel A: Difference-in-differences (published Table 3)							
Coefficient: HG × TGA							
(1) County and state × year FE	-1.531	[0.090]	[0.384]	[0.138]	[0.209]	[0.219]	20,800
(2) + population	-1.935	[0.179]	[0.439]	[0.241]	[0.332]	[0.381]	20,800
(3) + land value, farm size	-1.862	[0.235]	[0.480]	[0.298]	[0.388]	[0.436]	20,768
(4) + debt-to-value	-1.862	[0.255]	[0.496]	[0.318]	[0.408]	[0.455]	20,768
(5) Nine TGA states only	-3.757	[0.002]	[0.177]	[0.014]	[0.040]	[0.057]	9,551
(6) Continuous grazing-area share	-5.005	[0.004]	[0.202]	[0.015]	[0.052]	[0.085]	20,768
Panel B: Triple-difference (published Table 4)							
Coefficient: HG × TGA × HighDebt							
(1) Full sample	-4.194	[0.001]	[0.009]	[0.003]	[0.007]	[0.006]	20,800
(2) Full sample, covariates	-4.146	[0.042]	[0.072]	[0.052]	[0.071]	[0.088]	20,768
(3) 1918–1960 window	-4.284	[0.001]	[0.002]	[0.002]	[0.003]	[0.002]	13,582
(4) 1918–1960, covariates	-5.717	[0.017]	[0.040]	[0.026]	[0.036]	[0.038]	13,561
(5) Continuous grazing share	-7.536	[0.003]	[0.014]	[0.004]	[0.009]	[0.013]	20,800
(6) Continuous share, covariates	-7.554	[0.009]	[0.026]	[0.012]	[0.021]	[0.030]	20,768
Panel C: Gratitude, roll call (published Table 5, Panel A)							
Coefficient: HG × TGA, interacted or split by the representative's vote							
(1) × own representative's vote	-13.674	[0.000]	[0.003]	[0.000]	[0.001]	[0.002]	20,768
(2) Nay-representative subsample	9.709	[0.028]	[0.139]	[0.030]	[0.061]	[0.086]	3,917
(3) Yea-representative subsample	-4.327	[0.004]	[0.160]	[0.024]	[0.066]	[0.097]	16,851

Notes: Each row is one published column; each column is one inference scheme, reporting p-values in brackets (p-values rather than standard errors because the schemes differ in degrees of freedom; the point estimate does not depend on the scheme). "District" clusters on the county's 73rd-Congress electoral district (49 districts). The Conley variances use a Bartlett kernel in the distance between county centroids and allow serial correlation at all lags, so each one nests the county-clustered variance rather than trading the time dimension for a distance one. A variance built on distance alone is smaller than the county-clustered one here: county vote shares are persistent, so dropping the serial dimension discards most of the dependence the published errors already allow. Panel C row (1) interacts treatment with the own representative's vote on H.R. 6462; rows (2)–(3) split by it. Panel A row (6) and Panel B rows (5)–(6) use the continuous grazing-area share. The Gallup indices are individual-level data with no county geography and retain state clustering.

Table C.14: Farm-Household Attitude Gaps: Estimates by Question and Year

Question	Year	Gap (pp)	(SE)	Farm respondents Treated	Comparison
Panel A: Government ownership					
Government ownership of the railroads	1936	−15.7	(13.4)	34	30
	1937	−10.0	(10.2)	51	44
	1938	−5.9	(9.8)	58	54
	1945	10.8	(8.0)	58	89
	1947	−8.1	(7.6)	87	78
	1948	−7.2	(7.7)	55	66
Government ownership of the banks	1936	−14.8	(13.6)	34	31
	1937	−10.0	(8.2)	109	88
	1945	27.0	(9.7)	46	52
	1947	1.8	(7.2)	87	79
	1948	−16.3	(6.7)	51	69
Public ownership of electric power	1938	−1.7	(14.9)	26	27
	1940	12.6	(11.3)	52	52
	1945	27.3	(8.0)	76	77
	1947	6.7	(5.5)	188	170
	1948	−11.5	(8.4)	51	67
Postwar government ownership (coal)	1945	6.8	(9.6)	45	53
	1947	−14.6	(8.0)	88	82
	1948	1.9	(8.8)	57	66
Panel B: New Deal farm policy					
Government loans for tenants to buy farms	1936	1.6	(5.8)	76	60
Revive the AAA	1937	1.5	(8.3)	115	77
Government fixing of farm prices	1937	−6.1	(11.1)	65	45
Increase government spending on farm aid	1939	3.3	(7.8)	96	75
	1940	−11.3	(14.5)	40	28
Cut farm payments by 30 percent	1940	5.9	(11.3)	42	68
Reduce federal spending on farm benefits	1941	−10.1	(10.8)	48	50
End farm benefits until the end of the war	1942	5.0	(10.5)	53	66
Pass farm subsidies	1943	30.5	(13.4)	38	33

Notes: Each row is one survey question in one year, the estimates plotted in Figure C.9; Panel A carries the government-ownership domain. The railroad, bank and electric-power items are the three whose wording recurs across the whole period, and Figure C.8 plots those three; the coal item was asked only from 1945. The gap is the farm-minus-non-farm difference in support in treated states, net of the same difference in the comparison states, in percentage points, signed so that positive means more supportive of government. The last two columns count farm respondents in treated and comparison states; cells below 25 are dropped. Standard errors combine the sampling variances of the four cell means. Source: the pooled AIPO records of Caughey et al. (2019).