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The civil war reduced slave owners' economic power but increased their political influence

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Abstract
Did Southern elites' economic losses from abolition translate into diminished political influence? Using novel census-linked data on state lawmakers across four slave owning and two Northern states (1850–1880), we document a striking paradox: despite the massive wealth shock of emancipation, the political influence of former slave owners increased during Reconstruction and its aftermath. We show that former slave owners won office at similar rates as in the antebellum period and secured more committee assignments. Comparable patterns are not visible among wealthy legislators in Northern comparison states. This suggests that Southern elites responded to economic loss by tightening their grip on formal political institutions. Our findings point to formal political institutions as one channel through which defeated economic elites preserved influence during Reconstruction and its aftermath.

Keywords: wealth inequality; elites and development; US South; slavery; political power; reconstruction
JEL codes: D72; J62; N31; N41

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1 Introduction

The American Civil War fundamentally transformed the South's economic and political institutions. Above all, the abolition of slavery weakened the economic basis of the planter class, destroying the labor system that had sustained the region's elite for generations. Economic devastation of this magnitude might reasonably be expected to cause a corresponding drop in political influence: When an elite loses its economic resource base, its ability to dominate politics should be severely constrained (Bartels, 2008). The post-Civil War South is thus a crucial historical case. Did Southern elites' economic losses from abolition also translate into diminished political influence?

Scholars have long argued that the Southern planter class kept their economic standing in spite of abolition (Wiener, 1976; McKenzie, 1993), and that this led to their persistence in "de facto power"(Acemoglu and Robinson, 2008). Recent work, however, has shown that the South may have had more economic change than is commonly believed (Dupont and Rosenbloom, 2018, 2022). And while there is evidence that former slave owning elites remained active in politics (Bellani et al., 2022) and that this led to detrimental outcomes (Ager, 2013; Acharya et al., 2018), it remains unclear what the influence of Southern elites would have been had slavery not been abolished. We thus lack comparative quantitative evidence on whether abolition caused a meaningful shift in Southern elites' political influence.

This study provides the first systematic and comparative analysis of the effect of abolition on the Southern elite's political clout. We assemble newly collected longitudinal data on legislators from four slave states and two Northern states spanning the period from 1850 to 1880 across fifteen legislatures. The inclusion of two Northern states—where no large slave-owning class existed—as a comparison group allows us to disentangle the effects of the Civil War and abolition from broader 19th-century trends of elite entrenchment. By tracking three decades of legislative rosters and linking legislators to pre-war slave holding status and wealth, we can trace precisely whether Southern elites lost or, potentially, gained political influence after the abolition of slavery.

Our findings reveal a striking pattern: the end of slavery, while sharply reducing slave owners' wealth, did not diminish their political influence. If anything, the abolition of slavery was accompanied by an *increase* in political influence: Former slave holders not only continued to win elected office at high rates, they also secured a greater number of influential committee memberships within state legislatures compared to the period before abolition.

The finding that abolition led White elites to transform economic loss into political gains speaks to broader questions about elite persistence, institutional change, and long-run development after major economic upheaval. Our data illustrate how a powerful elite can withstand the destruction of its economic base by reconstituting its influence through political channels. The paradoxical gain of political influence among former Southern slave holders underscores the resilience of elite-dominated institutions.

2 Data

To map the political influence of Southern and Northern elites, we begin by systematically recording all elected legislators in the state Houses and Senates of six U.S. states from 1850 to 1880, covering one decade before the Civil War and 15 years after its end. Our data come from the annual journals published by the different respective chambers of state legislatures. In total, we compiled 13,993 legislator-legislature observations, all male. Geographically, we have three Southern states (Alabama, Florida, and Virginia), two Northern states (New York and Wisconsin) and the border state of Kentucky.[1] The legislative journals usually record the names and counties of residence for all legislators, thus providing us with the universe of legislators in a given chamber and year.

Next, we use the names of legislators and the counties they represented for the 13,993 legislator–legislature observations to manually link them to the searchable historical census

[1]Slavery was legal in Kentucky, but the majority of the population remained loyal to the United States government during the Civil War. In the case of Florida, Kentucky, and New York, we have data on the respective state houses only.

records available through Ancestry.com[2] in order to identify each legislator's most recent census entry prior to election. From this entry, we manually trace the individual to his household—or, where applicable, to his ancestral household—in the 1860 census (or the 1850 census for legislators elected before 1860). These links were established through manual searches rather than automated record linkage. The 1850 and 1860 censuses report individual-level information on real estate wealth and slaveholdings. In the Southern states, we use these records to code whether a legislator came from a slaveholding family. In the Northern states, we construct a comparable elite group by coding whether the legislator (or his family) belonged to the top quartile of real estate wealth in 1860, using state-specific real estate thresholds calculated from IPUMS (Ruggles et al., 2021).

In order to capture the respective elite's political influence, we measure the number of committees a given legislator sits on. Committee assignments constituted an important mechanism of legislative influence in nineteenth-century state legislatures because committees controlled agenda-setting, bill processing, and legislative specialization. While committees differed in importance, assignment patterns reflected legislators' standing within the chamber and their integration into legislative networks. Committee membership was also recorded consistently in our six legislatures, but in different documents. To link legislators to committee assignments, we merge the legislator and committee files using fuzzy string matching. All matches are performed separately by legislative year to prevent cross-session mismatches. Within each year, we apply Stata's `reclink` algorithm (a standard probabilistic record-linkage routine written by Michael Blasnik) in two passes: first using surnames only, and second using full names when first names are available in the committee data. Each candidate pair receives a similarity score, and for each legislator–year observation we retain the highest-scoring match subject to a high-confidence threshold of 0.90. Committee outcomes are constructed only for uniquely matched observations; ambiguous cases are excluded. This procedure prioritizes precision over coverage and yields a conservative, high-

[2]See the Appendix for full citations.

confidence legislator–committee panel. As a robustness check, we independently reconstruct the legislator–committee links using an alternative LLM-assisted fuzzy matching procedure. Re-estimating the main specifications using these alternative matches yields substantively similar estimates, indicating that the findings are not sensitive to the specific linkage procedure (see Appendix for details). Committee membership was widespread, with nearly every legislator in our sample serving on at least one committee. The average legislator sits on five committees, while the median is three. This difference reflects a pronounced right tail: 5% of legislators serve on 15 or more committees, with a maximum of 79. Committees vary widely in substantive importance. However, even membership in ceremonial committees—such as the committee appointed "to wait upon the provisional governor, and inform him that the two houses of the general assembly are now organized and ready to receive any communication he may have to make"—reflects a degree of recognition and standing among fellow legislators. Overall, we are able to link 7,054 legislator-legislature observations to both their committee memberships and to their slaveholding status. Of these, 3,017 hail from slave states (including Kentucky) and 4,037 from free states. For these, we know their legislative tenure, their committee membership, slaveholding background, as well as their real estate wealth (which we use as control variable to abstract from general wealth effects) prior to getting elected.

Table 1: State-Level Committee Assignments (Means and Standard Deviations)

State	Mean # Committees (SD)	N
Alabama	5.59 (4.92)	775
Florida	4.00 (4.62)	258
Kentucky	9.41 (9.48)	638
New York	5.11 (4.87)	1,873
Virginia	3.96 (3.19)	1,346
Wisconsin	4.13 (3.72)	2,164
Total	4.99 (5.13)	7,054

Notes: The table reports state-level summary statistics across all legislator-legislature observations. Cells show the mean with the standard deviation in parentheses. N denotes the number of legislator-legislature observations.

The final dataset allows us to conduct two related but distinct analyses. First, we document how the share of legislators with an elite background in 1860 evolves over time in Northern and Southern states. Second, we estimate the effects of the Civil War and the ensuing abolition of slavery on legislators' political influence within the legislature, measured by committee assignments. To do so, we employ an event-study difference-in-differences design that compares committee membership of elite and non-elite legislators over time, while flexibly allowing the elite–non-elite gap to vary across legislative periods. Specifically, for legislator i from state s observed in period t, we estimate

$$number_committees_{ist} = \alpha\, Elite_i + \sum_{\tau \in \mathcal{T},\, \tau \neq 1860\text{–}1861} \beta_\tau \left(Elite_i \times \mathbf{1}\{t = \tau\} \right) + X_i'\gamma + \delta_t + \lambda_s + u_{ist}, \tag{1}$$

where δ_t and λ_s denote calendar-year and state fixed effects, respectively, and u_{ist} is an error term. To reduce noise in the dynamic coefficients, we group years into two-year bins in the interaction terms $\mathbf{1}\{t = \tau\}$, while retaining calendar-year fixed effects to absorb common year-to-year shocks. The period 1860–1861 serves as the omitted reference category. The coefficients β_τ capture period-specific differences in committee assignments between elite and non-elite legislators relative to the 1860–1861 baseline, tracing the evolution of the elite–non-elite gap over time. The vector X_i includes individual real estate holdings measured in 1860 (or 1850 for legislators first elected before 1860), as well as an indicator for serving in the House (as opposed to the Senate). In the South, $Elite_i$ equals one if legislator i belonged to a slave-owning family in 1860 (or 1850 if first elected before 1860). In the North, $Elite_i$ equals one if i was in the top quartile of his state's real estate distribution in 1860 (or 1850 if first elected before 1860).

For ease of interpretation, we also estimate a more parsimonious specification that summarizes the dynamics into three periods: pre-war, Civil War, and post abolition. Specifically, we estimate a specification interacting the elite indicator with indicators for the Civil War years (1861–1865) and the post-abolition period (after 1865), with pre-war years ($\leq$ 1860)

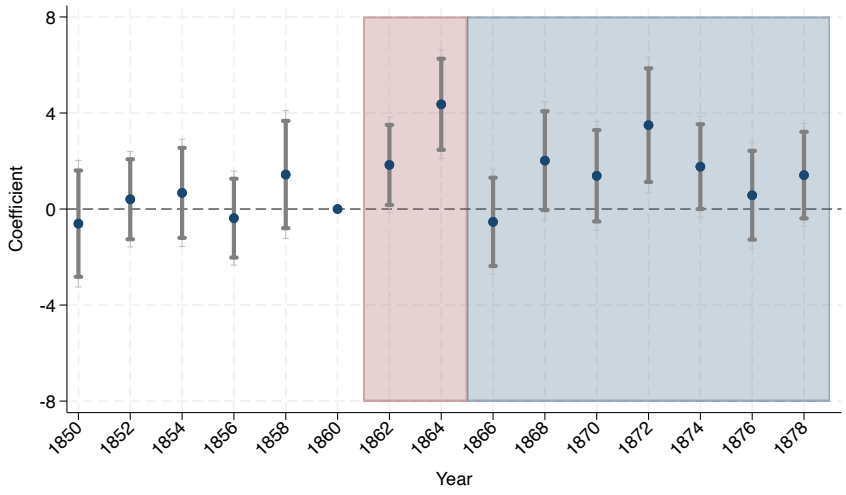
as the omitted category. The coefficients on these interactions capture how the gap in committee assignments between elite and non-elite legislators changes during and after the Civil War relative to the pre-war baseline. This specification provides a compact summary of the dynamic patterns captured by the event-study estimates.

3 Results

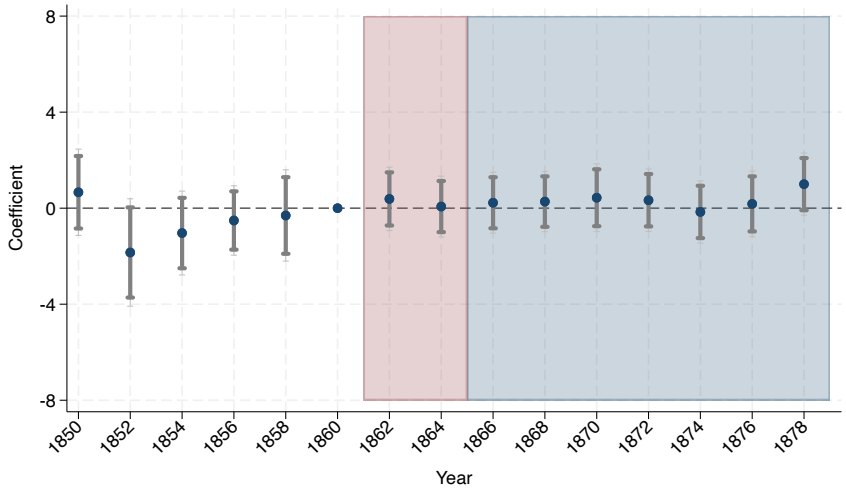
To assess whether the abolition of slavery affected the political influence of Southern elites, we examine the evolution of committee assignments over time. Figure 1 presents event-study coefficients capturing year-group-specific differences in the number of committee assignments held by elites relative to non-elites. In former slave states (Panel A), the pre-war coefficients fluctuate around zero, providing support for the parallel trends assumption. Following the Civil War and emancipation, the estimates become positive and statistically significant in most war and postwar years. Substantively, the postwar differential corresponds to roughly two additional committee memberships for former slave holders relative to non-slave holders. Thus, the Civil War and the abolition of slavery coincided, if anything, with an increase in the relative political influence of former slave holders. A potential concern is that this pattern reflects broader postwar developments—such as elite consolidation or party reorganization—rather than emancipation per se. To address this possibility, Panel B provides a placebo comparison using two Northern states, New York and Wisconsin, where no abolition shock occurred during this period. In these states, we define elites as legislators in the top quartile of the state real estate distribution. We do not observe a comparable postwar shift: the coefficients remain close to zero and statistically indistinguishable from the pre-war period. The absence of a similar pattern in the North suggests that the postwar increase in committee influence is specific to former slave states rather than reflecting a general nationwide change in elite influence.

To summarize these patterns in a more parsimonious specification, table 2 reports es-

Figure 1: Event-study estimates of elite legislators' committee assignments



(A) Former slave states



(B) Non-slave states

Notes: The figure plots event-study coefficients from a two-way fixed effects specification interacting an indicator for *Elite* with two-year period indicators. *Elite* indicates slave ownership in former slave states and top-quartile real estate wealth in non-slave states. The omitted reference period is 1860–1861. All regressions include state and two-year period fixed effects, as well as controls for individual real estate holdings and chamber (House vs. Senate). The shaded region over 1861–1865 denotes the Civil War period. The shaded region over 1866–1878 denotes the period after the abolition of slavery. Vertical bars denote 90 (bold) and 95% (light) confidence intervals based on heteroskedasticity-robust standard errors.

timates from a difference-in-differences specification that allows the elite–non-elite gap to vary across three periods: pre-war, Civil War, and post-abolition of slavery. In the South, there is little evidence of a systematic elite advantage before the war: the baseline coefficient

Table 2: Elite Legislators and Committee Assignments Before and After the Civil War

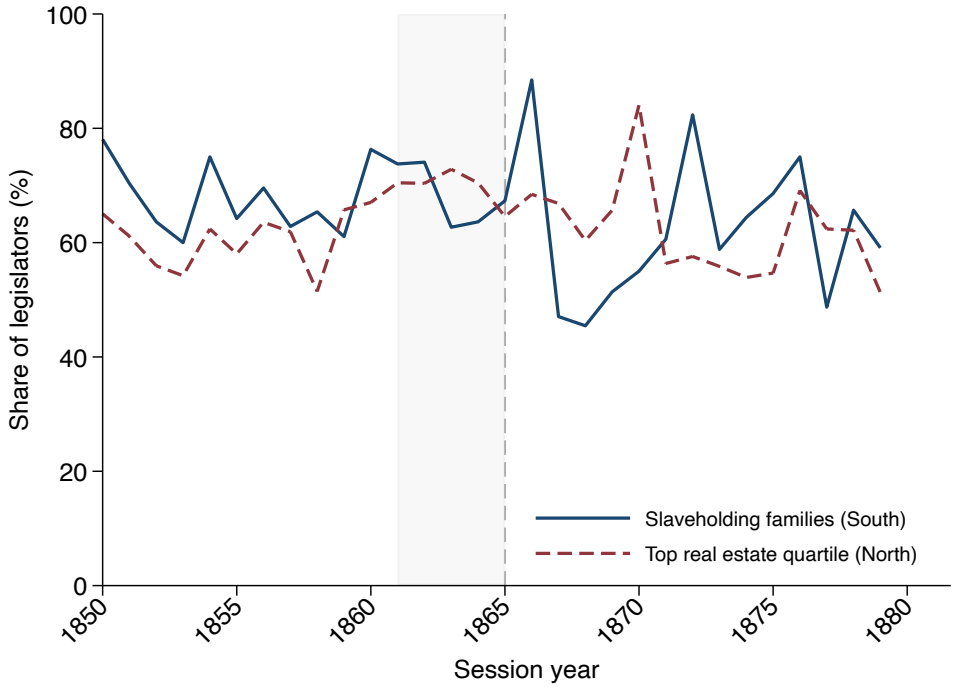
	(1)	(2)
	South	North
Civil War × Elite	1.900***	0.369
	(0.534)	(0.379)
After Abolition × Elite	1.218**	0.665*
	(0.434)	(0.317)
Elite	-0.194	-0.613*
	(0.334)	(0.293)
Mean dep. var.	5.54	4.58
Observations	3017	4037
R-squared	0.230	0.311
Sample	South	North
Controls	Yes	Yes
State FE	Yes	Yes
Year FE	Yes	Yes

Notes: The table reports difference-in-differences estimates for former slave states (South) and non-slave states (North). The dependent variable is the number of committee assignments held by a legislator in a given year. *Elite* is an indicator equal to one if the legislator belonged to the elite group (slave-owning families in the South and top real-estate holders in the North, as defined in the text). The coefficients on *Civil War* × *Elite* and *After Abolition* × *Elite* capture the change in committee assignments for elite legislators during the Civil War (1861–1865) and after the abolition of slavery (post-1865) relative to the pre-war elite–non-elite difference. All specifications include state and year fixed effects and control for individual real estate holdings and chamber (House vs. Senate). Robust standard errors are reported in parentheses.

on *Elite* is small and statistically insignificant. During the Civil War, however, the gap in committee assignments between elites and non-elites widens substantially. The coefficient on *War* × *Elite* indicates that elite legislators served on roughly 1.9 more committees than non-elites relative to the pre-war difference. This relative advantage remains positive in the post-abolition period, with elites holding about 1.2 more committee assignments compared to the pre-war elite–non-elite gap. The pattern in the North is markedly different. The interaction between elite status and the war period is small and statistically insignificant, suggesting that the Civil War did not generate a comparable shift in committee representation among elites. Although the post-abolition coefficient is positive, it is considerably smaller than in the South. Overall, the results point to a pronounced increase in committee

influence among elite legislators in the South during and after the Civil War, a pattern that is largely absent in Northern legislatures. These patterns are consistent with a consolidation of legislative influence among elite legislators in the South during and after the Civil War.

Figure 2: Elite Representation in Northern and Southern Legislatures



Notes: The solid line shows the share of legislators in slave states from slaveholding families. The dashed line shows the share of legislators in non-slave states whose household belongs to the top quartile of the real estate wealth distribution. The shaded area indicates the Civil War (1861–1865). The vertical dashed line marks 1865, the year of abolition.

Another potential concern is that the results may simply reflect a broader increase in the representation of wealthy elites in legislatures. Figure 2 therefore plots the share of Southern legislators from slaveholding backgrounds and the share of Northern legislators drawn from the top quartile of the real estate wealth distribution.[3] The two series follow broadly similar trends over time, with no sustained postwar increase in elite representation in the North. If anything, elite shares decline slightly in both regions. These patterns suggest that the

[3]Because committee information is not required for this analysis, the figure uses a slightly larger dataset of 7,778 legislator–legislature observations that could be matched to their 1850 or 1860 census records.

post-1865 increase in the relative committee influence of former slaveholders in the South does not reflect a general nationwide strengthening of economic elites, but instead points to a Southern-specific shift in political influence following abolition.

Taken together, the event-study estimates and the descriptive evidence on legislative composition suggest that the increase in committee influence among Southern elites reflects a shift in their relative political influence rather than a broader rise in the representation of economic elites. Overall, our results point to persistence—and even intensification—rather than displacement of the pre-abolition elite. Although abolition coincided with institutional adjustments and changes in legislative organization, former slaveholders continued to be elected to legislatures at roughly the same rates as before and became more influential within them. Rather than eroding their political power, the collapse of slavery as an economic institution appears to have been accompanied by greater political engagement and influence among former slaveholders.

4 Conclusion

This paper shows that the abolition of slavery did not produce a corresponding collapse in the political influence of Southern elites. Although former slaveholders experienced large economic losses after emancipation, they remained disproportionately represented in state legislatures and continued to occupy influential political positions during Reconstruction and beyond. Rather than disappearing with the destruction of slavery, elite power adapted and reemerged through political channels.

These findings speak to broader questions about elite persistence following major institutional and economic shocks. The destruction of an economic system does not necessarily eliminate the social networks, organizational advantages, or political resources that underpin elite influence. Instead, elites may respond to threats to their economic position by deepening their political entrenchment. In the postbellum South, this continuity likely contributed

to the backlash against Black political participation and officeholding documented by recent work (Logan, 2023; Chyn et al., 2024; Testa and Williams, 2026). By preserving influence within political institutions, former slaveholding elites were able to shape the trajectory of Reconstruction and constrain the transformative potential of emancipation, helping lay the foundations for the Jim Crow order that followed.

The broader pattern extends beyond the nineteenth-century American South. Similar dynamics emerged in post-emancipation Caribbean societies. In Jamaica and other parts of the British West Indies, for example, the abolition of slavery did not dismantle planter influence. Instead, planter elites adapted by restricting suffrage, maintaining control over colonial assemblies, and directing public resources toward their own interests (Bertinelli et al., 2025). Across these contexts, abolition weakened the economic foundations of slaveholding elites without fully dislodging their political power.

More generally, our findings suggest that political institutions can serve as a mechanism through which entrenched elites preserve influence when their economic dominance is threatened. This highlights an important limitation of institutional reforms that target formal economic arrangements alone. Even transformative shocks such as emancipation may leave underlying political hierarchies largely intact if incumbent elites retain access to political office and institutional power.

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A Legislator–Committee Record Linkage

This appendix documents in detail how legislator rosters are linked to committee records. Because historical sources do not contain unique personal identifiers and frequently exhibit missing first names and spelling variation, the matching procedure is explicitly rule-based, and conservative.

Data Inputs

For each state and chamber, two datasets are used:

1. **Legislator file**, containing one observation per legislator and legislative year, with variables:
    - first name (sometimes missing),
    - surname,
    - district,
    - legislative year.
2. **Committee file**, containing one observation per committee entry, with variables:
    - first name (often missing),
    - surname,
    - legislative year,
    - number of committees served on in that year.

The goal is to assign, for each legislator–year observation, the number of committees served on in that year, if a sufficiently confident match to the committee file exists.

Baseline Matching Algorithm (Stata Implementation)

The baseline matching is implemented in Stata using the `reclink` record-linkage routine. The algorithm is applied separately for each state, chamber, and legislative year.

Step 1: Exact Year Restriction. For a given legislator–year observation, only committee entries from the same legislative year are considered. Matches across different years are never allowed.

Step 2: Two-Pass Name Matching. Within each legislative year, fuzzy matching proceeds in two passes:

1. **Surname-only pass:** Legislators are matched to committee entries using surname similarity only.
2. **Full-name pass:** Matching is repeated using both surname and first name when first names are available in the committee data.

Both passes generate similarity scores between 0 and 1. The resulting candidate matches are appended.

Step 3: Best-Match Selection. For each legislator–year observation, the committee entry with the highest similarity score is selected.

Step 4: High-Confidence Threshold. A match is accepted only if the maximum similarity score is at least 0.90. Observations not meeting this threshold are treated as unmatched.

Step 5: Near One-to-One Restriction. To prevent ambiguous linkages, matches are classified as *unique* if:

- the legislator record is linked to exactly one committee entry, and

- the committee entry is linked to at most one legislator in the same year.

Observations involved in non-unique matches are not used to construct committee outcomes.

Construction of Committee Variables

Committee participation variables are defined only for uniquely matched legislator–year observations.

- If a uniquely matched observation has a missing value for `number_committees`, this is interpreted as zero committee service.
- For non-unique or ambiguous matches, committee variables are set to missing.

This rule ensures that ambiguous matches do not mechanically generate false non-membership assignments and that zero values reflect confirmed absence of committee service.

Record-Linkage Diagnostics

Table A1 reports diagnostic statistics for the legislator–committee linkage by state and chamber. "Match rate" denotes the share of legislator–year observations with an accepted unique match under the baseline Stata implementation. "Duplication share" measures the fraction of accepted matches in which a committee record is linked to more than one legislator in the same year. Confidence statistics refer to the similarity score of the accepted match. Across states and chambers, match rates are high and accepted matches exhibit very high similarity scores. No duplicate assignments are observed among accepted matches, reflecting the enforcement of the near one-to-one restriction. Potential duplicate linkages are excluded at the matching stage.

Decomposition of Match Failures

To further assess the sources of non-matches, Table A2 decomposes unmatched observations by failure type. Failure shares are computed conditional on being unmatched. "Fail: low

Table A1: Matching Diagnostics by State and Chamber

State	Chamber	Obs.	Match Rate	Dup. Share	Avg. Conf.	P10	Median	P90
Alabama	House	1365	0.691	0.000	0.998	1.000	1.000	1.000
Alabama	Senate	644	0.957	0.000	0.999	1.000	1.000	1.000
Florida	House	652	0.850	0.000	0.998	0.999	1.000	1.000
Kentucky	House	1864	0.528	0.000	1.000	1.000	1.000	1.000
New York	House	4031	0.959	0.000	0.999	1.000	1.000	1.000
Virginia	House	2192	0.929	0.000	1.000	1.000	1.000	1.000
Virginia	Senate	606	0.983	0.000	0.999	1.000	1.000	1.000
Wisconsin	House	2579	0.914	0.000	0.998	1.000	1.000	1.000
Wisconsin	Senate	942	0.978	0.000	0.999	1.000	1.000	1.000

Notes: The table reports the number of legislator–year observations, the share successfully matched to committee records (Match Rate), the share of duplicated matches among accepted links (Dup. Share), and the distribution of similarity scores for accepted matches. Matches are accepted if the similarity score is at least 0.90 and satisfy the near one-to-one restriction. Percentiles are computed across accepted matches only.

similarity" indicates that the highest-scoring candidate match falls below the acceptance threshold (0.90). "Fail: ambiguous legislator" captures cases in which multiple candidate matches are associated with the same legislator record. "Fail: reused committee" reflects cases in which a committee entry is linked to multiple legislators. The table shows that most non-matches arise from ambiguous candidate matches rather than low similarity scores. Cases involving multiple potential legislator matches account for the majority of unmatched observations, while failures due to insufficient similarity are comparatively rare. Reuse of committee records occurs in some unmatched cases, but is systematically excluded by the near one-to-one restriction. These patterns indicate that the conservative acceptance rule primarily excludes ambiguous multi-match cases rather than borderline similarity matches. Importantly, these exclusions reflect genuine ambiguity in historical records rather than weak string correspondence. The matching procedure therefore prioritizes precision over coverage by discarding cases in which multiple plausible links exist.

Table A2: Match Failure Diagnostics Conditional on Unmatched Observations

State	Chamber	Unmatched rate	Fail: low similarity	Fail: ambiguous legislator	Fail: reused committee
Alabama	House	0.309	0.047	0.924	0.090
Alabama	Senate	0.043	0.250	0.571	0.429
Florida	House	0.150	0.327	0.653	0.061
Kentucky	House	0.472	0.011	0.986	0.050
New York	House	0.041	0.145	0.831	0.361
Virginia	House	0.071	0.115	0.846	0.333
Virginia	Senate	0.017	0.500	0.400	0.700
Wisconsin	House	0.086	0.303	0.643	0.172
Wisconsin	Senate	0.022	0.238	0.762	0.381

Notes: "Unmatched rate" is the share of legislator–year observations without an accepted unique match. Failure shares are computed conditional on being unmatched. "Fail: low similarity" indicates that the highest-scoring candidate falls below the similarity threshold (0.90). "Fail: ambiguous legislator" reflects multiple candidate matches for the same legislator record. "Fail: reused committee" indicates reuse of the same committee entry across legislators.

Alternative LLM-Assisted Robustness Matching

As a robustness check, we independently reconstruct legislator–committee links using an alternative automated fuzzy-matching procedure. The LLM was used only to evaluate candidate name similarity within predefined legislative-year match sets and did not rely on contextual historical inference or manual reconciliation. This alternative procedure was implemented independently of the baseline Stata linkage routine and serves solely as a robustness exercise.

Let i index legislator–year observations and j index candidate committee records. The procedure proceeds as follows:

1. **Exact temporal blocking.** For each legislator i observed in legislative year t, candidate matches are restricted to committee records j from the identical year t. Cross-year matches are not permitted.

2. **Initial blocking.** Within year t, candidate comparisons are restricted to records whose surnames share the same first letter as the legislator's surname. If no such candidates exist, the algorithm expands the candidate set to all committee records within year

t. This blocking rule reduces computational burden without altering the scoring or acceptance criteria.

3. **Name normalization.** All names are lower-cased and stripped of leading and trailing whitespace. No manual harmonization or ex post corrections are introduced.

4. **String similarity scoring.** For each candidate pair (i, j), similarity scores are computed separately for surnames and first names using a string-matching metric bounded on $[0, 1]$. Let S_{ij}^{last} and S_{ij}^{first} denote the respective similarity measures.

5. **Weighted aggregation.** The overall similarity score is constructed as

$$S_{ij} = 0.7 \cdot S_{ij}^{\text{last}} + 0.3 \cdot S_{ij}^{\text{first}}.$$

   When first names are missing in either source, a neutral similarity value is assigned to the first-name component to prevent mechanical penalization due to incomplete records.

6. **Pre-filtering of implausible matches.** Candidate pairs with large surname-length discrepancies or extremely low surname similarity are discarded prior to aggregation. These filters eliminate comparisons that cannot satisfy the final acceptance criterion.

7. **Best-match selection.** For each legislator–year observation i, the committee record j with the highest S_{ij} is selected.

8. **Acceptance threshold.** A match is accepted if and only if

$$\max_j S_{ij} \geq 0.75.$$

   Legislator–year observations failing to meet this threshold are classified as unmatched and assigned zero committee memberships.

9. **Duplicate identification.** The procedure does not impose one-to-one constraints. Instead, committee records matched to multiple legislators are explicitly flagged, allowing robustness checks that exclude potentially ambiguous assignments.

The procedure was implemented using standardized prompts and predefined matching constraints without ex post manual reconciliation. The resulting linkage therefore constitutes a fully automated and independently implemented reconstruction of legislator–committee assignments. The prompts and matching instructions used in the LLM-assisted procedure are available from the authors upon request.

Table A3: Matching Diagnostics by State and Chamber

State	Chamber	Obs.	Match Rate	Dup. Share	Avg. Conf.	P10	Median	P90
Alabama	House	1171	0.968	0.009	0.957	0.940	1.000	1.000
Alabama	Senate	637	0.854	0.016	0.822	0.000	0.940	0.940
Florida	House	622	0.960	0.003	0.925	0.893	0.940	1.000
Kentucky	House	1431	0.988	0.004	0.957	0.940	0.940	1.000
New York	House	3964	0.981	0.005	0.929	0.940	0.940	0.940
Virginia	House	2127	0.952	0.016	0.910	0.940	0.940	1.000
Virginia	Senate	605	0.661	0.003	0.627	0.000	0.940	0.940
Wisconsin	House	2510	0.977	0.012	0.923	0.899	0.940	0.940
Wisconsin	Senate	935	0.986	0.000	0.932	0.940	0.940	0.940

Notes: The table reports the number of legislator–year observations, the share successfully matched to committee records (Match Rate), the share of duplicate matches (Dup. Share), and the distribution of match confidence scores. Matches are accepted if the similarity score is at least 0.75. Confidence percentiles are calculated across all legislator–year observations.

The table A3 reports the number of legislator–year observations, the share of legislators successfully matched to committee records, the share of duplicate matches, and the distribution of match confidence scores. Match rates are high across most states and chambers. Duplicate assignments are rare. Confidence scores are strongly concentrated near 0.94 and 1.00, indicating that the vast majority of accepted matches are well above the similarity threshold of 0.75.

Comparison Across Matching Strategies

The LLM-assistedprocedure produces match rates, similarity distributions, and committee assignments that closely mirror those obtained under the baseline Stata implementation. Re-estimating the main specifications using the LLM-linked dataset yields substantively identical results. (see Table A4)

Table A4: Robustness to Alternative Record-Linkage Procedures

	(1)	(2)
	Baseline match	AI match
Civil War × Elite	1.900***	1.632***
	(0.534)	(0.490)
After Abolition × Elite	1.218**	1.129**
	(0.434)	(0.412)
Elite	-0.194	-0.273
	(0.334)	(0.330)
Mean dep. var.	5.54	5.31
Observations	3017	3405
R-squared	0.230	0.187
Controls	Yes	Yes
State FE	Yes	Yes
Time FE	Yes	Yes

Notes: The table reports estimates for Southern states using two alternative procedures to link legislators across historical records. Column (1) uses the baseline record-linkage procedure described in the text, while column (2) uses the LLM-assisted fuzzy matching algorithm described in the Appendix. The dependent variable is the number of committee assignments held by a legislator in a given year. *Elite* indicates slave ownership. The coefficients on *Civil War* × *Elite* and *After Abolition* × *Elite* capture the change in committee assignments for elite legislators during the Civil War (1861–1865) and after the abolition of slavery (post-1865) relative to the pre-war elite–non-elite difference. All specifications include state and year fixed effects and control for chamber and real-estate wealth. Robust standard errors are reported in parentheses

The convergence of findings across two independently implemented matching strategies indicates that the paper's conclusions are not sensitive to the specific record-linkage algorithm employed.

B Data sources for census matching

Ancestry.com. *1860 U.S. Federal Census - Slave Schedules* [database on-line]. Lehi, UT, USA: Ancestry.com Operations Inc, 2010. Original data: United States of America, Bureau of the Census. *Eighth Census of the United States*, 1860. Washington, D.C.: National Archives and Records Administration, 1860. M653, 1,438 rolls.

—*1850 U.S. Federal Census - Slave Schedules* [database on-line]. Lehi, UT, USA: Ancestry.com Operations Inc, 2004. Original data: United States of America, Bureau of the Census. *Seventh Census of the United States*, 1850. Washington, D.C.: National Archives and Records Administration, 1850. M432, 1,009 rolls.

— *1850 United States Federal Census* [database on-line]. Lehi, UT, USA: Ancestry.com Operations, Inc., 2009. Images reproduced by FamilySearch. Original data: Seventh Census of the United States, 1850; (National Archives Microfilm Publication M432, 1009 rolls); Records of the Bureau of the Census, Record Group 29; National Archives, Washington, D.C.

— *1860 United States Federal Census* [database on-line]. Lehi, UT, USA: Ancestry.com Operations, Inc., 2009. Images reproduced by FamilySearch. Original data: 1860 U.S. census, population schedule. NARA microfilm publication M653, 1,438 rolls. Washington, D.C.: National Archives and Records Administration, n.d.

— *1870 United States Federal Census* [database on-line]. Lehi, UT, USA: Ancestry.com Operations, Inc., 2009. Images reproduced by FamilySearch. Original data: 1870 U.S. census, population schedules. NARA microfilm publication M593, 1,761 rolls. Washington, D.C.: National Archives and Records Administration, n.d. Minnesota census schedules for 1870. NARA microfilm publication T132, 13 rolls. Washington, D.C.: National Archives and Records Administration, n.d.

Ancestry.com and The Church of Jesus Christ of Latter-day Saints. *1880 United States Federal Census* [database on-line]. Lehi, UT, USA: Ancestry.com Operations Inc, 2010. 1880 U.S. Census Index provided by The Church of Jesus Christ of Latter-day Saints © Copy-

right 1999 Intellectual Reserve, Inc. All rights reserved. All use is subject to the limited use license and other terms and conditions applicable to this site. Original data: Tenth Census of the United States, 1880. (NARA microfilm publication T9, 1,454 rolls). Records of the Bureau of the Census, Record Group 29. National Archives, Washington, D.C.