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

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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 reveal 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 vote heterogeneity and 1936–37 Gallup data rule out simple gratitude: beneficiaries supported farm-specific policies but opposed the broader redistributive platform. 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, leading them to exit the redistributive coalition — not despite the policy's success, but because of it.

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 setting combines the features needed to distinguish reciprocity 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: the initial pro-Democratic effect should reverse as the wealth gain materializes, and the reversal should be largest where balance sheets improved most. 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 county debt-to-value ratios. To discriminate among competing mechanisms, we examine heterogeneity across House roll-call votes on the TGA and draw on 1936–37 Gallup surveys of farm-policy attitudes.

The initial electoral returns were positive but short-lived. 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 effect masks offsetting dynamics that a triple-difference reveals: counties with high pre-treatment leverage accounted for most of the anti-Democratic shift, while low-debt counties continued to reward Democrats.

Event-study pre-trends confirm that treated and control counties followed indistinguishable partisan trajectories before 1934. Livestock-share controls and placebo tests on non-TGA counties show that the 1940–42 reversal reflects the TGA's wealth channel, not WWII commodity demand alone, though wartime demand helped activate it by capitalizing the TGA's asset gains. A turnout event study confirms that the TGA changed *how* treated counties voted, not *whether* they voted.

Instrumental-variable estimates using the Bühler (2023) rainfall instrument corroborate the sign through both the main effect and the debt interaction. High- and low-debt TGA counties are balanced on pre-treatment observables, including prior Democratic vote share, and a placebo triple-difference on the pre-treatment window produces no effect.

To discriminate among competing mechanisms, gratitude, ideological conversion, and economic self-interest, we examine roll-call vote heterogeneity and individual-level survey data. Roll-call evidence is inconsistent with gratitude: states whose delegations championed the TGA saw the largest anti-Democratic swing. Gallup survey data from 1936–37 point to economic self-interest rather than ideological conversion: TGA-state respondents opposed reviving the AAA but favored higher farm prices. Beneficiaries supported the specific policies that raised their land values but opposed the broader redistributive platform — consistent with self-interest narrowing, not gratitude fading or ideology shifting.

These findings identify a boundary condition on distributive politics. In the canonical Meltzer–Richard framework, voters support redistribution when their wealth falls below the mean, because the expected transfer exceeds the expected tax. A program that permanently raises beneficiaries above that threshold converts them from net recipients into net contributors, reversing the electoral return. In the TGA case, the Act alone did not generate a large enough wealth gain by 1940; wartime commodity demand was required to push farm values past the break-even point where the tax cost of redistribution exceeded the transfer benefit.

The TGA case connects the distributive-politics literature to research on wealth and preferences for redistribution (Alesina and Giuliano, 2011), and to the debate over whether self-interest or values drive policy attitudes (Sears and Funk, 1991; Margalit, 2013). It also extends work on the political economy of the New Deal (Fishback et al., 2003, 2006) by documenting how one program's wealth effects eroded the coalition that enacted it. 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, they 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 identifies a boundary condition: 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 case yields the mirror image, suggesting that the form of the transfer determines whether political loyalty persists or reverses.

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) demonstrates 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, a pattern that parallels our triple-difference finding that the political shift concentrated where the collateral gain was largest. 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, a time horizon that no modern program evaluation can offer.

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 be-

[1] The TGA's rangeland crisis was geographically distinct from the Dust Bowl. The Dust Bowl centered on western Kansas, the Oklahoma panhandle, and the Texas panhandle and devastated cropland, not rangeland. The TGA states lie west of the Dust Bowl region and contain predominantly livestock-grazing economies. This geographic separation matters for identification: the 1934 rainfall instrument used in Section 5.5 captures rangeland conditions in TGA states, not Dust Bowl severity. Drought-relief programs (FERA, AAA drought payments) targeted the Dust Bowl states, not the western rangeland states in our sample. Conflating the two would overstate the confounding role of federal relief in TGA counties.

gin regulating grazing under his own authority. Third, dust storms in May 1934 gave 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. Grazing fees were set at $0.05 per animal unit month (roughly $1.65 in today's 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. 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, and revocation was possible only in cases of grazing violations when rights had been pledged as collateral, making permits de facto property rights tied to farms (Bühler, 2023). When a ranch 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 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]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.

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 poses a sharp political question: does a wealth transfer buy lasting loyalty for the party that enacted it? 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 large literature documents this channel, from federal spending in U.S. House elections (Levitt and Snyder, 1997) to conditional cash transfers in developing countries (De La O, 2013; Zucco, 2013; Manacorda et al., 2011). Applied to the TGA, gratitude predicts (a) a durable pro-Democratic shift in treated counties that persists as long as voters remember who enacted the policy; (b) a uniform shift across all treated counties, since all received the same policy regardless of pre-treatment economic conditions; and (c) in survey data, broadly pro-government attitudes among beneficiaries, and larger Democratic gains in states whose congressional delegations visibly championed the legislation.

[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.

Hypothesis 2: Wealth and the demand for redistribution. A competing prediction reverses the sign. Several theoretical channels link wealth gains to reduced demand for redistribution, and they are observationally equivalent with county-level data, so we refer to the composite as the "wealth channel" without claiming to isolate a single mechanism.[5]

Applied to the TGA, the wealth channel predicts not an immediate shift but a delayed reversal: the initial pro-Democratic boost reflects short-run policy salience before balance-sheet effects propagate, and the timing of the reversal depends on when market conditions capitalize the underlying asset (Section 7.3).[6] Specifically, the wealth channel predicts (a) an initial pro-Democratic effect that reverses as the wealth gain works through land markets and credit conditions; (b) an anti-Democratic shift that scales with the size of the wealth gain, concentrating in high-leverage counties where the balance-sheet improvement was largest; and (c) in survey data, selective opposition to redistribution: voters reject the platform that costs them while continuing to support the specific policies that benefit them directly. 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). Applied to the TGA, ideological conversion predicts (a) a shift away from Democrats, similar to the wealth channel; (b) a uniform shift across treated counties, since all experienced the same government intervention regardless of pre-treatment leverage; and (c) in survey data, uniformly lower support for government across *all* policy domains, not just redistribution but also regulation, public ownership, and government spending broadly. This pattern 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).

[5]Three channels contribute. In the Meltzer and Richard (1981) framework, voters whose income exceeds the mean oppose redistribution because net transfers flow away from them. Ansell (2014) emphasizes a complementary mechanism: rising asset values reduce demand for social insurance by providing a private buffer against economic shocks. A third channel operates through economic independence: the TGA gave ranchers pledgeable property rights, reducing their dependence on federal credit programs and, with it, their stake in the redistributive coalition. We use the 1930 median debt-to-value ratio as a proxy for exposure to all three: counties above the median experienced the largest balance-sheet improvement and the greatest shift in their net position relative to the redistributive program.

[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
Reciprocity	Pro-Dem (durable)	Uniform	Pro-TGA deleg. → larger Dem gain	Broadly pro-government
Wealth channel	Anti-Dem (after reversal)	Concentratesd in high-debt counties	Delegation vote irrelevant	Selective: pro farm benefits, anti-redistribution
Ideological conversion	Anti-Dem	Uniform	Delegation vote irrelevant	Uniformly anti-government

4 Data

4.1 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.

4.2 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

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

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 alongside county-level Democratic vote share in the 1932 presidential election and congressional electoral district boundaries. Treated counties span the full 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. The map also shows how grazing districts cut across electoral district boundaries, generating within-district variation in treatment exposure that our county-level analysis exploits.

4.3 Controls

We control for population-weighted New Deal spending to absorbs 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.

4.4 Roll-Call Votes

House roll-call votes on H.R. 6462 (the Taylor Grazing Act) come from the Congressional Record, April 1934. We classify each state's delegation as "pro-TGA" if more than 50% of its representatives voted "Yea". This binary classification captures the delegation's revealed stance on the legislation; majority support motivates the threshold, and the results hold under alternative cutoffs.

[8]The method weights each program by the inverse of its covariance with other programs, upweighting programs that provide independent information about the overall intensity of federal transfers. A single index is preferred over program-by-program controls because it avoids overfitting on six correlated regressors while capturing how heavily each county was targeted by New Deal spending.

4.5 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.

4.6 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 average Democratic vote share is 54.5%, with substantial cross-county variation (SD = 27.1). Average land values in 1930 were \$37.76 per acre, though this masks wide dispersion driven by differences between irrigated cropland and arid rangeland. The mean debt-to-value ratio stood at 35.1%, indicating that farm mortgage debt represented roughly a third of property values on the eve of the TGA. New Deal spending varied across programs: AAA transfers averaged \$66.65 per capita, more than FERA (\$21.90) and WPA (\$39.21), reflecting the program's focus on crop-producing counties. Appendix 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.

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: Summary statistics for the county × election-year panel used throughout the paper. The sample covers 742 counties in 13 western and plains states (AZ, CA, CO, ID, MT, ND, NV, NM, OR, SD, TX, UT, WY), observed in contested congressional elections from 1910 to 1972. Democratic vote share is the county-level Democratic share of the two-party vote (%). Agricultural variables are county-level measures from the 1930 Census of Agriculture, frozen at their pre-treatment values. Debt-to-value ratio is total farm mortgage debt divided by total farm property value, frozen at 1930 levels. New Deal program spending (FERA, WPA, AAA) is measured in dollars per capita using 1930 county population as the denominator, following 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.[9]

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

where Dem_{ct} is the Democratic vote share in county c at election t; $HG \times TGA_{ct}$ is the interaction of the treatment indicator with a post-1934 dummy; α_c are county fixed effects; γ_{st} are state-by-year fixed effects; and $z_c \times \mathbf{1}(t = k)$ denotes the population-weighted 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 con-

[9]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.

ditions. The identifying assumption is that, conditional on the range 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.

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 (Appendix 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 (Figure 2) provide the strongest evidence: over seven pre-treatment election cycles, treated and control counties followed indistinguishable partisan trajectories. Nothing about the pre-1934 political landscape distinguished grazing-land counties from their western neighbors: both groups participated in the same state-level political contests, faced the same Depression-era economic conditions, and received similar New Deal transfers through programs like FERA and the AAA.

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 our identification strategy is political selection: counties that received TGA treatment may have voted differently from control counties *before* 1934, precisely because their political preferences helped bring about the legislation. If grazing-land counties were already trending toward (or away from) the Democrats prior to the Act, any post-1934 shift could reflect a continuation of pre-existing dynamics rather than a causal effect of the TGA. More concretely, western ranchers who stood to benefit from formalized grazing rights may have lobbied for the Act through their congressional delegations, and the political conditions that produced this lobbying could independently predict future voting patterns.

We test this concern directly with an event-study specification:

$$\begin{aligned}\text{Dem}_{ct} = \sum_{k\neq 1934} &\beta_k \cdot \text{Historical Grazing}_c \times \mathbf{1}(t=k) + \alpha_c + \gamma_{st} \\ &+ \sum_j \delta_j \cdot z_c \times \mathbf{1}(t=j) + \varepsilon_{ct} \end{aligned} \tag{2}$$

where the omitted category is 1934, the last pre-treatment election.[10] The pre-treatment coefficients $\{\beta_k\}$ for $k < 1934$ test the parallel-trends assumption: if political selection drove both TGA assignment and voting behavior, we would expect these coefficients to diverge systematically from zero before the Act's passage. The post-treatment coefficients for $k > 1934$ trace the dynamic treatment effect. The sample runs from 1920 to 1950, providing seven pre-treatment election cycles (1920–1932) and eight post-treatment cycles (1936–1950).

The event study also serves a second purpose. If the TGA's effect operated through the wealth channel we propose, the post-treatment coefficients should not simply jump to a new level and remain there. Instead, we expect a specific temporal pattern: an initial pro-Democratic effect (reflecting short-run gratitude or New Deal enthusiasm) followed by a reversal as the wealth shock shifted preferences away from Democrats. A monotonic positive shift would point toward durable partisan loyalty; a reversal points toward the economic mechanism.

5.3 Triple-Difference: The Collateral Channel

The difference-in-differences estimate tells us whether TGA counties shifted their voting behavior, but not *why*. Several channels could produce such a shift. Voters might

[10] 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 grazing districts had not yet been established and no permits had been issued. We classify 1934 as the base year accordingly. For further details on the implementation timeline, see Bühler (2023).

reward Democrats out of gratitude for passing the Act. They might update their ideological beliefs after experiencing government intervention. Or, as we argue, the wealth shock from formalized property rights could reduce demand for redistribution, pushing beneficiaries away from the Democratic coalition.

These channels predict different cross-sectional patterns, and the balance-sheet mechanism offers a sharp test. The TGA raised property values by formalizing grazing rights. For counties with high pre-treatment debt, this wealth gain produced the largest improvement in net worth: leverage ratios fell, balance sheets strengthened, and the county's position relative to the redistribution threshold shifted most. For counties with low pre-treatment debt, the same property-value gain mattered less because their balance sheets were already strong. If the wealth channel drives the political shift, the effect should concentrate in high-debt counties, precisely those where formalized rights improved net worth the most. If gratitude or ideology drives the shift, pre-treatment debt should be irrelevant: all treated counties received the same policy, regardless of their balance sheets.

We test this prediction with a triple-difference specification that interacts the treatment with pre-treatment indebtedness:

$$\begin{aligned} \text{Dem}_{ct} = &\beta_1 \cdot \text{HG} \times \text{TGA}_{ct} + \beta_2 \cdot \text{HG} \times \text{TGA}_{ct} \times \text{HighDebt}_c \\ &+ \beta_3 \cdot \text{TGA}_t \times \text{HighDebt}_c + \alpha_c + \gamma_{st} + \varepsilon_{ct} \end{aligned} \tag{3}$$

where HighDebt_c indicates counties with above-median debt-to-value ratios in 1930 (median = 36.5%). The coefficient β_3 captures differential trends for high-debt counties in the post-TGA period, absorbing the possibility that indebted agricultural counties followed distinct political trajectories regardless of TGA exposure. The coefficient β_1 captures the TGA effect in low-debt counties; $\beta_1 + \beta_2$ captures the effect in high-debt counties. The key parameter is β_2. Under the gratitude hypothesis, β_2 should be zero or positive: counties that benefited most should reward Democrats most. Under the wealth channel hypothesis, β_2 should be negative: counties whose balance sheets improved most should shift furthest from the redistributive party.

5.4 Mechanism Discrimination

Even if the triple-difference establishes that the wealth channel amplifies the political shift, a competing explanation remains: voters may simply reward the party that delivered the policy. Under this gratitude account, Democratic vote gains should be largest where Democratic representatives visibly championed the TGA, because voters reward *their* representatives for fighting on their behalf. Under the self-interest account, voters respond to the material benefit regardless of who delivered it, so the identity of the representative should not matter. Distinguishing between these two stories requires variation in political credit that is orthogonal to the economic benefit itself.

Roll-call heterogeneity. House roll-call votes on the TGA provide exactly this variation. Some state delegations voted overwhelmingly in favor of the Act; others opposed it. Crucially, the economic benefit of the TGA (formalized grazing rights and the associated property-value gains) accrued to treated counties regardless of how their representatives voted. If gratitude drives the electoral response, voters should reward Democrats more in states where the delegation championed the legislation. If self-interest drives it, the delegation's vote should be irrelevant. We interact HG $\times$ TGA with an indicator for whether the county's state delegation voted majority-Yea on the TGA:

$$\text{Dem}_{ct} = \beta_1 \cdot \text{HG} \times \text{TGA}_{ct} + \beta_2 \cdot \text{HG} \times \text{TGA}_{ct} \times \text{ProTGA}_s + \alpha_c + \gamma_{st} + \varepsilon_{ct} \tag{4}$$

The coefficient β_2 is the key discriminator. Gratitude predicts $\beta_2 > 0$: voters in pro-TGA delegation states should shift more toward Democrats. Self-interest predicts $\beta_2 \leq 0$: the electoral response should be independent of, or even negatively related to, the delegation's vote, since the policy benefits accrue regardless. The test assumes that a delegation's TGA vote is orthogonal to county-level trends in Democratic voting, conditional on county and state-by-year fixed effects. Pro-TGA delegations may represent more agricultural or more rural districts, but county fixed effects absorb time-invariant differences and state-by-year fixed effects remove state-level partisan shocks. The remaining concern, that pro-TGA delegation counties experienced differential *within-state* political trends, is unlikely given the Act's bipartisan western roots (Section 2).

5.5 Instrumental Variables

We follow Bühler (2023), who constructs a rainfall instrument for TGA treatment in the nine western states and provides a detailed justification for its validity. The instrument exploits the fact that in October 1934, federal land surveyors classified rangeland into grades of degradation, and this classification reflected both the intensity of prior grazing and momentary rain-induced perceived land quality. Counties that received less rainfall during the survey period appeared more severely degraded, making them more likely to be placed under TGA regulation and ultimately to receive more grazing permits. To isolate the drought shock from persistent climate differences, Bühler (2023) standardizes October 1934 rainfall using each county's historical mean and standard deviation.

We instrument HG $\times$ TGA with the interaction of this standardized rainfall measure and a post-1934 indicator. The exclusion restriction requires that rainfall affected Democratic vote share only through its effect on TGA treatment intensity. This restriction is plausible: the rainfall variation captures a transitory weather shock during the month-long survey period (Bühler, 2023), not persistent climatic differences that might correlate with economic structure or political preferences. One might worry that drought counties experienced differential economic recovery paths that independently affected voting; however, the instrument isolates momentary perceived land quality during the survey, not long-run climatic conditions that would correlate with recovery trajectories.

6 Results I: The TGA and Democratic Vote Share

6.1 Event Study

Our difference-in-differences design is credible if the parallel-trends assumption holds: absent treatment, the outcome in treated and control counties would have evolved along the same trajectory. In our setting, this requires that counties with and without historic grazing land did not vote differentially for Democrats prior to the TGA's enactment. As discussed in Section 2, the qualitative record gives no reason to expect such divergence: before 1934, no federal policy distinguished grazing-land counties from their neighbors, and both groups faced the same economic and political conditions. Figure 2 reports the event-study coefficients from Equation (2) 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 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. The 1944 coefficient reaches -3.57 percentage points (SE $= 1.72$), and the effect remains negative through 1950. 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? Bühler (2023) shows that the TGA formalized grazing rights into pledgeable collateral, enabling ranchers to invest in range improvements and expand herds. But Appendix Figure D.3 reveals that the TGA alone had not yet produced a large enough wealth gain: an event study of farm values from the Census of Agriculture shows that the gap between TGA and non-TGA counties was modest and statistically insignificant at 1940, six years after enactment. The gap widened sharply only by 1945 ($4,075, $p = 0.038$) and 1950 ($9,500, $p = 0.004$), when wartime commodity demand capitalized the investments that secure tenure had enabled.[11] Secure tenure was necessary but not sufficient; wartime demand provided the second shock that activated the wealth channel (Section 7.3).

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 population-weighted 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 coefficient sharpens to -3.76*** (SE $= 1.23$), more than double the full-sample estimate. 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.

[11] 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.

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, the effect should concentrate in counties where the balance-sheet improvement was largest: those with high pre-treatment debt.

7.1 The TGA Reduced Farm Leverage

Figure 3 reports an event study of county-level debt-to-value ratios from the Agricultural Census, with 1930 as the base year and county plus state-by-year fixed effects ($N = 2{,}957$ county-year observations across 740 counties). Before the TGA, grazing-land counties carried significantly higher leverage than their non-grazing neighbors: the 1920 and 1925 coefficients are $+1.84$ (SE $= 0.93$) and $+3.03$ (SE $= 0.92$), both statistically significant.

This pre-existing leverage gap could threaten the wealth-channel interpretation if indebtedness also predicted Democratic voting before the TGA. It does not. Figure 2 shows that the pre-treatment coefficients for Democratic vote share are flat across the 1920–1932 period: none approaches conventional significance. The wealth channel we identify operates through the *change* in leverage induced by the TGA, the reversal from above- to below-average debt-to-value ratios, not through the pre-existing *level* of indebtedness. Higher pre-TGA leverage predicted balance-sheet exposure to the wealth shock, not prior partisan leanings.

After 1934, the leverage gap reversed. By 1940, treated counties had moved from above-average to below-average debt-to-value ratios, a swing of roughly 4–6 percentage points relative to the pre-treatment gap. The TGA did not reduce farm debt; it raised property values, pushing leverage down as the denominator grew. This pattern confirms the wealth channel documented by Bühler (2023) operating through farm balance sheets.

7.2 Who Turned Against Democrats?

If the wealth channel drove the political reversal, the anti-Democratic shift should concentrate among counties where the TGA's wealth gain most improved net worth: those with high pre-treatment debt. Counties already operating at low leverage experienced a smaller balance-sheet shift; their political response should differ. We exploit this variation in a triple-difference design.

Table 4 presents the estimates from Equation (3). The HighDebt interaction is negative and significant across all six specifications, ranging from -4.15^{**} to -7.55^{***}: counties that experienced the largest balance-sheet improvement moved most strongly *against* the party that enacted the policy. In the richest binary specification (1918–1960 with covariates, column 4), the base TGA effect in low-debt counties is $+5.67^{**}$ (SE $= 2.50$), while the interaction is -5.72^{**} (SE $= 2.40$), yielding a net effect in high-debt counties near zero.

Columns (5)–(6) replace the binary treatment with continuous grazing area share, testing whether the wealth channel operates at the intensive margin. The interaction strengthens to -7.54^{***} (SE $= 2.54$) without controls and -7.55^{***} (SE $= 2.88$) with controls: counties with both larger grazing-district coverage and higher pre-treatment leverage shifted most sharply against Democrats.

The split between low- and high-debt counties sharpens the paper's central finding. Low-debt TGA counties, those where the balance-sheet improvement was smaller, actually *rewarded* Democrats ($+5.67^{**}$). The negative pooled effect from Section 6 is largely attributable to high-debt counties, where the net effect ($5.67 - 5.72 \approx 0$) shows the wealth channel exactly offsetting the initial Democratic boost. The same policy produced opposite political responses depending on the magnitude of the balance-sheet improvement. Outside TGA areas, high-debt counties trended *toward* Democrats after 1934, the opposite of the pattern in treated counties. The triple-difference thus isolates a TGA-specific wealth effect, not a general shift among indebted agricultural counties.

7.3 The Timing of the Reversal: Two-Shock Complementarity

The event-study coefficients reveal a striking pattern in the timing of the reversal. The peak-to-trough swing from 1940 to 1942 spans a single two-year cycle. The decline is concentrated in this interval, not spread across multiple cycles, matching the wealth channel's prediction of a discrete break once the tax cost of redistribution exceeds the transfer benefit.

The sharp reversal coincides with U.S. entry into World War II, raising the concern that wartime agricultural demand — not the TGA's wealth channel — drove the anti-Democratic shift. Section 8.3 addresses this confound with three tests: controlling for livestock-share exposure, a placebo on non-TGA livestock counties, and a within-TGA balance check. All three reject the WWII commodity channel as the driver.

The evidence points to a two-shock complementarity rather than a single treatment effect. The TGA was necessary: it created the asset (formalized property rights, pledgeable collateral, enabled investment). But the TGA alone had not generated a large enough wealth gain by 1940 to trigger the reversal: farm values in treated counties had diverged only modestly and insignificantly (Appendix Figure D.3). WWII commodity demand was also necessary: it capitalized the TGA investments, pushing farm values past the threshold at which redistribution became net costly. Crucially, WWII alone was not sufficient either: non-TGA livestock counties experienced the same commodity boom but showed no political reversal.

7.4 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). Convergence predicts slow erosion, not a discrete break. 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

8.1 Instrumental Variables

As a complement to the DiD and event-study results, we report instrumental variables estimates using the rainfall instrument developed by Bühler (2023) for economic outcomes and described in Section 5.5. We present the IV as corroborating evidence rather than as the primary identification strategy. The exclusion restriction, that 1934 rainfall affected Democratic vote share only through TGA treatment intensity, is harder to defend for political outcomes than for the economic outcomes Bühler (2023) originally studied. Drought conditions could affect voting through channels other than the TGA, including federal relief allocation and out-migration. We therefore emphasize the reduced form, which confirms the sign of the main effect without requiring the exclusion restriction to hold exactly.

Appendix Table D.1 reports a strong first stage ($F = 34$). The reduced form confirms the sign through a three-step chain: counties that received more rainfall during the October 1934 survey appeared less degraded to federal surveyors, received fewer grazing permits as a result, and subsequently voted more Democratic. The positive reduced-form coefficient (+1.6*, SE = 0.90) therefore implies that greater TGA exposure reduced Democratic support. We regard the reduced form as the most credible IV result: it confirms the sign without requiring the exclusion restriction to hold exactly.

The 2SLS estimate with controls (−8.7*, SE = 5.04) is 2.3 times the western-subsample OLS estimate (−3.76). Two factors likely contribute: the binary treatment indicator introduces classical measurement error that the continuous instrument corrects, and the LATE among compliers (marginal rangeland counties whose classification depended on momentary 1934 rainfall) may exceed the ATE.[12]

The wealth channel also operates under exogenous variation (Appendix Table D.2). The reduced form is again the most informative result: interacting the rainfall instrument with HighDebt yields a coefficient of +3.6** (SE = 1.79), confirming that exogenous rainfall variation predicts larger anti-Democratic shifts in high-debt counties. The 2SLS triple-difference interaction is −26.5 ($p = 0.056$; first-stage $F = 242$ for the interaction), matching the sign of the OLS estimate.[13] The magnification fits the measurement-error and LATE explanations above.

8.2 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 (Appendix Figure C.1), yielding a two-sided p-value of 0.086, significant at the 10% level. For the triple-difference, a separate permutation reassigns HighDebt status: the real interaction falls entirely outside the placebo distribution (Appendix Figure C.3).

A leave-one-state-out analysis drops each of the thirteen sample states in turn (Appendix Figures C.2 and C.4). All thirteen leave-one-out triple-difference estimates remain negative, and twelve of thirteen baseline DiD estimates are negative. Excluding the three wartime election years (1942, 1944, 1946) leaves both estimates substantively unchanged (Appendix Table C.2).

8.3 WWII Commodity Demand

The 1940–42 reversal coincides with U.S. entry into World War II, raising the concern that wartime agricultural demand drove the anti-Democratic shift through differen-

[12] We cannot fully rule out a third possibility: if 1934 rainfall correlates with broader drought exposure beyond the TGA channel, the IV estimate would be biased away from zero.

[13] The 2SLS specification instruments two endogenous variables (HG × TGA and its HighDebt interaction) with a single cross-sectional source of variation (1934 rainfall) split by HighDebt status. Identification of the interaction therefore relies on the functional-form distinction between the instrument and its HighDebt interaction, a stronger assumption than the baseline IV. The reduced form avoids this assumption entirely: it asks only whether rainfall predicts Democratic vote share *differentially* in high-debt counties.

tial exposure to commodity booms rather than the TGA's wealth channel. Wartime cattle prices rose sharply, and TGA counties had higher livestock shares. We interact each county's 1930 livestock share (and, separately, crop value per farm) with year indicators, allowing counties with different agricultural compositions to follow different partisan trajectories in each election year. The triple-difference interaction holds: -6.443^{***} with livestock-share and crop-value controls (Appendix Table D.3).[14]

A placebo test on non-TGA counties strengthens this conclusion. If wartime demand caused all high-livestock counties to swing Republican regardless of TGA exposure, we should observe the same 1940–42 reversal among non-TGA counties with high livestock shares. We do not. An event study comparing high- vs. low-livestock non-TGA counties shows no statistically clear shift around 1940–42 (Appendix Figure D.1; DiD coefficient -1.5, $p = 0.20$). The placebo estimate is negative but imprecise, indicating no comparably sharp reversal among non-TGA livestock counties.

The triple-difference itself provides a within-TGA test. State-by-year fixed effects absorb wartime shocks common to all counties within a state. For WWII to explain the triple-difference interaction, wartime demand would need to affect high-debt TGA counties differently from low-debt TGA counties *within the same state*, conditional on livestock share and crop value controls. The within-TGA balance test (Appendix Table D.5) shows that high- and low-debt TGA counties are balanced on livestock share, farm size, land values, and New Deal spending, leaving no observable channel through which WWII could differentially affect them.

8.4 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, $p = 0.52$; Appendix Figure D.2). The turnout null rules out mobilization but not compositional change through migration, which we cannot test directly with available data. The within-TGA balance on population (Appendix Table D.5) makes migration an unlikely primary driver.

8.5 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 using tercile and quartile splits, preserves the result: the anti-Democratic shift concentrates in the top debt group

[14] The point estimate is numerically identical with and without crop-value controls because the livestock-share and crop-value varying slopes are orthogonal to the triple-diff interaction after conditioning on county and state-by-year fixed effects; standard errors widen as expected (2.02 to 2.26).

regardless of where the cutoff is drawn (Appendix Table C.1). Excluding WWII elections (1942–1946) leaves both the base TGA effect and the high-debt interaction substantively unchanged (Appendix Table C.2).[15]

High-debt and low-debt TGA counties could differ on dimensions beyond collateral sensitivity. Appendix Table D.5 reports within-TGA balance tests: none of the ten comparisons reaches conventional significance.[16] A placebo triple-difference estimated on 1920–1932 data with a fake treatment date of 1926 produces small and insignificant interactions with both 1920 and 1925 debt (Appendix Table D.4), confirming that the wealth channel is specific to the post-TGA period.

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 party realignment reshuffled coalitions along dimensions orthogonal 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). We test each prediction in turn.

9.1 Gratitude

Table 5 (Panel A) tests whether the TGA's electoral effect varied with the political alignment of the county's congressional delegation. If gratitude drove the vote shift, we would expect larger Democratic gains in states whose representatives championed the TGA. The data show the opposite. In states where the delegation voted against the TGA, treated counties shifted *toward* Democrats. In states where the delegation voted in favor, treated counties shifted *away* from Democrats, and the difference between the two groups is statistically significant. Pro-TGA delegations, the ones voters should have been most grateful toward, experienced the largest anti-Democratic swing. Gratitude cannot explain this pattern; it directly contradicts the prediction.[17]

[15] The triple-difference also holds when using presidential rather than congressional vote share as the outcome, eliminating concerns about candidate quality, incumbency advantage, and redistricting (Appendix Table D.7).

[16] The balanced variables are land values, farm size, livestock share, grazing area share, population, New Deal spending, and pre-TGA Democratic vote share (1930–32).

[17] A concern is that pro-TGA delegations represent states 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. The pro-TGA interaction holds at -8.806^{**} (SE $= 4.370$), confirming that

9.2 Ideology or Self-Interest?

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 less supportive of Roosevelt's spending policy ($p_{\text{raw}} = 0.03$, $p_{\text{BH}} = 0.11$). Notably, they were 9.6 percentage points *more* supportive of government takeover of business ($p_{\text{raw}} = 0.01$, $p_{\text{BH}} = 0.07$), the opposite of what ideological conversion predicts. The remaining differences (on profit regulation, public ownership, and government power) do not survive BH correction.

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 Gallup data complement the county-level analysis: the selective opposition supports self-interest over ideology.

The Anderson (2008) indices summarizing these questions into a farm-specific and a general-government index (Table 5, Panel B) 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 index result is itself informative: it indicates selective rather than blanket support for farm policy.

the delegation vote captures political dynamics, not grazing exposure (Appendix Table D.6).

10 Conclusion

The TGA made western ranchers wealthier, and wealthier ranchers turned against the party that enacted the policy. The triple-difference pins this reversal to counties where the balance-sheet gain was largest. Gallup survey data point to narrow self-interest, specifically opposition to redistribution rather than a blanket anti-government shift, though the evidence thins after multiple-testing correction. Roll-call heterogeneity rejects gratitude: pro-TGA delegation states experienced the largest anti-Democratic swing.

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, and neither shock sufficed alone. 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. The electoral return may reverse when market conditions capitalize the asset, a risk whose timing policy designers cannot control.

Limitations remain. The Gallup evidence, drawn from only nine treated states, thins after Benjamini–Hochberg correction: only one of thirteen comparisons survives at the 5% level. The two-shock structure limits the external validity of the specific timing. Whether the same dynamics operate for recurring transfers or in competitive party systems remains open.

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Tables and Figures

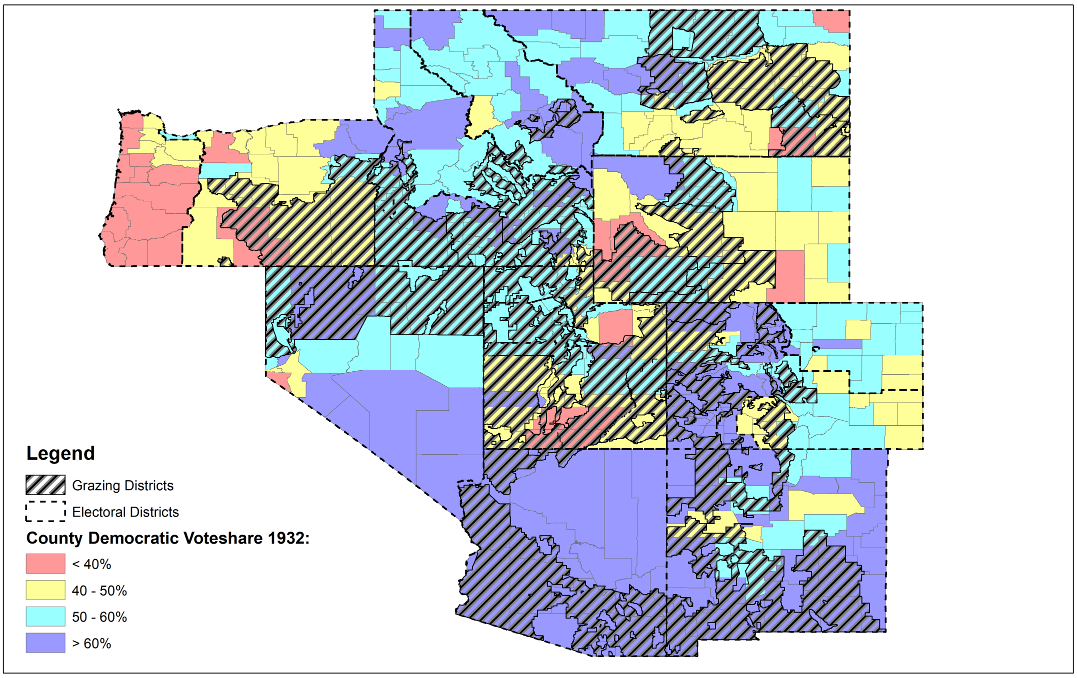


Figure 1: Grazing Districts, Electoral Districts, and Pre-Treatment Democratic Vote Share

Notes: Spatial distribution of TGA treatment and pre-treatment political conditions. Hatched areas indicate grazing districts established under the Taylor Grazing Act of 1934, which formalized access rights to 142 million acres of public rangeland for nearby farmers. Dashed lines delineate congressional electoral district boundaries (73rd Congress). County shading reflects Democratic two-party vote share in the 1932 presidential election, the last election before the TGA. Treated counties span the full 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. The nine TGA states are Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, and Wyoming. Four additional comparison states (California, North Dakota, South Dakota, Texas) contribute untreated counties to the sample.

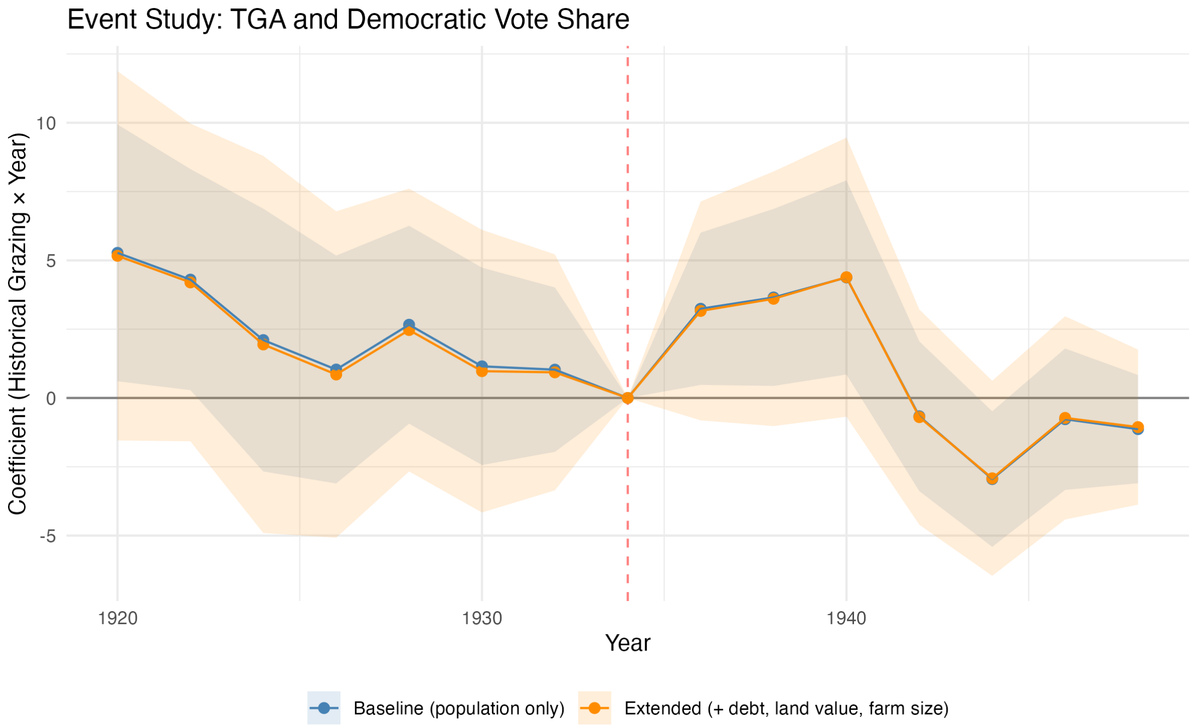


Figure 2: Event Study: TGA and Democratic Vote Share

Notes: Dynamic treatment effects of the Taylor Grazing Act on county-level Democratic vote share. 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). Pre-treatment coefficients (1920–1932) test the parallel-trends assumption: if these coefficients are indistinguishable from zero, TGA and non-TGA counties followed similar partisan trajectories before the Act. Post-treatment coefficients (1936–1950) trace the political response. The pattern shows (1) flat pre-trends, validating the identification strategy; (2) a temporary pro-Democratic boost peaking in 1940 (+3.7 percentage points); and (3) a sharp reversal by 1942, with persistently negative coefficients through the 1950s. The peak-to-trough swing from 1940 to 1942 is 5.1 percentage points. The baseline specification includes county and state $\times$ year fixed effects; the extended specification adds 1930 agricultural covariates (land value, farm size, debt-to-value) interacted with year indicators. Shaded bands show 95% confidence intervals based on county-clustered standard errors. Sample: 742 counties, 1920–1950.

Figure 3: Event Study: TGA and County Debt-to-Value Ratio

Notes: Dynamic treatment effects of the Taylor Grazing Act on county-level farm leverage (debt-to-value ratio, from the Census of Agriculture). Each point estimates the difference in debt-to-value ratios between TGA and non-TGA counties relative to 1930 (the last pre-treatment census). Before the TGA, grazing-land counties carried significantly higher leverage than non-grazing neighbors (the 1920 and 1925 coefficients are positive and significant). After the TGA, this leverage gap reversed: by 1940, treated counties moved from above- to below-average debt-to-value ratios. The TGA did not reduce farm debt; it raised property values by formalizing grazing rights into pledgeable collateral, pushing leverage down as the denominator grew. This pattern confirms that the wealth channel operated through farm balance sheets. Specification includes county and state × year fixed effects. Shaded band shows 95% confidence interval. $N = 2{,}957$ county-year observations across 740 counties. Census years with debt data: 1920, 1925, 1930, 1940.

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

Dependent Variable:	Democratic vote share					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
HG $\times$ TGA	-1.531^{*}	-1.935	-1.862	-1.862	-3.757^{***}	
	(0.902)	(1.439)	(1.567)	(1.635)	(1.229)	
GrazingShare $\times$ TGA						-5.005^{***}
						(1.756)
Fixed-effects						
County	Yes	Yes	Yes	Yes	Yes	Yes
State $\times$ 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: Difference-in-differences estimates of the Taylor Grazing Act's effect on county-level Democratic vote share (%). The treatment variable HG $\times$ TGA equals one for counties overlapping a grazing district established under the TGA, in elections after 1934. A negative coefficient indicates that treated counties shifted away from Democrats relative to untreated counties in the same state and year. Columns (1)–(4) progressively add controls: (1) county and state $\times$ year fixed effects only; (2) adds a population-weighted New Deal spending index (inverse-covariance-weighted combination of FERA, WPA, AAA, CCC, PWA, and RFC per capita) interacted with year indicators; (3) adds 1930 land value per acre and farm size, each interacted with year indicators; (4) adds 1930 debt-to-value ratio interacted with year indicators. Column (5) restricts the sample to the nine TGA states (AZ, CO, ID, MT, NV, NM, OR, UT, WY), dropping non-TGA comparison states. Column (6) replaces the binary treatment indicator with the continuous grazing area share (fraction of county area within grazing districts, ranging from 0 to 1), testing the intensive margin; the coefficient is interpreted as the effect of moving from zero to full grazing-district coverage. All covariates are frozen at 1930 values to avoid post-treatment bias. 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: Triple-difference estimates testing whether the TGA's political effect concentrated in counties where the wealth gain most improved net worth. The key interaction, HG $\times$ TGA $\times$ HighDebt, tests whether the anti-Democratic shift was larger in counties with above-median pre-treatment leverage. HighDebt 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. A negative interaction coefficient means that high-debt counties, those where formalized grazing rights produced the largest balance-sheet improvement, shifted most strongly against Democrats. This pattern is predicted by the wealth channel (Meltzer–Richard) but not by gratitude or ideological conversion, which predict uniform effects regardless of pre-treatment leverage. 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, fraction of county area within grazing districts), testing the intensive margin; HighDebt (HD) is defined identically. Covariates (where included) are 1930 land value, farm size, and the population-weighted 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 and Gallup Policy Indices

	Panel A: Roll Calls			*Panel B: Gallup*	
Dep. Variable:	Dem vote share			Farm index	Govt index
	Interaction	Anti-TGA	Pro-TGA		
Model:	(1)	(2)	(3)	(4)	(5)
HG × TGA	4.306	4.306	-4.500^{***}		
	(4.061)	(4.046)	(1.437)		
HG × TGA × ProTGA	-8.806^{**}				
	(4.306)				
TGA_State				0.001	0.001
				(0.003)	(0.003)
County FE	Yes	Yes	Yes		
State × Year FE	Yes	Yes	Yes		
Demographic FE				Yes	Yes
Covariates	Yes	Yes	Yes		
Observations	20,768	4,837	15,931	63,050	63,050
R^2	0.885	0.739	0.907	0.003	0.006

Notes: Two tests discriminating among competing explanations for the TGA's political effect. **Panel A** tests the gratitude hypothesis using congressional roll-call votes on the TGA. If voters rewarded Democrats out of gratitude, the pro-Democratic shift should be largest in states whose delegations championed the legislation. ProTGA $=1$ if more than 50% of the state's House delegation voted Yea on H.R. 6462 (the Taylor Grazing Act). Column (1) interacts TGA treatment with ProTGA; column (2) restricts to states with anti-TGA delegations; column (3) to states with pro-TGA delegations. The negative interaction in column (1) and the negative coefficient in column (3) are inconsistent with the gratitude hypothesis: states whose representatives championed the TGA experienced the largest *anti*-Democratic swing. All Panel A specifications include county and state × year fixed effects plus 1930 agricultural covariates interacted with year indicators. Standard errors clustered at the county level. **Panel B** tests whether TGA beneficiaries shifted against government uniformly (ideology) or selectively (self-interest), using Anderson (2008) inverse-covariance-weighted indices of individual-level Gallup survey responses (1936–37). The farm index aggregates five farm-specific policy questions; the government index aggregates eight general government questions. TGA_State equals one for respondents in the nine TGA states. Both indices are insignificant, but this reflects offsetting responses within the farm index (see Table 6 for disaggregated results). Demographic fixed effects: sex, race, age group, occupation. Standard errors clustered at the state level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 6: Gallup Poll Responses: TGA vs Non-TGA States (with Multiple-Testing Correction)

Question	TGA	Non-TGA	Diff	p (raw)	p (BH)
Farm-specific					
Govt loans for farmers	0.823	0.829	-0.006	0.769	0.769
Revive AAA	0.333	0.437	-0.104	0.000***	0.001***
Farm benefits	0.590	0.553	$+0.037$	0.361	0.544
Fix farm prices	0.479	0.456	$+0.023$	0.511	0.604
Higher farm prices	0.734	0.623	$+0.111$	0.022**	0.096*
General government					
Govt takeover business	0.602	0.506	$+0.096$	0.011**	0.073*
Regulate profits	0.765	0.707	$+0.058$	0.064*	0.166
Public electricity	0.714	0.664	$+0.050$	0.112	0.242
Govt own railroads	0.371	0.342	$+0.029$	0.451	0.586
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

Notes: Individual-level Gallup survey responses (1936–37) comparing TGA-state and non-TGA-state respondents on specific policy questions. Each 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. After BH correction, only one comparison (Revive AAA, -10.4 pp) is significant at the 5% level. The pattern supports narrow self-interest: TGA-state respondents opposed reviving the AAA (the New Deal's signature agricultural redistribution program, which imposed production controls on ranchers) but favored higher farm prices (the policy most directly tied to their land values). The Gallup comparison exploits state-level variation with nine treated states and should be interpreted as suggestive evidence on the behavioral mechanism, not as standalone causal evidence. Significance from two-sample t-tests; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

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 population-weighted 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.

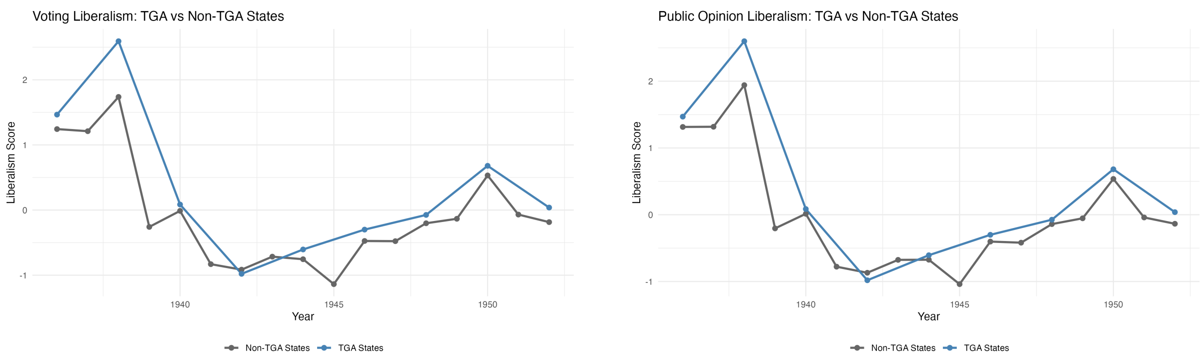
Table A.1: Balance Tests: Pre-Treatment Covariates by TGA Status

	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 an indicator for TGA treatment (Historical Grazing = 1), with state fixed effects. The "Difference" column reports the coefficient on TGA treatment, which captures the within-state difference between TGA and non-TGA counties. "Non-TGA" and "TGA" columns report unconditional group means. All covariates are measured in the 1930 Census of Agriculture or from county-level New Deal expenditure data (Fishback et al., 2003). The New Deal spending index aggregates per-capita FERA, WPA, AAA, CCC, PWA, and RFC expenditures using the Anderson (2008) inverse-covariance-weighted method. Standard errors clustered at the county level; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

B Caughey Economic Liberalism

State-level economic liberalism measures from Caughey et al. (2019) are available only from 1936 onward. Because all observations are post-treatment, TGA_State $\times$ Post1934 is perfectly collinear with state fixed effects, precluding a difference-in-differences design. Table B.1 reports cross-sectional comparisons with year fixed effects only.



(a) Voting Liberalism

(b) Public Opinion Liberalism

Figure B.1: Economic Liberalism: TGA vs Non-TGA States

Notes: Time series of state-level economic liberalism from Caughey et al. (2019) for TGA states (AZ, CO, ID, MT, NV, NM, OR, UT, WY) versus non-TGA states. Panel (a) plots EconLibVot, derived from congressional voting patterns; panel (b) plots EconLibPub, derived from public opinion surveys. If the TGA's wealth shock shifted state-level ideology, TGA states should diverge toward lower liberalism scores after 1934. However, the data begin only in 1936 (all post-treatment), so no pre-treatment baseline exists. Any visible gap could reflect pre-existing differences rather than a TGA effect.

Table B.1: Caughey Economic Liberalism: TGA vs Non-TGA States

Dependent Variable:	EconLibVot	EconLibPub
Model:	(1)	(2)
TGA_State	0.215*	0.147
	(0.122)	(0.119)
Year FE	Yes	Yes
States	49	49
Observations	816	816
R^2	0.676	0.702

Notes: Cross-sectional comparison of state-level economic liberalism between TGA and non-TGA states, using data from Caughey et al. (2019). EconLibVot measures economic liberalism derived from congressional voting patterns; EconLibPub measures economic liberalism derived from public opinion surveys. TGA_State equals one for the nine TGA states. The positive coefficient on TGA_State in column (1) suggests that TGA states had *more* liberal congressional voting records, not less, though the estimate is only marginally significant. These results are inconclusive because the Caughey data begin only in 1936 (all post-treatment), precluding a difference-in-differences design: TGA_State $\times$ Post1934 is perfectly collinear with state fixed effects, so only year fixed effects are included. The absence of pre-treatment observations means we cannot distinguish a TGA effect from pre-existing state-level differences. Standard errors clustered at the state level; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

C Robustness

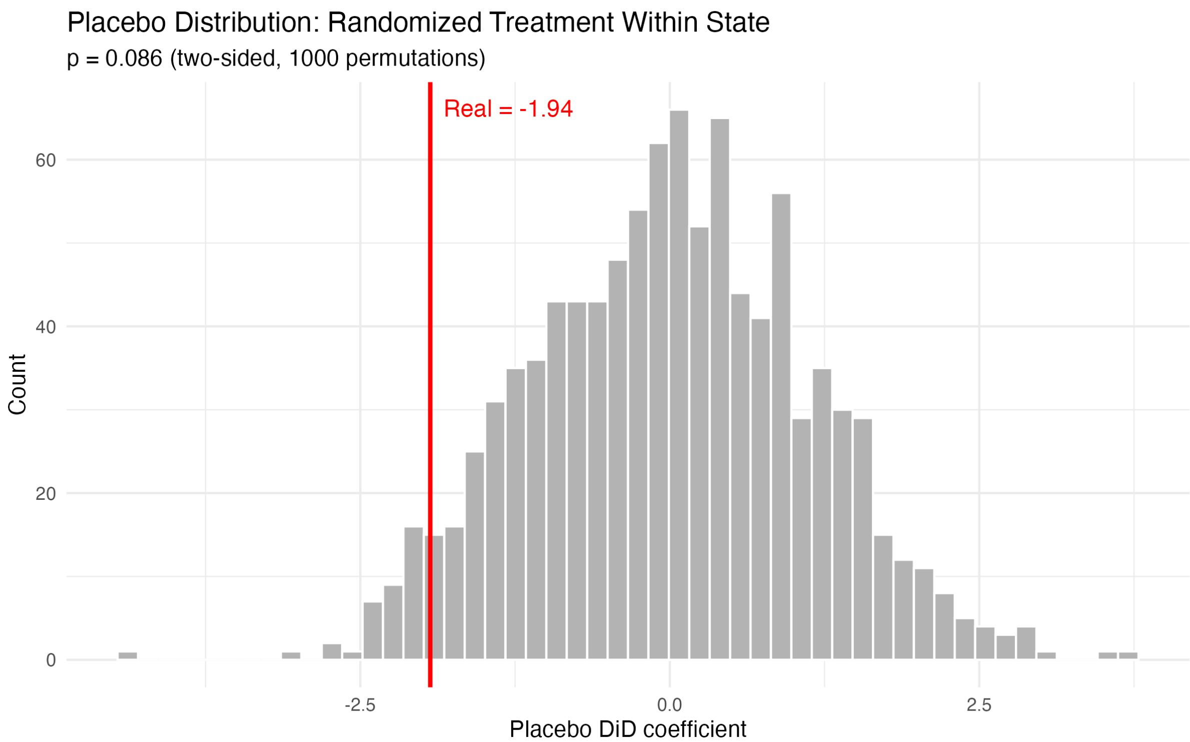


Figure C.1: Placebo Distribution: Randomized Treatment Within State

Notes: Distribution of 1,000 placebo DiD coefficients. Each iteration randomly reassigns HG_Dummy across counties within the same state, preserving the state-level share of treated counties, and re-estimates the baseline specification from Table 3 column (1) with county and state-by-year fixed effects and the population-weighted 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 as the real estimate. Standard errors clustered at the county level.

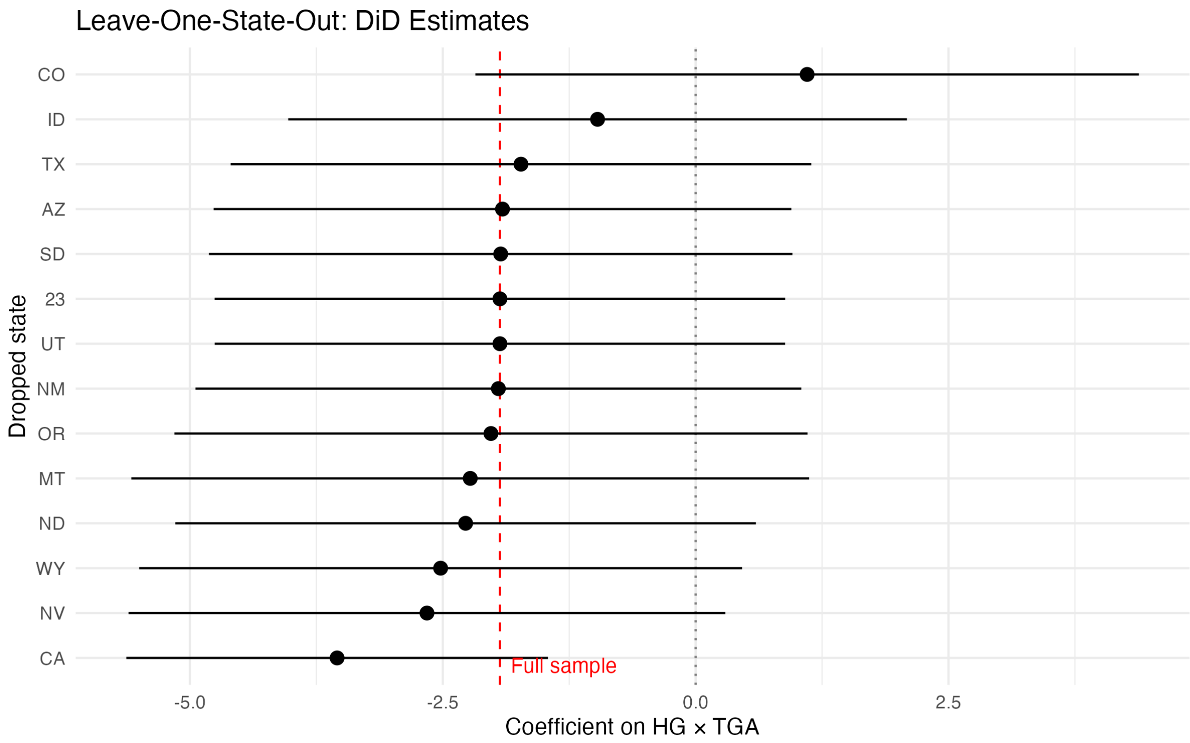


Figure C.2: Leave-One-State-Out: DiD Estimates

Notes: Each point reports the coefficient on HG × TGA from the baseline DiD specification (Table 3, column 1) after dropping the indicated state from the sample. Horizontal bars show 95% confidence intervals. The dashed red line marks the full-sample estimate. The sample includes thirteen states; each is dropped in turn to assess whether any single state drives the result. All specifications include county and state-by-year fixed effects and the population-weighted New Deal spending index interacted with year indicators. Standard errors clustered at the county level.

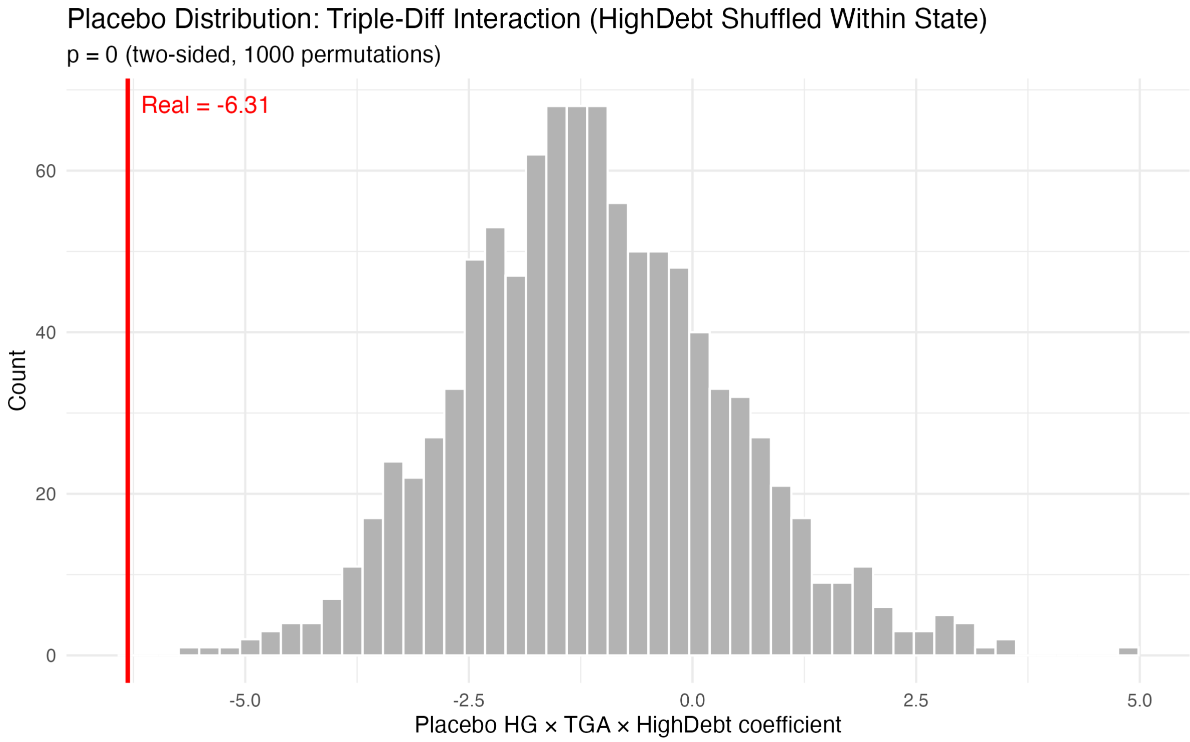


Figure C.3: Placebo Distribution: Triple-Difference Interaction

Notes: Distribution of 1,000 placebo triple-difference interaction coefficients. Each iteration randomly reassigns HighDebt status across counties within the same state, keeping HG (Historical Grazing) treatment fixed, and re-estimates the triple-difference specification from Table 4. HG equals one for counties overlapping a grazing district; TGA equals one for elections after 1934; HighDebt equals one for counties with 1930 debt-to-value ratios above the sample median. The vertical red line marks the real HG $\times$ TGA $\times$ HighDebt estimate. The permutation p-value is the share of placebo coefficients at least as extreme in absolute value as the real estimate. All specifications include county and state-by-year fixed effects and the population-weighted New Deal spending index interacted with year indicators. Standard errors clustered at the county level.

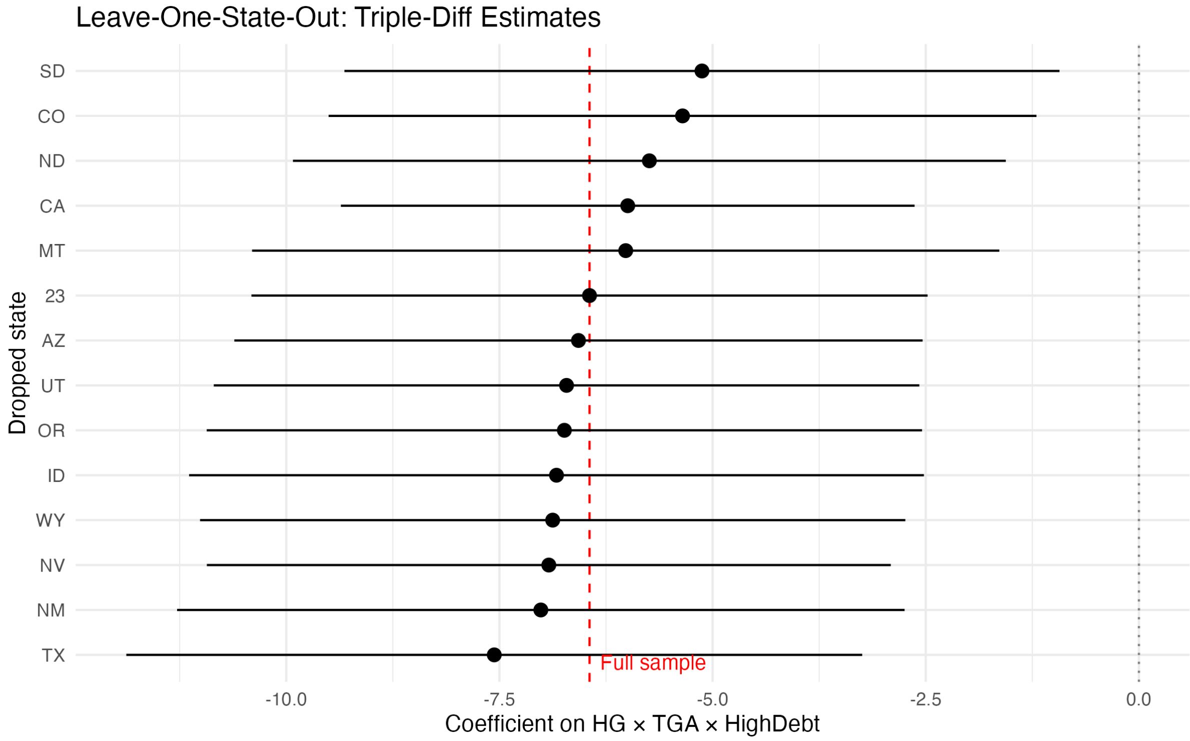


Figure C.4: Leave-One-State-Out: Triple-Difference Estimates

Notes: Each point estimates 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***). All thirteen estimates remain negative, and none crosses zero. Dropping Colorado, the TGA's political epicenter, attenuates the estimate to -5.4 (95% CI $[-9.5, -1.2]$), but it remains significant at the 5% level. No single state drives the collateral-channel result. Specifications include county and state $\times$ year FE plus 1930 agricultural covariates.

Table C.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: Alternative specifications of the triple-difference design from Table 4, varying the definition of high pre-treatment leverage. The dependent variable is county-level Democratic vote share (%). HG (Historical Grazing) equals one for counties overlapping a grazing district; TGA equals one for elections after 1934. Column (1) replaces the binary HighDebt indicator with a continuous interaction between HG × TGA and the 1930 debt-to-value ratio; column (2) adds 1930 agricultural covariates (land value per acre and farm size, each interacted with year indicators). Column (3) uses a tercile split, comparing counties in the top third of the 1930 debt-to-value distribution to the rest. Column (4) uses a quartile split, comparing the top quartile to the rest. A negative interaction coefficient indicates that counties with higher pre-treatment leverage shifted more strongly against Democrats after the TGA. 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 C.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: Re-estimation of the baseline DiD (Table 3) and triple-difference (Table 4) specifications after excluding the three wartime election years (1942, 1944, 1946). The dependent variable is county-level Democratic vote share (%). HG (Historical Grazing) equals one for counties overlapping a grazing district; TGA equals one for elections after 1934. Columns (1)–(2) report the DiD coefficient on HG × TGA without and with 1930 agricultural covariates (land value per acre, farm size, and debt-to-value ratio, each interacted with year indicators). Columns (3)–(4) report the triple-difference with the median HighDebt split (1930 debt-to-value ratio above the sample median of 36.5%), without and with agricultural covariates. All specifications include county and state-by-year fixed effects and the population-weighted New Deal spending index interacted with year indicators. Standard errors clustered at the county level; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

D Additional Robustness Tests

Table D.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: Instrumental variables estimation using the Bühler (2023) rainfall instrument. The instrument is the 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. Columns (1)–(2): first stage; (3)–(4): reduced form; (5)–(6): 2SLS. The IV estimates exceed OLS, consistent with measurement-error correction or LATE heterogeneity. 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 cross-sectional variation). Sample: nine TGA states. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table D.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: IV triple-difference using the Bühler (2023) rainfall instrument. The two endogenous variables (HG × TGA and HG × TGA × HighDebt) are instrumented with the standardized October 1934 rainfall and its interaction with HighDebt. Columns (1)–(2): first stage for each endogenous variable. Column (3): reduced form with the rainfall instrument only; the positive coefficient confirms that wetter counties (less TGA exposure) voted more Democratic. Column (4): reduced form with both instruments; the positive coefficient on Rainfall × TGA × High-Debt (+3.6**) confirms that exogenous rainfall variation predicts larger anti-Democratic shifts in high-debt counties, consistent with the wealth channel. Column (5): 2SLS. The IV interaction (−26.5*, $p = 0.056$) matches the sign of the OLS triple-difference (−6.4***); the magnification is consistent with measurement-error correction and LATE heterogeneity (Section 6). All covariates are 1930 agricultural variables (population, land value, farm size, 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 D.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: Robustness of the main DiD and triple-difference results to controls for differential wartime commodity exposure. Columns (1)–(4) report DiD estimates; columns (5)–(8) report triple-difference estimates. "LP" adds LivestockExposure (1930 livestock share × national cattle price) as a time-varying control. "AS" adds 1930 livestock share interacted with year indicators as a varying slope. "AS+Crop" adds both 1930 livestock share and 1930 crop value per farm interacted with year indicators. The triple-difference interaction HG × TGA × HighDebt is numerically identical across columns (5), (7), and (8) (-6.443***) because the livestock-share and crop-value varying slopes are orthogonal to the triple-diff interaction after conditioning on county and state-by-year fixed effects; standard errors widen from 2.02 to 2.26 as expected. This demonstrates that the wealth channel is not driven by differential exposure to wartime cattle price booms. All specifications include county and state × year fixed effects plus baseline covariates. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

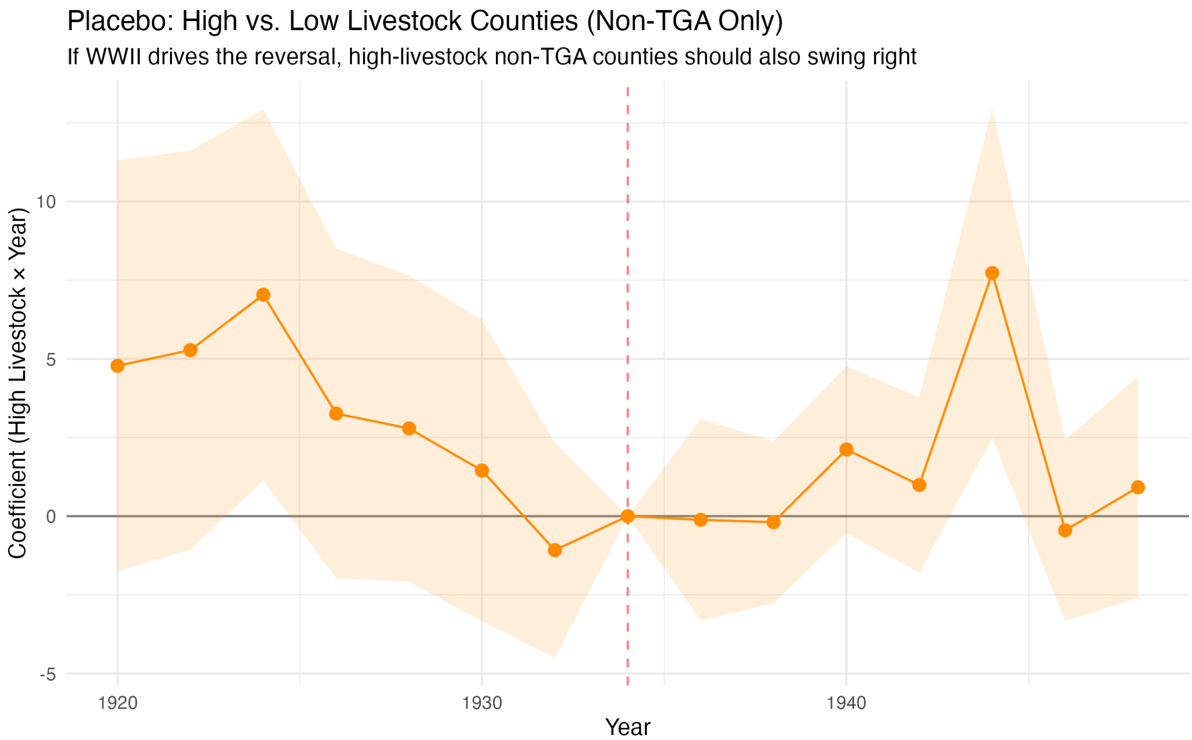


Figure D.1: Placebo: High vs. Low Livestock Counties (Non-TGA Only)

Notes: Event study comparing high-livestock to low-livestock counties *among non-TGA counties only*, with 1934 as the base year. If the 1940–42 Republican swing in TGA counties were driven by WWII's differential effect on livestock-producing areas (rather than by the TGA's wealth channel), non-TGA counties with high livestock shares should show the same pattern. They do not: no coefficient around 1940–42 is significant, and the DiD coefficient is -1.5 ($p = 0.20$). High livestock defined as above-median 1930 livestock share. County and state $\times$ year FE, New Deal spending control. $N = 527$ non-TGA counties. Shaded band: 95% CI.

Table D.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 fake treatment date of 1926. If the HighDebt interaction in Table 4 captured pre-existing differential trends rather than the TGA's wealth effects, it should appear in this placebo window. Column (1) uses 1920 debt-to-value ratios; column (2) uses 1925 debt-to-value ratios. The placebo interaction coefficients (-2.0, $p = 0.10$; $+0.9$, $p = 0.49$) are far smaller than the real estimate (-6.4***) and statistically insignificant, confirming that the wealth channel is specific to the post-TGA period. County and state × year FE. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table D.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: Comparison of observable characteristics between high-debt and low-debt counties *within TGA areas only* (213 TGA counties: 88 high-debt, 125 low-debt). HighDebt defined as 1930 debt-to-value ratio above the sample median. None of the ten comparisons reaches conventional significance, indicating that the HighDebt interaction in the triple-difference captures exposure to the TGA's wealth channel rather than pre-existing differences in economic structure, political preferences, or federal spending. p-values from two-sample t-tests.

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

Dependent Variable:	Democratic vote share	
	Original	+Grazing Share
Model:	(1)	(2)
Variables		
HG × TGA	4.306	4.306
	(4.061)	(4.122)
HG × TGA × ProTGA	-8.806**	-8.806**
	(4.306)	(4.370)
Fixed effects		
County	Yes	Yes
State × 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: Robustness of the roll-call heterogeneity test to controlling for county-level grazing intensity. A concern is that ProTGA delegation status captures the state's economic dependence on grazing rather than political credit for the legislation. Adding Grazing_Area_Share (the share of county area within grazing districts) as a varying-slope control absorbs differential trends driven by grazing-economy intensity. The HG × TGA × ProTGA interaction is unchanged (−8.81**), confirming that the delegation vote effect reflects political dynamics, not grazing exposure. All specifications include county and state × year FE plus 1930 agricultural covariates. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

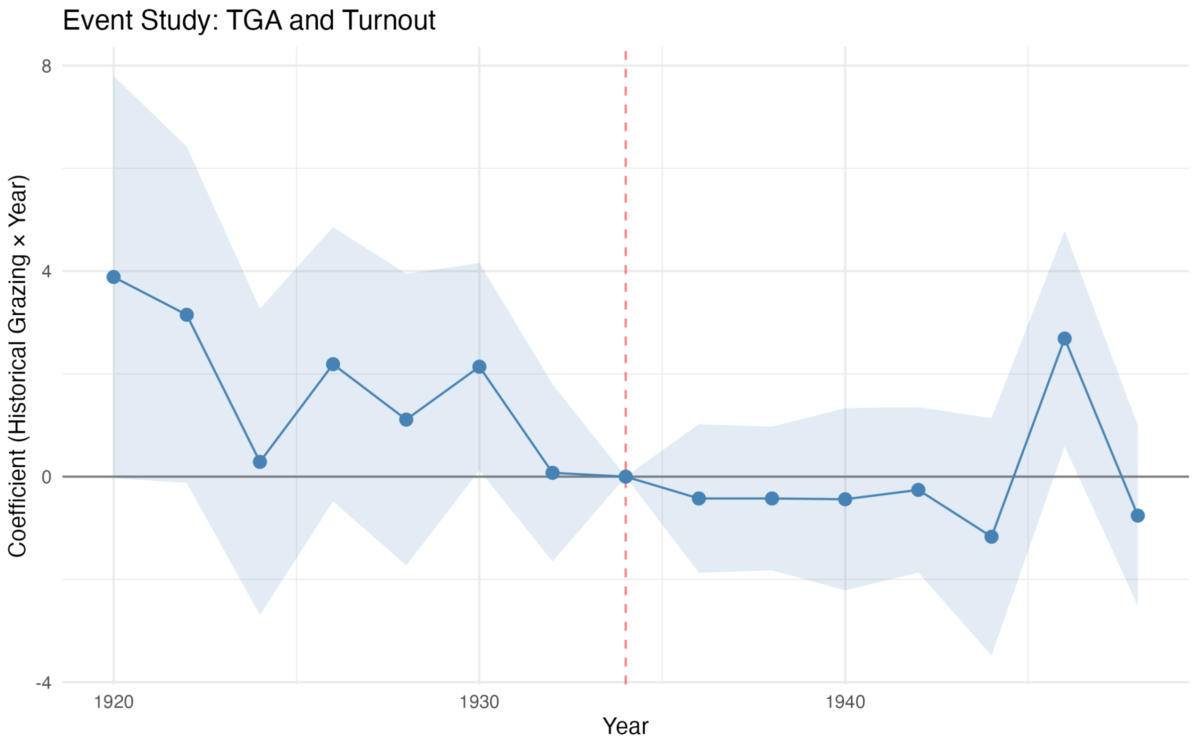


Figure D.2: Event Study: TGA and Turnout

Notes: Dynamic treatment effects of the TGA on county-level turnout (%). If the TGA's effect on Democratic vote share reflected changes in turnout composition rather than preference change, we would observe differential turnout in treated counties after 1934. The event study shows no significant turnout effect in any post-treatment year. The DiD coefficient is -1.1 ($p = 0.87$) with full controls, and the turnout triple-difference interaction (HG $\times$ TGA $\times$ HighDebt) is -5.7 ($p = 0.52$). The TGA changed how treated counties voted, not whether they voted. Observations with turnout exceeding 100% excluded as coding errors. County and state $\times$ year FE, New Deal spending control. 1920–1950.

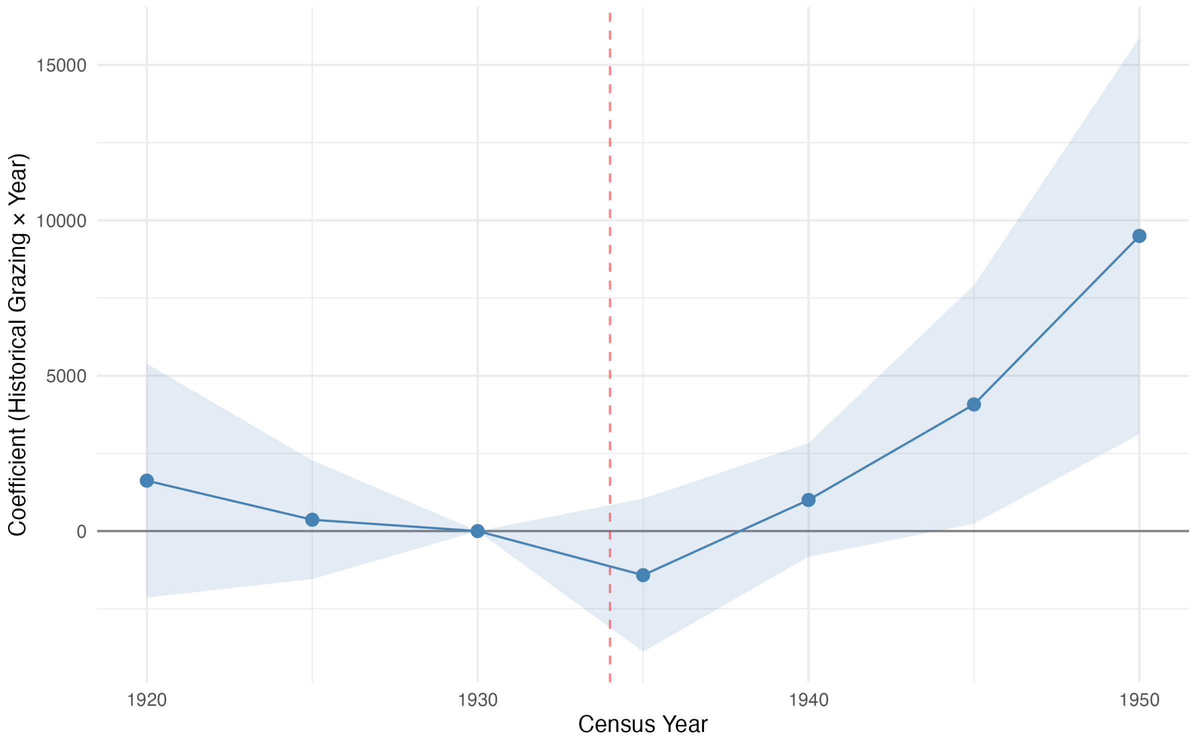


Figure D.3: Event Study: TGA and Average Farm Value

Notes: Dynamic treatment effects of the Taylor Grazing Act on average total farm value per farm (in dollars, from the Census of Agriculture). Each point estimates the difference in farm values between TGA and non-TGA counties relative to 1930. Pre-treatment coefficients (1920, 1925, 1935) are small and statistically insignificant, confirming parallel pre-trends. After the TGA, farm values in treated counties began to diverge: the gap was modest through 1940, then jumped sharply to \$4,075 by 1945 ($p = 0.038$) and \$9,500 by 1950 ($p = 0.004$). Bühler (2023) documents the mechanism: the TGA formalized grazing rights into pledgeable collateral, enabling investment in range improvements and herd expansion. Wartime commodity demand then capitalized these investments into sharply higher asset values. The timing aligns with the political reversal in Figure 2: the anti-Democratic shift sharpens precisely as the farm-value gap becomes large and statistically significant. Specification includes county and state × year fixed effects. Shaded band shows 95% confidence interval based on county-clustered standard errors. Census years: 1920, 1925, 1930, 1935, 1940, 1945, 1950. 739 counties.

Table D.7: 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 using county-level presidential vote share (Democratic %) as the outcome variable. Presidential elections eliminate candidate-quality, incumbency advantage, and redistricting concerns that affect congressional races. The Post TGA indicator equals one for elections after 1936. The 1936 election is classified as pre-treatment because grazing districts formed during 1935–1938; most 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 population-weighted New Deal spending index. The HighDebt interaction is negative and significant at 10%, confirming the congressional triple-difference (Table 4): counties where the TGA most reduced farm leverage shifted away from Democrats in presidential as well as congressional races. Sample: 741 counties, 16 presidential elections (1912–1972). Uncontested elections (Dem = 0 or 100) excluded. Standard errors clustered at the county level; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.